Minimum wage effects on employment, substitution, and the teenage labor supply: Evidence from personnel data

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Abstract: Using personnel data from a large U.S. retail firm, I examine the firm's response to the 1996 federal minimum wage increase. Compulsory increases in average wages had negative but statistically insignificant effects on overall employment. However, increases in the relative wages of teenagers led to significant *increases* in the relative employment of teenagers, especially younger and more affluent teenagers. Further analysis suggests a pattern consistent with non-competitive models. Where the legislation affected mainly the wages of teenagers and so was only moderately binding, it led both to higher teenage labor market participation and to higher absolute employment of teenagers.

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How do minimum wages affect the employment decisions of firms? Standard theory predicts a firm will respond to an increase in the minimum wage not only by cutting overall employment, but also by substituting high-skilled labor for the labor of less-skilled workers whose wages increase the most. The standard model thus predicts negative effects for the very group that is meant to benefit from a minimum wage, namely low-wage workers. Alternative models, however, suggest that so long as the minimum is not set too high, minimum wages can benefit low-wage workers by raising both their wages and their employment levels. These models incorporate a variety of labor market frictions, including sources of monopsony power (e.g., Burdett and Mortensen 1998; Manning 2003), search costs (e.g., Ahn, Arcidiacono and Wessels 2011; Flinn 2006), efficiency wages (Rebitzer and Taylor 1995), and informational asymmetries (Drazen 1986).

Given this theoretical ambiguity, empirical studies are needed that contain enough detail both to analyze overall employment effects at the establishment level and to test for heterogeneous effects on different groups of workers. Prior studies have lacked such detail. On one hand, establishment-level studies have not had enough information on employees to examine compositional changes in employment. This is important because small changes in overall employment can mask significant, but offsetting changes among different groups of workers. On the other hand, studies of specific groups of low-wage workers have relied on household survey data, and have been unable to examine changes within firms in relative wages, overall employment, and the composition of employment.[1]

Using personnel data from a large U.S. retail firm with more than 700 stores nationwide, the present study exploits geographic variation in initial wage levels to estimate the effects of the 1996 federal minimum wage increase. In particular, this study focuses on the differences

[1] See the review of empirical work in the next section.

between teenagers and adults in wage and employment effects. The key advantage of the data is that it allows precise measures of wage and employment changes both for a store's workforce as a whole and for different groups of workers within a store.

The results show the importance of distinguishing between subgroups of low-wage workers, and suggest the standard neoclassical predictions do not hold for teenagers. First, compulsory increases in the average wage had negative, but statistically insignificant effects on the full-time equivalent level of overall employment. This result, while not conclusive, is at least consistent with the standard neoclassical prediction. Second, however, required increases in the relative wage of teenagers led to *increases* in their relative employment. The estimated effect is small—a one percent increase in the teenage relative wage led to a 0.6 to 0.9 percentage point increase in the teenage employment share. But the effect is statistically significant and robust to various model specifications. This finding contradicts the standard prediction that firms respond to minimum wages by substituting away from workers whose wages increase the most.

Separate analyses of absolute employment levels for adults and teenagers help clarify the apparent contradiction in the first two findings. These analyses not only show differential employment effects for adults and teenagers, but also reveal that for teenagers the effect varies across markets. For adults, the effect of a compulsory wage increase is consistently negative. But for teenagers, the effect can be positive or negative depending on where the new minimum wage fell in a market's initial distribution of wages.

In markets with lower initial wages, the new minimum fell relatively high in the distribution and it affected the wages of both teenagers and adults. It thus had a relatively large effect on the store average wage, but only a small effect on the teenage relative wage. In such markets, the effect on teenage employment was zero or negative. And because the employment effect for adults was negative, stores in these markets experienced negative overall employment effects.

In higher-wage markets, the minimum wage was relatively low compared to the overall wage distribution. The minimum wage increase thus mainly affected the wages of teenagers. While a given increase in the teenage wage caused a smaller increase in the store average wage, it produced a larger increase in the teenage relative wage. In these markets, the wage increase had a positive effect on teenage employment. Furthermore, because there was no adult wage increase, there was little effect on adult employment and the overall employment effect was either positive or close to zero.

The conclusion that a moderately binding minimum wage can lead to an increase in employment is consistent with several models that incorporate labor market frictions. While it is hard to distinguish among specific models, additional analysis sheds light on the mechanisms driving the positive teenage employment effects. This analysis suggests the positive effects were driven by increases in the labor market participation of teenagers, especially younger and more affluent teenagers. There is also some evidence that the employment growth was driven by an increase in the quality of teenage applicants.

1. Related Empirical Literature

Most of the related empirical work falls into two groups. First, there are studies of the restaurant industry that employ establishment-level data. In two seminal papers, Katz and Krueger (1992) study the effect of the 1991 federal minimum wage law on restaurants in Texas, and Card and Krueger (CK 1994) study New Jersey's 1992 minimum wage increase. Both use a difference-in-difference methodology and analyze data from surveys conducted before and after the wage increases. Both studies find positive but often insignificant employment effects.

These controversial results prompted several follow-up studies. Neumark and Wascher (NW 2000) and Card and Krueger (CK 2000) both revisit CK's 1994 analysis with new employer-reported data. They reach opposing conclusions. NW find small negative employment effects, while CK find effects that are sometimes positive but small and insignificant. Two recent studies (Dube, Lester, and Reich 2010, and Addison, Blackburn, and Cotti 2008) analyze nation-wide, county-level panels of earnings and employment in the restaurant industry. Controlling in various ways for unobserved geographic heterogeneity in employment trends, these studies find employment effects that are either close to zero, or positive and statistically insignificant.

The second group of studies focuses on teenagers. Teenagers comprise a low-skilled group whose relative wages are likely to be most affected by minimum wages, and for whom standard theory would thus predict negative employment effects. The studies of teenagers, however, have also reached mixed conclusions. Again, studies by Card (1992), Card, Katz, and Krueger (1994) and NW (1992, 1994) are seminal to the debate. These studies all analyze teenage employment-to-population rates using state panels of aggregated CPS data, but they differ in their model specifications and in their methods for measuring certain key variables.[2] As a result, both Card and Card et al. find small and statistically insignificant effects while NW find significant negative effects on teenage employment.

Later studies of teenagers are also mixed. Lang and Kahn (1998) find the 1991 federal minimum wage caused negative employment effects for adults and positive effects for teenagers in food-service occupations. This suggests employment shifts away from adults and *toward* teenagers. Studies that use longer panels find small negative employment effects for teenagers

[2] Two key disagreements between Card/CKK and NW concern: (1) how to measure the impact of the minimum wage increase on the relative price of teenage labor and (2) how to measure school enrollment rates and the appropriateness of controlling for this variable.

that become statistically insignificant after controlling for unobserved heterogeneity in regional trends (NW 2007; Allegretto, Dube and Reich 2011). Finally, a few studies find different effects for different types of teenagers. Using matched CPS data, NW (1996) find a rise in the minimum wage makes it more likely that older teenagers leave school for employment and that younger, employed teenagers become unemployed. Ahn et al. (2011) estimate a structural model and find substitution away from teens who live in poorer, less educated households and toward more affluent teens.

A key weakness in the current literature is the lack of studies examining compositional changes within firms. Whereas the studies of restaurants have not had enough data on employees to examine compositional changes, the studies of teenagers have relied mainly on data from household surveys and hence cannot directly test theories of how firms respond to minimum wages. To my knowledge, only two prior studies have examined the effect of a wage floor on compositional changes in employment within establishments. Portugal and Cardoso (2006) focus on teenagers, though in a different context from the present study. Using a panel of linked employee-employer data from Portugal, they examine the effect of reforms in the mid-1980s that raised the minimum wage for teenagers. They find the rise in the teenage relative wage led to a decline in the teenage share of new hires, but also to a decline in the teenage share of job separations. Hence there was no net change in the teenage share of employment.

Fairris and Bujanda (2008) estimate the effects of the 1997 Los Angeles Living Wage Ordinance on changes in employee characteristics at a sample of city contract establishments. Using worker surveys, they compare incumbent workers to post-ordinance hires, and find evidence of substitution toward groups who had higher pre-ordinance market wages. In contrast to the present study, Fairris and Bujanda do not look at teenagers and are unable to estimate effects on overall employment. Also, their setting is different in that they examine a relatively large minimum wage increase that affected a narrowly defined group of establishments.

2. Data and Setting

The data set is constructed from the personnel records of a national retail employer from February 1, 1996, through July 31, 1998.[3] The analysis sample consists of more than 700 stores located throughout the United States. This sample includes all retail stores that had been open for at least four months before the sample period, and that during the first six months of the period had an average of at least five employees, including at least one adult and one teenager.[4]

Though geographically diverse, these stores are all part of a highly uniform chain where 90% of employees occupy the same frontline, entry-level position. This study focuses on employment in this entry position. The employees in this job rotate through several tasks that include serving customers and doing support work. These jobs require only basic skills and employees receive little training. As is typical in this sector, employees have high rates of turnover. The median spell is 91 days for the position studied, and roughly 80% of these spells end within a year.

At each store, a single overall store manager is responsible for all personnel decisions. Manager compensation is tied to performance mainly through bonuses based on store sales. To attract and retain qualified employees, managers may adjust wage offers and wages as they see fit. However, they must also manage to a year-end wage budget based partly on market wages in their store's region, and they receive small bonuses for meeting goals for the ratio of payroll to

[3] I have permission to use the data on the condition that I do not disclose information that may allow the firm to be identified (such as the specific industry or exact sample sizes).

[4] Sufficient employment during the first six months is needed to construct reasonably precise measures of the legislation's impact on wages. New stores are excluded because they tend to have few employees on record during the first few months, and because employment growth during these months is highly variable and may depend on unobserved factors. I also exclude the six stores that close during the sample period because they are too few to allow for a meaningful analysis of store closings.

sales.[5] There are 41 company-defined regions with an average of 23 stores per region, and the regression analysis below includes tests for sensitivity to the inclusion of region fixed effects.

The personnel records contain data on every individual employed at a retail store during the sample period. This includes wage, status (full-time vs. part-time), store of employment, date and description of each personnel action, age, race, gender, and residential ZIP code. This data is used to construct daily store-level employment variables, including a "full-time equivalent" measure of employment, the share of employees who work part-time, and the share who are teenagers. Since there is no data on hours worked, full-time equivalent (FTE) employment is calculated by assuming that part-time employees work half as many hours as full-time workers.[6]

Additional store-level variables include the store's size (sq. ft.), its location "type" (indoor mall, open mall, strip, etc.), its ZIP code, city, and state. The data was also merged with 1990 Census-based variables describing the population within a two-mile radius of each store's ZIP code, and with local unemployment rates averaged over the first six months of the sample.[7]

In Table 1, the first column shows sample statistics for the store-level variables for the first six months of the sample. A typical store has about 27 employees. On average, 94% of these employees are part-time, and average FTE employment is 14.7. The workforce is young—more than 40% of employees are teenagers, and more than 80% are less than 30 years old. It is also largely female (77%) and white (72% vs. 11% black and 9% Hispanic).

In the other columns of Table 1, the estimation sample is split at the median of each of the two impact measures used in the analysis—dividing stores into "high-impact" vs. "low-

[5] I do not have access to the formulas used in determining each store's wage budget.

[6] Full-time status required a minimum of 30 hours per week. Part-time employees were required to work a minimum of eight hours per week and a maximum of 29 hours.

[7] Unemployment rates are from the Local Area Unemployment Statistics of the Bureau of Labor Statistics, and are based on metropolitan areas as defined in that data set.

impact groups. High-impact stores (where wages were affected more by the minimum wage increase) were generally smaller and located in less densely populated areas. Prior to the legislation, these high-impact stores had somewhat smaller workforces with slightly larger shares of part-time workers and teenagers.

