Minimum Wage Shocks, Employment Flows and Labor Market Frictions

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Abstract

We provide the first estimates of the effects of minimum wages on employment flows in the U.S. labor market, identifying the impact by using policy discontinuities at state borders. We find that minimum wages have a sizeable negative effect on employment flows but not 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, but explanations focused only on quits or only on layoffs are unlikely to explain the full complement of findings.

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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 reduced-form 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 minimum wage elasticities utilize a border-discontinuity design that eliminates biases from spatial heterogeneity present in many previous studies.

We begin by showing that minimum wages have sizeable earnings impact for these two 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 increase. For a 10 percent minimum wage increase, the 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 minimum wage increases do not affect the employment stock: our estimated employment elasticities are small in magnitude and not statistically distinguishable from zero for both teens and restaurant workers. In addition, while workers remain at their jobs longer, the same 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 results on reduced employment flows are 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 is based on a job-ladder model, in which minimum wages reduce job-to-job transitions through a reduction in the arrival rate of better paying job offers. We show analytically that in a broad class of job-ladder models—including the well-known Burdett-Mortensen (1999) model—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 an appendix, 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-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. Although the QWI dataset does not allow us to directly measure EE versus EU transitions, we show that the full complement of QWI findings implies an explanation involving both types of transitions. As we discuss below, the combination of the absence of any effects on employment and non-employment durations of movers, along with sharp reductions in separations and hires, are difficult to explain in models with only endogenous quits or only endogenous layoffs. With some additional assumptions we show that we can decompose the separations into those transitioning to other jobs and those transitioning into unemployment. We do so by using cross-sectional worker flows in conjunction with the minimum wage elasticities for employment, turnover and non-employment duration of movers. We estimate that job-to-job transitions account for about 40 percent of teen separations to jobs or unemployment, and about 55 percent of such separations for restaurant workers. The estimated job-to-job share of the marginal separations are broadly similar to the cross-sectional proportion of separations to other jobs.

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. 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 those found in this paper: 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 our paper. 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-Mortensen case that the authors 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 exactly how quits are defined. Most relevant to this paper, they find an overall separation elasticity of between -0.27 and -0.35 for teens, which is not so different from what we find in this paper (-0.23). Similar to this paper, 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, which is similar to our findings as well. Brochu and Green differ from from us in finding a 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 serious concerns about their identification and inference. For example, Brochu and Green's empirical strategy cannot rule out that heterogeneous spatial trends are driving some of their findings on layoffs and employment—trends that we show are quite important in the U.S. context. Indeed, 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 at San Francisco International Airport.[1] Overall, compared to these papers, we are able to estimate the responses of employment flows to minimum wage changes using much richer variation and a more credible identification strategy.

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

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

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

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 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—a threshold selected by randomization inference with placebo laws.

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

[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).

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 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 so as to better understand 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_e to workers, and λ to the unemployed. Workers move if they receive a higher wage than at their current job, w. Given an offer wage distribution $F(w)$, this occurs at the rate $\lambda_e \cdot [1 - F(w)]$. 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 the Online Appendix (Section A1), we build on Nagypal (2005) and Hornstein, Krusell and Violante (2011) to analytically derive the implications of the job-ladder model for the relative magnitudes of the employment stock and flow responses to a minimum wage increase. While both the employment level and separations may fall from a minimum wage increase, we find that the ratio of the separations elasticity to the employment elasticity is rising in the extent of frictional wage dispersion. The intuition behind this result is that a greater wage dispersion implies a higher rate at which workers find a better paying job (i.e., $\lambda_e \cdot [1 - F(w)]$), and a higher dispersion in those rates across workers. A higher minimum wage leads to fewer job-to-job transitions, and the effect is magnified when job-to-job transitions are prevalent in the labor market.

In the Online Appendix, we also provide a calibration of the job-ladder model using cross-sectional employment flows from the Current Population Survey. 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 that these findings apply to a broad class of models with on-the-job search, including the well-known Burdett-Mortensen (1999) model with wage posting and the Pissarides (2000) or Flinn and Mabli (2008) 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-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. One way that may occur is 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 were 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 learning model, layoffs fall, causing the EU rate to fall.

The QWI data we use in this paper provides rich information on hires and separations, but it does not disaggregate separations into EE and EU components. The QWI does, however, provide information on non-employment durations among job-changers. We utilize this information, together with the cross-sectional data on gross worker flows from the Current Population Survey, to assess the relative importance of the quits and layoff channels in the U.S. context.

3 Research design and data sources

3.1 Identification strategy

Minimum wage policies are not randomly distributed across various 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 generalizes Card and Krueger (2000) and exploits minimum wage policy discontinuities at state borders by comparing outcomes from all U.S. counties on either side of a state border.[4] As shown in detail in Dube, Lester and Reich, this research design has desirable properties for identifying minimum wage effects. Measuring labor market outcomes from an immediately adjacent county provides a better control group, since firms and workers on either side are generally affected by the same idiosyncratic local trends and experience macroeconomic shocks at roughly the same time.[5] In the next section, 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

[4]Figure A1 provides a map of the border sample, and indicates which pairs have some variation in minimum wages. It also identifies which pairs are used in our estimation sample where county centroids are no more than 75 miles apart.

[5]Allegretto, Dube, Reich and Zipperer (2013) also show in detail that spatial controls are able to eliminate confounding pre-trends, and that the synthetic control estimator is indeed more likely to pick nearby donors as controls, thereby refuting the claims to the contrary made by Neumark, Salas and Wascher (2013).

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.

Since minimum wage policies in the U.S. also tend to exhibit spatial clustering, empirical methods that do not control for spatial heterogeneity can produce highly misleading estimates. In particular, using a national panel-data model with state (or county) and time fixed effects generates an omitted variables bias. As a result, such two-way fixed effects models often attribute to minimum wage policies the effects of regional differences in the growth of low-wage employment that are independent of minimum wage policies. As documentation of this point, 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 fixed effects specification; in contrast, there are no such pre-trends using the border counties approach.[6]

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.

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

$$y^k_{ipt} = \alpha_k + \beta_k \ln(MW_{it}) + \delta_k \ln(emp^{TOT}_{it}) + \gamma_k \ln(pop_{it}) + \psi_i + \tau_t + \epsilon_{ipt} \tag{1}$$

Here y^k_{ipt} refers to the dependent variable—which could be the log of earnings, employment, separations, hires, or the turnover rate—in county i, in county-pair p, at time t, for each of the specific industry or demographic groups k (e.g., restaurant workers or teens). Because the sample consists of all cross-state contiguous county pairs, a given county can be part of multiple pairs if it has more than one adjacent county across the state line. In addition, given the time frame of our panel dataset, a given county can be either a "treated" or "control" unit, depending on the timing of minimum wage changes between the affected states. The coefficient β_k on the minimum wage variable $\ln(MW_{it})$ is the primary coefficient of interest; it is reported in each of the tables below.[7]

[6]Evidence of bias in measured minimum wage effects due to spatial heterogeneity is also presented in Addison, Blackburn and Cotti (2009, 2010), Allegretto, Dube and Reich (2011), and Allegretto, Dube, Reich and Zipperer (2013).

[7]To be clear, we are estimating the equation separately for each outcome y and industry/demographic

Specification 1 also includes controls for the natural log of total private sector employment and population in each county.[8] The ψ_i term represents a county fixed effect. Crucially, the common time fixed effects τ_t are assumed to be constant across counties, which rules out possibly heterogeneous trends.[9]

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

$$y^k_{ipt} = \alpha'_k + \beta'_k \ln(MW_{it}) + \delta'_k \ln(emp^{TOT}_{it}) + \gamma'_k \ln(pop_{it}) + \mu_i + \tau_{pt} + e_{ipt} \qquad (2)$$

This specification is analogous to specification 1 in every respect except for the inclusion of a pair-specific time effect τ_{pt}, rather than a common time effect, τ_t. Hence, specification 2 uses the within-pair variation across all pairs and effectively pools the estimates. The identifying assumption for the border-discontinuity specification is that, conditional on covariates and county fixed effects, minimum wages are uncorrelated with the residual outcome within a county pair. This assumption is much weaker than the assumption justifying the conventional specification 1.[10]

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

group k. All coefficients, including fixed effects, are allowed to differ across regressions for each outcome and group.

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

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

[10]Our regressions are estimating the treatment effects of the minimum wage policy on outcomes y^k. The estimated coefficients are reduced form and do not have direct structural interpretations. For this reason, our controls are included only to account for possible confounders; they do not include possible intermediate variables or other outcomes y^{-k} through which the policy can effect an outcome.

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 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 has 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

[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.

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 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 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

[12]To protect confidentiality the QWI "fuzzes" the data for some observations, when necessary. 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 $t+1$.

[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.

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 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. Abowd and Vilhuber (2011) note that there are "differences in data quality between the 1990s and 2000s ... due to the inclusion of 30 states beyond the original 18 included in the 2003 initial release of the QWI." Moreover, 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 analysis; by 2000, *42* states had come on line.[17]

State minimum wage policies varied considerably during the 2000-2011 period. In Figure 2, 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

[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. While the results are very similar, the precision of our estimates is sometimes reduced, even with the increased sample, which is consistent with worse data quality in the 1990s. 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.

minimum wage increases when we pool across federal and local policy changes. Figure 3 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 4 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 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. Appendix Figure A1 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

[18] The youngest age category reported in the QWI is 14-18.
[19] These statistics on restaurants exclude drinking places.

