Getting Handcuffs on an Octopus: Minimum Wages, Employment, and Turnover*

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Abstract

Theoretical work on minimum wage policy emphasizes labor market dynamics, but the resulting implications for worker mobility remain largely untested. We show that in the teenage labor market minimum wages reduce worker flows and increase job stability. Furthermore, we find that the employment effects of the minimum wage vary considerably across markets with different degrees of labor market tightness. Our results help explain the small effects of minimum wages on employment commonly found in the aggregate data and are consistent with labor market models that involve search frictions.

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> Trying to understand the nature of unemployment is like trying to put handcuffs on an octopus. You think you have it tied down and then another pair of tentacles get you round the throat. (Baily 1982)

Martin Baily's colorful description of an economist-strangling octopus captures the difficulty of modeling the effect of minimum wages on employment. As he argued, a good model of the minimum wage, and its effects on youth employment, should address the ease with which teenage workers move in and out of jobs, and do so in a manner that is consistent with empirical evidence. In the thirty years since, theoretical models of the labor market have evolved to incorporate the dynamics of labor market adjustment and the presence of search and information frictions. In such models, the minimum wage can effect not just employment, but also turnover, the stability of employment, and the movement of employment between shrinking and expanding firms. Yet much of the research in empirical microeconomics on minimum wage policy continues to focus on teasing out implications of the link between minimum wages and the rate of employment. But Baily's argument was that, without understanding how labor market dynamics mediate their relationship, we cannot understand how minimum wages affect employment. It is as if we are fighting Baily's octopus without any clear knowledge of what it looks like.

In this paper, we examine the relationship between U.S. minimum wage policy, labor market flows, and employment in the teenage labor market using novel data from the Quarterly Workforce Indicators (QWI). The QWI provide quarterly measures of earnings, employment, hiring, separations, job creations, and job destructions, employment stability, and information on the speed at which workers move in and out of nonemployment. These data can be disaggregated by age, by sector, and by county, allowing us to use standard panel data methods to identify the effect of the minimum wage on teenage employment across states, both in aggregate, and across industries. We show that increases in the minimum wage are associated with decreased worker flows and increased employment stability, but not with employment or job reallocation across employers. Since the impact is largely to reduce the flow of workers into and out of jobs as well as increase the time workers spend in a match, minimum wages have a chilling effect on labor market volatility.

We then consider what this chilling effect means for the health of the labor market. There are two roles worker reallocation might play in mediating the effects of the minimum wage on employment. On one hand, the speed with which workers move from job-to-job can be a barometer of labor market competition, and hence labor market health. Lazear and Spletzer (2012) argue that reduced turnover can have substantial costs by preventing workers from moving to more productive matches. On the other hand, turnover may also be costly for firms, given the expense of recruiting and training workers. In this case, the chilling effect of a higher minimum wage could help firms, who offset the increased unit labor cost with lower recruiting costs, not unlike an efficiency wage. Hirsch et al. (2015) finds, for instance, that managers of fast food restaurants report, when surveyed, that they absorb the cost of minimum wage increases through channels other than employment.

Bringing this question to the data, we document striking relationships between the effects of minimum wage increases on employment, and the level of labor market "tightness". We implement two measures that proxy for labor market tightness - the worker reallocation rate (turnover) and the duration of nonemployment experienced by workers who separate. Evaluating these measures individually, in low turnover markets minimum wage increases have a negative effect on employment, while in high turnover markets, there is a statistically significant employment increase in response to the minimum wage. Similarly, markets where workers experience short durations of nonemployment also have negative employment effects of minimum wage increases, but we observe positive employment effects in markets where the duration of nonemployment for separated workers is long. When we classify markets two dimensionally based on both the level of turnover and the duration of nonemployment, the channels through which we observe positive and negative employment effects become more clear. Both turnover and the duration of nonemployment play mediating roles individually, but the employment effects of the minimum wage are associated with the way in which turnover and duration of nonemployment interact within a market as well.

In the next section, we discuss the literature to which this paper is most closely related, focusing on other recent work examining the link between minimum wage policy and labor market dynamics in the U.S. We then provide, in Section 2, a detailed discussion of the QWI, which are a relatively unknown and under-utilized source of data, along with the other data used in our analysis. Section 3 presents our empirical strategy, focusing on how we address the many well-known concerns about measurement and identification in minimum wage studies. Section 4 reports our baseline results, characterizing the effect of minimum wage changes on various labor market flow measures, job reallocation, and employment stability. Section 5 develops our motivation and methodology for investigating minimum wage effects across markets with differing levels of labor markets tightness and also presents our main results.

1 Related Literature

Drawing in data on market outcomes beyond employment has been an effective strategy for evaluating models of minimum wage policy. Aaronson et al. (2008) show that the relationship between minimum wage increases and restaurant prices is consistent with a competitive labor market, and not consistent with a model in which employers hold substantial monopsony power. Neumark et al. (2004) and Neumark et al. (2005) find that the effects of minimum wages on income and poverty are also consistent with the predictions of neoclassical theory. Our findings similarly help to distinguish between competing models of market frictions.

We find that aggregate estimates showing small employment effects of the minimum wage mask stronger effects across markets when disaggregated by turnover and average nonemployment durations. These findings are related to other work regarding the importance of treatment effect heterogeneity in minimum wage analysis. Neumark and Wascher (2002) and Yuen (2003) document variation in the extent to which the minimum wage binds for teenage workers. Singell and Terborg (2007) and Addison et al. (2009) provide evidence of heterogeneity in the minimum wage effects across industries. Our empirical results provide a theoretically sound and way to represent heterogeneity in the effect of the minimum wage that complements previous work.

Other papers have directly examined how minimum wage increases affect worker flows, but ours is the first to directly consider the role of turnover in mediating the employment effects of the minimum wage. The basic finding that minimum wages reduce worker flows is becoming a stylized fact. Portugal and Cardoso (2006) were the first to provide direct evidence on this topic. They use Portuguese employer-employee matched data to show that a large increase in the minimum wage for young workers decreased the rates at which they separated from, and were hired to, firms. Similarly, Brochu and Green (2013) exploit variation in minimum wages across Canadian provinces, finding that higher minimum wages lead to reductions in separations and hiring. They are also able to distinguish between quits and layoffs and argue that the reduction in separations is largely due to a reduction in layoffs.

In the U.S. context, Thompson (2009) uses QWI data to study the effects of the minimum wage on the employment of teenagers. As part of his analysis, Thompson (2009) shows that minimum wages also decrease the share of teenagers in new hires. Our paper is closely related to recent papers that use QWI data to study the effects of minimum wages on rates of hiring and separation Dube et al. (forthcoming) and on job flows (Meer and West 2013). Like those papers, we show that minimum wages reduce the flow of workers into and out of jobs, and seek to interpret this finding in the context of models of labor market frictions. In addition, our paper focuses on documenting heterogeneity in the response of employment to changes in the minimum wage across markets with different levels of labor market tightness. We are the first to directly demonstrate that the employment effects of the minimum wage are strongly correlated with turnover and labor market tightness. Our results confirm key predictions of frictional search models of the labor market and suggest that these measures may be very useful summaries of the heterogeneity across markets.

A secondary contribution relative to this quickly developing literature is that we estimate the effect of minimum wages on teenage employment, worker flows, and job flows using a state panel data design. This is relevant for two reasons. First, no paper has studied all three outcomes using a unified empirical framework. The state panel data framework is common to previous work studying the effects of minimum wages on teenage employment Neumark and Wascher (1992); Burkhauser et al. (2000), and our design incorporates the many forms of spatial heterogeneity found to be important. Second, our research design complements Dube et al. (forthcoming), who use county border pairs for identification, as in Dube et al. (2010). Since our empirical findings regarding basic outcomes such as employment and turnover are broadly consistent, the identification strategies and results in the two papers using different sources of variation should be viewed as complementary.

Finally our work is also relevant to the literature on comparative labor market institutions. Relatively high unemployment in some European countries is associated with muted levels of worker and job reallocation. To explain this 'Eurosclerosis', a number of papers develop frictional matching models in the spirit of Mortensen and Pissarides (1994) to analyze the effects of labor market regulations, including minimum wage laws, on levels of employment and labor market flows (Blanchard and Portugal 2001; Pries and Rogerson 2005; Gorry 2013). In these models, firms create vacancies and hire from a pool of unemployed workers. Labor market policies, including minimum wages, affect the expected profit from a vacancy, but also the value of remaining in a match for the worker. Evaluating these models in a cross-country setting is compromised by the difficulty of holding unobservable institutional features fixed. We avoid this criticism by exploiting variation across U.S. states in minimum wage policy. Our analysis is therefore similar in spirit to the work of Autor et al. (2006) and Kugler and Pica (2008) on employment protection legislation. While far from conclusive, our findings suggest that further efforts to estimate frictional labor market models using dynamic panel data on labor market flows, in the spirit Kiyotaki and Lagos (2007), may be a productive way forward.

2 Data

We combine data from three sources to study the effect of minimum wages on employment and labor market flows. From the Quarterly Workforce Indicators (QWI), we obtain quarterly measures of teenage worker and job reallocation. We also construct our own panel of state-level minimum wage laws that include information on the month in which each minimum wage became effective. Finally, we use state-level aggregates from the Current Population Survey Outgoing Rotation Groups (CPS-ORG) to generate control variables at the state level and to conduct analyses establishing that the minimum wage is binding for teenage workers.

2.1 The Quarterly Workforce Indicators (QWI)

Here, we draw the reader's attention to strengths of the QWI data as well as potential problems that may affect our analysis. For our purposes, the data have two primary advantages. First, the QWI measure many variables characterizing labor market dynamics; most importantly on the movements of workers into and out of firms. Second, the data can be disaggregated by detailed geography, industry, and by demographic characteristics, specifically age. The measurement of worker flows and the ability to cut the data by both age and industry allow us to study the interaction between minimum wages and employment across markets characterized by different levels of labor market tightness. These features distinguish the QWI from the Quarterly Census of Employment and Wages (QCEW), which are also based on administrative UI data, and which have been used in several recent studies of the effects of minimum wage laws on low-wage industries (Addison et al. 2009; Dube et al. 2010).

2.1.1 Background on QWI Data

The QWI data are a public-use product of the Longitudinal Employer Household Dynamics (LEHD) program at the U.S. Census Bureau. The microdata underlying the QWI, the LEHD Infrastructure files, are employer-employee matched data constructed from state Unemployment Insurance (UI) records. LEHD aggregates these records in partnership with state Departments of Labor that administer UI systems. The basic data element is a 'job', defined by the appearance of a UI record that reports the UI-taxable earnings paid to a specific individual by a particular employer. The LEHD program integrates the UI-based job frame with survey and administrative data that contain information on the individuals and employers involved. The microdata created by the program are then compiled into public-use products, of which the QWI are one. Because they are built from UI records, the LEHD data cover roughly 98 percent of all private-sector, non-agricultural employment, facilitating the release of statistics at high levels of demographic, geographic, and industry detail.[1]

The QWI data used in this paper cover 49 states for the period 1990:Q1–2010:Q4.[2] Our initial analysis uses data reported at the state level for two different age groups: teenagers, which QWI reports as 14-18 year olds, and adult workers, age 25-54. We go on to further disaggregate the data by NAICS 3-digit industry. We use the following QWI variables:

- B: employment at the start of the quarter.
- E: employment at the end of the quarter.
- A: workers newly hired this quarter (accessions).
- S: workers newly separated this quarter (separations).
- C: total jobs created across all firms this quarter.
- D: total jobs destroyed across all firms this quarter.
- Y: total earnings of workers employed at the end of the quarter.
- SH: workers hired into 'stable' jobs this quarter.
- SS: workers separated from 'stable' jobs this quarter.[3]
- NH: average number of periods of nonemployment for new hires.
- NS: average number of periods of nonemployment for separated workers.[4]

[1] For a comprehensive overview of the LEHD data and production of the QWI, see Abowd et al. (2009).

[2] We used the R2012Q1 version of the public use Quarterly Workforce Indicators. The raw data are available for download from `http://www.vrdc.cornell.edu/news/data/qwi-public-use-data/`.

[3] Stable jobs in the QWI are those that last at least one full quarter. Formally, a 'full quarter' job is one such that the worker is employed in that quarter, the quarter before, and the quarter after. The inference is that the job in question was in progress throughout the entire reference quarter.

[4] The variable NH measures periods of nonemployment for new hires as the average number of quarters of nonemployment experienced prior to being hired: `average`(max[non-employed quarters of new hire, 4]). Periods of nonemployment for separations represent the average number of quarters of nonemployment post-separation: `average`(max[non-employed quarters of separated worker, 4]).

In the process of aggregating the raw job-level records to geography-industry-demographic group cells, the LEHD program imposes two data quality controls that may affect analysis. First, the following accounting identities are imposed on the release statistics:

- JF: net job flows, with $JF = E - B = A - S = C - D$.
- WR: worker reallocation, $WR = A + S$.
- JR: job reallocation, $JR = C + D$.
- $B_t = E_{t-1}$.

These accounting identities do not directly affect our analysis, but they restrict the number of independent sources of variation. Second, the QWI data are prepared using a novel confidentiality protection procedure of noise infusion so that small cells can be published rather than suppressed in the released statistics.

