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Minimum Wage Shocks, Employment Flows and Labor Market Frictions

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Abstract

We provide the first estimates of the effects of minimum wages on employment flows in the U.S. labor market, identifying the impact using policy discontinuities at state borders. We find that minimum wages have a sizable negative effect on employment flows but not stocks: separations and accessions fall among affected workers. We interpret our findings using a job-ladder model, in which minimum wage increases can reduce job-to-job transitions. We find that a standard calibration of the model generates predicted relative magnitudes of the employment stock and flow elasticities that are very close to our reduced-form estimates.

Keywords: Minimum Wage, Labor Market Flows, Job Turnover, Search Frictions, Monopsony
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1 Introduction

While much attention has been paid to the question of how minimum wages affect employment stocks, there has been considerably less attention paid to the effects on employment flows. In this paper we use a relatively new dataset—the Quarterly Workforce Indicators (QWI)—to estimate the reduced-form minimum wage elasticities of average earnings, employment stocks as well as employment flows. The data permit us to estimate the responses of local labor market level separation, accession and turnover rates for two high-impact demographic and industry groups: teens and restaurant workers. To our knowledge, these are the first estimates of the effects of minimum wage increases on employment flows using nationally representative U.S. data. Our estimated minimum wage elasticities utilize a border-discontinuity design that eliminates biases from spatial heterogeneity present in many previous studies.

We find striking evidence that separations, new hires, and turnover rates for teens and restaurant workers fall substantially following a minimum wage increase–with most of the reductions coming within the first three quarters of the increase. We also find substantial positive effects on average earnings. But our estimated employment elasticities are small in magnitude and not statistically distinguishable from zero for either teens or restaurant workers; nor do we find any evidence of labor-labor substitution within the restaurant workforce with respect to age or gender.

We then use a model with on-the-job search to interpret our findings. We analytically derive the elasticities of employment and separation rates with respect to the minimum wage for the job ladder model, and show that the ratio of the two is a function of only three parameters: the offer arrival rates for the employed and the unemployed, and the exogenous job destruction rate. We show that a minimum wage increase will affect employment flows relatively more than stocks when there is greater equilibrium dispersion in the rates of job-to-job transitions; this dispersion itself stems from frictional wage inequality. Importantly, our result is independent of factors that may determine the wage offer distribution—such as the distribution of firm productivity or the nature of wage setting.

We also calibrate the model parameters following Hornstein Krussell and Violante (2011), and numerically calculate the ratio of the two minimum wage elasticities. We find that the model calibrated using U.S. data suggests a separation rate elasticity that is about five times as large in magnitude as the employment elasticity; this five to one ratio is virtually identical to our reduced-form empirical estimates. While existing evidence linking minimum wages and search friction has mostly focused on the size of the employment effect, we look at a new metric: the relative magnitudes of the employment and separation rate elasticities.

Our findings thus provide new evidence on the relevance of search frictions in explaining how minimum wages affect the labor market.

Our paper relates to three distinct literatures. First, a handful of papers have directly estimated the reduced-form effects of minimum wages on equilibrium turnover, separations, or tenure. Portugal and Cardoso (2006) find that teen separations fall substantially after a minimum wage increase in Portugal. However, the national-level policy change used for estimation makes it more like a single case study, raising concerns about both the identification strategy and inference that are not issues in our paper. Additionally, we are able to explicitly interpret our empirical results using a model with search friction, and show how the combination of small effects on employment stocks and bigger effect on employment flows is generated by a plausibly calibrated model. Using Canadian data, Brochu and Green (2011) find that teen hires and layoffs decline in the year after a minimum wage increase, while quits decline by much less; they find some reductions in employment levels as well. While quits are only about 38 percent of separations in their "low skilled" Canadian sample, JOLTS data for the U.S. indicate that quits account for over 70 percent of the separations in the Accommodation and Food Services sector during our sample period. This difference suggests that the layoff channel that Brochu and Green highlight has less relevance in the U.S. context. Unfortunately, the small number of Canadian provinces (and hence policy clusters) also raises serious concerns about their identification and inference. For example, Brochu and Green's empirical strategy cannot rule out that heterogeneous spatial trends are driving some of their findings on layoffs and employment—which we show are quite important in the U.S. context.

A few studies examine the effects of wage mandates on labor market flows in much more limited contexts. Dube, Naidu and Reich (2007) estimate employment and tenure effects in a single city—San Francisco—in response to a citywide wage mandate. The effects of "living-wage" laws on firm-based employee turnover have been studied in specific cities and sectors—for example, Fairris (2005) for local government service contractors in Los Angeles; Howes (2005) for homecare workers in selected California counties; and Reich, Hall and Jacobs (2005) for employers at San Francisco International Airport.[1] Overall, compared to these papers, we are able to estimate the responses of employment flows to minimum wage changes using much richer variation and a more credible identification strategy.

Second, our paper relates to firm-level estimates of labor supply elasticities and monopsony power. Card and Krueger (1995) propose a dynamic monopsony model, in which separation and recruitment rates are functions of the wage. They argue that empirically plausible magnitudes of the labor supply elasticities facing a firm are consistent with small

[1] See also the survey in Manning (2010).

positive or zero effects of a minimum wage increase on employment levels. Subsequent firm-level studies, such as those surveyed by Ashenfelter, Farber and Ransom (2010), have indeed found small firm-level separations elasticities (and hence labor supply elasticities), consistent with substantial wage-setting power. However, it is difficult to use these firm-level labor supply elasticities to deduce market-wide changes from an increase in the minimum wage. We build on this literature by showing how *equilibrium* flows respond to a minimum wage shock, and what this tells us about the extent of search frictions in the labor market.

Finally, a number of papers use structurally estimated search models to study minimum wage effects. These include Bontemps, Robin and van den Berg (1999, 2000), Flinn (2006) and Flinn and Mabli (2009).[2] These authors primarily use cross-sectional hazard rates and the wage distribution to estimate model parameters and then simulate the effect of a minimum wage policy. Our approach is different. We estimate the reduced-form effects of minimum wages on employment stocks and flows and compare our estimates with the predictions from a plausibly calibrated job-ladder model with search frictions. Our comparison constitutes a test of overidentifying restrictions for the calibrated model using exogenous policy variation. It therefore provides new evidence on the model's ability to fit the data.

The rest of the paper is structured as follows. We discuss our identification strategy, dataset and sample in Section 2 and report our empirical findings in Section 3. Section 4 interprets the estimated elasticities using a canonical job-ladder model. We present our conclusions in Section 5.

2 Empirical Methods

2.1 Identification strategy

To measure the impact of minimum wage changes on earnings, employment levels and employment flows, we build on the research design proposed and implemented in Dube, Lester and Reich (2010). This approach, which essentially generalizes Card and Krueger (2000), exploits minimum wage policy discontinuities at state borders by comparing outcomes from all U.S. counties on either side of a state border. As shown in detail in Dube, Lester and Reich, this research design has desirable properties for identifying minimum wage effects. Measuring labor market outcomes from an immediately adjacent county provides a better control group, since firms and workers on either side are generally affected by the same idiosyncratic local trends and experience macroeconomic shocks at roughly the same time.

