

**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 with more than 700 stores nationwide, this study examines the firm's response to the 1996 federal minimum wage increase. First, increases in average wages had negative, but statistically insignificant effects on overall employment. Second, however, increases in the relative wages of teenagers led to significant *increases* in the relative employment of teenagers, and especially of more productive teenagers from affluent ZIP codes. This second result is consistent with models that link labor demand to labor market participation, and in particular suggests informational asymmetries may be important in the teenage labor market.

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

Abstract: Using personnel data from a large U.S. retail firm with more than 700 stores nationwide, this study examines the firm's response to the 1996 federal minimum wage increase. First, increases in average wages had negative, but statistically insignificant effects on overall employment. Second, however, increases in the relative wages of teenagers led to significant *increases* in the relative employment of teenagers, and especially of more productive teenagers from affluent ZIP codes. This second result is consistent with models that link labor demand to labor market participation, and in particular suggests informational asymmetries may be important in the teenage labor market.

How do minimum wages affect the employment decisions of firms? The theoretical work on this question is ambiguous. Standard neoclassical theory predicts a firm will respond to a rise in the minimum wage in two ways: it will cut overall employment, and it will substitute high-skilled labor for the labor of less-skilled workers whose wages increase the most. While standard theory permits a positive or negative employment effect for high-wage workers, it unequivocally predicts a negative effect for the lowest-wage workers. In contrast, alternative models suggest that if the minimum is not set too high, minimum wages may increase the employment of low-wage workers. These models incorporate a variety of market frictions, including various sources of monopsony power (e.g., see Manning , 2003), search costs (e.g., Ahn, Arcidiacono and Wessles, 2008; Flinn 2006), efficiency wages (Rebitzer and Taylor, 1995), and informational asymmetries (Drazen, 1986).

In light of these theoretical ambiguities, 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 typically 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 (mainly teenagers and sub-groups of teenagers) 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 employment

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

effects of the 1996 federal minimum wage increase. The prime 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. Because teenagers at this firm tend to earn less than adults in the same job, the study focuses on the differences between teenagers and adults in wage and employment effects.

The results show the importance of distinguishing between subgroups of low-wage workers, and suggest that the standard neoclassical predictions do not hold for teenagers in markets where the relative wages of teenagers are initially low. In such markets, the results are more consistent with models that incorporate labor market frictions.

First, legislation-induced increases in average wages had negative, but statistically insignificant effects on the full-time equivalent level of employment. Estimates imply labor demand elasticities ranging from -0.09 to -0.80. While the estimates are imprecise, their sign is consistent with standard neoclassical predictions.

Second, however, in stores where the legislation led to larger increases in the relative wages of teenagers, there was a relative *increase* in the rate at which teenagers were employed, and a relative decline in the employment of young adults (ages 20-22). The estimated effects are small, but are statistically significant and robust to various model specifications. They imply that a one percent increase in the relative wage of teenagers led to 0.6 to 0.9 percentage point increase in the teenage share of employment. This finding contradicts the prediction of neoclassical theory that firms respond to increases in the minimum wage by substituting away from workers whose wages increase the most.

Third, separate analyses of employment for adults and teenagers reveal countervailing effects for these two groups, and help to explain the seeming contradiction in the first two

results. The analysis also suggests a non-monotonic effect on teenage employment. On one hand, legislated wage increases resulted in consistently negative employment effects for adults. On the other, wage increases resulted on average in higher levels of employment for teenagers. Importantly, however, the effect for teenagers varied depending on where the minimum wage fell in the wage distribution.

In low-wage markets, the employment effect for teenagers was close to zero or negative. Here, the pre-existing minimum was closer to the adult wage; consequently, the relative wage of teenagers was initially high, and the minimum wage increase was binding for both teenagers and adults. In higher-wage markets, however, the employment effect for teenagers was positive. Here, the pre-existing minimum was well below the adult wage and the teenage wage was relatively low. Hence the minimum wage increase affected only the teenage wage. This non-monotonic effect on teenage employment caused the legislation's impact on overall employment to vary from negative in low-wage markets to zero or positive in high-wage markets.

The finding of positive employment effects for teenagers cannot be explained by the standard theory. Hence the last section of this paper considers alternate explanations. I focus on two types of models in which market frictions create a link from increases in labor market participation to increases in labor demand. Both models could explain the results of the present study if, in markets where initial wages for teenagers are relatively low, the minimum wage increase induced teenagers to enter the labor market.

First, positive employment effects for teenagers could be explained by a set of models that incorporate search costs for both job-seekers and employers (e.g., Ahn, Arcidiacono and Wessels, 2005; Flinn 2006). In these models, minimum wages induce individuals to enter the labor market and search for jobs. This in turn reduces the cost of filling vacancies for firms and

may induce firms to create more job openings. Second, an alternate explanation is offered by an adverse selection model (Drazen, 1986) in which informational asymmetries prevent employers both from conditioning wage offers on productivity and from attracting higher-quality applicants with higher wage offers. Here a minimum wage may increase labor demand because it induces labor market entry of relatively productive workers, and this raises the average productivity of the applicant pool.

Further analysis provides support for the hypothesis that the minimum wage caused more teenagers to enter the labor market. First, the increase in teenage employment was driven mainly by a group that is likely to have had a relatively small surplus from employment—those living in high-income ZIP codes. The assumption that this group gains less from employment is supported by the fact more affluent teenagers receive starting wages similar to less affluent teens, but are more likely to quit their jobs in order to return to school. Second, an analysis of new hires shows the same pattern of compositional changes that is seen in employment rates. This suggests the compositional changes in employment are driven not simply by differential changes in turnover rates, but by changes in hiring patterns that could reflect changes in the labor pool.

While both the search and adverse selection models assume an increase in labor market participation, a key assumption that distinguishes the latter model is that the new labor market participants are relatively productive. In analyses of dismissals and sales, I find evidence that the more affluent teenagers who are driving the increases in teenage employment are indeed more productive than less affluent teenagers who are paid similar wages. Hence, while acknowledging the potential importance of search costs, I conclude that informational asymmetries are also likely to be a factor in the market for low-wage, teenage labor.

1. Related Empirical Literature

The empirical research falls into two groups. First, there are studies of the restaurant industry that employ establishment-level data. Seminal are the studies of fast-food establishments conducted by Katz and Krueger (1992) and Card and Krueger (CK, 1994). Katz and Krueger (1992) use surveys of restaurants in Texas before and after the 1991 federal minimum wage increase and exploit geographic variation in the law's impact on wages. Card and Krueger (1994) use surveys of restaurants in New Jersey and Pennsylvania before and after New Jersey's 1992 minimum wage increase. They identify the law's effects both by using stores in Pennsylvania as a control group and by exploiting variation in the law's impact among New Jersey stores. 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 different conclusions: while NW finding small negative employment effects, CK find effects that are sometimes positive but small and insignificant.² Two recent studies (Dube, Lester, and Reich, 2008, and Addison, Blackburn, and Cotti, 2008) analyze nation-wide, county-level panels of earnings and employment in the restaurant industry. Using various methods of controlling for unobserved geographic heterogeneity in employment trends, both studies find small negative, but statistically insignificant employment effects.

The second group of studies has focused 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. But studies of teenagers have

² NW (2000) use payroll data they collected and the EPI, and attribute the difference between their own findings and those of CK (1994) to advantages of their payroll data over CK's survey data. CK (2000) reanalyze NW's data and repeat their own analysis using employer-reported data from the Bureau of Labor Statistics (BLS). In both analyses, CK again find no evidence that the wage minimum reduced employment, and they attribute NW's negative estimates to the fact that NW's sample of Pennsylvania stores is not representative.

generally relied on data from household surveys such as the Current Population Survey (CPS), and hence they cannot directly test theories of how firms respond to minimum wages.

Studies of teenagers have also reached mixed conclusions, and again seminal studies by CK (1992, 1994) and NW (1992, 1994) fed opposite sides of the debate. Both sets of authors use state panels of aggregated CPS data and exploit changes in state minimum wages to identify the effects on teenage-to-population employment rates. But they differ in their model specifications and in their methods for measuring certain key variables, including the impact of the minimum wage increase on the relative price of teenage labor.³ As a result, NW find small negative employment effects that are consistent with substitution away from teenagers, but CK find positive or statistically insignificant effects. Results from later studies have also been mixed. Lang and Kahn (1998) find that the 1991 federal minimum wage caused negative employment effects for adults and positive effects for teenagers in food-service occupations, which suggests employment shifts away from adults and *toward* teenagers. Studies that use longer panels find small negative employment effects for teenagers that become statistically insignificant after controlling for unobserved heterogeneity in regional trends (Neumark and Wascher, 2007; Allegretto, Dube and Reich, 2008).

A few studies have found heterogeneous effects among different types of teenagers. Neumark and Wascher's (1996) analysis of matched CPS surveys finds that a rise in the minimum wage increases the probability that older teenagers leave school for employment and that younger, previously employed teenagers become unemployed. Ahn, Arcidiacono and Wessels (2008) estimate a structural model and their findings are similar, suggesting substitution away from teens who live in poorer, less educated households and toward more affluent teens.