Figure A2 shows the distribution of distances between centroids in the county pair sample, confirming the presence of such counties. Appendix Figure A3 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 Appendix Section A3 provides more details. When averaged over our five key outcomes, the 75 mile cutoff produced the smallest MSE, as shown in Appendix Figure A4.[20] This criterion retains about 81 percent of the sample, eliminating mostly Western counties, as illustrated in Figure A1. To show that our results are not affected by the choice of cutoffs, Appendix Table A1 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.

[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 A3.

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

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, with centroids no greater 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 lower. Hire, separation and turnover rates as well as the fraction short-term (employed less than one quarter) are close to identical in both samples. Among movers, earnings at the job and duration of non-employment are also very similar in both samples. We surmise that the border-county sample is composed of somewhat smaller counties, but this difference is modest. All the other characteristics of the two samples, 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

[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.

[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

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 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.

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.

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 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 differential 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 -0.161 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 -0.079 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

[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)

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.

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 change in the employment stock that is 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).

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 5 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

[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).

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 can produce distortions in the data. As an added check, columns 3 and 7 show the results using only the counties that never report any distorted data. Depending on the variables, this excludes between one-fourth and two-thirds of the sample. The loss of data is particularly large for the turnover rate, which is a composite measure (and whose data quality can be lowered by distortion in reported hires, separations or employment.) Qualitatively, our results still hold: we find sizeable earnings effects, small employment effects, and much larger reductions in employment flows.

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 $\ln(MW_t)$.[28] 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.[29]

[28] 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.

[29] 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).

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 doing (match-specific human capital) or from learning about match quality.[30] 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). The QWI does not report outcomes for those with less than a full-quarter tenure, but we can back 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. The earnings effects for less-than-full-quarter employment, although more noisy, are larger than for full-quarter employees in the restaurant sample. For the teen sample, the standard errors are much larger in this sample and rule out meaningful comparison.

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

[30] Nagypal 2006 discusses how these two cases can be distinguished.

[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

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 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 8, where we consider the sample of full-quarter hires—workers with tenure between 1 and 2 quarters. In this sample, we find earnings elasticity of 0.29 (0.30) for teens (restaurant workers), which also exceed the full-quarter earnings elasticity of 0.19 (0.15) for teens (restaurant workers). Overall, the evidence shows that earnings increases relatively more for low-tenured workers, but we also see substantial earnings increase among higher tenured workers.

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 (2006), 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 (2006) 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

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 $\left[\frac{dY_1}{d\underline{w}}\frac{\underline{w}}{Y_1}\right]$.

[33]Differences in sample sizes in regressions for the 3 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.

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, in order to make use of the highest quality QWI data, we focus our main results on the 2000-2011 time period. Here we check our main results using the more extended QWI sample, 1990-2011. Table 6 shows these results, displayed for teens in columns 1-4 and for restaurant workers in columns 5-8. Since the intensity of recessions varies in states that are more likely to increase minimum wages (Allegretto et. al 2013), and since recessions have very different patterns of employment flows, Table 6 also shows our results when we exclude recession years. Although the magnitudes of the individual coefficients for each outcome variable differ somewhat across these four samples, the qualitative results remain the same as before: 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. It is noteworthy that when averaged over outcomes and groups, the extended period has 27 percent more observations but 16 percent *larger* standard errors. This pattern of increased errors, which is consistent with the data quality issues in the 1990s discussed in Abowd and Vilhuber (2011), provides additional validation for using the 2000-2011 period as our primary sample.

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 disemployment effects, substitution effects might still be present, affecting the shares of different groups in particular jobs.

To address this question directly we report in Table 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 are modest (under -0.11 in magnitude) and are never statistically significant. In all, we do not find any labor-labor substitution along the age and gender categories in our data.

More generally, if minimum wage increases lead to a reallocation of workers, one would expect 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.[34] 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.[35]

4.6 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 being hired (new accessions) or those separating from the job 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. These findings constitute additional evidence that helps us estimate the channels of impact and assess alternative explanations.

The non-employment duration of hires and separations also provides additional information about the tightness of the labor market. Generally speaking, a tighter labor market is associated with shorter spells between jobs and more job-to-job transitions (Shimer 2012, 2005). 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. The top coding of the spells and the measurement of the underlying spells in quarters makes this measure somewhat coarse.[36] However, these measures vary substantially across areas and time in expected ways and they are correlated with labor market tightness.[37]

[34] 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.

[35] 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.

[36] 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.

[37] 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

Table 1 shows that, for teens and restaurant workers, hires have longer average non-employment spells than separations. As shown in Table 1, teens spent 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 are likely to be climbing a career ladder–implying a relatively greater proportion of job-to-job transitions among separations than among hires.

As shown in the first row of Table 8, minimum wages raise full-quarter earnings in this sample of movers by a larger amount than in the full sample (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, Table 8 shows that minimum wages have virtually no impact on the mean non-employment duration 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 this null effect on non-employment durations for movers. As we explain in the next section, it is easier to explain this combination of results with reductions in *both EE* and *EU* flows than with reductions in only one of these flows.

5 Assessing the the role of different types of transitions

The core finding in this paper is that minimum wage increases tend to reduce separations and hires, but not employment levels. As we noted in Section 2, and show in greater detail in the Online Appendix, the job-ladder model can rationalize such a finding in the presence of frictional wage dispersion. Moreover, a calibrated job-ladder model suggests that employment elasticities are indeed likely to be quite a bit smaller than separations and hires elasticities. Depending on the sample used to calibrate the model, the ratio of the

changes correspond to roughly one standard deviation in the cross-county variation in non-employment durations (Table 1).

employment elasticity to the turnover elasticity is predicted to be somewhere between 0.2 and 0.45. For teens, the estimated ratio of the empirical elasticities is around 0.34, while for restaurant workers it is around 0.1. The model can match the qualitative features of the data, although it tends to understate somewhat the disparity between the two elasticities.

As we have noted, the standard job-ladder model suggests that all the reduction in turnover occurs in the job-to-job (*EE*) component, since the rate of job destruction (and hence *EU* rates) is assumed to be exogenous. The Mortensen-Pissarides model as modified by Brochu and Green provides a polar opposite case: that model has no on-the-job search and hence no job-to-job transitions. Consequently, all of the reduction is assumed to occur in the EU rate. In reality, both *EE* and *EU* rates are important in the cross section. 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 A2). Nagypal (2008) also shows that a majority of separations to other jobs or unemployment were to other jobs.

In this section, we use our findings on the non-employment duration of movers—along with additional information on employment flows from the monthly CPS—to assess the likely importance of EE and EU transitions, the channels highlighted by the ladder and match quality models, respectively. We use the minimum wage elasticities of employment separations, employment rate, non-employment duration of separations, as well as the unemployment rate and the assumption of steady state relationships, to decompose the separation elasticity into job-to-job (EE) and job-to-unemployment (EU) components. This decomposition is not based on any specific model, such as the job-ladder model or the match quality model. It is consistent with a much wider class of models that share the following properties: (1) There are two states: employment and unemployment. (2) There are flows between jobs, as well as between jobs and unemployment. (3) There is a constant hazard rate out of unemployment. (4) Stocks and flows obey restrictions imposed by steady state.[38]

First, we note that by definition, the unemployment duration of all movers (D)—which we observe—equals the product of the EU share of separations ($\frac{EU}{S}$) and the unemployment duration of those transitioning to unemployment (D_U), i.e., $D = \left(\frac{EU}{S}\right) D_U$. This equality holds as the unemployment duration of EE movers is zero. Denoting as $\hat{x}$ the elasticity of x with respect to the minimum wage, we can then write the following accounting identity: $\hat{EU} = \hat{D} + \hat{S} - \hat{D}_U$.

Since we observe the overall separation elasticity $\hat{S}$, knowledge of what happens to D_U would allow us to deduce the elasticity of EU separations. Although we do not directly

[38]In these calculations we consider two states only: employment and unemployment. We therefore assume that, on the margin, all the relevant movements in the response to a minimum wage shock occur between employment and unemployment, and not out of the labor force.

observe D_U in our data, in the steady state flows and stocks have to satisfy $\frac{u}{1-u} = \frac{EU}{UE} = EU \cdot D_U$ where UE are unemployment-to-employment transitions and u is the unemployment rate.[39] In the Online Appendix (Section A2),we use the accounting identity, as well as the steady state relationship, derive the following expression for the minimum wage elasticity of the EU rate:

$$\hat{EU} = \left(\hat{D} + \hat{S} + \hat{e}\frac{1}{u}\right)\frac{1}{2} \tag{3}$$

$\hat{S}, \hat{e}, \hat{D}, \hat{EU}$ are the minimum wage elasticities for overall separation rate, employment, non-employment duration of movers, and the EU rate respectively, while u is the unemployment rate.

For teens, we use the employment elasticity of -0.06, separation rate elasticity of -0.20, and non-employment duration elasticity of 0.00, as reported in Tables 3 and 8.[40] We also use the average teen unemployment rate $u = 0.18$ during our sample period (2000-2011) and thereby estimate $\hat{EU} = \left(-0.000 - 0.203 - 0.059\frac{1}{0.18}\right)\frac{1}{2} = -0.265$. Therefore, while the overall transition elasticity $\hat{S}$ for teens is -0.203, the implied magnitude of the elasticity for EU transitions is somewhat larger, at -0.265. Consistent with the qualitative argument above, this decomposition suggests an important role for separations into unemployment.