Since minimum wage policies in the U.S. tend to exhibit spatial clustering, empirical

[2] Bontemps, Robin and van den Berg (1999, 2000) and Flinn and Mabli (2009) all consider on-the-job search and are closely related to the canonical job-ladder model considered here.

methods that do not control for spatial heterogeneity can produce highly misleading estimates. In particular, using a national panel-data model with state (or county) and time fixed effects generates an omitted variables bias. As a result, such two-way fixed effects models often attribute to minimum wage policies the effects of regional differences in the growth of low-wage employment that are independent of minimum wage policies. As documentation of this point, Dube, Lester and Reich (2010), Figure 4 shows that employment levels and trends are negative prior to the minimum wage change using a conventional fixed effects specification; in contrast, there are no such pre-trends using the border counties approach. [3]

One threat to identification using border counties comes from cross-border spillovers. For example, higher-end restaurants may sort into the state with a higher minimum wage, while lower-end restaurants sort into the lower minimum wage state. To assess the importance of cross-border spillovers, Dube, Lester and Reich compare the effects of minimum wages on border counties to the effects on the counties in the interior of the state, were spillovers are less likely to have an effect. They find (Dube, Lester and Reich, Table 4) that at the county level, the spillover effect is very close to zero and not statistically significant.

We use two different specifications to estimate minimum wage effects. Specification 1, which we call the conventional approach, is a two-way fixed effects model—with county and common time effects.

$$(1) y_{ipt}^{k} = \alpha + \beta \ln(MW_{it}) + \delta \ln(emp_{it}^{TOT}) + \gamma \ln(pop_{it}) + \psi_i + \tau_t + \epsilon_{ipt} \tag{1}$$

Here y_{ipt}^{k} refers to the dependent variable—which could be the log of earnings, employment, separations, hires, or the turnover rate—in county i, in county-pair p, at time t, for each of the specific industry or demographic groups k (e.g., restaurant workers or teens). Note that a given county can be part of multiple county pairs if it has more than one adjacent county across the state line. In addition, given the time frame of our panel dataset, a given county can be either a "treated" or "control" unit, depending on the timing of minimum wage changes between the affected states. The coefficient on the minimum wage variable $\ln(MW_{it})$ is the primary coefficient of interest; it is reported in each of the tables below.

Specification 1 also includes controls for the natural log of total private sector employment and population in each county.[4] The ψ_i term represents a county fixed effect. Crucially, the common time fixed effects τ_t are assumed to be constant across counties, which rules out possibly heterogeneous trends.[5]

[3]Evidence of bias in measured minimum wage effects due to spatial heterogeneity is also presented in Addison Blackburn and Cotti (2009, 2010) and Allegretto Dube and Reich (2011).

[4]We use county-level Census Bureau population data, which are reported on an annual basis.

[5]In the remainder of this paper, we will use the term common time effects for τ_t.

In specification 1, conditional on covariates and the county fixed effect, all other counties are used as controls for a treated county facing a minimum wage hike—regardless of their geographic locations. In contrast, our preferred border-discontinuity strategy consists of making a series of localized comparisons *within* contiguous county pairs. This strategy is represented in specification 2 below:

$$y_{ipt}^{k} = \alpha + \beta \ln(MW_{it}) + \delta \ln(emp_{it}^{TOT}) + \gamma \ln(pop_{it}) + \psi_i + \tau_{pt} + \epsilon_{ipt} \tag{2}$$

This specification is analogous to specification 1 in every respect except for the inclusion of a pair-specific time effect τ_{pt}, rather than a common time effect. Hence, specification 2 uses the within-pair variation across all pairs and effectively pools the estimates. The identifying assumption for the border-discontinuity specification is that, conditional on covariates and county fixed effects, minimum wages are uncorrelated with the residual outcome within a county pair. This assumption is much weaker than the assumption justifying the conventional specification 1.

Since policy is set at the state level, we cluster our standard errors at the state level as well. Note that the contiguous county pair sample stacks all pairs, so that a particular county will be in the sample as many times as it can be paired with a neighbor across the border. State-level clustering automatically accounts for the presence of county duplicates in the estimation of the standard errors. However, the presence of a single county in multiple pairs along a border segment also induces a mechanical correlation in the error term across state pairs, and potentially along an entire border segment. To account for this induced spatial autocorrelation, we additionally cluster the standard errors on the border segment using multi-dimensional clustering (Cameron, Gelbach and Miller 2011).

2.2 The Quarterly Workforce Indicators dataset

The recent minimum wage literature in the U.S. has drawn primarily upon two datasets: the Quarterly Census of Employment and Wages, or QCEW (e.g., Addison and Blackburn 2009, 2010; Dube, Lester and Reich 2010) and the Current Population Survey (e.g., Neumark and Wascher 2007; Allegretto, Dube and Reich 2011). The QCEW's advantage lies in providing essentially a full census of employment at the county and industry level, but it provides no information on demographics or job flows. The CPS's advantage lies in providing the worker-level demographic data needed to estimate employment effects by age or gender. However, the CPS's small sample size prevents us from estimating effects within local labor markets.[6] Therefore, neither data source allows researchers to test hypotheses regarding

[6] While the CPS contains information on separations based on household-reported data, it is likely much more error-prone than the QWI, which is a near-universe of employer-reported data based on official

employment flows in response to a minimum wage change at a local labor market level.

In this paper we use the QWI, which combines many of the virtues of both the QCEW and the CPS, while also allowing a richer analysis of dynamic responses to minimum wage changes.[7] The QWI data offer employment counts and average wages by detailed industry at the county level for specified age and gender groupings, and as well quarterly figures for hires, separations and turnover rates. We use five different dependent variables in our empirical analysis: (1) *Earnings*: Average monthly earnings of employees who worked on the last day of the reference quarter in county i. (2) *Employment*: Number of jobs on the last day of the quarter in county i. (3) *Accessions* (Hires): The number of workers who started a new job in the specified quarter in county i. This variable includes new hires as well as workers who have been rehired with the same employer within the last four quarters. (4) *Separations*: Number of workers whose job with a given employer ended in the specified quarter in county i. (5) *Turnover rate*: Average number of hires and separations as a share of total employment: $\frac{Accessions+Separations}{2\times Employment}$.

The first two variables are consistent with the data presented in the QCEW, while the three flow variables—hires, separations, and turnover rate—are unique to the QWI. In addition, the QWI offers separate tabulations of these outcome variables calculated only for workers who were employed at the firm for at least one full quarter.[8] We refer to this sample as workers with "tenure greater than 1 quarter."

Our paper focuses on labor turnover in response to minimum wage changes within a specific low-wage industry or a specific demographic group. Low-wage labor markets have long been recognized as highly volatile, with very short employment spells and frequent shifts between labor market participation and non-participation. Consequently, earnings, employment and turnover calculations may vary considerably with the proportion of workers who begin or complete job spells during the quarter. Thus, we present our empirical estimates

Unemployment Insurance records.