³ Another key disagreement between CK and NW is about how to measure school enrollment rates and the appropriateness of controlling for this variable.

To my knowledge, only one study examines the effect of a wage floor on compositional changes in employment within establishments. 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 the characteristics of incumbent workers to those of post-ordinance hires, and find evidence of substitution toward groups who commanded higher pre-ordinance market wages. In contrast to the present study, Fairris and Bujanda do not look explicitly at teenagers (presumably because of the age composition of their sample) and are also unable to estimate effects on overall levels of employment. Their setting is also 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 large national retail employer from February 1, 1996, through July 31, 1998.⁴ 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 at the start of the sample period, and that had an average of at least five employees, including at least one adult and one teenager, during the first six months.⁵

Though geographically diverse, these stores are all part of a national chain; hence they are very similar, and the products they sell are highly uniform. Importantly, roughly 90 percent of employees occupy the same frontline, entry-level positions. This study focuses on employment in these entry-level jobs. These positions all have the same job description: each

⁴ I have permission to use the data on the condition that I do not disclose any information (such as the specific industry or exact sample sizes) that may allow the firm to be identified.

⁵ 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 both 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 many unobserved factors. I also do not analyze the handful of stores that close during the sample period because there are too few of these to allow for a meaningful analysis of store closings.

employee rotates through several tasks that involve both dealing with customers and doing support work. These jobs require only basic skills and employees receive little training. As is common in this sector, employees have high rates of turnover. The median spell in a store for an entry-level employee is 91 days, and roughly 80 percent of employee 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 that are based on store sales. Managers may adjust wages and wage offers as they see necessary to retain and attract qualified employees. However, they must also manage to a year-end wage budget that is based partly on market wages in their store's region, and they receive small bonuses for meeting goals for the ratio of payroll to sales.⁶ 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 information on every individual employed at one of the company's retail stores during the 30-month sample period. This information includes employment status (full-time vs. part-time), wage, age, race, gender, residential ZIP code, store of employment, and the date and description of each personnel action taken. These records are used to construct daily store-level employment variables, including a "full-time equivalent" measure of employment, the fraction of employees who work part-time, and the fraction who are teenagers. Because there is no information on hours worked, "full-time equivalent" employment is calculated under the assumption that part-time employees work half as many hours as full-time employees.⁷

Additional store-level variables include the store's size (sq. ft.), its ZIP code, city and

⁶ I do not have access to the formulas used in determining each store's wage budget.

⁷ 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.

state, and its location “type” (indoor mall, open mall, street, or strip). 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 period (Feb.-July 1996).⁸

Table 1 shows sample statistics for the store-level variables during the first six months of the sample period, including store-level employment and average employee characteristics. During this period, a typical store has about 27 employees. On average, 94 percent of these employees are part-time and average full-time equivalent employment is 14.7. The workforce is young, predominantly female, and largely white. More than 40 percent of employees are teenagers, and more than 80 percent are less than 30 years old. About 77 percent of employees are female, and 72 percent are white (vs. 11 percent black and 9 percent Hispanic).

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

3.1. Legislation

The analysis focuses on the federal minimum wage law that was enacted in August 1996. For employers, the first key date regarding the law was likely July 10, 1996. Though 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 bill that was supported by President Clinton. The final bill was passed by the House on Aug. 2, and became law on Aug. 20.

The law mandated a 21 percent 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. The law also included a “training wage” provision that kept the minimum at \$4.25 for teenagers during their first 90 days on the job.

⁸ 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.

Several state minimum wage laws are also 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. Table 2 shows all of the minimum wages that are effective at some point during the sample period (Feb. 1996-July 1998). States where the state minimum exceeded the federal minimum are grouped into three categories.⁹ First, there are states where existing state law required that the state minimum stay above the federal minimum, and hence mandated that the state minimum rise in tandem with any federal increases. This category accounts for only three percent of the sample stores, and is comprised of Connecticut, Alaska, and Washington, D.C.¹⁰

Second, there are five states where the state minimum exceeded the federal minimum of \$4.25 at the start of the sample period, but where there was no independent increase in the state minimum. Stores in these “high initial minimum” states account for 8.6 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 wage on a schedule that differed from that of the federal

⁹ 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.

¹⁰ 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 in the sample.

legislation.¹¹ Stores in these states account for 20 percent of the sample, with roughly 14 percent being in California alone. Notably, California had the largest total increase of any state during the sample period, with the minimum rising 35 percent from \$4.25 to \$5.75. Because of differences in the timing of wage increases in these states, much of the analysis excludes stores in these states. However, all results are checked for robustness to the inclusion of these stores in the estimation sample.

3.2. Compliance with the Law

The personnel records indicate full compliance with the law at this company. The lowest wage on record rises from \$4.25 to \$4.75 on October 1, 1996, and from \$4.75 to \$5.15 on September 1, 1997. In states with higher minimums, the lowest wages on record are also consistent with the laws. Interestingly, the fact that \$4.75 was the company's lowest wage as of October 1, 1996 indicates that it did not utilize the "training wage" provision of the federal legislation.¹²

An examination of the wage adjustments made on October 1, 1996, and on September 1, 1997, also reveals evidence of "spillover" effects on higher-wage employees. First, employees whose wages were below the new minimum but above the old one received adjustments that were greater than necessary to meet the new minimum. Second, some employees whose wages exceeded the new minimum also received adjustments. In particular, wage adjustments were given on October 1, 1996 to all employees earning less than \$5.45, and adjustments were given on September 1, 1997 to employees earning less than \$5.65. Further, these adjustments were

¹¹ 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.

¹² Card & Krueger (1994), pp. 166-68, provide several sources of evidence showing that employers in the early 1990s rarely utilized the subminimum wage allowed by the 1989 amendment to the Fair Labor Standards Act.

made even in stores that had no employees earning less than the new minimum, including those in states with high initial minimum wages.

3.3. Measures of the Legislation's Impact on Wages

The research design exploits the fact while the stores are all part of the same national firm, there is significant variation across stores in the impact that the legislation has on entry-level wages. There are two basic sources of this variation. First, the impact of both the federal and state-legislated increases varies across stores, because the level of wages paid prior to the legislation varies across stores. Second, the effective increase in the minimum wage varies across states because of the differences in state laws (summarized above).

Table 3 summarizes store average hourly wages in the pre-legislation period from Feb. 1, 1996 to July 31, 1996.¹³ The mean of this variable is \$5.65 for stores in states where the federal minimum wage was the effective minimum in all months of the sample period (column 1). There is substantial variation within this sub-sample (std. dev. = \$0.51). Further, average wages are higher in states with high initial minimums and in California (columns 2-4).

Average wages are also constructed for three subgroups of employees within each store: part-time employees, teenagers, and adults. Full-time workers earn roughly 30 percent 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 (defined here as anyone who is at least 20 years old). In the pre-legislation period, teenagers earn an average of 90 cents for every dollar earned by

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.

adults.

Table 3 also summarizes the variables used to measure the impact that the federal and state legislation had on wages. These variables are constructed using the wages paid in the first six months (Feb. 1-July 31, 1996) and the ultimate minimum wage mandated by the 1996 laws. 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).¹⁵

In the analysis of average entry-level wages and employment, the legislation's impact on average wages is measured using the "store wage gap." This variable is defined as the average proportional increase necessary to bring all wages up to the new minimum.¹⁶ To construct it, I first define the individual wage gap for employee i in store j as the proportional increase in the employee's wage (w_{ij}) necessary to meet the ultimate new minimum in store j 's state (*minimum wage_j*). That is:

¹⁵ An alternate 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. The problem with treating these as two separate increases is that they were both anticipated as of August 1996, and hence stores may have adjusted both wages and employment gradually after October 1996 in anticipation of the second increase. Indeed, the fact that "spillovers" in October 1996 were relatively large suggests that stores got a head start on the second increase. For this reason, my analysis focuses on changes from the period before August 1996 to the period after September 1997.

There are two other issues regarding how to define the effective new minimum wage. One is whether to acknowledge the training wage provision for teenagers employed less than 90 days. I ignore this provision both because the data indicates that the company did not take advantage of it, and because previous literature suggests that this company's behavior is common (e.g. see Card & Krueger, 1994, pp.166-68). The other issue is how to treat spillover effects on higher-wage employees. The fact that the company raised wages of all those earning below \$5.65 might suggest an effective new minimum above \$5.15 for some employees. However, although the company did raise the wages of higher-wage employees who were present when the minimum wage increases took effect, it was not compelled by law to maintain the wage distribution that resulted from this initial spillover effect. For this reason, I do not incorporate observed spillover effects when constructing the measure of the legislation's impact on wages. Hence, with the caveat that I ignore the training wage, the impact measures capture the cost to the company of complying with the law, and nothing more.