3. The 1996 Minimum Wage Legislation and Measures of its Impact on Wages

3.1. Legislation

I focus on the federal minimum wage law enacted in August 1996. For employers, the first key date was likely July 10, 1996. While the original bill was passed by the House of Representatives on May 24, its fate remained uncertain until July 10 when the Senate passed an amended version supported by President Clinton. The final bill was passed by the House on Aug. 2, and became law on Aug. 20. The federal law mandated a 21% increase in the minimum wage—from \$4.25 to \$5.15—that was to be implemented in two steps. First, the minimum would rise to \$4.75 on Oct. 1, 1996; next, it would rise to \$5.15 on Sept. 1, 1997.[8]

Several state minimum wage laws are relevant to the analysis, because either they contribute to the geographic variation in initial wage levels or they result in additional wage increases during the sample period.[9] Table 2 shows all the minimum wages that are effective during the sample. States where the state minimum exceeded the federal minimum are grouped into three categories.[10] First, there are states where state law required that the state minimum stay above the federal minimum, and hence required that the state minimum rise in tandem with any federal increases. This category, which accounts for only three percent of the sample stores,

[8] The law also included a "training wage" provision allowing employers to pay a sub-minimum wage of \$4.25 to teenagers during their first 90 days. But payroll records show the firm did not utilize this provision.
[9] Unfortunately, the variation in state minimum wages is relatively small in this sample; hence it is not possible to obtain informative estimates using only this source of variation in the legislation's impact on wages.
[10] In all other states, the effective minimum is the same as the federal minimum, either because the state has no law or because the state minimum does not exceed the federal minimum.

is comprised of Connecticut, Alaska, and Washington, D.C.[11]

Second, there are five states where the state minimum exceeded the federal minimum of $4.25 at the start of the sample, but where there was no independent increase in the state minimum. These stores are labeled "high initial minimum" states; they account for nine percent of the sample, and are comprised of Rhode Island, Iowa, New Jersey, Washington, and Hawaii. In Rhode Island and Iowa, the new federal minimum surpassed the state level and thus became binding in Oct. 1996 when it rose to $4.75. In New Jersey and Washington, the federal law became binding only in Sept. 1997 when it rose to $5.15. And in Hawaii, where the initial minimum was $5.25, the federal minimum was never binding during the sample period.

Finally, five states—California, Delaware, Massachusetts, Oregon, and Vermont—increased their own minimum wages on schedules that differed from the federal legislation.[12] Because of these differences in timing, much of the analysis excludes stores in these states. But all results are checked for robustness to the inclusion of these stores.

3.2. Measures of the Legislation's Impact on Wages

The research design exploits the fact that while the stores belong to the same national firm, there is significant variation across stores in the legislation's impact on entry-level wages. There are two sources of this variation. First, because of the differences in state laws, the increase in the minimum wage varies across states. Second, because of local variation in initial wages, the impact of both the federal and state-legislated increases varies across stores.

Table 3 presents store average hourly wages in the pre-legislation period from Feb. 1,

[11] In Connecticut, the state minimum was kept at only ½ percent above the federal level. The state minimum was kept much higher in Alaska and Washington, D.C. (at $.50 and $1.00 above the federal rate, respectively). However, Alaska and D.C. are unimportant in the analysis because they have very few stores.

[12] In all these states except Vermont, the relevant legislation was passed by the end of 1996—anticipating all the minimum wage increases scheduled for 1997 and 1998. The Vermont law (May 1996) approved minimum wages of $5.00 and $5.15 effective January 1997 and January 1998. The law was amended in April 1997, changing the schedule to that shown in Table 2.

1996 to July 31, 1996.[13] The mean of this variable is $5.75 for the full sample. Average wages were lower ($5.67) in states where the federal minimum wage was the effective minimum throughout the sample period, and highest ($6.01) in the "high initial minimum" states. Average wages are also constructed for three subgroups within each store: part-time employees, teenagers, and adults. Full-time workers earn about 30% more than part-time workers in the same store; hence average wages tend to be slightly lower when the sample is restricted to part-time employees.[14] There is also a significant difference between the wages of teenagers and those of adults (20 years or older). In the pre-legislation period, teenagers earn an average of 90 cents for every dollar earned by adults.

As these statistics suggest, the company as a whole was not very constrained by the minimum wage that prevailed in the first six months of the sample ($4.25 in most cases). In fact, only 3.1% of all teenagers and 1.2% of all adults earned the pre-existing minimum.[15] However, as shown in the second panel of Table 3, about one quarter of a typical store's employees (34.9% of teenagers and 17.2% of adults) earned less than the ultimate new minimum that would be in effect two years later.[16] Moreover, there is also substantial variation across stores in the fraction of employees who earned less than the new minimum—the standard deviation of this variable is

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[13] The data set contains each employee's dates of employment, wages paid, and dates of wage changes, but not hours worked. To calculate the average hourly wage, I assumed that full-time employees worked twice the hours of part-time employees each day, and that hours were distributed evenly across all days that the employee was on the payroll.

[14] I do not report or analyze full-time wages separately because many stores have no full-time employees in the entry-level positions.

[15] A subset of stores was nevertheless quite constrained by the existing minimum—80% of the firm's minimum wage-earners were concentrated in 10% of the stores.

[16] Here and subsequently, the minimum wage at the end of the sample period—i.e. that shown in the last column of Table 2—is referred to as the "new minimum." With the exception of stores in Vermont, California, and Oregon, this ultimate new minimum is the minimum effective as of September 1997—$5.15 in states that are bound by the federal minimum, $5.18 in CT, $5.25 in MA and HI, $5.65 in AK, and $6.15 in DC. For stores in VT, the ultimate minimum is $5.25 (effective Oct. 1, 1997); for stores in CA, it is $5.75 (effective March 1, 1998); and for stores in OR, it is $6.00 (effective Jan. 1, 1998). In states bound by the federal minimum, the "training wage" provision of the federal legislation is ignored because payroll records indicate that the company did not utilize this provision (see fn. 8). Previous literature suggests this company's behavior is common in this regard (e.g., see Card and Krueger 1995, pp.166-68).

19.1%. Hence, the legislation had a substantial impact on those stores where the lowest-paid employees were concentrated.

The bottom panel of Table 3 summarizes the variables used in the analysis to measure the legislation's impact on wages. These variables are constructed using wages paid during the first six months of the sample, and the new minimum effective at the end of the sample period.[17] First, the legislation's impact on the store's average wage is measured using the "store average wage gap." This gap is defined as the average proportional increase necessary to bring all wages up to the ultimate new minimum.[18] To construct this variable, I first define the individual wage gap for employee i in store j ($wage\ gap_{ij}$) as the proportional increase in the employee's wage (w_{ij}) necessary to meet the new minimum in store j's state ($minimum\ wage_j$). That is:

$$wage\ gap_{ij} = (minimum\ wage_j - w_{ij})/w_{ij} \qquad \text{if } w_{ij} < minimum\ wage_j$$

$$= 0 \qquad \text{if } w_{ij} \geq minimum\ wage_j$$

The store average wage gap is then calculated as the average wage gap for wages paid to entry-level employees in a store between Feb. 1 and July 31, 1996.[19] Because wages differ significantly both by age and by full-time vs. part-time status, separate store wage gap measures are also constructed for teenagers, adults, and part-time employees.

The store average wage gap measures the legislation's impact on a store's cost of employing its initial workforce—with two caveats. First, stores may adjust wages by more than necessary for some, so as to partly preserve existing wage differentials. In fact, there could be

[17] See fn. 16 for a detailed definition of the "new minimum." An alternative approach—at least in the case of the federal legislation—would be to construct separate impact measures for the October 1996 increase and the September 1997 increase. I focus on the total increase rather than examining each increase separately because both increases were anticipated as of August 1996. It would therefore be difficult to distinguish responses to the first increase from anticipatory effects of the second.

[18] This measure is similar to that used by Katz and Krueger (1992) and by Card and Krueger (1994).

[19] The average is calculated in a way similar to that described in fn. 13; full-time wages receive twice as much weight as part-time wages, and hours are assumed to be distributed evenly across all days that an employee was on the payroll at a given wage.

two competing "spillover" effects.[20] To the extent that wage spillovers affect other employees in the same store, they would magnify the legislation's impact and cause the wage gap to under-predict wage growth. But at the same time, wage spillovers could also flow from high-impact to low-impact stores, and this would cause the wage gap to under-predict absolute wage growth in low-impact stores and thus to over-predict the differential wage growth in high-impact stores. Second, the law's impact could be reduced if the real value of the minimum were eroded over time by general wage inflation that would have occurred in the absence of the law. These caveats will be important for interpreting the results of the wage growth analysis.

As Table 3 shows, the store average wage gap in a typical store is modest, but there is substantial variation across stores. In the full sample (col. 1), the average wage gap has a mean of 2.6% and a standard deviation of 3.1%. The gap is substantially smaller (0.2%) in states with high initial minimums (col. 3), and is higher (4.1%) in the five states where new increases were not synchronized with the federal law (col. 4). In cols. 5 and 6, the sample is split at the median of the store average wage gap, creating two groups that will be compared in the analysis of employment growth.[21] In the "low-impact" group, the wage gap ranges from zero to 1.0 with a mean of 0.3 and a standard deviation of 0.3. Among "high-impact" stores, the wage gap ranges from 1.0 to 15.9 with a mean of 4.2 and a standard deviation of 3.1.

In addition to geographic variation, the wage gap also varies by type of employee. When based only on part-time workers, the store wage gap tends to be slightly higher. And teenagers have larger wage gaps than adults. In the full sample (col. 1), the store mean wage gap for teenagers is 3.8% and for adults is 1.6%. Among high-impact stores (col. 6), the corresponding

[20] If employees care about relative wages, then stores may wish to preserve internal wage differentials or to maintain a wage that is a markup above the minimum. Alternatively, if firms face upward-sloping labor supply curves and pay a monopsony wage, then an increase in the minimum can induce wage increases at firms higher in the wage distribution because it increases the elasticity of labor supply to these firms.

[21] States with "unsynchronized" minimum wage increases are excluded from this sample.

means are 6.1% for teenagers and 2.7% for adults.

Because the legislation raised teenage wages more than it raised adult wages, it tended to increase the relative wage of teenagers. To measure the legislation's impact on a store's *relative* wage, I construct a "store relative wage gap." This is defined as the proportional change in the relative wage that results from bringing all wages up to the ultimate new minimum. The store relative wage gap is constructed from the store wage gaps for teenagers and adults:[22]

$$\textit{store relative wage gap}_j = \frac{(\textit{store teenage wage gap}_j - \textit{store adult wage gap}_j)}{(1 + \textit{store adult wage gap}_j)}$$

The last row of Table 3 summarizes the relative wage gap. The mean for the full sample (col. 1) is 2.1% and the standard deviation is 2.7%. In the last two columns (7 and 8), the sample is split at the median of the relative wage gap, creating high and low-impact groups that will be used in the analysis of relative teenage employment. Here, the mean relative wage gap is 0.2 for the low-impact group and 3.6 for the high-impact group.

It is worth noting that when stores are grouped by the *relative* wage gap, the low-impact group includes stores at both extremes of the *average* wage gap distribution. Stores where there was a low impact on the relative wage include both those with the smallest average wage gaps (where all initial wages were high, so neither teenagers nor adults were much affected) and those with the largest average wage gaps (where all initial wages were closer to the initial minimum, so adults were affected nearly as much as teenagers). In contrast, the high-impact group consists of stores where the legislation mainly affected teenagers, and thus had a high impact on the relative wage but only a moderate impact on the average wage.

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[22] The relative wage in store *j* prior to the legislation is $\textit{store relative wage}_{j1}$=($\textit{store average teenage wage}_j$)/($\textit{store average adult wage}_j$). After wages are adjusted to comply with the new minimum, the relative wage becomes: $\textit{store relative wage}_{j2}$=($\textit{store average teenage wage}_j$)($1+\textit{store teenage wage gap}_j$)/($\textit{store average adult wage}_j$) ($1+\textit{store adult wage gap}_j$). The $\textit{store relative wage gap}_j$ is then: ($\textit{store relative wage}_{j2}$-$\textit{store relative wage}_{j1}$)/($\textit{store relative wage}_{j1}$), which can be reduced to the above expression.

