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Briggs Depew
University of Arizona

Todd A Sørensen
University of California, Riverside
IZA

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UNIVERSITY OF CALIFORNIA, LOS ANGELES

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Briggs Depew
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Abstract

Recent empirical work has found evidence that the elasticity of labor supply to individual firms is finite, implying that firms may have wage setting power. However, these studies capture only snapshots of the elasticity. We are the first to study how it changes between economic contractions and economic expansions. To do this, we extend the current identification strategy by relaxing the assumption that a firm replaces all separations with recruits. We take our identification strategy to data by studying two manufacturing firms during the volatile inter-war period. Our analysis suggests that the elasticity is lower during recessions than expansions, providing evidence of differential wage setting power over the business cycle. This differential wage setting ability renders an alternative explanation of the pro-cyclicality of real wages and provides a deeper insight into the phenomenon of jobless recoveries.

Contact Briggs Depew at bdepew@email.arizona.edu and Todd Sorensen at todd.sorensen@ucr.edu.
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1 Introduction

One of the most important, yet understudied, issues within the field of labor economics is the degree of wage setting power that firms possess. This wage setting power can help explain puzzling empirical findings such as the gender wage gap, the fact that low wage workers are more likely than high wage workers to search for new jobs and separate from existing employment and the lack of evidence of large effects of the minimum wage on employment, among other things (Manning, 2003: p. 361).[1] We are the first to study how wage setting power varies over the business cycle and we find evidence that it is counter-cyclical, providing insight into both the observed pro-cyclicality of real wages as well as the phenomenon of jobless recoveries.

Standard macroeconomic models account for the existence of search frictions which, even in a thick labor market with many competing firms, imply that individual firms may face an upward sloping labor supply curve. In wage setting models, a firm facing an upward sloping labor supply curve has the ability to mark down wages below the marginal revenue product, just as a firm facing a downward sloping product demand curve has the ability to mark up prices over marginal cost. The degree of this wage setting power depends on the value of the elasticity of the labor supply curve to the firm. The assumption that labor markets are frictionless, and thus the elasticity of labor supply to a firm is infinite, suggests that workers face no mobility constraints and receive no rents from jobs. In the words of Manning (2003), the fact that "people go to the pub to celebrate when they get a job rather than greeting the news with the shrug of the shoulders" and likewise that "people go to the pub to drown their sorrows when they lose their job rather than picking up another one straight away" belie the simple notion of frictionless labor markets.

The value of the elasticity of supply to an individual firm is ultimately an empirical question. Recent work has found evidence that this elasticity is in the range of 1 to 10, suggesting a broad range of firm wage setting power.[2] However, the heterogeneity of these estimates is not surprising:

[1] Card and Krueger (1995) argue that monopsonistic labor markets could explain the lack of empirical evidence of large employment effects of the minimum wage. Danziger (2010) argues that imperfectly enforced minimum wages may actually create monopsony power.

[2] Boal (1995), Ransom and Oaxaca (2005), Ransom and Oaxaca (2010), Hirsch, Shank and Schnabel (2006), Hirsch (2007), Hotchkiss and Quispe-Agnoli (2009), Ransom and Sims (2010), Hirsch, Shank and Schnabel (2010), Falch (2010), Ransom and Lambson (2011), Falch (2011) Dube, Lester and Reich (2011) and Webber (2012).

it follows from that fact that the individual firms studied compete for different types of workers in different geographic labor markets and may face different macroeconomic conditions.

The primary contribution of our paper is to move beyond previous snapshot estimates of the elasticity and to understand how the elasticity of labor supply to an individual firm varies over the business cycle. We find strong evidence that this parameter is finite and pro-cyclical, and thus wage setting power is counter-cyclical. Our result is consistent with the comparative statics in a simple wage-posting search model, where job offers to workers arrive at a lower rate during a recession; the slower arrival rate decreases the threat of workers separating from a firm that pays low wages. This is consistent with empirical evidence of the pro-cyclicality of real wages, but provides a different theoretical basis than has been offered in the past: differential wage setting ability over the business cycle.

To test the cyclicality of firm wage setting power, we study two different labor markets by analyzing employee behavior at two manufacturing firms. Hall and Krueger (2010) survey a representative sample of U.S. workers and provide evidence that a wage posting model best describes the labor market for blue collar workers, as only 5% of these workers bargain over their wages. The wage posting characteristics of the manufacturing industry provide an ideal environment in which to test the implications of a monopsonistic labor market as the underlying theoretical model is derived from a simple wage-posting search model (Burdett and Mortensen 1998). The firms we study were located in different geographic labor markets during the inter-war years of 1919 through 1940, covering five NBER-defined contractions and six such expansions. No other twenty-two year period contains the same frequency and degree of business cycle volatility, thus providing us with the variation needed to identify the cyclicality of the elasticity of labor supply to an individual firm.

We obtain these estimates by using individual payroll records on approximately 5000 workers to regress the decision to separate from the firm on an individual worker's wage. Even after conditioning on a rich set of covariates, it is likely that our point estimates suffer from omitted variable bias. However, the purpose of this paper is to study how the elasticities change over the business cycle. Therefore, the omitted variable bias is less of a concern if one is able to sign the direction of the bias and understand how it might change with economic expansions and contractions.

Our estimates likely suffer from two competing sources of endogenous variation: 1) high wage workers may be more productive and therefore likely have more outside options, and 2) high wage workers have high non-transferable firm-specific human capital. To address both of these potential biases resulting from unobserved individual heterogeneity, we include individual fixed effects in a robustness check, and find that the basic qualitative pattern in our results holds for major sub-periods in our data. However, when we analyze the data using finer sub-periods, we lack the necessary within-worker variation in wages to estimate the fixed effects models, leading us to prefer our estimates without individual fixed effects. Without individual fixed effects, the two sources of endogeneity likely bias our results in opposite directions. The results of Ransom and Sims (2010), who are able to exploit exogenous wage variation, are consistent with the first source of endogeneity dominating the second. If the size of the endogeneity bias is constant over time, then our results are informative on how the elasticity changes over the business cycle. Later, we argue that we are likely underestimating the elasticity more during expansions than during recessions because of downward nominal wage rigidities. Thus, the bias works against finding a pro-cyclical elasticity.

We find that the elasticity of supply to an individual firm is typically between 3 and 3.5 during expansions and is below 2.25 during recessions, implying that, under a simple wage setting model, firms can pay equally productive workers between 5% and 10% less during a typical recession than during an expansion. Additionally, the results suggest that the magnitude of this drop is related to the depth and persistence of a recession. Specifically, our preferred specification shows that a 10% decrease in economic activity leads to a decrease in the elasticity by 1.2 units.

Our model of wage setting relaxes the standard assumption that the elasticity of supply to the firm is finite. However, our wage setting model is still an abstraction from many of the characteristics of large industrial firms. For example, Boeringer and Piore (1971) discusses the role of on the job training and internal labor markets and their impact on the wages of workers. This implies that the wages that we observe workers earning at the firm may only be a proxy for potential earnings at the firm. However, we believe that examining the sensitivity of workers separation decisions as a function of their reported wage still informs us as to the degree of wage setting power that their current employer has. In the conclusion, we discuss how future research will try to explicitly incorporate the role of internal labor markets into dynamic monopsony analysis.

Our work also contributes to the growing literature on the identification of the elasticity of labor supply to an individual firm. First, our identification strategy extends the standard methodology by relaxing the assumption that the firm necessarily replaces all separations with recruits. This extension of previous methodology is necessary because our study covers a period with large fluctuations in employment. It also provides a more general and robust estimation strategy for future research in this area. Second, the rich firm-level data allows our empirical analysis to more consistently follow the implications of the underlying search model. Specifically, we are able to estimate separation elasticities using only voluntary separations and we document that including involuntary separations, as has been done in the past, creates bias.

2 Model and Empirical Strategy

Manning (2003) posits that labor market frictions imply that a model with upward sloping supply curves to each firm, as first described by Robinson (1933), best represents the labor market.[3] Previous work that has estimated the elasticity of labor supply to the firm has relied on Manning's (2003) extension of an insight by Card and Krueger (1995) which shows that the elasticity of labor supply to an individual firm is equal to negative two times the elasticity of separation. As Manning shows, this identity is satisfied if and only if a firm is neither expanding nor contracting in employment.

Previous work using this methodology to estimate the long run elasticity of labor supply to the firm has found estimates in the range of 1 to 10 (Boal (1995), Ransom and Oaxaca (2005), Ransom and Oaxaca (2010), Hirsch et al. (2006), Hirsch (2007), Hotchkiss and Quispe-Agnoli (2009), Ransom and Sims (2010), Hirsch et al. (2010), Falch (2010), Ransom and Lambson (2011), Falch (2011) and Dube et al. (2011)). One exception is Webber (2012), who employes the method developed in this paper which we detail below.

[3]For additional detailed summary of monopsonistic labor markets see Ransom (1993) and Boal and Ransom (1997).

2.1 Estimating Labor Elasticity of Supply to the Firm

In this section we derive a generalized relationship between the elasticity of labor supply to the firm, ϵ_{Lw}, and the elasticity of separation from the firm, ϵ_{sw}, by relaxing the assumption that the firm is neither expanding nor contracting in employment. As the goal of this paper is to understand how the elasticity of labor supply to the firm, ϵ_{Lw}, varies over the business cycle, we must consider how to calculate ϵ_{Lw} when the firm's employment levels are not constant. To the best of our knowledge, prior work has not developed an estimation strategy robust to growth or contraction in employment. We show that the researcher can identify the elasticity of labor supply to an individual firm if the researcher observes the firm's growth rate of employment, separation rate and elasticity of separation. Our result yields a straightforward relationship between ϵ_{Lw} and ϵ_{sw}.

Following Card and Krueger (1995), let $s(w)$ represent the *rate* at which workers separate from the firm and $R(w)$ represent the *number* of new recruits (workers) that are employed at the firm in a given time period. Therefore, if a firm has L_{t-1} workers last period and pays w_t, the firm's labor supply this period is

$$L_t = [1 - s(w_t)]L_{t-1} + R(w_t). \tag{1}$$

If the firm's employment is constant, $L_t = L_{t-1}$, replacing L_{t-1} for L_t in equation 1 and solving for L_t results in,

$$L(w) = \frac{R(w)}{s(w)}.$$

By taking the log of each side and differentiating with respect to w, the following equality holds (as shown by Card and Krueger (1995)):

$$\epsilon_{Lw} = \epsilon_{Rw} - \epsilon_{sw},$$

where ϵ_{Rw} is the recruitment elasticity and ϵ_{sw} is the separation elasticity. Manning (2003) shows that through a simplified version of the Burdett and Mortensen (1998) search model, $\epsilon_{sw} = -\epsilon_{Rw}$ and therefore, $\epsilon_{Lw} = -2\epsilon_{sw}$. This relationship results from separations and recruits having common job offer arrival rates and facing the same wage offer distribution in the labor market. As separations are much easier to observe in data than recruits, this result is extremely important in allowing researchers to estimate the ϵ_{Lw} under the assumption that the firm's employment level is constant.

It is often the case that firms are either expanding or contracting in employment over time. Therefore, because employment levels are not constant, multiplying the separation elasticity by negative two does not correctly estimate the ϵ_{Lw}. To relax this assumption, we introduce γ_t which is the inverse employment growth rate at time t for an individual firm. If $\gamma_t < 1$ the firm is expanding, if $\gamma_t > 1$ the firm is contracting, and if $\gamma_t = 1$ the firm's employment is constant (corresponding to the special case used in previous work). Relaxing the assumption that $\gamma_t = 1$ allows us to derive a more generalized relationship between the elasticity of labor supply to the firm, the elasticity of recruits to the firm, and the elasticity of separations from the firm.