¹⁶ This measure is similar to that used by Katz & Krueger (1992) and by Card & Krueger (1994).

$$\begin{aligned}
 \text{wage gap}_{ij} &= (\text{minimum wage}_j - w_{ij}) / w_{ij} && \text{if } w_{ij} < \text{minimum wage}_j \\
 &= 0 && \text{if } w_{ij} \geq \text{minimum wage}_j
 \end{aligned}$$

The store 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, 1996.¹⁷ 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 summary statistics in Table 3 show, first, that while the direct impact on average wages at a typical store is modest, there is substantial variation across stores. In states where the federal minimum was binding throughout the sample period (column 1), the mean store wage gap is 2.5 percent but the standard deviation is 3.1 percent. Indeed, stores in the bottom quartile of this group have store wage gaps of less than half of one percent, while the mean among the top quartile is 7.0 percent. Further, the store wage gap is also substantially smaller in states with high initial minimums, where the mean is 0.2 percent; and is much higher in California stores, where the mean is 5.2 percent.¹⁸

As expected, the store wage gap tends to be slightly higher when the focus is restricted to part-time workers. But because part-time workers make up the vast majority of the workforce, the part-time measures are not much different from the overall impact measures. Also as expected, teenagers have larger wage gaps than adults. For example, among the stores bound by the federal minimum, the average store teenage wage gap is 3.7 percent while the average adult wage gap is 1.6 percent.

¹⁷ The average is calculated in a way similar to that described in footnote 14 above; 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.

¹⁸ A similar pattern emerges if one looks at an alternate measure of the legislation's impact on wages: the fraction of a store's employees whose wages are affected by the legislation. For the sample as a whole, the average "fraction affected" is roughly 48 percent, and the standard deviation is 21 percentage points. The fraction affected is highest in California (59 percent) and lowest in states with high initial minimums (27 percent). It is also higher among part-time employees (49 percent) and teenagers (53 percent) compared to fulltime employees and adults (43 percent).

Because the legislation raised teenage wages more than it raised adult wages, it caused an increase in the relative wage of teenagers in most stores. To measure the legislation's impact on *relative wages*, I construct a variable called the “store relative wage gap.” This variable is again based on wages paid between Feb. 1 and July 31, 1996, and 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 as:¹⁹

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

In the last row of Table 3, we see that compliance with the law caused on average a two percent increase in the relative wage of teenagers. However, the size of the relative wage gap varies significantly across stores, with a standard deviation of 2.7 percent. Also, it is close to zero in states with high initial minimums, and is largest in stores located in California.

4. Methods of Analysis

The basic analysis consists of store-level regressions relating the legislation's impact on wages, as measured by the “wage gap” variables defined above, to changes in the outcome variables of interest. The regression equations take the form:

$$(1) \quad \Delta Y_j = \alpha + \beta \cdot X_j + \gamma \cdot \text{wage gap}_j + \varepsilon_j$$

where 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 one of the impact measures in Table 3.

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,

¹⁹ The relative wage in store j prior to the legislation is $\text{store relative wage}_{j1} = (\text{store average teenage wage}_j) / (\text{store average adult wage}_j)$. After wages are adjusted to comply with the new minimum, the relative wage becomes: $\text{store relative wage}_{j2} = (\text{store average teenage wage}_j)(1 + \text{store teenage wage gap}_j) / (\text{store average adult wage}_j)(1 + \text{store adult wage gap}_j)$. The $\text{store relative wage gap}_j$ is then: $(\text{store relative wage}_{j2} - \text{store relative wage}_{j1}) / (\text{store relative wage}_{j1})$, which can be expressed more simply by the above expression.

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 age initial 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.

This regression analysis has two important limitations. First, it cannot rule out endogeneity of the legislation's impact on wages. Of particular concern is the possibility that differences in initial wage levels (and hence in the wage gaps) are driven by unobserved, pre-legislative market conditions, and that these conditions, in turn, caused wage and/or employment trends to differ even before the legislation was passed. Second, the regression estimates reveal nothing about the timing of the adjustments. To address both concerns, I perform two complementary graphical analyses. For these analyses I construct a panel data set containing store averages of the relevant variables for each of the 30 months in the sample period. These analyses exclude states where the legislation is not synchronized with the federal law.

In the first graphical analysis, the sample is divided into "high-impact" and "low-impact" stores; these categories are defined using the median of the relevant wage gap variable as a cutoff. Then monthly averages of the relevant wage and employment variables are plotted for each group of stores. The resulting graphs show the overall time-series patterns of the wage and employment variables of interest and allow for a crude comparison between high and low-impact stores.

The second graphical analysis is based on estimates from store-level regressions. For each month of the sample period, I estimate equations of the form:

$$(2) \quad Y_j = \alpha + \beta \cdot X_j + \gamma \cdot \text{wage gap}_j + \varepsilon_j$$

where Y_j is the level of the wage or employment outcome of interest for store j . I then plot, by month, the wage gap coefficients (i.e. the estimates of γ) and their 95 percent confidence intervals. These coefficients are multiplied by .01 so that they measure, at each point in time, the difference in the outcome variable associated with a one percentage point increase in the wage gap.

The resulting graphs provide a way to assess both the exogeneity of the wage gaps and the magnitude and timing of any responses to the legislation. First, if the wage gap is indeed exogenous to unobserved determinants of wage and employment growth, then the estimates of γ should be zero—or, at least, shown no trend—during the six months before the legislation was passed. Second, responses to the legislation are reflected in the time series pattern of these coefficients during the months after the legislation was passed.

5. Main Results

5.1. Changes in Average Wages

The first row in Table 4 shows the regression estimates of the legislation's effect on store average wages. The estimates indicate that a one percentage point increase in the store wage gap corresponds to roughly a .77 percentage point increase in the growth rate of wages over two years. The estimate varies only slightly across different model specifications and estimation samples, ranging from .75 to .78. Robustness to the inclusion of numerous control variables suggests that these estimates reflect the legislation's impact on wages and not differences in market-driven wage trends.

The graphical analyses of average wages are shown in Figures 1a and 1b. Figure 1a shows the overall time-series pattern of wages for high impact stores (which have an average

wage gap of 0.042) and low-impact stores (with an average wage gap of 0.003). Both groups show similar upward trends over the sample period, except for the two discrete jumps in October 1996 and September 1997.²⁰ These jumps, which coincide with the dates of the federally mandated increases, are substantially larger in the high-impact stores, and overall wage growth is higher in this group as a result of these jumps.

In Figure 1a, the universal upward trend explains why the legislation's estimated impact on wages after two years is roughly 25 percent less than what is implied by the average wage gap. In short, the legislation's impact was weakened somewhat by wage growth that would have occurred in the absence of the law. Further, the fact that trends in the two groups are similar (except at the times of the two legislated increases) supports the interpretation that the regression estimates from Table 4 reflect the legislation's impact and not differences in pre-existing trends.

This interpretation is given further support by Figure 1b, which plots the wage gap coefficients from monthly regressions of average wages that control for all the store characteristics used in Table 4, column 4. Here again, there is no evidence that wages would have grown more quickly in high-impact stores without the legislation. First, the trend in the wage gap coefficients is flat during the pre-legislation period, indicating similar pre-existing trends in high and low-impact stores. Second, there is a slight negative trend after each legislated wage increase, suggesting that wages in higher-impact stores grew a bit more slowly than lower-impact stores following the legislation.

5.2. Changes in Full-Time Equivalent Employment

The second row of Table 4 shows the regression estimates of the legislation's effect on two-year changes in full-time equivalent employment. The estimated effect of a one percentage

²⁰ 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 footnote 26.

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, shown in row 3, range from -0.09 to -0.80 when evaluated at the sample mean of 14.7 full-time equivalent employees.

The fact that the estimates become increasingly negative with more controls in the regression suggests they could be biased toward zero due to unobserved market differences in pre-existing employment trends. However, the graphical analysis discussed below shows no evidence that this is the case.²¹

Another concern is potential measurement error in the full-time equivalent employment variable because of the (possibly inaccurate) assumption that part-time employees work half as many hours as full-time employees. This would be especially problematic if the fraction of part-time employees varies across stores or over time. The specification shown in column 5 addresses this concern by controlling for changes in the fraction of employees that is part-time. The coefficient on the store wage gap is very similar to that in column 4, and suggests that differences in the relative growth of part-time employment are not a significant source of bias.^{22,23}

Finally, in column 6, the estimation sample is expanded to include stores in states with “unsynchronized” minimum wage increases. Again, there is little change in the estimated

²¹ 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.

²² A fuller 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. However, this correlation is due entirely to the slight growth in part-time employment in less-impacted stores. In high-impact stores (as defined above), the fraction part-time is initially high—at roughly 96 percent—and remains high throughout the sample period. Hence, it does not appear that the legislation had a significant impact on the ratio of part-time to full-time employees. Complete results are available from the author.

²³ A 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 address this concern.

coefficient.

The graphical analyses of employment are shown in Figures 1c and 1d. Figure 1c plots by month the average levels of full-time equivalent employment for both high-impact and low-impact stores. While high-impact stores have lower initial employment levels, both groups show an upward trend in employment, and it is difficult to detect any difference in employment growth between the two groups. Instead, what stands out in this graph are the similarly large seasonal employment swings in both groups of stores that dwarf any relative decline in high-impact stores. For example, despite an overall upward trend, employment falls by roughly seven full-time equivalents between December of each year and the following May.