[7]The QWI data are produced though a partnership between the U.S. Census Bureau and the state Labor Market Information (LMI) offices. This partnership, called the Local Employment Dynamics (LED) program, combines administrative data from each participating state's unemployment insurance filings (which also make up the current QCEW) with current demographic information from other administrative censuses and population surveys. The underlying datasets consequently are much larger than the CPS or JOLTS. For detailed documentation of the QWI, see Abowd et al. (2009). Abowd and Vilhuber (2011) provides an extensive comparison of the QWI to CPS and JOLTS datasets. In Abraham's (2009) assessment of the quality of the QWI data the only major issue concerns imputed levels of education, which are are not pertinent here. Thompson (2009) also uses the QWI data to evaluate the effect of minimum wage on teen and young adult employment. Thompson's primary concern is whether the "bite" of the minimum wage explains the magnitude of the employment effect. In contrast, our focus is on separations and turnover.

[8]More precisely, according to the Census Bureau, the >1q hires measure equals the number of workers who began work with an employer in the previous quarter and remain with the same employer in the current quarter; and the >1q separations measure equals the number of workers who had a job for at least a full quarter and then the job ended in the current quarter.

for earnings, employment, hires, separations, and turnover for workers at all tenure levels as well as for those with tenure greater than 1 quarter.

2.3 Sample construction

The majority of states entered the QWI program between the late 1990s and early 2000s. To obtain a balanced panel, we take as our sample period 2001q1 through 2008q4.[9]

2.3.1 *Demographic groups and industries*

We estimate minimum wage effects for two broad employee groups, both of which have been the focus of much previous empirical research and which include high shares of minimum wage workers. The first employment group consists of teens. Using the demographic information contained in the QWI we present minimum wage elasticities for all teens aged 14-18.[10] The second high-impact group consists of establishments in the restaurant industry. In 2006 restaurants employed 29.9 percent of all workers paid within ten percent of the state/federal minimum wage, making restaurants the single largest employer of minimum wage workers at the 3-digit industry level (authors' analysis of the 2006 CPS). Restaurants are also the most intensive user of minimum wage workers, with 33 percent of restaurant workers earning within ten percent of the minimum wage (using 3-digit level industry data). We also provide additional estimates within the restaurant sample by age categories (teens, young adults who are 19-24 years old, and all other adults), and gender to test for substitution among these groups.

2.3.2 *Contiguous Border County Pair Sample*

Our research design is based on contiguous border county pairs. Our QWI sample consists of the 1,063 counties that border another state. Collectively, these border counties comprise 1,169 unique county pairs. Some of these pairs have a minimum wage differential and others do not. In addition, in any single regression we limit the sample to those counties that have a full panel of disclosed data. As is the case with the QCEW, the QWI does not report values for cells in which too few establishments comprise the sample and/or where the identity of a given establishment could be inferred. We merge information on overall local unemployment rates and the value of each state's minimum wage in each quarter with the

[9]The dataset we obtained from the Cornell University Virtual Data Repository—which hosts the QWI flat files—included data through 2009q1 at that time. Since the hires, separations and turnover variables with tenure greater than one quarter require information for a leading quarter, the last quarter for which these variables were defined is 2008q4.

[10]The youngest age category reported in the QWI is 14-18.

QWI county-pair panel dataset.[11]

What are the effects of restricting our sample to border-county pairs? Table 1 presents the means and standard deviations for our five outcome variables for all 2,960 U.S. counties and for the 1,063 contiguous counties in our border-county pair sample. We display these measures for all private sector employees, all employed teens, and all restaurant workers, and separately as well for workers at all tenure levels and those with at least one quarter of tenure. Depending upon the worker group and tenure level, average earnings are 0.5 to 1 percent lower in the border-county pair sample, while average employment is 7 to 10 percent lower. Hire, separation and turnover rates are virtually identical in both samples. We surmise that the border-county sample is composed of somewhat smaller counties, but this difference is modest. All the other characteristics of the two samples are quite close.

2.4 Descriptive statistics

Since the QWI may be relatively unfamiliar to many economists, here we provide some additional descriptive statistics in Table 2 for the workforce at large. As Table 2 indicates, the sample means for the outcome variables vary considerably by age and industry, as well as by tenure level. Earnings levels are much lower among teens, young adults and restaurant workers than among all older workers; women earn less than men; and workers with job tenure ≥ 1 quarter earn more than workers with less than one quarter of job tenure.[12] These are expected patterns. The proportion of workers with less than one quarter of tenure ranges from 31 percent among teens to 24 percent among restaurants.

Hire, separation and turnover rates also vary with age, industry, gender, and tenure level. Each of these three rates is higher for younger workers than for older workers. Teens, for example, have a turnover rate of 62 percent, followed by 53 percent among young adults, and 18 percent for older adults. The three rates are also higher among restaurant workers than among all workers, and much higher among workers with job tenure of less than one quarter. For differences by age, industry and tenure, each of these variables is inversely correlated with earnings levels: hire, separation and turnover rates are lower among higher-paid workers. These patterns are similar to those found in previous research.

Men have a slightly greater rate of turnover (23 percent) than women (21 percent). However, among workers with job tenure≥ 1 quarter, the hire, separation and tenure rates are virtually identical for males and females.

[11] We treat the county of San Francisco, California as a separate policy unit and compare it with neighboring counties. San Francisco has a county-level minimum wage that applies to all workers and establishments, analogous to a state minimum wage in every respect.

[12] Some of these pay differentials reflect differences in hours worked, experience and skill level, but our data do not permit us to quantify these effects.

Although not shown in Table 2, the data that underlie the separation rates yield surprising indications of how concentrated separations are in short-tenure jobs. Among all workers, jobs with less than one quarter of tenure account for 10.1 percent of all jobs, but 55.7 percent of all separations. In the restaurant industry, separations are as concentrated in short-term jobs, but such jobs are three times more common than in all industries. In restaurants, jobs with less than one quarter of tenure account for 31 percent of all jobs and 60.5 percent of all separations. This duration dependence of separation is useful for interpreting the results on the the turnover elasticity in the next section.

3 Empirical Findings

3.1 Main results

We present in Table 3 our main findings on the effects of minimum wage increases for teens and for restaurant workers. For each group we report estimates for five outcome variables and using two specifications, one with controls for common time effects (the conventional model), and the second with controls for county-pair specific time effects (the preferred model). Both are reported in the table to demonstrate the relevance of our border discontinuity-based research design. The text usually refers to our preferred specification, except when discussing how estimates from the conventional model can be misleading due to the presence of spatial heterogeneity.

We begin by showing that the minimum wage is binding for each of these groups. The estimated effects on log average monthly earnings are positive and highly significant—for both specifications and for both groups of workers. For each group of workers, the conventional specification (columns 1, 3) yields a somewhat smaller effect on earnings than our preferred border-discontinuity specification (columns 2, 4). The elasticity of earnings is 0.161 among all teen workers and 0.213 among all restaurant workers.[13] These findings put to rest any concerns that restricting the identifying variation to cross-border pairs leads to a lack of actual earnings differential across the treated and control units.