Consider the dynamic equation relating employment this year to employment last year defined in equation 1. We relax the assumption of employment being constant between periods by substituting L_{t-1} with $\gamma_t L_t$,

$$L_t = [1 - s(w_t)]\gamma_t L_t + R(w_t).$$

Therefore, by solving for L_t, we obtain the following expression

$$L_t = \frac{R(w_t)}{1 - [1 - s(w_t)]\gamma_t}. \tag{2}$$

Taking logs and simplifying, we have

$$ln(L_t) = ln(R(w_t)) - ln(1 + s(w_t)\gamma_t - \gamma_t).$$

Finally, by differentiating the above equation and multiplying by w we have

$$\epsilon_{Lw} = \epsilon_{Rw} - \epsilon_{sw} s(w_t) \frac{\gamma_t}{1 + \gamma_t s(w_t) - \gamma_t} \tag{3}$$

The above shows that there is now a more complex relationship between supply elasticity and the separation and recruitment elasticities. We now proceed to relating ϵ_{Rw} in terms of ϵ_{sw} using the strategy outlined by Manning (2003). The elasticity of recruitment with respect to the wage can be expressed by

$$\epsilon_{Rw} = \frac{w \times R'(w)}{R(w)}. \tag{4}$$

The recruitment function, as defined by Manning (2003), is defined as

$$R(w) = \lambda \int_0^w f(x)L(x)\mathrm{d}x,$$

Where λ is the job offer arrival rate and $f(w)$ is the density wage offers. Thus,

$$R'(w) = \lambda f(w)L(w). \tag{5}$$

Therefore, using equations (4) and (5), if follows that

$$\epsilon_{Rw} = \frac{w\lambda f(w)L(w)}{R(w)}. \tag{6}$$

By combining equation (2) and (6) the elasticity or recruitment with respect to wage,

$$\epsilon_{Rw} = \frac{w\lambda f(w)}{1 + s(w)\gamma_t - \gamma_t}. \tag{7}$$

In a simplified wage posting search model, such as Burdett and Mortensen (1998), the separation rate of employees from a firm, $s(w)$, is defined by

$$s(w) = \delta + \lambda[1 - F(w)], \tag{8}$$

where δ is the exogenous job destruction rate, λ is the job offer arrival rate and $F(w)$ the distribution of wage offers. The derivative of the separation function with respect to the wage is then

$$s'(w) = -\lambda f(w). \tag{9}$$

Therefore, by combining equations (7) and (9) the elasticity of recruitment is

$$\begin{aligned}\epsilon_{Rw} &= \frac{-ws'(w)}{1-(1-s(w))\gamma_t}, \\ \epsilon_{Rw} &= -\epsilon_{sw}\frac{s(w)}{1-(1-s(w))\gamma_t}. \end{aligned} \tag{10}$$

Finally, by substituting equation (10) into equation (3), the elasticity of labor supply with respect to wage can be identified through the elasticity of separation, ϵ_{sw}, the separation rate, $s(w)$, and rate of employment expansion or contraction at the firm, γ_t. Using this adjustment, the elasticity of labor supply to the firm is

$$\epsilon_{Lw} = -(1+\gamma_t)\epsilon_{sw}\frac{s(w)}{1-(1-s(w))\gamma_t}. \tag{11}$$

This equation is consistent with the special case in Manning (2003) as the equation collapses to $\epsilon_{Lw} = -2\epsilon_{sw}$ when employment is constant over time ($\gamma_t = 1$).[4]

Equation 11 shows that an incorrect assumption of constant employment between time periods will generate a biased result. Specifically, assuming a non-downward sloping supply curve, if the firm is expanding ($\gamma_t < 1$), ϵ_{Lw} will be overestimated, and if the firm is contracting ($\gamma_t > 1$), ϵ_{Lw} will be underestimated. This is important when studying how ϵ_{Lw} changes over the business cycle in order to avoid a spurious result regarding the pro-cyclicality of the elasticity which stems simply from overestimating the the elasticity of labor supply during expansions, and underestimating the elasticity of labor supply during contractions.

2.2 Pro-cyclicality in the Labor Elasticity of Supply to the Firm

Here we show how changes in the structural parameter of the simple wage-posting search model (the job offer arrival rate) affects the elasticity of labor supply to the firm. Specifically, during an economic expansion aggregate labor demand shifts right causing a higher rate of job arrivals. This decreases frictions in the labor market and decreases an individuals attachment to a particular firm. In sum, the model suggests that ϵ_{sw} will be counter-cyclical and therefore ϵ_{Lw} will be pro-cyclical.

[4]Note that the lower bound on $s(w)$ is determined by γ_t. As $R(w) \geq 0$, then $s(w) \geq 1 - \frac{1}{\gamma_t}$.

Using equation 8, the separation elasticity is defined as

$$\epsilon_{sw} = -\frac{ws'(w)}{s(w)} = \frac{-\lambda f(w)w}{\delta + \lambda[1 - F(w)]}.$$

During an economic downturn, we would expect λ to decrease, while the reverse should hold during an expansion. The effects of changes in this parameter on the elasticity of separation is

$$\frac{\partial \epsilon_{sw}}{\partial \lambda} = -\frac{\delta f(w)w}{(\delta + \lambda[1 - F(w)])^2} \leq 0.$$

Thus, in an economic downturn, as λ decreases, the elasticities of separation and supply both approach zero. In an economic expansion, as λ increases, the elasticities of separation (ϵ_{sw}) decreases towards negative infinity, while the elasticity of labor supply to the firm (ϵ_{Lw}) increases towards positive infinity, as the two elasticities are negatively related. The limiting case is as λ approaches infinity the market becomes frictionless and perfect competition prevails. This prediction is consistent with the empirical literature demonstrating that real wages are pro-cyclical, as a standard profit maximization model shows that firms set wages at

$$w = \frac{MRP_L}{1 + \frac{1}{\epsilon_{Lw}}}.$$

This wage markdown equation is equivalent to the Lerner Index for the price markup process in output markets. Seminal papers in Empirical Industrial Organization such as Berry, Levinsohn and Pakes (1995) assume that, under imperfect competition in output markets, price setting is determined by only the marginal cost of producing the good and the responsiveness of consumers to changes in the price of the good. Here, we use the standard model of wage setting under imperfect competition in input markets: wages are set based only on the marginal revenue product of the worker at the firm and the responsiveness of workers to changes in wages at the firm.

3 Data

To test how the elasticity of labor supply to an individual firm varies over the business cycle we use employee records from Ford Motors (Whatley and Wright 1995b) in Michigan, and A.M. Byers (Whatley and Wright 1995a), a steel company in Pennsylvania. The data's principal investigators, Warren Whatley and Gavin Wright, obtained employee work history through random sampling of archived records and have made the data available online at the Inter-University Consortium for Political Social Research. Previous work has used this data to study labor relations at these two firms.

Maloney and Whatley (1995) begin to convey the idea that Ford Motors may have had potential monopsony power over its workers. Later work by Whatley and Sedo (1998) studied the quit behavior of workers at Ford Motors. Although the labor supply parameters that are the focus of our paper are not estimated, their work did recognize the potential for monopsony power. They state that, "the additional monopsony power that employers have over black workers results in poorer job matches for black workers and lower reservation utilities. A lower reservation utility reduces job search and the propensity to quit." Foote, Whatley and Wright (2003) extended the work of Whatley and Sedo (1998) and also suggested the existence of potential monopsony power.

The A.M. Byers Company was a family-owned manufacturing company that produced high-quality wrought iron tubing and pipe. The employee records used in this paper come from two of their three Pennsylvania plants: Southside Pittsburgh and Ambridge. Byers also had plants located in Clearfield, Pennsylvania and Warren and Girard, Ohio. The company incorporated in 1876 and by the late 1920s it employed the largest number of puddlers in the nation. This was largely due to the fact that most wrought-iron producers moved away from using skilled craftsman to instead employing the new low-cost Bessemer process. Workers at Byers had a long history of craft unions. In 1937 Byers signed a union contract with the Steel Workers Organizing Committee (Whatley and Wright 1995a). For a more complete history of the A.M. Byers Company see Santos (1984).

Maloney (1998) uses the A.M. Byers data to investigate differences in health-related turnover between black and white workers. He finds that black workers were often placed into more hazardous jobs but did not gain higher wages in reward for the risk. This finding, along with Maloney and Whatley (1995), Whatley and Sedo (1998) and Foote et al. (2003), suggest the importance of controlling for race and job titles in our analysis.

The data from Ford Motors was obtained in such a way that only workers who had separated from Ford by 1947 were intended to be included in the sample. Therefore, observations of workers with hire dates closer to 1947 are fundamentally different from those in earlier time periods. Observations for these workers had shorter tenure spans by construction of the sample. We limit our sample to pre-1941 data not only because of this sample selection bias, but also because Ford Motors became unionized in 1941 and the industrial landscape began to change due to the war. Observations from 1918 may have also been affected by policies related to World War I and are thus also omitted. To be consistent across datasets, we also use only data from the years 1919-1940 for the analysis of A.M. Byers.

Each worker in the original sample is identified by a unique ID. When a job characteristic such as wage or job position changes, a new job record was recorded. Included in the job record is a variable that indicates when the job ended and whether the person moved internally in the firm, such as to a new position or a new wage at the current position, or whether the move was external through quitting, being fired, being laid-off, sickness, etc.[5]

We follow an estimation approach similar to Ransom and Oaxaca (2010), who used year-end payroll data from a retail firm. We use the original employee data to create semi-annual observations of employment status, wages, and tenure. We choose semi-annual observations rather than annual observations as it provides us with finer time periods which allow us to more precisely estimate the cyclicality of the elasticity of labor supply to the firm.[6]

The employee records provided the specific job title of each employee. However, these job titles were not systematically organized. To capture the effect of wages on separation, one needs to control

[5] An employee note was attached with most employee records often giving details of why the individual quit. Examples of typical voluntary quits include: "Quit for higher pay," "Quit to go to Cadillac," and "Got another job with more money." Examples of non-typical non-voluntary separations include: "Fired b/c caused trouble (fought) - punched foreman in face" and " When he applied, he gave the wrong name - claimed it was a mistake - he couldn't write. ... On final termination notice, it said he tried to knife a man at work."

[6] Ransom and Oaxaca (2010) use yearly observations while Hotchkiss and Quispe-Agnoli (2009) use monthly observations.

for jobs or tasks that may be correlated with wages and that also affect an employee's decision to separate from the firm, for example, compensating wage differentials for working in undesirable jobs. We use the job titles from the data to create job-specific indicator variables. We found the most commonly used words to describe a job position and matched it with a corresponding indicator variable.[7]

Summary statistics for the Ford and Byers data are given in Tables 1 and 2, respectively. Each observation in the table is a semi-annual worker observation as used in the analysis section. Table 3 provides detailed information on separations and nominal wage changes at Ford and Byers. Involuntary separations were not a trivial share of total separations in any time period for either firm. The number of nominal wage changes at Ford Motors was dominated by upward changes. However, there were downward changes in wages in each period, most notably during the Great Depression. Byers, on the other hand, had a large proportion of downward wage changes in both *The Roaring Twenties* and *The Great Contraction*. However, during *The New Deal* time period, of the few wages changes that occurred almost 90% were upward.

We also obtained average and peak employment data from Ford's archive to calculate year to year employment growth rates during our period of study (Archives 1903-1972). Data on employment growth rates is needed to correctly adjust the elasticities when a firm's employment levels are not constant over time. Figure 1 shows both average and peak employment at Ford Motors over the period of study. We were unable to obtain population employment at Byers, so we instead use the available data combined with information of the sampling rate to construct a measure of total employment at the Byers plants that we study. This measure is then used to calculate the employment growth rates over time.