The relationship between the wage gap and the time-series pattern in employment is easier to discern in Figure 1d, which plots the wage gap coefficients from monthly regressions of employment that control for store characteristics as in Table 4, column 4. Here, two patterns 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, and allays the concern that the regression estimates in Table 4 are biased due to unobserved geographic heterogeneity in employment trends. 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 legislation, the legislation tended to increase the relative wage of teenagers. However, because of variation in both the initial level and the initial distribution of wages, the legislation's impact on relative wages varied across stores. This section exploits variation in the *store relative wage*

gap to estimate the legislation'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 column 1, the estimate from the regression with no controls suggests that a one percentage point increase in the relative wage gap resulted in roughly a one percentage point increase in the relative wage. In specifications that control for region dummies and store characteristics (cols. 3-7), the estimate increases to around 1.3; this suggests the impact of the legislation on relative wages may have been greater than that implied by the relative wage gap. Sensitivity of the estimate to the inclusion of controls also suggests that the relative wage gap is correlated with market differences in the time series pattern of relative wages. These differences are investigated in Figures 2a-2f.

Figures 2a-2c show the grouped analysis of teenage relative wages. Here “high-impact” stores have a store relative wage gap of at least 0.01, with an average of 0.036; and in “low-impact” stores the relative gap is less than 0.01, with an average of 0.002. In figures 2a and 2b, the average teenage wage and average adult wage are plotted by month for the high- and low-impact groups of stores. Teenage wages (Fig. 2a) are flat over the sample period, 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 legislated increases, the difference between the two groups is cut in half by the end of the sample period.

Adult wages (Fig. 2b) exhibit upward trends over the sample period in both groups of stores, but there are two notable differences between the groups in the time series patterns. 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,

while there are noticeable 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. This latter set of wage increases results from merit raises given the last week in June. An analysis of merit raises shows that the difference between high- and low-impact stores is explained mainly by the fact higher-impact stores have higher turnover rates and thus lower rates of eligibility for merit raises.²⁴

Figure 2c plots the relative wage of teenagers (the average teenage wage divided by the average adult wage) for both groups of stores. There is a noticeable seasonal pattern to relative wages in both groups: the relative wage peaks in December due to a dip in adult wages (see Fig. 2b), and dips in July after merit raises are given (mainly to adults). The merit raise effect is especially large in July 1996 and, as anticipated by the pattern in Figure 2b, it causes an especially large drop in the relative wage in low-impact stores. The result is that relative wages in the two groups converge somewhat even before the minimum wage legislation. Nevertheless, as the rest of the graph makes clear, the legislation 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 of 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 column 4. Here again, we see a jump in the coefficient caused by the effect of the June 1996 merit raises on the teenage relative wage. However, the pattern in the five prior months shows no evidence that relative wages were following different trends before the legislation. Moreover, the pattern in the

²⁴ 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 percent, and approximately 80 percent of eligible employees receive one. The average merit raise for eligible adults does not differ significantly between high and low-impact stores. Teenagers in both groups 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.

following 18 months shows large jumps at the dates of the minimum wage increases. In sum, while the graphical analysis cannot rule out the possibility of some bias in the regression estimates, it does confirm that the minimum wage legislation had a substantial impact on relative wages and that the relative wage gap is a reasonable proxy for the size of the impact.

5.4. Changes in the Teenage Share of Employment

Did legislation-induced changes in the relative wages of teenagers lead to changes in the relative employment of teenagers? In the second row of Table 5, the results suggest the answer is yes. But contrary to conventional theory, increases in the relative wages of teenagers led to significant *increases* in the relative employment of teenagers. The estimated coefficients on the relative wage gap range from 0.62 to 0.93. These are small effects, implying that a one percentage point increase in the relative wage gap leads 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 (columns 2 and 3), and declines again with the inclusion of other time-invariant store-level variables (column 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. The variable "change in percent single female" is included 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.²⁵ 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 (column 7).

²⁵ See Blank (2002) for a review of the literature on the effects of welfare reform in the 1990s.

Rows 3-7 of Table 5 show in more detail how an increase in the relative wage gap affects the age distribution of employees. Here we see 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, and that the offsetting decline in the adult employment share is driven mainly by young adults who are 20-22 years old.

The graphical analysis in Figures 2e and 2f confirm that the positive relationship between the relative wage gap and growth in the teenage share of employment is not driven by differences in pre-existing trends. The grouped analysis (Figure 2e) shows that initially both high- and low-impact stores show a negative trend in the teenage employment share, but that teenage employment is falling more quickly in the high-impact stores. However, after the first minimum wage increase, this pattern is reversed. Between the first half of 1997 and the first half of 1998, the teenage employment share appears to level off (aside from seasonal swings) in the low-impact stores, and moreover it increases in the high-impact group.

Figure 2f shows that after controlling for observed differences across stores, there is no apparent correlation between the relative wage gap and pre-legislative trends in the teenage share of employment. This graph plots the coefficients from monthly regressions of the teenage employment share on the relative wage gap and the control variables in Table 5, column 4. In the months before the first wage increase, the coefficient shows some slight seasonal variation but no apparent trend. However, after the first minimum wage increase in October 1996, the coefficient shows a significant holiday-season spike (in December 1997) and then an upward climb beginning again in June of 1997. The holiday and summer seasons both tend to see increases in teenage employment at all stores (Figure 2e). But Figure 2f suggests these surges in teenage employment are especially large where teenagers' relative wages have risen most

because of the new minimum. Finally, the upward trend in the relative wage gap coefficient continues after the second minimum wage increase in September 1997.

5.5. Separate Analyses of Adult and Teenage Employment Levels

The results thus far indicate the minimum wage legislation had two different effects on employment. First, by increasing average wages, the legislation had a negative effect on stores' overall employment levels. Though the estimated effect is statistically insignificant, the direction of the effect is at least consistent with the prediction of the conventional model. But second, by increasing the relative wages of teenagers, the legislation had a positive effect on the relative employment of teenagers. This contradicts the conventional prediction that the largest employment declines should occur among those whose wages increase most.

These differing results imply that the estimated employment effects from Table 4 may mask the presence of countervailing employment effects for adults and teenagers. Table 6 shows the results from separate regressions of teenage and adult employment levels on the teenage and adult wage gaps. The results confirm the existence of countervailing effects for adults and teenagers, and also suggest a non-monotonic effect of the minimum wage on teenage employment.

The analysis of adults shows consistently negative employment effects. On average, an increase in the adult wage had a marginally significantly negative effect on adult employment (col. 1). Also, the estimates in column 2 (while not significant) suggest that the larger was the increase in the teenage wage, the more adult employment declined. This can be interpreted as the effect of substitution toward teenage employees in high-wage markets where the relative wages of teenagers went up.

In contrast, the analysis of teenage employment shows that on average an increase in the teenage wage led to a marginally significant increase in the level of teenage employment (col. 3). However, the employment effect for teenagers varied depending on how much the adult wage increased (col. 4). In markets where both teenage and adult wages increased equally, the estimates imply a zero or negative employment effect for teenagers. But in markets where the teenage wage increased more than the adult wage (i.e., where the relative wage of teenagers increased), there was a substantial positive employment effect for teenagers.

Put another way, the effect of a given wage increase on teenage employment was close to zero or negative in low-wage markets where the pre-existing minimum was closer to the adult wage. Here the teenage wage was already relatively high, and the minimum wage increase was binding for both teenagers and adults. However, in high-wage markets where the pre-existing minimum was well below the adult wage and where the rise in the minimum caused an increase in the relative wages of teenagers, the employment effect for teenagers was clearly positive.

Finally, column (5) shows the competing effects of adult and teenage wage increases on the total level of employment. Because both adult and teenage employment levels fell as the adult wage gap increased, an increase in the adult wage had a significant negative effect on the overall level of employment. But the larger the teenage wage gap and the smaller the adult wage gap, the smaller was the negative employment effect. And in markets where only the teenage wage was affected, employment increased.

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

This section considers why increases in teenage wages led to higher levels of teenage employment. I focus on two types of model—one employs search costs and the other asymmetric information. In both types of model, a modest minimum wage increase can lead to

higher employment by inducing an increase in labor market participation. Hence these models may apply particularly to the market for teenage labor.²⁶ Further analysis yields two insights. First, it supports the idea that the minimum wage induced teenagers to enter the labor market. Second, it suggests that while all teenagers at a given store are paid similar wages, nevertheless the new teenage entrants were relatively productive. This provides support for the model with informational asymmetries.

