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

R. Kaj Gittings
Department of Economics
Texas Tech University
kaj.gittings@ttu.edu

Ian M. Schmutte
Department of Economics
Terry College of Business
University of Georgia
schmutte@uga.edu

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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 that exhibit 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 involving 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 speed at which workers move in and out of non-employment. 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. (2010) 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 non-employment experienced by workers who separate. Evaluating these measures individually, low turnover markets have a strong disemployment response to minimum wage increases, while high turnover markets have a strong positive employment response to minimum wage increases. Similarly, markets where workers experience short durations of non-employment also have strong disemployment effects of the minimum wage, but we observe positive employment effects in markets where the duration of non-employment for separated workers is long. When we classify markets two dimensionally based on both the level of turnover and the duration of non-employment, the channels through which we observe positive and negative employment effects becomes more clear. Both turnover and the duration of non-employment play mediating roles, but the negative and positive employment effects are largely driven through heterogeneity across markets in the duration of non-employment.

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 non-employment 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 effective way to represent treatment effect heterogeneity in the minimum wage context complements previous work.

Other papers have looked directly at 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 (2013) and Brochu and Green (2011) exploit variation in minimum wages across Candian 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. Two recent working papers use QWI data to study the effects of minimum wages on rates of hiring and separation (Dube et al. 2013) and on job flows (Meer and West 2013). Our paper is most closely related to

Dube et al. (2013).[1] Both papers 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. (2013), 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 2010). 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

[1] We became aware of the independent work by Dube et al. (2013) and Meer and West (2013) while this paper was undergoing revision.

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. 2008; 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 integrated 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.[2]

The QWI data used in this paper cover 49 states for a period ending 1990:Q1–2010:Q4.[3]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.[4]
- NH: average number of periods of non-employment for new hires.
- NS: average number of periods of non-employment for separated workers.[5]

[2]For a comprehensive overview of the LEHD data and production of the QWI, see Abowd et al. (2008).

[3]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/.

[4]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.

[5]Periods of non-employment for new hires represents the number of quarters of non-employment experienced prior to being hired. Periods of non-employment for separations represent the number of quarters of non-employment post-separation.

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 our 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.

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,

$$C = \frac{JR + JF}{2} \quad (3)$$

$$D = \frac{JR - JF}{2}. \quad (4)$$

Therefore we face a modeling choice of whether to directly model 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, but it is difficult to interpret what it means for these flow measures to be affected by the level of the minimum wage. It is more straightforward conceptually and theoretically to model turnover as a stock variable, whose level responds to the minimum wage.

An inspection of the data provides support for this view. 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.[6] 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. 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).

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.

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

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 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.[7] 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 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.[8] 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 min-

[7] 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.

[8] 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

imum 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 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 in when 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.[9]

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.[10] 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.[11]

[9] 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.

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

[11] See data appendix for details.

2.4 Dependent Variables

We focus on the following variables:

- $EPOP$: 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 total 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 non-employment for separated workers.
- NH: average number of periods of non-employment 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 non-employment experienced in the previous year by the worker before being hired. For all separations, they report the number of quarters of non-employment experienced by the worker subsequent to leaving employment.[12] Consistent with the strong seasonality of their employment, teenage workers experience on average 2.63 quarters of non-employment after separating from jobs, and have experienced 1.81 quarters of non-employment 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. Finally, we demonstrate that increases in the statutory minimum wage actually affect the wages earned by teenage workers – a necessary condition for minimum wages to affect employment outcomes.

3.1 Empirical Models and Identification

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

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

s indexes the state and t indexes the period (quarter). y_{st} is one of many labor market outcomes, including employment, worker reallocation rate, job reallocation rate, etc. λ_t and μ_s are state and period-specific effects. $t\eta_s$ absorbs state-specific trends in the outcome. $\omega_{r(s)t}$ allows for the period specific shock to vary across Census regions. The function $r(s)$ maps state s to its Census region. $RECESSION_t$ 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

[12]The number of periods of non-employment are inclusive of zeros, i.e. those workers who did not experience a quarter of non-employment before being hired.

on the outcome from being in a recessionary period. $\chi_{s,q(t)}$ captures state-specific seasonal fluctuations. The function $q(t)$ maps period t to the quarter of the year (I, II, III, IV). $\ln MW_{st}$ is the natural logarithm of the effective minimum wage in state s as of the beginning of period t (first month of the quarter), 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. ν_{st} accounts for unobservables affecting state-level outcomes in period t.

We use the QWI data disaggregated by state and industry (3-digit NAICS) to study heterogeneity in the effect of the minimum wage on employment across markets with different levels of turnover. To do so, we adopt a semi-parametric approach whereby we assign to each state-industry pair its average worker reallocation rate over the sample. We then pool these rates into deciles, and measure the heterogeneity in minimum wage response across deciles. Specifically, we estimate

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

where k indicates the NAICS 3-digit industry. I_{ks}^d is an indicator equal to 0 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 average turnover. $Z_{kst}\delta$ absorbs observable characteristics that vary across sectors within a state. In practice, this includes the log of average earnings of adult workers.

Identification relies on the assumption that changes in the minimum wage are not correlated with unmodeled state-specific movements in the outcome. The appropriate model and research design continues to be the subject of much controversy (Neumark et al. 2013; Allegretto et al. 2013). Our preferred specification is highly saturated, and controls for many different confounders considered in the recent literature, including state-trends (Addison et al. 2009), region-period shocks(Meer and West 2013) and state-specific responses to recession (Allegretto et al. 2008; Neumark et al. 2013). 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 the Dube et al. (2013) working paper, which studies worker flows using a county border-pair research design, and the Meer and West (2013) working paper, which studies job growth and flows using state-level panel data. Altogether, our results a very robust, both to our own specification changes, and by comparison to the literature.