4 Estimation Strategy and Identification

Figures 1 and 2 show that between the year of 1919 and 1940, Ford Motors was often either expanding or contracting in employment while A.M. Byers was usually contracting.[8] Therefore,

[7]Detailed descriptions of this process and code are available upon request from the authors.
[8]The data was obtained from the Ford Motor archives through personal request (Archives 1903-1972).

to use these firms to study how the elasticity of labor supply to the firm varies over the business cycle, we must relax Manning's (2003) assumption that the firm's employment level is constant. Specifically, we apply the identification method detailed in Section 2 which shows that the elasticity of labor supply to a firm can be identified if one can identify the elasticity of separation, the probability of separation and the growth rate in employment at the firm. Here, we explain how we will estimate the separation elasticities and then solve for the labor elasticity of supply to these two firms.

4.1 Estimation Strategy

We use a linear probability model (LPM) to estimate the elasticity of separation with respect to the wage,[9]

$$s_i = \beta_0 + \beta_1 \ln(w_i) + X_i\Gamma + \mu_i, \tag{12}$$

where s_i is the binary variable indicating that individual i separated from their job by voluntarily quitting. w_i is the wage of individual i and the vector X_i represents other observable variables for each individual that affect the separation decision. X_i varies by specification and includes subsets of age, job tenure, race, marital status, job, plant, and year fixed effects, as described below. μ_i is a vector of unobservables. To flexibly control for heterogenous effects over different levels of tenure at the firm, we include a fourth degree polynomial of tenure. We also include a square term of age.

We estimate six specifications of the LPM. The first specification is an unconditional regression. The second includes only year fixed effects, which control for common shocks over the year, such as the price level and the general level of labor demand in the economy. In the third specification, we control for individual covariates. Controlling for actual experience is important, as we are trying to isolate the effect of wages on the decision to separate. Without controlling for this, the true effect of wages on separation is confounded by more tenured workers both receiving higher wages and being more attached to the firm. The fourth and fifth specifications include additional fixed effects

[9] We also estimated the elasticities with a Probit model and found similar results. The choice to use the LPM over the Probit model mainly resulted from the ease in bootstrapping the standard errors. The Probit specification is much more sensitive than the LPM specification when bootstrapping over smaller samples. However, point estimates and statistical significance were similar between the two models when there was a relatively large sample.

for the plant at which the worker was employed and the worker's job category, respectively. Plant fixed effects control for working conditions, which varied across plants (Foote et al. 2003). The job categories are used to control for unobserved tasks that were required of workers. Controlling for job categories and race is important in light of the evidence in Foote et al. (2003) and Maloney (1998) showing that black workers were often placed into more dangerous and less desirable jobs than were white workers. The sixth and final specification, which is used as an robustness check, includes three "group" variables. Specifically, we include the average wage of an individual's starting cohort, the average wage for workers at an individual's plant and the average wage of workers in the same job category as the individual. These "group" controls act as proxies for unobserved outside labor demand for workers with similar unobservable characteristics as the worker. If there is a large amount of endogenous wage variation remaining in our fifth specification, we would likely see that the estimated coefficient on log wage is sensitive to the inclusion of these variables.

The separation elasticity for each individual i is calculated from the linear regression model in the following way,

$$\hat{\epsilon}_{s,i} = \frac{\partial \hat{s}_i}{\partial w_i}\frac{w_i}{\hat{s}_i} = \frac{\hat{\beta}_1}{\hat{\beta}_0 + \hat{\beta}_1 \ln(w_i) + X_i\hat{\Gamma}}. \tag{13}$$

A point estimate of ϵ_{Lw} was calculated by first solving for the average separation elasticity, $\bar{\epsilon}_{sw} = \frac{1}{N}\sum_i \hat{\epsilon}_{sw,i}$, then applying equation 11 from Section 2. Period specific γ_t's were obtained from the year level data seen in the Figures 1 and 2.[10]

Given the different economic conditions over the time period of study, we begin our analysis of the variation in ϵ_{Lw} by estimating the value of the parameter for each of the following three major sub-periods: 1) 1919 through the first half of 1929 (*"The Roaring Twenties"*), 2) the end of 1929 through the first half of 1933 (*"The Great Contraction"*), and 3) the period from the second half of 1933 through 1940 (*"The New Deal"*). We then turn our attention to the NBER defined expansions and contractions during our period of study. No other 22-year period in the United States during the 20th century observed such large and frequent fluctuations in the business cycle. We partition

[10] In practice, γ_t could be computed using either the change between $t-1$ and t or the change between t and $t+1$. Because the choice of using next or previous period is arbitrary, we choose to use an average of both. Specifically, we find the ratio of employment between the pervious and succeeding calendar year and take the quartic root of this to obtain the semi-annual change over the period.

the data into 11 sub-periods for each expansion (6) and contraction (5) in the period of study. We then estimate the elasticity of supply to the firm for each sub-period.[11]

4.2 Identification

Identifying the elasticity of separation is the first step in the identification of ϵ_{Lw}. To obtain a consistent point estimate the elasticity of separation, we must have variation in wages which is independent from other unobserved factors that affect the probability of separating. One limitation to our study is that we lack an instrumental variable to satisfy this condition. Ransom and Sims (2010) are able to identify the elasticity of separation by instrumenting actual salaries with base-salaries for school teachers in Missouri. Similarly, Falch (2011) was able to exploit an exogenous wage change for a subset of school teachers. Ransom and Sims (2010) find that without instrumental variables, the elasticity of separation is positively biased and as a result the labor elasticity of supply to the firm is negatively biased. Other recent papers in this literature that have analyzed private sector data, like ours, have not been able to find a valid instrument to overcome similar endogeneity issues. There is an economic justification for why there should be some random residual variation in wages. Our data consists of workers who were hired at different points of time and therefore are earning different wages than otherwise similar workers in cohorts hired before or after. Our wage variation comes from different workers being paid different wages after conditioning on job title, plant location, year, tenure, and other demographic information.

We consider two main sources of endogenous variation in wages. First, there may exist characteristics of high productivity workers which are observable to firms but unobservable to the econometrician. Therefore, these workers are likely to be paid a higher wage and are likely to receive more outside offers than lower productivity workers. The higher the level of outside labor demand the more problematic this source of endogenous variation will be. This would cause the estimate on log wage to be biased upwards, away from negative infinity. Second, workers may earn high wages because they have large amounts of non-transferable firm-specific human capital, caus-

[11]Firms often adjust both the intensive margin of employment as well as the extensive margin of employment over the business cycle. Our estimates strictly deal with the extensive margin. Our prior of how the elasticity would vary over the business cycle through the intensive margin in not clear.

ing these workers to have lower separation rates. To the extent that flexibly conditioning on tenure does not purge this endogenous variation, the result would be a downward bias in the coefficient estimate. We believe that the first source of endogenous variation is dominant. Flexibly controlling for tenure and job category will significantly reduce the bias resulting from firm-specific human capital. Also consistent with the first source of endogeneity being dominant is the empirical evidence from Ransom and Sims (2010) which show that the least squares estimates of the responsiveness in separations to wages are positively biased when contrasted to the estimates from two stage least squares estimates.

Additionally, it is unclear that the magnitude of the effect of the second source of endogenous variation changes over the business cycle. However, the first source of endogenous variation is fundamentally linked to the business cycle. Particularly, during economic expansions there will be higher outside labor demand and thus the estimates will be more biased than during recessions when there is less outside labor demand. Given that we are interested in how these estimates change over the business cycle, it is important that we understand how the magnitude of the bias will change during expansions and contractions. Following the linear specification described above, suppose μ_i can be decomposed as $\tau y_i + \xi_i$. Therefore, the equation of interest is,

$$s_i = \beta_0 + \beta_1 \ln(w_i) + X_i\Gamma + \tau y_i + \xi_i,$$

where y_i represents outside labor demand, specific to individual i, that is correlated with $\ln(w_i)$ and ξ_i represents a vector of unobserved characteristics such that $E[\xi_i | ln(w_i), X_i, y_i] = 0$. It follows that

$$\text{plim } \hat{\beta}_1 = \beta_1 + \tau\theta,$$

where θ is the coefficient on $\ln(w)$ in the population regression of the omitted variable, y, on $\ln(w)$ and X. Given that firms may positively adjust wages during high outside labor demand, θ is positive. Likewise, τ is believed to be positive as positive market level labor demand shocks increase the job offer arrival rate, λ, and therefore the probability of separating from the firm. With $\tau\theta > 0$, the estimated coefficient on $\ln(w)$, $\hat{\beta}_1$, is biased in a positive direction. Therefore,

ϵ_{sw} would also be biased in a positive direction, which would cause ϵ_{Lw} to be biased in a negative direction.

It is unlikely that θ is constant over the business cycle. In Table 3 we see both increases and decreases in wages. However, we see a higher rate of wage increases during expansionary periods than wage decreases during contractionary periods. Table 3 is consistent with an economy in which downward nominal wage rigidities are stronger than upward nominal wage rigidities. This implies that the correlation between wages and outside labor demand (θ) is stronger in an expansion than in a contraction. Thus, there will be a greater bias in our estimated coefficient (and in our elasticity estimate as well) in expansions than in contractions.

Because the bias in the separation elasticity will be greater during an expansion than during a contraction, the difference between our estimated elasticity of separation during an expansion and our estimated elasticity of separation during a contraction will be *less* than the difference between the true value of the elasticity of separation during an expansion and the true value of the elasticity of separation during a contraction. The same argument holds for the elasticity of supply to the firm. For example, suppose that the true value of the elasticity of supply to the firm during a recession is 1, and it is underestimated by a value of .75, yielding an estimate of .25. Suppose that the true value of the elasticity of supply to the firm during an expansion is 3, and that it is underestimated by a value of 1, yielding an estimated value of 2. Therefore, the difference in the estimated values is only 1.75, despite the fact that the difference in the true values of the elasticities is 2. It should be clear that we *underestimate* the difference in the parameter over the business cycle, and a comparison of our expansionary and contractionary estimates should provide a lower bound of how pro-cyclical the parameter is.

4.3 Estimation Bias from Involuntary Separations

The underlying idea in the search framework is that employers will set wages higher when they know that individuals will be able to voluntarily separate because they receive a higher outside offer (Manning 2003). Therefore, identification of ϵ_{Lw} should come from seeing how wages and *voluntary* separations covary. The data used in previous empirical work has not specified the reason for separation, creating a measurement error problem. Our data specifies whether the separation was a voluntary quit or a forced lay-off or firing (involuntary separation). Therefore, we are in the position to eliminate and assess the potential bias due to the misclassification of reason for separating.

The bias is not straightforwardly signed when both voluntary and involuntary separations are used to estimate the population parameter on $\ln(w)$, β. In the extreme case, suppose that involuntary separations, s_i', are orthogonal to log wages. Therefore, the estimate of β, using both voluntary and involuntary separations, is biased towards zero as it is a weighted average of zero and β. However, it is not clear that log wages are orthogonal to involuntary separations. One possibility is that log wages and involuntary separations are negatively correlated even after controlling for X_i. We address the direction of this bias empirically in the next section by comparing point estimates of the elasticity of supply to the firm under inclusion and exclusion of involuntary quits. Furthermore, we separately analyze the correlation between wages and fires and layoffs.

5 Results and Discussion

5.1 LPM and Elasticity Estimates

Results from a set of linear probability model estimations using all years of data for Ford Motors and A.M. Byers are presented in Tables 4 and 5, respectively. The six specifications are described in Section 4. In each estimation, we find a negative and statistically significant coefficient on log wage. The relatively small magnitudes of these coefficients suggests a finite elasticity of labor supply to each of these firms. In the third specification, we control for individual covariates. The linear tenure term is always negative and significant, while age is positively correlated with separations. This may indicate the importance of job specific human capital in the separation decision. The significance of this term and its effect on the point estimate of log wage indicates the importance of considering the impact of internal labor markets and attempting to control for their impact by flexibly conditioning on tenure. The coefficient on an indicator variable for African-American workers is negative and always significant at Ford, consistent with Foote et al. (2003), but insignificant at Byers. The fourth and fifth specifications, which include additional fixed effects for the plant at which the worker was employed and the worker's job category, respectively, produce results similar to the third specification, and suggest stability in our results after conditioning on key demographic variables. In our sixth and final specification, as a robustness check, we include the three "group" variables. The coefficient on log wage is relatively invariant to the inclusion of these additional controls, suggesting that our previous estimates were not significantly biased.

