Minimum Wage Shocks, Employment Flows and Labor Market Frictions

Arindrajit Dube*, T. William Lester** and Michael Reich***

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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 by using policy discontinuities at state borders. We find that minimum wages have a sizeable negative effect on employment flows but not on stocks. Separations and accessions fall among affected workers, especially those with low tenure. We do not find changes in the duration of non-employment for separations or hires. This evidence is consistent with search models with endogenous separations.

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

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*Department of Economics, University of Massachusetts, Amherst and IZA. **Department of City and Regional Planning, University of North Carolina, Chapel Hill. ***Department of Economics and Institute for Research on Labor and Employment, University of California, Berkeley.

1 Introduction

While much attention has been paid to the question of how minimum wages affect employment stocks, considerably less attention has been given to their effects on employment flows. In this paper we use a relatively new dataset—the Quarterly Workforce Indicators (QWI)—to estimate the minimum wage elasticities of average earnings, employment stocks and employment flows. The QWI data permit us to estimate the responses of local labor market accession, separation 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 elasticities utilize a border-discontinuity design that eliminates biases from spatial heterogeneity present in many previous minimum wage studies (Allegretto et al. 2013).

We begin by showing that minimum wages have sizeable effects on earning on the two most affected groups: a 10 percent increase in the minimum wage raises average weekly earnings by 2.2 percent for teens and 2.1 percent for restaurant workers. We find striking evidence that separations, 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 higher minimum. For a 10 percent minimum wage increase, turnover rates decline by around 2.0 percent for teens and 2.1 percent for the restaurant workforce. In contrast to our results on employment flows, the impact of minimum wage increases on the employment stock is small: for both teens and restaurant workers our estimated employment elasticities are small in magnitude and they are not statistically distinguishable from zero. In addition, while workers remain at their jobs longer, that does not appear to be true for time spent between jobs. We do not detect changes in the average duration of non-employment spells for those transitioning in and out of jobs (with the caveat that this variable is measured somewhat coarsely). Finally, for the restaurant workforce, we also do not find any evidence of labor-labor substitution with respect to age or gender.

Our finding of reduced employment flows is consistent with models of the labor market with search frictions and endogenous separations that take the form either of transitions to other jobs ("quits") or to non-employment ("layoffs"). One explanation of reduced flows comes from a job-ladder model, in which minimum wages reduce job-to-job transitions by lowering the arrival rate of better paying job offers. We show analytically that in a broad class of job-ladder models, a minimum wage increase affects employment flows relatively more than stocks when there is greater equilibrium dispersion in job-to-job transition rates. Such dispersion stems from frictional wage inequality. In online Appendix A, we show that a calibrated job ladder model predicts relatively larger elasticities for employment flows than for stocks, which is consistent with our evidence.

An alternative explanation suggests that higher minimum wages reduce transitions to non-employment, possibly through reduced layoffs. However, both the canonical Mortensen and Pissarides (1994) model with endogenous separations, and the variant in Pissarides (2000) that incorporates uncertainty about match quality, generate predictions that higher minimum wages should *increase* layoffs, since fewer matches are profitable. This reduced profitability of matches would lead to a higher equilibrium separation rate, contradicting the evidence. Recently, Brochu and Green (2013) have extended the match quality model by adding the condition that match quality is realized after an initial probationary period, and (importantly) that the costs of posting a vacancy are heterogeneous. In this model a minimum wage increase reduces an employer's willingness to lay off workers with lower match values and then search anew. A higher minimum wage raises the costs to hiring a new recruit during the probationary period, thereby reducing the value of the termination option for a current employee. Put differently, search has sunk costs that increase with the minimum wage.

Since most workers who are laid off enter non-employment, the Brochu-Green match quality model implies that minimum wages reduce employment-to-unemployment (EU) transitions. This prediction contrasts with the job-ladder model, which suggests that the reduction in flows takes the form of reduced employment-to-employment (EE) transitions, i.e., job quits. Unfortunately, the QWI dataset does not allow us to directly measure EE versus EU transitions. However, the consistency between the predictions of the job-ladder model calibrated using cross-sectional flows and the empirical findings on the relative magnitudes of the employment and separations elasticities provide evidence consistent with the job-ladder model. At the same time, the biggest reduction in separation occurs in low-tenure jobs, suggesting that the match quality effect may be important as well.

Our paper relates to four 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 in continuing firms fall substantially after a youth-specific minimum wage increase in Portugal. Since the share of teens hired in new firms also falls, overall teen employment does not change substantially. Portugal and Cardoso find that the teen share of separations fell by about 15 percent in response to a 50 percent increase in the minimum wage—an implicit separations elasticity of -0.3. These findings are similar to ours: we estimate a separations elasticity of -0.23 for teens. However, since their estimation relies on a national-level policy change, Portugal and Cardoso's paper is more like a single case study, raising concerns about both the identification strategy and inference that are not issues for us. In particular, since Portugal and Cardoso's primary control group consists of all adults in the country, any age-specific shocks affecting the national labor market could confound their estimates. In contrast, we are able to use 196 different minimum wage changes with geographically proximate control groups to account for a rich array of heterogeneous trends. Additionally, we provide further evidence on the channels by explicitly showing the implications of a general job-ladder model, which subsumes the Burdett and Mortensen (1998) model that Portugal and Cardoso invoke to explain their results.

In a paper written concurrently with ours, Brochu and Green (2013) use Canadian data and find that hires, quits and layoffs of low-skilled teens decline in the year after a minimum wage increase. They find that layoffs account for a larger proportion of the reduction, although the magnitudes depend on how quits are defined. Most relevant to our paper, they find an overall separation elasticity of between -0.27 and -0.35 for teens, which is similar to what we find here (-0.23). Like us, Brochu and Green also find that the reduction in separations is concentrated among lower tenure workers. With a more inclusive definition of quits that uses job-to-job transitions, they find that quits can explain close to 40 percent of this reduction.

Brochu and Green differ from from us in finding a more negative impact on employment of low-skilled teens, with an elasticity of -0.25. (However, for low-skilled adults, they find reductions in separations but not employment.) Unfortunately, the small number of Canadian provinces (and hence policy clusters) raises 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—trends that we show are quite important in the U.S. context. For example, our estimates, when uncorrected for spatial heterogeneity, produce teen disemployment estimates similar to theirs, but we show that such estimates are driven by confounders. Additionally, we use administrative data on separations from the near universe of employers, which substantially reduces measurement error problems that arise in self-reported data from the household surveys used by Brochu and Green. Overall, however, we regard our findings on employment flows as quite complementary with the limited international evidence: minimum wages tend to have much larger impacts on employment flows than on employment levels.

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 in the 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 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 result, together with our estimates of these flows, tells us about the extent and nature of search frictions in the labor market.

Third, a number of papers use structurally-estimated search models to study minimum wage effects. These papers 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. In contrast, we estimate the reduced-form effects of minimum wages on employment stocks and flows using exogenous policy variation, and compare our estimates with the predictions from alternative models. The comparison of our estimates to the predictions from a calibrated job-ladder model constitutes a test of overidentifying restrictions, thereby providing new evidence on the model's ability to fit the data. We find that the job ladder model can fit some, but not all, of the moments estimated using minimum wage variation.

Fourth, we make both substantive and methodological contributions to the literature on minimum wage effects on employment rates. Dube, Lester and Reich (2010) used a cross-border design to estimate the impact of minimum wages on restaurant employment. Here we use a similar border discontinuity approach to additionally study teens—the most commonly studied group in the literature. This approach constitutes a substantial improvement upon our previous estimates for teens, which used the Current Population Survey data and coarser

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

[2] Bontemps, Robin and van den Berg (1999, 2000) and Flinn and Mabli (2009) all consider models with on-the-job search, one of the key mechanisms considered in this paper for explaining the finding that minimum wage increases reduce separations. We use the Bontemps et al. (1999, 2000) model in our Appendix A to derive the effect of minimum wages on employment stocks and flows.

spatial controls (Allegretto, Dube and Reich 2011). Methodologically, we provide additional support for the border discontinuity design by directly showing that cross-border contiguous counties are substantially more similar in levels and trends of covariates than counties farther away.[3] We find that even among border county pairs, counties with more proximate centroids are more similar to each other, as measured by covariates. For this reason, we implement a further refinement by limiting attention to county pairs whose centroids are within 75 miles of each other. As we describe in online Appendix B, this threshold is selected by a randomization inference procedure using placebo laws that seeks to minimize the mean squared error of the estimator.

The rest of the paper is structured as follows. In Section 2 we discuss alternative channels through which minimum wages can affect separations. We present our identification strategy, dataset and sample in Section 3 and report our empirical findings in Section 4. Section 5 evaluates alternative theoretical channels in light of the empirical evidence and quantifies the likely importance of job-to-job and employment-to-nonemployment transitions in explaining the results. We present our conclusions in Section 6.

2 Minimum wage and separations: Alternative channels

Since search models examine how worker-firm matches are created and dissolved, they provide a natural framework for understanding the impact of policies on separations. For separations to occur endogenously, a search model needs the match value or the outside options available to workers or firms to vary over time. Two popular classes of models have such endogenous separations. The first is the job-ladder model, in which workers search both on and off the job. Here the arrival of a superior offer affects a worker's willingness to stay at her current job; therefore, changes in the offer wage distribution can affect the steady state rate at which workers leave their jobs to take better ones. In the second type of model, match quality is uncertain. Over time, as more information about the match value is revealed, employers and workers decide whether to stay in their current match, or to dissolve the match and search anew. Since the job-ladder model predicts a reductions in quits in response to a higher minimum wage, it predicts a lower rate of job-to-job (EE) transitions. In contrast, in the match quality model, reduced layoffs lead to lower rates of

[3] In a recent paper, Neumark, Salas and Wascher (2013) argue that neighboring counties do not comprise better controls. Dube, Lester and Reich (2010) did show that comparing border pairs was a stronger research design than the canonical two-way fixed effects model as the former did not exhibit pre-existing trends prior to treatment. However, we did not directly show covariate similarity of contiguous as opposed to other counties—but which we do address here. We respond further in Allegretto, Dube, Reich and Zipperer (2013).

job-to-unemployment (EU) transitions. In this section we discuss these two classes of models in greater detail. Since Brochu and Green (2013) extensively analyze the match quality case, in this paper we devote more attention to the job-ladder case and its implications.

In the job-ladder model, workers search both on and off the job, with possibly different search efficiencies: offers arrive at a rate λ to the unemployed and $\lambda_e = \phi \cdot \lambda$ to the employed, where ϕ is an exogenous parameter capturing the relative efficiency of on-the-job search. Workers move if they receive a higher wage than at their current job, w. If they are unemployed, they accept a job if it pays above the reservation wage, b, assumed to be below a binding minimum wage, $\underline{w}$. Given an offer wage distribution $F(w)$, this occurs at the rate $\lambda_e \cdot [1 - F(w)]$. In addition, exogenous job destructions occur at the rate σ. Therefore, the total separation rate at a job equals the sum of the exogenous job destruction rate σ and the rate at which workers leave to take better paying jobs, $\lambda_e \cdot [1 - F(w)]$. This latter term, reflecting EE transitions, constitutes the channel through which a policy such as the minimum wage affects the separation rate.

In online Appendix A, we show that in any job ladder model, the ratio of the elasticities of the mean separations and employment rates with respect to the minimum wage can be written as a function of equlibrium unemployment, mean separations and offer arrival rates:

$$\frac{d\ln e/d\ln \underline{w}}{d\ln E(s)/d\ln \underline{w}} = \frac{u}{\left(\frac{\sigma}{E(s)} - \frac{\sigma}{\sigma+\phi\lambda}\right)} = \frac{u}{\left(\frac{\phi\lambda}{\sigma+\phi\lambda} - \frac{E(s)-\sigma}{E(s)}\right)} \tag{1}$$

The numerator in equation (1) is the equilibrium unemployment rate, u. The denominator equals the difference between (1) the job-to-job share of separations for workers 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, i.e., when workers at the lowest wage jobs are less likely to stay at their jobs as compared to the workforce as a whole.[4] 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). This is a novel result—the relative magnitudes of the employment stock and flow elasticities is a function only of the equilibrium offer arrival rate λ, the job destruction rate σ, and the relative efficiency of on-the-job search, ϕ.

The determination of the equilibrium offer arrival and unemployment rates depend on the specific features of the model. In online Appendix A, we use the wage posting model of Bontemps, Robin, and van den Berg (1999, 2000) as an illustration, and express the ratio of

[4] 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). Both are reflections of frictional wage inequality.

the two elasticities in equation (1) as a function of the primitives of the model. However, we stress that equation (1) holds for the broad class of job-ladder models regardless of the details regarding wage setting and entry. This result is useful because it suggests that the effects of a minimum wage policy change on the relative magnitudes of the employment stock and flow elasticities depend only on parameters that can all be calibrated using cross-sectional flows. In the online Appendix A, we also provide a calibration of the job-ladder model using cross-sectional employment flows from the Current Population Survey (see online Appendix Table A1). Our calibration predicts a minimum wage elasticity of employment that is less than half (45 percent) as large as the separation elasticity when using the teen employment flows; and one-fourth (25 percent) as large when using cross-sectional flows from the restaurant workforce. In other words, a calibrated job-ladder model predicts that minimum wages have a much larger effect on gross worker flows than on employment rates. We stress again that these findings apply to a broad class of models with on-the-job search, including the well-known Burdett and Mortensen (1999) and Bontemps et al. (1999, 2000) models with wage posting and the Pissarides (2000) or Flinn and Mabli (2009) models with on the job search and bargained wages.

An alternative account focuses on changes in the quality of a match. However, standard models with stochastic match quality, such as Mortensen and Pissarides (1994), or the Pissarides (2000, Ch. 2) extension to uncertain match quality, actually imply the "wrong" prediction on separations. As shown in Pissarides (2000), when match quality (x) is unknown until the end of the probationary period, the firm's choice of retaining matches has a reservation value property such that only matches $x > x^*$ are kept. With a binding minimum wage, it is straightforward to show that $x^* = w_M$. This result in turn implies that a rise in the minimum wage deems more matches unprofitable *ex post* from the firm's perspective—thereby leading to more terminations ("layoffs").

Since the direct effect of a minimum wage increase goes in the "wrong" direction, one needs an indirect effect in the opposite direction to produce a reduction in separations. That may occur through the effect of the policy on the firms' outside option. If a higher minimum wage makes vacancies even less profitable than a marginal match, firms may opt to produce with the existing worker as long as the profits are positive.

To operationalize this logic, Brochu and Green (2013) modify the model by adding heterogeneous vacancy costs faced by employers: potential employers first draw a stochastic vacancy cost prior to posting the vacancy. Once they fill the vacancy, they pay workers the minimum wage during a probationary period. Subsequently they learn the true match value, and then decide whether to terminate or continue the match. In this setting, the minimum wage alters the outside option of incumbent employers who have the knowledge of the true match quality: they have already paid the "sunk cost" of discovery. For the marginal incumbent firm, a rise in the minimum wage reduces the asset value of a vacancy as compared to the current match, since it would have to re-pay the (higher) costs of the probationary period. As a result, x^* may actually fall with the minimum wage, and employers lay off fewer workers, and correspondingly have fewer hires. Of course, the direct effect may dominate, and layoffs could rise, as in Pissarides (2000). Therefore, whether separations actually fall is ambiguous in the model, and depends on parameter values. Similarly, the effect on employment rate is also ambiguous, and depends on the extent to which vacancy creation is diminished. As a corollary, since the employer learning occurs early in the new employment relationship (at the end of the probationary period), the Brochu-Green model predicts that layoffs are tenure duration-dependent.

Both classes of models are consistent with declines in employment flows that substantially exceed changes in employment levels. As our brief comparison of the two models suggests, the effects of minimum wages on separations occurs through different channels in each model. In the job-ladder model, quits fall with a higher minimum wage, leading to lower EE rates; in the match quality learning model, layoffs fall, causing the EU rate to fall.

3 Research design and data sources

3.1 Identification strategy

Minimum wage policies are not randomly distributed across U.S. states. Allegretto, Dube, Reich and Zipperer (2013) show that states that were more likely to increase their minimum wage over the past two decades were also systematically different in other labor market attributes. They tended to experience greater reductions in routine-task occupations, higher growth in upper-half wage inequality, and sharper economic downturns. And they were highly correlated with Democratic party vote share, which suggests the possibility of other confounding policy changes.

In this paper we use a border discontinuity design to account for potential confounds, as proposed and implemented in Dube, Lester and Reich (2010). This approach, which 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.[5] 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

[5]Figure B2 provides a map of the border sample, and indicates which pairs have some variation in minimum wages. It also identifies the pairs used in our estimation sample, with county centroids no more than 75 miles apart.

generally affected by the same idiosyncratic local trends and experience macroeconomic shocks at roughly the same time. In section 3.5, we show that contiguous counties are substantially more similar in levels and trends of covariates. The border discontinuity design also offers a way to address concerns about policy endogeneity. Minimum wage policies may react to shocks affecting the whole state, not just those affecting counties right at the border. Therefore, policy differences within cross-border pairs are unlikely to reflect endogeneity concerns that may severely bias studies using state-level variation.