6.1. Theories Linking Labor Market Participation to Labor Demand

One possible link connecting minimum wages, labor market participation, and labor demand is the presence of search costs for both jobs seekers and employers. For example, in models by Flinn (2006) and Ahn, Arcidiacono and Wessels (2008), wages are set by Nash bargaining and a binding minimum effectively raises a worker's bargaining power and the resulting wage. This induces more individuals to search for jobs (despite a lower probability of finding a "match" conditional on searching). From the firm's perspective, the increase in the number of searchers increases the probability of filling vacancies, and thus reduces the cost of creating new jobs. If this cost reduction is large enough to offset the wage increase, firms are induced to create more vacancies and thus to increase employment. Of course, if the wage increase is too high, it will outweigh the benefit of lower search costs, and employment will fall.

A second model that links increased labor market participation to increased labor demand assumes adverse selection in a labor market where the reservation wages of individuals are increasing in the quality of their labor. Drazen (1986) demonstrates that asymmetric information

²⁶ Another explanation could be that the firm has monopsony power with respect to teenagers but not adults, and can employ wage discrimination against teenagers generally, but not among subgroups of teenagers. Manning (2003, Ch. 7, and 1996) argues that the combination of gender wage differentials and the lack of negative employment effects for women following Britain's Equal Pay Act can be explained by a model in which firms have greater monopsony power vis-à-vis women than men. He argues that this is plausible because women care more than men do about non-pecuniary aspects of jobs (e.g. location). To explain the results of the present study with such a model, an explanation would be required for why the market for teenagers is more monopsonistic than the market for adults.

on the part of both firms and potential job-seekers may lead to an inefficient equilibrium where firms offer low wages and only low-quality workers apply for jobs. First, imperfect information about the quality of individual applicants prevents firms from conditioning wage offers on productivity. Second, potential job seekers have imperfect information about the wage offers of individual firms, and so the decision of whether to enter the labor market depends on the average market wage that is observed. This prevents individual firms from attracting high-quality applicants by unilaterally offering higher wages—even if doing so would reduce the effective cost of labor. In this situation, a minimum wage can increase labor demand by inducing an improvement in the average quality of job applicants that more than offsets the wage increase. Again, labor demand will fall if the minimum wage is set too high.

The adverse selection model may be especially relevant here because the informational asymmetries that it rests on are likely to be more severe in the teenage labor market than the adult labor market. First, because teenage applicants are more likely to lack an employment history, their productivity may be more difficult to observe. Second, because many non-working teenagers have never searched for a job, they may have little awareness of the wage offers of specific employers.

6.2. The Effects of Relative Wage Increases on Teenage Labor Market Participation: Evidence on Teenagers from “High-Income” ZIP Codes

Both the search cost and adverse selection models suggest that a positive response in teenage employment to an increase in relative wages would operate through an increase in labor market participation by teenagers. These theories thus imply other empirical predictions that can be examined with personnel data. First, increases in teenage employment should be driven by individuals whose opportunity cost is most likely to exceed the benefit of employment at the

initial wage. Second, changes in the composition of employment should be caused mainly by changes in the composition of new hires, and not simply by differential changes in turnover rates.

To test the first prediction, I analyze compositional changes in a measure of socio-economic status among teenagers. Socioeconomic status is likely to be negatively correlated with a teenager's surplus from being employed at a given wage. First, teenagers who live in more affluent households are likely to have a lower marginal utility of income.²⁷ Second, they may also have higher opportunity costs—for example, access to higher quality schools may increase their return to schooling. My measure of socio-economic status is constructed by first merging employee residential ZIP codes with data on median household income from the 1990 Census, and ranking the ZIP codes of all of a store's employees by the median household income.²⁸ I then define as “high-income” those whose ZIPs are in the highest-income quartile and as “low-income” those in the lowest quartile, and construct dummy variables based on these definitions.

Analysis that distinguishes between teenagers from high-, middle-, and low-income ZIP codes leads to a set of three results that, together, suggest an increase in the labor-market participation of teenagers. First, among teenagers, wage offers are not correlated with socio-economic status. Second, “high-income” teenagers are more likely to terminate employment to return to school. And third, the positive effect of higher relative wages on teenage employment is driven mainly by “high-income” teenagers. The first two results suggest that high-income

²⁷ For evidence that parental income negatively affects the teenage labor supply through an income effect, see Dustmann et al. (forthcoming).

²⁸ 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.

teenagers do indeed obtain a smaller surplus from employment; and the third result suggests that these teenagers in particular are drawn into the labor market by the minimum wage.

The first two results are shown in Table 7. First, column 1 shows the coefficients from regressions of starting wages on dummy variables indicating residence in high- and low-income ZIP codes. These regressions control for store fixed effects and for individual characteristics that include the employee's month of hire, age, race, gender, fulltime vs. part-time employment status, and an indicator for previous employment with the company. Separate models are estimated for teenagers, young adults (ages 20 to 22), and adults over 22 years old. The results for the teenage sample show that the starting wages are very similar across all groups of teenagers. This suggests either that productivity is not correlated with the measure of socio-economic status or that the employer does not pay wages proportional to output (e.g., because of imperfect information). Interestingly, the lack of wage differentials is not seen in the adult samples; adults residing in high-income ZIP codes earn significantly more than those from lower-income locations.²⁹

Column 2 of Table 7 shows the hazard ratios from a Cox proportional hazard model of the rate at which employees terminate employment to return to school. The hazard function is stratified by store and includes as regressors the same employee variables that are in the wage model (col. 1).³⁰ Among teenagers and young adults, employees from high-income ZIP codes are roughly 35 percent more likely than those in middle-income ZIP codes, and 50 percent more

²⁹ In the case of adults, the positive relationship between the wage and the high-income ZIP code indicator could be due to the influence of wages on socio-economic status. Such reverse causality is much less likely for teenagers, whose residence depends on their parents' income and not their own.

³⁰ 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. Thus, in modeling the hazard rate of dismissal, exits for other reasons are treated as censored. For comparison, the table also shows estimates from a model of the hazard of quitting. The hazard of being laid off is not correlated with employee ZIP code (results not shown).

likely than those in low-income ZIP codes, to terminate employment to return to school.³¹

Hence, the utility of employment relative to non-market time appears to be lower for teenagers from more affluent ZIP codes.

Evidence that the legislation-induced increase in teenage employment came disproportionately from “high-income” teenagers is shown in Table 8 and in Figure 3. Rows 1-4 of Table 8 show the coefficients on the store relative wage gap from regressions in which the dependent variables are the two-year changes in: (1) the fraction of employees who are “high-income” teenagers; (2) the fraction of employees who are “middle-income” teenagers; (3) the fraction of employees who are “low-income” teenagers; and (4) the fraction of teenage employees who live in high-income ZIP codes. The coefficients in rows 1-3 are all positive, suggesting that where stores were forced to increase the relative wages of teenagers, they subsequently increased their employment of *all* teenagers. But the coefficients in row (4), where the dependent variable is the fraction of teenagers from high-income ZIP codes, are also positive and statistically significant. This indicates that the increase in teenage employment came disproportionately from high-income teenagers.³²

Figure 3 plots, by month, the coefficient on the store relative wage gap from a regression in which the dependent variable is the fraction of teenage employees who live in high-income ZIP codes, and which includes control variables as in Table 8, column 2. The time-series pattern here looks very similar to the pattern seen in Figure 2f, where the dependent variable is the teenage share of employment. There is no trend during the pre-legislation period; there is a jump during the post-legislation holiday season; and then it begins a steady increase in June 1997.

³¹ 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 percent lower for the indicated group than for the omitted group.

³² The finding that the minimum wage led to an increase in the labor supply of relatively affluent teenagers is consistent with the findings of Newmark and Wascher (1996) and Ahn et al. (2008) (see Section I). However, the results here differ in that I find no evidence of substitution away from less affluent or less educated teenagers.

This further supports the interpretation that the increase in teenage employment was driven mainly by the increased employment of more affluent teenagers.

6.3. Compositional Changes in Employment Flows: New Hires and Exit Rates

An increase in the employment of teenagers could reflect either an increase in the number of teenagers who are hired or a reduction in the exit rate of teenagers who are already employed. But if teenage employment went up due to increased labor market participation, then the change must have been driven mainly by the hiring of more teenagers.

Table 9 shows some evidence that this is true. First, rows 1 and 2 show the estimated effects of the relative wage gap on changes in the shares of new hires who are teenagers (row 1) and 20-22 year-olds (row 2). Though the estimates are less precise than those from the regressions of changes in the employment stock, the pattern of change is similar—there are positive effects for teenagers and negative effects for 20-22 year-olds. Next, the estimates in column 3 show the estimated effect of the relative wage gap on the share of new teenage hires from high-income ZIP codes. Again, while not statistically significant, the results are consistent with the estimates based on employment shares and support the hypothesis that new teenage hires came disproportionately from high-income ZIP codes.

Estimates from regressions of changes in exit rates are shown in rows 4-7. The point estimates for both teenagers and young adults are positive and somewhat larger for teenagers, but all are statistically insignificant. Hence there is no evidence either that the legislation reduced turnover rates of teenagers or that it increased the turnover rates of young adults. It is therefore unlikely that the increase in teenage employment and decline in young adult employment were driven by differential effects of the legislation on overall turnover rates.