Our preferred specification also controls state-specific seasonal cycles, which are a central feature of QWI data (which are not seasonally adjusted). This feature of the data seems not to have been appreciated in other working papers in this area, and we find that our results are sensitive to this control. In our case, we suspect this may be associated with the link between the timing of minimum wage changes and seasonal employment. We therefore recommend that other researchers using QWI data evaluate the sensitivity of their results to this feature of the data.

3.2 Does the Minimum Wage Bind?

For our analysis to be credible, 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 data on the wages of teenagers from the CPS. Table 2 summarizes the deciles and means of the teen wage distribution across state-quarters. The distribution is relatively narrow. The average of the ninetieth percentile wage is just \$8.89 with a standard deviation of \$2.254. This highlights that teenagers are low-wage workers regardless of the market in which they are employed.

In Table 3, 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)[13] 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.

[13]Note that the number of teens in each state/month cell to calculate the statistics in Table 2 is relatively small. However, any measurement error this generates should attenuate our estimates.

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, consistent with Table 2, 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 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 5 where we explore the link between employment and worker reallocation more formally.

4.2 Job Stability and Periods 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 number of periods of non-employment 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 non-employment durations. We check for this effect using variables in QWI that measure the average number of quarters in non-employment for newly hired workers and newly separated workers. Increasing the minimum wage has no statistically significant effect on periods of non-employment 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 periods of non-employment for workers who separate. That is, when the minimum wage is higher, workers experience slightly longer spells of non-employment. We conclude that the evidence for increased durations of non-employment is mixed. Since the data are measured at a quarterly frequency, these variables may be too coarse to pick up small changes in non-employment durations. Furthermore, the number of periods of non-employment for newly hired and separated workers are measured as averages that include workers who do not experience a full quarter of non-employment as 'zeros'. Shifts toward hiring from unemployment should therefore be picked up as increases in the number of periods of non-employment. 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 non-employment (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 state, 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

[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.

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.

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.

[15] Specifically, we estimate

$$y_{ist} = \zeta_{st} + Z_{ist}\delta + \varepsilon_{ist} \tag{7}$$

$$\zeta_{st} = \lambda_t + \mu_s + t\eta_s + \beta \ln MW_{st} + X_{st}\gamma + \nu_{st} \tag{8}$$

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.

5 Employment, Turnover, and Labor Market Tightness

Our analysis to this point has assumed that the effect of the minimum wage on employment is the same across markets. However, the disaggregated nature of the QWI data allow us to study variation across specific submarkets. We focus our attention by examining heterogeneity in the minimum wage effect across markets characterized by different amounts of labor market "tightness". A well-developed theoretical literature postulates that the employment effects of the minimum wage may vary greatly depending on how "tight", or competitive, the labor market is. In markets with substantial 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 by labor market tightness in the employment effects of the minimum wage.

Our main results are summarized graphically in Figure 3, which reports elasticities of the minimum wage at different deciles of labor market tightness, estimated under the model of Equation 6. The QWI provide two proxy measures of labor market tightness. The first is our measure of turnover – the worker reallocation rate; the second is the number of periods of non-employment for separating workers. Each measure has its own advantages and potential drawbacks, which we discuss in detail below. As we will see, both lead us to similar, though not identical conclusions, opening fresh questions about the link between minimum wages and the dynamics of teenage labor markets. To measure 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 periods of non-employment for separating workers – for each state-NAICS 3-digit industry and pool these measures into deciles for estimation. Details of this procedure are described in Section 5.2.

Figure 3a illustrates that the employment effect of the minimum wage exhibits strong heterogeneity across markets with different levels of turnover. 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 Figure 3b using deciles based on average non-employment duration of separating workers. In markets where separating workers have longer non-employment spells, the minimum wage has a large positive effect on employment. In markets where workers have shorter non-employment spells, the minimum wage has a large negative effect. The remainder 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 non-employment (or, equivalently, the fraction of workers that are hired from non-employment). In our data, for each market we observe the average number of periods of non-employment 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 non-employment durations. Therefore, the average duration of non-employment 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 non-employment spell is low, the market is tighter both because workers move out of non-employment quickly and/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, so there we should see the strongest negative effects of minimum wages on employment.

However, the average non-employment duration is measured using quarterly data, and conflates movements to non-employment and durations of non-employment. This is because workers who separate but accede to employment again in the same quarter will be recorded as having zero periods of non-employment. 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 how turnover is related to market tightness. 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, say because demand is volatile, then high turnover may be associated with relatively slack markets. Therefore it is unclear a priori how employment effects will vary across markets with heterogeneous turnover. 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. To address the concern that our tightness measures are endogenous to innovations in the minimum wage and employment, for each of the observed 4,269 state-industry combinations that appear in the data, we measure the average level of turnover and the average non-employment duration of separations throughout the sample. We then assign to each state-industry-quarter observation in the original dataset the non-time-varying decile in the distribution of turnover (or periods of non-employment) across the population of state-industry pairs. This procedure coarsens the data on two dimensions, and is effective in eliminating the part of variation in our measures of tightness that are 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.

Our findings for heterogeneous minimum wage effects on employment and earnings are robust across a variety of specification changes in Equation 6, and are very similar across two quite different measures of labor market tightness we described here. We have also estimated models in which each state-industry-quarter was assigned a decile based on its current period turnover relative to the distribution of turnover across the full sample. This design is more flexible in that it allows the measured tightness of state-industry markets to evolve over time, but is potentially subject to the endogeneity issues described above since Table 4 shows that minimum wages affect both the contemporaneous level of turnover and periods of non-employment for separations. Nevertheless, results from that specification are very close to the results in Figure 3.[16]

[16] Results for time-varying deciles 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.