More formally, consider the following data generating process:

$$y_{it} = \alpha + \beta \ln(MW_{s(i)t}) + \Gamma \mathbf{X_{it}} + \epsilon_{it} \tag{2}$$

Here y_{it} refers to the dependent variable—which could be the log of earnings, employment, separations, hires, or the turnover rate—in county i, at time t, for each of the specific industry or demographic groups (e.g., restaurant workers or teens). The minimum wage variable $\ln(MW_{s(i)t})$ in a given county i is set at the level of the state, $s(i)$, and β is the primary coefficient of interest. In addition, there is a vector of time varying controls, $\mathbf{X_{it}}$, which include the natural log of total private sector employment and population in each county.[6]

We will estimate equation (2) using a panel of cross-state contiguous county pairs. We note that a given county i can be part of multiple pairs if it has more than one adjacent county j across the state line. The dataset stacks observations from counties by each pair formed by i, j. Therefore, each observation is indexed by ijt, where i is the primary county for the observation, j denotes county to which this replicate of county i is paired with, and t, as before, denotes time. While we use the indexing convention of ijt for relevant variables to clarify that the county pairs nature of the dataset, for all original variables z, the values for the same county i are the same irrespective of the county j that they are paired with: i.e., $z_{ijt} = z_{ij't} = z_{it}$ for all j, j'.

Estimation of equation (2) is complicated due to heterogneity across time and place that are not captured by observables, which makes it quite likely that $E(\ln(MW_{s(i)t}), \epsilon_{it}) \neq 0$. The first, conventional, approach to estimating β in equation (2) includes two-way (county and time) fixed effects as controls to account for such unboserved heterogeneity. A regression with such two-way fixed effects is identical to a regression using de-meaned data, a formu-

[6]Together, these two variables represent a flexible formulation of the employment-to-population ratio, which captures the state of the overall local labor market conditions. The overall private sector population and employment are unlikely to be affected by a policy targeting a small fraction (typically between 5 and 10 percent) of the private sector workforce. As a result, their inclusion is unlikely to block any legitimate causal pathways. We use county-level Census Bureau population data, which are reported on an annual basis.

lation that is useful when comparing it with our preferred border discontinuity design. For any variable z, define the de-meaned variable $\tilde{z}_{ijt} = z_{it} - \overline{z}_i - \overline{z}_t + \overline{z}$. Here $\overline{z}_i$ is the mean of z specific to county i across all time periods; $\overline{z}_t$ is the mean of z across all observations at time t; while $\overline{z}$ is the overall sample mean. The model with a county and time fixed effect can be estimated represented as follows (see, e.g., Conley and Taber 2011)[7]:

$$\tilde{y}_{ijt} = \alpha + \beta \ln(M\tilde{W}_{s(i)t}) + \Gamma \tilde{\mathbf{X}}_{\mathbf{it}} + \tilde{\epsilon}_{ijt} \tag{3}$$

The identifying assumption behind the consistency of the two-way fixed effects estimator, $\hat{\beta}_{FE}$, is that purging the data of county-specific and common time-specific fixed effects is sufficient for removing confounds, ruling out time-varying heterogeneity with the assumption that $E(\ln(M\tilde{W}_{s(i)t}), \tilde{\epsilon}_{ijt}) = E(\ln(M\tilde{W}_{s(i)t}), \tilde{\epsilon}_{it}) = 0$. However, minimum wage policies in the U.S. tend to exhibit strong geographical clustering, and there are a myriad of factors affecting the low wage labor market (other than the minimum wage) that vary across U.S. regions. By ignoring such spatial confounds, the two-way fixed effects estimator may be subject to an omitted variables bias. Existing research shows that, indeed, the two-way fixed effects model often attributes 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, Figure 4 of Dube, Lester and Reich (2010) shows that employment levels and trends are negative prior to the minimum wage change using a conventional two-way fixed effects specification. [8]

An alternative, and much less restrictive, strategy restricts identifying variation to geographically proximate units which are more likely to share common economic shocks. First, for any variable, z_{ijt}, we define the locally-differenced value $\tilde{\tilde{z}}_{ijt} = z_{it} - \overline{z}_{ijt} - \overline{z}_i + \overline{z}$. Here the object $\overline{z}_{ijt}$ is the mean of z at time t within the pair p formed by counties i, j.[9] The

[7]This representation holds exactly when all county panels are balanced, which we assume for expositional purpose, but this is not assumed in the actual estimation. When estimating this equation in practice, we include time and county dummies as independent variables in the regression, which are efficiently estimated using a conjugate gradient method in STATA using the `twfe` command written by Nikolas Mittag.

[8]Evidence of bias in estimates from a two-way fixed effects model due to spatial heterogeneity is also presented in Addison, Blackburn and Cotti (2009, 2012), Allegretto, Dube and Reich (2011), and Allegretto, Dube, Reich and Zipperer (2013) for the case of low-wage employment; and in Dube (2013) for the case of family incomes, who shows that the two-way fixed effects model fails a variety of falsification tests. Allegretto, Dube, Reich and Zipperer (2013) also show in detail that spatial controls are able to eliminate confounding pre-trends. That paper also shows that the synthetic control estimator is more likely to pick nearby donor states as controls based on matching pre-intervention outcomes and covariates, thereby providing additional evidence to address concerns in Neumark, Salas and Wascher (2013) about the desirability of local or regional controls.

[9]Recall that a county i may be part of multiple pairs, and hence in the dataset it appears as many times as it is paired with another contiguous cross-border county. For this reason, in general, $\overline{z}_{ijt} \neq \overline{z}_{ij't}$. Also note that reversing the order of the indices is irrelevant when forming the pair-specific mean, so that $\overline{z}_{ijt} = \overline{z}_{jit}$.

border discontinuity estimating equation can then be written as follows:

$$\tilde{\tilde{y}}_{ijt} = \alpha + \beta \ln(M\tilde{\tilde{W}}_{s(i)t}) + \Gamma \tilde{\tilde{\mathbf{X}}}_{\mathbf{ijt}} + \tilde{\tilde{\epsilon}}_{ijt} \tag{4}$$

In contrast to the regression in equation (3), the preferred border discontinuity equation (4) consists of a series of localized comparisons *within* contiguous county pairs, since the differencing in equation (4) washes out all the variation within pairs. Hence, this second strategy 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, p: $E(\ln(M\tilde{\tilde{W}}_{s(i)t}), \tilde{\tilde{\epsilon}}_{it}) = 0$. This assumption can also be written as: $E(\ln(M\tilde{W}_{s(i)t}), \tilde{\epsilon}_{ijt}|i, j \in p) = 0$, which clarifies that it is much weaker than the assumption justifying the two-way fixed effects, $E(\ln(M\tilde{W}_{s(i)t}), \tilde{\epsilon}_{ijt}) = 0$. The consistency of $\hat{\beta}_{FE}$ assumes that $\ln(M\tilde{W}_{s(i)t})$ and $\tilde{\epsilon}_{ijt}$ are uncorrelated generally, while the consistency of $\hat{\beta}_{BD}$ only assumes it to hold within neighboring pairs, p, where counties are more likely to face similar shocks.

A simple example of a data generating process can demonstrate when (4) provides a consistent estimate while (3) does not. For the ease of exposition, in this example we assume all counties are matched exactly once in the sample, so counties i and i' are only matched with each other an no other county. Consider an error components model in which $\epsilon_{it} = u_{it} + e_{it}$, where e_{it} is a classical disturbance term, with $E(\ln(MW_{s(i)t}, e_{it}) = 0$. However, there is a also confounder u_{it} such that $E(\ln(MW_{s(i)t}, u_{it}) \neq 0$. Moreover, it is the case that demeaning the data by time and county is not sufficient to remove the bias from the presence of the u_{it} component, so that: $E(\ln(M\tilde{W}_{s(i)t}), \tilde{\epsilon}_{it}) = E(\ln(M\tilde{W}_{s(i)t}), \tilde{u}_{it}) \neq 0$. As a result, $\hat{\beta}_{FE}$ estimated using equation (3) is inconsistent.

Consider further the case in which the confounder u_{it} is distributed smoothly over space, so that the magnitude of the gap in the confounder between counties i and i' becomes vanishingly small as the distance between their centroids shrinks to zero: i.e., $|u_{it} - u_{i't}| \to 0$ as $D(i, i') \to 0$. As the county centroids become arbitrarily close, the locally differenced value of the error component, u, also becomes vanishingly small: $\tilde{\tilde{u}}_{it} \to 0$, making the covariance with the (locally differenced) treatment vanishingly small as well, i.e., $E(\ln(M\tilde{\tilde{W}}_{s(i)t}), \tilde{\tilde{u}}_{it}) \to 0$. Therefore, as the distance $D(i, i') \to 0$, it is also the case that: $E(\ln(M\tilde{\tilde{W}}_{s(i)t}), \tilde{\tilde{\epsilon}}_{it}) \to E(\ln(M\tilde{\tilde{W}}_{s(i)t}), \tilde{\tilde{e}}_{it}) = 0$. As a result, in the limiting case, $\hat{\beta}_{BD}$ estimated using equation (4) is consistent. Simply put, local differencing can remove confounds that are smoothly distributed over space, even if these confounds are correlated with minimum wages in an otherwise arbitrary time-varying fashion.

While the de-meaned represenation is useful expositionally, in practice we estimate regression equation (4) by including county and pair-time fixed effects. This approach automatically accounts for unbalanced panels and degrees-of-freedom corrections for estimating group means. 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 may induce 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, Dube, Lester and Reich 2010).[10]

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 (2010) compare the effects of minimum wages on border counties to the effects on the counties in the interior of the state, where 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. That paper also shows that, in contrast, the border discontinuity approach is not contaminated by pre-existing trends, which is corroborated in this paper as well.

Equations (3) and (4) are estimated separately for each outcome y and industry/demographic group (teens, restaurant workers). All coefficients, including fixed effects, are allowed to differ across regressions for each outcome and group, and no cross-equation restriction is imposed.

3.2 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, or CPS (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. Therefore, neither data source allows researchers to test hypotheses

[10] We estimate regression equation (4) in STATA using the `twfe` command as described in footnote 7. The `twfe` command also allows for two-way clustering of standard errors.

regarding 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. The QWI data, which are produced though a partnership between the U.S. Census Bureau and the state Labor Market Information (LMI) offices, provides a public use aggregation of the matched employer-employee Longitudinal Employer Household Dynamics (LEHD) database. These in turn are compiled from administrative records collected by 49 states and the District of Columbia for both jobs and firms (Massachusetts had not yet entered the program). The operational unit in the QWI is a worker-employer pair. The primary source of information in the micro-data is the near-universe of employer-reported Unemployment Insurance (UI) records, covering around 98 percent of all private-sector jobs. The UI records provide details on employment, earnings as well as place of work and industry. The Census Bureau uses other data—primarily from Social Security records—to either match or impute demographic information of workers. The underlying datasets consequently are much larger than the CPS or JOLTS. While the CPS contains information on separations based on household-reported data, it is much more error-prone than the QWI. For detailed documentation of the QWI, see Abowd et al. (2009).[11]

The public use QWI series offers monthly employment counts and average earnings by detailed industry at the county level for specified age and gender groups, and as well quarterly figures for hires, separations and turnover rates.[12] We use five different dependent variables in our primary empirical analysis: (1) *Earnings*: Average monthly earnings of employees who were on the payroll on the last day of the reference quarter t in county i. (2) *Employment*: Number of workers on the payroll on the last day of quarter t in county i.[13](3) *Accessions* (Hires): The number of workers who started a new job at any point during quarter t in county i. This variable includes new hires, as well as workers who have been recalled to work. If the individual had worked for the employer sometime during the four

[11] 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. The QWI does not contain data on employee hours. Abraham et al. 2013 find that although the CPS data are monthly, the QWI captures many more short-term jobs. Details on the number of states participating by year in the QWI are included in the appendix. 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.

[12] To protect confidentiality the QWI "fuzzes" the data. For some observations, flagged by a code of 9 in the data, substantial distortions are made. We discuss this issue further in Section 4.2, "Robustness Checks."

[13] A worker is defined as employed at the end of the quarter when she has valid UI wage records for quarters t and $t + 1$.

quarters prior to the accession, the hire is considered a recall; otherwise it is categorized as a new hire.[14] While the minimum length of employment is one day, the employment stock measure includes only the full universe of individuals who are on the employer's payroll at the end of the quarter. (4) *Separations*: Number of workers whose job with a given employer ended in the specified quarter t in county i. A job is defined as ending in quarter t when the worker has no valid wage record with the employer in $t+1$. (5) *Turnover rate*: Average number of hires and separations as a share of total employment: $\frac{Accessions+Separations}{2\times Employment}$. The operational unit is the worker-employer pair—a job. Workers who are employed at more than one employer in a quarter will be included in multiple worker-employer pairs. For this reason, employment, hires and separation are job-based and not person-based concepts in the QWI.

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.[15] We refer to this group of workers as the "full-quarter sample." The QWI also provides additional information on workers moving in or out of jobs. For those workers who were hired in the past quarter, or who separated in the past quarter, we know the duration—from 0 to 4 quarters—of their non-employment spells prior to their being hired or following their separation. Although the QWI does not disaggregate separations to other jobs from separations to non-employment, the non-employment duration data is valuable for assessing how minimum wage policy affects EE and EU transitions. Finally, for the full-quarter sample of hires and separations, we also know their full-quarter earnings during quarter t (i.e., prior to separation, or subsequent to being hired).

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 characterized by high turnover, 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 for earnings, employment, hires, separations, and turnover for workers at all tenure levels

[14] Nearly all the hires in our samples (88 percent of teens and restaurant workers) are new hires and not recalls. For this reason, in this paper we do not separately report disaggregated results by type of hire. However, the elasticities for new hires are nearly identical to those for all hires; and new hires account for virtually all of the reduction in hires documented in this paper (results not shown).

[15] 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.

as well the full-quarter sample.[16]

3.3 Sample construction

The majority of states entered the QWI program between the late 1990s and early 2000s. Figure 1 shows the number of available states by year. The states were non-randomly missing: for example, large states were over-represented in early years. For these reasons, we use data from the 2000s in our main analysis; by 2000, *42* states had come on line.[17]

State minimum wage policies varied considerably during the 2000-2011 period. In online Appendix Figure B1, we show the timing of minimum wage increases in each of the state-border pairs in our sample. We see substantial variation on the 88 policy borders, especially between 2004 and 2009. This period includes the three steps of the 2007-2009 federal minimum wage increases and many state-level changes. There are 196 incidents of quarter-over-quarter minimum wage increases when we pool across federal and local policy changes. Figure 2 shows that the mean 1-quarter change associated with these minimum wage increases was 0.09 log points and the distribution of changes has a right skew. Figure 3 shows that the gaps between the two sides of the border were substantial. 70 percent of the sample border counties had some minimum wage variation with its contiguous pair. For these counties, the maximum gap in log minimum wages within pairs averaged 0.212 log points, a substantial difference. Limiting our attention to cross-border comparisons still provides us with sizeable policy variation that we can use for estimating minimum wage effects.

3.3.1 *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 age

[16]The QWI does not report hours worked nor whether a new hire worked one day or almost the entire quarter. However, if employers adjust to minimum wage increases by cutting hours, we would expect to find lower earnings effects. As we show below, we find earnings effects with the QWI that are very similar to those we have found using CPS and Census/ACS data on hourly earnings (Allegretto, Dube, Reich and Zipperer 2013), indicating that the limitations of the QWI are unlikely to be important in explaining the findings here. The same reasoning applies to workers who hold multiple jobs and are therefore counted multiple times in the QWI, but not in the CPS or Census/ACS. Fallick et al. (2012) report that 95 percent of employer-to employer flows occur from a main job to a main job, where the main job is defined as the primary source of earnings in that quarter.

[17]This 2000-2011 sample represents 77 percent of the observations in the 1990-2011 period. We also report the results using the full 1990-2011 sample in Table 6. The results are very similar. The dataset we obtained from the Cornell University Virtual Data Repository—which hosts the QWI flat files—included data through 2011q4 at the time of access. 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 are defined is 2011q3.

14-18.[18] Teens are disproportionately likely to be minimum wage workers. Based on the Current Population Survey, during the 2000-2011 period, 29.8 percent of teens earned within 10 percent of the minimum wage. And teens comprised of 25.2 percent of all workers earning within 10 percent of the minimum wage. The second high-impact group consists of establishments in the restaurant industry. During the same period, restaurants employed 24.3 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 . Restaurants are also the most intensive user of minimum wage workers, with 22.8 percent of restaurant workers earning within ten percent of the minimum wage (using 3-digit level industry data).[19] 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.

3.3.2 *Contiguous border county pair sample*

Our research design is based on contiguous border county pairs. Our QWI sample consists of the 1,130 counties that border another state. Collectively, these border counties comprise 1,181 unique county pairs. Online Appendix Figure C1 shows a map of the border county sample. While most counties in the border pair sample are geographically proximate, counties in the western United States are much larger in size and irregular in shape. In some cases the geographic centroids of the counties in such pairs lie several hundred miles apart. Appendix Figure C2 shows the distribution of distances between centroids in the county pair sample, confirming the presence of such counties. Appendix Figure C3 non-parametrically plots the mean absolute difference in key covariates between counties in a pair by the distance between the pairs using a local polynomial smoother. The covariates include log of overall private sector employment, log population, employment-to-population ratio, log of average private sector earnings, overall turnover rate and the teen share of the population. We show the results for these variables in levels as well as 4 quarter and 12 quarter differences. As expected, in 17 out of 18 cases the differences increase as we consider counties with more distant centroids. These differences are small for counties within 50 miles of each other, but they become sizeable when the distances reach 100 miles or more.

For this reason, in our primary sample we exclude counties whose centroids are more than 75 miles apart. A smaller distance cutoff trades off lower error variance from greater similarity against higher error variance from a smaller sample. The exact choice of cutoff was based on a data-driven randomization inference procedure that minimized the mean-squared error (MSE) of the estimator in the border sample using placebo treatments; Online

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

[19]These statistics on restaurants exclude drinking places.