In addition, the overall separation elasticity is simply the EE share-weighted average of of $\hat{EE}$ and $\hat{EU}$ (see Appendix A2 for details). So, if we denote $r_{EE} = \frac{EE}{EU}$, we can calculate the former as:

$$\hat{EE} = \left(\frac{1 + r_{EE}}{r_{EE}}\right)\hat{S} - \left(\frac{1}{r_{EE}}\right)\hat{EU} \tag{4}$$

We use matched monthly CPS data over our sample period and calculate that monthly EE separations account for about 53.6 percent of all $EE + EU$ separations for teens; Appendix Table A2 reports the ratio $r_{EE} = 1.155$. With this added parameter, we can also calculate the EE elasticities as follows: $\hat{EE} = \frac{1+1.155}{1.155}(-0.203) - \left(\frac{1}{1.155}\right)(-0.265) = -0.149$. In other words, we find negative elasticities for both types of separations and larger drops in EU separations. These calculations suggest that the reduction in EE transitions account for around 39 percent $\left(\frac{-0.149 \times 0.536}{-0.203}\right)$ of all teen transitions.

We also perform analogous decompositions for the restaurant sample. It is more difficult to estimate the unemployment rate facing workers in an industry than for a demographic

[39] With a constant hazard out of unemployment, $D_U = \frac{1}{UE}$,

[40] For $\hat{S}$, 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 separation levels. Moreover, since the steady state turnover and separation rate elasticities are by contraction equal, we use the turnover rate elasticity as the estimate for $\hat{S}$.

group. For this reason we use two approaches. First, we use the definition $u = \frac{EU}{EU+UE}$ and use the CPS to estimate the rates at which unemployed individuals transition into restaurant employment (UE) and from restaurant employment to unemployment (EU) to estimate $u = 0.08$. As a check, we also simply calculate the mean unemployment rate of individuals who report restaurants as their industry status, and also obtain $u = 0.08$. We estimate the EE share of separations for restaurant workers to be 0.59. Using this result, along with minimum wage elasticities for restaurants $\hat{e} = -0.022$, $\hat{S} = -0.212$, $\hat{D} = 0.022$, we then use equations (3) and (4) to calculate:

$$\hat{EU} = -0.232, \hat{EE} = -0.198$$

For restaurants, the implied elasticities for EU and EE transitions are similar in magnitude and EE transitions account for about 55 percent of the reduction in transitions.

We note that the estimation of the elasticity $\hat{EE}$ (but not $\hat{EU}$) is somewhat sensitive to the EE share parameter. For example, if the share is larger, say 0.7, which is similar to Nagypal (2005), then the reduction in EE transitions would account for around 61 percent (56 percent) of the reduction for teens (restaurant workers). In contrast, if EE transitions are only 40 percent of $EE + EU$ transitions, they would account for about 22 percent (50 percent) of the reduction for teens (restaurant workers). For this reason, we consider 20 to 60 percent as the plausible range for the EE share of reduced separations for teens, and 50 to 60 percent as the range for restaurant workers.

Several additional caveats apply to this analysis. Perhaps most importantly, we do not account for movement out of the labor force; with movements between three states we would not be able to decompose the separation elasticities—given the number of estimates we have—without imposing additional assumptions. We of course do acknowledge the importance of movements out of the labor force, especially for teens: the CPS tabulations suggest that a majority of teens leaving their jobs are also leaving the labor force. However, to the extent that such separations are mostly exogenous (such as returning to school), they will represent only a small component of the *marginal* flows; unfortunately the actual amount is unknown. Second, we do not account for possible heterogeneity or duration dependence in the hazard out of unemployment. These simplifications could affect the decomposition of the reduction in separations into the EE and EU components. Better data would more clearly identify job-to-job transitions and obviate the need for making these assumptions to deduce the impacts on different types of transitions.

These caveats notwithstanding, by taking cross-sectional moments and steady state logic into account, our decompositions clearly point to the likely qualitative importance of both types of separations (EE andEU) in explaining minimum wage effects on stocks, flows and durations of employment. For example, equation (3) suggests that in a model without any EE transitions (i.e., $\hat{EU} = \hat{S}$), the predicted elasticity of non-employment duration $\hat{D}$ for teens would be +0.12. In a model with EE transitions but with exogenous job destruction rates (i.e., $\hat{EU} = 0$)$\hat{D}$ would be +0.53. Both predictions are at odds with the empirical values of $\hat{D}$, which are close to zero.

6 Discussion

The contributions of this paper are twofold. First, we provide minimum wage elasticities of earnings, employment stocks and employment flows for teens as well as for a high impact industry—restaurants. 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 remain stable in response to a minimum wage increase, employment flows fall substantially. Average separations, hires and turnover rates decline significantly among teen workers and restaurant establishments. These changes occur within three quarters of the minimum wage increase and they persist. 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. We do not detect any such labor-labor substitution in restaurants in response to minimum wage increases with respect to age and gender.

Second, 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. For this reason, we additionally use results on the non-employment durations of those moving in and out of jobs along with the results on employment stocks and flows and steady-state restrictions to perform a decomposition exercise. This exercise suggests that both the quits and layoffs channels are important in explaining reduced separations.

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. While the QWI dataset does not separately report job-to-job transitions versus transitions to non-employment, the combination of cross-sectional flows and reduced-form estimates suggests that a substantial part (perhaps half) of the reduced separations involve reduced flows to other jobs, consistent with a job-ladder model. Transitions to unemployment also fall at least as much. This result suggests some type of match quality mechanism is also at work. The duration dependence in the reduction in separations is consistent with the job being an experience good, as in the match quality model. However, an extended job-ladder model with job-specific capital can also explain such a finding.

Overall, these results emphasize the importance of looking beyond employment rates to understand the impacts of minimum wages. Clearly, minimum wage policies substantially reduce turnover and increase job stability, even without affecting overall employment levels for highly affected groups, such as teens. An important proportion of this reduced turnover seems to occur by reducing job-to-job transitions, indicating the presence of frictional wage dispersion. The likely reduction in flows to unemployment suggests the minimum wage also affects firm decisions to lay off workers and search anew, versus retain an existing match. Both channels underscore the relevance of the search and matching process in the low-wage labor market.

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 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? Answers to these questions are important for fully understanding 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
<u>All</u>								
Monthly Earnings	478	788	825	262	466	140	818	240
Employment	1255	3879	2810	9538	1218	2963	2741	7759
Hire Rates	0.691	0.533	0.434	0.367	0.661	0.349	0.423	0.228
Separation Rates	0.552	0.336	0.418	0.170	0.528	0.241	0.411	0.162
Turnover Rate	0.624	0.398	0.428	0.244	0.597	0.267	0.419	0.182
Fraction Short Term (tenure<1q)	0.309	0.114	0.250	0.077	0.303	0.106	0.246	0.074
<u>Full Quarter (Tenure ≥ 1q)</u>								
Monthly Earnings	588	198	979	317	573	177	968	277
Employment	872	2728	2161	7610	848	2065	2109	6152
Hire Rates	0.205	0.057	0.146	0.048	0.206	0.054	0.145	0.047
Separation Rates	0.145	0.050	0.143	0.047	0.143	0.045	0.142	0.046
Turnover Rate	0.262	0.091	0.198	0.156	0.257	0.076	0.194	0.081
<u>Movers (Separations)</u>								
Monthly Earnings (Full Qtr)	531	210	738	295	519	190	729	248
Quarters of Non-Employment	1.955	0.387	1.738	0.391	2.035	0.376	1.870	0.392
<u>Movers (Hires)</u>								
Monthly Earnings (Full Qtr)	575	208	730	267	565	188	723	234
Quarters of Non-Employment	2.658	0.368	2.052	0.404	2.715	0.353	2.169	0.401
Fraction Female	0.533	0.064	0.647	0.093	0.535	0.059	0.652	0.090
Fraction Teen	-	-	0.214	0.082	-	-	0.216	0.081
Fraction Young Adult	-	-	0.151	0.044	-	-	0.151	0.042