2.1.2 Historical Availability

As illustrated in Figure 1, the historical availability of QWI data varies by state. Entry dates are determined primarily by the adoption of computerized record-keeping. The earliest states appear in 1990 (MD, IL, WA, and WI). By 2000 the panel is almost balanced. Since our identification strategy relies on cross-sectional variation in state minimum wage laws, the entry pattern has some potential to affect our analysis. Our benchmark model uses all of the available data. In studying bindingness of the minimum wage, we find that the manner in which states enter QWI does not affect our results. For robustness, we also report estimates restricted to the post-2000 sample where the sample is nearly balanced.

2.1.3 Choice of Dependent Variables

When working with linear models, the accounting identities imposed in creating the QWI mean there are really only three independent sources of variation when measuring employment flows. Given the level of job flows JF, and worker reallocation, WR, we have

$$A = \frac{WR + JF}{2} \tag{1}$$

$$S = \frac{WR - JF}{2}. \tag{2}$$

In addition, given job reallocation, JR,

$$\begin{aligned} C &= \frac{JR + JF}{2} && (3) \\ D &= \frac{JR - JF}{2}. && (4) \end{aligned}$$

Therefore we face a modeling choice of whether to directly model worker reallocation (turnover), job reallocation, and job flows, treating accessions, separations, creations and destructions as derived, or vice-versa. The related literature models accessions and separations directly, along with creations and destructions. We focus on turnover and job reallocation, since most of the movements in hires and separations are highly correlated. For comparability with existing research, we also report estimates using accessions, separations, job creation and job destruction as dependent variables with our robustness checks (Table 5).

Note that A and S are very highly correlated and have nearly identical level- and time series variation within states. Figures 2a and 2c plot the least-squares residuals in worker reallocation and employment from two representative states, California and Illinois, after removing state and period effects. Figures 2b and 2d plot the least-squares residuals in hires and and separations.[5] The series for hires and separations track each other exceptionally closely, which reflects that most hiring activity is driven by the need to replace workers who turn over. These observations support our primary focus on turnover and job reallocation.

2.1.4 Noise Infusion and Cell Suppression

Like the Business Dynamics Statistics (BDS) and the Quarterly Census of Employment and Wages (QCEW), the QWI data are prepared using confidentiality protection procedures. These procedures prevent the disclosure of information when the number of firms or individuals contributing to a statistic is small. The QWI use a novel noise infusion method that maintains the analytic validity of the released statistics but reduces the incidence of individual cell suppression and eliminates the need for complementary suppression (Abowd et al. 2012). Briefly, the procedure works by adding a small, but non-zero, amount of noise to the underlying microdata prior to aggregation. The noise makes it impossible to "reverse engineer" the actual cell counts from the release data, even when the number of units contributing data is small.

All cells and all QWI statistics are distorted through this process, but the upshot is that the QWI data are therefore subject to very little cell suppression relative to other

[5]We show plots from just two states for brevity. Plots of the data from all states are available from the authors upon request.

protection schemes and produces a more representative sample in the published statistics. The QWI includes a flag when cells are "significantly distorted" – that is, when the published value deviates from the confidential value by a specific (confidential) threshold. To ease the reader's mind about potential measurement issues, we drop the flagged "significantly distorted" cells as a robustness check.[6] Due to the high level of aggregation, our state-level data have no cells flagged as "significantly" distorted. In our state-industry data, about 4 percent of employment cells are suppressed and 7.8 percent of employment cells are flagged as significantly distorted. This occurs almost exclusively in cells with very low employment, just on the margin of suppression. The median end-of-period employment in state-NAICS 3 cells that are flagged as distorted is 15 workers, the 10th decile is 4 workers and the 90th decile is 279 workers.

2.1.5 Other Concerns

The QWI data also do not currently include reliable or consistent information on public-sector employment or self-employment.[7] We restrict our analysis to jobs where the employer reports being privately owned. Like the QCEW, the QWI contain measures of total labor market earnings, but do not separately report wage rates or hours of work. We use the CPS to measure the teenage and adult wage distributions – teen wages are used in preliminary analyses and adult wages are used to construct a control variable. We have no measure of labor utilization at the intensive margin. Like much of the minimum wage literature, our analysis is restricted, by virtue of the available data, to the extensive margin of employment adjustment. However, for our analyses regarding worker and job reallocation, the extensive margin is exactly what we need to observe.

2.2 Minimum Wage Data

We exploit state-level variation in the level and timing of minimum wage laws for identification. To better measure timing, we have collected new data that identify the month in which each minimum wage law was enacted by states between 1979-2010. We constructed our minimum wage data from the Monthly Labor Review in combination with primary data from state Departments of Labor. The levels in our series are nearly identical to those reported

[6]It should be noted, however, that there is no clarity whether dropping these cells produces better or worse estimates. In fact, since it is clear that these cells are small to begin with, dropping them generates a sample selection issue. Regardless, even when doing so, our results are unchanged.

[7]As of this writing, the LEHD program is in the process of incorporating the self-employed and public sector employment into the QWI, so the limitation to private-sector employment is not permanent

in other published work, in particular (Sabia 2009). Our series is available upon request.

We record 246 increases in the minimum wage between 1990 and 2010. That count includes seven increases in the federal minimum wage, which we represent as separate increases in each state that previously had a minimum wage lower than the new federal minimum. There are, on average, 5 changes in the effective minimum wage for each of the 49 states in the QWI. Among those 246 changes, 87 (35.3 percent) occur in the first quarter of the year, 11 (4.5 percent) in the second quarter, 94 (38.2 percent) in the third quarter, and 54 (22 percent) in the fourth quarter.

Figure 1 shows the state-level variation within and across states of the minimum wage. Note that while our analysis is based on quarterly changes in the minimum wage, the figure is reported annually for ease of presentation. Thirty-four states had minimum wages above the federal level at some point between 1990 and 2010. The black highlighting identifies the pattern of QWI availability. Numeric entries indicate the level of a particular state's minimum wage if it superseded the federal minimum wage at some point during the year. Blank entries indicate the federal minimum wage superseded the state minimum wage for the entire year.[8]

2.3 Current Population Survey

We use the NBER extracts from the Current Population Survey Merged Outgoing Rotation Groups (CPS-ORG) to construct contextual state-level variables used in our main analysis, to analyze the minimum wage effects on the teen wage distribution, and to compare our results across employment measures in the QWI and CPS.[9] We aggregate the household-level data using the CPS sampling weights, into the following state-quarter variables: the employment-to-population ratio of teenagers, the teenage share of the state population aged 16–65, the average wage rate for teenagers (age 16–18), the average wage of adults aged 25–54, and the unemployment rate for men aged 25–54.[10]

2.4 Dependent Variables

We focus on the following variables:

[8] Figure 1 conveys that the timing of entry into QWI through the 1990s misses some of the state-level variation in minimum wages. This observation motivates us to perform robustness checks of our main results by restricting the sample to the more balanced 2000:Q1–2010:Q4 period. The results are largely consistent across the sample periods.

[9] The Merged Outgoing Rotation Group files are available from NBER at http://www.nber.org/morg/annual/ or by request from the authors.

[10] See data appendix for details.

- $EPOP$: end-of-quarter teenage QWI employment to population ratio (E/POP);
- WRR: worker Reallocation Rate ($(A+S)/\overline{EMP}$);
- JRR: job Reallocation Rate ($(C+D)/\overline{EMP}$);
- $\ln Y$: log of average monthly earnings of teen workers employed at the end of the quarter;
- HSF: fraction of hires that are into 'stable' jobs $= SH/A$;
- SSF: fraction of separations that are from 'stable' jobs $= SS/S$,
- NS: average number of periods of nonemployment for separated workers.
- NH: average number of periods of nonemployment for new hires.

where POP is teenage population, and $\overline{EMP} = \frac{B+E}{2}$ is average QWI teen employment over the quarter. Our decision to normalize WRR and JRR (and in later analyses: hiring, separation, creation, and destruction) by employment is for consistency with the literature on labor market flows, but in the state-level models, where a direct comparison is possible, the results are not sensitive to an alternative normalization by the teenage population.

The worker reallocation rate (WRR) measures the flow of workers across employers. The job reallocation rate (JRR) measures the flow of employment (jobs) from contracting to expanding employers. Hence, worker reallocation can be low or high in markets with stable employment, simply through turnover.

2.4.1 Descriptive Statistics

Table 1 reports summaries of the state-level data. The entries are means of state-quarter observations, weighted by the teenage population. The average worker reallocation rate for teenage workers in the sample is 1.19. That is, teenage workers turn over a little more than once per quarter. The average job reallocation rate is 0.38, which means that a little more than a third of teenage jobs are reallocated from employers reducing their teen workforce to those expanding it during a quarter. In the QWI, the average ratio of worker to job reallocations is approximately three. In other words, for every job that is reallocated across firms during the quarter, three workers are reallocated.

The variable "teen fraction of stable hires" measures the fraction of total hires during the quarter that become stable jobs. The reported value of 0.34 means that, on average, a third of jobs that teenagers are hired into are stable, as opposed to temporary, jobs. We also report the corresponding statistic for separations. Slightly less than a third of the jobs that teenage workers separate from were stable jobs.

The QWI also reports the number of quarters of nonemployment experienced in the previous year by the worker before being hired. For all separations, they report the number of quarters of nonemployment experienced by the worker subsequent to leaving employment. Consistent with the strong seasonality of their employment, teenage workers experience on average 2.63 quarters of nonemployment after separating from jobs, and have experienced 1.81 quarters of nonemployment before being hired.

3 Empirical Strategy

In this section, we introduce our main empirical strategy, which is an extension of the state panel data approach used in much of the related literature. Given the current debate over the appropriate empirical method for measuring minimum wage effects, we discuss the assumptions underlying identification and our approach to assessing robustness to violations of those assumptions. Also in this section, we demonstrate that (1) increases in the statutory minimum wage materially affect the wages earned by teenage workers and (2) that minimum wages are binding within and across states and industries for teenagers.

3.1 Empirical Models and Identification

For the state-level analysis, our benchmark specification is:

$$y_{st} = \alpha + \lambda_t + \mu_s + t\eta_s + \omega_{r(s),t} + \chi_{s,q(t)} + \pi^s RECESSION_t + \beta \ln \mathrm{MW}_{st} + X_{st}\Gamma + \varepsilon_{st}. \quad (5)$$

The variable s indexes the state and t indexes the period (quarter). Variable y_{st} can be one of many labor market outcomes, including employment, worker reallocation rate, job reallocation rate, etc. State and period-specific effects are denoted μ_s and λ_t, and $t\eta_s$ is a state-specific linear time trend. We also include $\omega_{r(s)t}$, which allows for the period specific shocks to vary by Census region. The function $r(s)$ maps state s to its Census region. The variable $RECESSION_t$ is equal to 1 in periods of recession, as dated by the NBER, and zero otherwise. The term π^s measures any state-specific effect on the outcome from being in a recessionary period. State-specific seasonal fluctuations in the outcome are included as $\chi_{s,q(t)}$. The function $q(t)$ maps period t to the quarter of the year (I, II, III, IV). We denote the natural logarithm of the effective minimum wage in state s as of the beginning of period t (first month of the quarter) as $\ln \mathrm{MW}_{st}$, and X_{st} is a vector including the log adult wage, the share of teenagers in the adult population, and the unemployment rate of prime-age males. The residual, ν_{st}, accounts for unobservables affecting state-level outcomes in period t.

Heterogeneity by Labor Market Conditions

The QWI data also allow us to study variation in the effect of minimum wage increases on teenage employment across state and industry (3-digit NAICS). A well-developed theoretical literature postulates that the employment effects of the minimum wage may vary greatly depending on how competitive, or "tight", the labor market is. In markets with substantial market frictions, such as in the static monopsony model, it is even possible that increases in the minimum wage could correspond to increased employment.

Motivated by these theoretical observations, our goal is to empirically study variation in the employment effects of the minimum wage across different levels of labor market tightness. The QWI provide two proxy measures that should be correlated with labor market tightness. The first is our measure of turnover – the worker reallocation rate; the second is the number of periods of nonemployment for separating workers. We adopt a semi-parametric approach whereby we assign each state-industry pair to a decile in the state-industry distribution of both proxies separately.