Appendix Section B provides more details. When averaged over our five key outcomes, the 75 mile cutoff produced the smallest MSE, as shown in Appendix Figure C4.[20] This criterion retains about 81 percent of the sample, eliminating mostly Western counties, as illustrated in Figure C1. To show that our results are not affected by the choice of cutoffs, Appendix Table B1 reports the key results with cutoffs ranging between 45 and 95 miles.

In addition, in any single regression we limit the sample to counties that have a full panel of disclosed data. 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. In our primary sample, we exclude counties that ever report a non-disclosed or null quantity for a given outcome (data quality flags 0 or 5). We exclude counties with any non-disclosure data issues because observations for these counties may be selected out of the sample the when minimum wage is high (through reducing employment). Depending on the variable, this exclusion leads to dropping between 1 percent and 14 percent of the sample. Additionally, some cell values are substantially distorted from the fuzzing of the data that is undertaken to ensure confidentiality (data quality flag 9).[21] Depending on the variable, up to half of the counties have some instances of distorted data. As a robustness check, we also report below estimates excluding these distorted observations.

We merge data on the county's overall and teen population, and the value of each state's minimum wage in each quarter, with the QWI county-pair panel dataset.[22]

3.4 Descriptive statistics

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 972 contiguous counties in our border-county pair sample whose centroids are no more than 75 miles apart. We display these measures for 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. Table 1 also displays summary statistics for movers, whom we examine separately later in the paper.

Depending upon the worker group and tenure level, average monthly earnings are 1 to 3 percent lower in the border-county pair sample, while average employment is 2 to 3 percent

[20] The problem of choosing a cutoff is similar to the optimal bandwidth selection in a regression discontinuity design. However, the county-pair design does not lend itself to standard cross-validation based approaches because each cutoff entails a different sample. For this reason we use a randomization inference procedure to estimate the MSE of the estimator for alternative cutoffs, as described in Online Appendix Section B.

[21] See Abowd and Vilhuber (2011) for more details.

[22] 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.

lower. Hire, separation and turnover rates as well as the fraction short-term (employed less than one quarter) are 2-3 percent lower in the border-county pair sample. Among movers, earnings at the job are slightly lower in the border-county pair sample, while the duration of non-employment is slightly higher.. 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, including the demographic characteristics shown at the bottom of Table 1, are quite close.

In our border-county pair sample, the teen workforce is about evenly divided by gender, with 54 percent female. In contrast, over 65 percent of the restaurant workforce is female. Unsurprisingly, the teen and restaurant workforces overlap: 22 percent of all restaurant workers are teens. Another 15 percent are young adults under 25. Although not shown in this table, about 35 percent of all teen workers are employed in restaurants.

In general, we find that quarterly turnover rates for teens are around 60 percent, while those of restaurant workers are around 40 percent. These figures indicate high rates of turnover in the low-wage labor market.[23] We also find a high prevalence of short-term jobs, and striking indications of how concentrated the separations are in short-term jobs. Among restaurant workers (teens), jobs with less than one quarter of tenure account for 25 (30) percent of all jobs, and 74 (81) percent of all separations.[24] This duration dependence of separation is useful when we interpret the results on the the turnover elasticity in Section 4.

3.5 Similarity of control groups

To examine whether local controls are indeed more similar, we consider six key covariates: log of overall private sector employment, log population, private sector employment-to-population ratio (EPOP), log of average private sector earnings, overall turnover rate and teen share of population. For each covariate, we test for differences in mean absolute values between contiguous counties and other pairs. We note first that none of these variables is likely to be substantially affected by the treatment status. Therefore, a finding that contiguous counties are more alike in these dimensions cannot be attributed to having more similar minimum wages. More specifically, for each of these six covariates, we calculate the mean absolute differences between (1) a county in our border sample and its contiguous

[23] As discussed in Abowd and Vilhuber (2011), the QWI turnover rates are consistently higher than those reported in JOLTS because the QWI "captures essentially all of the short-term jobs, while JOLTS apparently misses most of them."

[24] Denoting the less-than-full-quarter employees as group 1 and full-quarter employees as group 2, the less-than-full-quarter share of separations $s_1$was calculated as $\frac{s-(1-f_1)s_2}{s}$, where s is the overall separation rate, s_2 is the separation rate for full-quarter employees, and f_1 is the fraction of workers with less-than-full-quarter tenure. All three of these quantities are reported in Table 1.

cross-state-border pair, and (2) a county in our border sample and every non-contiguous pair outside of the state. For the latter, each of the 972 counties in 966 cross-border pairs is paired with every possible out-of-state county, for a total of 1,737,884 pairings. For each time period, we calculate the absolute differences in levels and changes of these variables between the county and (1) its cross-border pair and (2) its non-contiguous pair, respectively. Subsequently, we collapse the dataset back to the county-pair-period level and calculate the means of the absolute differences in covariates between counties within pairs. The standard errors are calculated allowing for clustering multi-dimensionally on each of the two counties in the cross-border pair.

Table 2 shows the results for these variables in levels as well as in 4-quarter and 12-quarter changes. In all cases, the mean absolute differences are larger for non-contiguous pairs and in all cases the gaps are statistically significant at the 1 percent level. The average percentage gap in absolute differences for the twelve variables is about 19 percent. The gaps are substantially higher for levels of employment and earnings, for 4-quarter and 12-quarter changes in EPOP, and for 12-quarter changes in the turnover rate. We conclude that cross-border counties do offer an attractive control group that better balances observed covariates–especially as they relate to the state of the labor market. These local controls therefore reduce the scope for bias stemming from omitted confounders.

4 Empirical findings

4.1 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 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 similar measured effects on earnings as our preferred border-discontinuity specification (columns 2, 4). The elasticity of earnings in our preferred specification is 0.222 among all teen workers and 0.207 among all restaurant workers.[25] These findings put to rest any concerns that restricting the identifying variation to cross-border pairs leads to a lack of actual earnings differentials between 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, the conventional specification (column 1) yields an estimated employment elasticity of -0.173 for teen workers. But when we account for spatial heterogeneity using the border-discontinuity specification (column 2), the coefficient is very small in magnitude (-0.059) and it is not significantly different from zero.[26]In other words, we find strong evidence that spatial heterogeneity produces a spurious disemployment effect for teen workers and we demonstrate the magnitude of the disemployment bias among studies using the conventional specification. Second, we replicate the qualitative findings in Dube, Lester and Reich (2010) using the QWI sample: among all restaurant workers the conventional estimate of the employment elasticity is -0.073 and statistically significant. But accounting for spatial heterogeneity reduces the effect (in magnitude) to -0.022 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 (-0.233) and for restaurant workers (-0.225). The accessions (hires) elasticities are quite similar to the separations elasticities, which is consistent with the responses reflecting steady state to steady state comparisons.[27] For each group, the estimated effects for separations and hires are smaller using the preferred specification as compared to the conventional one. In part, 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.) However, we also note that the turnover *rate* reductions were nearly twice as large in the conventional specifications.

[25]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).

[26]The conventional estimates on teens are very close to those found by researchers using the CPS and similar models (Allegretto, Dube and Reich 2011).

[27]As we mentioned in the data section, the elasticities for new hires are nearly identical to those for all hires; and new hires account for virtually all of the reduction in hires documented in this paper (results not shown).

Summarizing to this point, our border-discontinuity estimates find strong positive responses of earnings to a minimum wage increase. This rise in earnings is met with a small change in the employment stock that is statistically indistinguishable from zero. However, we find clear evidence that employment flows (hires and separations) 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).

To illustrate the importance of our estimated elasticities for the affected groups, we consider a hypothetical increase in the federal minimum wage from \$7.25 to \$9.50 (a 31 percent increase). This increase is similar in percentage terms to others in our dataset and the higher minimum wage level falls within the range of our data. This minimum wage increase would reduce turnover by 6.3 percent among teens and 6.6 percent among restaurant workers. At the mean turnover rates listed in Table 2, average turnover for the county-pair sample would fall from 58.9 percent to 52.6 percent among teens and from 41.2 percent to 34.6 percent for restaurant workers. Similarly, based on the point estimates for employment (although not significant), the same hypothetical minimum wage increase would result in a small reduction in employment among teens and in the restaurant sector (2.3 percent and 0.9 percent, respectively). Since county employment levels average 1,194 for teens and 2,847 in restaurants, this minimum wage increase would result in about 27.7 fewer teens employed and 24.6 fewer restaurant jobs in the average county.

4.2 Robustness checks

Table 4 presents three robustness checks for our main results, using our preferred specification with pair-specific time effects and estimated for teens and for restaurant workers.

One potential concern is that the flow results for teens and restaurant workers may be affected by unobserved overall county labor market trends. As a check on our identification strategy, columns 1 and 5 include county-specific linear trends. The results are largely similar to our preferred specification in Table 3. As an added check, columns labeled 2 and 6 include the overall private sector level outcome (earnings, separation, turnover, etc.) as an additional control. (Note that all regressions in the paper include log of overall private sector employment as a regressor.) Unlike employment, a disproportionately large share of overall separations and new hires come from the low wage sector. For this reason, including the overall private sector flow measure constitutes a particularly tough test. For teens, adding these controls slightly reduces the magnitude of the flow coefficients, while for restaurant workers including these controls does not alter the size of the coefficients. In all cases, the flow coefficients retain statistical significance at the conventional levels. 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.

As described in the data section, in some cases the fuzzing of the QWI data for confidentiality reasons might produce substantial distortions in the data, where the (unreported) true and the (reported) fuzzed value for a given variable differ substantially.[28] As a further check, columns 3 and 7 show the results using only the observations that are not substantially distorted. This exclusion reduces the sample size by less than three percent among teens and between six and eleven percent among restaurant workers (depending upon the variables). Comparing these results with the baseline results in columns 2 and 4 in Table 3, we find the results to be virtually identical: we find sizeable earnings effects, small employment effects, and much larger reductions in employment flows.

Super columns 4 and 8 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 $\ln(MW_t)$.[29] All three of the coefficients are reported in the table. Across all our outcomes, we do not find any statistically significant leading or lagged effects, which are all less than 0.1 in magnitude. Moreover, including the leading and lagged minimum wage terms does not attenuate our statistically significant contemporary coefficients for the earnings and flow measures reported in Table 3. These results provide additional internal validity to our research design and rule out the possibility that the large reductions in the flows are driven by pre-existing trends. In the same vein, we do not detect any anticipation effects in the earnings or flow measures. Nor is there evidence of substantial lagged effects—the rise in earnings and the reductions in employment flows occur immediately—within three quarters of the minimum wage increase. These results also show that the reduction in flows represents a *permanent* change in response to the policy; they are not transitional dynamics. The latter observation justifies our assumption that these elasticities reflect changes from one steady state to another, which becomes important when we use these elasticities in Section 5 to perform steady-state based decomposition and calibration exercises.[30]

4.3 Effects by tenure on the job

As we mentioned in our discussion of the descriptive statistics, turnover generally is concentrated among short-term jobs—those of one quarter or less. Existing evidence shows that separation probability declines with tenure, which can result either from learning by

[28]These are denoted in the QWI dataset by a value of 9 for the data quality flag associated with a variable.

[29]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.

[30]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).

doing (match-specific human capital) or from learning about match quality. At the same time, if minimum wage workers are concentrated in lower-tenure categories, there may appear to be a duration-specific effect that in reality reflects worker heterogeneity.

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 (marginally) statistically significant only for the restaurant sample. By dividing the coefficients by the share of less-than-full-quarter employment (from Table 1) we obtain elasticities of -0.08 for teens and -0.11 for restaurants.

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.[31] Table 5 displays our previously-displayed results for workers at all tenure levels (column 1 for teens and 4 for restaurant workers), as well as for those who have at least one quarter of tenure (columns 2 and 5). Since the QWI does not report outcomes for those with less than a full-quarter tenure, we have backed these values out using "all" and "full-quarter" outcomes. (This procedure is somewhat problematic, however, for earnings. Average earnings for workers with less-than-full-quarter tenure is affected both by actual earnings per unit of time and the extent of time employed during the quarter.)

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. But they continue to be statistically significant. For the teen sample, the standard errors are much larger in this sample and rule out meaningful comparison. The earnings effects for less-than-full-quarter employment, although more noisy, are larger than for full-quarter employees in the restaurant sample.

Given the imprecision with the less-than-full-quarter earnings sample, we also perform an alternative calculation for the less-than-full-quarter earnings elasticity. We back out this estimate by using the overall and full-quarter earnings elasticities, the full-quarter share elasticity, and average earnings for full-quarter and all jobs. These imply less-than-full-quarter earnings elasticities of 0.32 and 0.47 for teens and restaurant workers, respectively.[32] The restaurant estimates are quite similar in both cases—showing much larger earnings effects for less-than full-quarter employees. For teens, given the imprecision of the original

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

[32] We note that overall earnings can be written as $Y = f_1 Y_1 + (1 - f_1)Y_2$, where group 1 is those with less than a full quarter of tenure, and f_1 is its share of employment. By differentiating with respect to minimum wage $\underline{w}$, we get: $\frac{dY}{d\underline{w}}\frac{\underline{w}}{Y} = f_1\left(\frac{Y_1}{Y}\right)\left[\frac{dY_1}{d\underline{w}}\frac{\underline{w}}{Y_1}\right] + (1 - f_1)\left(\frac{Y_2}{Y}\right)\left[\frac{dY_2}{d\underline{w}}\frac{\underline{w}}{Y_2}\right] + \left(\frac{Y_1 - Y_2}{Y}\right)\left[\frac{df_1}{d\underline{w}}\underline{w}\right]$.Table 1 reports Y,Y_2 and f_1. Table 5 reports $\left[\frac{dY_2}{d\underline{w}}\frac{\underline{w}}{Y_2}\right]$, $\left[\frac{dY}{d\underline{w}}\frac{\underline{w}}{Y}\right]$, $\left[\frac{df_1}{d\underline{w}}\underline{w}\right]$. We use these to solve for $\left[\frac{dY_1}{d\underline{w}}\frac{\underline{w}}{Y_1}\right]$.

estimates, we put more stock in the alternative calculation, which also shows much higher earnings effects at lower tenure levels. A final piece of evidence is provided below in Table D1 (and discussed in greater detail in online Appendix C), where we consider the sample of full-quarter hires—workers with tenure between 1 and 2 quarters. In that sample, we find earnings elasticities of 0.29 (0.30) for teens (restaurant workers), which also exceed the full-quarter earnings elasticities of 0.19 (0.15) for teens (restaurant workers). Overall, the evidence shows that earnings increase relatively more for low-tenured workers, but we see substantial earnings increase among higher tenured workers as well..

Employment effects for the full-quarter tenure sample are very small in magnitude, but more negative than for less-than-full-quarter employees, as expected given the reduction in their share. Among full-quarter employees, the estimated effects on hires and separations are smaller than among workers of all tenure levels and they are no longer significant. In contrast, the separations, hires and turnover rate elasticities for less-than-full-quarter employees are statistically significant and sizeable, and much larger than for full-quarter employees.[33]

These findings suggest that minimum wage changes reduce turnover more sharply for workers with a lower tenure level, a group whose earnings also grow more. However, since earnings rise substantially for full-quarter employees, it seems unlikely that a compositional change can explain the differential impact by tenure level. Rather, some form of duration dependence is a likely part of the explanation, an interpretation that is consistent with the decline in the separation rate with greater tenure as shown in the descriptive statistics (Table 1). Duration dependence could reflect learning about match quality early in a worker's tenure—the channel highlighted by Brochu and Green. However, a job-ladder model extended to include learning by doing can also rationalize why separations would fall over time. As shown in Nagypal (2007), a growth in the value of the match over time—e.g., from job-specific learning by doing—also generates a fall-off in the EE transitions in an extended job-ladder model. Moreover, Nagypal (2007) finds that learning by doing tends to be the dominant factor at very short tenure, while learning about match quality is more important subsequently. Therefore, while we view our results as consistent with an explanation involving either some learning about match quality or some learning by doing, it is difficult to separate the two, or to identify whether this effect occurs through quits or layoffs.

4.4 Results by time period

As we mentioned in the introduction, our main results focus on the 2000-2011 time

[33] Differences in sample sizes in regressions for the three tenure groups (all, full-quarter, less-than-full-quarter) are the result of using counties where there is never any data suppression for a given outcome, in order to avoid a sample selection bias. When, however, we use a common sample (across columns 1,2,3 and 4,5,6, respectively) the results are very similar to those reported in Table 5.

period, when most states are in the dataset. In this section we check our main results using two extended QWI samples, one that begins in 1990 and another that begins in 1994. Table 6 shows these results, displayed for teens in columns 1 and 3 and for restaurant workers in columns 5 and 7. The results for the full QWI sample (1990-2011) and for the 1994-2001 time period in Table 6 are qualitatively similar to our main results in Table 3. The earnings effects are slightly larger in Table 6 for both groups; the employment effect for teens is slightly larger in magnitude while that for restaurant workers is nearly the same. The flow estimates are very similar to those in Table 3.

To distinguish between samples by years and the effects of distorted data, Table 6 shows our results when we also exclude distorted observations. These results are displayed in columns 2 and 4 for teens and in columns 6 and 8 for restaurant workers. The qualitative results remain the same as before; they are, if anything, more precisely determined: minimum wage increase average earnings, they do not have a substantial or statistically significant effect on employment, and they have clear negative effects on employment flows. Moreover, the differences between estimates across time periods are very small when we limit attention to the undistorted sample. [34]

4.5 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 substantial disemployment effects, substitution effects might still be present, affecting the shares of different groups in particular jobs, thereby affecting employment prospects of various low-skill workers.