We turn next to the estimated employment effects, shown in the second row of Table 3. We highlight two results in this row. First, although the conventional specification (column 1) yields an estimated employment elasticity of -0.200 for teen workers, once we account for spatial heterogeneity the coefficient in border-discontinuity specification (column 2) is very small in magnitude (-0.039) and it is not significantly different from zero. The conventional

[13] The elasticities for teens and for restaurant workers are very close to our estimates for these groups using the CPS for teens (Allegretto, Dube and Reich 2011) and the QCEW for restaurants (Dube, Lester and Reich 2010).

estimates on teens are very close to those found by researchers using the CPS and similar models (Neumark and Wascher 2007; Allegretto, Dube and Reich 2011). In other words, we find strong evidence that spatial heterogeneity produces a spurious disemployment effect for teen workers, thereby demonstrating the scope of the disemployment bias among studies using the conventional specification. Second, we replicate the qualitative findings in Dube, Lester and Reich using the QWI sample: among all restaurant workers the conventional estimate of the employment elasticity is -0.121 and statistically significant. But accounting for spatial heterogeneity reduces the effect (in magnitude) to -0.057 and renders it indistinguishable from zero.

Finally, we consider the estimates for the flow outcomes—log hires, log separations and log of the turnover rate. The findings here contrast sharply with those on employment levels. As rows three to five of Table 3 indicate, hires, separations and the turnover rate fall substantially and significantly with minimum wage increases. For our preferred specification (columns 2 and 4), the separations elasticity is substantial both for teens (-0.253) and for restaurant workers (-0.319). The accessions (hires) elasticities are quite similar to the separations elasticities, which is consistent with the responses reflecting steady state to steady state comparisons. For each group, the estimated effects for separations and hires are smaller using the preferred specification as compared to the conventional one. This result is to be expected because the downward bias in employment estimates in the conventional specification mechanically imparts an analogous bias to the separations and hires elasticities, but not to the turnover rate elasticity, or any other rate elasticities. (The separation rate elasticity is equal to the separations elasticity less the employment elasticity.)

Summarizing to this point, we find that our border-discontinuity estimates find strong positive responses of earnings to a minimum wage increase. This rise in earnings is met with a change in the employment stock that is indistinguishable from zero. However, we find clear evidence that employment flows (hires and separations) both fall strongly in response to the policy change. And these patterns hold whether we consider a high-impact demographic group (teens) or a high-impact industry (restaurants).

3.2 Robustness checks

Table 4 presents three robustness checks for our main results, using our preferred specification and estimated for teens and for restaurant workers. One concern is the presence in our sample of geographically large counties, which are located primarily in the western U.S. For these counties, border contiguity need not imply proximity of population centers. As a check, columns labeled 1 and 4 add a restriction for county size ($<$ 2,000 square miles). This restriction does not substantially affect any of the estimated effects on earnings, employment, hires, separations and turnover rates.

A second concern is that the flow results for teens and restaurant workers may be affected by unobserved overall county labor market trends. To check for this possibility, columns labeled 2 and 5 in Table 4 include the overall private sector level outcome (separation, turnover, etc.) as an additional control. Unlike employment, a disproportionately large share of overall separations and new hires come from the low wage sector. For this reason, inclusion of the overall private sector flow measure is a particularly tough test. For teens, adding this control reduces the absolute value of the flow coefficients, the hires coefficient becomes insignificant and the separations and turnover estimates retain statistical significance. For restaurant workers, adding this control also reduces the estimates somewhat, but they continue to be statistically significant. Overall, we conclude that the reductions in flows in low wage sectors and demographic groups are not driven primarily by unobserved local trends in flows.

The group of columns labeled 3 and 6 in Table 4 report results from a test for the presence of pre-existing trends that might confound the estimates, as well as for possible lagged effects. We estimate a single specification that includes both a one year (4 quarters) lead $\ln(MW_{t+4})$ and a one year (4 quarters) lag $\ln(MW_{t-4})$, in addition to the contemporaneous minimum wage $\ln(MW_t)$.[14] All three of the coefficients are reported in the table. We do not find any statistically significant (or quantitatively large) leading or lagged terms for any of our outcomes. Moreover, including the leading and lagged minimum wage terms does not attenuate our statistically significant contemporary coefficients for the flow measures reported in Table 3. These results provide additional internal validity to our research design and rules out the possibility that the large reductions in the flows are driven by pre-existing trends. The reductions in employment flows occur immediately—within three quarters of the minimum wage increase. They also show that the reduction in flows represents a *permanent* change in response to the policy and not transitional dynamics. The latter observation justifies our assumption that these elasticities reflect changes from one steady state to another when we interpret our findings using a job search model below.[15]

[14] The coefficient for $\ln(MW_t)$ represents the short run elasticity, while the sum of the coefficients for $\ln(MW_t)$ and $\ln(MW_{t-4})$ represents the long run elasticity.

[15] As in Dube, Lester and Reich (2010), when we compare outcomes in border versus interior counties to detect cross-border spillovers, we do not find such spillovers (results not shown). Additionally, we find that using coarser forms of controls for heterogeneity such as Census division-specific time effects instead of pair-specific time effects produces similar results—as would be expected based on the findings in Dube, Lester and Reich (2010) and Allegretto, Dube and Reich (2011) (results not shown).

3.3 Effects by tenure on the job

As mentioned during our discussion of the descriptive statistics, turnover generally is concentrated among short-term jobs—those of one-quarter or less. Since not all teens may be in minimum wage jobs (or in jobs whose wages are affected by minimum wage policy), we might expect the effect of the minimum wage on separations to have a tenure-specific component. Lower-tenure workers are, *ceteris paribus*, more likely to be minimum wage workers. In this section, we provide some additional evidence that is consistent with the interpretation that minimum wage has a causal effect on separations.

First, if minimum wage increases reduce labor market flows, we would expect to find that they also reduce the fraction of workers with such short-term jobs. Columns 1 and 4 of Table 5 provide estimated effects on the fraction short-term, for teens and restaurant workers, respectively. The estimated effect is negative for both groups, although statistically significant only for the restaurant sample. To investigate further how minimum wage effects vary by tenure, we estimate our preferred specification for workers who have at least one quarter of job tenure.[16] Table 5 displays our previous results for workers at all tenure levels (column 2 for teens and 5 for restaurant workers, as well as those who have more than one quarter of tenure (columns 3 and 6).

When we limit attention to workers with at least one quarter of job tenure, the earnings estimates for both teens and restaurant workers are somewhat smaller than among workers of all tenure levels and they continue to be statistically significant. The somewhat smaller magnitude of the earnings elasticity for the sample with more tenure is consistent with the idea that higher tenure workers tend to be higher wage workers. Employment effects for this sample are again very small and not significant. Among workers with at least one quarter of tenure, the estimated effects on hires and separations are smaller than among workers of all tenure levels and they are no longer significant.