The base specification is the analog of Equation 5 but now the unit of observation is a state-industry:

$$\begin{aligned} y_{kst} = &\alpha + \lambda_t + \mu_{ks} + t\eta_{ks} + \omega_{r(sk)t} + \chi_{ks,q(t)} + \pi^{ks} RECESSION_t \\ &+ \sum_{d=1}^{10} \mathrm{I}_{ks}^d \left(\kappa^d + \beta^d \ln(MW_{st})\right) + X_{st}\gamma + \varepsilon_{kst}, \end{aligned} \tag{6}$$

where k indicates the NAICS 3-digit industry, and I_{ks}^d is an indicator equal to 1 when state-industry pair ks is in decile d and zero otherwise. β^d is the effect of interest, and measures heterogeneity in the responsiveness of markets to the minimum wage with respect to the level of turnover. The remaining effects are the same as they were in Equation 5 but defined at the state-industry level here in this model.[11]

[11] There are 4,269 unique state-NAICS 3-digit industry pairs, and estimating all the effects in Equation 6 at the NAICS 3-digit level results in a very large number of parameters. Therefore, some of the effects will be estimated at the state-sector level to ease the computational burden. Those instances are made explicit in both the text and tables. Also note that in Equation 6, κ^d and λ_t are not identified since we control for state-industry and region-period effects. The notation is preserved for consistency of presentation with

To measure the tightness of individual labor markets (defined at the state-industry level), we compute the within-sample average of both proxies separately – the worker reallocation rate and the duration of nonemployment for separating workers – for each state-NAICS 3-digit industry and pool these measures into deciles for estimation. We adopt several alternative methods that vary the rule for decile assignment, as described in Section 5.2. Each proxy has advantages and potential drawbacks. As we show, both lead us to similar, though not identical conclusions, opening fresh questions about the link between minimum wages, employment, and the competitive structure of teenage labor markets.

Identification

Identification of Equation 5 (and similarly Equation 6) relies on the assumption that changes in the minimum wage are not correlated with residual movements in the outcome. The appropriate model and research design continues to be the subject of much controversy (Neumark et al. 2014; Allegretto et al. 2013). Our benchmark specification is highly saturated, and controls for many different confounding unobservables considered in the recent literature, including state-specific linear time trends (Addison et al. 2009), region-period shocks (Meer and West 2013) and state-specific responses to recession (Allegretto et al. 2011; Neumark et al. 2014). In the empirical work, we establish that our results are robust to further saturating the model as well as to more relaxed specifications. We also compare our results against others in the quickly developing literature that studies the link between minimum wages and labor market flows, including Dube et al. (forthcoming), who study worker flows using a county border-pair research design, and Meer and West (2013), who study job growth and flows using state-level panel data. Altogether, our results are very robust, both to our own specification changes, and by comparison to the literature.

For users unfamiliar with the QWI, we advise that these data are not seasonally adjusted and exhibit clear seasonal patterns in the raw data. It is for this reason our benchmark specifications also control for state-specific seasonal cycles (or state-sector cycles in the heterogeneity analysis). This feature of the data has not been addressed in other minimum wage research using the QWI; but is important, as we find that our results are sensitive to this control. We suspect this may be associated with a coincidental link between the timing of minimum wage changes and seasonal employment of teenage workers. We therefore recommend that other researchers using QWI data evaluate the sensitivity of their results to this feature of the data.

Equation 5.

3.2 Does the Minimum Wage Bind?

To affect employment, changes in the statutory minimum wage should have a first-order effect on the wages of employed teenage workers. We show this is the case using individual-level data on the wages of teenagers from the CPS. In Table 2, we report estimated minimum wage elasticities for the mean and each decile of the teenage wage distribution. The elasticities are estimated using the specification in Equation (5).[12] Each row in the table presents a different cut of the data. The two rows labeled (QWI) are restricted to state-quarters that appear in the QWI, allowing us to see any effects of the selection imposed by the timing of state entry to the QWI. The last two rows restrict the sample to the 2000–2010 period where the QWI is nearly balanced. The results show that raising the minimum wage increases the mean and increases all deciles of the teen wage distribution up through the median, but has little effect past the sixth decile. We interpret these results as confirmation that the minimum wage is binding on teenage wages during the period of our study. The restriction to observations with QWI data has a mild attenuating effect in the full sample, and no statistically meaningful effect in the 2000-2010 sample.

Bindingness of the Minimum Wage by State and Industry

A key advantage of our research design is that we observe a low-wage group – teenage workers – in every industry across states. This allows us to examine heterogeneity in the response to the minimum wage across state-industry markets. However, there could be substantial variation in the bindingness of the minimum wage for teen workers across states and industries. If so, minimum wage effects that vary by state-industry might be an artifact of the extent to which the minimum wage binds across state-industry pairs rather than variation in market tightness.

Table 3 uses the CPS microdata to provide evidence that the minimum wage is binding for teenagers across most state-industry pairs. For each worker, i, we measure the gap between the log of their reported wage and the log of the minimum wage in their state of residence ($s(i)$) in the month of collection ($t(i)$):

$$GAP_i = \ln(wage_i) - \ln(MW_{s(i),t(i)}). \tag{7}$$

For each industry k, GAP_k^* is the number such that twenty percent of workers in k have $GAP_i \leq GAP_k^*$. The first two columns of Table 3 show the distribution of GAP_k^* across

[12] Note that the number of teenage workers in each state-month cell is relatively small. However, any measurement error this generates should attenuate our estimates.

all NAICS 3-digit industries for teenagers and for adult workers. The third and fourth columns show the distribution of GAP^*_{sk} computed within state-industry pairs. The fifth and sixth columns, which complement the state-level bindingness results in Table 2, show the distribution of GAP^*_s computed by state.

The entry .04 in the first column indicates that, at the third decile of the industry distribution, twenty percent of teen workers earn within 4 percent of the minimum wage. In the median industry, twenty percent of teen workers earn within 9 percent of the current minimum wage. By contrast, in the median industry, twenty percent of adult workers have wages 54 percent higher than the minimum. At the ninth decile, twenty percent of teen workers earn within 22 percent of the minimum wage. The corresponding figure for adults is 83 percent. These patterns are nearly identical in the third and fourth columns which computed the gap measure for state-industry pairs. At the state-level (which masks the variation in the bindingness across industries) at the 90th percentile of the gap distribution across states, twenty percent of teens earn within 7 percent of the minimum wage.

Taken together, the results suggest that, while there is indeed more variation in the bindingness across industries than across states, the minimum wage has some bite virtually everywhere. The average increase in the minimum wage in our data is 10.2 percent, with an interquartile range of 4.8–12.0 percent. Intuitively, if the true employment elasticity is, 0.5, and the minimum wage increases by 10 percent, we need at least 5 percent of the workforce to earn within 10 percent of the minimum wage. Based on the evidence in the table, this will be the case in most of the markets in our data.[13]

4 Effects on Labor Market Outcomes

In this section, we present the results of estimating Equation (5) for each of our employment stock and flow outcomes. We find that increases in the minimum wage have no effect on teenage employment, but substantially decrease turnover and increase the stability of teenage employment. Minimum wage increases have no effect on the rate at which teenage employment is reallocated from firms that are reducing teenage employment to those that are expanding it. Finally, average teen monthly earnings increase as well, indicating that despite any reduction in hours at the intensive margin, the overall wage bill for teenagers increases. After presenting these results, we discuss their magnitude, and then demonstrate

[13]The raw number of teenagers sampled in the CPS each month is not sufficient to produce a table analogous to Table 2 across state-industry pairs (nor across either of these dimensions in isolation) that accounts for heterogeneity in the effect of minimum wages on teen wages.

that they are robust to a range of alternative model specifications.

4.1 Earnings, Employment, Turnover and Job Flows

Panel A of Table 4 presents estimates of the relationship between the minimum wage and earnings, employment, turnover, job flows and job reallocation from the state-level panel-data models in Equation (5). The table includes point estimates for the minimum wage and primary control variables, and the estimated elasticity pertaining to the point estimate for the minimum wage. Increases in the minimum wage have a positive and statistically significant effect on earnings. The employment effect of the minimum wage in the state data is not statistically different from zero at conventional levels. A zero employment effect cannot rule out an hours reduction on the intensive margin, but since we observe higher earnings with minimum wage increases, any reduction in hours should be relatively small. However, our zero employment effect also cannot rule out labor-labor substitution in the market for teenage workers (Giuliano 2013; Ahn et al. 2011).

Moving to the flow variables, minimum wages reduce the rate of worker reallocation, and do not have any effect on job reallocation. That is, the flow of teenage workers into and out of jobs is reduced by the minimum wage. One direct consequence of a reduction in worker reallocation is that the duration of jobs and of unemployment spells should increase even if there is no net effect on employment levels. We check these auxiliary predictions in Section 4.2, but defer further interpretation of our results to Section 5 where we explore the link between employment and worker reallocation more formally.

4.2 Job Stability and the Duration of Nonemployment

The QWI data also provide insight about the mechanisms driving observed changes in employment, job flows, and worker flows. In thinking about labor market dynamics, it is useful to distinguish between 'stable' and 'unstable' employment. The teenage labor market, in particular, involves seasonal work, with employers using teenagers as a flexible source of cheap labor in periods of temporarily high demand. The institutional setting raises the question of whether minimum wages also affect the composition of permanent and seasonal jobs and, if so, in what way?

Panel B of Table 4 investigates the effect of minimum wage increases on the fraction of stable hires and separations and the duration of nonemployment for new hires and separations. The minimum-wage elasticity for the fraction of stable hires is 0.15, meaning that a 10 percent increase in the minimum wage increases the fraction of stable hires by about 1.5

percent relative to a baseline average of about 34 percent. The result is similar for stable separations, with an elasticity of 0.19 and a baseline average stable separation rate of 27 percent.

An increase in both the fraction of stable hires and stable separations indicates a longer tenure on all jobs. This finding is consistent with employers attempting to compensate for higher labor costs by screening for workers less likely to turn over. Alternatively, workers avoid termination when the re-employment probability is low. Employers may also put more emphasis on training and open fewer temporary jobs or reduce seasonal employment. Regardless of the mechanism, an increase in the fraction of stable hires and separations is broadly consistent with any decline in turnover.

Given that increased minimum wages reduce the rate at which teenage workers turn over, minimum wages might also increase nonemployment durations. We check for this effect using variables in QWI that measure the average number of quarters in nonemployment for newly hired workers and newly separated workers. Increasing the minimum wage has no statistically significant effect on the duration of nonemployment for new hires. However, we find a small, but statistically significant (at the 10 percent level) positive effect of increasing the minimum wage on the duration of nonemployment for workers who separate. That is, when the minimum wage is higher, workers experience slightly longer spells of nonemployment. We conclude that the evidence for increased durations of nonemployment is mixed. Since the data are measured at a quarterly frequency, these variables may be too coarse to pick up small changes in nonemployment durations. Furthermore, the number of periods of nonemployment for newly hired and separated workers are measured as averages that record workers who may move job-to-job and do not experience a full quarter of nonemployment as 'zeros'. Shifts toward hiring from unemployment should therefore be picked up as increases in the number of periods of nonemployment. The evidence suggests that higher minimum wages are not associated with a change in the composition of newly-hired workers on the basis of their prior employment history. However, increased minimum wages may be associated with workers being more likely to enter nonemployment (and therefore less likely to be making job-to-job moves), which would be consistent with an decrease in the rate of voluntary separations. In our state-level analysis, the point estimate is small, and sensitive to our specification, so we regard the result as being tentative and suggestive.

4.3 Alternative Specifications

In Table 5, we evaluate the robustness of our initial results. Each row corresponds to a different empirical specification, and each cell reports the estimated elasticity of the minimum wage (and standard error) with respect to the variable in the Column heading. Row (1) restates our benchmark specification from Table 4. To facilitate direct comparison to related research, we add results for separation, hiring, job creation, and job destruction rates.

Rows (2)-(4) relax our preferred specification. Row (2) is identical to row (1), but drops state-specific seasonality effects. Row (3) drops the state-specific recession effects from row (2); row (4) further removes the region-period effects. That is, row (4) is left with controls for state effects, period effects and state-specific time trends. Row (5) then saturates the model by adding a State-Recession-Unemployment rate interaction to our benchmark specification in row (1), which allows for the effect of unemployment to vary by recession and by recessions within states.

Across these five specifications, the results are broadly consistent in terms of sign and significance, although the magnitudes of the elasticities vary. Effects on job reallocation are sensitive to model specification. Furthermore, consistent with a fall in worker reallocation, minimum wages reduce hiring and separation rates with a magnitude that is nearly identical across all specifications. This is to be expected, as Figures 2b and 2d show that hiring rates and separation rates are nearly identical in the aggregate. In all but the most saturated specification (Row 5), higher minimum wages are associated with lower rates of job creation, but the elasticity is very sensitive to model specification.[14]

Some research suggests that minimum wage effects occur with some lag (Neumark and Wascher 1992; Baker et al. 1999; Burkhauser et al. 2000). Row (6) shows the elasticity on the average of the contemporaneous and lagged log minimum wage in a specification that also includes the first difference of the lagged minimum. This “lag operator filter” is motivated by the specification in Equation (2) of Baker et al. (1999). The reported value picks up the long-run influence of the minimum wage after netting out short-run variation. Unlike (Baker et al. 1999), but consistent with other recent findings (Addison et al. 2009; Dube et al. 2010), we do not find evidence of a significant negative long-run effect of the minimum wage on employment. For all of the outcome variables, the long-run filter is very close to the baseline estimate.

[14]In a recent working paper, Meer and West (2013) find that minimum wages negatively affect job creation when looking across all workers in the QWI. When we estimate across all workers, we get similar results. The results for job creation seem particularly sensitive to controls for the state-specific seasonal component, but results for job growth across all workers are robust.

In Row (7), we report elasticities from the county-level analogue of Equation 5. This allows us to control for county-specific observables, trends, and cycles. The results are nearly identical to our state-level results, with the exception that minimum wages have a small negative elastiticity with respect to job destruction rates. This is our only finding of any effect on the job destruction rate across any specification.