5.3 Full Results and Robustness

Employment Effects

Tables 6 and 7 present the results for each proxy measure of labor market tightness along with a set of robustness checks. The table entries are estimated minimum wage elasticities at a decile of the "tightness" distribution, proxied either by the turnover rate or average non-employment duration for separating workers. Column (1) is the benchmark specification described by Equation (6), and corresponding to the results displayed in Figure 3. Column (2) reports results from the model with saturated heterogeneity controls that adds state-specific unemployment, state-recession, and state-unemployment-recession effects to our benchmark specification, while Column (4) reports a model that controls only for state effects, period effects and state-specific trends.[17] Column (3) shows that the results are not sensitive to dropping observations reported as being "significantly distorted".

The results exhibit very little variation across specifications, and across tables. One exception is the anomalously low elasticity at the fourth decile of average non-employment duration (Table 7). Given the magnitude of the confidence intervals, the deviation from the overall pattern of results is not of major concern, but merits investigation. As we will see, the difference seems to be associated with variation in the long-run relative to short-run response to minimum wage adjustments.

Earnings Effects

If the pattern of employment elasticities in Tables 6 and 7 are driven by labor market tightness, then we expect a similar pattern in labor market earnings. Specifically, in markets where we see disemployment effects, we should also see a decrease in earnings, and we should observe earnings increases in markets where employment increases. Table 8 presents estimated elasticities of average teen earnings with respect to a change in the minimum wage across deciles of both tightness proxies. Panel A presents results using turnover deciles. While the pattern in the point estimates is increasing across deciles, they are imprecisely estimated. Panel B presents elasticities across deciles of the average non-employment duration distribution. The pattern of results is strikingly consistent with the results for total teen employment: markets with longer non-employment duration experience increases in teen earnings. Markets with short non-employment durations experience decreases in teen earnings. These results add support for our interpretation of non-employment duration of separating workers as a preferred proxy for labor market tightness.

[17] The specifications for Columns (2) and (3) correspond to Rows 5 and 4 of Table 5.

The Relationship Between Turnover and Tightness

We next turn to an analysis that considers the possibility that our two proxies are measuring different things. Periods of non-employment for separating workers is a noisy, but theoretically clear proxy. Turnover is better measured, but its theoretical relationship to labor market health is not as clear. As a proxy for labor market tightness, we might expect that higher turnover would correspond to a healthier labor market, as suggested by Lazear and Spletzer (2012). However, turnover can also be a cost channel for firms, in which case high turnover markets may be less dynamic. Our results indicate that employment effects are strong and negative in low turnover markets, and positive in high turnover markets, which is consistent with the latter view.

To get at what each proxy is measuring, we classify markets by grouping them by turnover and average non-employment duration for separations. We aggregate turnover by combining the bottom five deciles and top five deciles. We do the same for the periods of non-employment deciles. This identifies four types of submarkets: (1) low turnover and low durations of non-employment; (2) low turnover and high durations of non-employment; (3) high turnover and low duration of non-employment; and (4) high turnover and high duration of non-employment. These four categorical variables are interacted with the minimum wage in the same way the deciles were, and the model we estimate is our benchmark specified in Equation (6).[18]

Table 9 presents the results for our benchmark and alternative specifications as in Tables 6 and 7. Our heterogeneous employment effects are driven largely by variation in non-employment duration. Regardless of whether turnover is low or high, negative employment effects only occur in markets with short average non-employment durations. Similarly, positive employment effects are only occur in slack markets with high average non-employment durations. Furthermore, conditional on being a low turnover market, as the duration of non-employment falls (the market becomes more competitive), the employment elasticity become more negative. The same is true for high turnover markets – when durations of non-employment shorten, the employment elasticity becomes more negative. However, the level of turnover does play a role in mediating the employment effect of the minimum wage as well. Conditional on being in a more competitive market (short durations of non-employment), higher turnover is associated with a more positive employment elasticity. The same is true when turnover is decreased in a market with high durations of non-employment.

[18] We have estimated this model using a 3×3 classification of markets by turnover and non-employment durations. The qualitative results are the same, but harder to digest. We focus on the 2×2 case for ease of presentation.

We conclude that average non-employment durations is a cleaner proxy for market "tightness". However, turnover also plays a role, albeit a smaller one, in mediating the effect of the minimum wage, even within markets with the same average non-employment duration. In unreported results, we also find that the largest decreases in turnover occur in markets with high non-employment durations (the most slack markets).

Variation in Speed of Adjustment

Our results so far indicate that in markets where workers are more likely to enter non-employment, and to stay there longer, increases in the minimum wage have a positive effect on aggregate employment and earnings. However, 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 assess this empirically, we estimate an extension of Equation (6) that incorporates seven quarterly lags of the minimum wage. Figure 4 displays the estimated long-run effects. Each line presents the long-run period-specific elasticity up to two years out from the minimum wage change for a given decile of average non-employment duration. In the upper deciles, the long-run effects are essentially identical to the results from the benchmark specification (positive employment effects) and the elasticities do not appear to trend up or down with each additional lag of the minimum wage. However, in the lower deciles – tighter labor markets – there is some evidence that employment declines take time to materialize. In particular, at the fourth decile, the very strong negative employment effect we see in Figure 3b and Table 7 takes a year and a half to emerge. Overall, these results suggest that speed of adjustment is not driving our results.