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 132,390 to 146,519 for the all county sample, and 80,966 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	0.182***	0.222***	0.204***	0.207***
	(0.037)	(0.048)	(0.027)	(0.060)
	86,310	86,310	84,792	84,792
Employment	-0.161**	-0.059	-0.079**	-0.022
	(0.071)	(0.086)	(0.039)	(0.093)
	87,558	87,558	81,835	81,835
Hires	-0.510***	-0.219**	-0.468***	-0.264*
	(0.092)	(0.095)	(0.084)	(0.137)
	83,678	83,678	77,037	77,037
Separations	-0.539***	-0.233**	-0.465***	-0.225*
	(0.099)	(0.099)	(0.077)	(0.129)
	77,578	77,578	75,538	75,538
Turnover Rate	-0.377***	-0.204***	-0.389***	-0.212**
	(0.062)	(0.074)	(0.068)	(0.091)
	77,123	77,123	74,079	74,079
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) $lnMW_{t+4}$	$lnMW_t$	$lnMW_{t-4}$	(5)	(6)	(7)	(8) $lnMW_{t+4}$	$lnMW_t$	$lnMW_{t-4}$
Earnings	0.185***	0.215***	0.237***	-0.058	0.207***	-0.043	0.176***	0.206***	0.172***	-0.011	0.210**	-0.022
	(0.063)	(0.048)	(0.044)	(0.040)	(0.058)	(0.050)	(0.060)	(0.060)	(0.046)	(0.047)	(0.083)	(0.032)
	86,310	86,310	58,872		86,310		84,792	84,792	39,106		84,792	
Employment	-0.003	-0.059	-0.051	0.084	-0.052	0.098	-0.084	-0.022	-0.069	0.093	0.027	0.004
	(0.086)	(0.086)	(0.071)	(0.068)	(0.114)	(0.068)	(0.099)	(0.093)	(.068)	(0.068)	(0.113)	(0.074)
	87,558	87,558	66,539		87,558		81,835	81,835	47,044		81,835	
Hires	-0.180*	-0.164**	-0.171	-0.005	-0.252*	0.080	-0.305**	-0.222*	-0.199*	0.031	-0.256	0.023
	(0.105)	(0.073)	(0.111)	(0.085)	(0.132)	(0.102)	(0.140)	(0.129)	(0.112)	(0.099)	(0.174)	(0.116)
	83,678	83,678	45,706		83,678		77,037	77,037	27,519		77,037	
Separations	-0.225**	-0.183**	-0.187**	0.049	-0.236	0.076	-0.264**	-0.205*	-0.159	0.046	-0.212	0.030
	(0.105)	(0.073)	(0.093)	(0.091)	(0.151)	(0.084)	(0.132)	(0.123)	(0.099)	(0.094)	(0.168)	(0.087)
	77,578	77,578	41,432		77,578		75,538	75,538	29,204		75,538	
Turnover Rate	-0.212***	-0.146***	-0.118	-0.085	-0.258***	0.021	-0.203**	-0.184**	-0.147*	-0.067	-0.254**	0.015
	(0.073)	(0.048)	(0.075)	(0.065)	(0.100)	(0.057)	(0.097)	(0.081)	(0.085)	(0.079)	(0.126)	(0.099)
	77,123	77,123	37,297		77,123		74,079	74,079	24,944		74,079	
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 only use counties that never report any "distorted" data (data quality flag=9) due to fuzzing. 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*		
	(0.017)			(0.016)		
	84,601			79,882		
Earnings	0.222***	0.185***	0.180	0.207***	0.153***	0.527***
	(0.048)	(0.050)	(0.190)	(0.060)	(0.053)	(0.100)
	86,310	85,764	75,551	84,792	84,323	80,002
Alternative Calculation			0.321			0.474
Employment	-0.059	-0.034	-0.146*	-0.022	0.016	-0.127
	(0.086)	(0.092)	(0.076)	(0.093)	(0.095)	(0.114)
	87,558	84,601	84,530	81,835	79,882	79,836
Hires	-0.219**	-0.114	-0.405***	-0.264*	-0.061	-0.340*
	(0.095)	(0.105)	(0.123)	(0.137)	(0.132)	(0.181)
	83,678	69,398	68,773	77,037	64,155	63,655
Separations	-0.233**	-0.107	-0.328***	-0.225*	0.005	-0.318**
	(0.099)	(0.072)	(0.117)	(0.129)	(0.088)	(0.151)
	77,578	61,505	61,504	75,538	65,882	65,882
Turnover Rate	-0.204***	-0.082	-0.175**	-0.212**	-0.065	-0.279***
	(0.074)	(0.050)	(0.076)	(0.091)	(0.084)	(0.078)
	85,225	77,123	75,810	83,722	74,079	72,981

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***	0.233***	0.222***	0.191***	0.256***	0.225***	0.207***	0.172***
	(0.072)	(0.080)	(0.048)	(0.047)	(0.071)	(0.076)	(0.060)	(0.058)
	110,057	89,314	86,310	66,101	108,115	87,728	84,792	64,939
Employment	-0.093	-0.081	-0.059	-0.041	0.004	-0.035	-0.022	-0.057
	(0.095)	(0.101)	(0.086)	(0.089)	(0.122)	(0.131)	(0.093)	(0.100)
	111,184	90,591	87,558	67,499	103,801	84,542	81,835	63,095
Hires	-0.211*	-0.175	-0.219**	-0.175*	-0.241*	-0.291*	-0.264*	-0.310**
	(0.112)	(0.124)	(0.095)	(0.104)	(0.144)	(0.157)	(0.137)	(0.147)
	106,409	86,725	83,678	64,510	97,701	79,564	77,037	59,395
Separations	-0.246*	-0.223*	-0.233**	-0.198**	-0.225	-0.261*	-0.225*	-0.250*
	(0.131)	(0.131)	(0.099)	(0.093)	(0.140)	(0.152)	(0.129)	(0.136)
	98,843	80,195	77,578	59,416	96,206	78,025	75,538	57,861
Turnover Rate	-0.179**	-0.160**	-0.204***	-0.181**	-0.237**	-0.243***	-0.212**	-0.211**
	(0.077)	(0.075)	(0.074)	(0.071)	(0.094)	(0.091)	(0.091)	(0.083)
	98,323	79,782	77,123	59,068	94,358	76,524	74,079	56,740
Samples:								
Extended Sample (1990-2011)	Y	Y			Y	Y		
Primary Sample (2000-2011)			Y	Y			Y	Y
Exclude Recessions		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 primary sample (2000-2011) as well as the extended sample (1990-2011), and for each additionally leaving out recessions. Recessionary quarters are as defined by *NBER*: 1990q3-1991q1, 2001q1-2001q4, 2007q4-2009q2. 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	0.361	0.206***	0.023
		(0.073)	(0.015)
		79,766	76,731
Female	0.647	0.224***	-0.018
		(0.047)	(0.017)
		84,792	81,835
Teen	0.217	0.370***	-0.026
		(0.068)	(0.018)
		77,084	73,909
Young Adult	0.156	0.319***	0.012
		(0.064)	(0.009)
		73,181	67,414
Adult 25+	0.632	0.135***	0.007
		(0.042)	(0.018)
		63,576	69,579

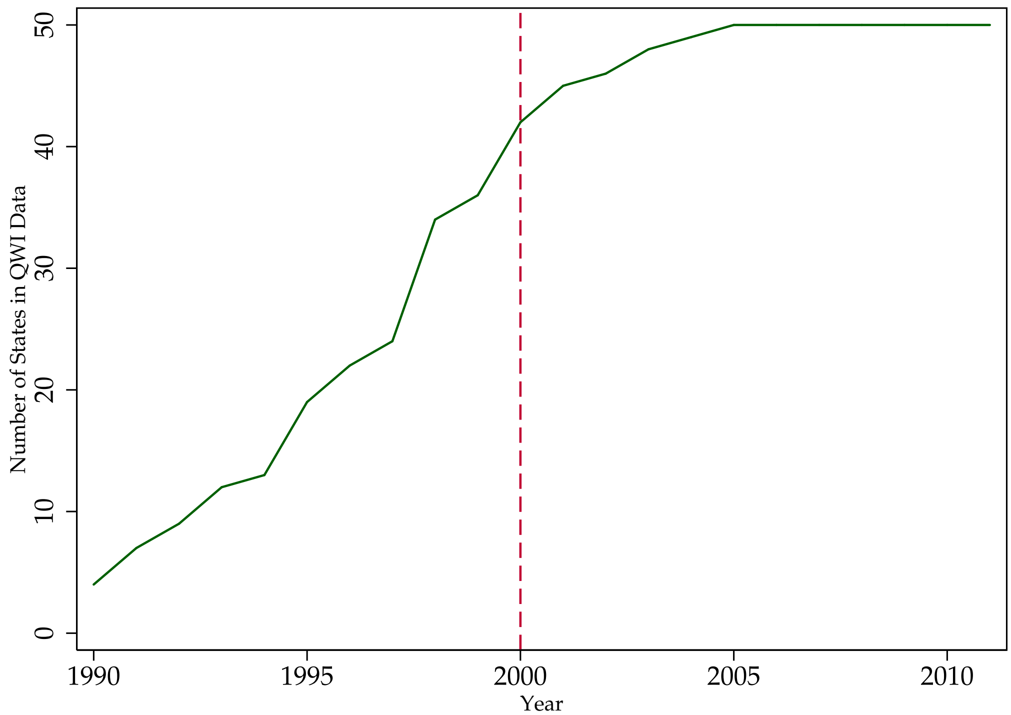
Notes. Column 1 reports the employment share of each demographic group in the overall restaurant workforce. Columns 2 and 3 report the regression coefficient associated with log of the minimum wage. In column 2, the outcome is log of average earnings; the coefficient is therefore the minimum wage elasticity 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.