Row (8) reports estimates from a county-level model that allows for arbitrary time-series correlation of common within-state shocks while retaining the ability to control for county-level observables: specifically adult earnings and the teen share in the county. We estimate this model using the procedure described in Hansen (2007).[15] The point estimates in Row (8) are close to Row (1) and Row (7). The real point is to correct the standard errors relative to Row (7) for correlated state-specific shocks, and indeed we observe that the estimates in Row (8) are less precisely estimated than those in Row (7), and more precise than Row (1).

Finally, to address concerns about the unbalanced nature of the QWI, Row (9) shows that restricting the benchmark model to the years 2000–2010 has little effect on our results.

5 Employment, Turnover, and Labor Market Tightness

In this section, we present results for our analysis of heterogeneity in the effects of minimum wages on employment across state-industry pairs with different levels of labor market "tightness", based on the model of Equation (6). Recall that we use two variables to proxy for differences in market conditions: average turnover, and average duration of nonemployment for separating workers. Table 6 shows that in high turnover markets, the employment elasticity is large and positive. In low turnover markets, the elasticity is large and negative. This result is mirrored in Table 7 using deciles based on average nonemployment duration of separating workers. In markets where separating workers have longer nonemployment spells, the minimum wage has a large positive effect on employment. In markets where workers have shorter nonemployment spells, the minimum wage has a large negative effect. The remainder

[15]Specifically, we estimate

$$y_{ist} = \zeta_{st} + Z_{ist}\delta + \varepsilon_{ist} \tag{8}$$

$$\zeta_{st} = \lambda_t + \mu_s + t\eta_s + \beta \ln MW_{st} + X_{st}\gamma + \nu_{st} \tag{9}$$

where i indexes a county in state s, and Z_{ist} are county-level log adult earnings and teen share of adult population. X_{st} includes the controls from our state-level models. We estimate in two stages. First, we regress county-level outcomes on time-varying county-level observables to obtain $\hat{\zeta}_{st}$. Second, we estimate the state-level model on $\hat{\zeta}_{st}$. See Bertrand et al. (2004); Hansen (2007) for details.

of this section details the motivation, estimation, and interpretation of these results.

5.1 Motivational Framework

Our investigation is motivated by search-theoretic wage-posting models popular in the analysis of minimum wage policy (Manning 2003; 2006). Markets are characterized by a parameter governing labor market tightness which has the intuitive interpretation as a measure of distance from the perfectly competitive neoclassical benchmark. When labor market tightness is very low, new employment opportunities are relatively difficult to find. Wage dispersion arises with firms extracting monopsony rents from relatively immobile workers. When tightness is high, the distribution of wages collapses to the competitive outcome where firms pay workers their marginal product.

Intuitively, the effect of the minimum wage should vary across markets with different levels of labor market tightness. In tighter markets, the employment effects will be strong and negative. In more slack markets, the minimum wage can potentially increase employment through a process akin to moving firms up individual labor supply curves.

Manning (2003) shows tightness is negatively correlated with the fraction of workers who separate to nonemployment (or, equivalently, the fraction of workers that are hired from nonemployment). In our data, for each market we observe the average number of periods of nonemployment for workers who separate. A worker who separates and is in a new job within the same quarter, contributes a value of zero to the calculation of average nonemployment durations. Therefore, the average duration of nonemployment for separations picks up variation across markets in the share of separations that involve direct job-to-job transitions. This may be a good proxy for tightness. When the average nonemployment spell is low, the market is tighter both because workers move out of nonemployment quickly or because a larger fraction of workers are making job-to-job transitions. The basic wage-posting model leads us to expect that the tightest markets are the most competitive, where we should see the strongest negative effects of minimum wages on employment.

However, the average nonemployment duration is measured using quarterly data, and conflates movements to nonemployment and durations of nonemployment. This is because workers who separate but accede to employment again in the same quarter will be recorded as having zero periods of nonemployment. Our other measure of labor market tightness – the rate of turnover, or worker reallocation rate – does not have this issue. While turnover is measured with greater accuracy, it is less clear that it is related to market conditions rather than employer or industry-specific behavior. On one hand, when markets are tight, workers may turn over more often because job offers arrive more rapidly, making high turnover an indicator of labor market health. On the other hand, if workers also lose employment rapidly for exogenous reasons, because either demand or supply are volatile, then high turnover may be associated with large adjustment costs. These two scenarios make different predictions on heterogeneity in the effect of the minimum wage on employment with respect to the tightness of the labor market. The question is ultimately empirical.

5.2 Estimation Details

To estimate Equation (6), we begin with QWI data that are disaggregated by state and NAICS 3-digit industry. For the period 1990-2010, this yields 244,332 unique state-industry-quarter observations. For each of the 4,269 state-industry combinations that appear in the data, we measure the average level of turnover and the average nonemployment duration of separations throughout the sample. For each state separately, we compute deciles from the state-specific distribution of average industry turnover (or average nonemployment duration) and assign each state-industry-quarter observation in the original dataset to the corresponding (time-invariant) decile. This method emphasizes variation across industry in our proxy measures for labor market tightness, but is flexible enough to allow, say, the food service industry in New Jersey to occupy a different position in the turnover distribution than in California (should that be the case). Furthermore, it captures the fact that workers are more likely to turnover and reallocate within markets that are geographically close together. Lastly, these decile assignments are constant over time. To the extent that turnover maybe be endogenous to innovations in the minimum wage and employment, our non-time varying decile assignments mechanically break that relationship in these data.

This procedure coarsens the data on two dimensions, and is effective in eliminating the part of variation in our measures of tightness that is associated with the minimum wage – the pairwise correlation between the state-industry average turnover and the contemporary minimum wage is -0.0013 and statistically insignificant. However, we show additional results that coarsen the data even further and assign deciles based on the national industry distribution of turnover. Here, each industry is assigned a decile based on the average turnover in that industry over time and across states. This second method should further mitigate concerns about endogeneity, but also has drawbacks. Specifically, it combines geographic variation in turnover across states with variation across industries within state. Also, each particular industry is forced to have the same decile assignment in every state. If the oil and gas industry in Texas has a different level of turnover (or duration of nonemployment) than the oil and gas industry in Oregon, then this decile assignment rule imposes measurement error into our estimates. Our qualitative result remains the same when we use this coarser measure, though the monotonicity in the estimated employment effect of the minimum wage across deciles is weaker, which would be consistent with measurement error associated with pooling state-industry pairs with different levels of turnover.

We have also used two additional methods to assign deciles that yield the same pattern of results as our preferred estimates. One assigns each state-industry a time-invariant decile based on the average turnover distribution over all state-industry pairs (not just over industries within the same state as in our primary measure described above). We have also performed estimates using deciles that vary over time based on current period turnover. This design is more flexible but raises concerns about endogeneity, since Table 4 shows that minimum wages affect both the contemporaneous level of turnover and periods of nonemployment for separations.[16]

Estimating (6) where all of the unobserved heterogeneity is specific to each state- 3-digit NAICS industry is computationally demanding. The NAICS collapses 3-digit industries into a set of twenty major sectors, and most of the time-series variation within-state is common to these major sectors. Our main results fit trends, seasonality, and recession effects for each state-sector (NAICS major sector) effects rather than state-industry (NAICS 3-digit industries). For robustness, we also estimate a specification with state-3-digit industry trends with seasonality and recession effects that are state-sector specific.

5.3 Full Results and Robustness

Employment Effects

Tables 6 and 7 report our main results on heterogeneity in the effect of minimum wages by each proxy measure of labor market tightness. The table entries are estimated minimum wage elasticities for each decile of either the turnover rate or average nonemployment duration for separating workers. Columns (1)-(4) of Table 6 assign deciles based on the within-state distribution of turnover whereas Column (5) assigns deciles based on the national distribution of average industry turnover. Table 7 has the same structure. Also, for both tables, all of the models in columns (1)-(5) include state-3-digit NAICS industry and region-period

[16]The results described in this paragraph are available upon request. We considered assigning deciles based on measures of tightness for non-teenage workers as well. However, non-teen or adult turnover are poor proxies for teen turnover for two reasons: (1) adult and teen turnover are weakly correlated and (2) because non-teen turnover is correlated with the employment of teenage workers, which generates the same endogeneity concerns if one would use teen turnover to begin with.

effects, as well as controls for the adult wage, teen share of the state working age population, and the prime-aged male unemployment rate from our state-level models. Column (1) reports a base model that includes state-sector-specific linear time trends. Column (2) adds state-sector-specific trends, state-sector-specific seasonality and state-sector-specific recession effects. Column (3) shows that the results in Column (2) are not sensitive to dropping observations reported as being "significantly distorted" due to the QWI confidentiality protection methods. Column (4) replaces the state-sector trends in Column (2) with NAICS 3-digit state-industry trends. Column (5) estimates the same model as Column (2) but uses the method to assign deciles based on the more coarse national industry distribution of average turnover (or periods of nonemployment).

The magnitude of the elasticities and their statistical significance are very similar in columns (1)-(3) for both tables. Table 6 shows there are negative employment effects of the minimum wage where turnover is low and positive employment effects where turnover is high. Table 7 shows the same pattern for the duration of nonemployment for separations. When durations of nonemployment are short, higher minimum wages yield lower employment; but when durations of nonemployment are high, there are positive employment effects.

Column (4) of Table 6 controls for trends at the state-industry level. We find the negative employment effects in the lower two deciles are attenuated relative to the first three columns that control for state-sector trends. However, the estimated elasticities at deciles 3 and 4 become more negative, at $-.425$ and $-.497$, and statistically significant at the 5 and 10 percent level. The results in the upper deciles remain roughly the same, and, if anything, increase in magnitude.

Nearly identical changes occur in column (4) of Table 7, which adds state-industry trends (rather than state-sector trends) to our model with heterogeneity by decile of nonemployment duration. The employment effect in decile 1 becomes smaller and insignificant. However, the effect in the deciles 2 and 4 become more negative (-0.791 and -0.977) and significant at the 5 and 2.5 percent level. In this model the estimated positive effects at higher deciles become larger and increase in statistical significance.

Column (5) in Table 6 reports estimates using the national distribution of average industry turnover. The overall pattern of elasticity estimates is similar to the corresponding specification in Column (2), but less monotonic and some statistical significance is lost. As discussed in Section 5.2, this measure likely introduces additional measurement error in the decile assignments. Two of the bottom three deciles in this column are negative and statistically significant; columns 4, 5, and 6 not statistically different from zero; and the estimated elasticities for deciles 7, 8, 9, and 10 are all positive, three of which are statistically different from zero. One interpretation is that the noise in the pattern of results relative to the results in Column (2) arises because Column (5) pools state-industry pairs with different levels of turnover. The Column (5) in Table 7 similarly shows that using the national distribution of average industry durations of nonemployment for separations yields lightly noisier estimates, but does not meaningfully change the results or main conclusions.

Earnings Effects

Table 8 reports estimates of the effect of minimum wage increases on average earnings of workers employed at the end of the quarter. The model in Table 8 is analogous to Column (2) in Tables 6 and 7. In Panel A, higher minimum wages do not appear to have any differential effect on earnings across markets exhibiting different levels of turnover. However, Panel B shows that increases in the minimum wage reduce earnings in markets with low average duration on nonemployment, and increase earnings in markets with high durations of nonemployment.

The expected effect on earnings for retained workers is not straightforward. If a reduction in hours worked (intensive margin) dominates the extent to which the higher minimum wages increase wages, then we would expect a reduction in average earnings when minimum wages rise. If the wage increase due to an increase in the minimum wage dominates any reduction in hours worked, then we would expect average earnings to increase. Of course, employers can also keep their wage bill constant by reducing hours to offset the higher unit labor costs and there would be no effect on earnings due to higher minimum wages.

The Relationship Between Turnover and Non-Employment Duration

Tables 6 and 7 show that higher minimum wages reduce employment in markets where turnover is low and in markets where the average duration of nonemployment for separating workers is low. These results raise a natural question about the relationship between our mediating variables. In principle, they seem related. If turnover is high, one might also expect the duration of nonemployment to be short, since our measure of nonemployment picks up job-to-job transitions. It is important to note that these proxies do not necessarily group the same industries, and that a low turnover subsector does not necessarily also have short periods of nonemployment. The two mediating variables we construct measure complementary, but distinct, facets of the labor market.

We can push our proxy measures of labor market conditions a little further to get at these questions. We do so by jointly classifying markets based on both the level of turnover and the duration of nonemployment. In doing so, we classify a given state-industry across two dimensions of heterogeneity rather than one. Specifically, we group state-industry pairs into "low" and "high" turnover and "low" and "high" nonemployment duration, by combining the bottom five deciles and top five deciles. This yields a four-way classification of state-industries into: (1) low turnover and low durations of nonemployment; (2) low turnover and high durations of nonemployment; (3) high turnover and low duration of nonemployment; and (4) high turnover and high duration of nonemployment.