To address this question directly, we report in Table 7 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 and young adult workers in restaurants obtain earnings increases that are more than double that of adult restaurant workers. Yet, as the table indicates, none of the share coefficients are significant or substantial. The implied share elasticities

[34] Although not displayed, we also investigated whether excluding recessionary periods from the sample would affect the results. Tthis exclusion made no material difference to our estimates–whether to the baseline sample in Table 3 or the extended samples with earlier start dates in Table 6.

are modest (under -0.11 in magnitude) and are never statistically significant. In sum, we do not find substantial labor-labor substitution among the age and gender categories in our data.

More generally, if minimum wage increases lead to a reallocation of workers, one would expect differences in short and long term responses in separations and hires—as additional gross flows accommodate re-allocation in the short run. 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. The lack of substitution away from teens or young adults in response to a rise in their relative earnings is similar to Giuliano (2013), who studies the impact of minimum wages using payroll data from a retail chain.[35] This lack of labor-labor substitution sharpens the anomaly for the competitive labor market model's explanation of minimum wage effects, and hence provides an additional reason to consider models with search frictions.[36]

4.6 Non-employment duration of movers

In addition to possibly affecting earnings, employment and separation rates, minimum wages may also affect how long workers spend between jobs. If the reduction in separations from a minimum wage increase reflects a lower job finding probability in the labor market for the employed and the non-employed alike, the minimum wage increase would raise the length of spells between jobs. Estimating the effect of minimum wages on the duration of jobless spells between jobs can therefore provide additional evidence of the impact of the policy on the tightness of the low-wage labor market.

The QWI data reports the average number of quarters (up to a maximum of four) spent by each separating worker without a job subsequent to leaving their current job. Ideally, we would estimate separately the impact of minimum wages on the non-employment duration of EN separations, and the incidence of EE transitions.. (By definition, the EE transitions have a non-employment duration of zero.) Unfortunately, the QWI data only reports the mean duration of non-employment for all separations (i.e., EE and EN separations together). However, the effect on mean non-employment duration is still informative. If a minimum wage increase leads to a greater difficulty in finding jobs in general through a lower offer arrival rate to workers and the non-employed alike, it would also raise the mean non-employment duration both through longer spells between jobs, and fewer job-to-job

[35] Giuliano does find a greater and positive labor supply response for teens than for adults, leading to an *increase* in the teen share of employment.

[36] 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 in the case for employment overall.

transitions. Indeed, longer spells between jobs and fewer job-to-job transtions both occur during economic downturns, e.g., Shimer 2005, 2012.

Online Appendix Table D1 reports minimum wage effects on non-employment duration for teen movers as well as for those moving in and out of restaurant jobs. (The non-employment duration measures and the results are discussed in greater detail in online Appendix C.) Overall, we find little impact of minimum wages on the length of spells between jobs. Based on the point estimates, a 10 percent minimum wage increase changes the mean duration of non-employment by no more than 0.3 percent in magnitude for both separations and hires–and for both teens and restaurant workers. While our results in previous sections show that fewer workers move in and out of jobs when the minimum wage rises, those who are moving do not appear to spend a longer time between jobs.

We note that the stable mean non-emplyment duration following all separations, D, is consistent with reductions in both EE and EN transitions which leave the share f_{EE} the same, coupled with a stable duration of non-employment for those transitioning out of work, D_{EN}. However, since the QWI does not distinguish between EE and EN separations, the lack of an impact on the overall duration of non-employment could mask a combination of (1) a shift in the job-to-job (EE) share of separations along with (2) a change in the non-employment duration of those separating from employment to non-employment (EN). What our findings do rule out is the possibility of reduced job finding probabilities for the employed and the non-employed alike, which would have unambiguously raised the average non-employment duration, D.

5. Discussion

We provide minimum wage elasticities of earnings, employment stocks and employment flows for teens as well as for a high impact industry—restaurants. We do so using a border discontinuity design that accounts for the kind of spatial heterogeneity that has been shown to be important in the literature. The results on employment flows constitute the first evidence on this topic using representative data from the U.S. Our border discontinuity design shows that even though teen and restaurant employment stocks are not substantially reduced 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. We do not find an impact on the duration of non-employment for those leaving or joining jobs. 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, or males for females. We do not detect such labor-labor substitution in restaurants in response to minimum wage increases with respect to age and gender.

We consider alternative explanations for our findings by using two different models of endogenous separations. In particular, we show that the relative magnitudes of the employment and separation rate elasticities are qualitatively similar to what one would expect from calibrating a model with on-the-job search. At the same time, an alternative explanation based on match quality learning suggests that layoffs and hence transitions to unemployment may also be an important margin.

Both the job-ladder and the match quality models can explain the combination of a small employment effect combined with a larger effect on separations—but through different types of transitions. Our analysis of the matched sample of teens (restaurant workers) using the 2000-2011 Current Population Survey shows that separations to other jobs constituted 53 (59) percent of *EU+EE* separations (see online Appendix Table A1). Nagypal (2008) also shows that a majority of separations to other jobs or unemployment were to other jobs. Unfortunately, the QWI dataset does not separately report job-to-job transitions versus transitions to non-employment, making it impossible to direclty measure the importance of these two channels. However, the consistency between the predictions of the job-ladder model calibrated using cross-sectional flows and the empirical findings on the relative magnitudes of the employment and separations elasticities provide evidence consistent with the *EE* channel. At the same time, the duration dependence in the reduction in separations is consistent with the job being an experience good, as suggested by the the match quality model.Finally, the fact that mean non-emplyment duration following all separations is unaffected by the policy is consistent with a reduction in both types of separations. The Census Bureau plans to make job-to-job flows available as part of the QWI program, which should help researchers better distinguish these channels.[37]

Overall, these results emphasize the importance of looking beyond employment rates to understand the impacts of minimum wages. Minimum wage increases over the past decade appear to have substantially reduced turnover and increased job stability, with small effects on overall employment levels for highly affected groups, such as teens.

However, important questions remain unanswered. First, we need better data to more directly estimate the impact of minimum wages on separations to other jobs as opposed to non-employment. Micro-data from the LEHD can be very helpful for this exercise. Second, future research should try to determine how reductions in job-to-job transitions affect the earnings profiles of low-skilled and young workers. Is the primary effect of a minimum wage increase to reduce the *variability* in earnings growth by reducing frictional wage dispersion

[37]For an announcement regarding the future availability of job-to-job flows along with characteristics of destination jobs, see: http://lehd.ces.census.gov/doc/workshop/2014/Presentations/Job-to-Job_LED_20140909.pdf

through raising pay at the bottom? Or does it lead to reduced *overall* earnings growth over time as workers stay longer at lower wage positions? Relatedly, what happens to pay growth within the firm? This issue is especially relevant given possible changes to firms' incentives to train workers in a low-turnover environment. Do other factors, such as replacement costs and more intensive screening of hires, also play a role? And finally, is most of the reduction in turnover occuring within existing firms, or from does it stem instead from a reallocation of workers across very different types of firms? Answers to these questions are important for fully understanding the mechanisms and the welfare implications of the findings in this paper.

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Table 1
Descriptive Statistics

	All Counties Sample				*Contiguous County Pair Sample*			
	All Teens		Restaurants		All Teens		Restaurants	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
All								
Monthly Earnings	478	789	827	258	466	133	818	239
Employment	1273	3904	2924	9714	1194	2937	2847	8001
Hire Rates	0.677	0.391	0.424	0.194	0.655	0.330	0.415	0.184
Separation Rates	0.542	0.276	0.414	0.149	0.522	0.227	0.405	0.144
Turnover Rate	0.611	0.301	0.421	0.160	0.589	0.249	0.412	0.154
Fraction Short Term (tenure<1q)	0.308	0.110	0.248	0.071	0.302	0.104	0.243	0.068
Full Quarter (Tenure ≥ 1q)								
Monthly Earnings	588	188	983	313	572	169	969	275
Employment	891	2755	2253	7758	840	2200	2109	6361
Hire Rates	0.205	0.048	0.144	0.037	0.206	0.047	0.142	0.035
Separation Rates	0.144	0.039	0.141	0.036	0.142	0.036	0.139	0.035
Turnover Rate	0.261	0.079	0.197	0.151	0.256	0.071	0.193	0.075
Movers (Separations)								
Monthly Earnings (Full Qtr)	531	210	738	295	519	190	729	248
Quarters of Non-Employment	1.947	0.365	1.732	0.347	2.032	0.369	1.866	0.364
Movers (Hires)								
Monthly Earnings (Full Qtr)	575	208	730	267	565	188	723	234
Quarters of Non-Employment	2.654	0.353	2.048	0.360	2.715	0.348	2.167	0.370
Fraction Female	0.533	0.057	0.642	0.087	0.536	0.056	0.647	0.086
Fraction Teen	-	-	0.217	0.087	-	-	0.217	0.078
Fraction Young Adult	-	-	0.157	0.037	-	-	0.156	0.036

Notes. Sample means are reported for all counties in the US and for all contiguous border county pairs with county centroids no greater than 75 miles apart. Monthly earnings are in nominal dollars. Hires, separations and turnover rates are quarterly. Sample sizes vary by demographic group, industry and tenure and range from 114,996 to 146,519 for the all county sample, and 59,260 to 89,078 for the contiguous county pair sample. Sample period is from 2000Q1 through 2011Q4. Data Source: Quarterly Workforce Indicators.

Table 2
Mean Absolute Differences in Covariates between Counties in Contiguous versus Other Pairs

	Non Contiguous Pair	Contiguous Pair	Gap	Pct. Gap
Levels:				
Log Employment	1.744***	1.233***	0.511***	41.4
	(0.026)	(0.027)	(0.033)	
Log Population	1.518***	0.964***	0.554***	57.5
	(0.023)	(0.023)	(0.029)	
EPOP	0.042***	0.039***	0.003***	8.0
	(0.001)	(0.001)	(0.001)	
Log Earnings	0.229***	0.1695***	0.060***	35.1
	(0.004)	(0.004)	(0.004)	
Turnover Rate	0.057***	0.048***	0.009***	18.1
	(0.001)	(0.001)	(0.001)	
Teen Share	0.006***	0.005***	0.001***	21.7
	(0.0001)	(0.0001)	(0.0001)	
4 Quarter Difference:				
Log Employment	0.062***	0.058***	0.003***	5.2
	(0.001)	(0.001)	(0.001)	
Log Population	0.048***	0.047***	0.002**	3.9
	(0.001)	(0.001)	(0.001)	
EPOP	0.014***	0.011***	0.003***	26.9
	(0.0002)	(0.0002)	(0.0002)	
Log Earnings	0.013***	0.012***	0.001***	7.3
	(0.0002)	(0.0003)	(0.0002)	
Turnover Rate	0.002***	0.001***	0.0002***	16.7
	(0.0000)	(0.0000)	(0.0000)	
Teen Share	0.038***	0.036***	0.002***	5.6
	(0.001)	(0.001)	(0.001)	
12 Quarter Difference				
Log Employment	0.099***	0.091***	0.008***	8.5
	(0.001)	(0.002)	(0.001)	
Log Population	0.069***	0.066***	0.004***	5.5
	(0.001)	(0.002)	(0.001)	
EPOP	0.037***	0.027***	0.010***	36.3
	(0.001)	(0.001)	(0.001)	
Log Earnings	0.018***	0.017***	0.001***	8.5
	(0.0003)	(0.0004)	(0.0003)	
Turnover Rate	0.003***	0.002***	0.001***	25.0
	(0.0000)	(0.0000)	(0.0000)	
Teen Share	0.045***	0.041***	0.004***	9.5
	(0.001)	(0.001)	(0.001)	

Notes. Each of the 972 counties in 966 cross-border pairs with centroids within 75 miles is merged with every possible out-of-state county, a total of 1,737,884 pairings. Absolute differences in levels and changes are calculated between the county, its border pair and its randomly assigned pair, respectively. Subsequently, the dataset is collapsed back to county-pair-period level and means of the absolute differences in covariates between counties within pairs are calculated, clustering standard errors multi-dimensionally on each of the two counties in the cross-border pair. "Gap" is a test of difference in mean absolute value of the covariate between contiguous and other pairs. "Pct. Gap" divides this gap value by the mean for the contiguous pairs. Significance levels are indicated by: * for 10%, ** for 5%, and *** for 1%.

Table 3
Minimum Wage Elasticities for Teens and Restaurant Workers: Earnings, Employment Stocks and Flows

	Teens		Restaurant Workers	
	(1)	(2)	(3)	(4)
Earnings	0.177***	0.222***	0.203***	0.207***
	(0.036)	(0.047)	(0.028)	(0.059)
	83,462	83,462	81,954	81,954
Employment	-0.173**	-0.059	-0.073*	-0.022
	(0.071)	(0.084)	(0.042)	(0.091)
	84,702	84,702	79,089	79,089
Hires	-0.515***	-0.219**	-0.467***	-0.264**
	(0.094)	(0.094)	(0.087)	(0.134)
	80,944	80,944	74,365	74,365
Separations	-0.552***	-0.233**	-0.467***	-0.225*
	(0.100)	(0.098)	(0.080)	(0.126)
	74,952	74,952	72,859	72,859
Turnover Rate	-0.377***	-0.204***	-0.392***	-0.212**
	(0.061)	(0.072)	(0.067)	(0.090)
	74,509	74,509	71,438	71,438
Controls:				
Common time effects	Y		Y	
Pair-specific time effects		Y		Y

Notes. The table reports coefficients associated with log minimum wage on the log of the dependent variable noted in the first column. All regressions include controls for natural log of county population and total private sector employment. Specifications 1 and 2 provide estimates for all teens age 14-18 regardless of industry, and also include log of teen population. Specifications 3-4 are limited to all workers in the restaurant industry (NAICS722). 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%. Sample sizes are reported below the standard errors for each regression.

Table 4
Minimum Wage Elasticities for Earnings and Employment Stocks and Flows: Robustness Checks

	Teens						Restaurant Workers					
	(1)	(2)	(3)	(4)			(5)	(6)	(7)	(8)		
				$lnMW_{t+4}$	$lnMW_t$	$lnMW_{t-4}$				$lnMW_{t+4}$	$lnMW_t$	$lnMW_{t-4}$
Earnings	0.185***	0.215***	0.225***	-0.058	0.207***	-0.043	0.176***	0.206***	0.201***	-0.011	0.210**	-0.022
	(0.062)	(0.048)	(0.047)	(0.040)	(0.057)	(0.049)	(0.059)	(0.059)	(0.043)	(0.046)	(0.082)	(0.031)
	83,462	83,462	81,757		83,462		81,954	81,954	74,434		81,954	
Employment	-0.003	-0.059	-0.051	0.084	-0.052	0.098	-0.084	-0.022	-0.001	0.093	0.027	0.004
	(0.084)	(0.084)	(0.079)	(0.067)	(0.112)	(0.067)	(0.097)	(0.091)	(0.073)	(0.067)	(0.111)	(0.073)
	84,702	84,702	83,470		84,702		79,089	79,089	74,297		79,089	
Hires	-0.180*	-0.164**	-0.241**	-0.005	-0.252*	0.080	-0.305**	-0.222*	-0.254**	0.031	-0.256	0.023
	(0.103)	(0.072)	(0.100)	(0.084)	(0.130)	(0.101)	(0.138)	(0.126)	(0.110)	(0.097)	(0.171)	(0.114)
	80,944	80,944	79,146		80,944		74,365	74,365	68,811		74,365	
Separations	-0.225**	-0.183**	-0.239**	0.049	-0.236	0.076	-0.264**	-0.205*	-0.218**	0.046	-0.212	0.030
	(0.103)	(0.072)	(0.095)	(0.090)	(0.148)	(0.083)	(0.130)	(0.121)	(0.102)	(0.092)	(0.165)	(0.085)
	74,952	74,952	73,426		74,952		72,859	72,859	67,623		72,859	
Turnover Rate	-0.212***	-0.146***	-0.202***	-0.085	-0.258***	0.021	-0.203**	-0.184**	-0.216**	-0.067	-0.254**	0.015
	(0.071)	(0.047)	(0.073)	(0.064)	(0.098)	(0.056)	(0.095)	(0.079)	(0.093)	(0.077)	(0.124)	(0.098)
	74,509	74,509	71,917		74,509		71,438	71,438	63,847		71,438	
Controls and Samples:												
County trends	Y						Y					
Overall outcome		Y						Y				
Undistorted data			Y						Y			

Notes. The table reports coefficients associated with log minimum wage on the log of the dependent variable noted in the first column. All regressions include controls for natural log of county population and total private sector employment. Specifications 1 - 4 provide estimates for all teens age 14-18 regardless of industry, and also include log of teen population. Specifications 5-8 are limited to all workers in the restaurant industry (NAICS722). All samples and specifications include county fixed-effects and pair-specific time effects. Specifications 1 and 5 also include county-specific linear time trends. Specifications 2 and 6 also include the overall private sector outcome (e.g., private sector turnover rate) as a control. Specifications 3 and 7 exclude any "distorted" data (data quality flag=9). Specifications 4 and 8 also include a 4 quarter lead and a 4 quarter lag in log minimum wage. Robust standard errors, in parentheses, are clustered at the state and border segment levels for all regressions. Sample sizes are reported as well for each regression. Significance levels are indicated by: * for 10%, ** for 5%, and *** for 1%. Sample sizes are reported below the standard errors for each regression.