Table 8
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***
	(0.043)	(0.051)	(0.063)	(0.052)
	79,177	73,352	73,958	73,593
Non-employment duration	-0.011	-0.000	-0.026	0.022
	(0.033)	(0.051)	(0.039)	(0.053)
	85,846	79,396	81,054	75,398

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 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
Timing of Minimum Wage Changes by State Border Pair

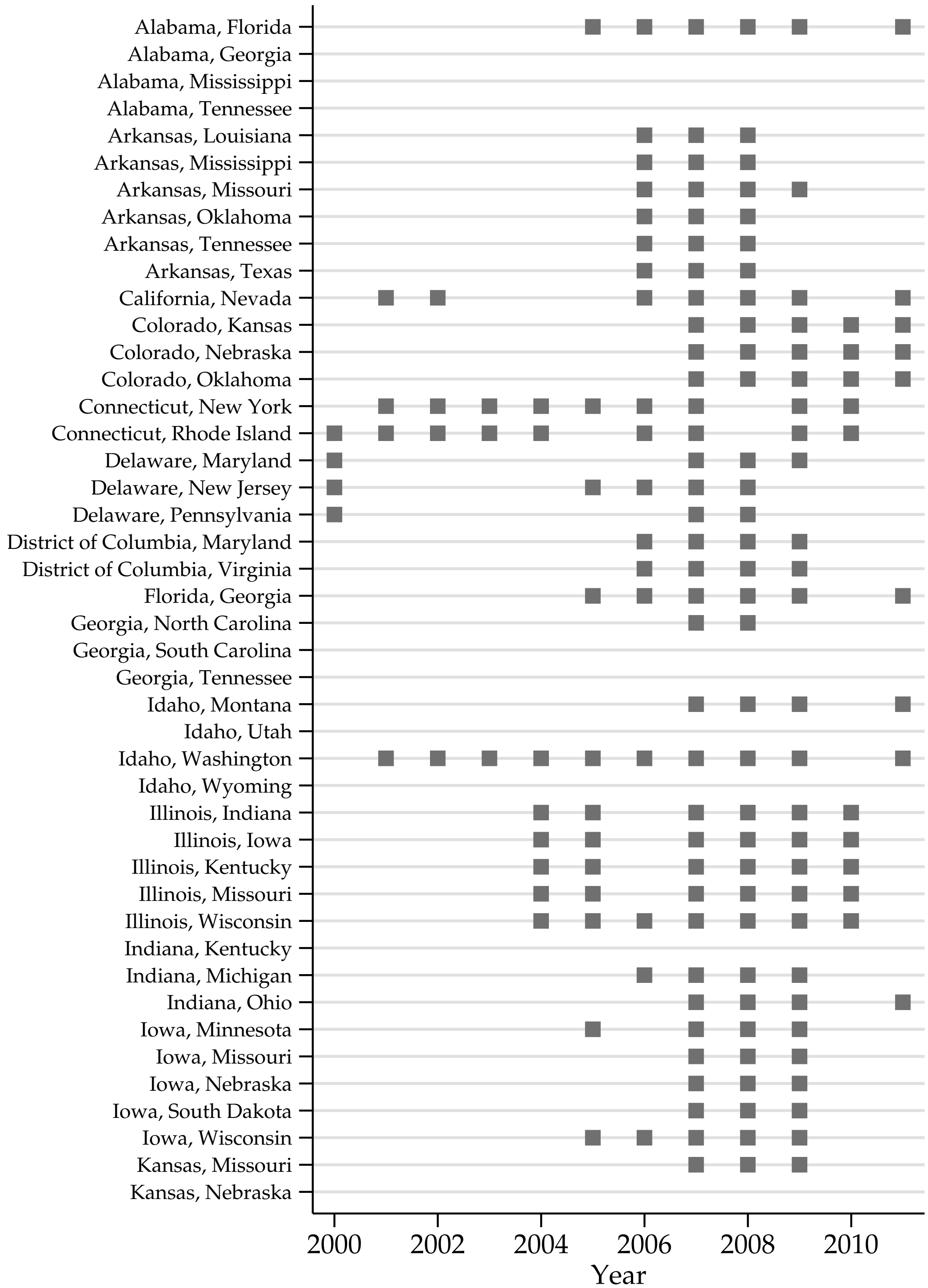
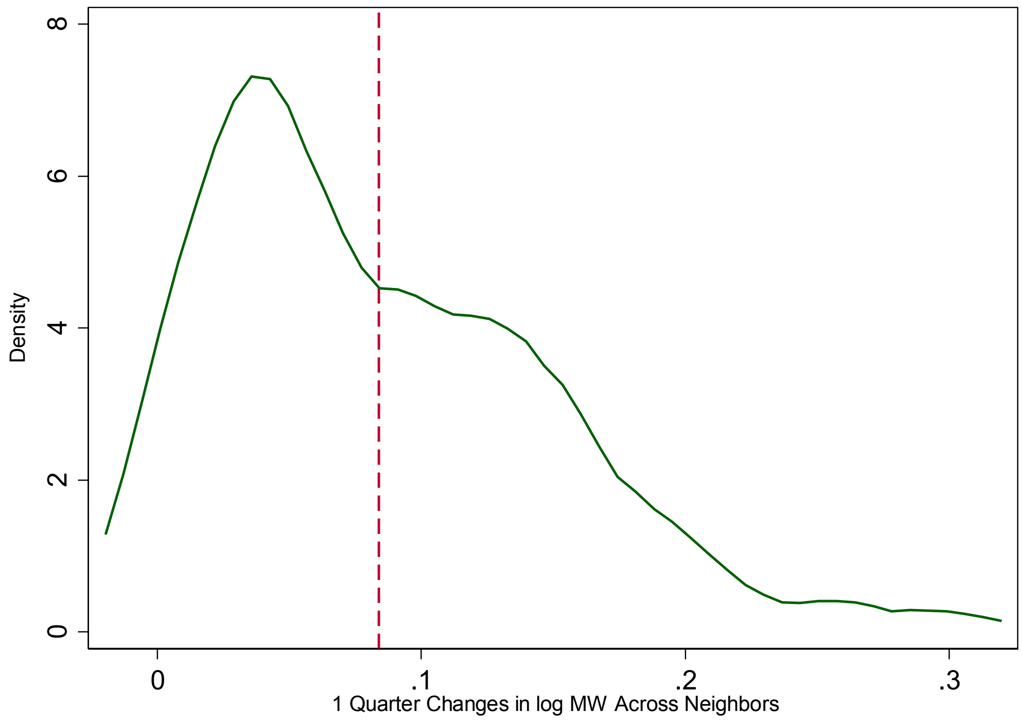


Figure 2 (continued)
Timing of Minimum Wage Changes by State Border Pair

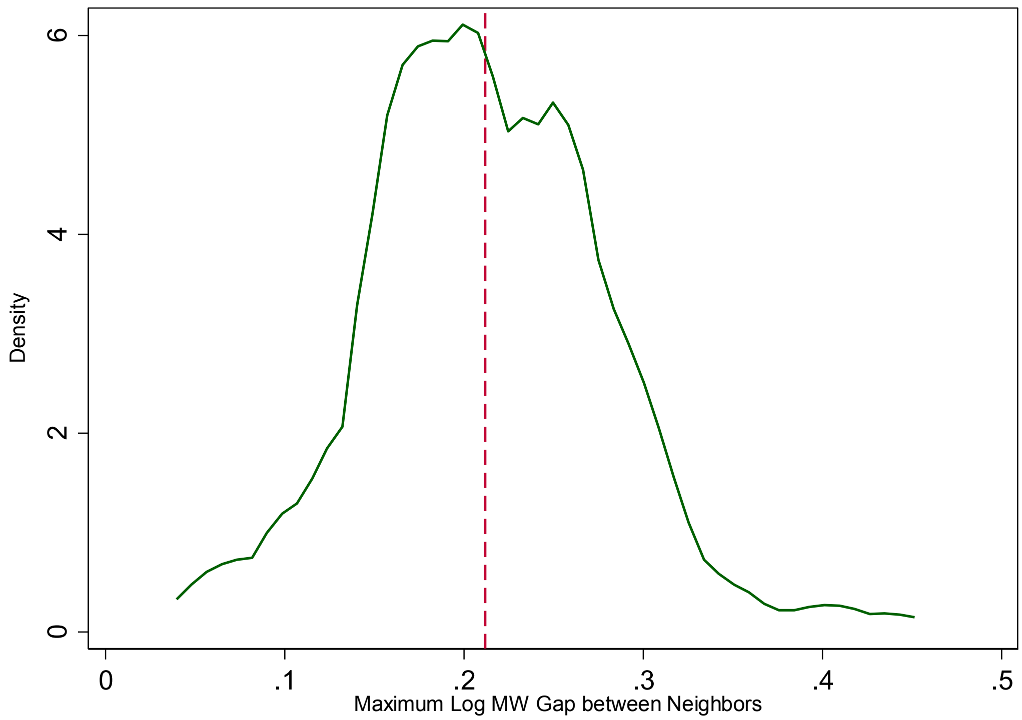
Notes: The table reports all the 88 policy-border-pairs in our primary estimation sample that have minimum wage variation for the sample of counties in pairs whose centroids are within 75 miles. Cells with minimum wage events are marked in grey. Minimum wage events are defined as periods when there are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

Figure 3
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 4
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 (Not for Publication)

A1. Minimum Wage Effects in a Job-Ladder Model

In this appendix, we derive 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 canonical 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.

In the 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 w^*, assumed to be below a binding minimum wage $\underline{w}$; employers are assumed to only offer wages above the reservation wage. 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. Without loss of generality, here we do not specify the determinants of the wage offer distribution $F(w)$ or the accepted wage distribution $G(w)$, as our key results on employment stocks and flows do not depend on features of the wage distributions once cross sectional flows are accounted for. The reason for this feature of the job-ladder model is anticipated in Hornstein et al. (2011), who show the tight link between cross-sectional flows and frictional wage inequality in the job-ladder model. We discuss this point at greater length below.