These findings suggest that minimum wage changes reduce turnover more sharply for workers with lower tenure level—precisely those whose wages are more likely to be affected by the policy. However, the composition of the sample of those with less than one quarter of tenure may also be affected by the policy change. While such compositional changes are unlikely to fully explain these results, we prefer to be cautious when interpreting these estimates.[17]

[16] The QWI data do not provide breakdowns for tenure longer than one quarter.

[17] If a compositional change in the sample is induced by the minimum wage, a single index model suggests that those changing categories from <1 quarter to >1 quarter of tenure are likely to have the lowest separation propensity in the former group, and the highest separation propensity in the latter. As a result, the compositional change is likely to make the separation elasticity estimate more negative in both groups. Since we find very strong reductions in separations in the former group and much smaller reductions in the latter, the compositional story is unlikely to fully explain these patterns.

3.4 Labor-labor substitution? Effects on employment shares of different demographic groups

An important question in the minimum wage literature concerns whether higher minimum wages induce employers to substitute away from some demographic groups. Previous researchers, such as Neumark and Wascher (2007), find disemployment effects and also report substitution away from some groups of teens. Although we do not find disemployment effects, substitution effects might still be present, affecting the shares of different groups in particular jobs.

To address this question directly we report in Table 6 estimates of the impact of minimum wage increases on outcomes for the demographic groups in our key industry—restaurants. The first column reports the employment share of each of the demographic groups in the restaurant workforce. The second and third columns report the impact of a log point change in the minimum wage on log average earnings (column 2) and share of employment (column 3). Teen workers in restaurants see earnings increases many times greater than adult restaurant workers. Yet, as the table indicates, none of the share coefficients are significant or substantial. The implied share elasticities are modest (under -0.11 in magnitude) and never statistically significant. In all, we do not find any labor-labor substitution along the age and gender categories in our data.

More generally, if minimum wage increases lead to a reallocation of workers, one would expect a short term increase in gross flows (separations and accessions). As we saw in Table 4, the data suggests the opposite—both separations and accessions fall immediately and the short and long run changes are quite similar. This lack of labor-labor substitution sharpens the "anomaly" for the frictionless labor market model's explanation of minimum wage effects, and hence provides an additional reason to consider models with search frictions, which we turn to next.[18]

4 Interpreting the Reduced-Form Findings using a Job Search Model

In this section, we examine whether the combination of a small employment reduction and a relatively larger reduction in the separation rate is predicted by a plausible calibration of a model with labor market search. First, we note that models without on-the-job search can-

[18] Although not shown in the table, the conventional specification does spuriously suggest substitution away from teens and males and toward older workers and females. These results suggest the importance of controls for spatial heterogeneity when testing for substitution effects, just as is the case for employment overall.

not explain why the equilibrium separation rate would fall in response to a minimum wage increase, since in such models the only source of separations is the exogenous destruction of jobs. The most common framework allowing on-the-job search is the job-ladder model. Here we use this canonical model to analytically derive the minimum wage elasticities of employment level and separations. Then we use a standard calibration of the model—following Hornstein et al. (2011)—to ask the following question: What does such a calibrated model predict regarding minimum wage elasticities, and how do the predictions compare with our empirical findings?

In the job-ladder model, offers arrive to unemployed workers at the rate λ, who accept the offer if the wage is above some reservation wage w^*. Once employed, exogenous job destructions occur at the rate σ. Employed workers also engage in on-the-job search, and offers arrive to them at the rate $\lambda_e = \phi \cdot \lambda$, where ϕ is a parameter capturing the relative efficiency of on-the-job search. Employed workers always take a higher wage offer. Here we make no assumptions about the nature of the wage offer distribution. Our results below do not depend on the nature of wage determination—such as bargaining or wage-posting—or the distribution of firm characteristics that may underlie the wage offer distribution.

As is well known, the flow-balance between unemployment and employment, $\lambda(1-e) = \sigma e$, implies that the employment rate is only a function of the ratio $\kappa = \frac{\lambda}{\sigma}$:

$$e = \frac{\lambda}{\lambda + \sigma} = \frac{\kappa}{1 + \kappa} \tag{3}$$

It is also well known (e.g., Nagypal 2005, Hornstein et. al 2011) that the mean total separation rate is equal to:

$$E(s) = \frac{\sigma \left(\lambda_e + \sigma\right) \ln \left(\frac{\sigma + \lambda_e}{\sigma}\right)}{\lambda_e} = \frac{\sigma \left(1 + \phi\kappa\right) \ln \left(1 + \phi\kappa\right)}{\phi \cdot \kappa} \tag{4}$$

where the mean separation rate equals the job-to-job transition rate plus the exogenous job destruction rate (σ). Equation (4) shows that the mean separations rate is solely a function of σ and λ_e. In other words, while the employment rate depends on the offer arrival rate for the unemployed, the separation rate depends on the offer arrival rate for those who already have a job.

How does an increase in the minimum wage affect these two rates—employment and separation? We analytically derive these two minimum wage elasticities by taking logs and differentiating equations (3) and (4) with respect to the minimum wage, $\underline{w}$, taking advantage of the fact that minimum wage affects the employment and the separation rates only through its effect on the offer arrival rate $\lambda(\underline{w})$, and hence $\kappa(\underline{w})$—which we now write explicitly as functions of $\underline{w}$. Here we utilize the assumption that ϕ is a constant: the relative efficiency of on-the-job search is not affected by the minimum wage.[19]

$$\frac{d\ln e}{d\ln \underline{w}} = \kappa'(\underline{w}) \cdot \underline{w} \cdot \left(\frac{1}{\kappa(\underline{w})} - \frac{1}{\kappa(\underline{w})+1} \right) = \frac{\kappa'(\underline{w})\underline{w}}{\kappa(\underline{w})} \cdot \left(\frac{1}{(\kappa(\underline{w})+1)} \right) \tag{5}$$

$$\frac{d\ln E(s)}{d\ln \underline{w}} = \kappa'(\underline{w}) \cdot \underline{w} \cdot \phi \left[\frac{1}{1+\phi\kappa(\underline{w})} + \frac{1}{(1+\phi\kappa(\underline{w}))(\ln(1+\phi\kappa(\underline{w}))} - \frac{1}{\phi\kappa(\underline{w})} \right]$$

$$\frac{d\ln E(s)}{d\ln \underline{w}} = \frac{\kappa'(\underline{w}) \cdot \underline{w}}{\kappa(\underline{w})} \cdot \left[\frac{\phi\kappa(\underline{w})}{(1+\phi\kappa(\underline{w}))(\ln(1+\phi\kappa(\underline{w}))} - \frac{1}{1+\phi\kappa(\underline{w})} \right] \tag{6}$$

The term $\frac{\kappa'(\underline{w})\underline{w}}{\kappa(\underline{w})}$ in equations (5) and (6) represents the elasticity of the offer arrival rates with respect to the minimum wage, since σ is unaffected by $\underline{w}$. For instance, a higher minimum wage may push some firms out of the market and thereby reduce the offer arrival rates (as in Bontemps, Robin and van den Berg 1999, 2000). Or in a matching model, employers may post fewer vacancies due to higher wages.