4. Methods of Analysis

I estimate the legislation's effect on the outcomes of interest by fitting store-level regression equations that take the form:

(1) $\Delta Y_j = \alpha + \beta \cdot X_j + \gamma \cdot wage\ gap_j + \varepsilon_j$

Here ΔY_j is the change in the wage or employment outcome of interest for store j, X_j is a set of characteristics of store j or its location, and $wage\ gap_j$ is either the store average or store relative wage gap. Because the sample period begins six months before the legislation was passed, changes are calculated using the first six months (Feb. 1, 1996-July 31, 1996) and last six months (Feb. 1, 1998-July 31, 1998) of the sample period. I examine the sensitivity of these regression estimates to the inclusion of an increasingly detailed set of control variables, including the initial age distribution of employees (whose wages are used to construct the wage gap variables), region fixed effects (based on regions defined by the company), and the other store characteristics described in Table 1. I also test for robustness to the inclusion of states where state minimum wage increases are not synchronized with the federally mandated increases.

The regression estimates have two key limitations. First, to interpret the wage gap coefficient (γ) as the causal effects of the legislation, one must assume that the wage gap is uncorrelated with unobserved determinants of the trends in wages and employment (ε_j). Second, the regression estimates reveal nothing about the timing of stores' adjustments to the legislation. Both issues are addressed in two complementary graphical analyses. These analyses employ a panel of store-month averages of the wage and employment variables, and they exclude states where the legislation is not synchronized with the federal law.

In the first graphical analysis, the sample is split at the median of the relevant wage gap variable into "high-impact" and "low-impact" stores. Then monthly averages of the wage and employment variables are plotted for each group. These plots show the overall time-series patterns for wages and employment, and allow a crude comparison between high- and low-impact stores.

The second graphical analysis is based on estimates from cross-sectional regressions using store-level wage and employment data for each month of the sample period. For each month t, I estimate equations of the form:

$$(2) \qquad Y_{jt} = \alpha_t + \beta_t \cdot X_j + \gamma_t \cdot wage\ gap_j + \varepsilon_{jt}$$

where Y_{jt} is the level of the wage or employment outcome of interest for store j in month t, $wage\ gap_j$ is either the store average or store relative wage gap, and X_j is the complete set of fixed store-level covariates used to estimate equation (1). A plot of the wage gap coefficients (the estimates of γ_t) serves two functions. First, it provides a way to assess the identifying assumption that the wage gap is uncorrelated with the residual in equation (1). If the identifying assumption is correct, then the estimates of γ_t from equation (2) should show no trend in the months before the legislation was passed. Second, the time series pattern of the coefficients in the months *after* the legislation was passed should reveal the timing of any wage and employment adjustments that stores made in response to the legislation.

5. Main Results

5.1. Changes in Average Wages

The regression estimates of the legislation's effect on store average wages are shown in the first row of Table 4. The coefficient on the wage gap varies only slightly across model specifications and ranges from .75 to .78. These estimates imply that the legislation's impact on wages after two years is highly significant, though it is roughly 25% less than what is implied by the average wage gap at the start of the sample period. The robustness of the wage gap coefficient to the inclusion of numerous control variables suggests these estimates reflect the legislation's causal impact on wages and not heterogeneity in underlying wage trends.

The graphical analyses of average wages are shown in Figs. 1a and 1b. Figure 1a compares the overall time-series pattern of wages for high-impact and low-impact stores. The pattern here supports the causal interpretation of the regression estimates. Both groups of stores show underlying upward trends that are similar. The only places the two groups visibly differ are at the dates of the compulsory increases, where there are discrete jumps that are much larger in the high-impact stores.[23] It should also be noted that the common upward trend suggests some wage growth would have occurred without the law, and this is consistent with the wage gap coefficient of less than one.

Figure 1b, which plots the wage gap coefficients from monthly regressions of store average wages, also supports the causal interpretation of the original regression estimates.[24] First, the trend in the wage gap coefficient is flat during the pre-legislation period, and this indicates pre-existing trends were similar in high and low-impact stores. Second, there are clear jumps in the coefficient at the dates of the compulsory increases. It should also be noted that the coefficients show a slight negative trend in the months after each jump, and again this is consistent with a less-than-proportional effect of the minimum wage gap. It suggests higher-impact stores partly offset the cost of the increases by slowing subsequent wage growth relative to low-impact stores.

5.2. Changes in Full-Time Equivalent Employment

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[23] Figure 1a also shows a small amount of seasonal variation in wages. Wages dip in December and peak in July, and there is a visible increase between June and July of each year. The June-to-July increases are largely due to merit raises, which are made during the last week in June in all stores. For more on merit raises, see fn 28.

[24] The regressions for Fig. 1b control for all the fixed store-level variables used in Table 4, col. 4, including the initial age distribution, region fixed effects, and other store characteristics (see Table 4 notes for more detail). The coefficients on the wage gap are multiplied by .01 so that they measure, at each point in time, the difference in outcome Y_{jt} associated with a one percentage point difference in the wage gap.

The second row of Table 4 shows the regression estimates of the legislation's effect on two-year changes in FTE employment. The estimated effect of a one percentage point increase in the store wage gap on the change in employment ranges from -0.01 to -0.09 and is more negative in specifications with more controls. But in no case is it statistically significant. The corresponding labor demand elasticities are shown in row 3, and are based on the estimated wage increases in row 1 and the sample mean of 14.7 for FTE employment. These estimates range from -0.09 to -0.80.

The fact that the estimated employment effect becomes somewhat more negative as store-level covariates are added as controls (cols. 2-4) suggests that the col. 4 estimates might still be biased toward zero due to remaining unobserved heterogeneity in pre-existing employment trends. However, the graphical analysis below shows no evidence of such bias.[25]

Another potential source of bias is the assumption that part-time employees work half as many hours as full-time employees. Because this assumption is made when constructing both the average wage gap and the full-time equivalent employment variable, an inaccurate weighting of part-time employees would result in measurement error in both variables. Moreover, the measurement error would be correlated with the fraction part-time, creating a spurious correlation between these two variables. To address this issue, the specification shown in col. 5 controls for both the initial fraction of employees that is part-time and for the change in this fraction. The wage gap coefficient in col. 5 lies between those in cols. 3 and 4, and suggests the weight assigned to part-time employees was not an important source of bias.[26,27]

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[25] Additional sensitivity tests show that if employment growth is expressed as a *proportional* change in employment rather than the change in levels, the estimated effect of the minimum wage is small and *positive*, but again statistically insignificant. The discrepancy in sign occurs because higher-impact stores have lower initial levels of employment than lower-impact stores, but similar upward trends in employment levels. As a result, proportional employment growth has a slightly more positive trend in higher-impact stores even before the legislation is passed.

[26] A full analysis of changes in the fraction part-time reveals a small negative correlation between the store wage gap and changes in the fraction part-time. But this correlation is due entirely to the slight growth in part-time

Finally, in col. 6, the estimation sample is expanded to include stores in states with "unsynchronized" minimum wage increases. This has very little effect.

The graphical analyses of employment are shown in Figs. 1c and 1d. Figure 1c compares high and low-impact stores. While high-impact stores have lower initial employment levels, both groups show an upward trend in employment, and it is hard to see any difference between them in employment growth. Instead, what stands out are the similarly large swings in seasonal employment. For example, despite an overall upward trend, employment falls in both groups by about seven full-time equivalents between each December and the next May.

Figure 1d plots the coefficients from regressions of monthly average employment on the wage gap, and here it is easier to discern the relationship between the wage gap and the time-series pattern in employment. Two things are noteworthy. First, the wage gap coefficient is nearly constant throughout the pre-legislation period. This suggests the wage gap is not correlated with pre-existing trends in employment (conditional on the controls in the model), and it allays the concern that the regression estimates in Table 4 are biased due to unobserved geographic heterogeneity. Second, consistent with the regression results, employment growth during the post-legislation period appears to be slightly lower in stores with higher wage gaps.

5.3. Changes in Relative Wages

Because teenagers were typically paid less than adults before the minimum wage law, the law tended to increase the relative wage of teenagers. However, because of variation in both the initial level and the initial distribution of wages, the law's impact on relative wages varied across

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employment in less-impacted stores. In high-impact stores, the fraction part-time is initially high (at roughly 96%) and remains high throughout the sample period. Hence it does not appear the legislation had a significant impact on the ratio of part-time to full-time employees. Complete results are available from the author.

[27] A distinct but related concern is that the store may adjust hours worked by each individual instead of adjusting the number of employees. Unfortunately, like many minimum wage studies, the present study cannot estimate the impact of the minimum wage increase on hours worked.

stores. This section exploits variation in the *store relative wage gap* to estimate the law's effect on the relative wages of teenagers, and the next section estimates the effect on the relative employment of teenagers.

Table 5, row 1, shows the estimates from the regression of two-year changes in the teenage relative wage on the store relative wage gap. In col. 1, the estimate from the regression with no controls suggests that a one percentage point difference in the relative wage gap resulted in roughly a one percentage point difference in the growth of the relative wage. In specifications that control for region dummies and other store characteristics (cols. 3-7), the estimate increases to around 1.3. This suggests the law's impact on relative wages was somewhat larger than that implied by the relative wage gap. Further evidence of this "overshooting" of the relative wage is seen below in section 5.5. The sensitivity of the estimate to the controls also suggests the relative wage gap is correlated with market differences in the time series pattern of relative wages. These differences are investigated in Figs. 2a-2f.

Figures 2a-2c split the sample at the median of the *relative* wage into high- and low-impact stores, and show grouped plots of teenage wages, adult wages, and teenage relative wages. Teenage wages (Fig. 2a) are flat, except for the jumps in October 1996 and September 1997. The average teenage wage is initially about \$.60 lower in the high-impact group, but because of the required increases, the difference between the groups is cut in half by the end of the sample.

Adult wages (Fig. 2b) show upward trends in both groups of stores, but there are two notable differences between the groups. First, initial wages are lower in high-impact stores, so these stores have larger jumps in wages in October 1996 and September 1997 and also have slightly higher overall wage growth. Second, merit raises are given the last week in June, and while there are wage increases in both groups of stores between June and July of each year, these increases are more pronounced in the low-impact stores (especially in 1996). An analysis of merit raises shows the difference between high- and low-impact stores is due to the fact higher-impact stores have higher turnover rates and thus lower rates of eligibility for merit raises.[28]

The relative wage of teenagers (Fig. 2c) exhibits a seasonal pattern for both groups of stores. It peaks in December due to a dip in adult wages (see Fig. 2b), and dips in July after the merit raises that are given mainly to adults. The merit raise effect is especially large in July 1996 and, as anticipated by the pattern in Fig. 2b, it causes a drop in the relative wage in low-impact stores. As a result, relative wages in the two groups converge somewhat even before the minimum wage law. Nevertheless, as the graph makes clear, the law still had a substantial effect on relative wages. Indeed, while the relative wage began substantially lower in the high-impact stores, it ends up higher in this group after October 1997.

Figure 2d plots the coefficients from regressions of the teenage relative wage on the store relative wage gap, controlling for the store-level variables used in Table 5 col. 4. As expected, the June 1996 merit raises lead to a jump in the coefficient. However, it is two other patterns that are important. First, the pattern prior to the law shows no evidence that relative wages were following different trends before the legislation.[29] Moreover, the pattern after the law shows large jumps at the dates of the compulsory increases. In sum, then, the graphical analyses confirm that the minimum wage law had a substantial impact on relative wages and that the relative wage gap is a reasonable proxy for the size of the impact.

[28] Employees must be employed for at least 90 consecutive days to be eligible for a merit raise. The average merit raise is about 2.2%, and approximately 80% of eligible employees receive one. For adults, the average merit raise does not differ significantly between high and low-impact stores. Teenagers in both groups of stores are much less likely than adults to be eligible for a merit raise, they are less likely to receive raises when they are eligible, and they also receive smaller raises on average. Complete results are available from the author.