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

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

It is also well known (e.g., Nagypal 2005, Hornstein et al .2011), that in steady state, balance of flows in and out of unemployment imply the following relationship between the wage offer and realized wage distribution: $G(w) = \frac{\sigma F(w)}{\sigma + \lambda_e[1-F(w)]}$. This relationship implies that the total separation rate can be written as:

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

$$= \frac{\sigma\left(1 + \frac{\lambda_e}{\sigma}\right)\ln\left(1 + \frac{\lambda_e}{\sigma}\right)}{\frac{\lambda_e}{\sigma}} = \frac{\sigma\left(1 + \phi\frac{\lambda}{\sigma}\right)\ln\left(1 + \phi\frac{\lambda}{\sigma}\right)}{\phi \cdot \frac{\lambda}{\sigma}} \tag{6}$$

where the mean separation rate equals the job-to-job transition rate (EE) plus the exogenous job destruction rate (σ), and the derivation uses integration by parts.[1] Equation (6) 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.

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

Equation (6) 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.

Equilibrium contact rate

Since the job destruction rate σ is exogenous in the canonical job ladder model, the sole endogenous variable that determines employment stocks and flows is the contact rate λ. The equilibrium contact rate λ^* is determined by the relative measures of searching employers versus searching workers. In the literature, there are alternative ways of specifying how the the interaction of firm and worker search, as well as the entry and exist of firms, determines the equilibrium contact rates. We will discuss two specific examples from the literature for the purpose of exposition. However, as we note in the next section, our key results on the *relative* magnitudes of the employment stock and flow elasticities do not hinge on the details of this process.

In the wage posting models of Bontemps, Robin and van den Berg (1999, 2000), firms are heterogeneous in their potential productivity, p, and only those firms with productivity exceeding the minimum wage (i.e., $p > \underline{w}$) are active. The equilibrium contact rate depends on the measure of active firms. Specifically, $\lambda^* = \lambda_0(1 - \Gamma(\underline{w}))$ where Γ is the CDF of the productivity of potentially active firms (p), and here λ_0 is a constant. A higher minimum wage reduces the measure of active firms, since only firms with productivity $p > \underline{w}$ have positive profits from production. The extent to which there is a reduction λ^* from a rise in $\underline{w}$ depends on the shape of the firm productivity distribution $\Gamma(p)$: the elasticity with respect to the minimum wage is $\frac{d\lambda^*}{d\underline{w}}\frac{\underline{w}}{\lambda^*} = -\frac{\Gamma'(\underline{w})}{1-\Gamma(\underline{w})}$.

An alternative formulation explicitly specifies a matching function, with the measure of matches depending on the measure of workers who are searching and the measure of posted vacancies, $M = m(\tilde{u}, v)$, where $\tilde{u}$ is the measure of unemployed and employed searchers in efficiency units, and v is the measure of vacancies. Following Petrongolo and Pissarides (2001) and Nagypal (2005), we can assume that that employed and unemployed searchers are perfect substitutes, albeit with different efficiencies, with the total measure of searching workers $\tilde{u} = u + \phi e = 1 - (1+\phi)e$, and hence $M = m\left(1 - (1+\phi)\, e, v\right)$. The contact rate to the unemployed is then:

$$\lambda = \frac{m(u + \phi e, v)}{u + \phi e} = m(1, \frac{v}{1 - (1+\phi)e}) = m(1, \theta) \tag{7}$$

.

Here θ is the tightness of the labor market—and the equilibrium θ^* pins down the offer arrival rates, λ^* and λ_e^*, as well as the employment rate e^* and the separation rate $E(s^*)$. Free entry in vacancies drive the expected profits from a posted vacancy, and determines the equilibrium labor market tightness, θ^*. Mortensen (2000) takes this approach, embedding the Burdett-Mortensen model with wage posting within a matching framework and free entry.[2] Instead of wage posting, Pissarides (2000), Nagypal (2007) and Flinn and Mabli (2009)[3] assume Nash bargaining for sharing the surplus following Mortensen and Pissarides (1994). We do not explicitly model the asset value from a vacant job to characterize θ^*. As we show below, our key results on the *relative* magnitudes of the employment and separations elasticities do not hinge on the wage-setting process, costs of vacancy creation, or the distribution of firm productivities. These results

[2]He considers the simplified setting where the efficiency of on and off the job search is the same, i.e., $\phi = 1$.
[3]Here we are referring to the case without renegotiation as analyzed by Flinn and Mabli.

hold for any model satisfying the job ladder features of (1) on the job search, and (2) workers accepting the higher wage offer.[4] Of course, the details are critical for determining the magnitudes of absolute levels of the employment and separations elasticities, as well the distribution of wages. However, we point out below that in all of these cases, the effect of a policy like the minimum wage on employment stocks and flows is through its effect on labor market tightness, θ^*. This result is a consequence of the flow balance restrictions that underlie the employment and separation rates in equations (5) and (6).

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

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^*.

$$\frac{d\ln e}{d\ln \underline{w}} = \frac{d\ln \lambda^*}{d\ln \underline{w}} \cdot \left(\frac{1}{\left(\frac{\lambda}{\sigma}^* + 1\right)} \right) = \frac{d\ln \lambda^*}{d\ln \underline{w}} \cdot u^* \tag{8}$$

The employment elasticity equals the product of the equilibrium unemployment rate u^* and the elasticity of the contact rate with respect to minimum wage. As discussed in the previous section, this could reflect the productivity distribution of potentially active firms (e.g., Bontemps et al. 2000), or be determined by the measure of vacancies from the free entry condition (e.g., Mortensen 2000).

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

$$\frac{d\ln E(s)}{d\ln \underline{w}} = \frac{d\ln \lambda^*}{d\ln \underline{w}} \left[\frac{1}{1+\phi\frac{\lambda^*}{\sigma}} + \frac{1}{\left(1+\phi\frac{\lambda^*}{\sigma}\right)\ln\left(1+\phi\frac{\lambda^*}{\sigma}\right)} - \frac{1}{\phi\frac{\lambda^*}{\sigma}} \right]$$

$$= \frac{d\ln \lambda^*}{d\ln \underline{w}} \cdot \left[\frac{\phi\lambda^*}{(\sigma+\phi\lambda^*)(\ln(1+\phi\frac{\lambda^*}{\sigma})} - \frac{1}{1+\phi\frac{\lambda^*}{\sigma}} \right] \tag{9}$$

Note the presence of the offer arrival elasticity $\frac{d\ln\lambda^*}{d\ln\underline{w}}$ in both equations (6) and (7). 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 $\frac{d\ln\lambda^*}{d\ln\underline{w}}$:

$$\frac{d\ln e/d\ln \underline{w}}{d\ln E(s)/d\ln \underline{w}} = \frac{\frac{1}{1+\frac{\lambda^*}{\sigma}}}{\frac{\phi\frac{\lambda^*}{\sigma}}{\left(1+\phi\frac{\lambda^*}{\sigma}\right)\ln(1+\phi\frac{\lambda^*}{\sigma})} - \frac{1}{1+\phi\frac{\lambda^*}{\sigma}}} \tag{10}$$

[4] These assumptions do rule out cases with bidding wars between employers, as in Cahuc, Postel-Vinay and Robin (2006).

$$= \frac{u^*}{\left(\frac{\sigma}{E(s^*)} - \frac{\sigma}{\sigma+\phi\lambda^*}\right)} = \frac{u^*}{\left(\frac{\phi\lambda^*}{\sigma+\phi\lambda^*} - \frac{E(s^*)-\sigma}{E(s^*)}\right)} \tag{11}$$

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. Moreover, this result applies to various job ladder models we discussed—e.g., Bontemps et al. (1999, 2000), Mortensen (2000)—regardless of the specific mechanisms for entry or matching.

Prior to that, it is useful to glean more intuition behind the result by further examining equation (11). The numerator in equation (11) 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.[5] 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).

Calibrating the Job-Ladder Model

Equation (9) also allows us to answer the following question: if we calibrate $\frac{\lambda}{\sigma}$ and ϕ using cross sectional 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})

$$r_{EE} = \frac{EE}{EU} = \frac{EE}{\sigma} = \frac{\left(1+\phi\frac{\lambda}{\sigma}\right)\ln\left(1+\phi\frac{\lambda}{\sigma}\right)}{\phi\cdot\frac{\lambda}{\sigma}} - 1 \tag{12}$$

Using 0.9 as the left hand side value (r_{EE}) in equation (12) 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 $\phi = \frac{\frac{\lambda_e}{\sigma}}{\frac{\lambda}{\sigma}} = \frac{3.30}{14.33} = 0.23$.

[5] 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.

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 (12), evaluated at the calibrated parameter values to the empirical one, provides a test of an overidentifying restriction of the model. The steady state flows used to calibrate the relevant model parameters (κ, ϕ) have further testable implications about how those flows respond to an exogenous minimum wage shock.[6]

The first column in Table A2 reports that when we substitute the calibrated values $\phi = 0.23$ and $\kappa = 14.33$ into equation (10), we find:

$$\frac{\hat{e}}{\hat{E(s)}} = 0.22 \tag{13}$$

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 $\left(0.10 = \frac{-0.022}{-0.212}\right)$.[7] 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 A2. 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.[8] 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 A2 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.[9] Using these values in equation (7) suggests a predicted ratio of elasticities:

$$\frac{\hat{e}}{\hat{E(s)}} = 0.45 \tag{14}$$

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:

$$\frac{\hat{e}}{\hat{E(s)}} = 0.25 \tag{15}$$

[6]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.