These results also help resolve an apparent puzzle. In Section 4, we found that there is no affect of the minimum wage on job reallocation. However, the results of Table 6 and 7 seem to imply a reallocation from low to high decile markets. This could occur in the long-run through differential rates of job creation across markets. If so, we might expect to see a slow adjustment as different markets contract and expand in response to the minimum wage. Figure 4 suggests this is not the case. Whatever reallocation occurs happens quickly, which is plausible given our analysis uses quarterly data and we are focusing on teenage workers.

Variation in Bindingness of the Minimum Wage

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, our main result might be an artifact of the extent to which the minimum wage binds across state-industry pairs rather than variation in market tightness.

Table 10 provides evidence based from the CPS microdata that the minimum wage is binding for teenagers across all state-industry markets. For each worker, i, we measure the gap between the log of his/her reported wage and the log of the minimum wage in his/her state of residence $(s(i))$ in the month of collection $(t(i))$:

$$GAP_i = \ln(wage_i) - \ln(MW_{s(i),t(i)}). \tag{9}$$

For each industry k, we record GAP^*_k – the gap such that 20 percent of workers in k have $GAP_i \leq GAP^*_k$. The first two columns of Table 10 show the distribution of GAP^*_k across 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, which is the unit of analysis in our empirical model. For completeness, the fifth and sixth columns also 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, 20 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, 20 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. 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 for that elasticity to be consistent. Based on the evidence in the table, this will be the case in most of the markets in our data.[19]

[19]The raw number of teenagers sampled in the CPS each month is not sufficient to produce a table analogous to Table 3 across state-industry pairs (nor across either of these dimensions in isolation) that

6 Conclusions

We find that the effect of the minimum wage on employment varies considerably across markets with different levels of labor market tightness. Our proxies for labor market tightness come from a new data set – the Census Bureau's Quarterly Workforce Indicators (QWI) that provides a host of information regarding labor market dynamics. The employment effects of an increase in the minimum wage are strong and negative in markets with low turnover and in markets with short average non-employment durations for workers who exit their jobs. Conversely, increases in the minimum wage are associated with increases in employment in high turnover markets and in markets with high non-employment durations. These results are robust to various specifications and are not driven by variation in the speed of response to minimum wage increases. A key implication is that employment effects of the minimum wage are obscured in empirical models that pool or aggregate data over labor markets with differing levels of worker reallocation and non-employment durations.

Our results are consistent with the view that shorter average non-employment durations 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 increased minimum wages may increase employment. Our results disaggregating the data by average non-employment duration conform to these stylized predictions. The conclusion remains across markets with both high and low levels of turnover.

Markets with low turnover, holding the duration of non-employment constant, have stronger disemployment effects from a minimum wage increase than do markets with high turnover. This result could be consistent with a model in which turnover is a cost channel for firms. If turnover is very costly, then by reducing it, minimum wage hikes can "pay for themselves".[20] Alternatively, in high turnover markets, employers have more scope to adjust the equilibrium tradeoff between wages and job stability when the minimum wage rises.

We also provide a comprehensive summary of the effects of the minimum wage on employment, earnings, worker flows, and job flows. Increasing the minimum wage reduces the aggregate level of worker reallocation, but has no impact on the job reallocation rate. Like much of the recent literature, we, too, find no evidence for an aggregate effect of the minimum wage on the level of employment. We also find evidence that job stability increases.

accounts for heterogeneity in the effect of minimum wages on teen wages.

[20]This implication emerges from a simple model of adjustment costs based on Manning (2006).

Our results provide some evidence that increasing the minimum wage reduces the rate of job creation, and increases the average duration of non-employment for workers who leave their jobs, but these findings are sensitive to model specification.

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 how different markets respond to changes in the minimum wage. The primary contribution of this paper, motivated by Martin Baily's observation about the complexity of the relationship between minimum wages and teenage employment, was to document heterogeneity in the effects of the minimum wage across labor markets with different types of labor market friction.

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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.