[29] Recall that the law's enactment could have been anticipated as early as the end of May 1996, and was generally expected by July 10; hence the five months from February-June 1996 are the most relevant for assessing "pre-treatment" trends.

5.4. Changes in the Teenage Share of Employment

Did changes in the relative wages of teenagers lead to changes in the relative employment of teenagers? The estimates in the second row of Table 5 suggest the answer is yes. But contrary to the substitution effect predicted by the standard model, they imply that increases in the relative wages of teenagers led to *increases* in the relative employment of teenagers. The estimated coefficients on the relative wage gap range from 0.62 to 0.93. These effects are small, and imply that a one percentage point increase in the relative wage led to less than a one percentage point increase in the teenage share of employment. However, the estimates are all statistically significant at a one percent level.

The estimated effect varies somewhat across model specifications. It increases with controls for the store's initial age distribution and region dummies (cols. 2 and 3), and declines again with the inclusion of other time-invariant store-level variables (col. 4). Columns 5 and 6 control for changes in percent part-time and percent single female, both of which are correlated with changes in the teenage employment share.[30] These controls have little effect on the estimated coefficient. There is also very little change in the coefficient when the sample is expanded to include stores in all states (col. 7).

Rows 3-7 of Table 5 present a more detailed analysis of changes in the employee age distribution. A comparison of rows 3 and 4 shows that at least two-thirds of the increase in the teenage share of employment is due to an increase in the share of 16-17 year-olds. Rows 5-7 show that the offsetting decline in the adult employment share is driven mainly by young adults 20-22 years old.

Figures 2e and 2f show that the positive relationship between the relative wage gap and

[30] The variable "change in percent single female" is meant to control for compositional changes that may have resulted from the August 1996 welfare reform legislation, which thrust many unemployed single mothers into the labor force. See Blank (2002) for a review of the literature on the effects of welfare reform in the 1990s.

the growth in the teenage share of employment is not driven by differences in pre-existing trends. The grouped analysis (Fig. 2e) shows that initially the teenage share of employment is trending downward in both high- and low-impact stores, and is falling more quickly in the high-impact stores. However, this pattern begins to change several months after the first minimum wage increase. While the teenage employment share in the low-impact stores appears to level off (aside from seasonal swings), it *increases* in the high-impact group.

Figure 2f plots the coefficients from monthly regressions of the teenage employment share on the relative wage gap, controlling for the other variables in Table 5, col. 4. In the months before the first wage increase, the coefficients show some slight seasonal variation but no apparent trend. However, after the first compulsory increase in October 1996, there is a significant seasonal spike (in Dec. 1997), and then an upward climb beginning again in June of 1997. This pattern suggests that while teenage employment tended to increase in all stores during the holiday and summer seasons (recall Fig. 2e), the surges in teenage employment were especially large where the teenage relative wage increased most. Finally, the teenage employment share continued to increase more quickly in higher-impact stores until three months after the second compulsory increase.

5.5. Separate Analyses for Adults and Teenagers of Wage and Employment Levels

The preceding results indicate that minimum wage increases had statistically insignificant effects on a store's overall level of employment, but significant effects on the age composition of employment. This suggests the employment effects in Table 4 mask countervailing effects for adults and teenagers. This section presents separate analyses for adults and teenagers, and uses the adult and teenage wage gaps as the measures of the law's impact. Table 6 shows regression estimates of wage and employment growth, and Figs. 3a-3f are based on monthly wage and employment regressions. The results confirm that compulsory wage increases did have different employment effects for adults and teenagers. But they also suggest the effects for teenagers varied depending on where the minimum wage fell in the distribution of wages.

Both the wage growth regressions (top panel of Table 6) and the graphical analyses of wages (Figs. 3a, 3c and 3e) confirm that the adult and teenage wage gaps predict significant increases in wages for their respective groups. However, whereas a one percentage point increase in the adult wage gap is associated with only a 0.89% increase in the average adult wage (col. 1), a similar increase in the teenage wage gap is associated with a 1.05% increase in the teenage wage (col. 3). For adults, the less-than-proportional effect suggests some erosion of the law's impact over time, and is consistent with the evidence presented above that adult wages followed a steady upward trend in all markets (Fig. 2b). This erosion is confirmed in Fig. 3a, which shows a relative slowing of adult wage growth in high-impact stores in the months after the compulsory increases.

For teenagers, the finding that high-impact stores raised teenage wages by slightly more than that implied by the teenage wage gap is suggestive of some "over-adjustment" of teenage wages and/or spillovers onto higher-wage teenagers in the same store. Such adjustments may have been made to preserve existing wage differentials. Interestingly, results from a regression that includes both the adult and the teenage wage gap suggest that this over-adjustment of teenage wages occurred mainly in stores with small adult wage gaps (col. 4). This is consistent with the "overshooting" of the relative wage in response to a higher relative wage gap, which was seen above in Table 5.[31]

[31] An analysis of teenage wage adjustments made on the dates of the compulsory increases provides some direct evidence on the nature of wage spillovers. First, in a typical store with a teenage wage gap of 3.8%, the average total adjustment to teenage wages on these dates was roughly 5.1%—about 25% larger than necessary. Second, there is also some evidence of between-store spillovers—indeed, low-wage teenagers receive small wage

While both the adult and teenage wage gap predict significant increases in the wages of each group, the employment effects for these two groups are very different. For adults, the effects are consistently negative. First, the estimates in the bottom panel of Table 6, col. 1, show a negative (though statistically insignificant) effect of the adult wage gap on adult employment growth. Fig. 3b supports this finding; starting after the first minimum wage increase, there is a negative relationship between the adult wage gap and adult employment growth. Second, the estimates in Table 6, col. 2, suggest that conditional on the adult wage gap, adult employment declined more in stores where the teenage wage gap was larger. This is consistent with substitution toward teenage employees in markets where the relative wages of teenagers went up.

For teenagers, a compulsory increase in the teenage wage led to a marginally significant increase in the level of teenage employment. This is evident both from the coefficients in Table 6, col. 3, and from the pattern in Fig. 3d. However, the employment effect for teenagers varied significantly across markets, and depended on how much the minimum wage affected adult wages. When the adult wage gap is added to the regression for teenage employment, the coefficient on the adult wage gap is negative and slightly larger in magnitude than the positive coefficient on the teenage wage gap (Table 6, col. 4). Hence in markets where both teenage and adult wages increased equally, the estimates imply a small negative employment effect for teenagers. But in markets where the adult wage did not go up and an increase in the teenage wage meant an increase in the relative teenage wage, there was a significant positive employment effect for teenagers.

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adjustments even in stores with zero wage gaps. For this reason, the between-store variation in wage adjustments due to "spillovers" (i.e. adjustments beyond what is required) is smaller than the variation in compulsory wage adjustments (as measured by the wage gap). Finally, conditional on the teenage wage gap, the spillover effect was larger in stores with smaller adult wage gaps. This last result confirms that differential spillovers help explain the more-than-proportional effect of the relative wage gap on relative wages. Complete results are available from the author.

The effect of controlling for the adult wage gap can also be seen by comparing Figs. 3f and 3d. Both figures plot monthly regression coefficients for the teenage wage gap, but the regressions used for Fig. 3f control for the adult wage gap while those in Fig. 3d do not. Figure 3f shows a much steeper upward trend following the first minimum wage increase.

Finally, Table 6, col. 5 shows the competing effects of adult and teenage wage increases on the total level of employment. Because both adult and teenage employment levels fall as the adult wage gap increases, an increase in the adult wage gap has a significant negative effect on overall employment. But the overall employment effect is less negative where the wage gap was larger for teenagers than for adults, and is positive in markets where only the teenage wage was affected.

6. Why Did Teenage Wage Increases Lead to Increases in Teenage Employment?

Why did increases in teenage wages lead to higher levels of teenage employment, at least in markets where the minimum wage was relatively low? Recent theoretical work based on labor market frictions suggests several possible explanations. This section surveys the recent theory, and presents some further analysis. Though I cannot distinguish between models, the additional findings shed light on the mechanisms driving the positive employment effects.

6.1. Theoretical Explanations for the Positive Teenage Employment Effects

A positive employment effect from a moderately binding minimum wage is consistent with two sets of models that incorporate labor market frictions. Monopsony models show that a wage floor can produce higher employment by compelling employers to move up their labor supply curves. A second set of models predicts that a minimum wage can generate higher labor demand by inducing labor market entry.

Modern models of dynamic monopsony (Burdett and Mortensen 1998; Manning 2003) and monopsonistic competition (Bhaskar and To 1999) show that even if there are many firms in a market, firms can exercise monopsony power and a minimum wage can cause them to employ more labor.[32] These models assume that wage competition between employers is imperfect, either because job-seekers have imperfect information or because they care about workplace location and other distinctive non-wage job attributes. In this context, the increase in firm-level employment may come from two sources. First, firms employ more people who previously were not employed because their reservation wages exceeded the monopsony wage. Second, the employment of some firms may grow at the expense of others. This could happen either because low-wage firms lose fewer workers to higher-wage firms (Burdett and Mortensen 1998), or because some firms may expand as others exit the market (Bhaskar and To 1999).[33]

The second set of models is based on job-search costs and wage-setting constraints. These models agree on two predictions. First, a minimum wage can generate higher labor demand by inducing labor market entry. Second, while total employment may rise as more individuals enter the market, the new job-seekers may crowd out those already in the market and employment of some types of low-wage workers may fall.

Flinn (2006) and Ahn et al. (2011) assume that job search is costly for both employers and job-seekers and that wages are determined through ex-post bargaining. Because it is cheaper to fill vacancies when there are more applicants, a firm's incentive to create new jobs depends on the number of individuals searching for jobs. And people search for jobs only if their expected wages are high enough relative to their reservation wages and search costs. If workers have low bargaining power and thus low expected wages, then a minimum wage could raise the expected

[32] A special issue of the *Journal of Labor Economics* (2010, vol. 28, no. 2) presents empirical evidence of monopsony power in variety of markets. Ashenfelter, Farber and Ransom (2010) summarize the evidence.
[33] In Bhaskar and To's model, a minimum wage may raise firm-level employment without raising aggregate employment.

wage, and thus induce job search and lead to job creation.

Drazen (1986) and Lang and Kahn (1998) assume that workers vary in quality but wages cannot be conditioned on quality, and predict that a minimum wage can increase labor demand by increasing applicant quality. Drazen's model relies on two types of asymmetric information: firms do not observe individual worker quality and potential job seekers do not observe the wages of individual firms. Here high quality workers have higher reservation wages and are drawn into the labor market by a minimum wage. Because this raises average labor quality, it may increase the overall demand for labor. In Lang and Kahn, both firms and workers face search costs, and wages are set to achieve a separating equilibrium for high and low-quality workers. Here a minimum wage can induce high-quality workers to apply for low-wage jobs, and job openings respond to both the greater quality and quantity of applicants.

Any of these models could explain the basic findings of this paper. Each could clearly explain why a minimum wage would lead to higher teenage employment. What's more, each could arguably explain why the teenage employment effect varies across markets and why there are differential effects for teenagers and adults.

First, these models readily account for the finding that the positive employment effects for teenagers are limited to markets where the wage floor was only moderately binding. They all predict that while a moderate minimum wage may increase employment, a wage floor will always reduce employment if it is set too high.

Second, these models are arguably consistent with the finding of positive effects for teenagers but not for adults. This is because the labor market frictions they rest on are likely to be more severe for teenagers. First, firms are likely to have greater monopsony power over teenagers. Teenagers may face greater scheduling and transportation constraints than adults, and so may care more about non-wage aspects of their jobs.[34] And teenagers tend to lack employment experience, and so may have less information about alternative jobs. Second, frictions that inhibit labor market entry are more likely to apply to teenagers. For example, because of limited employment experience, teenagers may have lower bargaining power and less information about the job market. Thus they may have lower expected wages or be unaware of the wages being offered.