[7]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 $\hat{S}$.

[8]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 $-\ln(1-\tilde{EE})$.

[9]We also validate our approach by closely replicating the predicted ratio of elasticities using our approach in column 2 of Table A2. 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 HKV calibration.

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.[10] 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.

[10] 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.

A2. Decomposing the Separations Elasticity into EE and EU Components

In this section, we use minimum wage elasticities of employment separations, employment rate, non-employment duration of separations, along with the cross-sectional unemployment rate and the assumption of steady state relationships, to decompose the separation elasticity into job-to-job (EE) and job-to-unemployment (EU) components. These decompositions are consistent with a wide class of models that share the following properties: (1) There are two states: employment and unemployment. (2) There are flows between jobs, as well as between jobs and unemployment. (3) There is a constant hazard rate out of unemployment. (4) Stocks and flows obey restrictions imposed by steady state. These properties include the job-ladder and match quality models discussed in this paper. However, we do not impose any of the additional assumptions of those two models in the calculations below.

We begin with defining various transitions between states: EU is the rate of flow from employment to unemployment (or the job destruction rate), UE is the rate between unemployment to employment (or the job finding rate), and EE is the job-to-job transition rate, with $S = EE + EU$ as the full separation rate.

We denote as u the unemployment rate, and $e = 1 - u$ is the employment rate. The definition of steady state implies:

$$u = \frac{EU}{EU + UE} = \frac{1}{1 + \frac{UE}{EU}}$$

$$\frac{UE}{EU} = \frac{e}{u} \tag{16}$$

For all variables x, we denote as $\hat{x}$ the minimum wage elasticity $\frac{dx}{d\underline{w}}\frac{\underline{w}}{x}$ for convenience. We take logs and differentiate equation (16) with respect to the minimum wage.

$$\hat{UE} - \hat{EU} = \hat{e} - \hat{u} = \hat{e} - \frac{e}{1-e}\frac{1}{e}\hat{e} = \hat{e}\left(1 - \frac{e}{1-e}\right) = \hat{e}\frac{1}{u}$$

$$\hat{UE} = \hat{EU} + \hat{e}\frac{1}{u}$$

$$\hat{e} = u\left(\hat{UE} - \hat{EU}\right) = \frac{EU}{EU + UE}\left(\hat{UE} - \hat{EU}\right)$$

By definition, EE transitions have no intervening unemployment spells. In contrast, EU transitions have an expected duration $D_U = \frac{1}{UE}$. This last equality assumes a constant hazard out of unemployment, ruling out duration dependence or heterogeneity. This further implies:

$$\hat{D}_U = -\hat{UE} = -\hat{EU} - \hat{e}\frac{1}{u}$$

Averaged over both types of separations (EE and EU), the mean duration D can be written as the product of the EU share of separations and the unemployment duration of EU separations: $D = \left(\frac{EU}{S}\right) D_U$. This implies an elasticity:

$$\hat{D} = 2\hat{EU} - \hat{S} - \hat{e}\frac{1}{u}$$

The above relationship allows us to back out the EU elasticity from the elasticity of average non-employment duration of those separating ($\hat{D}$), the overall separations elasticity $\hat{S}$, the employment elasticity $\hat{e}$, and the unemployment rate u :

$$\hat{EU} = \left(\hat{D} + \hat{S} + \hat{e}\frac{1}{u}\right)\frac{1}{2} \tag{17}$$

We also know that the total separations elasticity can be decomposed as follows: $\hat{S} = \frac{EE}{EE+EU}\hat{EE} + \frac{EU}{EE+EU}\hat{EU} = \frac{r_{EE}}{1+r_{EE}}\hat{EE} + \frac{1}{1+r_{EE}}\hat{EU}$, where $r_{EE} = \frac{EE}{EU}$ is the ratio of the two separation rates. If we have an estimate for r_{EE} we can additionally back out the EE elasticity as follows:

$$\hat{EE} = \left(\frac{1+r_{EE}}{r_{EE}}\right)\hat{S} - \left(\frac{1}{r_{EE}}\right)\hat{EU} \tag{18}$$

A3. Choice of Distance Cutoff for Contiguous County Pair Design

In this section, we provide 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 A1 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 A2 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 A3 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. 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. Figure A4 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 Figure A1.

To show that our results are not affected by the choice of cutoffs, Appendix Table A1 reports our key results with cutoffs ranging between 45 and 95 miles.

Table A1
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***
	(0.055)	(0.051)	(0.050)	(0.048)	(0.046)	(0.046)	(0.061)	(0.065)	(0.063)	(0.060)	(0.058)	(0.057)
	44,526	67,162	80,508	86,310	90,849	90,849	43,914	65,832	78,991	84,792	88,771	91,999
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
	(0.117)	(0.097)	(0.087)	(0.086)	(0.084)	(0.084)	(0.090)	(0.107)	(0.097)	(0.093)	(0.101)	(0.103)
	45,138	67,986	81,628	87,558	92,099	92,099	42,864	64,314	76,754	81,835	85,326	87,965
Hires	-0.218**	-0.229**	-0.203**	-0.219**	-0.233**	-0.233**	-0.243**	-0.283*	-0.270*	-0.264*	-0.296**	-0.297**
	(0.107)	(0.101)	(0.093)	(0.095)	(0.095)	(0.095)	(0.124)	(0.151)	(0.140)	(0.137)	(0.145)	(0.143)
	42,812	65,210	78,221	83,678	87,743	87,743	40,803	61,112	72,608	77,037	80,432	82,975
Separations	-0.255**	-0.246**	-0.230**	-0.233**	-0.240**	-0.240**	-0.207*	-0.253*	-0.240*	-0.225*	-0.259*	-0.260*
	(0.118)	(0.109)	(0.101)	(0.099)	(0.095)	(0.095)	(0.123)	(0.139)	(0.129)	(0.129)	(0.138)	(0.139)
	39,706	60,789	72,501	77,578	81,370	81,370	39,717	60,065	71,298	75,538	78,673	81,162
Turnover Rate	-0.211***	-0.210***	-0.207***	-0.204***	-0.203***	-0.203***	-0.193**	-0.212**	-0.214**	-0.212**	-0.212**	-0.219**
	(0.068)	(0.073)	(0.073)	(0.074)	(0.071)	(0.071)	(0.091)	(0.089)	(0.092)	(0.091)	(0.088)	(0.087)
	39,345	60,428	72,140	77,123	80,729	80,729	39,174	58,969	69,839	74,079	77,120	79,609
Maximum distance between centroids	45	55	65	75	85	95	45	55	65	75	85	95
Percent of all pairs	42	63	76	81	86	89	42	63	76	81	86	89

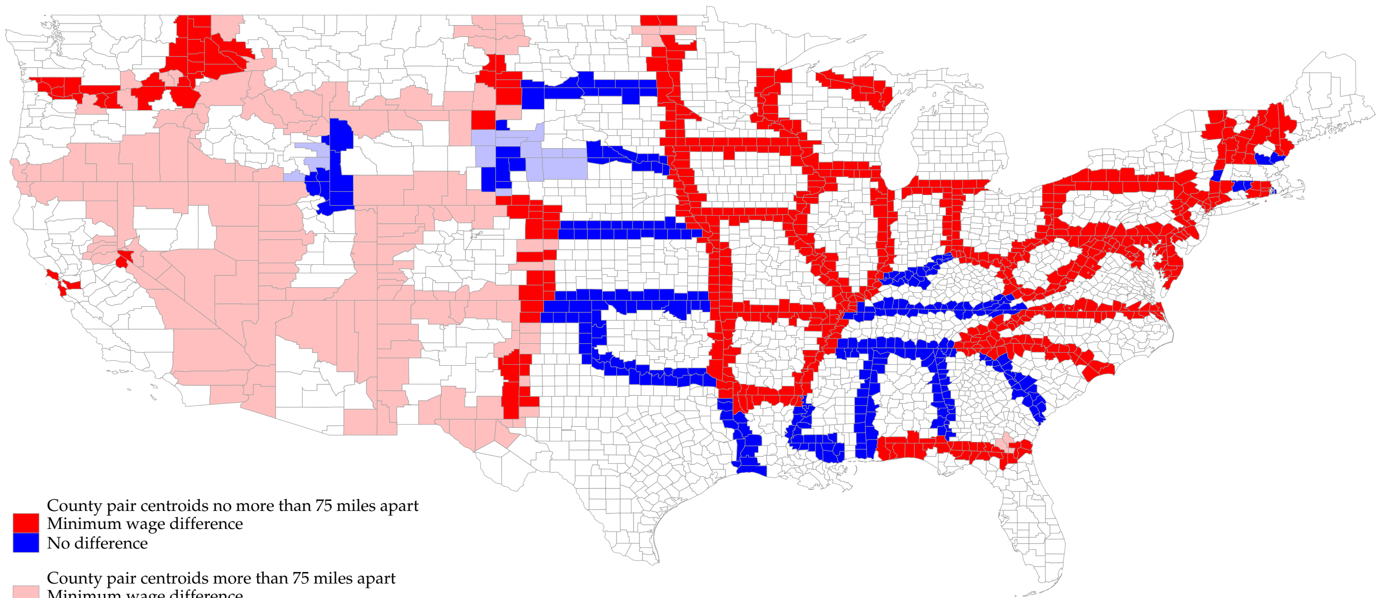
Notes. The table reports estimates for alternative cutoffs in the maximum distance in miles between county centroids within a pair, as reported in the second to last row. The last row reports the fraction retained of the overall border pair sample in the 2000-2011 period when using each cutoff. The reported 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 A2
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.