The offer arrival elasticity affects both the employment rate and separations rate: the sharper the drop in offer arrivals, the larger is the fall in employment and separations. However, the ratio of the two elasticities, i.e. $\frac{\frac{d\ln e}{d\ln \underline{w}}}{\frac{d\ln E(s)}{d\ln \underline{w}}}$, does not depend on the $\kappa'(\underline{w})$ term:

$$\frac{d\ln e/d\ln \underline{w}}{d\ln E(s)/d\ln \underline{w}} = \frac{\frac{1}{1+\kappa(\underline{w})}}{\frac{\phi\kappa(\underline{w})}{(1+\phi\kappa(\underline{w}))\ln(1+\phi\kappa(\underline{w}))} - \frac{1}{1+\phi\kappa(\underline{w})}} \tag{7}$$

$$= \frac{u^*}{\left(\frac{\sigma}{E(s^*)} - \frac{\sigma}{\sigma+\phi\lambda^*} \right)} = \frac{u^*}{\left(\frac{\phi\lambda^*}{\sigma+\phi\lambda^*} - \frac{E(s^*)-\sigma}{E(s^*)} \right)} \tag{8}$$

This is a novel result—the ratio of these two elasticities in equation (7) is a function only of κ and the relative efficiency of on-the-job search, ϕ. By multiplying the numerator and the denominator of equation (7) by σ and rearranging terms, the ratio of the elasticities can also be expressed as a ratio of equilibrium quantities to provide intuition behind this result. The numerator in equation (8) is the equilibrium unemployment rate, u^*. The denominator is the difference between (1) the job-to-job share of separations for workers

[19] This assumption is satisfied in a wide variety of models including Burdett and Mortensen (1998), Bontemps, Robin and van den Berg (1999, 2000). Moreover, it is satisfied in matching models with on-the-job search in which search by the employed and the unemployed are linear substitutes (Petrongolo and Pissarides 2001). In that case, the measure of matches $M = m\left(u + \phi e, v(\underline{w})\right)$, where $v(\underline{w})$ is the measure of vacancies posted; the offer arrival rates are $\lambda_e(\underline{w}) = \phi\frac{M}{u+\phi v(\underline{w})}$ and $\lambda_u(\underline{w}) = \frac{M}{u+\phi v(\underline{w})}$ for the employed and unemployed, respectively.

earning the lowest wage, $\frac{\phi\lambda^*}{\sigma+\phi\lambda^*}$, and (2) the job-to-job share of separations for the workforce as a whole, $\frac{E(s^*)-\sigma}{E(s^*)}$. The difference between these two shares will be greater precisely when there is more frictional wage inequality, when workers at the lowest wage job are less likely to stay at their job as compared to the workforce as a whole.[20] Overall, the ratio of the employment and the separation rate elasticities will be small in magnitude when the initial unemployment rate is low as compared to the dispersion in job-to-job transitions (which in turn reflects frictional wage inequality).

Equation (7) also allows us to answer the following question: what does a plausible calibration of κ and ϕ predict in terms of the relative magnitudes of the employment and separation rate elasticities? We closely follow Hornstein et al. (2011) in calibrating both of these parameters using cross-sectional flows between employment and unemployment, as well as flows between jobs. Drawing upon a number of recent studies that use the SIPP or the CPS, Hornstein et al. estimate that monthly job-to-job flows lie between 0.022 and 0.032, with an average of 0.027. Drawing upon Shimer (2007), they estimate that the monthly exogenous job destruction rate, σ, equals 0.03. The ratio of monthly job flows to the monthly separation rates is therefore around 0.9. Using 0.9 as the left hand side value ($E(s)$) in equation (4) above, we can solve for $\phi\kappa = \frac{\lambda_e}{\sigma}$ to obtain a value of 3.30. Recall that λ equals the monthly job-finding rate out of unemployment, which, based also upon Shimer (2007), Hornstein et al. take to be 0.43 (43 percent). This value of λ implies that $\kappa = \frac{\lambda}{\sigma} = \frac{0.43}{.03} = 14.33$. We can also now calculate the relative efficiency of on the job search $\phi = \frac{\frac{\lambda_e}{\sigma}}{\frac{\lambda}{\sigma}} = \frac{3.30}{14.33} = 0.23$. Moreover, while these estimates are based on the overall workforce, Nagypal (2008) shows that the ratio of job-to-job flows to overall separations for 16-19 year olds is quite similar to that of the overall workforce (even though both of the separation rates are higher for younger workers).

What does this calibration using cross-sectional flows suggest about the relative magnitudes of the two minimum wage elasticities? Can it rationalize a relatively small employment effect and a larger reduction in the separation rate? Comparing the empirical ratio of the two minimum wage elasticities to the theoretical ratio of equation (7) evaluated at the calibrated parameter values to the empirical one provides a test of an overidentifying restriction of the model. The steady state flows used to calibrate the relevant model parameters (κ, ϕ) have further testable implications about how those flows respond to an exogenous minimum wage shock.[21]

[20] This gap between the mean versus minimum rates of job-to-job transitions has obvious parallels with the mean to minimum wage ratio discussed in Hornstein et al. (2011).They are both reflections of frictional wage inequality.

[21] Our approach implicitly assumes that the minimum wage elasticities are measuring changes in steady state flows, as opposed to possible transitional dynamics. This assumption is supported by the evidence in Table 3 that the accession and separation elasticities are quantitatively similar; and that the short and long

Substituting the calibrated values $\phi = 0.23$ and $\kappa = 14.33$ into equation (7), we find:

$$\frac{d\ln e/d\ln \underline{w}}{d\ln E(s)/d\ln \underline{w}} = 0.22 \tag{9}$$

From our empirical results (Table 3), we calculate the ratio of these same two elasticities to be 0.18 for teens and 0.23 for restaurant workers. (Recall that the separation *rate* elasticity is equal to the separations elasticity less the employment elasticity). We find, in other words, that standard calibrations of the job-ladder model using cross-sectional flows suggest relative magnitudes of the two elasticities that are virtually identical to our empirical findings—with a separation rate elasticity that is roughly five times as large as the employment elasticity. As Hornstein et al. show, the same calibration of the job-ladder model can also explain a moderate extent of frictional wage inequality, suggesting a mean-to-minimum (Mm) wage ratio of 1.22.[22]

Overall, these findings are consistent with the idea that an increase in the minimum wage reduces frictional wage inequality and hence job-to-job transitions. We stress that our evidence regarding the importance of search frictions is based on the *relative* magnitudes of the employment stock and flow elasticities. This result contrasts with the usual argument, which has used a finding of small disemployment effect itself as evidence for the importance of search frictions and monopsony. By considering additional margins such as separations, we are able to provide new evidence regarding whether search friction can help explain the effects of minimum wages on labor market outcomes.

5 Discussion and Conclusion

The contributions of this paper are twofold. First, we provide minimum wage elasticities of earnings, employment stocks and employment flows for teens as well as for a high impact industry—restaurants. Second, we show that the relative magnitudes of the employment and separation rate elasticities are very close to what one would expect from a standard calibration of a model with search frictions and on-the-job search. Our approach allows us to assess the importance of search frictions in the low-wage labor market, especially in mediating the effects of minimum wage increases.