Table 5
Minimum Wage Effects on Earnings and Employment Stocks and Flows: by Job-Tenure

	Teens			Restaurant Workers		
	(1) All	(2) Tenure≥1q	(3) Tenure<1q	(4) All	(5) Tenure≥1q	(6) Tenure<1q
Fraction with tenure<1q	-0.023			-0.026*		
	(0.016)			(0.015)		
	81,813			77,158		
Earnings	0.222***	0.185***	0.180	0.207***	0.153***	0.527***
	(0.047)	(0.049)	(0.187)	(0.059)	(0.052)	(0.098)
	83,462	82,930	72,936	81,954	81,485	77,274
Alternative Calculation			0.321			0.474
Employment	-0.059	-0.034	-0.146**	-0.022	0.016	-0.127
	(0.084)	(0.091)	(0.074)	(0.091)	(0.093)	(0.112)
	84,702	81,813	81,742	79,089	77,158	77,112
Hires	-0.219**	-0.114	-0.405***	-0.264**	-0.061	-0.340*
	(0.094)	(0.103)	(0.121)	(0.134)	(0.130)	(0.177)
	80,944	66,999	66,378	74,365	61,780	61,281
Separations	-0.233**	-0.107	-0.328***	-0.225*	0.005	-0.318**
	(0.098)	(0.071)	(0.115)	(0.126)	(0.087)	(0.148)
	74,952	59,260	59,259	72,859	63,483	63,483
Turnover Rate	-0.204***	-0.082*	-0.175**	-0.212**	-0.065	-0.279***
	(0.072)	(0.049)	(0.074)	(0.090)	(0.083)	(0.077)
	74,509	82,468	73,275	71,438	80,963	70,441

Notes. The table reports coefficients associated with log minimum wage on the log of the dependent variable noted in the first column, except for "Fraction with tenure<1q" where the outcome is not in logs. All regressions include controls for natural log of county population and total private sector employment. Specifications 1-3 provide estimates for all teens age 14-18 regardless of industry, and also include log of teen population. Specifications 4-6 are limited to all workers in the restaurant industry (NAICS722). All samples and specifications include county fixed-effects and pair-specific time effects. Specifications 2 and 5 limit the sample to "full quarter" employees with 1 or more quarter of tenure; specifications 3 and 6 limit samples to employees with less than full quarter tenure. The alternative calculations for the less than full-quarter earnings elasticity use the overall and full-quarter earnings elasticities, less than full-quarter share elasticity, and sample means of full-quarter share and earnings differences by tenure, as explained in the text. 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%. Sample sizes are reported below the standard errors for each regression.

Table 6
Minimum Wage Elasticities for Earnings and Employment Stocks and Flows: Choice of Time Period

	Teens				Restaurant Workers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Earnings	0.258***	0.261***	0.246***	0.249***	0.256***	0.243***	0.237***	0.226***
	(0.067)	(0.068)	(0.060)	(0.060)	(0.066)	(0.053)	(0.060)	(0.044)
	94,890	93,063	94,056	92,233	93,195	84,746	92,366	83,967
Employment	-0.093	-0.078	-0.076	-0.059	0.004	0.020	-0.006	0.014
	(0.088)	(0.088)	(0.084)	(0.081)	(0.113)	(0.101)	(0.103)	(0.100)
	96,059	94,748	95,225	93,921	89,623	84,389	88,845	83,634
Hires	-0.211**	-0.228**	-0.189*	-0.206**	-0.241*	-0.221**	-0.228*	-0.207*
	(0.105)	(0.108)	(0.097)	(0.102)	(0.134)	(0.109)	(0.134)	(0.107)
	91,921	90,026	91,115	89,228	84,397	78,256	83,647	77,535
Separations	-0.246**	-0.254**	-0.220**	-0.227**	-0.225*	-0.214*	-0.213	-0.199*
	(0.122)	(0.131)	(0.112)	(0.108)	(0.130)	(0.110)	(0.131)	(0.106)
	85,286	83,646	84,518	82,885	82,761	77,028	82,011	76,306
Turnover Rate	-0.179**	-0.176**	-0.169***	-0.165**	-0.237***	-0.233***	-0.209**	-0.210**
	(0.071)	(0.070)	(0.073)	(0.071)	(0.087)	(0.090)	(0.084)	(0.087)
	84,822	82,053	84,054	81,296	81,237	72,884	80,487	72,182
Samples:								
Sample Start Year	1990	1990	1994	1994	1990	1990	1994	1994
Undistorted Data		Y		Y		Y		Y

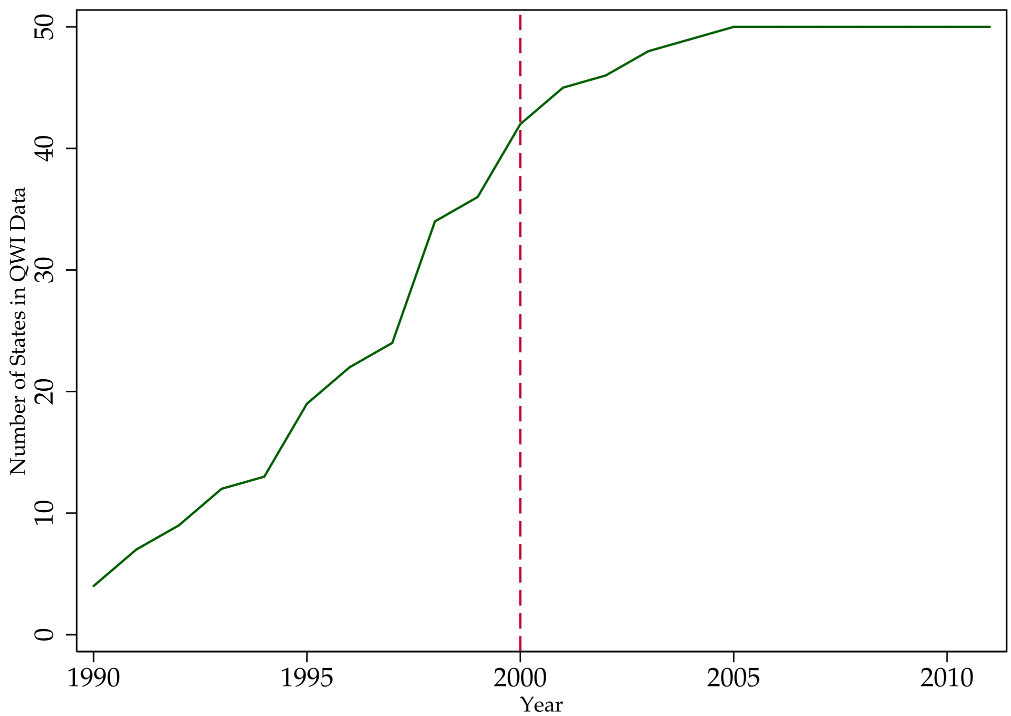
Notes. The table reports coefficients associated with log minimum wage on the log of the dependent variable noted in the first column. All regressions include controls for natural log of county population and total private sector employment. Specifications 1-4 provide estimates for all teens age 14-18 regardless of industry, and also include log of teen population. Specifications 5-8 are limited to all workers in the restaurant industry (NAICS722). All samples and specifications include county fixed-effects and pair-specific time effects. The results are shown using the extended 1990-2011 or 1994-2011 samples, including all data or excluding distorted data, as indicated. 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%. Sample sizes are reported below the standard errors for each regression.

Table 7
Labor-Labor Substitution within Restaurants

		Minimum Wage Effect	
	Employment Share	*Log Earnings*	*Employment Share*
Male	0.360	0.206***	0.023
		(0.072)	(0.015)
		77,108	74,101
Female	0.647	0.224***	-0.018
		(0.047)	(0.017)
		81,954	79,089
Teen	0.217	0.370***	-0.026
		(0.067)	(0.018)
		74,431	71,361
Young Adult	0.156	0.319***	0.012
		(0.063)	(0.009)
		70,565	64,884
Adult 25+	0.632	0.135***	0.007
		(0.041)	(0.018)
		61,266	67,153

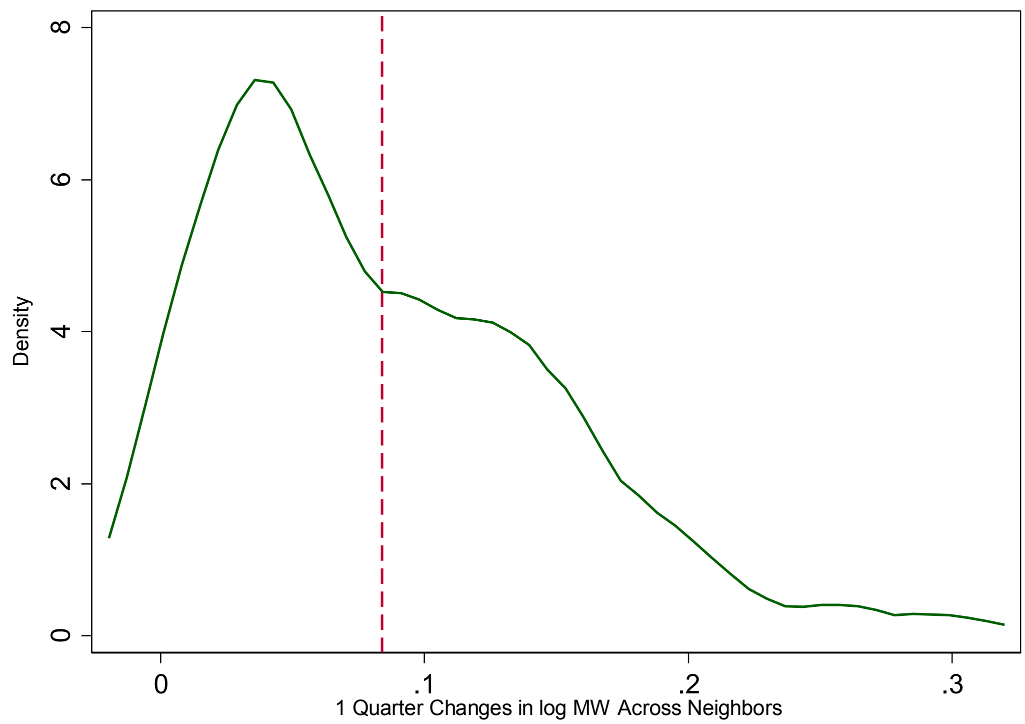
Notes. Column 1 reports the employment share of each demographic group in the overall restaurant workforce. Columns 2 and 3 report the regression coefficient associated with log of the minimum wage. In column 2, the outcome is log of average earnings; the coefficient is therefore the minimum wage elasticity of average earnings. In column 3, the outcomes are the demographic group's share of overall restaurant employment. Teens are ages 14-18; young adults are ages 19-24. All regressions include controls for natural log of county population, total private sector employment, county fixed-effects and 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%. Sample sizes are reported below the standard errors for each regression.

Figure 1
Entry of States into the QWI Program



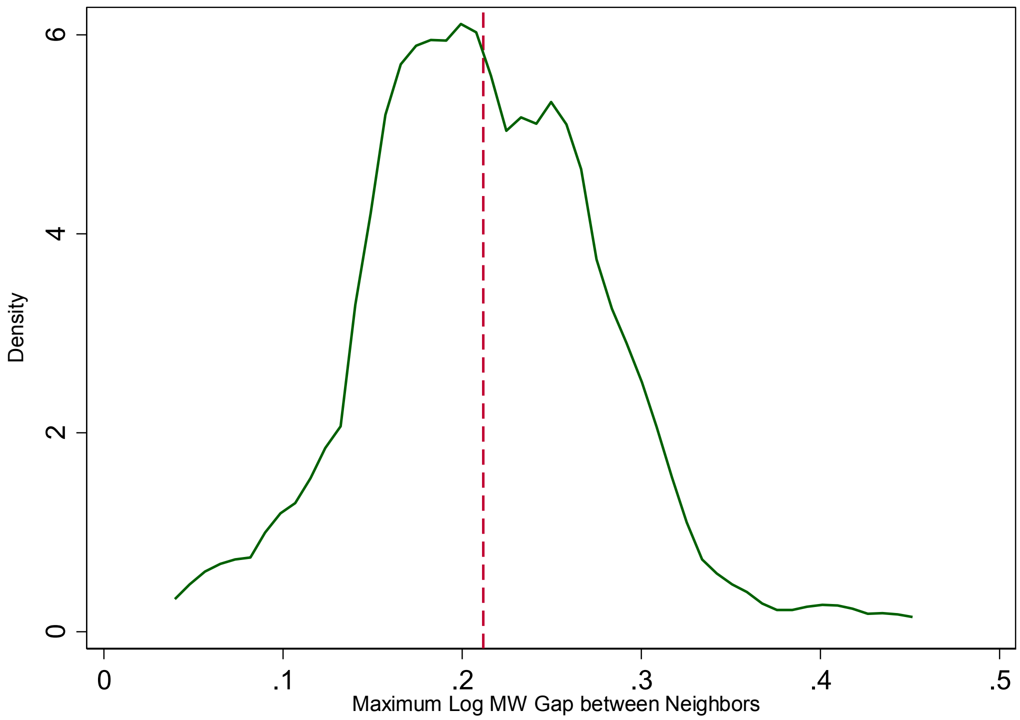
Notes. The figure shows the number of states reporting data as part of the QWI program by year. A state is denoted as reporting data for a calendar year if it reports it for any of the quarters during that year.

Figure 2
Distribution of Changes in Relative Minimum Wages within Pairs



Notes. The figure shows the kernel density estimate of changes in relative minimum wages for the sample border county pairs (with centroids within 75 miles) in 2010-2011. Specifically, this is the density of the absolute value of the 196 1-quarter changes in the gap in log minimum wage across neighboring counties within a pair, for those periods with changes in the gap. The vertical dashed line denotes the average change in minimum wage of 0.09 log points.

Figure 3
Distribution of Maximum Gap in log Minimum Wages within Pairs



Notes. 70 percent of the sample border counties (with centroids within 75 miles) had a minimum wage gap at some point in the sample in 2000-2011. The figure shows the kernel density estimate of the maximum gap in log minimum wages, over the sample period, between two neighboring counties in each pair for those counties that had such a gap. The vertical dashed line shows that the average gap in minimum wages was 0.212 log points for these counties.

Online Appendix A: Minimum Wage Effects in a Job-Ladder Model

Appendix A derives some implications of the canonical job-ladder model regarding the impact of a minimum wage increase on employment stocks and flows. The job-ladder model is the most common framework for incorporating on-the-job search. Here we use this model to analytically derive the minimum wage elasticities of employment level and separations. We then assess when the latter is likely to be relatively larger. We also examine whether the combination of a small employment reduction and a relatively larger reduction in the separation rate is predicted by the model when calibrated to be consistent with cross-sectional flows. We then compare the predicted elasticities with those estimated empirically in this paper.

For expositional ease, we begin by using the wage posting model of Bontemps, Robin, and van den Berg (1999, 2000), which is a specific variant of the job ladder model. Afterward, we demonstrate that our key prediction aboutthe relative magnitudes of the minimum wage elasticities of separations and employment arises more generally.

In a job-ladder model, offers arrive to unemployed workers at the rate λ from an offer wage distribution $F(w)$, who accept the offer if the wage is above some reservation wage b, assumed to be below a binding minimum wage $\underline{w}$. Once employed, exogenous job destructions occur at the rate σ. Employed workers engage in on-the-job search, and offers arrive to them at the rate $\lambda_e = \phi \cdot \lambda$, where ϕ is an exogenous parameter capturing the relative efficiency of on-the-job search. Importantly, employed workers always accept an offer if the offered wage exceeds their current wage. (For more on the job ladder model, see Hornstein et al. 2010, Nagypal 2005).

The Bontemps et al. (1999, 2000) model specializes this general setup in two ways.. The first relates to the determination of the offer wage distribution, $F(w)$. Firms post wages w to attract and retain workers, and are assumed to only offer wages at or above the minimum wage, $\underline{w}$. Firms earn profits $N(p - w)$ where N is the measure of workers. Firms are heterogeneous in their potential productivity, p, with a cumulative distribution function $\Gamma(p)$. Only those firms with productivity exceeding the minimum wage (i.e., $p > \underline{w}$) are active. (The wage posting assumption makes this a variation of the Burdett and Mortensen (1998) model.)

The second specific feature of the model in Bontemps et al. relates to the determination of the equilibrium contact rate, λ, which is assumed to depend on the measure of active firms. The intuition here is that the offer arrival rate should depend on the relative measure of workers (fixed at N) and firms actually looking for workers. Specifically, the equilibrium contact rate is posited as $\lambda = \lambda_0(1 - \Gamma(\underline{w}))$, where $\Gamma(p)$ is the CDF of the productivity of potentially active firms with productivity less than p, and λ_0 is a constant reflecting the extent of search frictions. The larger is the measure of potentially active firms with productivity exceeding the minimum wage $(1 - \Gamma(\underline{w}))$, the greater is the rate λ at which offers arrive to workers, whose measure is fixed at N. Therefore, in this model, an increased minimum wage necessarily lowers the offer arrival rate by reducing the measure of active firms, but the extent to which it does so depends on the shape of the firm productivity distribution, $\Gamma(p)$.