In Table 6, we report the elasticity of labor supply to each firm (ϵ_{Lw}) using the coefficients from specification five and the elasticity estimation strategy outlined in Sections 2 and 4, incorporating γ_t. The standard errors on the elasticity estimates are obtained by bootstrapping with 5000 replications.[12] Estimates of the elasticity obtained from pooling all years range from 2.94 to 3.88 for Ford, and 1.38 to 3.09 for Byers. These estimates are in the range of results found in the prior literature. It is interesting to note that, despite being a relatively larger firm relative to the labor market that it competes in, the elasticity estimates for Ford are generally higher than those for Byers. This is indicative of finite elasticities coming through frictions (new monopsony) rather than just through size of the firm (traditional monopsony).

The estimates from three major sub-periods are also reported in Table 6.[13] Across all specifications we note a similar pattern. First, the estimate of the elasticity of labor supply to the firm in *The Roaring Twenties* is similar in magnitude to the estimate in the pooled sample (this is likely because a large fraction of our total observations come from those years). Second, the estimate of the elasticity of labor supply falls sharply during *The Great Contraction*. Given the large decrease in labor demand during this period,[14] this result is not particularly surprising and is consistent

[12] To obtain the bootstrapped standard errors, we used the bootstrapping package provided by Stata. This package called a program from which an LPM was run and the supply elasticity was calculated post-estimation. We then found the standard deviation of the distribution of all 5000 estimates of the elasticity provided, including from replications in which not all parameters or their standard could be estimated by the package. We presume that some parameters could not be estimated in every bootstrapped sub-sample on account of the high number of fixed effects in the full model combined with the relatively small sample size in some of the sub-periods.

[13] In the appendix, tables 1 through 16 report the estimates for all six specifications of the linear probability model in each of the three major sub-periods as well as the entire period as a whole. Results for each time period are reported for both Ford and Byers. Finally each company/time period set of estimations is run both including and excluding involuntary quits. We discuss the effect of including involuntary quits on the estimates in more detail in a later subsection.

[14] Bresnahan and Raff (1991), show that from the peak of 1929 to the trough of 1933, half of the U.S. auto plants shut down and one third of U.S. manufacturing establishments were closed.

with the comparative statics derived above for the simple wage posting model. During *The Great Contraction* the elasticity estimates are negative but not significantly different from zero once we condition on observable characteristics of the individual. Thus we cannot reject a null hypothesis of perfectly inelastic labor supply to the firm during this period. It should be noted that, as shown in Table 3, a much larger share of wage changes during this sub-period were downward rather that upward, as compared to other sub-periods. Downward wage changes are more problematic in this study because it may be the case that workers quit preemptively in response to announced future wage cuts. This creates a positive spurious correlation between wage and separations and thus may account for our negative supply elasticity estimates.[15] Elasticity estimates during *The New Deal* time period were slightly higher than during *The Roaring Twenties* for the fifth and sixth specifications for both firms. This suggests that policies enacted during *The New Deal* may have had an impact on labor market frictions.[16]

The results in Table 6 are prima facia evidence of the pro-cyclicality of the elasticity of labor supply to the firm: the elasticity plummeted as the country sank into the Great Depression, and then increased in value as the economy began to recover. A cleaner test for the pro-cyclicality of the elasticity requires a sharper definition of the state of the economy. Therefore, we turn to NBER business cycle data that defines peak and trough dates of the business cycle throughout the 1920s and 1930s in order to directly test whether recessions decrease the elasticity of labor supply to the firm. Figure 3 presents the 11 NBER defined sub-periods and the estimates of the elasticity of labor supply to each firm in the sub-period using our preferred fifth specification.[17] We define

[15]While we would expect that the economic conditions during this period would create very inelastic labor supply, we also believe that our estimates tend to be biased downwards, for reasons noted previously. Ransom and Sims (2010) find that estimates of the elasticity are biased downward prior to implementing instrumental variables. This is consistent with Falch (2010) who also finds downward omitted variables bias.

[16]The Fair Labor Standards Act of 1938 set a national minimum wage and overtime requirements. This likely changed the wage offer distribution. The Federal Emergency Relief Administration, the Civil Works Administration, and the Works Progress Administration, all created under the New Deal, offered government sponsored work relief on a large scale. These programs would have the effect of increasing the job arrival rate. Social insurance programs were created through the Social Security Act of 1935. Such reform likely impacted the reservation wage of workers. The Wagner Act of 1935 expanded employee collective bargaining rights and as a result union membership increased. Specifically, Ford did not unionize until 1941, while General Motors and Chrysler unionized during the mid 1930s. As union contracts increased wages significantly (we see this in our data from Ford when comparing 1940 and 1941), this likely shifted the wage offer distribution to the right for Ford workers (as GM and Chrysler's wages increased more quickly than Ford's.) See Fishback (2008) for a more detailed summary of New Deal policies.

[17]We chose Specification 5 as our preferred specification because of the large set of controls. However, the results are robust to any number of the later specifications.

a semi-annual period as being part of an expansion/recession if more than half of the period was in an NBER defined expansion/recession. For each firm, six blue dots represent estimates from expansionary periods, while five red dots represent estimates from recessionary periods. The size of each dot is proportional to the number of periods represented. For both firms, we see a general pattern of the elasticity estimates being higher during expansions than during recessions.[18] These estimation results are also presented in Tables 7 and 8 for Ford and Byers, respectively. The arrows indicate the direction of economic growth in that period (up for expansions and down for recession). The tables also display the potential wage markdown, the proportion of MRP_L the firm pays in wages for the given estimated elasticity. As a result of the elasticities being lower in recession than expansions, we see that the markdown is higher in recessions than expansions.[19]

If unobserved match or job specific human capital plays a significant role at Ford or Byers then one would expect that workers who are a better match earn higher wages and are thus less likely to separate. This may cause the LPM coefficient on log wage to be biased towards negative infinity. Without controls for individual heterogeneity, we are not able to disentangle this endogeneity issue from the endogeneity resulting from higher outside labor demand shocks, which bias the results in the opposite direction (as discussed in the previous section). To explore the degree of this potential bias we include an individual fixed effect in the estimation to control for individual heterogeneity.

Results from the LPM regressions including the individual fixed effect are reported in the seventh Column of Table 9. The first six Columns report the coefficient on log wage. The estimates from the first row can also be found in Tables 4 and 5. The estimates from the bottom three rows can be found in the Appendix. Column 8 presents the standard deviation of wage when each observation is treated independently and Column 9 presents the average within worker standard deviation of wage for workers we observe more than once in the data. As shown by comparing Columns 8 and 9 only a small share of total variation is within worker. Therefore, the fixed effects model is not capable of precisely estimating these coefficients and are preferred specification remains specification 5.

[18]Figure 1 in the Appendix is similar to Figure 3 but the elasticities are estimated using the assumption that employment is constant, $\epsilon_{Lw} = -2\epsilon_{sw}$. These *unadjusted* elasticity estimates overemphasize the pro-cyclicality of the parameter.

[19]The potential markdown is defined as $\frac{MRP_L - w}{MRP_L}$ and was equated through the identity that the rate of potential employee exploitation is equal to the inverse of ϵ_{Lw}.

However, it is important to note that unobserved match or job specific human capital seem to play a limited role. Specifically, at Ford Motors during the 1919(I) to 1929(I) sub-period, which has a modest amount of within variation, shows very similar point estimates to those presented in columns 5 and 6. At Byers, we see the point estimate becomes more negative in this time period, suggesting that unobserved individual heterogeneity played a more important role. Nevertheless, Column 7 suggests that workers are more responsive to wages during the *Roaring Twenties* than the *Great Contraction*.[20]

5.2 Formal Test of Cyclicality

In Table 10 we show results from a series of tests of the null hypothesis that the elasticity estimates presented in Tables 7 and 8 are independent of the business cycle. We test this by running regressions of the pooled elasticity estimates from the two firms on two measures of macroeconomic activity. The top panel reports estimates from regressions of the elasticity estimates on an indicator variable equal to zero for an expansionary period and equal to one for a recessionary period. The bottom panel reports estimates from regressions of the elasticity estimates on a variable equal to zero for an expansionary period and equal to the percentage decrease in output for a recessionary period.[21] The columns denote the different specifications as discussed above. The rows report different weights used in the regressions. The six weighting strategies are as follows: (1) no weights, (2) length of the expansion/recession, (3) inverse of the standard error on the point estimate of the elasticity in the expansion/recession, (4) number of individual workers observed in the expansion/recession, (5) the product of (2) and (3), and (6) the product of (2) and (5). Including information on the length of the expansion/recession is important if the duration or persistence of that period of the business cycle affects changes in the parameter; it also allows us to reflect fact that longer expansions/recessions are more representative of the entire period of our study than are shorter sub-periods. Including information on the number of observations or the standard error of

[20]We note the positive and relatively large coefficient during the *Great Contraction* for Ford. Recall from above that we are concerned that worker quits to preempt expected wage changes could have created positive bias during this sub-period. In our fixed effects estimation, where identification is coming solely off of within worker variation, we would expect this bias to be even larger.

[21]This measure is drawn from data compiled at http://en.wikipedia.org/wiki/List_of_recessions_in_the_United_States.

the estimates takes into account the precisions of the estimates being used.

Estimates derived from the preferred fifth specification are always significant at the 10% level when weights are applied in the regression. Furthermore, nine of the ten are significant at the 5% level and six of the ten estimates are significant at the 1%. The coefficient on the recession indicator variable is always negative, suggesting a smaller elasticity during recessions. For the preferred specification the coefficient for weighted regressions are between -2.03 and -3.26. The average elasticity during an expansion was 3.26. Thus, the results suggest that an average recession decreased the elasticity from a relatively competitive level to a value which indicates firms having a large degree of monopsony power in these labor markets during recessions. The coefficient on the Output Gap measure shows the effect of the severity of a recession on the elasticity estimate. Specifically, the preferred specification with weights shows that a 10% decrease in output, the average level of decline for recessions in the inter-war period, resulted in a decrease in the value of the estimated elasticity between .792 (weighting by $\frac{1}{St.Error}$) and 1.24 (weighting by $\frac{Periods}{St.Error}$).[22]

5.3 Discussion and Robustness Checks

5.3.1 Employment Adjustment and Voluntary Quits

Here we address the importance of adjusting for changes in the employment level over time and using only voluntary quits to estimate ϵ_{Lw}. To examine the potential bias we 1) re-estimated the model without adjusting for changes in the employment level and using only voluntary separations, and 2) re-estimated the model adjusting for employment changes but including both voluntary and involuntary separations in the analysis. Both types of specification error impact the estimates of ϵ_{Lw}.

For a firm facing an upward sloping labor supply curve, an expansion in employment will cause the traditional approach of setting $\epsilon_{Lw} = -2\epsilon_{sw}$ to overestimate ϵ_{Lw}. The converse holds for

[22]In the Appendix, Table 17 and Table 18 report results for alternative treatments of the left hand side variable for these estimations. In Table 17, we replace the theoretically impossible negative elasticity estimates with zeros, to reduce the likelihood that outliers are driving our results. In Table 18, we obtain the elasticity of supply by simply multiplying our separation elasticities by negative two. Without the γ_t based adjustment, we tend to overstate the pro-cyclicality of the elasticity. In both tables, the same general pattern of pro-cyclicality holds, though the results are not always significant. The results discussed are from estimates pooling pooling the point estimates of both firms. We ran separate regressions for each firm and found very similar coefficients across firms for the weighted regressions.

decreases in employment. In Table 11 we report elasticity estimates from our preferred specification in the right column ("Adjusted Quits"), along with two alternative specifications. In the middle column ("Unadjusted Quits") we impose the assumption that the firm is neither expanding nor contracting in employment (but continue to use only voluntary separations). In the left column ("Adjusted All Separations"), we use the γ_t adjustment, but also include involuntary separations in the analysis. During Ford's employment expansion of the *Roaring Twenties* period the unadjusted elasticity estimate is 3.98 at Ford while the adjusted estimate is 3.46, which implies that not adjusting for employment growth would cause us to overestimate the elasticity by 17%. At Byers, employment shrank during the same period, as seen in Figure 2.[23] For Byers, the unadjusted elasticity is 1.48, while the adjusted is 1.57, implying that non-adjustment for employment changes would cause us to underestimate the elasticity by 6%. During the *New Deal* period, both Byers and Ford were expanding. Ignoring the γ_t adjustment would lead us to overestimate the elasticities by 18% and 11% at Ford and Byers, respectively. Things are more complicated during the *Great Contraction* as the point estimates of the elasticities, though not statistically different from zero, are not consistent with an upward sloping supply curve.