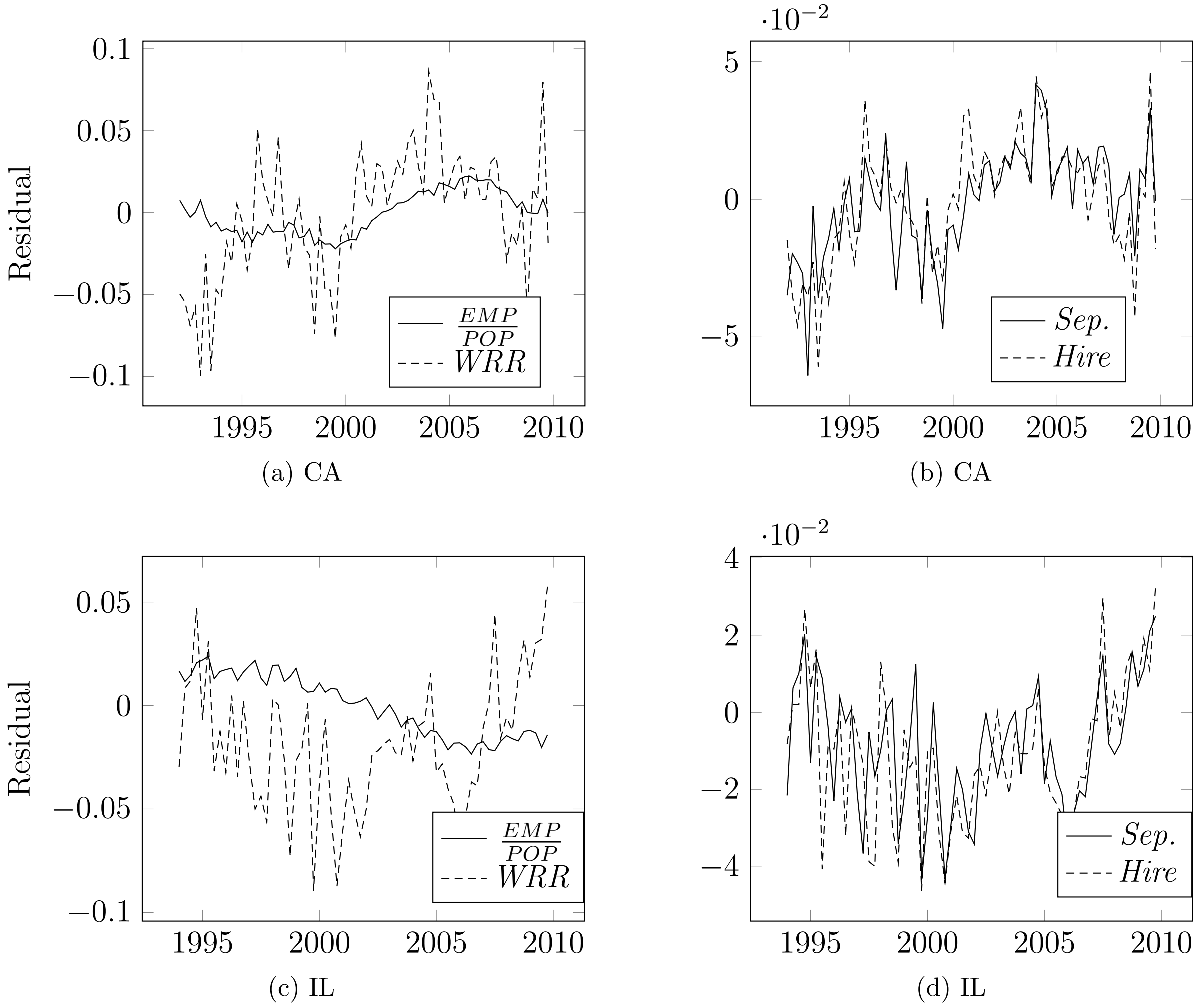
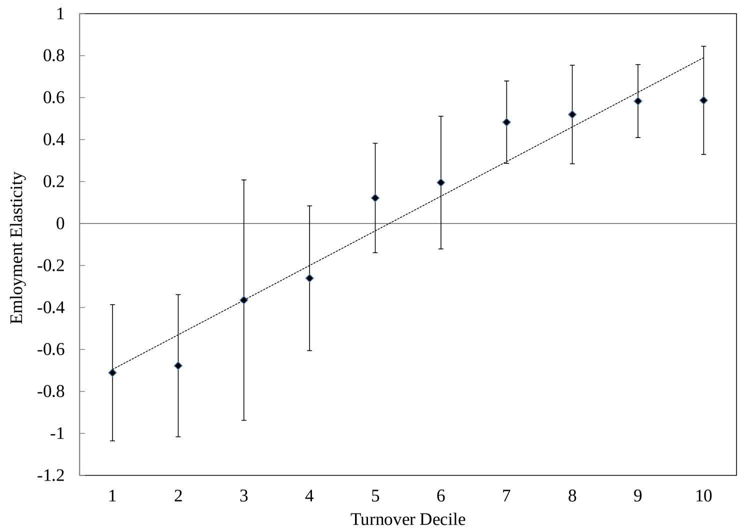
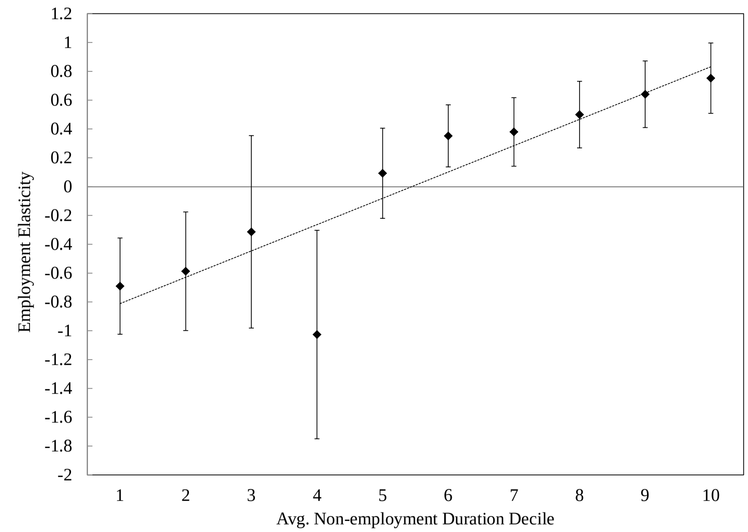


Figure 2: Residual Plots: Employment and Worker Reallocation Rate, and Separation and Hiring Rate for CA and IL



(a) Turnover



(b) Non-Emp. Dur.