Unfortunately, I cannot directly test specific models with the available data. However, further analysis suggested by these theories sheds light on the mechanisms that are driving my findings. I look at three questions. First, was the firm's increase in teenage employment driven mainly by new labor market entrants? Second, did newly hired teenagers displace other groups of teenagers who were willing to work at the previous wage? And third, were newly hired teenagers more productive than those previously employed by the firm?

6.2. Changes in Employment Flows: Hiring vs. Retention Rates

One way to assess whether the teenage employment effect was driven by labor market entry is to analyze employment flows. A store could increase its teenage employment rate either by hiring more teenagers, or by reducing the exit rate of those already employed.[35] But only the former is consistent with a role for labor market entry. The results below suggest the minimum wage did indeed cause more teenagers to enter the labor market, though it may also have reduced the rate that they leave the market to return to school.

Table 7 analyzes three outcomes: the teenage share of new hires, the teenage separation

[34] Manning (2003, Ch. 7, and 1996) makes a similar argument regarding women vs. men, and presents evidence that monopsony power vis-à-vis women can explain the lack of negative employment effects for women following Britain's Equal Pay Act. The firm in our setting might also derive monopsony power vis-à-vis teenagers from the popularity of the firm's product.

[35] Portugal and Cardoso (2006) emphasize the importance of distinguishing between types of worker flow. They find negative effects of a minimum wage both on hirings and on separations, resulting in net employment effects that are close to zero. Their findings suggest that if firms are constrained by large costs of hiring and firing, then short-run increases in teenage employment could be driven entirely by reductions in teenage quit rates.

rate, and the rate at which teenagers end employment to return to school.[36] First, the minimum wage had a positive effect on the teenage share of new hires (row 1), and this supports a role for labor market entry. The estimates are all positive, and are nearly as large as (though less precise than) the effects on teen employment shares from Table 5. Second, the separation estimates are all positive and statistically insignificant (row 2), and so provide no evidence that the increase in teenage employment was driven by a fall in overall turnover rates. But third, if attention is restricted to exits for the purpose of returning to school, the estimated effect is negative and marginally significant (row 3).

6.3. Changes in the Socio-Economic Composition of Teenage Employees

This section analyzes changes in the composition of teenagers to help assess the role of labor market entry in the rise in teenage employment, and to address the question of whether new entrants displaced other groups of teenagers. To examine the former, I ask whether the rise in teenage employment was driven by individuals whose opportunity costs exceeded their benefit of employment at the pre-existing wage. If so, there should be an increase in the share of this group among both teenage employees and teenage new hires.

This analysis requires a variable to serve as a proxy for a teenager's net benefit of employment. Because my data on individuals is limited, I rely on a measure of the socio-economic status of one's household constructed from data on residential ZIP codes. There are good reasons to expect that the socio-economic status of a teenager's household would be correlated with net benefit of employment. Teenagers from more affluent households are likely to have a lower marginal utility of income. And more affluent teens are likely to have higher

[36] The regression models are similar to those that produced the estimates in Table 5. The analysis of new hires excludes rehires (employees with prior company experience). The analysis of school-related exits focuses on the months of August, September, January and February, and examines changes from these months in the 1996-97 school year to the same months one year later. This choice is motivated by the sample's time frame and by Fig. 2F, which shows the permanent increase in teenage employment began in the summer of 1997.

opportunity costs (for example, access to higher quality schools may increase their return to schooling).[37] To construct the SES variable, I merge the residential ZIP codes of employees with 1990 Census data on median household income, and use the Census income variable to rank the ZIP codes of all employees in a store.[38] I then define as "high-income" those whose ZIPs are in the highest quartile and as "low-income" those in the lowest quartile.

From Table 8, one can argue that teenagers from high-income ZIP-codes had a lower surplus from being employed. First, col. 1 indicates that starting wages during the pre-legislation period were very similar for all groups of teenagers. The estimates here use the first six months of the sample, and are from regressions of starting wages on indicators for high-income and low-income ZIP codes. The regressions control for store fixed effects and also the individual's age, race, gender, part-time status, month of hire, and previous employment with the company. The coefficients are both very close to zero.[39]

The next two columns show hazard ratios from Cox proportional hazard models predicting the rate that employees exit the firm for job-related reasons (col. 2) or in order to return to school (col. 3). The hazard functions are stratified by store and include as regressors the same employee variables that are in the wage model (col. 1).[40] There are no significant differences by socio-economic status in job-related quit rates, but there are large differences in the rates that teenagers terminate employment to return to school. Those from high-income ZIP

[37] Dustmann, Micklewright and van Soest (2009) present evidence that parental income negatively affects the teenage labor supply through an income effect. Also, previous studies have found that minimum wages induce more positive labor supply responses from more educated and affluent teenagers (NW 1996, Ahn et al. 2011).

[38] The ranking is constructed using all individuals employed at a store at any time during the sample period. The average number of residential ZIP codes per store is 28; the standard deviation is 18.

[39] Similar regressions for adults show a positive association between wages and socio-economic status, which likely reflects the influence of wages on socio-economic status. For teenagers, socio-economic status is unlikely to be affected by their wages since their residence usually depends on the income of their parents.

[40] Stratifying by store controls for all fixed characteristics of the store and its location, while allowing each store to have its own, flexible baseline hazard. The hazards associated with various ways of terminating employment are treated as independent conditional on the covariates in the model.

codes are roughly 14% more likely to return to school than those in middle-income ZIP codes, and 35% more likely than those in low-income ZIP codes.[41] Given that they receive similar wage offers, the fact teenagers from more affluent ZIP codes exit employment for school at much higher rates suggests they do indeed have a relatively high opportunity cost of employment.[42]

The question of whether these "high-income" teenagers were responsible for the positive teenage employment response to the minimum wage is addressed in Table 9 and in Fig. 4. Row 1 of Table 9 shows the estimated effects of the store relative wage gap on the change in the share of "high-income" teenagers among all teenage employees. The estimates are positive and statistically significant, and indicate that the increase in teenage employment did indeed come disproportionately from this high-income group. In row 2, the dependent variable is defined with respect to teenage hires rather than the stock of teenage employment. Though these estimates are less precise, they are again positive and suggest that high-income teenagers made up a disproportionate share of new teenage hires.

Figure 4 presents evidence that supports the causal interpretation of the regression coefficients in row 1. This figure plots the relative wage gap coefficients from monthly regressions of the high-income share of teenage employees. The time-series pattern here looks very similar to the pattern seen in Fig. 2f, where the dependent variable is the teenage share of overall employment. There is no trend during the pre-legislation period, there is a jump during the post-legislation holiday season, and there is a steady increase beginning in June 1997.

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[41] The table reports hazard ratios (exponentiated coefficients); e.g., a hazard ratio of 0.80 for a dummy variable implies that the daily rate of dismissal is 20% lower for the indicated group than for the omitted group.
[42] Given that applicant ZIP codes are observed by the employer and that they appear to be correlated with reservation wages (as well as productivity—see section 6.4 below), it is perhaps a puzzle why the firm does not differentiate wage offers by ZIP code. One possibility is that workers care about their relative wages, especially in comparison to those of same-age coworkers. Card et al. (2011) present evidence that job satisfaction and search activity are both affected by relative pay comparisons between coworkers.

Overall, then, the evidence suggests that teenagers from high-income zip codes had a lower surplus of being employed, and that after the implementation of the minimum wage, the shares of these high-income teenagers rose both among new teenage hires and the total stock of teen employees. The evidence thus supports hypothesis that the increases in teenage employment were driven by labor market entry.

Did the new entrants in the labor market displace those who were willing to work at the old wages? This question is addressed in rows 3-8 of Table 9, which shows estimated effects of the store relative wage gap on changes in the shares of overall employment and hiring for each “type” of teenager (those from low, middle, and high-income ZIP codes). Though the effects on both employment and new hires are largest for the high-income group, the coefficients from the fully specified model (col. 2) are positive for all groups. Hence stores that were forced to raise teenage relative wages subsequently hired and employed more of *all* types of teenagers, and did not displace those from the lower-income groups. Instead, if anyone was displaced by the new teenage hires, it was young adults (ages 20-22). As seen above in Table 5, the employment shares of young adults declined significantly where teenage relative wages increased.[43]

6.4. Changes in the Quality of Teenage Employees

The evidence presented thus far suggests that where the minimum wage raised teenage employment, the employment effect was driven by increased employment of relatively affluent teenagers who were likely to be new labor market entrants. This last section examines whether these “high-income” teenagers were also relatively productive.

[43] Recall that NW (1996) and Ahn et al. (2011) find that minimum wages led to increased employment of relatively high-SES teenagers but also to decreased employment of low-SES teens. However, since they study aggregate employment and not firm employment, my findings need not be viewed as inconsistent with theirs, nor should they be interpreted as evidence against the “search cost” theories. For example, one reason why high-income teenagers may appear to have displaced young adult employees rather that low-income teenagers in my setting could be that the minimum wage made other (previously lower-paying) jobs relatively attractive to young, college-age adults and this reduced the supply of young adult labor to the studied firm.

At the individual level, productivity can be measured by the probability of being fired.[44] Column 4 of Table 8 shows the results from a model similar to those in cols. 2 and 3, except that the dependent variable is the hazard rate of being fired. Teenage employees from high-income ZIP codes are 27% less likely to be fired than those from middle-income ZIPs, and 49% less likely than those from low-income ZIPs. What's more, results from a pooled regression (not shown) indicate that while the probability of being fired generally declines with age, teenagers from high-income ZIP codes are significantly *less* likely to be fired than a 20-22 year-old, and are only slightly more likely to be fired than adults on average. By this measure at least, high-income teenagers are significantly more productive than other teenagers and are roughly as productive as adults.[45]

Table 10 examines whether the minimum wage led to higher average worker productivity using store-level data on monthly sales and on yearly shrinkage. The coefficients in the top panel are from regressions in which the dependent variable is the change in the log of store sales between the first six months (Feb.-July 1996) and the last six months (Feb.-July 1998) of the sample period. Though there is a positive association between sales growth and the adult wage gap, there is no evidence that sales grew faster in stores where only teenage wages were affected.

In the bottom panel, the dependent variable is the change in the yearly shrinkage rate between the fiscal year ending in Feb. 1996 and the fiscal year ending in Feb. 1998.[46] Here there are negative coefficients for the teenage wage gap that become slightly larger in specifications

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[44] An employee is identified as being fired if their termination is labeled as "involuntary" and the reason given is dishonesty, substandard performance, tardiness, absenteeism, or violation of company policies. Fires comprise roughly seven percent of the observed employment terminations in the data.

[45] Autor and Scarborough (2008) analyze data from a different U.S. service sector firm with a young, low-wage workforce and also find evidence that employees from high-income ZIP codes are more productive. Specifically, these employees score higher on a screening test that, in turn, predicts various measures of productivity.

[46] In retail, "shrinkage" refers to inventory loss due to shoplifting, employee theft, errors, etc. The average yearly shrinkage rate in the sample is -0.011 (standard deviation 0.006). Unfortunately, the data does not contain shrinkage information for the full fiscal year following the second wage hike (Feb. 1998-Jan. 1999); hence, these estimates may understate the full effect of the minimum wage.

that control for the adult wage gap. This suggests shrinkage rates tended to fall in stores where mainly teenage wages were affected. Though the coefficients are statistically insignificant, they support the idea that an increase in the minimum wage produced an increase in the quality of teenage employees.

Conclusion

Using detailed personnel data on adults and teenagers in the same job, I examine the effects of a minimum wage increase on both the level and the composition of a firm's employment. While the effects on overall employment are statistically insignificant, these average effects mask significant compositional effects. Moreover, the compositional effects are contrary to those predicted by standard theory. Instead of substitution away from teenagers, I find that compulsory increases in the relative wages of teenagers led to *increases* in their relative employment.