Table 3 shows the number of observed separations that are not voluntary. During the whole period of study, involuntary separations accounted for around 23% of total separations at Ford and 46% at Byers. Notably, layoffs and firings were an even larger share of total separations during the two sub-periods in the Great Depression. Figures 4 and 5 show voluntary and involuntary separation rates over the entire period of study. Voluntary separations appear to be pro-cyclical and involuntary separations appear to be counter-cyclical. The potential bias from including involuntary separations in the analysis can be found by comparing the estimates from the column labeled "Adjusted All Separations" in Table 11 to the preferred estimates, "Adjusted Quits." Adding involuntary separations to voluntary quits leads to lower estimates of the elasticity of labor supply in all three sub-periods at Ford and in the first and third sub-period at Byers. Previous work that has identified ϵ_{Lw} by using all separations may have underestimated ϵ_{Lw} if involuntary separations occurred with high frequency. To further analyze these involuntary separations, we present the

[23] During our entire period of study, the trend of employment at Byers was downward. This could be due to the fact that Byers was not quick in adopting the newer and more cost effective Bessemer Process as their technology.

results from six regressions in Tables 12 and 13. The estimates cover our entire period of study, and the specification is identical to the preferred LPM specification described earlier, which also include year, job category and plant fixed effects. Lower wage workers were over-represented in layoffs and fires. Furthermore, Ford does not appear to discriminate in laying-off low wage workers while the results from Byers shows that low wage workers were much more likely to be laid-off.

5.3.2 Efficiency Wages at Ford Motors and Separation

Ford Motors is commonly described as having paid an efficiency wage. Here we address how this wage policy may affect our estimates. Raff (1986) and Raff and Summers (1987) establish that Ford did pay higher than market wages around 1914. However, Rae (1965) finds that in the mid to late 1920s all auto companies were paying wages that were nearly 40% greater than firms in other manufacturing industries. Using the 1940 U.S. Census (Ruggles, Alexander, Genadek, Goeken, Schroeder and Sobek 2010) and the Ford employee data from 1939 and 1940, we find that Ford's wages were 17.6% higher than other durable good manufacturing wages in Michigan, even after controlling for age and race. The underlying model suggests that workers are voluntarily separating to take higher paying jobs. Therefore, the estimates in the paper are capturing information about the labor market that Ford is competing in.

Table 3 shows that during the sub-periods of study, the semi-annual ratio of voluntary quits to non-separations ranged from .13 during *The Great Contraction* to .35 during *The Roaring Twenties* at Ford Motors. Therefore, if Ford had been paying an efficiency wage, the size of the efficiency wage was not large enough to stop high voluntary turnover. Importantly, the identification strategy relies on estimating voluntary separations on log wages. Therefore, as long as there is sufficient variation in log wages and observed voluntary separations, one can estimate ϵ_{Lw}. Furthermore, our focus is on how this parameter changed over this period of time. Thus, our results about the changes in the elasticity over time should be informative, even if our point estimates from Ford are not representative of the typical firm.

5.4 Potential Wage Mark Downs, Pro-cyclical Real Wages and Jobless Recoveries

In a standard wage setting model, a firm facing a finite elasticity of labor supply is able to pay a wage less than the marginal revenue product of labor. Therefore, given the pro-cyclicality of the elasticity of labor supply, the negative effects of a recession can be mitigated through lower labor costs. Table 11 reports the potential wage markdown for the entire time period as well as the three major sub-periods (Tables 7 and 8 display the markdown for each of the NBER defined expansions/recessions). While we provide no direct evidence that Ford or Byers exploited the finite elastic labor supply by actually paying workers less than their MRP_L. However, these results suggest that during *The Roaring Twenties*, the potential markdown on wages was 23% and 39% for Ford and Byers, respectively. We cannot reject a perfectly inelastic supply of labor to either firm during *The Great Contraction*, suggesting that these firms could have potentially paid workers only their reservation wages. During *The New Deal* sub-period the potential markdown was 16% and 32% respectively.

The relationship between real wages and the business cycle has been studied in a number of settings, including in the context of price markups in noncompetitive *output* markets (Abraham and Haltiwanger 1995). However, to our knowledge, the relationship has not yet been studied in a setting with noncompetitive *input* markets, where firms can pay below the perfectly competitive wage by virtue of a finite ϵ_{Lw}. A pro-cyclical ϵ_{Lw} allows for firms to mark down wages more during periods of economic downturns and forces firms to pay more competitive wages during economic growth. Our results provide an additional explanation for pro-cyclical real wages. Bils (1985) shows convincing evidence that real wages are pro-cyclical by using disaggregated panel data. Beaudry and DiNardo (1991) develops a contract model to understand how labor market conditions affect real wages. Under the condition that mobility between firms is costly (labor market frictions), the model predicts that the unemployment rate at the time of hire and the individual's entered into contract wage to be negatively correlated. This is consistent with the monopsonistic outcome that labor market frictions can grant firms the ability to pay workers less than their marginal revenue product of labor. Solon, Whately and Stevens (1997) use the Ford Motor Employee dataset to explain why empirical evidence suggests that real wages are pro-cyclical. While we focus on ϵ_{Lw} as a potential cause, they show that intra-firm mobility can also explain the pro-cyclicality of real wages for individuals who do not separate from the firm.

Our results may also be informative in helping to better understand the phenomenon of jobless recoveries. Over most of the 20th Century, Okun's law seemed to hold: there was a fixed relationship between output gaps and deviations from full employment. However, this relationship did not seem to hold after the 1991 and 2001 recessions (Knotek and II 2007). One possible explanation for this is that firm wage setting power had increased since previous business cycles. Work by Erickson and Mitchell (2007) connects monopsonistic labor markets to jobless recoveries by suggesting that monopsonistic labor markets result an inefficiently low number of workers being hired as the economy expands. For any given increase in labor demand, an upward sloping labor supply curve (and the correspondingly steeper marginal cost of employment curve) will yield a smaller increase in workers hired than would a perfectly elastic labor supply curve. Hence, the more wage setting power, the more likely there will be a lag in employment growth after output starts to grow. While our finite elasticity results are consistent with this, our finding of a pro-cyclical elasticity somewhat mitigates the jobless recovery implication of imperfectly competitive labor markets. This happens because as labor demand shifts to the right for all firms during a general economic recovery, labor supply should flatten as well, decreasing the cost of hiring more workers.

6 Conclusion

In a standard wage setting model, the wage is a function of a worker's marginal revenue product and the worker's elasticity of labor supply to the firm. Search models suggest that this elasticity is finite and recent empirical work corroborates these theoretical predictions. This relatively recent body of work stands in contrast to what has classically be assumed in the field of labor economics and can help to explain important unanswered questions within the field. Specifically, we address how the elasticity of labor supply to the firm varies over the business cycle, which can help to explain the pro-cyclicality of real wages and as well as jobless recoveries.

Our results suggest that both A.M. Byers and Ford Motors competed in labor markets with significant amounts of wage setting power. Additionally, we find that the degree of this wage setting power (elasticity of labor supply to the firm) varies counter-cyclically (pro-cyclically). Our results suggest that the average elasticity is approximately one unit lower in a recession than in an expansion. This is consistent with a firm having the ability to pay equally productive workers 10 percent less in a recession than an expansion. Although we do not have an exogenous source of wage variation to point identify the elasticities, Ransom and Sims (2010) and the existence of downward nominal wage rigidities suggest that we are likely underestimating the cyclicality of the elasticity.

We also present two identification related contributions. Prior work estimating the elasticity of labor supply to the firm has relied on the assumption that a firm replaces all separations with recruits. We relax this assumption by deriving a generalized estimation strategy that can be applied to data sets where the firm's employment level is not constant over time. We believe that this contribution will aid future researchers working on this topic. Second, to our knowledge, we are the first to estimate the elasticity of labor supply to the firm using only voluntary quits. We find that the inclusion of involuntary separations in the estimation of the elasticity of supply to the firm can significantly bias the point estimates.

As we acknowledge, all work in the "New Monopsony" framework assumes the existence of a very simple wage setting process. Specifically, firms post wages are based on only the productivity of workers and their ability to mark down these wages, as summarized by the value of the elasticity of labor supply to the firm. Relaxing the assumption that the elasticity of labor supply to the firm is finite represents a large leap forward in labor economics by generating a model that is both more representative of how labor markets actually work and also more informative to unanswered questions in the field of labor economics. However, this literature has only advanced off the standard neo-classical model in one dimension. For decades, Personnel Economics has opened the black box of firms' wages policies and brought to light important features in the wage setting process such as the efficiency wage, behavioral factors such as motivation and moral, and the role of internal labor markets. These contributions also represent an important improvement from a simple neo-classical model of wages. Future work should try to unify these models in order to develop a model best suited to explain the why different workers are paid different wages than either of these areas alone can. Specifically, we plan in future research to explore estimating how the possibility of promotion through internal labor markets can be viewed as an option; value of remaining in employment at a given firm, and how this generalization affects estimates of the elasticity of labor supply to the firm.

Table 1: Ford: Summary Statistics

	Mean	**SD**	**Min**	**Max**	**N**
1919(I)-1940(II) (Whole Period)					
Separations	0.222	0.416	0.000	1.000	6979
Wage	0.799	0.143	0.130	2.030	6979
Age	31.201	7.413	15.000	57.500	6967
Tenure	2.413	2.935	0.000	21.000	6967
Married	0.535	0.499	0.000	1.000	6967
Black	0.077	0.266	0.000	1.000	6967
1919(I)-1929(I) (*The Roaring Twenties*)					
Separations	0.257	0.437	0.000	1.000	5255
Wage	0.788	0.139	0.300	2.030	5255
Age	30.567	6.644	16.500	57.500	5243
Tenure	1.802	2.043	0.000	14.000	5243
Married	0.527	0.499	0.000	1.000	5243
Black	0.072	0.258	0.000	1.000	5243
1929(II)-1933(I) (*The Great Contraction*)					
Separations	0.113	0.317	0.000	1.000	820
Wage	0.878	0.169	0.130	1.630	820
Age	34.659	7.955	18.000	51.000	820
Tenure	4.273	3.438	0.000	14.500	820
Married	0.632	0.483	0.000	1.000	820
Black	0.066	0.248	0.000	1.000	820
1933(II)-1940(II) (*The New Deal*)					
Separations	0.123	0.328	0.000	1.000	904
Wage	0.797	0.119	0.250	1.150	904
Age	31.744	9.838	15.000	57.500	904
Tenure	4.268	4.769	0.000	21.000	904
Married	0.494	0.500	0.000	1.000	904
Black	0.115	0.319	0.000	1.000	904