The flow-balance between flows in and out of employment, $\lambda(1 - e) = \sigma e$, implies that the employment rate is only a function of the *relative* rates of exiting versus entering unemployment: $\frac{\lambda}{\sigma}$:

$$e = \frac{\lambda}{\lambda + \sigma} = \frac{1}{1 + \frac{\sigma}{\lambda}} \tag{5}$$

When we utilize the equilibrium condition for λ, we can rewrite equation (5) as:

$$e = \frac{1}{1 + \frac{\sigma}{\lambda_0(1-\Gamma(\underline{w})}} \tag{6}$$

A higher minimum wage always reduces employment in this model by lowering the equilibrium offer arrival rate, λ. In the Bontemps et al. model, the magnitude of employment loss depends on the shape of the firm productivity distribution, $\Gamma(p)$. [1]

Steady state flow balance also imposes a tight restriction between the offer wage distribution $F(w)$ and the reailzed wage distribution $G(w)$. The flow into the set of workers who earn wage w or less is $\lambda F(w)u$. The outflow from that set is $[\sigma + \lambda_e(1 - F(w))]\, G(w)(1 - u)$. In steady state, the balance of these flows, together with the definition of equilibrium employment in equation (5) implies the following relationship between the wage offer and realized wage distribution:

$$G(w) \quad = \quad \frac{\sigma F(w)}{\sigma + \lambda_e[1 - F(w)]} \tag{7}$$

Next, we can derive the expression for equilbrium separation rate. First, the mean separation rate, $E(s)$ is composed of both exogenous job destruction rate (σ), and the endogenous job-to-job transition rate (EE).

$$E(s) = \sigma + EE = \sigma + \int_{\underline{w}} \lambda_e \left[1 - F(w)\right] dG(w)$$

Using the expression for realized wage from equation (7), and integration by parts, we can derive the following expression for the equilibrium separation rate[2]:

$$E(s) = \frac{\sigma \left(1 + \frac{\lambda_e}{\sigma}\right) \ln \left(1 + \frac{\lambda_e}{\sigma}\right)}{\frac{\lambda_e}{\sigma}} \tag{8}$$

Utilizing the equilibrium assumption that $\lambda = \lambda_0(1 - \Gamma(\underline{w}))$, the mean separations rate can be rewritten as a function of the primitives of the model:

$$E(s) = \frac{\sigma \left(1 + \phi \frac{\lambda_0(1-\Gamma(\underline{w}))}{\sigma}\right) \ln \left(1 + \phi \frac{\lambda_0(1-\Gamma(\underline{w}))}{\sigma}\right)}{\phi \cdot \frac{\lambda_0(1-\Gamma(\underline{w}))}{\sigma}} \tag{9}$$

Equations (8,9) shows that the mean separations rate is solely a function of σ and λ. However, while the employment rate depends on the relative magnitude of the offer arrival rate to the job destruction rate, the separation rate depends on the magnitudes of both. Equation (8) shows that if we observe the relative transition rates to another job as opposed to unemployment, we can back out the value of $\frac{\lambda_e}{\sigma}$, or equivalently $\phi\frac{\lambda}{\sigma}$. This will be useful when we calibrate the model below.

So far, we have not utilized the specific assumptions in the model regarding the determination of the offer wage distribution $F(w)$. As shown in Bontemps et al., the assumption of wage posting in this model leads to an equilibrium wage function $K(p)$ which is a rising function of firm productivity, p. However, we stress that for our purposes, the equilibrium employment and separations rates in equations (5) and (8) do

[1] Allowing heterogeneity in workers' reservation wages or allowing endogenous search intensity in this model would make it possible for employment to rise via an expanded labor force. We abstract from that possibility here for tractability.

[2] This expression is also derived in Hornstein et al. (2011); see their equation 11.

not depend on the details of the wage determination process.

Comparative Statics from Minimum Wage Variation: Effect on Stocks versus Flows

The extent to which there is a reduction in the equilibrium offer arrival rate λ from a rise in $\underline{w}$ depends on the shape of the firm productivity distribution $\Gamma(p)$. The elasticity of the offer arrival rate with respect to the minimum wage is $\frac{d\lambda}{d\underline{w}}\frac{\underline{w}}{\lambda^*} = -\frac{\Gamma'(\underline{w})\underline{w}}{1-\Gamma(\underline{w})}$.

We first analytically derive the minimum wage elasticity for employment by taking logs and differentiating equation (5) with respect to the minimum wage, $\underline{w}$, keeping in mind that the minimum wage affects the employment and the separation rates only through its effect on the offer arrival rate λ, since σ is assumed to be constant in the model.

$$\frac{d\ln e}{d\ln \underline{w}} == \frac{d\ln\lambda}{d\ln \underline{w}}\cdot\left(\frac{\sigma}{\lambda+\sigma}\right) = \frac{d\ln\lambda}{d\ln \underline{w}}\cdot u \tag{10}$$

The employment elasticity equals the product of the equilibrium unemployment rate u and the elasticity of the contact rate with respect to minimum wage. We can express this elasticity in terms of the primitives of the Bontemps et al. (1999, 2000) model, which shows how the response to the minimum wage depends on the shape of the productivity distribution of potentially active firms, $\Gamma(p)$.

$$\frac{d\ln e}{d\ln \underline{w}} = \left(-\frac{\Gamma'(\underline{w})\underline{w}}{1-\Gamma(\underline{w})}\right)\left(\frac{1}{1+\frac{\lambda_0(1-\Gamma(\underline{w}))}{\sigma}}\right) \tag{11}$$

We can take logs and differentiate equation (6) with respect to the minimum wage:

$$\begin{aligned}\frac{d\ln E(s)}{d\ln \underline{w}} &= \frac{d\ln\lambda}{d\ln \underline{w}}\left[\frac{1}{1+\phi\frac{\lambda}{\sigma}} + \frac{1}{\left(1+\phi\frac{\lambda}{\sigma}\right)\ln\left(1+\phi\frac{\lambda}{\sigma}\right)} - \frac{1}{\phi\frac{\lambda}{\sigma}}\right] \\ &= \frac{d\ln\lambda}{d\ln \underline{w}}\cdot\left[\frac{\phi\lambda}{(\sigma+\phi\lambda)\ln\left(1+\phi\frac{\lambda}{\sigma}\right)} - \frac{1}{1+\phi\frac{\lambda}{\sigma}}\right]\end{aligned} \tag{12}$$

Since $\lambda = \lambda_0\left(1-\Gamma(\underline{w})\right)$, we can also rewrite the expression for the separations elasticity as:

$$\frac{d\ln E(s)}{d\ln \underline{w}} = -\frac{\Gamma'(\underline{w})\underline{w}}{1-\Gamma(\underline{w})}\cdot\left[\frac{\phi\lambda_0\left(1-\Gamma(\underline{w})\right)}{\left(\sigma+\phi\lambda_0\left(1-\Gamma(\underline{w})\right)\right)\ln\left(1+\phi\frac{\lambda_0(1-\Gamma(\underline{w}))}{\sigma}\right)} - \frac{1}{1+\phi\frac{\lambda_0(1-\Gamma(\underline{w}))}{\sigma}}\right] \tag{13}$$

Note the presence of the offer arrival elasticity $\frac{d\ln\lambda}{d\ln \underline{w}} = -\frac{\Gamma'(\underline{w})\underline{w}}{1-\Gamma(\underline{w})}$ in both equations (10) and (13). 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. When considering the equilibrium in a job ladder model, it is not possible for separations to fall without some fall in employment. Note as well that 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 offer arrival elasticity $\frac{d\ln\lambda}{d\ln \underline{w}}$:

$$\frac{d\ln e/d\ln \underline{w}}{d\ln E(s)/d\ln \underline{w}} = \frac{\frac{1}{1+\frac{\lambda}{\sigma}}}{\frac{\phi\frac{\lambda}{\sigma}}{\left(1+\phi\frac{\lambda}{\sigma}\right)\ln\left(1+\phi\frac{\lambda}{\sigma}\right)} - \frac{1}{1+\phi\frac{\lambda}{\sigma}}} \tag{14}$$

Substituting the value of equilibrium $\lambda = \lambda_0(1 - \Gamma(\underline{w}))$, we can rewrite the above expression as:

$$\frac{d\ln e/d\ln \underline{w}}{d\ln E(s)/d\ln \underline{w}} = \frac{\frac{1}{1+\frac{\lambda_0\left(1-\Gamma(\underline{w})\right)}{\sigma}}}{\frac{\phi\frac{\lambda_0\left(1-\Gamma(\underline{w})\right)}{\sigma}}{\left(1+\phi\frac{\lambda_0\left(1-\Gamma(\underline{w})\right)}{\sigma}\right)\ln(1+\phi\frac{\lambda_0\left(1-\Gamma(\underline{w})\right)}{\sigma})} - \frac{1}{1+\phi\frac{\lambda_0\left(1-\Gamma(\underline{w})\right)}{\sigma}}} \tag{15}$$

To shed some more light on the interpretation of the above formula, we rewrite the expression in terms of equilibrium values of the unemployment (u), mean separations ($E(s)$) andoffer arrival (λ) rates:

$$\frac{d\ln e/d\ln \underline{w}}{d\ln E(s)/d\ln \underline{w}} = \frac{u}{\left(\frac{\sigma}{E(s)} - \frac{\sigma}{\sigma+\phi\lambda}\right)} = \frac{u}{\left(\frac{\phi\lambda}{\sigma+\phi\lambda} - \frac{E(s)-\sigma}{E(s)}\right)} \tag{16}$$

The numerator in equation (16) is the equilibrium unemployment rate, u. The denominator equals the difference between (1) the job-to-job share of separations for workers 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 jobs are less likely to stay at their jobs as compared to the workforce as a whole.[3] 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). This is a novel result—the relative magnitudes of the employment stock and flow elasticities is a function only of the equilibrium offer arrival rate λ, the job destruction rate σ, and the relative efficiency of on-the-job search, ϕ. This result is useful because it suggests that the effects of a minimum wage policy change on the relative magnitudes of the employment stock and flow elasticities depend only on parameters which can all be calibrated using the cross-sectional flows. This calibration is exactly what we do in the section below.

Here we have closed the model using the assumptions of wage posting and a fixed number of potential firms with heterogeneous productivity, as in Bontemps et al. (1999, 2000). These assumptions are reflected in equations (15) which express the ratio of the employment and separations elasticity as a function of the primitives of the model. However, note that in equations (14) and (16), the same ratio is expressed in terms of equlibrium values that derive from the more general job ladder model as derived in equations (5),(8),(10), and (12). Therefore, our key result in equation (16) does not depend on specific features of the wage distribution—which could be determined via wage posting or by wage, bargaining—once cross-sectional flows are accounted for.[4] Similarly, the details regarding how λ is affected by the minimum wage—which may depend on assumptions regarding firm entry, etc.—are also unimportant for the ratio of the employment and turnover elasticities in equation (16), once we condition on cross-sectional flows, since the term $\frac{d\ln\lambda}{d\ln \underline{w}}$ enters both the numerator and the denominator. Finally, in the section below, we use cross-sectional moments to calibrate $\frac{\lambda}{\sigma}$ and ϕ in equation (14); again, those results hold for the wider class of job ladder models.

Calibrating the Job-Ladder Model

Equation (14) also allows us to answer the following question: if we calibrate $\frac{\lambda}{\sigma}$ and ϕ using cross sectional

[3]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.

[4]Hornstein et al. (2011) also discuss the close links between employment flows and frictional wage inequality.

employment flows, what would we predict for the relative magnitudes of the employment and separation rate elasticities?

First, we calculate the predicted elasticities using parameters from the Hornstein et al. (2011) calibration of both of these parameters using cross-sectional flows between employment and unemployment, as well as flows between jobs, for the U.S. workforce as a whole. Drawing upon a number of recent studies that use the SIPP or the CPS, Hornstein et al. estimate that monthly EE flows for the U.S. workforce lie between 0.022 and 0.032, with an average of 0.027. Drawing upon Shimer (2012), they estimate that the monthly exogenous job destruction rate, σ, equals 0.03. The ratio of monthly EE flows to the monthly job destruction rates (EU) is therefore about 0.9. Note that we can rewrite equation (6) to derive an expression for the relative rates of EE and EU transitions (r_{EE})

$$r_{EE} = \frac{EE}{EU} = \frac{EE}{\sigma} = \frac{\left(1+\phi\frac{\lambda}{\sigma}\right)\ln\left(1+\phi\frac{\lambda}{\sigma}\right)}{\phi\cdot\frac{\lambda}{\sigma}} - 1 \tag{17}$$

Using 0.9 as the left hand side value (r_{EE}) in equation (17) above, we can solve for $\frac{\lambda_e}{\sigma} = \phi\frac{\lambda}{\sigma}$ 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. 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 $\phi = \frac{\frac{\lambda_e}{\sigma}}{\frac{\lambda}{\sigma}} = \frac{3.30}{14.33} = 0.23$.

What does this calibration using cross-sectional flows for the workforce as a whole 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 (14), evaluated at the calibrated parameter values to the empirical one, provides a test 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.[5]

The first column in Table A1 reports that when we substitute the calibrated values $\phi = 0.23$ and $\kappa = 14.33$ into equation (15), we find:

$$\frac{\hat{e}}{\widehat{E(s)}} = 0.22 \tag{18}$$

Here we again use the notation $\hat{x} = \frac{dx}{dMW}\frac{MW}{x}$ to represent minimum wage elasticity for a variable x. So the job ladder model calibrated by using aggregate U.S. data on cross-sectional flows suggests a substantially (nearly five times) larger separation elasticity than the employment elasticity of minimum wage. This is qualitatively similar to our results using teens $\left(0.29 = \frac{-0.059}{-0.204}\right)$ or restaurant workers $\left(0.10 = \frac{-0.022}{-0.212}\right)$.[6] However, low-wage workers tend to have much higher unemployment rates, suggesting different relative flows between employment and unemployment. For this reason, we present a calibration using teen flows in column 3 of Table A1. We first estimate the monthly transition probabilities $\tilde{UE}$ and $\tilde{EU}$ using the matched monthly CPS between 2000-2011. Based on Shimer (2012), we correct for time aggregation bias to recover UE, EU.[7]

[5] 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 run elasticities in Table 4 are statistically indistinguishable.

[6] We use the turnover rate elasticity from Table 3, since $\hat{S}$ is the elasticity of the separation rate, whereas our separations elasticities were estimated using for separation levels. Moreover, the steady state turnover and separation rate elasticities are by construction equal, so we use the turnover rate elasticity as the estimate for $\hat{S}$.

[7] The continuous time hazard rates EU, UE can be solved as functions of the discrete time probabilities $\tilde{EU}, \tilde{UE}$ as follows: $EU = \frac{\tilde{EU}\left[-\ln\left(1-\tilde{EU}-\tilde{UE}\right)\right]}{\tilde{EU}+\tilde{UE}}$ and $EU = \frac{\tilde{UE}\left[-\ln\left(1-\tilde{EU}-\tilde{UE}\right)\right]}{\tilde{EU}+\tilde{UE}}$. Analogously, the instantaneous EE rate is equal to $-\ln(1-\tilde{EE})$.

We then set $\frac{\lambda}{\sigma}$ equal to $\frac{UE}{EU} = \frac{0.225}{.035} = 6.43$. Unsurprisingly, the relative flow into employment is much lower for teens, consistent with greater unemployment rates. To estimate the relative efficiency of on the job search (ϕ) for teens, we match individuals in the CPS across months to estimate the teen hazard rates; Appendix Table A1 reports the estimated rates $EE = 0.040$ and $EU = 0.035$. We set $r_{EE} = \frac{EE}{EU} = \frac{0.040}{.035} = 1.15$ in equation (12), along with the value $\frac{\lambda}{\sigma} = 6.43$ to solve for $\phi = 0.77$. Teens have much higher EE rates than the workforce overall (0.04 versus 0.02), while also having a much higher unemployment rate (0.18 versus 0.055), therefore implying a higher efficiency of on-the-job search.[8] Using these values in equation (7) suggests a predicted ratio of elasticities:

$$\frac{\hat{e}}{\hat{E(s)}} = 0.45 \tag{19}$$

A similar calculation can be performed for restaurant workers who have transition rates $EE = 0.027$ and $EU = 0.019$, and $UE = 0.235$. We calculate $\kappa = 12.37$ and $\phi = 0.64$, generating a ratio of predicted elasticities:

$$\frac{\hat{e}}{\hat{E(s)}} = 0.25 \tag{20}$$

From our empirical results (shown in columns 2 and 4 of Table 3), we calculate the ratio of these same two elasticities to be 0.34 for teens and 0.10 for restaurant workers, as compared to the predicted ratios of 0.45 and 0.25. We find, in other words, that calibrations of the job-ladder model using cross-sectional flows suggest relative magnitudes of the two elasticities that are qualitatively similar to our empirical findings—although the relative size of the separations versus employment elasticity is not as dramatic in the model.

Overall, these findings are consistent with the idea that an increase in the minimum wage reduces job-to-job transitions that are more prevalent in the presence of frictional wage inequality.[9] 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.

[8] We also validate our approach by closely replicating the predicted ratio of elasticities using our approach in column 2 of Table A1. While the relative magnitude of the on the job search efficiency ϕ is slightly larger in our sample, we obtain a predicted ratio of elasticities of 0.19, as opposed to 0.22 using the Hornstein et al. calibration.

[9] As Hornstein et al. show, their 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. 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.

Online Appendix B: Timing of Minimum Wage Changes by State Border Pairs

Appendix B reports the timing of minimum wage changes in the state border pairs for our contiguous border county pair sample. Appendix Table B1 reports when minimum wages changes occur in each of the the 88 policy-border-pairs with such changes in our primary estimation sample of counties in pairs whose centroids are within 75 miles. Cells with minimum wage changes are marked in grey. Minimum wage events are defined as periods when there are differential increases in minimum wages across the counties within a pair.