These basic findings are clarified by separate analyses of adults and teenagers. First, the average effect of compulsory wage increases on employment is negative for adults, and positive for teenagers. But second, the employment effect for teenagers varies across markets and can be positive or negative. Positive effects occur in markets where the wage floor was only moderately binding, and they appear to be driven by the increased entry of teenagers into the labor market—especially younger and more affluent teenagers.

My results are consistent with several models of imperfect labor market competition, and suggest that labor market frictions can cause a wage floor to have positive employment effects. In the present setting, such frictions may be more important for teenagers than for adults, and the positive effects are more likely in markets where the wage floor is relatively low. The findings underscore the importance of distinguishing both between types of workers and between types of markets. They also suggest that empirical work should move beyond the question of whether wage floors can increase employment, and should strive to identify the sources of variation in their effects.

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TABLE 1. CHARACTERISTICS OF STORES AND STORE LOCATIONS, FEB. '96-JULY '96

	All Stores	Grouped analysis sample, by impact on: average wage Low	average wage High	relative wage Low	relative wage High
Number of entry-level employees	27.3	29.8	25.4	29.0	26.2
	(14.0)	(15.3)	(12.3)	(15.4)	(12.4)
Full-time equivalent (FTE) employment[a]	14.7	16.2	13.4	15.8	13.8
	(7.9)	(8.8)	(6.7)	(8.9)	(6.9)
% Part-time	93.7	92.3	95.3	92.5	95.1
	(6.5)	(6.9)	(5.1)	(7.0)	(5.1)
% Teenagers	41.5	38.9	42.6	39.7	41.7
	(12.9)	(12.1)	(13.2)	(12.5)	(12.9)
% Ages 16-17	16.5	15.5	16.7	15.3	17.0
	(9.0)	(8.8)	(9.0)	(9.0)	(8.8)
% Ages 18-19	23.9	22.1	24.5	23.1	23.4
	(9.7)	(9.2)	(9.2)	(9.4)	(9.2)
% Adults	58.5	61.1	57.4	60.3	58.3
	(12.9)	(12.1)	(13.2)	(12.5)	(12.9)
% Ages 20-22	22.1	21.3	22.0	22.6	20.6
	(10.2)	(9.4)	(10.6)	(9.4)	(10.5)
% Ages 23-29	17.1	17.6	17.1	17.8	16.8
	(9.0)	(9.3)	(9.1)	(9.2)	(9.2)
% Ages 30 & up	19.2	22.3	18.4	19.9	20.9
	(13.1)	(13.0)	(12.8)	(12.8)	(13.3)
% Female	76.9	77.1	78.8	75.9	80.0
	(13.5)	(14.0)	(12.0)	(13.7)	(12.0)
% White	71.9	70.0	78.7	68.7	80.1
	(22.3)	(24.0)	(17.9)	(24.3)	(16.7)
% Black	11.4	12.9	11.2	13.9	10.2
	(12.6)	(13.7)	(12.4)	(14.3)	(11.4)
% Hispanic	9.3	9.9	5.8	10.3	5.4
	(12.8)	(12.9)	(10.8)	(13.6)	(9.6)
Square feet (1,000s)	6.98	7.19	6.80	7.08	6.91
	(3.85)	(3.84)	(3.96)	(3.74)	(4.07)
Population within 2-mi. radius[b] (1,000s)	83.3	104.5	55.4	102.8	57.2
	(88.8)	(126.8)	(31.9)	(127.1)	(33.2)
% Population that is white[b]	79.4	79.1	83.2	79.0	83.3
	(16.7)	(17.1)	(15.2)	(17.0)	(15.3)
% Population that is black[b]	7.6	7.5	8.7	8.5	7.6
	(9.5)	(9.6)	(10.8)	(10.9)	(9.5)
% Population that is Hispanic[b]	5.4	5.7	3.8	5.0	4.6
	(8.9)	(9.7)	(8.6)	(8.6)	(9.9)
Local area unemployment rate[c]	5.1	5.5	4.5	5.3	4.6
	(1.6)	(1.6)	(1.2)	(1.6)	(1.2)
Number of stores	>700	>300	>300	>300	>300

NOTE.—Sample means (standard deviations) based on employment weighted averages from Feb.1st-July 31st, 1996. Grouped analysis sample excludes states where minimum wage changes are not synchronized with federal law (see section 3.1). Impact measures and method for grouping stores are explained in Section 3.2.

[a] FTE employment defined as the number of full-time employees plus ½ × the number of part-time employees.

[b] From 1990 Census; based on 2-mile radius from center of each store's ZIP code.

[c] Based on monthly unemployment rates for metropolitan areas from the U.S. Bureau of Labor Statistics.

TABLE 2. FEDERAL AND STATE MINIMUM WAGES DURING THE SAMPLE PERIOD

	Feb. '96		Oct. '96		Sep. '97	
Federal law	4.25		4.75		5.15	
Synchronized state laws						
CT	4.27		4.77		5.18	
AK	4.75		5.25		5.65	
DC	5.25		5.75		6.15	
High initial state minimums						
RI	4.45		4.75		5.15	
IA	4.65		4.75		5.15	
WA	4.90		4.90		5.15	
NJ	5.05		5.05		5.15	
HI	5.25		5.25		5.25	
Unsynchronized state laws						
CA	4.25		4.75	5.00[b]	5.15	5.75[d]
DE	4.25	4.65[a]	4.75	5.00[c]	5.15	
MA	4.75		4.75	5.25[c]	5.25	
OR	4.75		4.75	5.50[c]	5.50	6.00[e]
VT	4.75		4.75	5.00[c]	5.15	5.25[f]

SOURCE.—Bureau of Labor Statistics, *Monthly Labor Review*, January Issues, 1997, 1998, 1999.
NOTE.—Wages are in nominal dollars.
[a] Effective as of April 1, 1996.
[b] Effective as of March 1, 1997.
[c] Effective as of January 1, 1997.
[d] Effective as of March 1, 1998.
[e] Effective as of January 1, 1998.
[f] Effective as of October 1, 1997.

TABLE 3. STORE AVERAGE INITIAL WAGES AND MEASURES OF THE LAWS' IMPACT ON WAGES

	All Stores	Federal Min.	HI,IA, NJ,RI, &WA	CA,DE, MA,OR, &VT	Grouped analysis sample, by: average wage gap Low	average wage gap High	relative wage gap Low	relative wage gap High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Store average wages ($):								
Average wage	5.75	5.67	6.01	5.88	6.08	5.36	5.97	5.46
	(0.51)	(0.51)	(0.40)	(0.46)	(0.38)	(0.35)	(0.48)	(0.41)
Average P/T wage	5.64	5.56	5.90	5.75	5.94	5.28	5.85	5.37
	(0.45)	(0.45)	(0.38)	(0.37)	(0.33)	(0.31)	(0.42)	(0.36)
Average teenage wage	5.41	5.32	5.67	5.56	5.66	5.07	5.65	5.08
	(0.44)	(0.45)	(0.32)	(0.40)	(0.34)	(0.33)	(0.35)	(0.33)
Average adult wage	6.08	5.97	6.33	6.24	6.44	5.62	6.26	5.81
	(0.68)	(0.67)	(0.55)	(0.68)	(0.57)	(0.51)	(0.71)	(0.56)
Avg. relative teenage wage	0.90	0.90	0.90	0.90	0.88	0.91	0.91	0.88
	(0.08)	(0.08)	(0.07)	(0.09)	(0.07)	(0.08)	(0.08)	(0.07)
Fraction of store wages below new minimum:								
All entry-level (%)	24.5	24.8	6.9	32.6	7.7	37.6	11.8	33.4
	(19.1)	(18.3)	(11.9)	(19.1)	(7.3)	(14.1)	(14.5)	(15.8)
P/T employees (%)	25.8	26.0	7.4	34.9	8.3	39.2	12.4	35.0
	(20.0)	(18.9)	(12.5)	(20.1)	(7.8)	(14.3)	(15.0)	(16.2)
Teenage employees (%)	34.9	36.1	11.2	43.2	15.4	50.8	15.2	51.0
	(24.8)	(23.8)	(17.6)	(23.8)	(15.0)	(19.0)	(15.7)	(18.1)
Adult employees (%)	17.2	17.3	3.7	23.7	3.2	28.2	9.8	21.4
	(18.4)	(18.1)	(8.6)	(19.6)	(5.2)	(17.2)	(16.0)	(17.6)
Measures of law's impact on store wages:								
Store average wage gap (%)	2.6	2.5	0.2	4.1	0.3	4.2	0.9	3.6
	(3.1)	(3.0)	(0.5)	(3.5)	(0.3)	(3.1)	(1.8)	(3.2)
Store P/T wage gap (%)	2.7	2.6	0.3	4.3	0.3	4.3	0.9	3.8
	(3.2)	(3.1)	(0.5)	(3.7)	(0.3)	(3.2)	(1.9)	(3.2)
Store teenage wage gap (%)	3.8	3.7	0.3	5.9	0.6	6.1	1.0	5.7
	(4.3)	(4.1)	(0.6)	(4.9)	(0.6)	(4.0)	(1.8)	(4.2)
Store adult wage gap (%)	1.6	1.6	0.2	2.5	0.1	2.7	0.8	2.0
	(2.5)	(2.5)	(0.5)	(2.8)	(0.2)	(2.9)	(2.0)	(2.6)
Store relative wage gap (%)	2.1	2.1	0.2	3.2	0.5	3.2	0.2	3.6
	(2.7)	(2.5)	(0.4)	(3.4)	(0.6)	(2.8)	(0.6)	(2.4)

NOTE.—Entries are means (and standard deviations) of store-level variables. Each variable is an employment-weighted average based on all of a store's entry-level employees during the pre-legislation period from Feb.-July 31, 1996. Wages are in nominal dollars.

TABLE 4. EFFECTS OF MINIMUM WAGE INCREASE ON AVERAGE WAGE AND FULL-TIME EQUIVALENT EMPLOYMENT

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome:						
1. Change in log of average wage	0.77**	0.78**	0.75**	0.78**	0.78**	0.77**
	(0.05)	(0.05)	(0.07)	(0.08)	(0.08)	(0.07)
2. Change in avg. FTE employment	-1.02	-1.67	-4.03	-9.03	-6.48	-6.68
	(4.70)	(4.93)	(7.12)	(7.51)	(7.68)	(6.91)
3. Implied labor demand elasticity	-0.09	-0.15	-0.37	-0.79	-0.57	-0.59
Controls in model specification:						
Initial age distribution	no	yes	yes	yes	yes	yes
Region fixed effects	no	no	yes	yes	yes	yes
Store & location characteristics	no	no	no	yes	yes	yes
Initial %PT and change in %PT	no	no	no	no	yes	yes
Sample includes CA, DE, MA, OR & VT	no	no	no	no	no	yes
Number of stores	>600	>600	>600	>600	>600	>700

NOTE.—Each entry is the regression coefficient on the store average wage gap in a model of the change in the outcome variable between the first six months (Feb.-July 1996) and the last six months (Feb.-July 1998) of the sample period. Controls for initial age distribution are the fraction in each of the five age categories shown in Table 1. Controls for stores and location characteristics include the store’s square footage; the location type (mall, open mall, street, or strip); the Census-based population variables shown in Table 1; and the average local unemployment rate for Feb.-July ’96. Robust standard errors in parentheses.