Table 2: Byers: Summary Statistics

	Mean	**SD**	**Min**	**Max**	**N**
1919(I)-1940(II) (Whole Period)					
Separations	0.160	0.367	0.000	1.000	8909
Wage	0.480	0.133	0.220	1.285	8909
Age	32.311	11.028	13.000	71.500	8891
Tenure	7.255	7.784	0.000	48.500	8891
Married	0.613	0.487	0.000	1.000	8891
Black	0.165	0.371	0.000	1.000	8891
1919(I)-1929(I) (*The Roaring Twenties*)					
Separations	0.223	0.416	0.000	1.000	5563
Wage	0.445	0.096	0.220	1.120	5563
Age	31.737	11.016	13.000	71.500	5553
Tenure	5.505	6.525	0.000	45.500	5553
Married	0.599	0.490	0.000	1.000	5553
Black	0.221	0.415	0.000	1.000	5553
1929(II)-1933(I) (*The Great Contraction*)					
Separations	0.057	0.232	0.000	1.000	1475
Wage	0.478	0.130	0.290	1.250	1475
Age	33.559	10.922	15.000	65.500	1473
Tenure	8.385	7.935	0.000	41.000	1473
Married	0.670	0.470	0.000	1.000	1473
Black	0.095	0.293	0.000	1.000	1473
1933(II)-1940(II) (*The New Deal*)					
Separations	0.055	0.228	0.000	1.000	1871
Wage	0.587	0.168	0.310	1.285	1871
Age	33.038	11.029	14.000	70.500	1865
Tenure	11.571	9.148	0.000	48.500	1865
Married	0.611	0.488	0.000	1.000	1865
Black	0.052	0.222	0.000	1.000	1865

Table 3: Summary of Separations and Wage Changes

	1919(I) - 1940(II)	**1919(I) - 1929(I)**	**1929(II) - 1933(I)**	**1933(II) - 1940(II)**
		Ford		
# **Stays**	5422	3902	727	793
# **Quits**	1545	1341	93	111
# **Fires**	193	163	20	10
# **Laid Off**	260	32	148	80
Wage Δ Per Worker	1.34	1.13	1.28	1.87
Wage Δ Up	0.89	0.92	0.65	0.93
Wage Δ Down	0.11	0.08	0.35	0.07
Avg. Days Between Δ	152.41	150.65	169.89	174.97
		Byers		
# **Stays**	7468	4316	1389	1763
# **Quits**	1423	1237	84	102
# **Fires**	157	132	20	5
# **Laid Off**	1079	765	229	85
Wage Δ Per Worker	1.75	1.95	1.05	0.55
Wage Δ Up	0.57	0.57	0.35	0.89
Wage Δ Down	0.43	0.43	0.65	0.11
Avg. Days Between Δ	248.06	143.34	392.63	618.46

[a] The unit of observation for "# Stays," "# Quits," "# Fires" and "# Laid Off" are at the semi-annual cross worker level. The unit of observation for "Wage Δ Per Worker," "Wage Δ Up," "Wage Δ Down" and "Avg. Days Between Δ" are at the worker level.

Table 4: LPM Estimates From Ford Years 1919(I) through 1940(II) (vol quits only)

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Log Wage	-0.4444	-0.4739	-0.3572	-0.3809	-0.3878	-0.3978
	(0.03406)	(0.03901)	(0.03995)	(0.04103)	(0.04285)	(0.04326)
Black			-0.0750	-0.0735	-0.0800	-0.0792
			(0.01573)	(0.01639)	(0.01691)	(0.01699)
Age			0.0094	0.0077	0.0078	0.0079
			(0.00528)	(0.00530)	(0.00541)	(0.00543)
Age^2			-0.0002	-0.0001	-0.0001	-0.0001
			(0.00008)	(0.00008)	(0.00008)	(0.00008)
Tenure			-0.0735	-0.0688	-0.0625	-0.0474
			(0.01218)	(0.01222)	(0.01222)	(0.01312)
$Tenure^2$			0.0105	0.0097	0.0083	0.0056
			(0.00328)	(0.00328)	(0.00328)	(0.00340)
$Tenure^3$			-0.0005	-0.0005	-0.0004	-0.0002
			(0.00031)	(0.00031)	(0.00031)	(0.00031)
$Tenure^4$			0.0000	0.0000	0.0000	0.0000
			(0.00001)	(0.00001)	(0.00001)	(0.00001)
Married			0.0121	0.0138	0.0115	0.0117
			(0.01070)	(0.01078)	(0.01090)	(0.01092)
Year FE		Yes	Yes	Yes	Yes	Yes
Plant FE				Yes	Yes	Yes
Job Category FE					Yes	Yes
Group						Yes
R-squared	0.036	0.099	0.117	0.119	0.126	0.128
N	6979	6979	6967	6967	6967	6928

[a] Robust standard errors displayed in parentheses.

Table 5: LPM Estimates From Byers Years 1919(I) through 1940(II) (vol quits only)

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Log Wage	-0.2285	-0.1545	-0.1215	-0.1231	-0.1284	-0.1161
	(0.01442)	(0.01539)	(0.01520)	(0.01523)	(0.01861)	(0.02372)
Black			0.0114	0.0124	0.0177	0.0224
			(0.01092)	(0.01092)	(0.01170)	(0.01453)
Age			0.0027	0.0030	0.0036	0.0046
			(0.00183)	(0.00183)	(0.00189)	(0.00231)
Age^2			-0.0001	-0.0001	-0.0001	-0.0001
			(0.00002)	(0.00002)	(0.00002)	(0.00003)
Tenure			-0.0788	-0.0785	-0.0755	-0.0702
			(0.00398)	(0.00399)	(0.00402)	(0.00453)
$Tenure^2$			0.0068	0.0068	0.0066	0.0060
			(0.00043)	(0.00043)	(0.00043)	(0.00049)
$Tenure^3$			-0.0002	-0.0002	-0.0002	-0.0002
			(0.00002)	(0.00002)	(0.00002)	(0.00002)
$Tenure^4$			0.0000	0.0000	0.0000	0.0000
			(0.00000)	(0.00000)	(0.00000)	(0.00000)
Married			-0.0078	-0.0091	-0.0101	-0.0075
			(0.00861)	(0.00863)	(0.00900)	(0.01061)
Year FE		Yes	Yes	Yes	Yes	Yes
Plant FE				Yes	Yes	Yes
Job Category FE					Yes	Yes
Group						Yes
R-squared	0.021	0.126	0.214	0.214	0.225	0.253
N	8909	8909	8891	8891	8891	6731

[a] Robust standard errors displayed in parentheses.

Table 6: Elasticity Estimates by Major Sub-Period

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
			Ford			
1919(I)-19405(II)	3.64	3.88	2.94	3.13	3.19	3.27
	(0.27)	(0.31)	(0.33)	(0.34)	(0.36)	(0.35)
1919(I)-1929(I)	3.63	4.15	3.16	3.37	3.41	3.40
	(0.26)	(0.30)	(0.32)	(0.32)	(0.33)	(0.34)
1929(II)-1933(I)	1.37	1.04	-1.19	-0.53	-1.02	-0.59
	(1.18)	(1.47)	(1.65)	(1.80)	(1.90)	(1.93)
1933(II)-1940(II)	4.01	4.54	4.27	4.59	5.42	5.69
	(1.04)	(1.27)	(1.46)	(1.62)	(1.67)	(1.72)
			Byers			
1919(I)-1940(II)	3.09	2.09	1.64	1.67	1.74	1.38
	(0.18)	(0.20)	(0.20)	(0.21)	(0.26)	(0.28)
1919(I)-1929(I)	2.76	3.66	1.60	1.56	1.57	1.73
	(0.24)	(0.27)	(0.26)	(0.26)	(0.34)	(0.38)
1929(II)-1933(I)	-2.47	-2.38	-1.00	-1.03	-0.68	-1.32
	(1.31)	(1.35)	(1.41)	(1.40)	(1.62)	(1.65)
1933(II)-1940(II)	-0.53	-0.62	1.77	1.80	2.09	3.36
	(0.61)	(0.59)	(0.74)	(0.71)	(0.95)	(1.29)

[a] Bootstrapped standard errors displayed in parentheses.

Table 7: Ford: Business Cycle (NBER) Estimates

Start Year	**End Year**	**Direction**	**Estimate**	**Markdown**	**Obs**
1919(I)	1919(II)	↗	1.94	0.34	230
			(2.20)		
1920(I)	1921(I)	↘	5.56	0.15	410
			(1.28)		
1921(II)	1923(I)	↗	3.74	0.21	979
			(0.69)		
1923(II)	1924(I)	↘	2.86	0.26	712
			(0.84)		
1924(II)	1926(II)	↗	3.81	0.21	1698
			(0.83)		
1927(I)	1927(II)	↘	2.21	0.31	364
			(2.90)		
1928(I)	1929(I)	↗	3.47	0.22	862
			(0.83)		
1929(II)	1933(I)	↘	-1.02	1.00	820
			(1.86)		
1933(II)	1937(I)	↗	8.05	0.11	504
			(2.12)		
1937(II)	1938(I)	↘	10.08	0.09	118
			(12.78)		
1938(II)	1940(II)	↗	2.82	0.26	282
			(3.11)		

[a] Bootstrapped standard errors displayed in parentheses.

Table 8: Byers: Business Cycle (NBER) Estimates

Start Year	End Year	Direction	Estimate	Markdown	Obs
1919(I)	1919(II)	↗	4.47	0.18	375
			(1.51)		
1920(I)	1921(I)	↘	-1.25	1.00	912
			(0.89)		
1921(II)	1923(I)	↗	2.49	0.29	1296
			(0.46)		
1923(II)	1924(I)	↘	-0.20	1.00	543
			(1.19)		
1924(II)	1926(II)	↗	1.70	0.37	1356
			(0.71)		
1927(I)	1927(II)	↘	1.13	0.47	473
			(1.84)		
1928(I)	1929(I)	↗	2.50	0.29	608
			(1.74)		
1929(II)	1933(I)	↘	-0.68	1.00	1475
			(1.59)		
1933(II)	1937(I)	↗	4.45	0.18	908
			(1.79)		
1937(II)	1938(I)	↘	3.04	0.25	242
			(3.20)		
1938(II)	1940(II)	↗	-0.64	1.00	721
			(1.11)		

[a] Bootstrapped standard errors displayed in parentheses.

Table 9: LPM Results with Individual Fixed Effects

		Coefficient on Log Wage							St. Dev. of Log Wages	
Start Year	**End Year**	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**All**	**Within**
					Ford Motors					
1919(I)	**1940(II)**	-0.444	-0.474	-0.357	-0.381	-0.388	-0.398	-0.225	0.18	0.06
		(0.034)	(0.039)	(0.040)	(0.041)	(0.043)	(0.043)	(0.069)		
1919(I)	**1929(I)**	-0.544	-0.622	-0.473	-0.503	-0.510	-0.508	-0.488	0.17	0.05
		(0.039)	(0.045)	(0.047)	(0.048)	(0.050)	(0.050)	(0.108)		
1929(II)	**1933(I)**	-0.051	-0.039	0.045	0.020	0.038	0.022	0.584	0.23	0.05
		(0.042)	(0.053)	(0.059)	(0.063)	(0.067)	(0.067)	(0.157)		
1933(II)	**1940(II)**	-0.289	-0.328	-0.308	-0.332	-0.391	-0.411	-0.246	0.19	0.07
		(0.078)	(0.094)	(0.104)	(0.114)	(0.119)	(0.120)	(0.237)		
					A.M. Byers					
1919(I)	**1940(II)**	-0.228	-0.154	-0.121	-0.123	-0.128	-0.116	-0.207	0.23	0.06
		(0.014)	(0.015)	(0.015)	(0.015)	(0.019)	(0.024)	(0.059)		
1919(I)	**1929(I)**	-0.290	-0.385	-0.168	-0.163	-0.165	-0.211	-0.418	0.18	0.07
		(0.026)	(0.029)	(0.028)	(0.028)	(0.035)	(0.046)	(0.103)		
1929(II)	**1933(I)**	0.051	0.049	0.021	0.021	0.014	0.030	0.160	0.23	0.05
		(0.027)	(0.028)	(0.029)	(0.029)	(0.033)	(0.038)	(0.094)		
1933(II)	**1940(II)**	0.016	0.019	-0.054	-0.054	-0.063	-0.104	-0.940	0.25	0.02
		(0.018)	(0.018)	(0.022)	(0.022)	(0.030)	(0.040)	(0.342)		

[a] Robust standard errors displayed in parentheses.