Online Appendix C: Choice of Distance Cutoff for Contiguous County Pair Design

Appendix C provides more details on the choice of distance cutoff for a contiguous county pair design. Our QWI sample consists of the 1,130 counties that border another state. Collectively, these border counties comprise 1,181 unique county pairs. Appendix Figure C1 shows a map of the border county sample.

While most counties in the border pair sample are geographically proximate, counties in the western United States are much larger in size and irregular in shape. In some cases the geographic centroids of the counties in such pairs lie several hundred miles apart. Appendix Figure C2 shows the distribution of distances between centroids in the county pair sample, confirming the presence of such counties.

As a motivation, we show that contiguous counties whose centroids are farther apart are less similar to each other. Appendix Figure C3 non-parametrically plots the mean absolute difference in key covariates between counties in a pair by the distance between the pairs using a local polynomial smoother. The covariates include log of overall private sector employment, log of population, log of employment-to-population ratio, log of average private sector earnings, overall turnover rate and the teen share of the population. (None of these are expected to be substantially affected by the minimum wage policy.) We show the results for these variables in levels as well as 4 quarter and 12 quarter differences. As expected, in 17 out of 18 cases the differences increase as we consider counties with more distant centroids. These differences are small for counties within 50 miles of each other, but they become sizeable when the distances reach 100 miles or more.

A smaller distance cutoff trades off lower error variance from greater similarity against higher error variance from a smaller sample. The problem of choosing a cutoff is similar to the optimal bandwidth selection in a regression discontinuity design. However, the county-pair design does not lend itself to standard cross-validation based approaches because each cutoff entails a different sample. For this reason we use a data-driven randomization inference procedure to estimate the mean-squared error (MSE) of the estimator for alternative cutoffs.

We randomly assigned placebo treatments at the state level by randomly assigning minimum wage series (picked from the states in our sample) to each side of the border. This procedure retains the pattern of within-state correlation in the treatment, as well as the unconditional distribution of the treatment across all counties. By construction, the estimator has a mean of zero. We then calculate the mean-squared error of the regression coefficients averaged over the five key outcomes (log of earnings, employment, separations, hires, turnover rate) and over the teens and restaurant samples. (Given zero mean, the MSE is just the variance of the estimator.) Regressions are estimated for 100 placebo treatments using pair-specific time effects and covariates, as in Table 3, for cutoffs between 45 and 105 in increments of 10. Appendix Figure C4 shows that the 75 mile cutoff is associated with the lowest overall MSE when averaged over outcomes and samples. This criterion retains about 81 percent of the sample, eliminating mostly Western counties, as illustrated in Table B1.

To show that our results are not affected by the choice of cutoffs, Appendix Table B1 reports our key results with cutoffs ranging between 45 and 105 miles.

Online Appendix D: Impact of Minimum Wages on Non-employment Duration of Movers

The QWI dataset allows us to examine minimum wage effects separately for the sample of movers—i.e., all those who are hired (new accessions) or who separate from their employer at any point in the current quarter. In particular, we are able to assess the impact of the policy on the duration of non-employment spells of movers, which can provide additional information about how minimum wage policy affects the tightness of the low-wage labor market.

The QWI reports the average number of quarters (up to a maximum of four) spent by each separating (acceding) worker without a job subsequent (prior) to their current job.[10] Top coding of the spells and the measurement of the underlying spells in quarters makes this measure somewhat coarse.[11] However, these measures vary substantially across areas and time in expected ways and they are correlated with labor market tightness.[12]

It is useful to write the average duration of non-employment for all separations (D) as the product of the EN share of separations (f_{EN}) and the non-employment duration for workers actually transitioning out of employment (D_{EN})

$$D = f_{EN} \cdot D_{EN} = (1 - f_{EE})D_{EN}$$

This relationship holds because the non-employment duration of EE transitions is, by defintion, equal to zero. We can see that there are two ways that this duration of non-employment, D, may be affected by the policy. First, if the EE share of separations falls, but the non-employment duration D_{EN} remains constant, the mean non-employment duration across all separations, D, necessarily increases. Second, if the EE share remains constant, but the non-employment duration of EN separations rises, the non-employment duration of all separations, D, will again increase. Finally, in this paragraph we discussed the non-employment duration following a separation; but a parallel logic applies to the non-employment duration prior to being hired for those starting a new job.

Ideally, we would estimate separately the effect of the policy on EE share and on non-employment duration of those actually transitioning into non-employment (ie., D_{EN}). Unfortunately, the QWI data only reports the mean duration of non-employment for all separations, D, and does not report f_{EN} and D_{EN} separately. However, the effect on the mean non-employment duration, D, is still informative. Assuming a minimum wage increase leads to greater difficulties in finding jobs in general—through a lower offer arrival rate to workers (λ_e) and the non-employed (λ) alike—it would also raise the mean non-employment duration, D, both through longer spells between jobs and fewer job-to-job transitions. Indeed, longer spells between jobs and fewer job-to-job transitions both occur during economic downturns (Shimer 2005, 2012).

Table 1 shows that, for teens and restaurant workers, hires have longer average non-employment spells

[10] This variable refers to the duration of non-employment spell faced by those separating from a job subsequent to their separation. The value of this variable at date t refers to the average future duration of non-employment (top-coded at 4 quarters) for all workers who are separating at date t from their job.

[11] Using the LEHD, Fallick, Haltiwanger and McEntarfer (2012) report that 44 percent of separations involve re-employment in the same quarter; another 23 percent experience re-employment in the subsequent quarter; 17 percent experience re-employment within 2-3 quarters; and 21 percent of all separations last four quarters or longer.

[12] In unreported results, we find that the duration of non-employment spells for movers—especially for separations—are highly cyclical. The mean duration of non-employment spells for movers rose about 15 percent between 2006 and 2009 for all separations and 20 percent for teen separations. These peak to trough changes correspond to roughly one standard deviation in the cross-county variation in non-employment durations (Table 1).

than separations. As the contiguous county pair sample descriptive statistics in Table 1 show, teens experienced around 2.7 quarters without employment prior to being hired, and around 2.0 quarters without employment after a separation. For restaurant workers, the mean non-employment durations are 2.2 and 1.9 quarters, respectively. The somewhat lower non-employment duration for teen separations is consistent with the idea that young, low-wage workers aree climbing a career ladder—implying a relatively greater proportion of job-to-job transitions among separations than among hires.

As the first row of Table D1 shows, minimum wages raise full-quarter earnings in this sample of movers by a larger amount than in the full sample (compare with Table 5). This result confirms that earnings are indeed growing strongly for the sample of movers, as we also discussed in the context of earnings differences by tenure (Table 5). Importantly, the second row of Table D1 shows that minimum wages have virtually no impact on mean non-employment durations prior to being hired, or subsequent to separating from a job. Based on the point estimates, a 10 percent minimum wage increase changes the mean duration of non-employment by no more than 0.3 percent in magnitude for both separations and hires–and for both teens and restaurant workers. While fewer workers move in and out of jobs when the minimum wage rises, those who are moving do not appear to spend longer time between jobs. For restaurant workers, we also do not find any changes in non-employment durations among movers, with duration elasticities of -0.026 and 0.022 for hires and separations, respectively. Our findings thus indicate small effects on the employment level, large effects on employment flows, and a null effect on the non-employment durations of movers.

We note that the stable mean non-emplyment duration following all separations, D, is consistent with reductions in both EE and EN transitions which leave the share f_{EE} the same, coupled with a stable duration of non-employment for those transitioning out of work, D_{EN}. However, since the QWI does not distinguish between EE and EN separations, the lack of an impact on the overall duration of non-employment could mask a combination of (1) a shift in the job-to-job (EE) share of separations along with (2) a change in the non-employment duration of those separating from employment to non-employment (EN). What our findings do rule out is the possibility of reduced job finding probabilities for the employed and the non-employed alike, which would have unambiguously raised the average non-employment duration, D.

Table A1

Calibrated Job Ladder Model: Predicted Ratio of Employment and Separation Elasticities

	HKV Calibration (All Workers)	*Our Calibration using CPS 2000-2011 (All Workers)*	*Our Calibration using CPS 2000-2011 (Teens)*	*Our Calibration using CPS 2000-2011 (Restaurant Workers)*
EU	0.030	0.014	0.035	0.019
UE	0.430	0.238	0.224	0.235
EE	0.027	0.020	0.040	0.027
$r_{EE} = \frac{EE}{EU}$	0.900	1.419	1.153	1.435
$\phi = \frac{\lambda_E}{\lambda}$	0.23	0.32	0.77	0.64
$\kappa = \frac{\lambda}{\sigma}$	14.33	16.25	6.43	12.37
$\frac{dlne}{dln\underline{w}} \Big/ \frac{dlnS}{dln\underline{w}}$	0.22	0.19	0.45	0.25

Notes. Column 1 shows calculations using calibrated values from Hornstein, Krussell and Violante (2011). Columns 2-4 show our calibration using 2000-2011 CPS, matching individuals across months. Our estimates of the *EU*, *EE* and *UE* rates using the CPS are CPS are reported in the first three rows. Column 2 shows the calibration for all worker sample, while columns 3 and 4 shows our calibration using the teen and the restaurant samples, respectively. In each case, using the relevant samples, we first use the relative monthly probabilities $\frac{\widetilde{UE}}{\widetilde{EU}}$ in the CPS of exiting versus entering unemployment, and correct for time aggregation based on Shimer (2012) to approximate the instantaneous rate $\frac{\lambda}{\sigma} = \frac{UE}{EU}$. Next, we estimate the relative share of EE and EU transitions $r_{EE} = \frac{EE}{EU}$ for the monthly sample and use Equation (11) in the Online Appendix to solve for the relative efficiency of on the job search, ϕ. In the final row, we report the predicted ratio of employment to separation elasticities of minimum wage using Equation (9) in the Online Appendix.

Table B1

Minimum Wage Elasticities for Earnings and Employment Stocks and Flows: Robustness to Distance Cutoff

	Teens						Restaurant Workers					
	(1)	(2)	(3)	(4)	(5)	(6)	(8)	(9)	(10)	(11)	(12)	(13)
Earnings	0.204***	0.218***	0.211***	0.222***	0.223***	0.223***	0.159***	0.197***	0.196***	0.207***	0.200***	0.196***
	(0.054)	(0.050)	(0.049)	(0.047)	(0.045)	(0.044)	(0.060)	(0.064)	(0.062)	(0.059)	(0.057)	(0.056)
	43,237	65,188	77,925	83,462	87,961	90,967	42,611	63,872	76,422	81,954	85,893	89,078
Employment	-0.077	-0.058	-0.041	-0.059	-0.067	-0.067	-0.041	-0.055	-0.025	-0.022	-0.052	-0.046
	(0.116)	(0.095)	(0.085)	(0.084)	(0.083)	(0.086)	(0.088)	(0.105)	(0.095)	(0.091)	(0.100)	(0.101)
	43,855	66,014	79,041	84,702	89,203	92,275	41,627	62,420	74,273	79,089	82,540	85,144
Hires	-0.218**	-0.229**	-0.203**	-0.219**	-0.233**	-0.240**	-0.243**	-0.283*	-0.270*	-0.264**	-0.296**	-0.297**
	(0.105)	(0.100)	(0.092)	(0.094)	(0.094)	(0.098)	(0.122)	(0.149)	(0.138)	(0.134)	(0.142)	(0.141)
	41,587	63,324	75,752	80,944	84,969	87,762	39,606	59,268	70,187	74,365	77,720	80,228
Separations	-0.255**	-0.246**	-0.230**	-0.233**	-0.240**	-0.250**	-0.207*	-0.253*	-0.240*	-0.225*	-0.259*	-0.260*
	(0.116)	(0.107)	(0.099)	(0.098)	(0.094)	(0.099)	(0.121)	(0.136)	(0.127)	(0.126)	(0.135)	(0.136)
	38,541	58,977	70,122	74,952	78,704	81,438	38,510	58,214	68,866	72,859	75,954	78,408
Turnover Rate	-0.211***	-0.210***	-0.207***	-0.204***	-0.203***	-0.207***	-0.193**	-0.212**	-0.214**	-0.212**	-0.212**	-0.219**
	(0.067)	(0.072)	(0.072)	(0.072)	(0.070)	(0.068)	(0.090)	(0.088)	(0.091)	(0.090)	(0.087)	(0.085)
	38,192	58,628	69,733	74,509	78,075	80,715	37,985	57,146	67,445	71,438	74,439	76,893
Maximum distance between centroids	45	55	65	75	85	95	45	55	65	75	85	95
Percent of all pairs	42	65	78	83	87	90	44	66	79	83	87	90

Notes. The table reports estimates for alternative cutoffs in the maximum distance in miles between county centroids within a pair, as reported in the second to last row. The last row reports the fraction retained of the overall border pair sample in the 2000-2011 period when using each cutoff. The reported coefficients are for log minimum wage on the log of the dependent variable, as noted in the first column. All regressions include controls for natural log of county population and total private sector employment. Specifications 1-4 provide estimates for all teens age 14-18 regardless of industry and also include log of teen population. Specifications 5-8 are limited to all workers in the restaurant industry (NAICS722). All samples and specifications include county fixed-effects and 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 D1
Minimum Wage Elasticities for Movers: Non-Employment Duration and Earnings Changes

	Teens		Restaurant Workers	
	(1)	(2)	(3)	(4)
	Hires	Separations	Hires	Separations
Full-quarter earnings	0.287***	0.241***	0.299***	0.261***
	(0.042)	(0.050)	(0.062)	(0.051)
	76,542	70,787	71,477	70,936
Non-employment duration	-0.011	-0.000	-0.026	0.022
	(0.033)	(0.050)	(0.038)	(0.052)
	83,213	76,632	78,549	72,710

Notes. The table reports coefficients associated with log minimum wage on the log of the dependent variable noted in the first column for movers (hires and separations). "Non-employment duration" is the average number of quarters (for a maximum of 4) of that the hire was not employed prior to the new job; or the average number of quarters (for a maximum of 4) that the separating worker will stay non-employed subsequent to the separation. "Full quarter earnings" refers to log of (full-quarter) average earnings at time $t-$ at the new job for hires, and the old job for separations. All regressions include controls for natural log of county population and total private sector employment. Specifications 1-2 provide estimates for all teens age 14-18 regardless of industry, and also include log of teen population. Specifications 3-4 are limited to all workers in the restaurant industry (NAICS722). All samples and specifications include county fixed-effects and 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%. Sample sizes are reported below the standard errors for each regression.