** $p<.001$.

TABLE 5. EFFECTS OF LEGISLATION ON THE RELATIVE WAGE AND RELATIVE EMPLOYMENT OF TEENAGERS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome:							
1. Change in relative teenage wage	1.02**	1.04**	1.28**	1.32**	1.31**	1.31**	1.26**
	(0.10)	(0.10)	(0.12)	(0.12)	(0.12)	(0.12)	(0.11)
2. Change in % teenagers	0.62**	0.74**	0.92**	0.79**	0.75**	0.70**	0.70**
	(0.20)	(0.19)	(0.21)	(0.21)	(0.21)	(0.20)	(0.17)
3. Change in % ages 16-17	0.48**	0.59**	0.64**	0.60**	0.59**	0.55**	0.48**
	(0.16)	(0.16)	(0.18)	(0.18)	(0.18)	(0.17)	(0.13)
4. Change in % ages 18-19	0.14	0.14	0.28‡	0.19	0.17	0.15	0.22
	(0.18)	(0.14)	(0.17)	(0.17)	(0.17)	(0.17)	(0.14)
5. Change in % ages 20-22	-0.41**	-0.60**	-0.76**	-0.79**	-0.80**	-0.80**	-0.72**
	(0.16)	(0.12)	(0.15)	(0.16)	(0.16)	(0.16)	(0.14)
6. Change in % ages 23-29	-0.03	-0.03	-0.17	-0.13	-0.11	-0.09	-0.15
	(0.14)	(0.12)	(0.14)	(0.15)	(0.15)	(0.15)	(0.13)
7. Change in % ages 30 & up	-0.19	-0.11	0.01	0.13	0.16	0.19	0.17
	(0.13)	(0.13)	(0.15)	(0.14)	(0.14)	(0.14)	(0.11)
Controls in model specification:							
Initial age distribution	no	yes	yes	yes	yes	yes	yes
Region fixed effects	no	no	yes	yes	yes	yes	yes
Store & location characteristics	no	no	no	yes	yes	yes	yes
Initial %PT and change in %PT	no	no	no	no	yes	yes	yes
Change in % single female	no	no	no	no	no	yes	yes
Sample includes CA, DE, MA, OR & VT	no	no	no	no	no	no	yes
Number of stores	>600	>600	>600	>600	>600	>600	>700

NOTE.—Each entry is the regression coefficient on the store relative wage gap in a model of the change in the outcome variable between the first six months (Feb.-July 1996) and the last six months (Feb.-July 1998) of the sample period. Controls for initial age distribution are the fraction in each of the five age categories shown in Table 1. Controls for stores and location characteristics include the store's square footage; the location type (mall, open mall, street, or strip); the Census-based population variables shown in Table 1; and the average local unemployment rate for Feb.-July '96. Robust standard errors in parentheses.

‡ $p<.10$.

* $p<.05$.

** $p<.01$.

TABLE 6. EFFECTS OF ADULT AND TEENAGE WAGE GAPS ON WAGES AND FULL-TIME EQUIVALENT EMPLOYMENT OF ADULTS AND TEENAGERS

	Change in Adult average wage		Change in Teenage average wage		Change in average wage
	(1)	(2)	(3)	(4)	(5)
Store adult wage gap	0.89**	1.09**		-0.39**	0.39**
	(0.18)	(0.21)		(0.11)	(0.12)
Store teenage wage gap		-0.20	1.05**	1.20**	0.35**
		(0.13)	(0.05)	(0.07)	(0.07)
Number of stores	>600	>600	>600	>600	>600
R-squared	0.23	0.23	0.55	0.55	0.45
	Change in Adult FTE employment		Change in Teenage FTE employment		Change in FTE employment
	(1)	(2)	(3)	(4)	(5)
Store adult wage gap	-8.79	-3.10		-18.88**	-21.96*
	(6.00)	(7.54)		(6.08)	(9.85)
Store teenage wage gap		-5.69	5.66‡	13.43**	7.73
		(5.05)	(2.98)	(3.91)	(6.46)
Number of stores	>600	>600	>600	>600	>600
R-squared	0.20	0.20	0.34	0.35	0.21

NOTE.—Entries are regression coefficients from models of the change in the average wage or employment variable, with changes measured between the first six months (Feb.-July 1996) and the last six months (Feb.-July 1998) of the sample period. All models include controls as in Table 5, column (6). Robust standard errors in parentheses.

‡ $p<.10$.
* $p<.05$.
** $p<.01$.

TABLE 7. EFFECTS OF STORE RELATIVE WAGE GAP ON TEENAGE EMPLOYMENT FLOWS

	(1)	(2)	(3)
Outcome:			
1. Change in % of new hires who are teenagers	0.50	0.99*	0.51
	(0.35)	(0.47)	(0.39)
2. Change in average daily exit rate of teenagers	0.02	0.01	0.04
	(0.03)	(0.02)	(0.02)
3. Change in average daily rate at which teens exit for school	-0.02‡	-0.02‡	-0.00
	(0.01)	(0.01)	(0.01)
Model includes controls as in Table 5 , col. (6)	no	yes	yes
Sample includes CA, DE, MA, OR & VT	no	no	yes
Number of stores	>600	>600	>700

NOTE.—Each entry is the regression coefficient on the store relative wage gap in a model of the change in the outcome variable. In rows (1) and (2) changes are measured between the first six months (Feb.-July 1996) and the last six months (Feb.-July 1998) of the sample period. In row (3) changes are measured between the "back-to-school months" of January, February, August and September during the 1996-97 school year and the same months during the 1997-98 school year. Robust standard errors in parentheses.
‡ p<.10.
* p<.05.

TABLE 8. COMPARISON OF TEENAGERS LIVING IN HIGH-INCOME VS. LOW-INCOME ZIP CODES

		Hazard rate for exit due to:		
	(1) Log of starting wage	(2) job-related quit	(3) return to school	(4) firing
Employee lives in high-income ZIP code	-0.000	0.97	1.14**	0.73**
	(0.002)	(0.04)	(0.05)	(0.08)
Employee lives in low-income ZIP code	0.001	0.99	0.79**	1.23‡
	(0.003)	(0.05)	(0.06)	(0.14)

NOTE.—Column (1) shows coefficients from linear regressions predicting the log of starting wage. Remaining columns show hazard ratios from Cox proportional hazard models for the likelihoods of: quitting for a job-related reason (col. 2); leaving employment to return to school (col. 3); and being fired (col. 4). Estimation sample for all models includes all employees who were hired as teenagers between Feb. 1-July 31, 1996 (N=7,634). All models control for store fixed effects (hazard models are stratified by store) and indicators for the employee's age, race, gender, part-time status, prior company experience, and month of hire. Omitted ZIP code category is the middle two quartiles of median household income. Robust standard errors in parentheses.
‡ p<.10.
* p<.05.
** p<.01.

TABLE 9. ESTIMATED EFFECTS OF STORE RELATIVE WAGE GAP ON EMPLOYMENT AND HIRING SHARES OF TEENAGERS, BY ZIP CODE TYPE

	(1)	(2)	(3)
Outcome:			
1. Change in % of teenage employees who are from high-income ZIPs	0.76**	0.89**	0.58*
	(0.27)	(0.34)	(0.27)
2. Change in % of teenage new hires who are from high-income ZIPs	0.55	0.48	0.35
	(0.47)	(0.61)	(0.69)
3. Change in % of employees who are teens from high-income ZIPs	0.39*	0.47**	0.34**
	(0.16)	(0.16)	(0.13)
4. Change in % of new hires who are teens from high-income ZIPs	0.48	0.56	0.07
	(0.25)	(0.31)	(0.33)
5. Change in % of employees who are teens from middle-income ZIPs	0.06	0.03	0.09
	(0.18)	(0.20)	(0.16)
6. Change in % of new hires who are teens from middle-income ZIPs	-0.11	0.22	0.06
	(0.28)	(0.35)	(0.38)
7. Change in % of employees who are teens from low-income ZIPs	0.15	0.21	0.27*
	(0.10)	(0.14)	(0.13)
8. Change in % of new hires who are teens from low-income ZIPs	0.19	0.25	0.00
	(0.16)	(0.20)	(0.27)
Model includes controls as in Table 5 , col. (6)	no	yes	yes
Sample includes CA, DE, MA, OR & VT	no	no	yes
Number of stores	>600	>600	>700

NOTE.—Each entry is the regression coefficient on the store relative wage gap in a model of the change in the outcome variable between the first six months (Feb.-July 1996) and the last six months (Feb.-July 1998) of the sample period. Robust standard errors in parentheses.

‡ p<.10.

* p<.05.

** p<.01.

TABLE 10. REDUCED-FORM EFFECTS OF WAGE GAPS ON SALES GROWTH AND SHRINKAGE

	Sales Growth			
	(1)	(2)	(3)	(4)
Store adult wage gap	0.566[‡]		0.639	0.911[**]
	(0.296)		(0.390)	(0.349)
Store teenage wage gap		0.194	-0.072	-0.312
		(0.194)	(0.257)	(0.226)
Sample includes CA, DE, MA, OR & VT	no	no	no	yes
Number of stores	>600	>600	>600	>700
R-squared	0.33	0.32	0.33	0.31
	Change in Shrinkage			
	(1)	(2)	(3)	(4)
Store adult wage gap	-0.009		0.005	0.004
	(0.016)		(0.019)	(0.017)
Store teenage wage gap		-0.012	-0.014	-0.014
		(0.012)	(0.015)	(0.012)
Sample includes CA, DE, MA, OR & VT	no	no	no	yes
Number of stores	>600	>600	>600	>700
R-squared	0.11	0.11	0.11	0.11

NOTE.—Sales growth is the change in the log of total sales between the first six months (Feb.-July 1996) and the last six months (Feb.-July 1998) of the sample period. Change in shrinkage is the change in the yearly shrinkage rate between the fiscal year ending in Feb. 1996 and the fiscal year ending in Feb. 1998. All regressions include store-level control variables as in Table 4, column 5, plus a control for the change in full-time equivalent employment. Robust standard errors in parentheses.

[‡] p<.10.
[*] p<.05.
[**] p<.01.

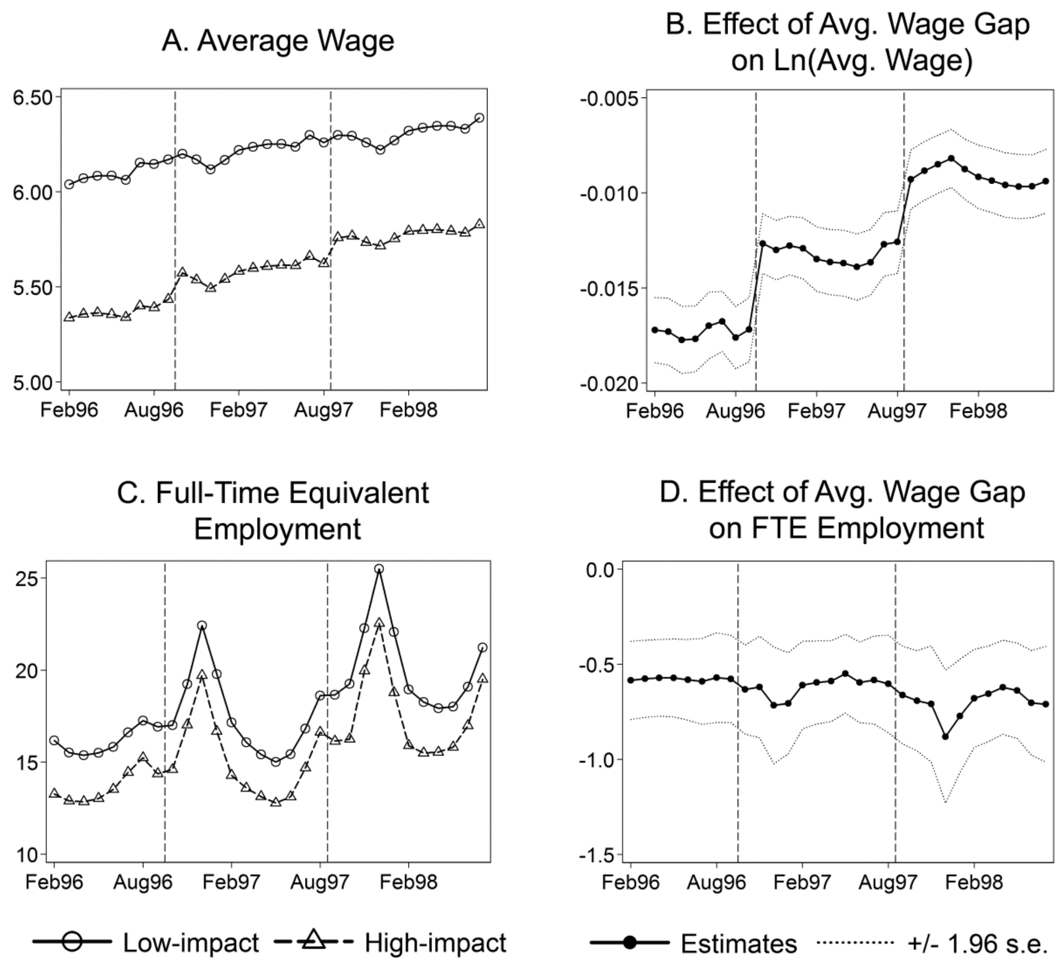

FIG. 1.— Vertical lines indicate dates of federal minimum wage increases (Oct. 1, 1996 and Sept. 1, 1997). Panels A and C plot group means for high- (low-) impact stores, defined as those with average wage gaps above (below) the sample median. Panels B and D plot coefficient estimates from monthly regressions of log average wage (B) or full-time equivalent employment (D) on the store average wage gap (see Equation 2). Regression models include all fixed store-level controls as in Table 4, col. 4. Coefficients are rescaled to measure differences associated with a .01 difference in the store average wage gap.