Table 9 presents the results of estimating heterogeneity in the response of employment to the minimum wage according to our two-dimensional classification. The models are otherwise identical to those reported in Tables 6 and 7. We observe that minimum wages are only associated with negative employment effects in markets where turnover is low and durations of nonemployment are short, consistent with the results for both proxies separately. Minimum wages have no statistically or quantitatively meaningful employment effect in markets with both low turnover, and long durations of nonemployment. However, high turnover markets with short durations of nonemployment exhibit positive employment effects, perhaps indicating that positive employment effects associated with high turnover dominate negative effects associated with low employment duration. Markets with high turnover and long durations of nonemployment have positive employment effects (also consistent with the results for both proxies separately).

Within state-industries that are have low turnover (low WRR), the employment effect of the minimum wage is more negative where nonemployment durations are low: (-0.490 in low nonemployment duration markets versus 0.021 in high duration markets). Conditional on short average nonemployment durations (Low NSEP), the employment effect of the minimum wage is higher where turnover is higher. The one case where the basic pattern fails to hold is in high turnover markets. Conditional on high turnover, the employment effect is positive and statistically significant in low NSEP markets (0.311), but slightly lower in high NSEP markets (0.236).

Our previous results suggest turnover may primarily represent a cost channel for firms in a particular sector, whereas average non-employment duration is a measure of labor market conditions facing the industry in a particular state. This analysis, that jointly classifies markets based on both turnover and nonemployment duration, indicates that negative employment effects most likely occur when the outside market is extremely competitive, and adjustment costs are low. That is, minimum wages induce unemployment where labor market conditions are most like a "spot market". Conversely, minimum wages appear to generate positive employment effects only when turnover is high, re-emphasizing our interpretation of that measure as reflecting the costs of adjusting labor.

Variation in Speed of Adjustment

Table 7 suggests that in markets where nonemployment durations are long, increases in the minimum wage have a positive effect on aggregate employment and earnings. One concern is that in these markets, the speed of adjustment may be relatively slow. If so, part of the heterogeneity we have measured may be coming from differences in the speed of response to the minimum wage.

To check whether this might be the case, we estimate an extension of Equation (6) that incorporates five quarterly leads and five quarterly lags of the minimum wage. Figure 3a displays the estimated long-run effects for the top three deciles (Deciles 8, 9, and 10).[17] The horizontal axis measures the number of quarters relative to a change in the minimum wage. The lines plot the cumulative effect of the minimum wage on employment, expressed as an elasticity. The figure shows no evidence of a long-run negative effect up to five quarters out.[18] Furthermore, there is no evidence of a systematic trend in any of the five quarters prior to the minimum wage change.

For completeness, we display the same information for deciles 4, 5, 6, and 7 (Figure 3b) and deciles 1, 2, and 3 (Figure 3c). Figure 3c indicates there may be a residual trend at deciles 1, 2 and 3 in the quarters prior to the minimum wage change under our benchmark specification with state-sector trends, seasonality, and recession effects. We address this by controlling for the most disaggregated state-industry trends in Column (4) of Table 7. When we do, the effect at deciles 2 and 3 become more negative and statistically significant, and the results at higher deciles become more positive.[19]

Turnover and the Bindingness of the Minimum Wage

Section 3.2 shows that the minimum wage binds for teenagers across industries and states. Nevertheless, there may be specific state-industry combinations for which the minimum wage is not binding, or binds little. A remaining concern is that our results are driven by variation in the extent to which the minimum wage binds on teenage workers across markets with different levels of turnover.

Table 10 presents evidence that this is not likely to be the case. To build on the evidence

[17] Specifically, the leads and lags are added to the model represented by column (2) in Table 7.

[18] Sorkin (forthcoming) argues true long-run effects are hard to identify using U.S. data.

[19] Controlling for state-sector quadratic trends does not substantially change our results relative to the benchmark specification with state-sector linear trends.

from Table 3, we create two measures of the bindingness of the minimum wage within state-industry cells and show these measures are very high for teen workers across deciles of the turnover and nonemployment duration. To construct these measures, we return to the CPS and compute, for each teen, an indicator equal to 1 if their wage is within 10 percent of the prevailing minimum wage in the same state and month.[20] We then calculate the share of teens for which the minimum wage binds in each state-industry cell (using the CPS weights).

The rows of Table 10 correspond to state-industry deciles of turnover (Columns 1 and 2) and nonemployment duration (Columns 3 and 4). The entries in Columns (1) and (3) report the share of state-industry markets in each decile in which at least 10 percent of the teenagers face a binding minimum wage. The intuition underlying this measure is that if 10 percent of workers earn within 10 percent of the minimum wage, then at least 10 percent of workers would be affected by the average minimum wage increase. The fraction of markets that have at least 10 percent of teenagers with wages within a 10 percent increase in the minimum wage is substantial. Turnover deciles one through nine all reflect having at least 93 percent of markets with binding minimum wages. Decile 10 has a somewhat lower fraction of markets with binding minimum wages, but the minimum wage still binds in 84.6 percent of these markets. The results are similarly strong for deciles of nonemployment duration.

Columns (2) and (4) report the average share of teens for whom the minimum wage binds in each state-industry. This is a measure of the number of teenagers we expect to be affected by a minimum wage increase of 10 percent. For turnover (Column 2), these estimates range from 26.4 to 46.8 percent of teenagers. For nonemployment duration, they range from 28.4 to 47.8 percent. While there is variation in these numbers, economically speaking they illustrate that minimum wage increases will have a bite in all deciles. Furthermore, what variation exists in bindingness across our measures measures of tightness is not strongly associated with the pattern of employment effects we observe in the previous tables.

Finally, some caution is required in interpreting Table 10. The number of teens in the CPS in a particular state and a particular industry is very small from month to month and year to year. To construct Table 10, we must pool information across years. Hence, our statistics may conflate periods in which the minimum wage is very binding and those in which it is not binding at all.

[20] We choose 10 percent because it is the size of the average minimum wage increase in our sample.

6 Conclusions

There is a troubling gap between theoretical models of the effects of the minimum wage and empirical evidence. We suspect this is because so many aspects of the labor market response have been hidden from view. New data from the Census Bureau's Quarterly Workforce Indicators combine geographic, industry, and demographic detail on the stock and flow of employment and earnings. The detail in these data facilitates a much finer analysis of the many margins along which labor markets respond to changes in the minimum wage. The primary contributions of this paper have been to use the QWI (i) to further characterize the effect of minimum wage policy on labor market dynamics, particularly turnover and job reallocation, and (ii) to document heterogeneity in the effects of the minimum wage across industries characterized by different labor market dynamics.

Theoretically, the effect of the minimum wage on employment is a function of the degree of competition in the market and the ability of the firm to easily adjust its labor input. We use two variables in the QWI that proxy for these market and employer characteristics: turnover and the average duration of nonemployment for separating workers. Both measures capture heterogeneity across industries in the response of employment to the minimum wage. The employment effects of an increase in the minimum wage are strong and negative in markets with low turnover and in markets with short durations of nonemployment for workers who separate from their jobs. Conversely, increases in the minimum wage are associated with increases in employment in high turnover markets and in markets with high nonemployment durations. These results are robust to various specifications and are not driven by variation in bindingness of the minimum wage.

Our results are consistent with the view that shorter durations of nonemployment correspond to more competitive markets. In the most competitive markets, we expect the data to behave similarly to the neoclassical demand model – increases in the minimum wage bring decreased employment. In the least competitive markets, conditions may be closer to dynamic oligopsony, where its possible that higher minimum wages may increase employment. Our results that estimate the effect of the minimum wage on employment, conditional on the duration of nonemployment, conform to these stylized predictions.

The mechanism for which turnover is mediating the effect of the minimum wage on employment is less clear. The pattern we observe – negative employment effects in low turnover markets – runs counter to a wage posting model in which high turnover firms are at the bottom of the job ladder. However, our results by turnover are consistent with a model in which turnover represents a cost channel for firms. If turnover is very costly, then minimum wages hikes can "pay for themselves" by reducing turnover. This implication emerges from a simple model of adjustment costs based on Manning (2006). In this environment, our observed pattern would be expected – negative employment effects where turnover is low, and positive effects where turnover is high.

Moving forward, more work is needed to better understand the link between turnover, nonemployment durations, minimum wage policy, and employment. Our results indicate that heterogeneity in the effects of the minimum wage are meaningfully summarized by turnover and nonemployment, but it remains an open question whether these variables proxy for variation in conditions within, or across, local labor markets, and whether they proxy for variation in market frictions, supply conditions, or production technology.

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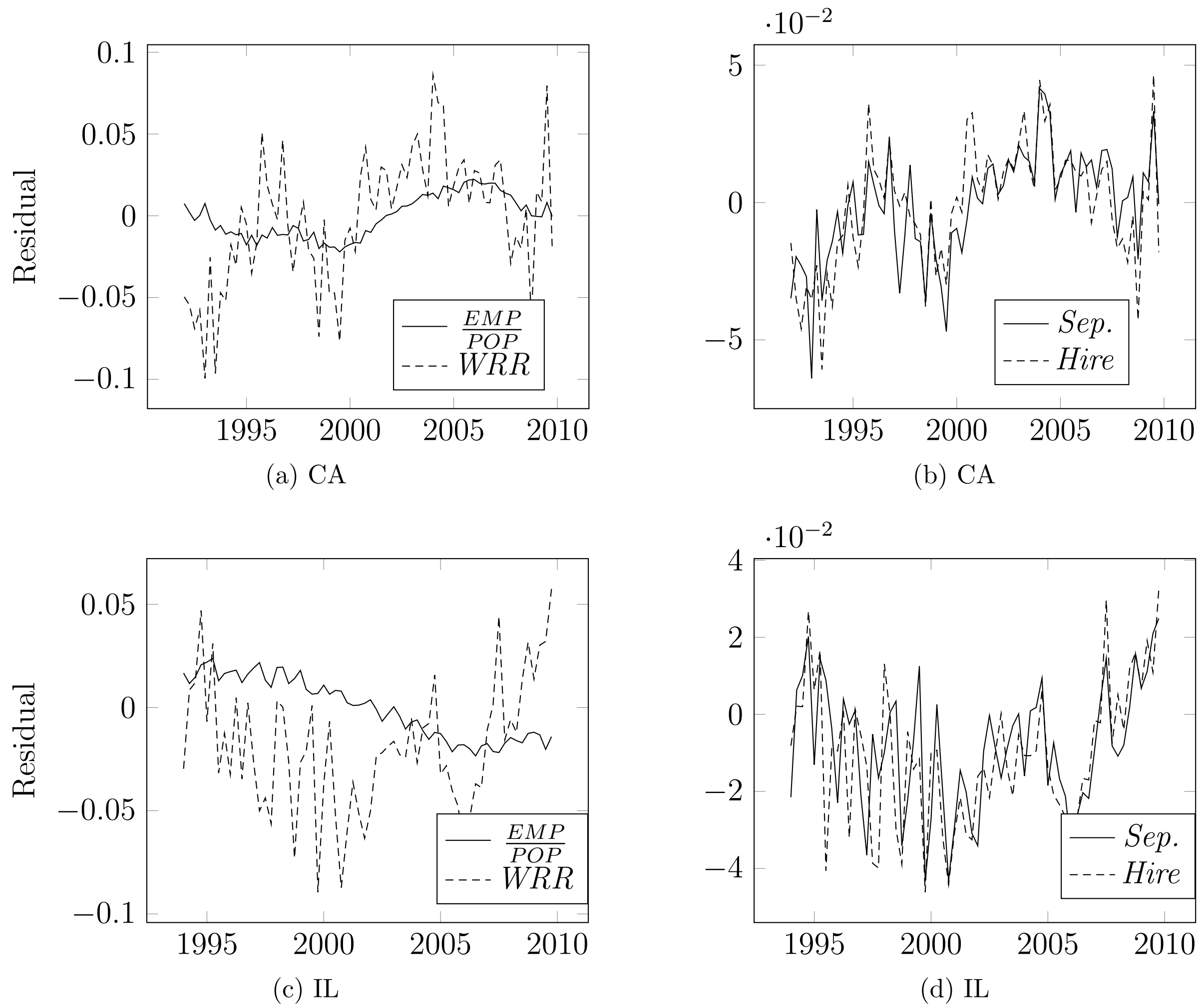
	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010
AK	4.30	4.75	4.75	4.75	4.75	4.75	5.25	5.65	5.65	5.65	5.65	5.65	5.65	7.15	7.15	7.15	7.15	7.15	7.15	7.15	7.75
AL																					
AR																	6.25	6.25	6.25		
AZ																		6.75	6.90	7.25	
CA	4.25	4.25						5.00	5.75	5.75	5.75	6.25	6.75	6.75	6.75	6.75	6.75	7.50	8.00	8.00	8.00
CO																		6.85	7.02	7.28	
CT	4.25	4.27	4.27	4.27	4.27	4.27	4.77	5.18	5.18	5.65	6.15	6.40	6.70	6.90	7.10	7.10	7.40	7.65	7.65	8.00	8.25
DC				5.25	5.25	5.25	5.75	6.15	6.15	6.15	6.15	6.15	6.15	6.15	6.15	6.15	6.15	6.85	7.55	8.25	8.25
DE							4.65	5.00		5.65	6.15	6.15	6.15	6.15	6.15	6.15	6.15	6.65	7.15	7.15	
FL																	6.40	6.67	6.79	7.21	
GA																					
HI	3.85	3.85	4.75	5.25	5.25	5.25	5.25	5.25	5.25	5.25	5.25	5.25	5.75	6.25	6.25	6.25	6.75	7.25	7.25	7.25	
IA	3.85	4.25	4.65	4.65	4.65	4.65	4.65											6.20	7.25	7.25	
ID																					
IL															5.50	6.50	6.50	7.50	7.75	8.00	8.25
IN																					
KS																					
KY																					
LA																					
MA	3.75						4.75	5.25	5.25	5.25	6.00	6.75	6.75	6.75	6.75	6.75	6.75	7.50	8.00	8.00	8.00
MD																		6.15	6.15		
ME	3.85	3.85											5.75	6.25	6.35	6.50	6.75	7.00	7.25	7.50	7.50
MI																	6.95	7.15	7.40	7.40	7.40
MN	3.95	4.25														6.15	6.15	6.15	6.15		
MO																		6.50	6.65	7.05	
MS																					
MT																		6.15	6.25	6.90	
NC																		6.15	6.15		
ND	3.40																				
NE																					
NH	3.75																	6.50	7.25	7.25	
NJ			5.05	5.05	5.05	5.05	5.05	5.05								6.15	7.15	7.15	7.15	7.15	
NM																			6.50	7.50	7.50
NV																		6.33	6.85	7.55	8.25
NY																6.00	6.75	7.15	7.15	7.15	
OH																		6.85	7.00	7.30	7.30
OK																					
OR	4.25	4.75	4.75	4.75	4.75	4.75	4.75	5.50	6.00	6.50	6.50	6.50	6.50	6.90	7.05	7.25	7.50	7.80	7.95	8.40	8.40
PA	3.70																	7.15	7.15	7.15	
RI	4.25	4.45	4.45	4.45	4.45	4.45	4.45	5.15		5.65	6.15	6.15	6.15	6.15	6.75	6.75	7.10	7.40	7.40	7.40	7.40
SC																					
SD																					
TN																					
TX																					
UT																					
VA																					
VT	3.85	3.85				4.50	4.75	5.25	5.25	5.75	5.75	6.25	6.25	6.25	6.75	7.00	7.25	7.53	7.68	8.06	8.06
WA	4.25	4.25			4.90	4.90	4.90	4.90		5.70	6.50	6.72	6.90	7.01	7.16	7.35	7.63	7.93	8.07	8.55	8.55
WI	3.65															5.70	6.50	6.50	6.50		
WV																	5.85	6.55	7.25	7.25	
WY																					