Figure B1
Timing of Minimum Wage Changes by State Border Pair

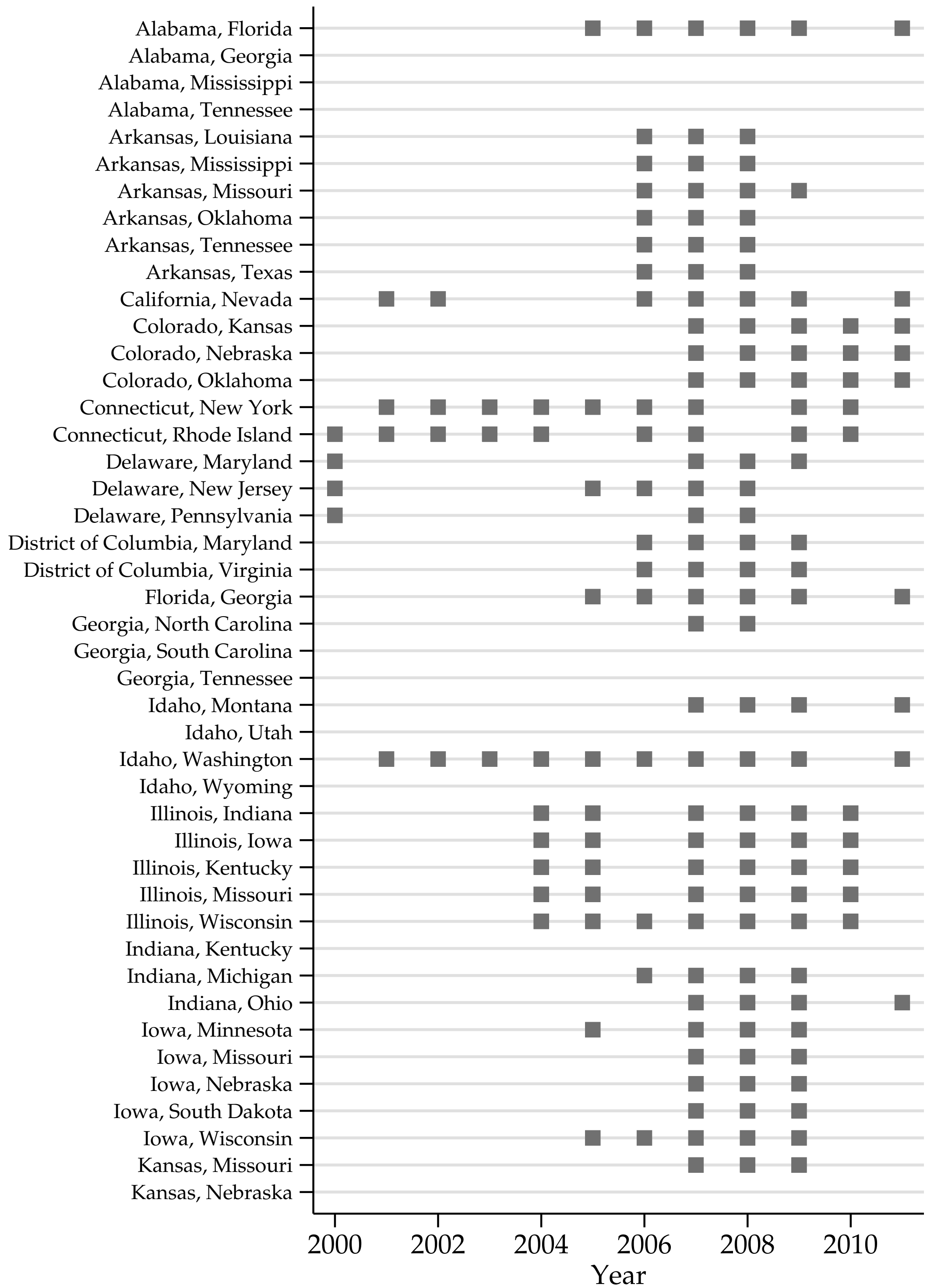
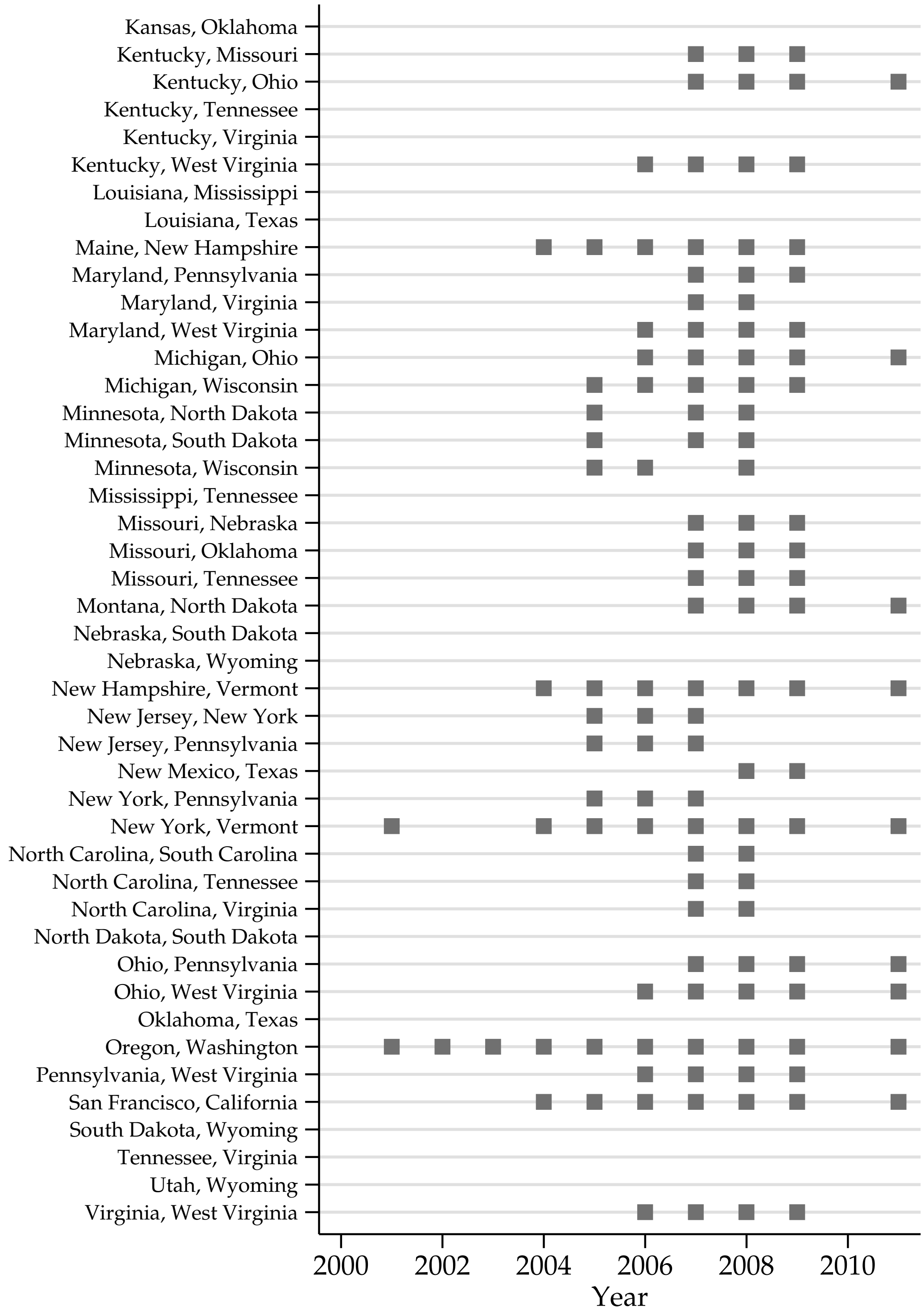


Figure B1 (continued)
Timing of Minimum Wage Changes by State Border Pair



Notes: The table reports all the 88 policy-border-pairs in our primary estimation sample that have minimum wage variation for the sample of counties in pairs whose centroids are within 75 miles. Cells with minimum wage events are marked in grey. Minimum wage events are defined as periods when there are differential increases in minimum wages across the counties within a pair.

Figure C1
Map of Contiguous Border Pairs

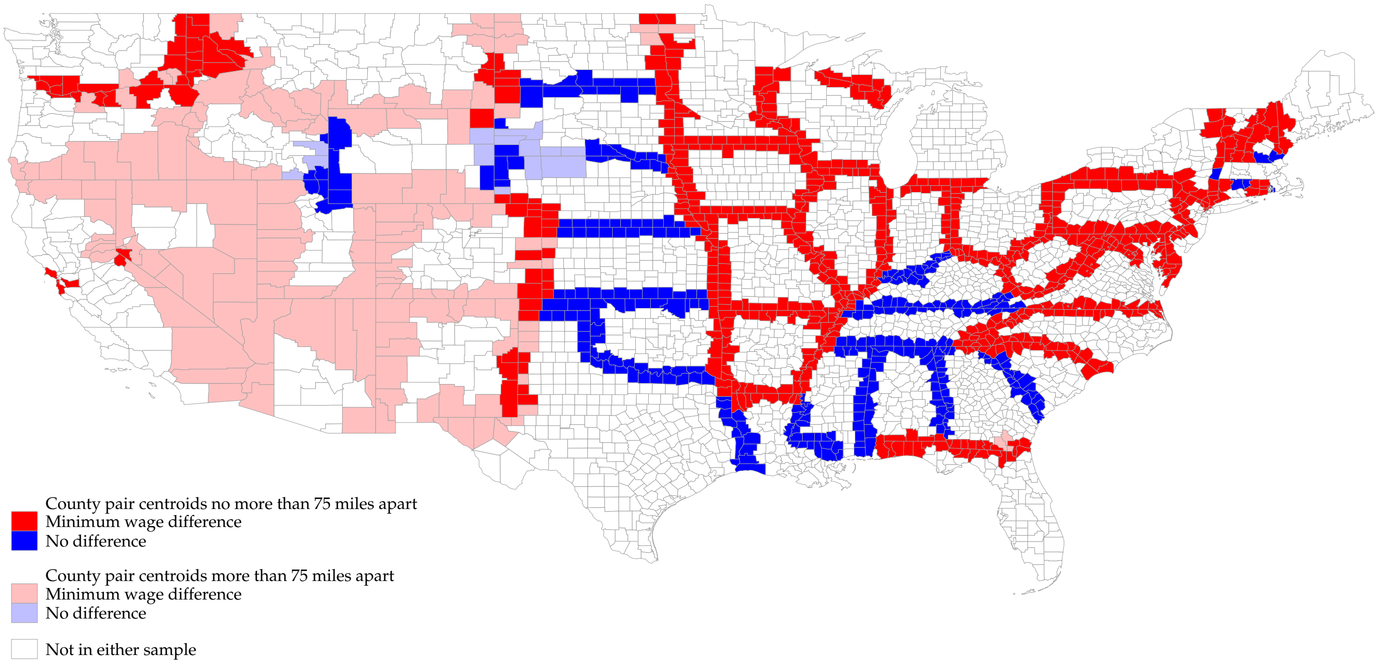


Figure C2
Distribution of Distances between Centroids in County-pair Sample

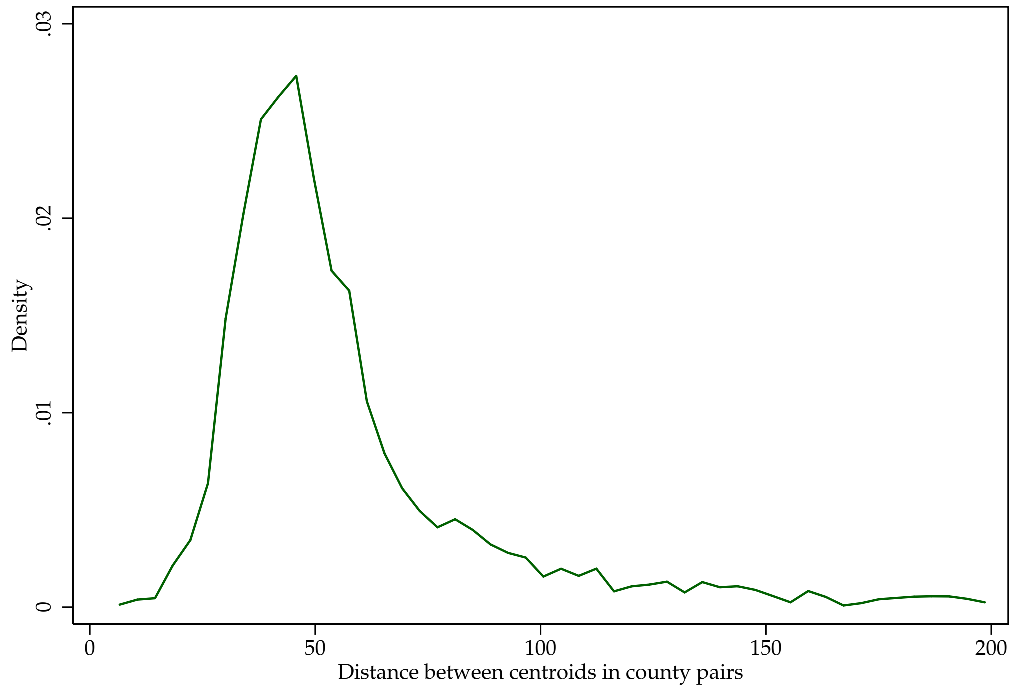
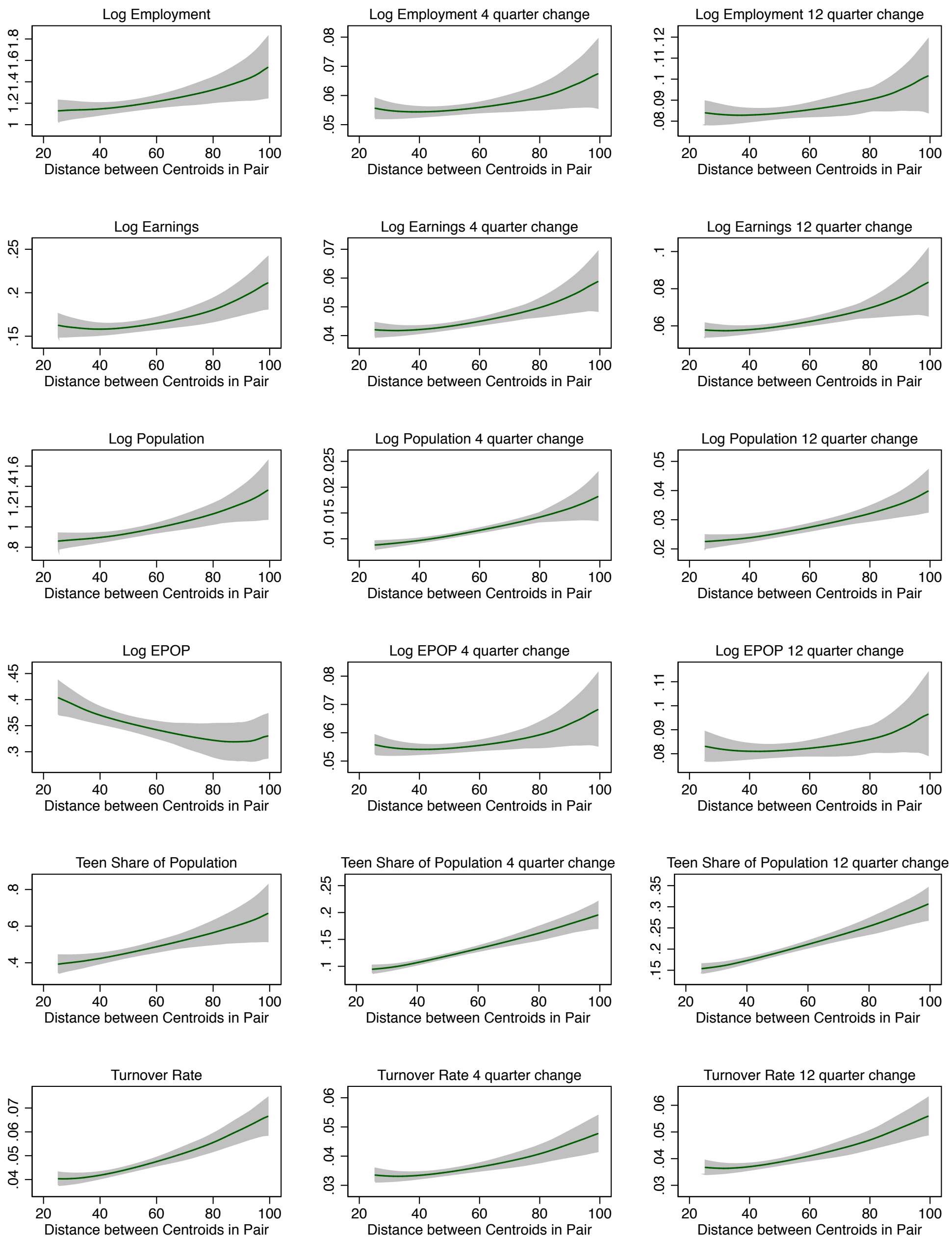
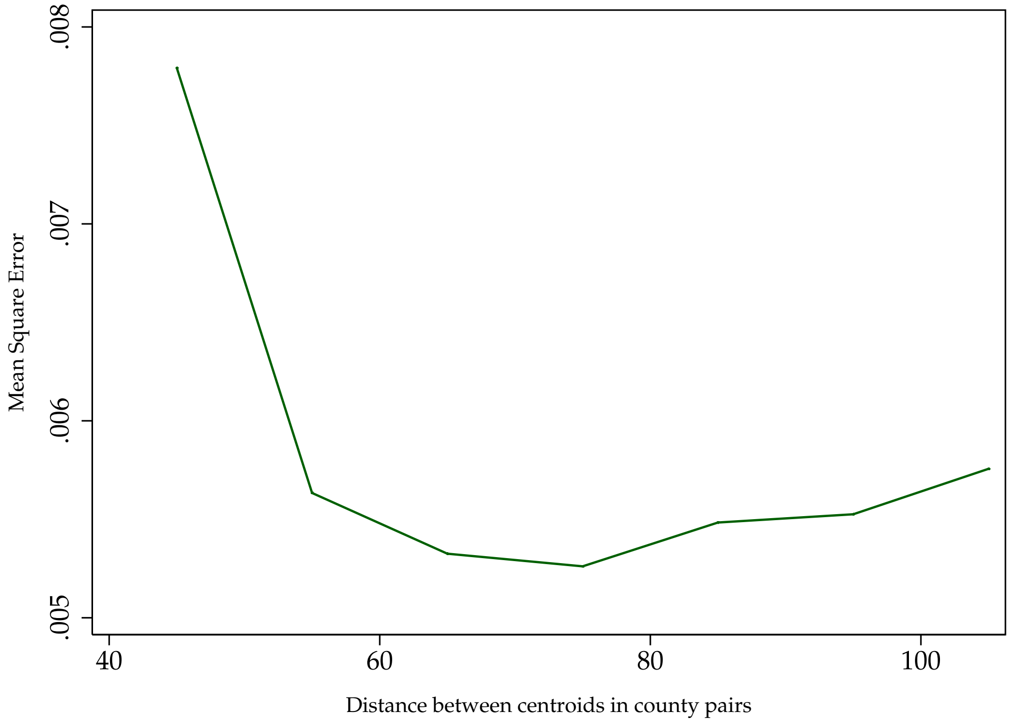


Figure C3
Mean Absolute Difference in Covariates by Distance between Centroids of a Pair



Notes. The figure plots local polynomial regressions of the mean absolute difference in the covariates on the distance between geographic centroids of the two counties for each of the pairs in the border pair sample. The covariates include levels as well as 3 and 12 quarter changes in: employment, earnings population, employment-to-population ratio, teen share of population, and turnover rate. These outcomes (in levels and changes) are computed for the 2000-2011 estimation sample. 90% confidence intervals are represented by shaded areas.

Figure C4
Choice of Distance Cutoff: Mean Squared Error of Estimator using Randomization Inference



Notes. The figure plots the mean squared error of the regression coefficients from randomly assigned placebo treatments at the state level---averaged over the five key outcomes (log of earnings, employment, separations, hires, turnover rate) and over the teens and restaurant samples. Regressions are estimated for 100 placebo treatment using pair-specific time effects and covariates as in Table 3 for cutoffs between 45 and 105 in increments of 10.