Table 10: Recession Effects

Weighting	(1)	(2)	(3)	(4)	(5)	(6)
			Recession Dummy			
None	0.21	-0.16	-0.50	-1.82**	-1.06	-1.98
	(1.18)	(1.14)	(0.98)	(0.80)	(1.15)	(1.17)
# of Periods	-0.99	-1.40	-1.75	-2.53***	-2.49*	-3.01**
	(1.16)	(1.21)	(1.07)	(0.80)	(1.29)	(1.25)
$\frac{1}{\text{St. Error}}$	-0.73	-1.01	-1.38*	-1.52**	-2.03**	-2.63**
	(1.12)	(1.05)	(0.66)	(0.61)	(0.77)	(0.93)
# of Obs.	-1.36	-1.67	-1.71**	-1.99***	-2.26***	-2.85***
	(1.13)	(1.17)	(0.71)	(0.64)	(0.78)	(0.91)
Periods×Obs.	-1.73	-2.07	-2.24**	-2.25***	-2.90***	-3.47***
	(1.10)	(1.24)	(0.80)	(0.71)	(0.90)	(0.94)
$\frac{\text{Periods}}{\text{St. Error}}$	-2.50**	-2.89**	-2.69***	-2.79***	-3.26***	-3.78***
	(1.03)	(1.13)	(0.71)	(0.63)	(0.88)	(0.92)
			Output Gap			
None	1.49	2.19	2.62	5.95	5.51	7.67
	(4.48)	(4.19)	(3.86)	(3.90)	(4.47)	(5.04)
# of Periods	5.38	6.51	7.34	9.05**	10.50**	11.61**
	(4.24)	(4.57)	(4.25)	(3.69)	(4.93)	(5.06)
$\frac{1}{\text{St. Error}}$	4.37	4.53	5.08	5.41*	7.92**	8.97**
	(4.09)	(4.01)	(3.05)	(2.82)	(3.40)	(4.09)
# of Obs.	6.87	7.27	6.65**	7.23**	9.22***	10.72**
	(4.06)	(4.62)	(3.08)	(2.85)	(3.15)	(3.81)
Periods×Obs.	7.27*	8.11	8.15**	8.10**	10.76***	12.20***
	(3.90)	(4.76)	(3.46)	(3.10)	(3.62)	(3.96)
$\frac{\text{Periods}}{\text{St. Error}}$	10.06**	11.11**	10.06***	10.17***	12.40***	13.99***
	(3.72)	(4.42)	(2.93)	(2.66)	(3.35)	(3.63)

[a] Standard errors displayed in parentheses.

[b] Output gap is 0 if expansion, negative by degree of contraction if in recession.

[c] *: 10% level (1.729 for 19 d.o.f.), **: 5% level (2.093 for 19 d.o.f), ***: 1% level (2.861 for 19 d.o.f.)

Table 11: Bias Reduction and Potential Wage Markdown

	Adjusted All Separations		Unadjusted Quits		Adjusted Quits	
	Estimate	**Markdown**	**Estimate**	**Markdown**	**Estimate**	**Markdown**
			Ford			
1919(I)-1940(II)	2.64	0.27	3.50	0.22	3.19	0.24
	(0.30)		(0.39)		(0.36)	
1919(I)-1929(I)	3.19	0.24	3.98	0.20	3.41	0.23
	(0.30)		(0.39)		(0.33)	
1929(II)-1933(I)	-1.84	1.00	-0.67	1.00	-1.02	1.00
	(0.98)		(1.26)		(1.90)	
1933(II)-1940(II)	4.44	0.18	6.37	0.14	5.42	0.16
	(0.99)		(1.97)		(1.67)	
			Byers			
1919(I)-1940(II)	1.36	0.42	1.60	0.38	1.74	0.37
	(0.17)		(0.24)		(0.26)	
1919(I)-1929(I)	1.07	0.48	1.48	0.40	1.57	0.39
	(0.23)		(0.32)		(0.34)	
1929(II)-1933(I)	1.75	0.36	-0.50	1.00	-0.68	1.00
	(0.69)		(1.18)		(1.62)	
1933(II)-1940(II)	0.83	0.55	2.31	0.30	2.09	0.32
	(0.58)		(1.05)		(0.95)	

[a] Bootstrapped standard errors displayed in parentheses.

Table 12: Ford: LPM Estimates on Involuntary Separations

	Fires and Layoffs	**Fires**	**Layoffs**
Log Wage	-0.0961	-0.0863	-0.0270
	(0.03526)	(0.02361)	(0.03046)
Black	-0.0001	0.0020	-0.0024
	(0.01239)	(0.00889)	(0.00950)
Age	-0.0019	-0.0010	-0.0010
	(0.00407)	(0.00262)	(0.00339)
Age^2	0.0000	-0.0000	0.0000
	(0.00006)	(0.00004)	(0.00005)
Tenure	-0.0122	-0.0174	0.0047
	(0.00927)	(0.00581)	(0.00793)
$Tenure^2$	0.0025	0.0036	-0.0009
	(0.00284)	(0.00147)	(0.00264)
$Tenure^3$	-0.0003	-0.0003	-0.0000
	(0.00029)	(0.00013)	(0.00029)
$Tenure^4$	0.0000	0.0000	0.0000
	(0.00001)	(0.00000)	(0.00001)
Married	-0.0095	0.0038	-0.0136
	(0.00742)	(0.00569)	(0.00535)
R-squared	0.113	0.042	0.183
N	5875	5615	5682

[a] Bootstrap standard errors displayed in parentheses.

Table 13: Byers: LPM Estimates on Involuntary Separations

	Fires and Layoffs	**Fires**	**Layoffs**
Log Wage	-0.1342	-0.0527	-0.1291
	(0.02006)	(0.01372)	(0.01949)
Black	0.0249	0.0181	0.0213
	(0.01256)	(0.01042)	(0.01245)
Age	0.0026	0.0029	0.0033
	(0.00221)	(0.00167)	(0.00217)
Age^2	-0.0001	-0.0000	-0.0001
	(0.00003)	(0.00002)	(0.00003)
Tenure	-0.0736	-0.0367	-0.0705
	(0.00439)	(0.00353)	(0.00435)
$Tenure^2$	0.0065	0.0034	0.0062
	(0.00049)	(0.00038)	(0.00049)
$Tenure^3$	-0.0002	-0.0001	-0.0002
	(0.00002)	(0.00001)	(0.00002)
$Tenure^4$	0.0000	0.0000	0.0000
	(0.00000)	(0.00000)	(0.00000)
Married	-0.0299	-0.0120	-0.0269
	(0.00981)	(0.00781)	(0.00965)
R-squared	0.165	0.106	0.157
N	9204	8125	9047

[a] Bootstrap standard errors displayed in parentheses.