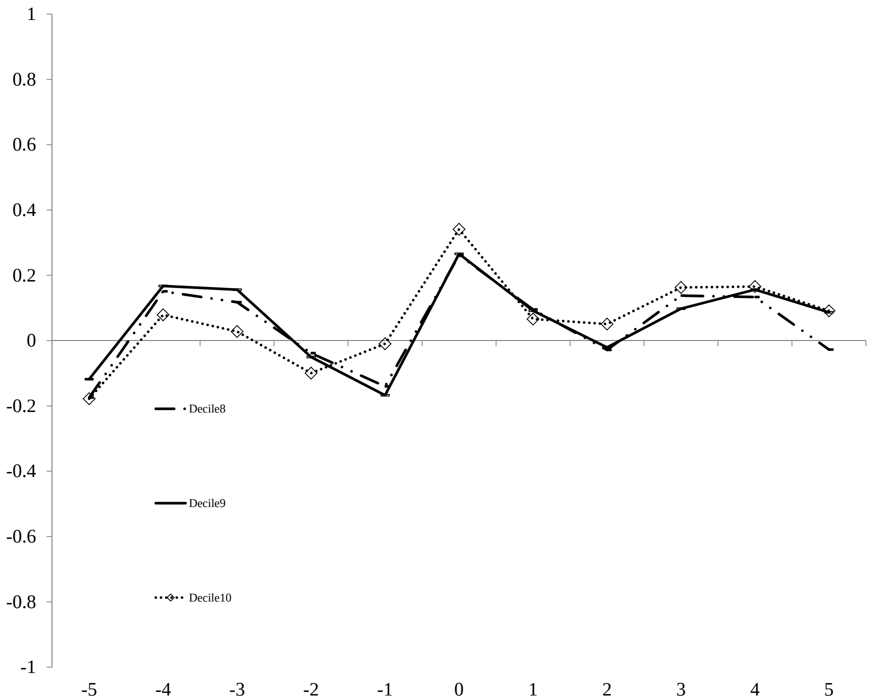
Figure 1: Annual State Minimum Wage Laws overlaid with QWI availability. Cells contain the maximum state minimum wage that exceeds the Federal minimum wage in a given year. The cell contains no entry when the state minimum is never binding. The cells are highlighted black if QWI data are available in that year. While the figure is annual, the minimum wage and QWI data used in our analysis are quarterly. Full data are available from the authors upon request.



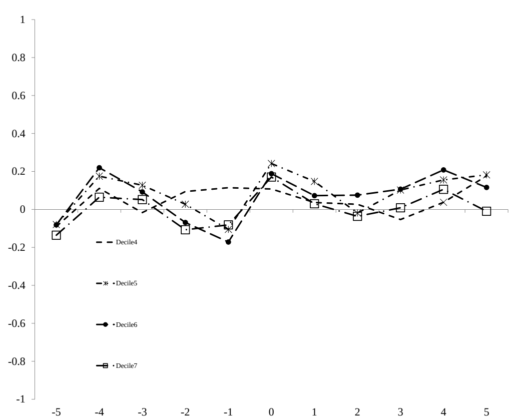
(a) CA (b) CA

(c) IL (d) IL

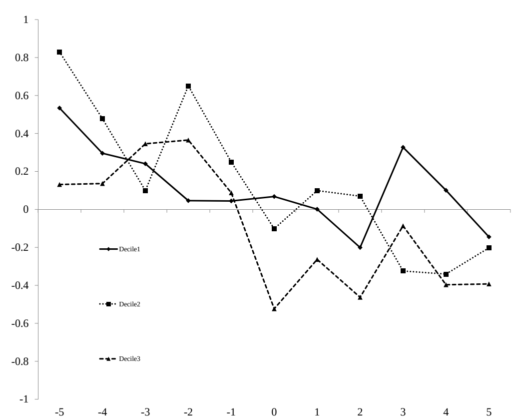
Figure 2: Residual Plots: Employment and Worker Reallocation Rate, and Separation and Hiring Rate for CA and IL



(a) High



(b) Mid



(c) Low

Figure 3: Cumulative Employment Elasticity of the Minimum Wage by Decile of the Average Duration of Non-employment for Separations

Table 1: Summary Statistics

Variable	State-Level
QWI: ($EPOP$) Teen end-of-quarter employment-population ratio	0.28 (.080)
QWI: (lnY) Log avg. end-of-quarter teen earnings	6.22 (.243)
QWI: (WRR) Teen worker reallocation rate	1.19 (.312)
QWI: (JRR) Teen job reallocation rate	0.38 (.060)
QWI: (HSF) Teen fraction of stable hires	0.34 (.114)
QWI: (SSF) Teen fraction of stable separations	0.27 (.060)
QWI: (NS)Teen avg. periods of non-employment for all separations	1.81 (.219)
QWI: (NH)Teen avg. periods of non-employment for new hires	2.63 (.222)
QWI: Log avg. end-of-quarter adult earnings	8.10 (.220)
CPS: Share of teens in working-age pop.	0.09 (.010)
CPS: Prime-male unemployment rate	0.05 (.026)
CPS: Teen quarterly employment-population ratio	0.38 (.102)
CPS: Log teenage wage	1.96 (.170)
CPS: Log adult wage	2.85 (.179)
MW: Log state minimum wage(quarterly)	1.73 (.184)
Number of Observations	$2,786$

Summary statistics for the state-level combined QWI and CPS data. The universe is all state-quarter observations that appear in the QWI. The source for each variable is indicated in its title. All reported summary statistics are weighted by the teenage population. Standard deviations in parentheses.

Table 2: Minimum Wage Elasticities along the Teen Wage Distribution

	Decile									
Sample	1	2	3	4	5	6	7	8	9	Mean
1990–2010	0.32***	0.43***	0.33***	0.22***	0.17***	0.10***	0.04	0.07*	−0.00	0.15***
($N = 4,284$)	(0.055)	(0.027)	(0.026)	(0.024)	(0.025)	(0.030)	(0.033)	(0.043)	(0.044)	(0.039)
1990–2010 (QWI)	0.30***	0.38***	0.32***	0.20***	0.11***	0.05	0.01	0.04	−0.04	0.11***
($N = 2,754$)	(0.070)	(0.031)	(0.037)	(0.034)	(0.032)	(0.040)	(0.044)	(0.050)	(0.051)	(0.048)
2000–2010	0.29***	0.36***	0.30***	0.18***	0.08***	0.05	−0.00	0.06	−0.01	0.11**
($N = 2,244$)	(0.095)	(0.031)	(0.039)	(0.030)	(0.033)	(0.047)	(0.049)	(0.040)	(0.064)	(0.051)
2000–2010 (QWI)	0.27***	0.35***	0.29***	0.17***	0.08**	0.04	0.01	0.07	−0.01	0.10*
($N = 2,058$)	(0.099)	(0.032)	(0.040)	(0.032)	(0.035)	(0.050)	(0.051)	(0.043)	(0.067)	(0.053)

Minimum wage elasticities estimated under the state-level panel data model described in Section 5. The column lists the dependent variable and the row heading lists the sample restriction. The models are estimated under the benchmark specification that controls for average adult wage, prime-male unemployment, the share of teenagers in the working-age population, state and period effects, state-specific trends, state-specific season cycles, state-specific recession effects, and region-period shocks. The models are weighted by the teenage population. The parenthesized values are robust standard errors clustered at the state level.
(*), (**), or (***) indicate the estimate is statistically different from zero at the 10, 5, and 2.5 percent level.

Table 3: Bindingness of the Minimum Wage by State and Industry

	Industry		State-Industry		State	
	Teen	Adult	Teen	Adult	Teen	Adult
Decile 1	.00	.23	$-.02$	.21	$-.02$	.42
Decile 2	.01	.31	.00	.31	.00	.44
Decile 3	.04	.43	.00	.42	.00	.44
Decile 4	.07	.49	.03	.49	.00	.49
Decile 5	.09	.54	.06	.56	.02	.50
Decile 6	.12	.61	.08	.61	.02	.52
Decile 7	.15	.66	.12	.66	.03	.54
Decile 8	.17	.71	.15	.73	.04	.56
Decile 9	.22	.83	.23	.85	.07	.60

SOURCE–CPS-MORG 2000-2010, authors' calculations.
NOTE–The table entries measure bindingness of the minimum wage and are calculated in two stages. The raw CPS microdata are merged to the minimum wage based on the observed state and month. We then compute for each worker the log gap between their reported wage and the minimum wage. We then compute the population-weighted first quintile (twentieth percentile) of the distribution of this gap by NAICS 3-digit industry, by state, and by state-NAICS3 pair. The table entries report the distribution of these quintile cutoffs across sectors and states for teenagers and for adult workers. For state-NAICS3 pairs, we omit cells with fewer than 10 (unweighted) observations.

Table 4: Minimum Wage Effects on Teenage Employment and Labor Market Flows (QWI: 1990–2010)

Panel A

Variable	Log(Earnings)	Employment	Worker Realloc. Rate	Job Realloc. Rate
Log state min. wage	0.10*	0.00	−0.22***	−0.03
	(.054)	(.021)	(.082)	(.017)
CPS: Log avg. adult wage	0.10***	0.03*	−0.12***	−0.04**
	(.042)	(.014)	(.048)	(.019)
CPS: Teen share	0.03	−3.50***	0.32	−0.03
	(.124)	(.157)	(0.210)	(.079)
Prime-male unemp. rate	−0.04	−0.11	−0.54***	−0.00
	(.111)	(.085)	(.196)	(.031)
Min. Wage Elasticity	.10*	.02	−.20***	−.06
	(.054)	(.074)	(.074)	(.040)
R^2	.984	.966	.975	.947
N	2,786	2,786	2,737	2,737

Panel B

Variable	$\frac{\text{Stable Hires}}{\text{Total Hires}}$	$\frac{\text{Stable Sep.}}{\text{Total Sep.}}$	Non-Emp. New Hires	Non-Emp Sep.
Log state min. wage	0.06***	0.06***	−0.06	0.13*
	(.016)	(.013)	(.126)	(.070)
CPS: Log avg. adult wage	0.05***	0.01	−.08*	−0.02
	(.018)	(.010)	(.038)	(.062)
CPS: Teen share	−0.11*	−0.09*	0.06	0.03
	(.061)	(.045)	(0.204)	(.184)
Prime-male unemp. rate	0.05	0.09***	0.63***	0.64***
	(.053)	(.031)	(.238)	(.257)
Min. Wage Elasticity	.15***	0.19***	−.02	.07*
	(.041)	(.041)	(.046)	(.038)
R^2	.977	.972	.957	.953
N	2,688	2,688	2,590	2,685

The dependent variable in each model is listed at the top of the column. The models in every column also contain state effects, period (quarter of sample) effects, state-specific time trends, effects for state-specific seasonality, state-specific recession effects, and region-period interactions. The regressions are estimated by weighted least squares where the weights are the teenage population. Robust standard errors are clustered by state.
(*), (**), or (***) indicate the coefficient is statistically different from zero at the 10, 5, and 2.5 percent level.