Our border discontinuity design shows that even though teen and restaurant employ-

run elasticities in Table 4 are statistically indistinguishable.

[22] The 1.22 estimate for the Mm ratio is based on a calibration in which the relative value of unemployment benefits to the average wage is 0.4. The Mm estimate climbs to as high as 1.56 for smaller relative values of unemployment benefits or additional disutility from unemployment. Although beyond the scope of this paper, allowing for additional margins such as endogenous search intensity produces more realistic Mm ratios and can also rationalize positive employment effects from minimum wage increases.

ment stocks remain stable in response to a minimum wage increase, employment flows fall substantially. Average separations, hires and turnover rates decline significantly among teen workers and restaurant establishments. These changes occur within three quarters of the minimum wage increase and they persist. Our data also permit us to test directly whether the absence of an employment effect in the restaurant sector simply reflects the substitution of older workers for teens. We do not detect any such labor-labor substitution in restaurants in response to minimum wage increases with respect to age and gender.

To interpret these results, we show that the canonical job-ladder model contains remarkably clear predictions about the relative magnitudes of the minimum wage elasticities of employment and separations. The combination of a small effect on the employment level along with sizable reductions in the average separation rate equals exactly what is suggested by standard calibrations of a job-ladder model. Both the calibrated model and the empirical estimates suggest a ratio of one to five for the relative magnitudes of these two elasticities.

Overall, our results provide new evidence on an old question: are search frictions important for understanding the effects of minimum wages? The combination of our reduced-form empirical evidence and the results using a calibrated model of job search suggests an affirmative answer. By compressing the wage distribution, minimum wage increases can reduce the churning that characterizes the low-wage segment of the labor market. As a consequence, a properly designed minimum wage policy has the possibility of improving the structure and functioning of the low wage labor market without substantially affecting employment.

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Table 1
Comparing Samples in the QWI Data

	All Counties Sample			Contiguous County Pair Sample		
	Total Private Sector	**All Teens**	**Restaurants**	**Total Private Sector**	**All Teens**	**Restaurants**
All						
Monthly Earnings	2,326	457	789	2,323	453	782
(st. dev)	*585*	*134*	*238*	*587*	*131*	*221*
Employment	40,564	1,383	2,945	38,055	1,290	2,748
(st. dev)	*143,386*	*3,952*	*9,558*	*125,310*	*3,272*	*8,097*
Hire rates	0.224	0.686	0.440	0.230	0.697	0.446
(st. dev)	*0.092*	*0.382*	*0.194*	*0.091*	*0.369*	*0.189*
Separation rates	0.217	0.557	0.432	0.222	0.562	0.440
(st. dev)	*0.072*	*0.264*	*0.149*	*0.074*	*0.225*	*0.181*
Turnover Rate	0.220	0.618	0.433	0.220	0.618	0.433
(st. dev)	*0.074*	*0.327*	*0.158*	*0.073*	*0.350*	*0.159*
Tenure > 1 quarter						
Monthly Earnings	2,548	562	939	2,537	557	929
(st. dev)	*650*	*176*	*276*	*647*	*175*	*269*
Employment	35,139	952	2,248	32,992	888	2,095
(st. dev)	*125,217*	*2,761*	*7,558*	*109,714*	*2,269*	*6,366*
Hire rates	0.104	0.308	0.200	0.106	0.308	0.204
(st. dev)	*0.034*	*0.085*	*0.056*	*0.036*	*0.085*	*0.059*
Separation rates	0.102	0.221	0.198	0.104	0.224	0.204
(st. dev)	*0.041*	*0.082*	*0.081*	*0.046*	*0.082*	*0.087*
Turnover Rate	0.103	0.265	0.202	0.103	0.266	0.203
(st. dev)	*0.028*	*0.077*	*0.084*	*0.027*	*0.078*	*0.081*
Number of counties		2,960			1,063	
Number of county pairs		NA			1,169	

Notes. Sample means are reported for all counties in the US and for all contiguous border county pairs with a full balanced panel of observations. Standard deviations are shown in italics below the means. Monthly earnings are in nominal dollars. Turnover rates are quarterly. Sample sizes vary by demographic group, industry and tenure and ranges from 28,000 to 66,112. Sample period is from 2001Q1 through 2008Q4. Data Source: Quarterly Workforce Indicators.

Table 2
Descriptive Statistics

Dependent Variable	Teens	Young Adults	Adults 25 +	Females	Males	Restaurant Workers
All						
Monthly Earnings	453	952	2,559	1,784	2,856	782
	131	*220*	*594*	*452*	*741*	*221*
Employment	1,290	2,055	40,679	18,779	19,276	2,748
	3,272	*5,736*	*126,186*	*61,549*	*63,831*	*8,097*
Hiring Rate	0.697	0.551	0.189	0.219	0.243	0.446
	0.369	*0.234*	*0.061*	*0.089*	*0.104*	*0.189*
Separation Rate	0.562	0.524	0.189	0.211	0.234	0.440
	0.225	*0.165*	*0.057*	*0.072*	*0.084*	*0.181*
Turnover Rate	0.618	0.527	0.182	0.209	0.232	0.433
	0.350	*0.169*	*0.055*	*0.071*	*0.082*	*0.159*
Tenure >1 quarter						
Monthly Earnings	557	1,144	2,774	1,953	3,125	929
	175	*259*	*638*	*508*	*809*	*269*
Employment	888	1,453	37,217	16,337	16,655	2,095
	2,269	*4,104*	*113,957*	*54,021*	*55,753*	*6,366*
Hiring Rate	0.308	0.245	0.091	0.105	0.107	0.204
	0.085	*0.060*	*0.026*	*0.038*	*0.039*	*0.059*
Separation Rate	0.224	0.227	0.092	0.103	0.105	0.204
	0.082	*0.071*	*0.026*	*0.051*	*0.049*	*0.087*
Turnover Rate	0.266	0.237	0.091	0.102	0.104	0.203
	0.078	*0.063*	*0.020*	*0.030*	*0.030*	*0.081*

Notes. Sample means are reported for all contiguous border county pairs with a full balanced panel of observations. Standard deviations are shown in italics below the means. Monthly earnings are in nominal dollars. Turnover rates are quarterly. Teens are of ages 14-18; young adults are of ages 19-24. Sample sizes vary by demographic group, industry and tenure and ranges from 28,000 to 66,112. Sample period is from 2001Q1 through 2008Q4. Data Source: Quarterly Workforce Indicators.