Figure 3: Employment Elasticity of the Minimum Wage by Labor Market Tightness

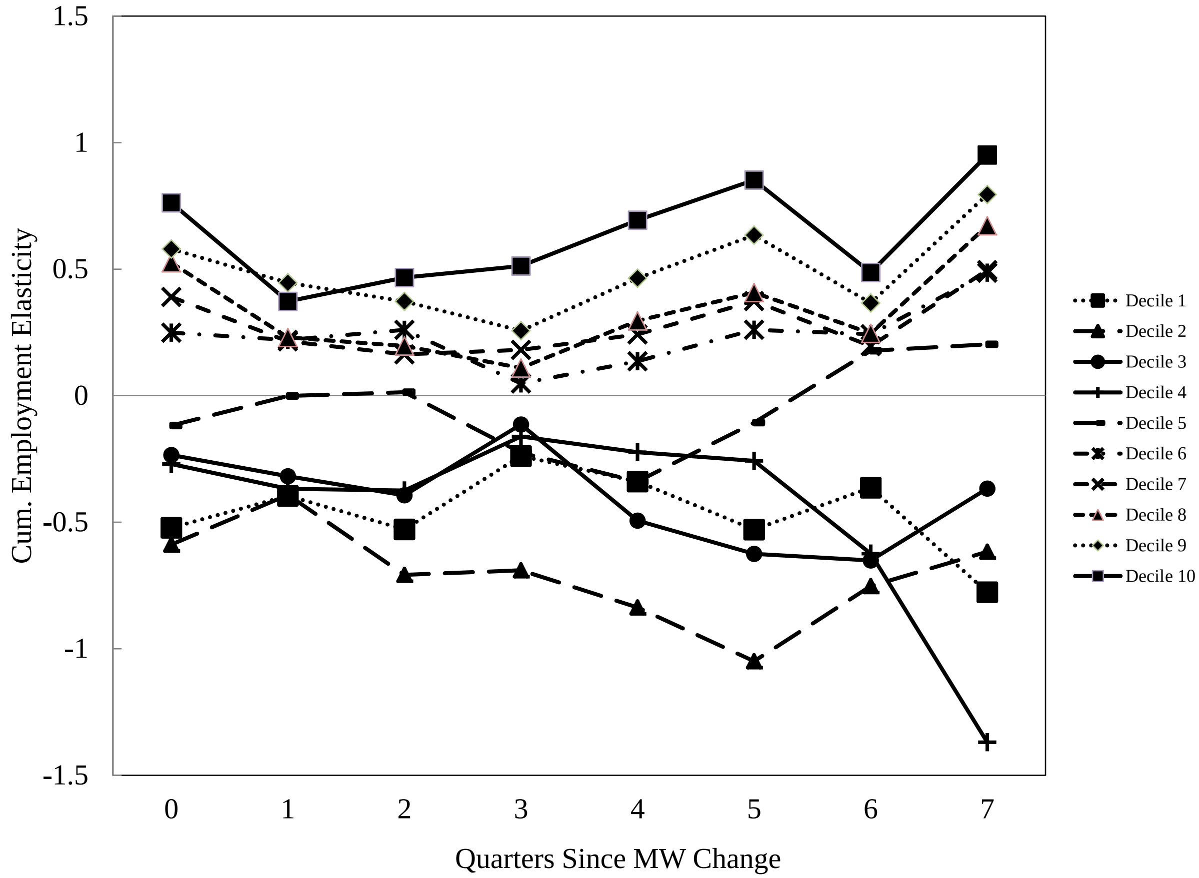


Figure 4: Lagged Employment Elasticity of the Minimum Wage by Decile of the Average Duration of Non-employment for Separations

Table 1: Summary Statistics

Variable	State-Level
QWI: ($EPOP$) Teen end-of-quarter	0.28
employment-population ratio	(.080)
QWI: (lnY) Log avg. end-of-quarter	6.22
teen earnings	(.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	1.81
for all separations	(.219)
QWI: (NH)Teen avg. periods of non-employment	2.63
for new hires	(.222)
QWI: Log avg. end-of-quarter	8.10
adult earnings	(.220)
CPS: Share of teens in	0.09
working-age pop.	(.010)
CPS: Prime-male unemployment rate	0.05
	(.026)
CPS: Teen quarterly	0.38
employment-population ratio	(.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: The Teenage Wage Distribution (CPS)

Period	Variable	Mean	Std. Dev.	Min.	Max.
1990–2010	Mean	6.67	1.578	3.48	37.87
($N = 4,284$)	Decile 1	4.90	1.148	0.35	8.50
	Decile 2	5.40	1.087	0.43	9.00
	Decile 3	5.66	1.133	0.50	9.50
	Decile 4	5.90	1.189	1.00	12.00
	Median	6.18	1.259	3.50	12.00
	Decile 6	6.52	1.350	3.50	12.09
	Decile 7	6.95	1.477	3.85	14.75
	Decile 8	7.63	1.729	4.00	28.12
	Decile 9	8.89	2.254	4.50	28.12
	Count	43.04	28.901	2	213

Population-weighted summaries of the mean and deciles of the within state-quarter distribution of teenage wages estimated from CPS-ORG. 'Count' reports the unweighted number of teenage household members that contribute data for a state-quarter.

Table 3: 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 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.	Stable Hires / Total Hires	Stable Sep. / 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