However, when attention is restricted to exits for the purpose of returning to school in

“back-to-school months,” the legislation’s estimated effect is negative for teenagers and positive for young adults.³³ For teenagers, the negative effect suggests the relative wage increases not only induced labor market entry by students, but also induced more of them to *stay* in the labor force during the months that school was in session. The positive effect for young adults is more difficult to interpret because the data contains no information on college enrollment. The minimum wage could have induced more college students to end employment with the firm during the school year—perhaps because minimum-wage student jobs on college campuses were now relatively attractive. Or the increased rate at which young adults left employment for school could reflect a compositional change among young adults—perhaps because the firm substituted toward college students and away from other young adults.

6.4. Did the Minimum Wage Raise the Average Productivity of Teenage Employees?

While both the search and adverse selection models feature increases in labor market participation, a key assumption that distinguishes the latter is that new labor market entrants are relatively productive. In the current setting, the relevant assumption is that teenagers from high-income ZIP codes are more productive than those from low-income ZIP codes. Hence testing for such productivity differences can shed more light on the plausibility of the adverse selection model.³⁴

The data set offers two ways to test for productivity differences among these groups of teenagers. First, at the individual level, productivity can be measured by the probability of being

³³ Back-to-school months are defined as January, February, August and September. For this analysis only, I examine the change from these back-to-school months of the 1996-97 school year to the same months one year later. This choice is motivated by the sample’s time frame (it does not contain January 1996) and by Figure 2F, which shows the permanent increase in teenage employment beginning in the summer of 1997.

³⁴ Unfortunately, the data set does not contain information on vacancies, so it is not possible to test the “search cost” assumption that the minimum wage caused vacancies to be filled more quickly.

fired.³⁵ Table 7, column 3 shows the results from a model similar to that estimated in column 2, except that the dependent variable is the hazard rate of being dismissed. Teenage employees from high-income ZIP codes are 17 percent less likely than teenagers from middle-income ZIPs to be fired and 28 percent less likely than those from low-income ZIPs. Moreover, results from a pooled regression (not shown) indicate that while the probability of being fired generally declines with employee age, teenagers from high-income ZIP codes are significantly *less* likely to be fired than a typical 20-22 year-old, and are only slightly more likely to be fired than adults on average. So, by this measure at least, teens from high-income ZIP codes are roughly as productive as adults.

Second, at the store level, the relationship between employee ZIP codes and productivity can be measured using data on monthly sales. Table 10 shows the results from a regression of sales on the employment shares of employees from each of four groups: teenagers from high-income ZIP codes, other teenagers, adults from high-income ZIP codes, and other adults. The estimation equation includes store fixed effects, month and year dummies, and controls for full-time equivalent employment and the fraction of employees that is part-time. The results indicate a significant positive relationship between sales and the fraction of employees who are from high-income ZIP codes—and especially teenagers from high-income ZIP codes. While causality in this relationship is difficult to prove, it is plausible that hiring more high-income employees could cause sales to increase. For example, more affluent employees might attract more affluent customers.³⁶

³⁵ Dismissals are defined as involuntary terminations that result from dishonesty, substandard performance, tardiness, absenteeism, or violation of company policies. Dismissals comprise roughly seven percent of the observed employment terminations in the data.

³⁶ In another study that analyzes data from a large U.S. service sector firm employing relatively young, low-wage workers, Autor and Scarborough (2008) also find evidence that employees from high-income ZIP codes are more productive. They find that the median household income of an employee's ZIP code is a significant predictor of the employee's score on a screening test that predicts various measures of productivity.

In sum, the evidence presented here suggests that among teenagers in my data set, those from relatively high-income ZIP codes were more productive than those from lower-income ZIPs. The combination of this finding with the lack of wage differences among teenagers lends plausibility to the adverse selection model. Moreover, in the face of quality differences among teenagers, the adverse selection model better explains the finding that while the increased employment of “high-income” teenagers displaced some young adults, it did not displace teenagers from low-income ZIPs who were apparently less productive, but were paid similar wages. Models relying solely on search costs do not offer an explanation for the lack of disemployment effects among less productive teens.³⁷ But when the adverse selection model is applied to the market for teenagers, it predicts that an increase in the labor market participation of more productive teenagers raises the demand for all teenagers by increasing their average productivity.

7. Conclusion

Using personnel data from a large U.S. retail firm, this study examines how the firm responds to minimum wages with respect both to overall employment and to compositional changes in employment. The results show the importance of distinguishing between sub-groups of low-wage workers, and suggest that standard neoclassical predictions do not hold for teenagers in markets where the relative wages of teenagers are initially low. In such markets, the results fit better with more recent models that link labor demand to labor market participation.

The main findings are, first, that legislation-induced increases in average wages had

³⁷ Flinn (2006) and Ahn et al. (2008) assume uniform worker quality. Lang and Kahn (1998) describe a model with search costs in which there are two types of employer and two types of workers, and in which the wage in low-wage jobs is set at a level that achieves a separating equilibrium. Like Drazen’s model, this model also predicts that a minimum wage can induce high-quality workers to apply for low-wage jobs earning the same wage as low-quality workers. In this model, however, the individual quality is observed and so the model predicts that low-quality workers are displaced by high-quality types.

negative but statistically insignificant effects on overall employment levels. Though the estimates are imprecise, their sign is consistent with standard theory. Second, however, increases in the relative wage of teenagers had *positive* and significant effects on the relative employment of teenagers. This finding contradicts the prediction of neoclassical theory that firms respond to increases in the minimum wage by substituting away from workers whose wages increase the most.

Third, separate analyses of employment for teenagers and adults reveal countervailing effects for these two groups. The analysis also suggests a non-monotonic effect on teenage employment. On one hand, wage increases had consistently negative employment effects for adults. On the other, wage increases resulted on average in higher levels of employment for teenagers. However, the effect for teenagers varied depending on how high the minimum wage was relative to adult wages.

In low-wage markets—where adult wages were closer to the minimum, teenage wages were already relatively high, and the increase in the minimum affected the wages of both teenagers and adults—the employment effect for teenagers was close to zero or negative. But in higher-wage markets—where the minimum wage increase raised teenage wages from relatively low levels—the employment effect for teenagers was positive. This non-monotonic effect on teenage employment caused the legislation’s impact on overall employment to vary from negative in low-wage markets to zero or positive in higher-wage markets.

Further analysis suggests that the positive employment effect for teenagers was driven by teenagers from high-income ZIP codes, and that the minimum wage is likely to have induced an increase in labor market participation by this group of teenagers. Such an increase in labor market participation could have reduced the effective cost of employing teenagers, either by

reducing search costs (as in Flinn, 2006 and Ahn et al., 2008) or by alleviating adverse selection (as in Drazen, 1986).

Finally, analysis also suggests that the more affluent teenagers who are driving the increases in teenage employment are more productive than less affluent teenagers who are paid similar wages. This is consistent with informational asymmetries that could lead to adverse selection in the market for teenage labor. Hence, while not ruling out a role for search costs, I conclude that informational asymmetries are a likely explanation for the positive effect of the minimum wage on teenage employment.

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

<u>Variable</u>	<u>Mean</u>	<u>Std. Dev.</u>
Number of entry-level employees	27.3	14.0
Full-time equivalent employment ^a	14.7	7.9
% Part-time	93.7	6.5
% Teenagers	41.5	12.9
% ages 16-17	16.5	9.0
% ages 18-19	23.9	9.7
% Adults	58.5	12.9
% ages 20-22	22.1	10.2
% ages 23-29	17.1	9.0
% ages 30 & up	19.2	13.1
% Female	76.9	13.5
% White	71.9	22.3
% Black	11.4	12.6
% Hispanic	9.3	12.8
Square feet	6,978	3,845
Population with 2-mi. radius ^b	83,275	88,780
% population that is white ^b	79.4	16.7
% population that is black ^b	7.6	9.5
% population that is Hispanic ^b	5.4	8.9
Local Area Unemployment Rate ^c	5.1	1.6

Note: Based on employment weighted averages from Feb.1st, 1996-July 31st, 1996.

^a 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 Local Area Unemployment 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	\$4.75	\$5.25 ^c	\$5.25	
OR	\$4.75	\$4.75	\$4.75	\$5.50 ^c	\$5.50	\$6.00 ^e
VT	\$4.75	\$4.75	\$4.75	\$5.00 ^c	\$5.15	\$5.25 ^f

Source: Bureau of Labor Statistics, *Monthly Labor Review*, January Issues, 1997, 98, 99.

Note: Effective dates: ^a Apr. '96; ^b Mar. '97; ^c Jan. '97; ^d Mar. '98; ^e Jan. '98; ^f Oct. '97.