Table 5: Minimum Wage Effects: Alternative Specifications

	Panel A: Full Sample									
	Log(Earnings)	Emp/ Pop.	Worker Realloc.	Job Realloc.	$\frac{\text{Stable Hires}}{\text{Total Hires}}$	$\frac{\text{Stable Sep.}}{\text{Total Sep.}}$	Hire Rate	Sep. Rate	Job Creat. Rate	Job Destr. Rate
(1) Benchmark	0.10*	.02	−.20***	−.06	0.15***	0.19***	−.19***	−.20***	−.08*	−0.03
	(0.054)	(.074)	(.074)	(.040)	(.041)	(.041)	(.073)	(.077)	(.058)	(.043)
(2) No State Cycles	0.12**	−.01	−.26***	−.13***	0.20***	0.20***	−.27***	−.25***	−.19*	0.21
	(0.058)	(.066)	(.079)	(.058)	(.055)	(.045)	(.077)	(.104)	(.097)	(.097)
(3) No Cycles/Rec.	0.13**	−.01	−.20***	−.10**	0.24***	0.21***	−.23***	−.16***	−.20*	0.09
	(0.054)	(.065)	(.065)	(.050)	(.060)	(.046)	(.073)	(.069)	(.108)	(.092)
(4) No Cyc/Rec/RegxP	0.07	−.05	−.27***	−.10	0.35***	0.29***	−.28***	−.26***	−.16*	0.02
	(0.058)	(.086)	(.100)	(.063)	(.048)	(.066)	(.096)	(.108)	(.086)	(.060)
(5) Saturated	0.15***	.01	−.18**	−.04	0.14***	0.17***	−.17**	−.19**	−.05	−.02
	(0.042)	(.069)	(.082)	(.045)	(.051)	(.058)	(.083)	(.083)	(.065)	(.053)
(6) Lagged MW	0.10*	.01	−.16***	−.06	0.18***	0.33***	−.17***	−.15***	−.10	−.00
	(0.056)	(.057)	(.065)	(.036)	(.052)	(.079)	(.070)	(.061)	(.066)	(.007)
(7) County-level	0.16***	0.02	−.24***	−.06***	0.14***	0.18***	−.21***	−.28***	−.05***	−0.08**
	(0.019)	(.038)	(.031)	(.021)	(.023)	(.021)	(.029)	(.038)	(.000)	(.039)
(8) County Two-Stage	0.16***	−0.02	−.16***	−.05	0.14***	0.24***	−.16***	−.16***	−.08	−0.02
	(0.046)	(.075)	(.062)	(.034)	(.040)	(.054)	(.064)	(.063)	(.052)	(.034)
(9) Benchmark-2000+	0.10*	.02	−.15***	−.07	0.15***	0.23***	−0.16***	−.13***	−.13	0.04
	(0.056)	(.093)	(.053)	(.051)	(.049)	(.061)	(.058)	(.053)	(.089)	(.068)

Each entry is the estimated elasticity of the outcome variable named in the column heading with respect to the minimum wage. Row (1) reports estimates from the preferred specification. Row (2) drops state-specific seasonality from the benchmark. Row (3) then drops state-specific recession effects from row (2). Row (3) drops region-period effects from Row (4). Row (5) saturates the row (1) benchmark by adding a StateXRecessionXUnemployment interaction (and the corresponding two-way interactions) to our benchmark in row (1). Row (6) reports the elasticity of $\frac{(\ln MW_t + \ln MW_{t-1})}{2}$ in a model that also includes $\frac{(\ln MW_t - \ln MW_{t-1})}{2}$. Row (7) is a count-level model analogous to the benchmark in Row (1). Row (8) reports the benchmark model estimated using county-level data in a two-stage procedure. Row (9) reports the benchmark restricted to observations after 1999. Parenthesized values are robust standard errors clustered by state, except row (7) which clusters at the county-level.
(*), (**), or (***) indicate the coefficient is statistically different from zero at the 10, 5, and 2.5 percent level.

Table 6: Heterogeneity in Estimated Employment Elasticity by Turnover

	Within-State				Nat'l
	(1)	(2)	(3)	(4)	(5)
Turnover Decile					
1	-0.673^{***}	-0.660^{***}	-0.702^{***}	0.001	-0.847^{***}
	(.2304)	(.2357)	(.2520)	(.1054)	(.2449)
2	-0.426^{***}	-0.405^{**}	-0.425^{**}	-0.136	-0.072
	(.1793)	(.1858)	(.1857)	(.2016)	(.0919)
3	-0.169	-0.152	-0.159	-0.425^{**}	-0.731^{***}
	(.1180)	(.1226)	(.1201)	(.1845)	(.1726)
4	-0.176	-0.150	-0.157	-0.497^{*}	0.095
	(.1689)	(.1797)	(.1786)	(.2928)	(.0740)
5	-0.033	-0.009	-0.014	-0.028	0.117
	(.0985)	(.1040)	(.1050)	(.2291)	(.0825)
6	0.191	0.219^{*}	0.220	0.001	0.085
	(.1307)	(.1309)	(.1358)	(.2669)	(.0641)
7	0.306^{***}	0.339^{***}	0.328^{***}	0.145^{*}	0.779^{***}
	(.0866)	(.0911)	(.0973)	(.0817)	(.1835)
8	0.448^{***}	0.483^{***}	0.498^{***}	0.414^{***}	0.430^{***}
	(.1114)	(.1159)	(.1215)	(.0991)	(.0954)
9	0.325^{***}	0.360^{***}	0.364^{***}	0.404^{***}	0.258^{***}
	(.0995)	(.1042)	(.1099)	(.1167)	(.1086)
10	0.096	0.120	0.101	0.255^{***}	0.068
	(.0690)	(.0709)	(.0708)	(.0698)	(.1172)
St.-Sec. Trend	*Y*	*Y*	*Y*	*N*	*Y*
St.-Sec. Cycle	*N*	*Y*	*Y*	*Y*	*Y*
St.-Sec. Recession	*N*	*Y*	*Y*	*Y*	*Y*
Exclude Distorted	*N*	*N*	*Y*	*N*	*N*
St.-Ind. Trend	*N*	*N*	*N*	*Y*	*N*
Num Obs.	244,332	244,332	224,244	244,332	244,332

Table entries are elasticities of employment with respect to the minimum wage evaluated at each decile of the turnover (worker reallocation rate) distribution. The unit of observation is a state-NAICS3-quarter combination. All models control for state-NAICS3 and region-period effects as well as state-sector trends. In columns 1–3, each observation is assigned into a decile of the within-state distribution of turnover across 3-digit NAICS industries. Column (1) reports a benchmark specifiction. Column (2) adds state-sector-quarter-of-year and state-sector-recession effects. Column (3) drops observations that were substantially distorted as part of QWI confidentiality protection (flag=9). Column (4) uses the specification from Column (2), but controls for state-NAICS3-specific trends. Column (5) uses the specification from Column (2), but assigns observations to deciles based on the position of the 3-digit NAICS code in the national distribution of turnover across industries over the sample period. The parenthesized values are robust standard errors clustered at the state level.

(*), (**), or (***) indicate the coefficient is statistically different from zero at the 10, 5, and 2.5 percent level.

Table 7: Heterogeneity in Estimated Employment Elasticity by Average Non-employment Duration for Separating Workers

	Within-State				Nat'l
	(1)	(2)	(3)	(4)	(5)
Avg. Duration Decile					
1	-0.271	-0.258	-0.281^{**}	-0.103	-0.318^{***}
	(.1032)	(.1101)	(.1221)	(.1404)	(.1234)
2	-0.455	-0.441	-0.488^{*}	-0.791^{**}	-1.369^{***}
	(.2613)	(.2736)	(.2759)	(.4054)	(.3538)
3	-0.587^{***}	-0.557^{***}	-0.580^{***}	-0.977^{***}	-0.018
	(.1896)	(.1903)	(.1942)	(.2994)	(.1000)
4	0.209	0.241^{*}	0.225	0.108	0.672^{***}
	(.1373)	(.1414)	(.1374)	(.1389)	(.1255)
5	0.213^{***}	0.241^{***}	0.237^{***}	0.243^{*}	0.181
	(.0906)	(.0950)	(.1001)	(.1384)	(.0753)
6	0.183^{**}	0.212^{**}	0.208^{**}	0.343^{***}	0.242^{***}
	(.0879)	(.0915)	(.0970)	(.1213)	(.0804)
7	0.073	0.102	0.093	0.303^{***}	0.242^{***}
	(.0685)	(.0739)	(.0763)	(.1131)	(.0781)
8	0.115	0.143	0.124	0.286^{***}	0.201^{***}
	(.1020)	(.1049)	(.1148)	(.0983)	(.0801)
9	0.181^{**}	0.209^{**}	0.202^{**}	0.276^{***}	0.223^{*}
	(.0912)	(.0970)	(.1043)	(.1204)	(.1248)
10	0.208^{***}	0.229^{***}	0.222^{**}	0.457^{***}	0.308^{***}
	(.0870)	(.0918)	(.1001)	(.1214)	(.0758)
St.-Sec. Trend	*Y*	*Y*	*Y*	*N*	*Y*
St.-Sec. Cycle	*N*	*Y*	*Y*	*Y*	*Y*
St.-Sec. Recession	*N*	*Y*	*Y*	*Y*	*Y*
Exclude Distorted	*N*	*N*	*Y*	*N*	*N*
St.-Ind. Trend	*N*	*N*	*N*	*Y*	*N*
Num Obs.	244,332	244,332	224,244	244,332	244,332

Table entries are elasticities of employment with respect to the minimum wage evaluated at each decile of the average non-employment duration distribution. The unit of observation is a state-NAICS3-quarter combination. All models control for state-NAICS3 and region-period effects as well as state-sector trends. In columns 1–3, each observation is assigned into a decile of the within-state distribution of turnover across 3-digit NAICS industries. Column (1) reports a benchmark specification. Column (2) adds state-sector quarter-of-year and state-sector recession effects. Column (3) drops observations that were substantially distorted as part of QWI confidentiality protection (flag=9). Column (4) uses the specification from Column (2), but controls for state-NAICS3-specific trends. Column (5) uses the specification from Column (2), but assigns observations to deciles based on the position of the 3-digit NAICS code in the national distribution of average non-employment durations across industries over the sample period. The parenthesized values are robust standard errors clustered at the state level.
(*), (**), or (***) indicate the coefficient is statistically different from zero at the 10, 5, and 2.5 percent level.

Table 8: Heterogeneity in Estimated Earnings Elasticity

Panel A		Panel B	
Turnover Decile		Avg. Dur. Decile	
1	0.018	1	-0.168^{***}
	(.0824)		(.0616)
2	0.032	2	-0.125^{***}
	(.0790)		(.0612)
3	0.015	3	-0.074
	(.0758)		(.0603)
4	0.035	4	-0.032
	(.0699)		(.0619)
5	0.043	5	0.048
	(.0786)		(.0697)
6	0.078	6	0.072
	(.0646)		(.0825)
7	0.048	7	0.104
	(.0780)		(.0646)
8	0.052	8	$0.161**$
	(.0906)		(.0910)
9	0.023	9	0.191^{***}
	(.0724)		(.0710)
10	-0.107	10	0.113
	(.0800)		(.0863)
Num. Obs.	237,415	Num. Obs.	237,415

Table entries are earnings elasticities with respect to the minimum wage. In Panel A, the elasticities are evaluated at each decile of the turnover (worker reallocation rate) distribution. In Panel B, the elasticities are evaluated at deciles of the distribution of average non-employment duration for separations. The unit of observation is a state-NAICS3-quarter combination. Parenthesized values are robust standard errors clustered at the state level. Both models include state-industry and region-period effects, and state-sector trends.

(*), (**), or (***) indicate the coefficient is statistically different from zero at the 10, 5, and 2.5 percent level.

Table 9: Two Dimensional Classification of Labor Market Tightness: Employment Elasticities

	Within-State			Nat'l
	(1)	(2)	(3)	(4)
Low WRR/Low NSEP	-0.490^{***}	-0.472^{***}	-0.476^{***}	-0.512^{***}
	(.1588)	(.1677)	(.1655)	(.1465)
Low WRR/High NSEP	0.021	0.044	0.031	0.084
	(.0777)	(.0835)	(.0866)	(.0750)
High WRR/Low NSEP	0.312^{***}	0.344^{***}	0.358^{***}	0.464^{***}
	(.0700)	(.0721)	(.0769)	(.0817)
High WRR/High NSEP	0.236^{***}	0.265^{***}	0.254^{***}	0.280^{***}
	(.0871)	(.0906)	(.0960)	(.0863)
St.-Sec. Trend	*Y*	*Y*	*Y*	*Y*
St.-Sec. Cycle	*N*	*Y*	*Y*	*Y*
St.-Sec. Recession	*N*	*Y*	*Y*	*Y*
Exclude Distorted	*N*	*N*	*Y*	*N*
Num. Obs.	244,332	244,332	224,244	244,332

Table entries are employment elasticities with respect to the minimum wage evaluated at cross-clasifications of turnover by duration of non-employment for separations. For example, Low WRR/Low NSEP inidicates a state-NAICS3 combination that has average turnover and periods of non-employment for separating workers below their state-specific or national medians. The unit of observation is a state-NAICS3-quarter combination. All models include state-NAICS3 and region-period effects. Parenthesized values are robust standard errors clustered at the state level. The columns are organized as in Tables 6 and 7.
(*), (**), or (***) indicate the coefficient is statistically different from zero at the 10, 5, and 2.5 percent level.