	(1)	(2)	(3)	(4)
Turnover Decile				
1	-0.700^{***}	-0.703^{***}	-0.703^{***}	-0.737^{***}
	(0.1328)	(0.1441)	(0.1356)	(0.1564)
2	-0.659^{***}	-0.664^{***}	-0.643^{***}	-0.709^{***}
	(0.1551)	(0.1859)	(0.1498)	(0.1623)
3	-0.338	-0.344	-0.332	-0.400
	(0.2377)	(0.2342)	(0.2312)	(0.2804)
4	-0.232	-0.237	-0.199	-0.295^{*}
	(0.1468)	(0.1348)	(0.1457)	(0.1649)
5	0.157	0.152	0.179	0.087
	(0.1352)	(0.1494)	(0.1343)	(0.1265)
6	0.226	0.225	0.224	0.159
	(0.1410)	(0.1627)	(0.1418)	(0.1525)
7	0.525^{***}	0.527^{***}	0.528^{***}	0.447^{***}
	(0.1295)	(0.1079)	(0.1346)	(0.0966)
8	0.565^{***}	0.565^{***}	0.567^{***}	0.484^{***}
	(0.1019)	(0.0836)	(0.1065)	(0.1114)
9	0.627^{***}	0.632^{***}	0.629^{***}	0.547^{***}
	(0.1502)	(0.1312)	(0.1713)	(0.0886)
10	0.626^{***}	0.627^{***}	0.595^{***}	0.552^{***}
	(0.0789)	(0.1116)	(0.0809)	(0.1212)
State-Quarter	*Y*	*Y*	*Y*	*N*
State-Recession	*Y*	*Y*	*Y*	*N*
Region-Period	*Y*	*Y*	*Y*	*N*
Saturated	*N*	*Y*	*N*	*N*
Exclude Distorted	*N*	*N*	*Y*	*N*
Num. Obs.	244,332	244,332	224,244	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. Parenthesized values are robust standard errors clustered at the state level. Column (1) presents the benchmark specifiction. Column (2) presents the fully saturated specification with state-specific unemployment, state-recession, and state-unemployment-recession effects. Column (3) presents the benchmark, but excludes observations that were substantially distorted as part of QWI confidentiality protection (flag=9). Column (4) presents a standard panel model with state controls, period controls, and state-specific trends. (*), (**), or (***) indicate the coefficient is statistically different from zero at the 10, 5, and 2.5 percent level.

Table 7: Estimated Employment Elasticity by Average Duration of Non-Employment for Separating Workers

	(1)	(2)	(3)	(4)
Avg. Duration Decile				
1	-0.690^{***}	-0.679^{***}	-0.785^{***}	-0.713^{***}
	(0.166)	(0.165)	(0.162)	(0.189)
2	-0.587^{***}	-0.582^{***}	-0.603^{***}	-0.643^{***}
	(0.205)	(0.209)	(0.209)	(0.220)
3	-0.314	-0.306	-0.305	-0.391
	(0.332)	(0.340)	(0.322)	(0.375)
4	-1.026^{***}	-1.017^{***}	-0.975^{***}	-1.129^{***}
	(0.360)	(0.369)	(0.336)	(0.357)
5	0.093	0.101	0.095	-0.0001
	(0.156)	(0.148)	(0.150)	(0.205)
6	0.352^{***}	0.362^{***}	0.338^{***}	0.254^{*}
	(0.107)	(0.099)	(0.105)	(0.137)
7	0.379^{***}	0.391^{***}	0.367^{***}	0.281^{***}
	(0.118)	(0.112)	(0.118)	(0.080)
8	0.500^{***}	0.515^{***}	0.482^{***}	0.394^{***}
	(0.115)	(0.123)	(0.122)	(0.072)
9	0.641^{***}	0.655^{***}	0.621^{***}	0.531^{***}
	(0.115)	(0.120)	(0.120)	(0.078)
10	0.752^{***}	0.772	0.729^{***}	0.640^{***}
	(0.121)	(0.131)	(0.124)	(0.079)
State-Quarter	Y	Y	Y	N
State-Recession	Y	Y	Y	N
Region-Period	Y	Y	Y	N
Saturated	Y	Y	N	N
Exclude Distorted	Y	N	Y	N
Num. Obs.	$244,332$	$244,332$	$224,244$	$244,332$

Table entries are elasticities of employment with respect to the minimum wage evaluated at each decile of the distribution of periods of non-employment for separations. The unit of observation is a state-NAICS3-quarter combination. Parenthesized values are robust standard errors clustered at the state level. Column (1) presents the fully saturated specification with state-specific unemployment, state-recession, and state-unemployment-recession effects. Column (2) presents the benchmark, but excludes observations that were substantially distorted as part of QWI confidentiality protection (flag=9). Column (3) presents a standard panel model with state controls, period controls, and state-specific trends.
(*), (**), 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 Across Sub-Markets

Panel A		Panel B	
Turnover Decile		Avg. Dur. Decile	
1	−0.065	1	−0.408***
	(0.070)		(0.071)
2	−0.070	2	−0.239***
	(0.071)		(0.068)
3	−0.062	3	−0.169***
	(0.079)		(0.067)
4	−0.021	4	−0.051
	(0.088)		(0.065)
5	0.048	5	−0.076
	(0.077)		(0.076)
6	0.028	6	0.099
	(0.066)		(0.083)
7	0.017	7	0.219***
	(0.068)		(0.070)
8	0.070	8	0.173***
	(0.072)		(0.067)
9	−0.007	9	0.201***
	(0.072)		(0.073)
10	0.099	10	0.188
	(0.074)		(0.153)
State-Quarter	*Y*		*Y*
State-Recession	*Y*		*Y*
Region-Period	*Y*		*Y*
Saturated	*N*		*N*
Exclude Distorted	*N*		*N*
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 elasticities are estimated under the model in benchmark specification of Equation (6). The unit of observation is a state-NAICS3-quarter combination. 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 9: Two Dimensional Classification of Labor Market Tightness: Employment Elasticities