Table 3
Minimum Wage Elasticities for Earnings, Employment Level and Flows

	Teens		Restaurant Workers	
Dependent Variable	(1)	(2)	(3)	(4)
ln Earnings	0.107**	0.161**	0.169***	0.213***
	(0.048)	(0.064)	(0.035)	(0.072)
ln Employment	-0.200***	-0.039	-0.121***	-0.057
	(0.066)	(0.065)	(0.043)	(0.104)
ln Hires	-0.454***	-0.224**	-0.466***	-0.342**
	(0.089)	(0.111)	(0.081)	(0.172)
ln Separations	-0.463***	-0.253**	-0.468***	-0.319**
	(0.096)	(0.102)	(0.076)	(0.133)
ln Turnover Rate	-0.266***	-0.194**	-0.327***	-0.257**
	(0.066)	(0.079)	(0.072)	(0.123)
Controls:				
Common time effects	Y		Y	
Pair-specific time effects		Y		Y

Notes. Sample sizes in regressions range from 46,944 to 59520, depending on sample (due to nondisclosure policy). All regressions include controls for natural log of county population and total private sector employment. Specifications 1 and 2 provide estimates for all teens aged 14-18 regardless of industry. Specifications 3-4 are limited to all workers in the restaurant industry (NAICS 722). All samples and specifications include county fixed-effects. Specifications 1 and 3 include common time period fixed-effects. For specifications 2 and 4, period fixed-effects are interacted with each county-pair. Robust standard errors, in parentheses, are clustered at the state *and* border segment levels for all regressions. Significance levels are indicated by: * for 10%, ** for 5%, and *** for 1%.

Table 4
Minimum Wage Elasticities - Robustness Checks

	Teens					**Restaurant Workers**				
	(1)	(2)	(3)			(4)	(5)	(6)		
Dependent variable			$\ln MW_{t+4}$	$\ln MW_{t}$	$\ln MW_{t-4}$			$\ln MW_{t+4}$	$\ln MW_{t}$	$\ln MW_{t-4}$
ln Earnings	0.193^{***}	0.158^{**}	-0.047	0.105	-0.012	0.193^{**}	0.212^{***}	0.033	0.224^{**}	0.010
	(0.063)	(0.065)	(0.053)	(0.087)	(0.047)	(0.083)	(0.071)	(0.067)	(0.099)	(0.056)
ln Employment	-0.022	-0.039	0.051	-0.012	0.026	-0.038	-0.057	0.069	-0.017	-0.013
	(0.074)	(0.065)	(0.066)	(0.085)	(0.077)	(0.106)	(0.104)	(0.066)	(0.106)	(0.128)
ln Hires	-0.242^{*}	-0.154	-0.052	-0.243^{*}	-0.021	-0.323^{*}	-0.292^{*}	-0.108	-0.429^{*}	-0.058
	(0.133)	(0.100)	(0.096)	(0.144)	(0.149)	(0.193)	(0.169)	(0.115)	(0.220)	(0.196)
ln Separations	-0.277^{**}	-0.193^{**}	-0.061	-0.312^{**}	0.052	-0.316^{**}	-0.294^{**}	-0.025	-0.354^{*}	-0.059
	(0.117)	(0.091)	(0.100)	(0.145)	(0.122)	(0.148)	(0.137)	(0.098)	(0.179)	(0.137)
ln Turnover Rate	-0.212^{**}	-0.129^{*}	-0.118	-0.252^{**}	-0.037	-0.264^{*}	-0.222^{*}	-0.125	-0.347^{**}	-0.047
	(0.093)	(0.070)	(0.092)	(0.112)	(0.107)	(0.147)	(0.118)	(0.109)	(0.173)	(0.151)
Controls:										
County size <2000 sq. mi.	Y					Y				
All priv. sector ln(outcome)		Y					Y			
Lead and lag ln(MW)			Y	Y	Y			Y	Y	Y

Notes. Sample sizes in regressions range from 50,912 to 58,848. All regressions include controls for log of county population and pair-specific time effects. Specifications 1-3 provide estimates for all teens 14-18 regardless of industry. Columns 4-6 are limited to all workers in the restaurant industry (NAICS 722). Columns 1 and 6 restrict the sample to counties of less than 2000 square miles. Columns 2 and 5 include as controls the value of the dependent variable for all workers in the county's private sector (i.e. rather than the group in focus, e.g., teens). Specifications 3 and 6 include a 4-quarter lead and lag in the minimum wage to control for pre-existing trends and delayed effects. All samples and specifications include county pair-specific time effects. Robust standard errors, in parentheses, are clustered at the state *and* border segment levels for all regressions. Significance levels are indicated by: * for 10%, ** for 5%, and *** for 1%.

Table 5
Minimum Wage Elasticities - Effects by Tenure

Dependent Variable	Teens (1)	(2)	(3)	Restaurant Workers (4)	(5)	(6)
		All	*Tenure>1q*		*All*	*Tenure>1q*
Fraction Short-Term (tenure<1q)	-0.026			-0.039**		
	(0.018)			(0.020)		
ln Earnings		0.161**	0.135*		0.213***	0.167**
		(0.064)	(0.068)		(0.072)	(0.071)
ln Employment		-0.039	-0.002		-0.057	0.003
		(0.065)	(0.072)		(0.104)	(0.108)
ln Hires		-0.224**	-0.147		-0.342**	-0.058
		(0.111)	(0.098)		(0.172)	(0.113)
ln Separations		-0.253**	-0.113		-0.319**	-0.028
		(0.102)	(0.073)		(0.133)	(0.119)
ln Turnover Rate		-0.194**	-0.096		-0.257**	-0.148
		(0.079)	(0.087)		(0.123)	(0.108)

Notes. Sample sizes in regressions range from 38,080 to 65,689, depending on sample. All regressions include controls for natural log of county population, total private sector employment and pair-specific time effects. Specifications 1-3 provide estimates for all teens 14-18 in the private sector. Specifications 4-6 provide estimates for all restaurant workers. All samples and specifications include county fixed-effects as well as period fixed-effects interacted with each county-pair. Robust standard errors, in parentheses, are clustered at the state *and* border segment levels for all regressions. Significance levels are indicated by: * for 10%, ** for 5%, and *** for 1%.

Table 6
Labor-Labor Substitution within Restaurants

		Dependent Variable	
	Employment Share	**ln Earnings**	**Employment Share**
Male	0.651	0.182*	0.009
		(0.103)	(0.023)
Female	0.355	0.242***	-0.008
		(0.057)	(0.025)
Teen	0.234	0.404***	-0.024
		(0.085)	(0.021)
Young Adult	0.149	0.300***	0.000
		(0.087)	(0.011)
Adult 25+	0.624	0.101	0.016
		(0.088)	(0.023)

Notes. Column 1 reports the employment share of each demographic group in the overall restaurant workforce. Columns 2 and 3 report the regression coefficient associated with log of the minimum wage. In column 2, the outcome is the log of average earnings; the coefficient is, therefore, the minimum wage elasticity of average earnings. In column 3, the outcomes are the demographic group's share of overall restaurant employment. Teens are of ages 14-18; young adults are of ages 19-24. All regressions include controls for natural log of county population, total private sector employment and pair-specific time effects. Sample sizes in regressions range from 37,504 to 56,736, depending on sample. Robust standard errors, in parentheses, are clustered at the state *and* border segment levels for all regressions. Significance levels are indicated by: * for 10%, ** for 5%, and *** for 1%.