Table 10: Bindingness of the Minimum Wage by State-Industry Turnover and Non-Employment Duration Decile

	Turnover		Non-Emp. Dur.	
	Share > 0.1 (1)	Avg. (2)	Share > 0.1 (3)	Avg. (4)
Decile 1	0.931	0.351	0.959	0.346
Decile 2	0.964	0.468	0.980	0.478
Decile 3	0.973	0.391	0.972	0.418
Decile 4	0.970	0.432	0.932	0.351
Decile 5	0.970	0.397	0.838	0.284
Decile 6	0.961	0.413	0.910	0.314
Decile 7	0.967	0.376	0.852	0.284
Decile 8	0.982	0.344	0.916	0.320
Decile 9	0.963	0.329	0.976	0.367
Decile 10	0.846	0.264	0.999	0.381

SOURCE–CPS-MORG and QWI, authors' calculations.
NOTE–The table entries report the share of state-NAICS3 pairs for which the minimum wage is binding in each turnover (non-employment duration) decile. We define bindingness of the minimum wage in two stages. The raw CPS microdata are merged to the minimum wage based on the observed state and month. The minimum wage binds for a worker if the difference between the natural log of his or her reported wage and the log minimum wage is less than 0.1. We then define the bindingness of the minimum wage in a state-NAICS3 market as the (weighted) share of workers for whom the minimum wage binds. Column (1) reports, for each turnover decile, the share of state-NAICS3 markets in which the minimum wage binds for at least 10 percent of teen workers. Column (2) reports, for each turnover decile, the weighted mean bindingness across state-NAICS3 markets. Columns (3) and (4) report the same statistics by deciles of the distribution of average non-employment duration. In all cases, we omit cells with fewer than 10 (unweighted) observations.

A Appendix

A.1 Graphical Evidence in Support of Identification

Figure 4 plots raw outcomes and the residual variation in the employment-to-population ratio and worker reallocation rate for the eight quarters before and eight quarters after an increase in the effective minimum wage. These plots support the identifying assumption by demonstrating that there is no pre-event pattern in the residual after absorbing heterogeneity associated with state, period, and state-trend effects. Figures 4a and 4c display the raw (seasonally adjusted) average level of the outcome variable, and Figures 4b and 4d display the average residual after controlling for state and period heterogeneity, and state-specific linear time trends. In each plot, the dashed lines represent robust 90 percent confidence intervals that allow for arbitrary within state error correlation. [21]

Figures 4a and 4c show that minimum wage increases are associated with decreased teenage employment and worker reallocation. In Figure 4b, after introducing basic controls, there is no visible effect for employment. However, Figure 4d still shows a drop in residual worker reallocation following a minimum wage adjustment. These figures do not include all of the controls that appear in our final models, and do not allow for heterogeneity in the size of the minimum wage change. Nevertheless, the full analysis supports the impression given by the figures.

B Data and Variable Construction

B.1 State-level Analysis

Merging CPS Estimates to QWI

Individual micro data from the CPS were aggregated to state-month estimates using the individual survey weights provided by the CPS. As in previous work, teens are defined in these data as those who are 16-19 years old, adults are those 25-54, and working age population is defined as those 16-64. These CPS variables include the teen wage rate, the teen employment rate, the prime-age male unemployment rate, a measure of the adult wage rate, the teen share of the working age population, and an estimate of the teen population that is used as a weight in the summary statistics and the estimation results.

There were some individuals who reported a zero wage but positive hours worked in the MORG micro data. These individuals were dropped when calculating the state-level aggregate data. Note that, the state-level estimate for the prime-age male unemployment

[21] To produce Figure 4, we obtain residuals by regressing the raw outcome onto state and period dummies along with state-specific linear time trends and quarter-of-year indicators. We then identify state-specific events where the binding minimum wage changes. We restrict attention to events for which another minimum wage change does not occur within the same six quarters. For each such event, we construct an event-period dataset whose variables are the raw outcomes, residual outcomes, and time-to-event indicators. The figures plot the point estimates and associated 90 percent cluster robust confidence intervals from a regression of the raw or residual outcome onto time-to-event indicators on the pooled events.

rate identifies individuals who are unemployed with a zero/one indicator and takes the weighted average of the indicator (rather than finding the weighted average of the numerator and denominator separately and then taking the ratio).

Teen Wages and Bindingness of Minimum Wage

The definition of teen statistics varies slightly in the CPS from the QWI. In the QWI, data are published in pre-defined age categories that pool teens aged 14-18. The CPS collects labor force information only from members of the civilian working-age population, which the U.S. Census Bureau and BLS restricts to individuals age 16 or older (Bureau 2006, ch. 5). It is therefore not possible to get measures of the wage rate for 14 and 15 year-old workers. We use workers aged 16-18 to get as close an overlap as possible with the QWI definition.

Employment to Population Ratio

The employment count is taken from the QWI while the population estimate is generated from the CPS. Since the QWI publish data for teens in a 14-18 group, we have two choices for constructing the employment-to-population ratio: either to construct quarterly measures of the population of those age 16-18 from the CPS-MORG, or use annual estimates of the 15-19 year-old population from Census http://www.census.gov/popest/methodology/index.html. We choose the former approach, which allows us to benchmark our employment results against prior state-level analyses using CPS data. Our results are not sensitive to this choice.

An alternative is to calculate and use the employment to population ratio from the CPS instead, and not use the employment count from the QWI in the dependent variable. This measure has drawbacks, however, since the group of teens would be inconsistent with the rest of the analysis. Regardless, our results are consistent whether we use employment measured from the CPS or the QWI as a dependent variable. This is important to note. The QWI use an administrative, jobs-based measure of end-of-quarter employment while the CPS is a household survey-based analogue. The average employment-population ratio for teenagers 16-18 in the CPS is 0.38, and the corresponding statistic in the QWI for 14-18 year olds is 0.28. The discrepancy is partly due to composition. The QWI measure reports the number of jobs held in private sector firms by 14-18 year old workers, while the CPS reports the employment status of workers aged 16 and over. There are also different forms of measurement error in the two sources. Using microdata that link CPS individuals to their LEHD records, Abraham et al. (2009) find that approximately 10 percent of those reporting employment in the CPS do not have a corresponding job recorded in the LEHD data underlying the QWI. That estimate is consistent with our employment ratios noted above. Since the QWI correspond to the universe of private sector non-agricultural employment covered by UI records, those individuals appearing in the CPS but not the QWI either misreport employment, work in agriculture or are employed in a position not covered by UI.

Other Dependent Variables

The measurement issue regarding the numerator and denominator of the employment to population ratio is not present in the other dependent variables we consider (worker reallocation rate, job reallocation rate, etc). The denominator in these other variables is the measure of employment taken from the QWI; hence, the numerator and denominator reference the same age group.

B.2 County-level Analysis

Because the geocodes in the CPS only allow statistics to be aggregated to the state-level and not lower-levels of geography, our county-level models use slightly different measures of the variables in the state-level analysis. Even though the county variables are not 1-1 counterparts of the state-level variables, the county and state results are very similar.

Employment to Population Ratio

The employment count at the county-level is taken from the QWI as in the state-level analysis, but we use county-level population estimates from the U.S. Census Bureau. These population estimates are for teenagers 15-19 years old, and they are annual estimates rather than quarterly. The Internet release dates are 6-23-2003 for the 1990–1999 files, June 2010 for 2000–2009 files, and May 2011 for the 2010 population estimates. These data are available for download at http://www.census.gov/popest/data/index.html/ or by request from the authors. We are able to construct analogous measures of adult earnings and teen share of the population at the county-level. However, unemployment data for prime age males are only available at the state-level in the public-use CPS.

Control Variables

To match the state-level specification as closely as possible, we need county-level estimates for (1) adult wage rate, (2) teen share of the working age population and (3) prime age male unemployment rate. Our county-level estimate of the adult wage rate is the adult average monthly earnings from the QWI. This measure is the (QWI) employment weighted average of monthly earnings for workers 22-54 years old (QWI groups: A04, A05, A06). The teen share of the working age population is taken from the county annual population estimates files. It is the ratio of the teen population 15-19 divided by the population of individuals 15-64. Quarterly or annual county-level estimates for prime-age male unemployment are not available. Therefore, in the county-level specifications, we use the state-level measure of prime age male unemployment calculated from the CPS.

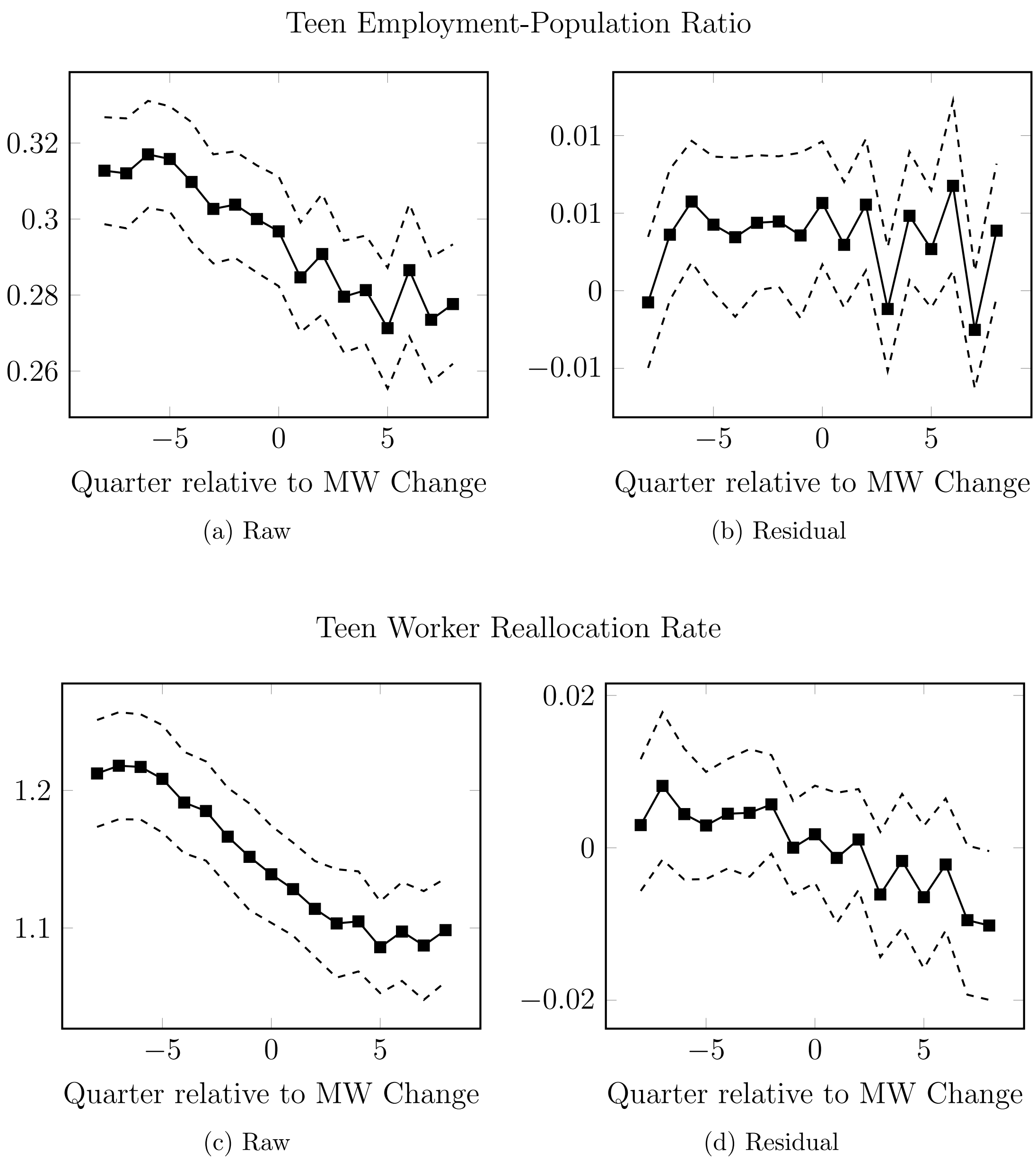


Figure 4: Event Study of Employment and Worker Reallocation Around Minimum Wage Changes. Figures (a) and (c) plot the raw data in each quarter surrounding a minimum wage change, while (b) and (c) plot the residual variation that remains after removing unobserved state and period effects as well as state-specific linear time trends. Dashed lines are 90 percent confidence intervals.