Figure 1: Ford: Peak and Average Employment

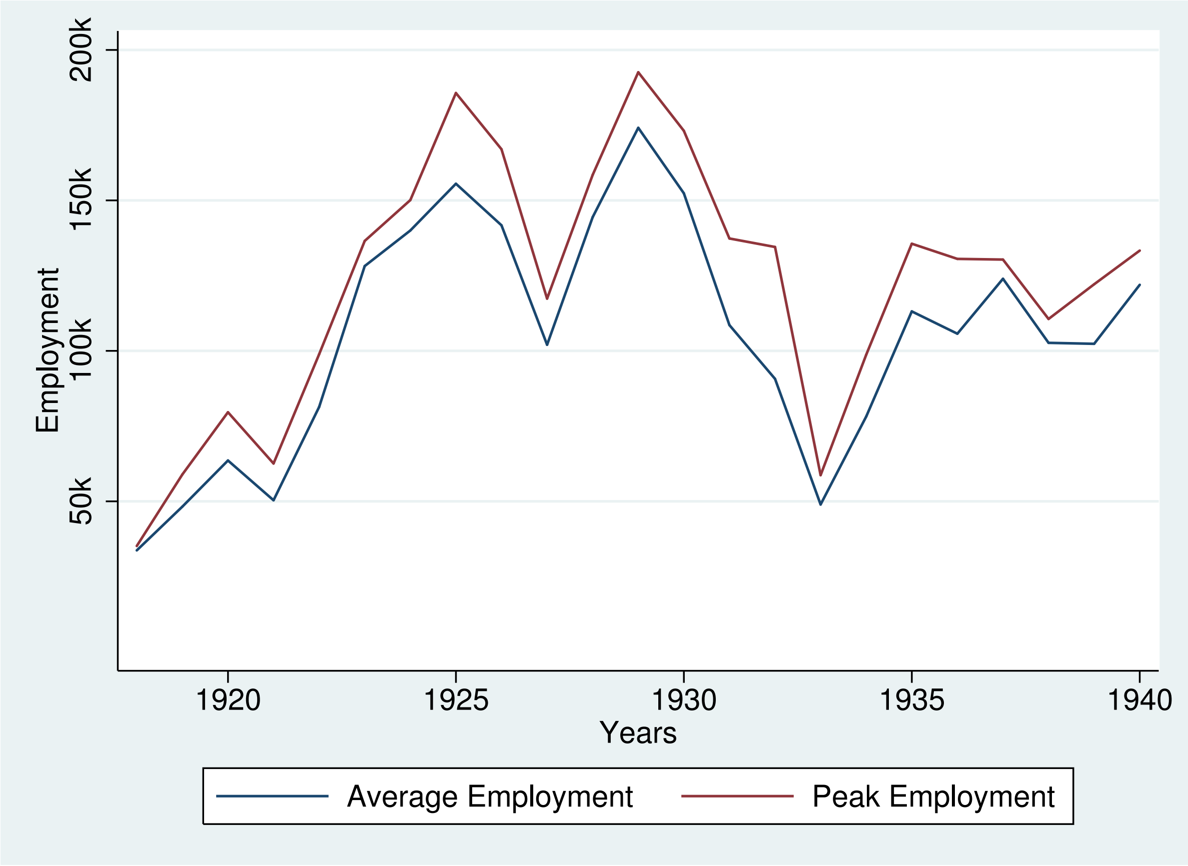


Figure 2: Byers: Average Employment

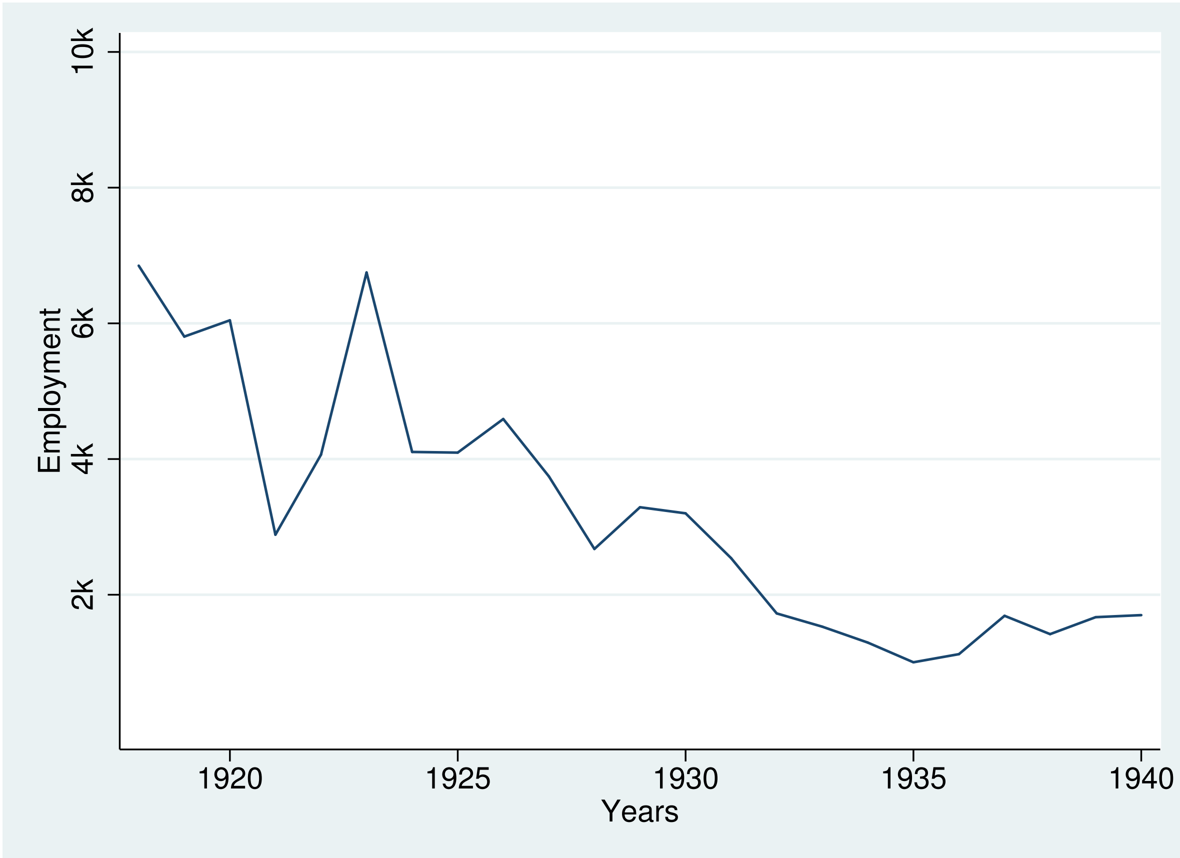


Figure 3: Elasticity of Supply to the Firm and the Business Cycle (NBER)

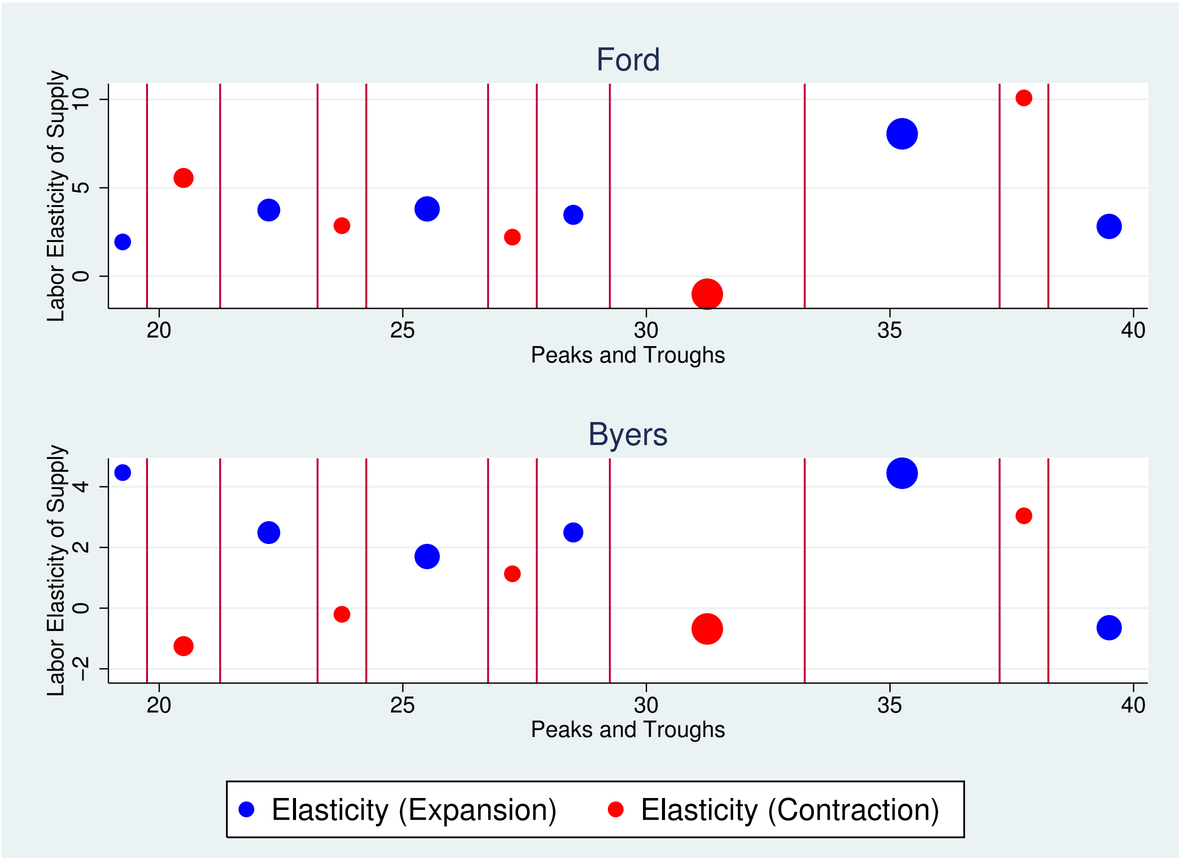


Figure 4: Ford: Separation Rates by Type

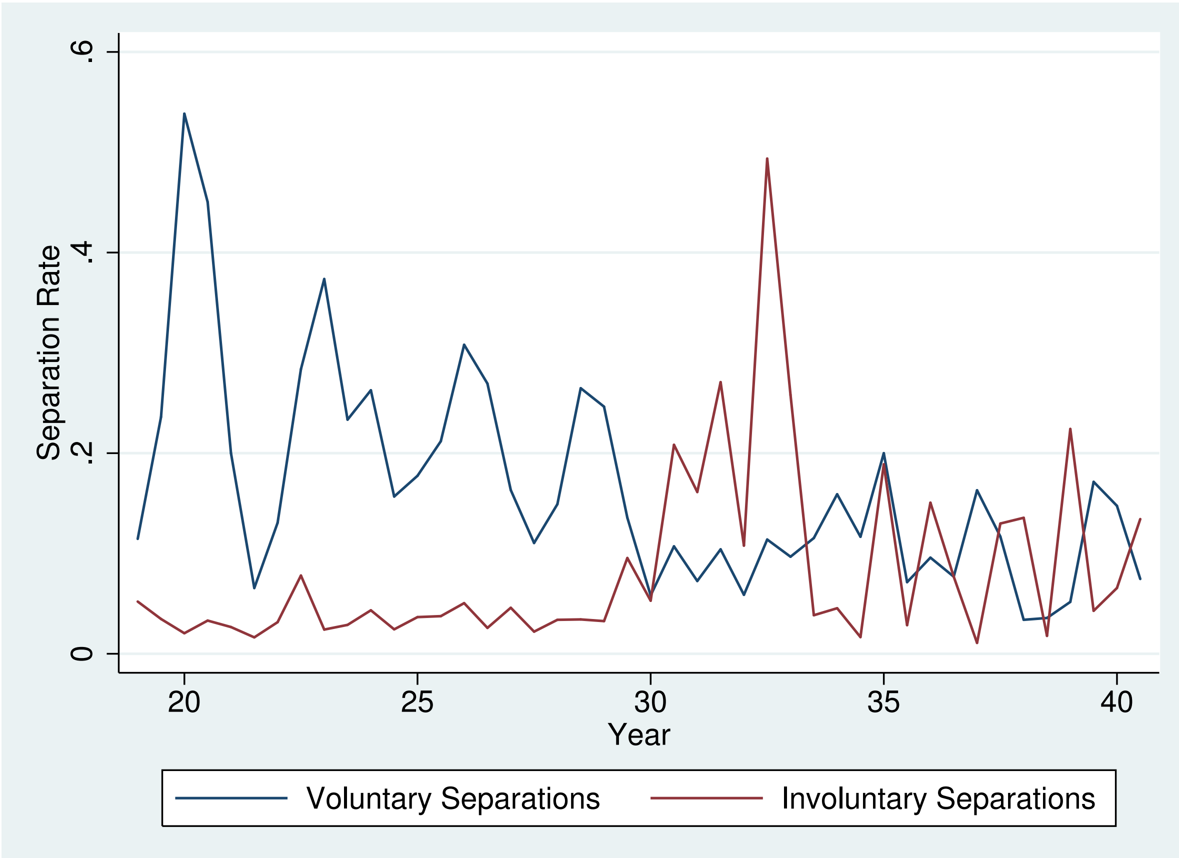
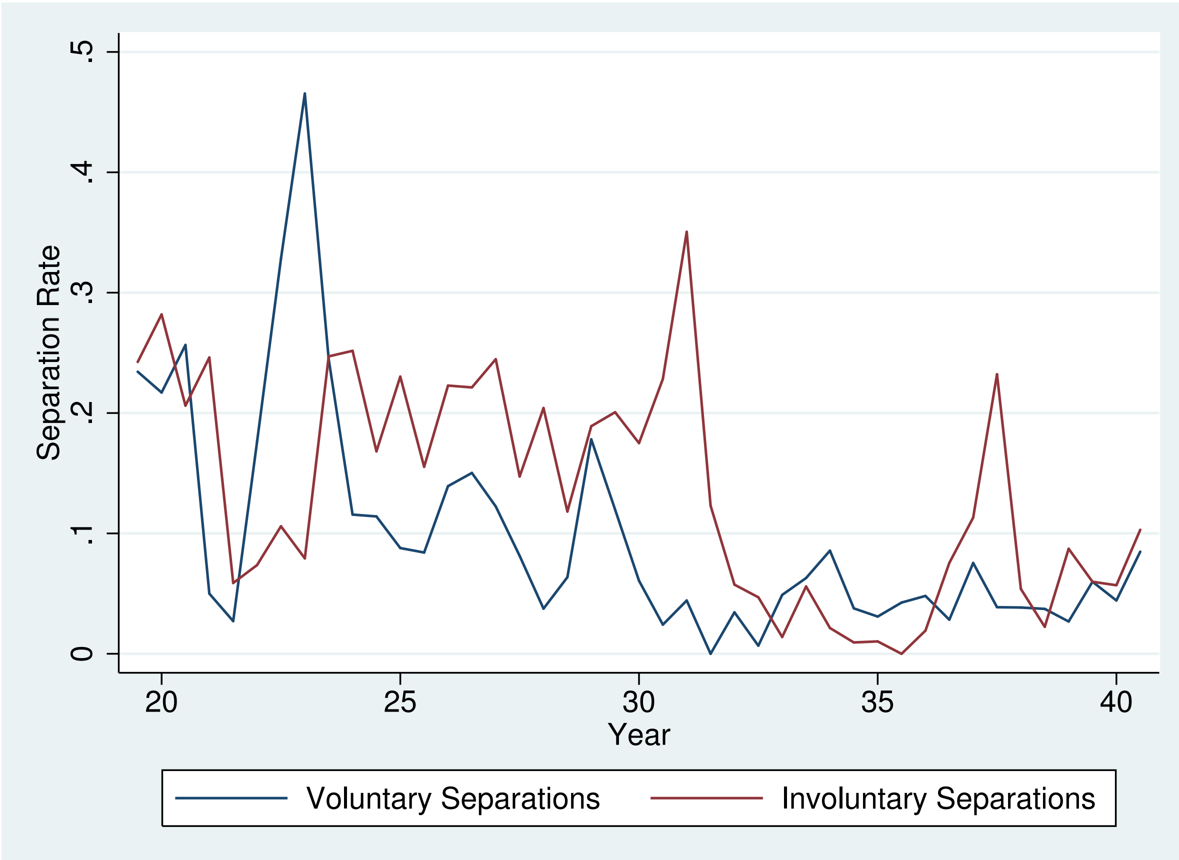


Figure 5: Byers: Separation Rates by Type



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