Figure A1
Map of Contiguous Border Pairs



County pair centroids no more than 75 miles apart
Minimum wage difference
No difference

County pair centroids more than 75 miles apart
Minimum wage difference
No difference

Not in either sample

Figure A2
Distribution of Distances between Centroids in County-pair Sample

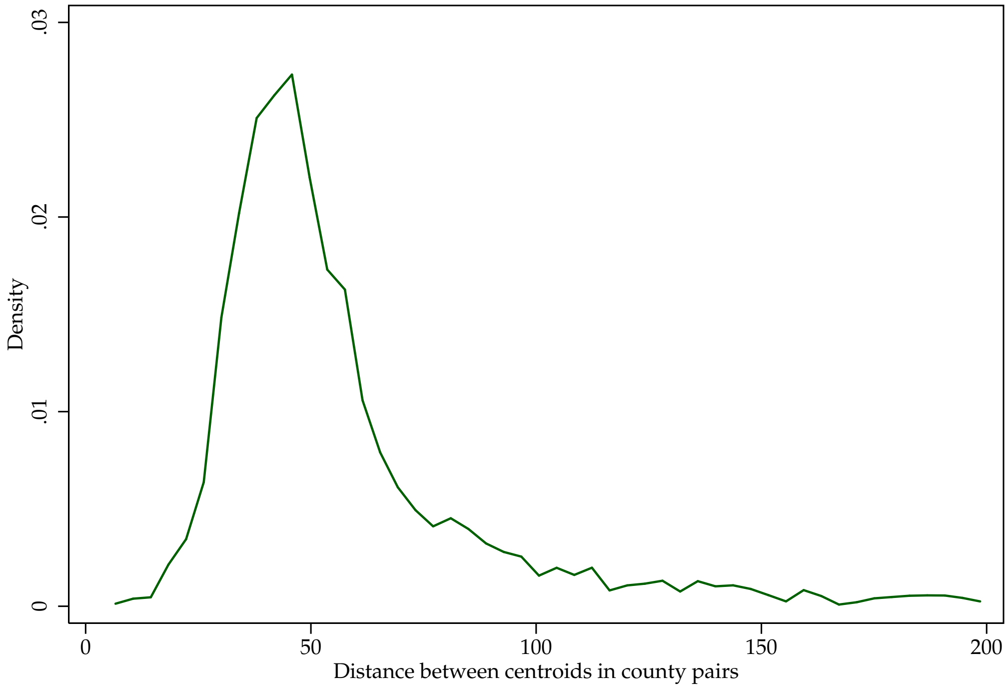
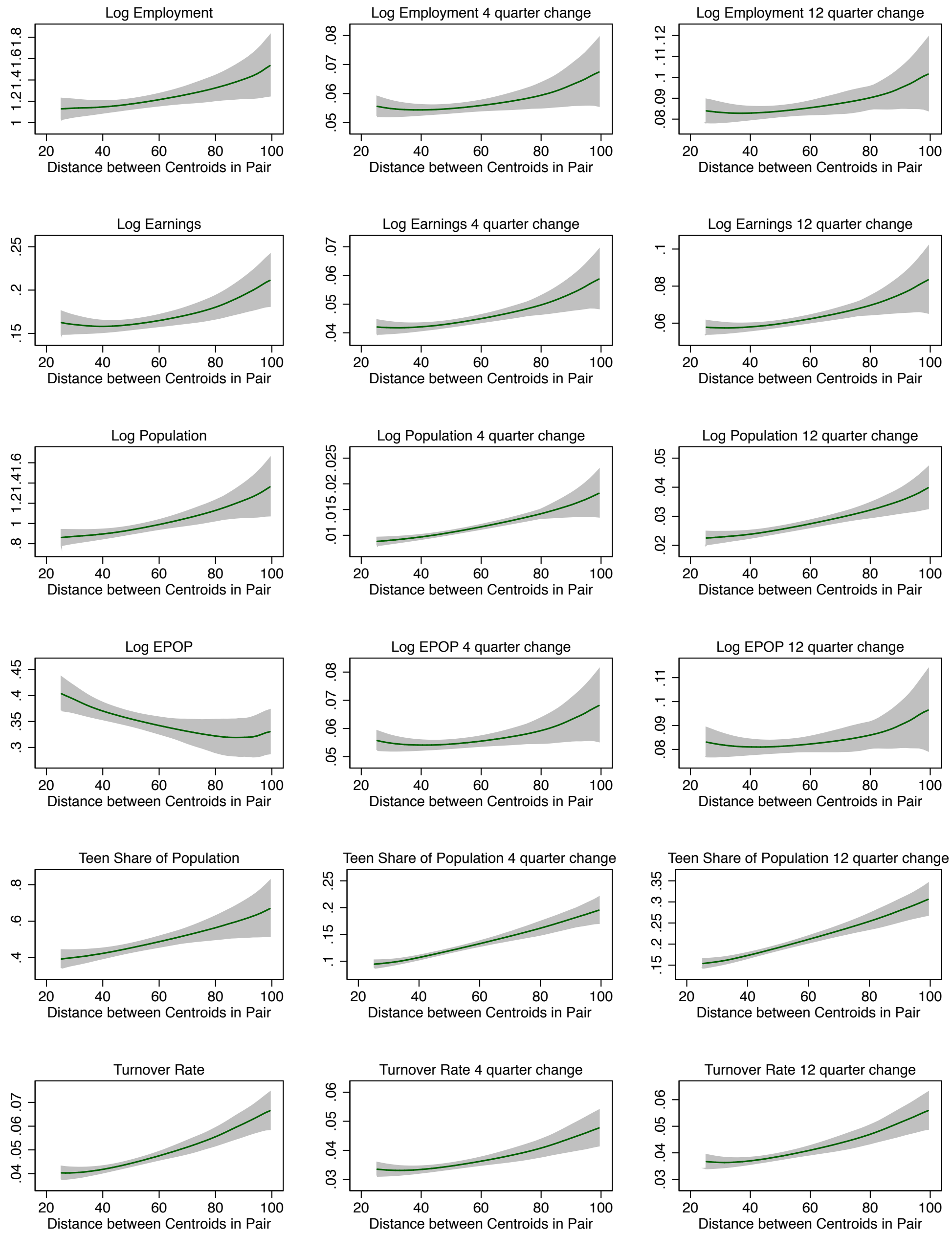
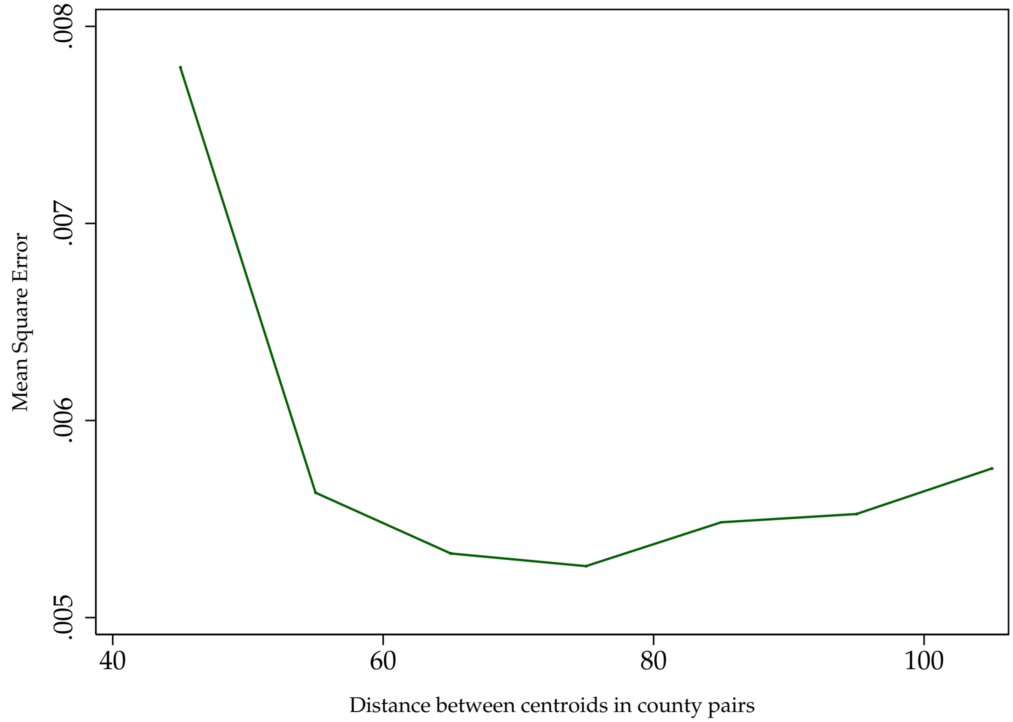


Figure A3
Mean Absolute Difference in Covariates by Distance between Centroids of a Pair



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 A4
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.