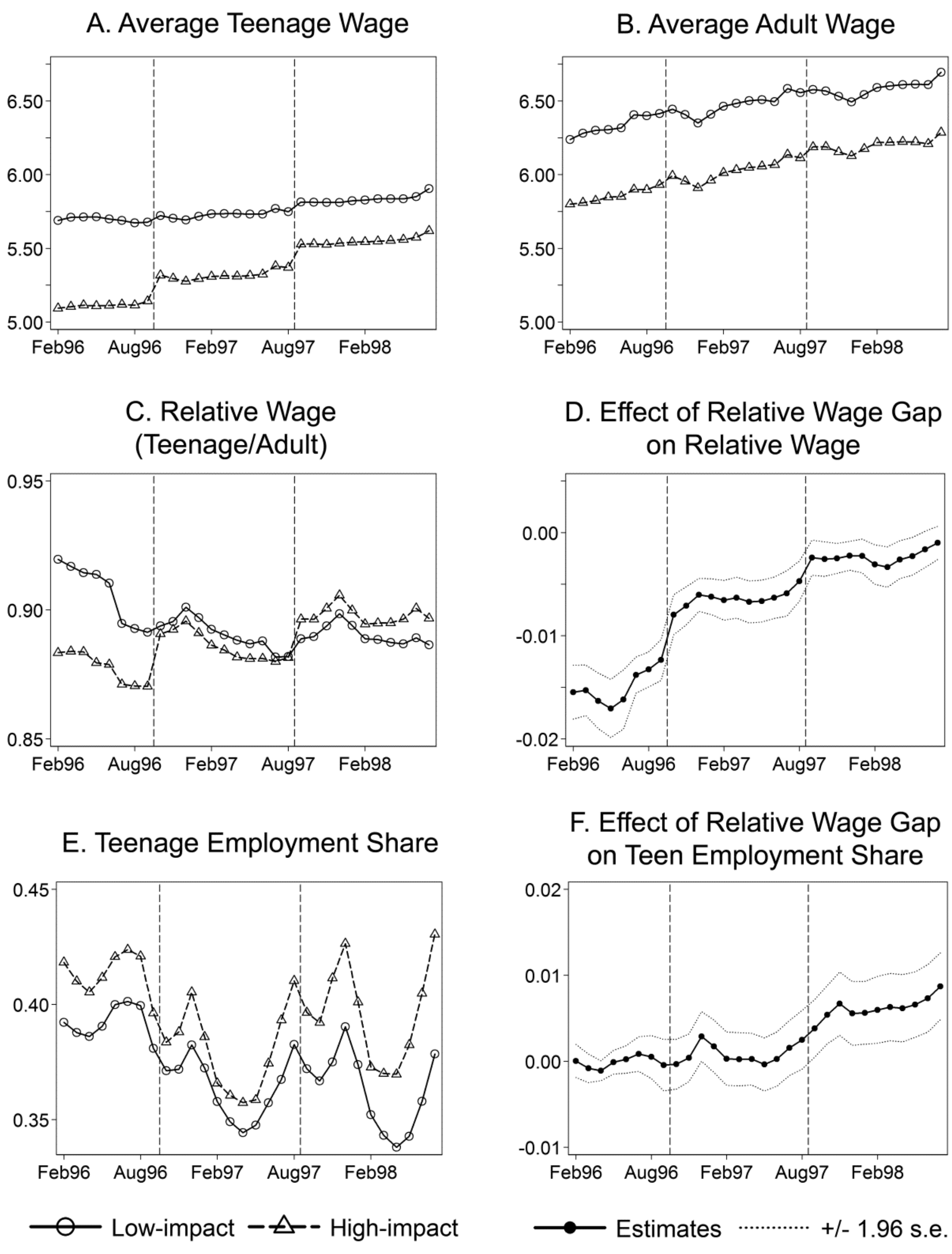

FIG. 2.— Vertical lines indicate dates of federal minimum wage increases (Oct. 1, 1996 and Sept. 1, 1997). Panels A, B, C and E plot group means for high- (low-) impact stores, defined as those with store relative wage gaps above (below) the sample median. Panels D and F plot coefficient estimates from monthly regressions of teenage relative wage (D) or teenage employment share (F) on the store relative wage gap (see Equation 2). Regression models include all fixed store-level controls as in Table 5, col. 4. Coefficients are rescaled to measure differences associated with a .01 difference in the store relative wage gap.

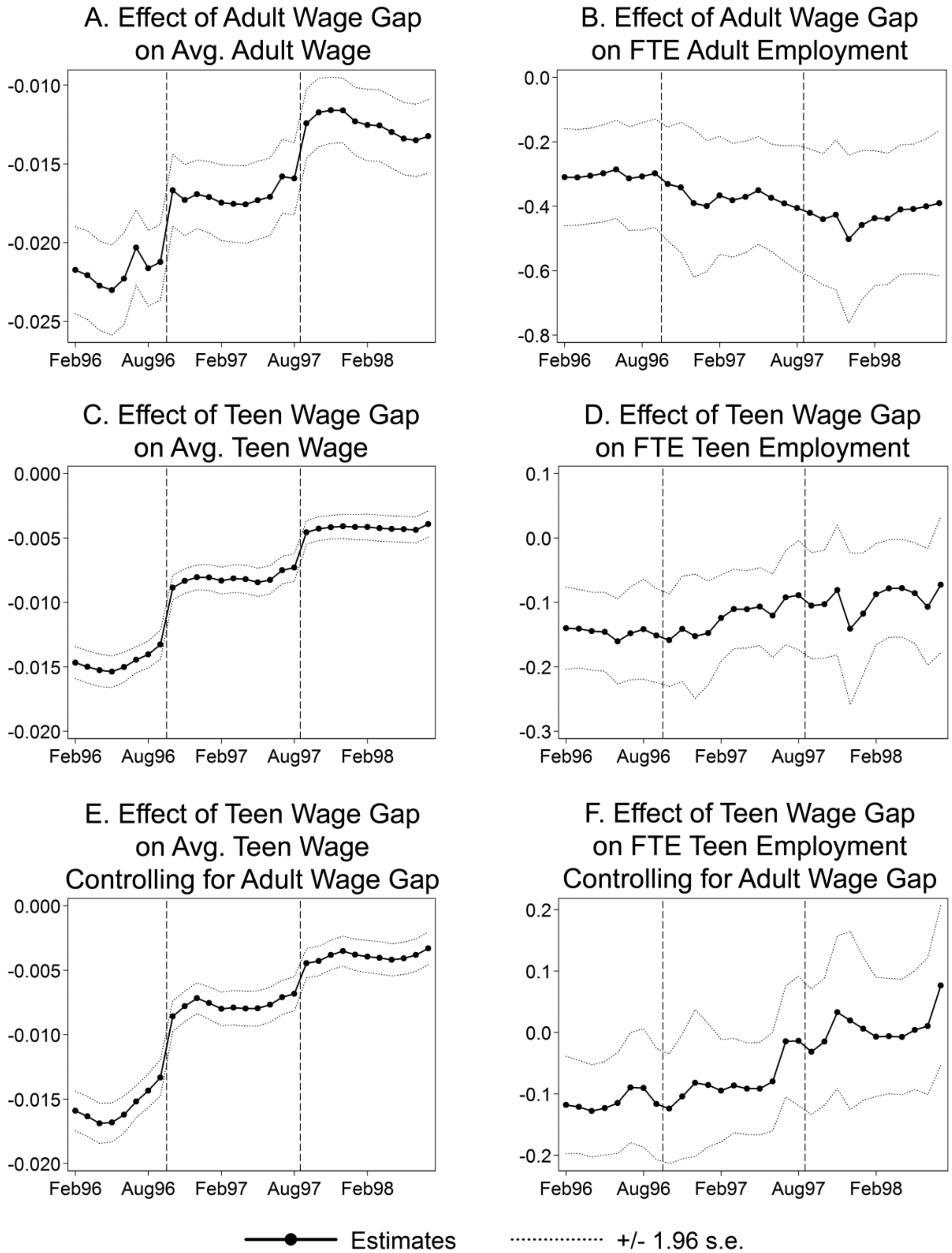

FIG. 3.— Vertical lines indicate dates of federal minimum wage increases (Oct. 1, 1996 and Sept. 1, 1997). Panels A and B plot coefficient estimates from monthly regressions of log average adult wage (A) or FTE adult employment (B) on the adult wage gap. Panels C-F plot estimates from regressions of log average teen wage (C and E) or FTE teenage employment (D and F) on the teen wage gap (see Equation 2). All models include all fixed store-level controls as in Table 4, col. 4; models in E and F also control for the adult wage gap. Coefficients are rescaled to measure differences associated with a .01 difference in the relevant wage gap.

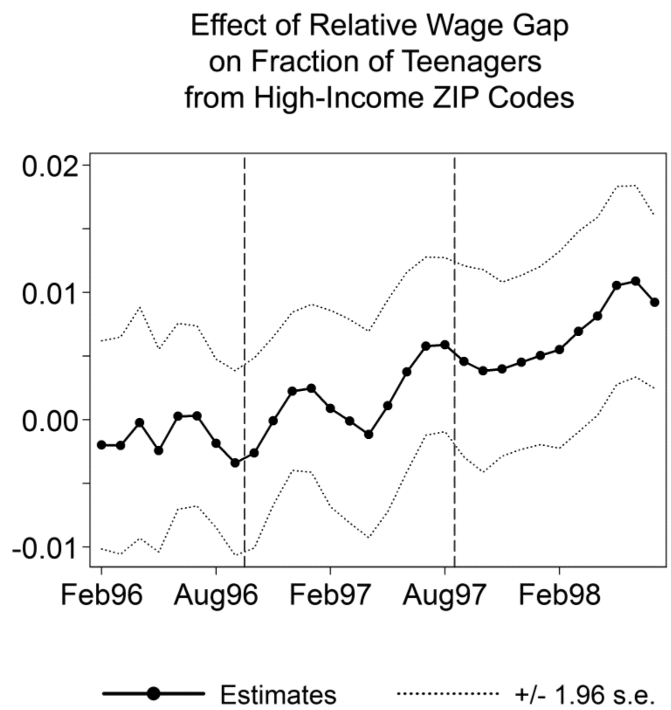

FIG. 4.— Vertical lines indicate dates of federal minimum wage increases (Oct. 1, 1996 and Sept. 1, 1997). Figure plots coefficient estimates from monthly regressions of the fraction of teenage employees who live in high-income ZIP codes on the store relative wage gap (see Equation 2). Regression model includes all fixed store-level controls as in Table 4, col. 4. Coefficients are rescaled to measure differences associated with a .01 difference in the store relative wage gap.