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

Variable	Federal Min.	HI, IA, NJ, RI & WA	CA	DE, MA, OR & VT	Full Sample
store average wage (all entry-level jobs)	\$5.67 (\$0.51)	\$6.00 (\$0.40)	\$5.93 (\$0.48)	\$5.78 (\$0.37)	\$5.75 (\$0.51)
store average P/T wage	\$5.56 (\$0.45)	\$5.90 (\$0.38)	\$5.79 (\$0.39)	\$5.67 (\$0.29)	\$5.64 (\$0.45)
store average teenage wage	\$5.31 (\$0.45)	\$5.67 (\$0.32)	\$5.58 (\$0.45)	\$5.50 (\$0.25)	\$5.40 (\$0.44)
store average adult wage	\$5.97 (\$0.67)	\$6.33 (\$0.55)	\$6.31 (\$0.69)	\$6.09 (\$0.63)	\$6.08 (\$0.68)
store average relative wage of teenagers	\$0.90 (\$0.08)	\$0.90 (\$0.07)	\$0.89 (\$0.09)	\$0.91 (\$0.08)	\$0.90 (\$0.08)
store wage gap (all entry-level jobs)	2.5% (3.1)	0.2% (0.5)	5.2% (3.4)	1.4% (1.5)	2.6% (3.1)
store P/T wage gap	2.6% (3.1)	0.2% (0.5)	5.5% (3.4)	1.4% (1.6)	2.2% (3.2)
store teenage wage gap	3.7% (4.1)	0.3% (0.6)	7.4% (4.9)	1.9% (2.0)	3.8% (4.3)
store adult wage gap	1.6% (2.7)	0.2% (0.5)	3.2% (2.9)	0.8% (1.2)	1.6% (2.5)
store relative wage gap	2.1% (2.7)	0.2% (0.4)	4.1% (3.6)	1.2% (1.3)	2.1% (2.7)

Note: Based on employment-weighted averages of wages during the pre-legislation period from Feb.1, 1996-July 31, 1996. Standard deviations in parentheses.

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

	(1)	(2)	(3)	(4)	(5)	(6)
<u>Outcome:</u>						
1. Change in log of average wage	0.773** (0.051)	0.777** (0.052)	0.749** (0.073)	0.778** (0.079)	0.740** (0.079)	0.722** (0.065)
2. Change in avg. FTE employment	-1.021 (4.697)	-1.665 (4.927)	-4.032 (7.123)	-9.029 (7.505)	-8.745 (7.541)	-7.202 (6.736)
3. Implied labor demand elasticity	-0.09	-0.15	-0.37	-0.79	-0.80	-0.68
<u>Controls in model specification:</u>						
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
Change in percent part-time	no	no	no	no	yes	yes
Sample incl. CA, DE, MA, OR & VT	no	no	no	no	no	yes

Note: Entries are regression coefficients of the store average wage gap (for all entry-level employees) in models for the change in the outcomes 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. ** Significant at 1%.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<u>Outcome:</u>							
1. Change in relative teenage wage (Feb.-July '96) to (Feb.-July '98)	1.021** (0.104)	1.042** (0.103)	1.280** (0.120)	1.321** (0.124)	1.325** (0.122)	1.323** (0.122)	1.270** (0.109)
2. Change in percent teenagers (Feb.-July '96) to (Feb.-July '98)	0.621** (0.204)	0.735** (0.191)	0.925** (0.212)	0.790** (0.214)	0.798** (0.215)	0.729** (0.199)	0.747** (0.173)
3. Change in percent ages 16-17	0.479** (0.161)	0.592** (0.158)	0.642** (0.177)	0.604** (0.177)	0.610** (0.177)	0.562** (0.166)	0.497** (0.133)
4. Change in percent ages 18-19	0.143 (0.175)	0.143 (0.137)	0.284 [‡] (0.165)	0.186 (0.167)	0.188 (0.168)	0.167 (0.167)	0.250 [‡] (0.133)
5. Change in percent ages 20-22	-0.409** (0.157)	-0.603** (0.124)	-0.762** (0.153)	-0.787** (0.156)	-0.789** (0.155)	-0.788** (0.155)	-0.702** (0.136)
6. Change in percent ages 23-29	-0.027 (0.141)	-0.025 (0.116)	-0.168 (0.144)	-0.134 (0.146)	-0.140 (0.146)	-0.118 (0.145)	-0.189 (0.126)
7. Change in percent ages 30 & up	-0.186 (0.133)	-0.107 (0.126)	0.005 (0.145)	0.131 (0.142)	0.131 (0.143)	0.178 (0.137)	0.144 (0.106)
<u>Controls in model specification:</u>							
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
Change in percent part-time	no	no	no	no	yes	yes	yes
Change in percent single female	no	no	no	no	no	yes	yes
Sample incl. CA, DE, MA, OR & VT	no	no	no	no	no	no	yes

Note: Entries are regression coefficients of the store relative wage gap in models for the change in the outcomes 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. [‡] Significant at 10%; * significant at 5%; ** significant at 1%.

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

	Change in Adult FTE employment		Change in Teenage FTE employment		Change in FTE employment
	(1)	(2)	(3)	(4)	(5)
Store adult wage gap	-11.42 [‡] (5.94)	-4.45 (7.57)		-18.72** (6.09)	-23.16* (9.92)
Store teenage wage gap		-6.81 (5.04)	5.71 [‡] (2.95)	13.56** (3.89)	6.74 (6.39)
R-squared	0.19	0.20	0.34	0.35	0.21

Note: Estimation equations include control variables as in Table 5, columns (5) & (6). [‡] Significant at 10%; * significant at 5%; ** significant at 1%.

TABLE 7. COMPARISONS OF EMPLOYEES LIVING IN HIGH-INCOME VS. LOW-INCOME ZIP CODES

	(1) Log of Starting Wage	Hazard Rate for Employment Termination Due to:		
		(2) School	(3) Dismissal	(4) Quit
<u>Sample: Teenagers</u>				
employee lives in high-income ZIP code	-0.000 (0.001)	1.247** (0.026)	0.828** (0.031)	0.970* (0.014)
employee lives in low-income ZIP code	0.000 (0.001)	0.847** (0.026)	1.146** (0.046)	1.031 [‡] (0.018)
<u>Sample: 20-22 Year Olds</u>				
employee lives in high-income ZIP code	0.002 (0.001)	1.279** (0.037)	0.841** (0.047)	0.991 (0.018)
employee lives in low-income ZIP code	-0.005** (0.001)	0.868** (0.029)	1.162** (0.056)	1.037* (0.019)
<u>Sample: Over 22 Years Old</u>				
employee lives in high-income ZIP code	0.010** (0.002)	1.094 (0.068)	0.730** (0.044)	0.959* (0.016)
employee lives in low-income ZIP code	-0.011** (0.001)	0.941 (0.060)	1.343** (0.064)	1.022 (0.016)

Note: Column (1) shows coefficients from linear regressions predicting the log of starting wage. Columns (2)-(4) show estimated hazard ratios from Cox proportional hazard models predicting the likelihood of (1) terminating employment to return to school; (2) being dismissed; (3) quitting (for job-related reason). All regression models control for store fixed effects (hazard models are stratified by store), as well as for dummy variables indicating the employee's age, race, gender, part-time status, prior company experience, and month of hire. Omitted ZIP code category is ZIPs in middle quartiles of median household income. Robust standard errors in parentheses; [‡] significant at 10%; * significant at 5%; ** significant at 1%.

TABLE 8. ESTIMATED EFFECTS OF STORE RELATIVE WAGE GAP ON EMPLOYMENT SHARE OF TEENAGERS, BY ZIP CODE TYPE

Outcome Variable:	(1)	(2)	(3)
(1) Δ % Employees who are teenagers from high-income ZIPs	0.390* (0.157)	0.467** (0.159)	0.347* (0.130)
(2) Δ % Employees who are teenagers from middle-income ZIPs	0.064 (0.184)	0.040 (0.197)	0.119 (0.166)
(3) Δ % Employees who are teenagers from low-income ZIPs	0.151 (0.095)	0.221* (0.142)	0.281* (0.125)
(4) Δ % Teenagers who are from high-income ZIPs	0.755** (0.270)	0.864* (0.338)	0.567* (0.271)
Model includes controls as in Table 5 , col. (6)	no	yes	yes
Sample Includes CA, DE, MA, OR & VT	no	no	yes

Note: Entries are regression coefficients of the store relative wage gap in models for the change in the outcomes 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. ‡ Significant at 10%; * significant at 5%; ** significant at 1%.

TABLE 9. ESTIMATED EFFECTS OF STORE RELATIVE WAGE GAP ON COMPOSITION OF NEW HIRES AND EXIT RATES

Outcome Variable:	(1)	(2)	(3)
(1) Δ % New hires who are teenagers	0.499 (0.352)	0.985* (0.465)	0.512 (0.391)
(2) Δ % New hires who are ages 20-22	-0.292 (0.271)	-0.598 (0.379)	-0.415 (0.321)
(3) Δ % Teenage hires who are from high-income ZIPs	0.548 (0.467)	0.482 (0.605)	0.347 (0.687)
(4) Δ Avg. daily exit rate for teenagers	0.017 (0.028)	0.008 (0.016)	0.031 (0.021)
(5) Δ Avg. daily exit rate for ages 20-22	0.003 (0.008)	0.005 (0.010)	0.007 (0.008)
(6) Δ Avg. daily rate at which teens exit employment for school (back-to-school months in '96-97 vs. '97-98 school yrs)	-0.016‡ (0.009)	-0.017‡ (0.010)	-0.003 (0.009)
(7) Δ Avg. daily rate at which 20-22 yr-olds exit empl. for school (back-to-school months in '96-97 vs. '97-98 school yrs)	0.034 (0.033)	0.059 (0.046)	0.040 (0.030)
Model includes controls as in Table 5 , col. (6)	no	yes	yes
Sample Includes CA, DE, MA, OR & VT	no	no	yes

Note: Entries are regression coefficients of the store relative wage gap in models for the change in the outcomes 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. ‡ significant at 10%; * significant at 5%; ** significant at 1%.