	(1)	(2)	(3)	(4)
Low WRR/Low NSEP	-1.187^{***}	-1.191^{***}	-1.144^{***}	-1.21^{***}
	(0.176)	(0.178)	(0.174)	(0.193)
Low WRR/High NSEP	0.214^{**}	0.214^{**}	0.204^{*}	0.155^{**}
	(0.101)	(0.097)	(0.105)	(0.077)
High WRR/Low NSEP	0.179	0.179	0.173	0.131
	(0.171)	(0.203)	(0.180)	(0.245)
High WRR/High NSEP	0.538^{***}	0.540^{***}	0.525^{***}	0.468^{***}
	(0.086)	(0.077)	(0.089)	(0.082)
State-Quarter	*Y*	*Y*	*Y*	*N*
State-Recession	*Y*	*Y*	*Y*	*N*
Region-Period	*Y*	*Y*	*Y*	*N*
Saturated	*N*	*Y*	*N*	*N*
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 cross-clasifications of turnover by duration of non-employment for separations. For example, Low WRR/Low NSEP identifies a market where state-industries are in the bottom half of the turnover distibution and the bottom half of the periods of non-employment distribution. Indicators for these markets are interacted with the minimum wage as decribed by Equation (6). The unit of observation is a state-NAICS3-quarter combination. Parenthesized values are robust standard errors clustered at the state level. Column (1) presents the benchmark specifiction. Column (2) presents the fully saturated specification with state-specific unemployment, state-recession, and state-unemployment-recession effects. Column (3) presents the benchmark, but excludes observations that were substantially distorted as part of QWI confidentiality protection (flag=9). Column (4) presents a standard panel model with state controls, period controls, and state-specific trends.
(*), (**), 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 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.

A Appendix

A.1 Graphical Evidence in Support of Identification

Figure 5 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 5a and 5c display the raw (seasonally adjusted) average level of the outcome variable, and Figures 5b and 5d 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 5a and 5c show that minimum wage increases are associated with decreased teenage employment and worker reallocation. In Figure 5b, after introducing basic controls, there is no visible effect for employment. However, Figure 5d 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 5, 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.

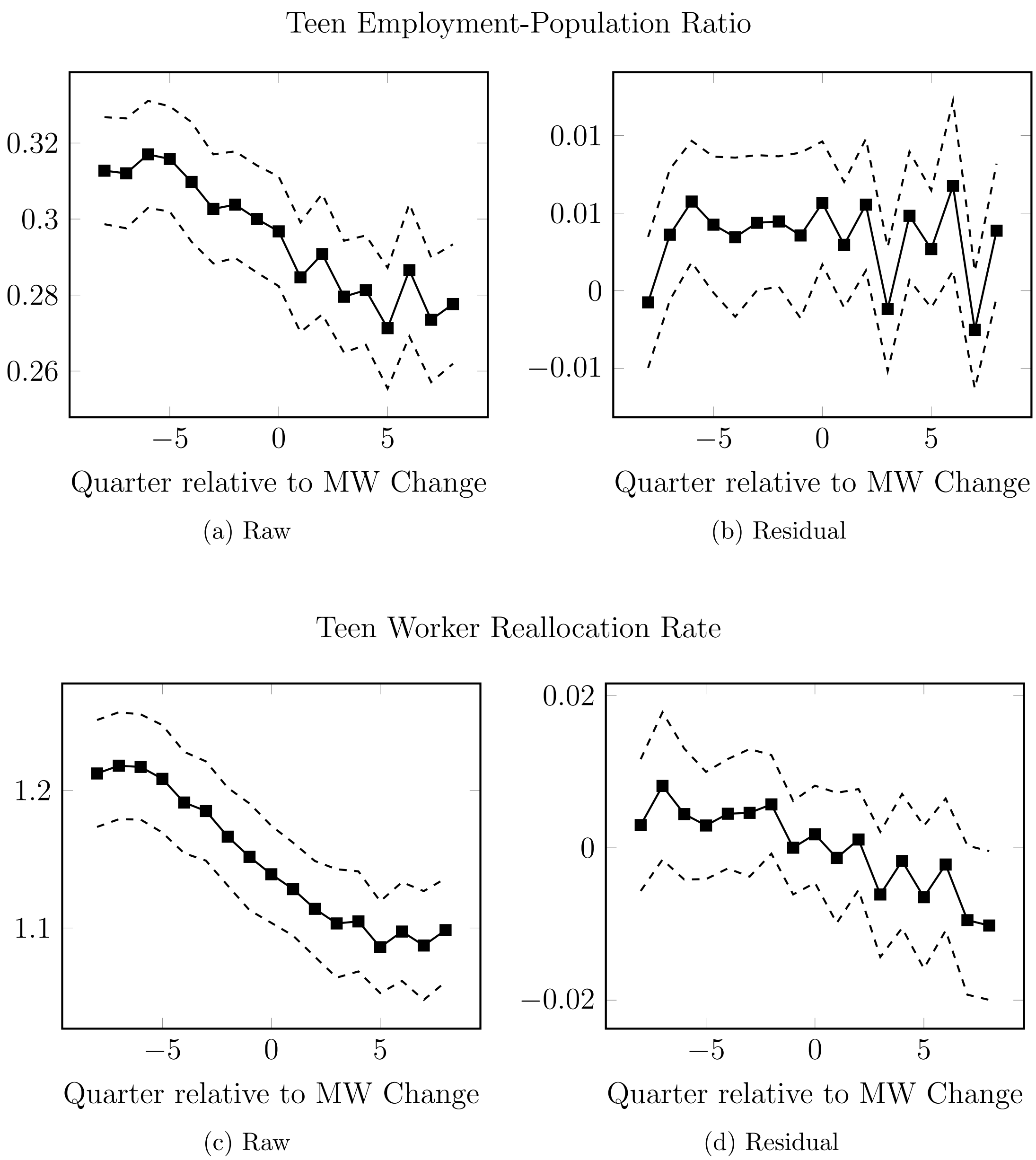


Figure 5: Event Study of Employment and Worker Reallocation Around Minimum Wage Changes. Figures (a) and (c) plot the raw data in each quarter surrounding a minimum wage change, while (b) and (c) plot the residual variation that remains after removing unobserved state and period effects as well as state-specific linear time trends. Dashed lines are 90 percent confidence intervals.