TABLE 10. ESTIMATED EFFECTS OF EMPLOYEE AGE AND ZIP CODE COMPOSITION ON SALES

	<u>Log Real Monthly Sales</u>
intercept	2.636** (0.055)
% employees who are teenagers from high-income ZIPs	0.109** (0.036)
% employees who are adults <i>not</i> from high-income ZIPs	-0.011 (0.026)
% employees who are adults from high-income ZIPs	0.042 (0.035)
<hr/>	
R-squared	0.90
<hr/>	
F test of H ₀ : %not-rich adults=%rich adults (<i>Prob. >F</i>)	2.21 (0.137)

Note: Table shows coefficients and robust standard errors from a linear regression with store fixed effects and controls for full-time equivalent employment, the fraction of employees who are part-time, and month indicators. The omitted category is % employees who are teenagers *not* from high-income ZIP codes. ** Significant at 1%

**FIGURE 1A.
AVERAGE ENTRY-LEVEL WAGE**

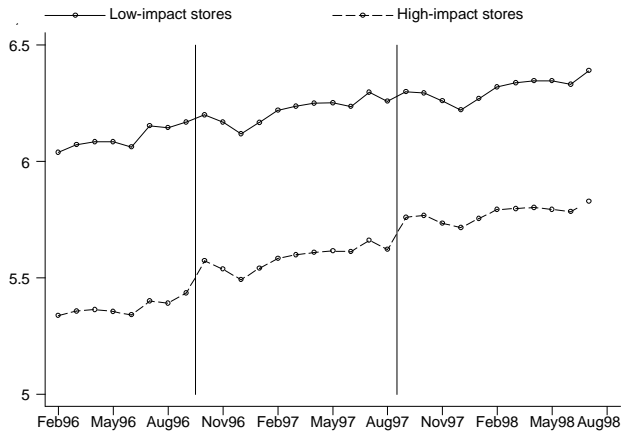
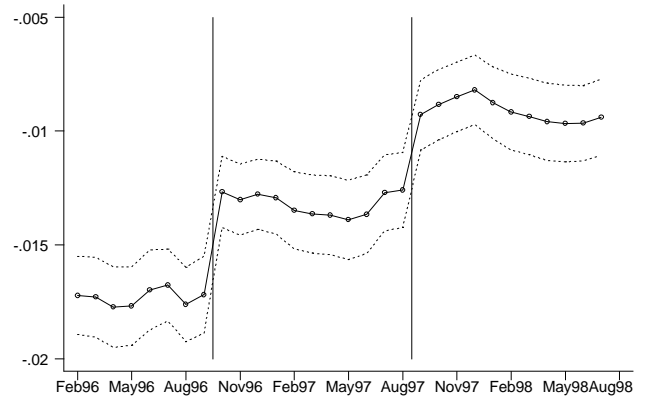


FIGURE 1B. EFFECT OF A .01 INCREASE IN STORE WAGE GAP ON LN(AVG. ENTRY-LEVEL WAGE)



**FIGURE 1C.
FULL-TIME EQUIVALENT ENTRY-LEVEL EMPLOYMENT**

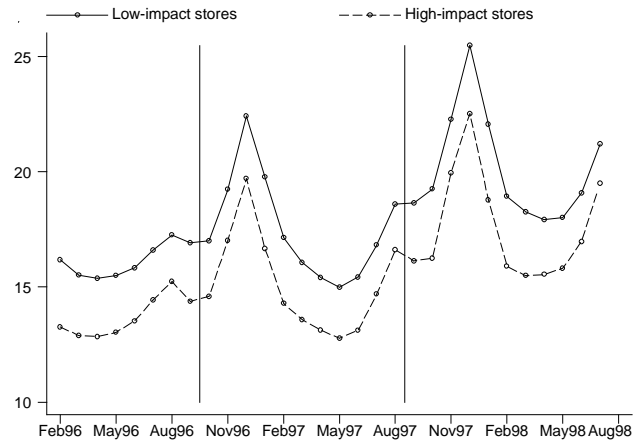
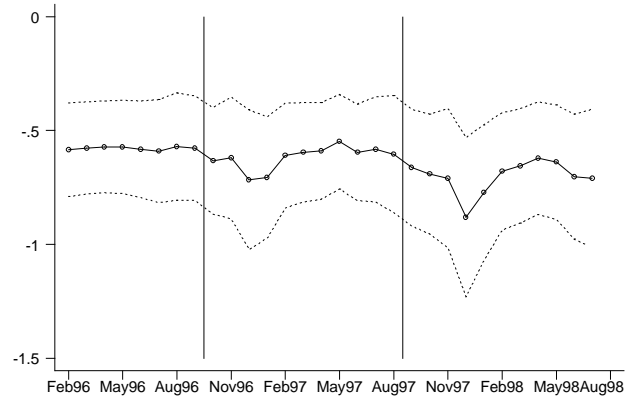


FIGURE 1D. EFFECT OF A .01 INCREASE IN STORE WAGE GAP ON FULL-TIME EQUIVALENT EMPLOYMENT



Note: Vertical lines indicate dates of October 1996 and September 1997 federal minimum wage increases. Dotted lines in Fig. 1b, 1d show 95 percent confidence interval.

FIGURE 2A. AVERAGE TEENAGE WAGE

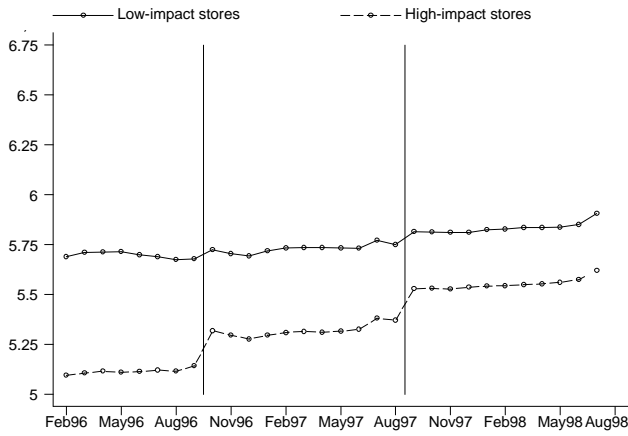


FIGURE 2B. AVERAGE ADULT WAGE

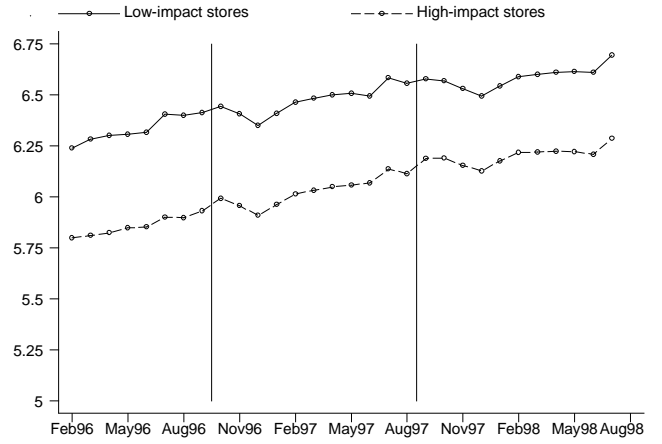


FIGURE 2C. TEENAGE WAGE/ ADULT WAGE

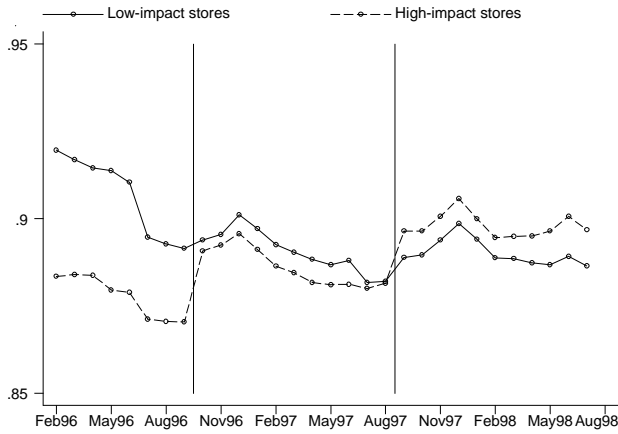


FIGURE 2D. EFFECT OF A .01 INCREASE IN RELATIVE WAGE GAP ON TEENAGE RELATIVE WAGE

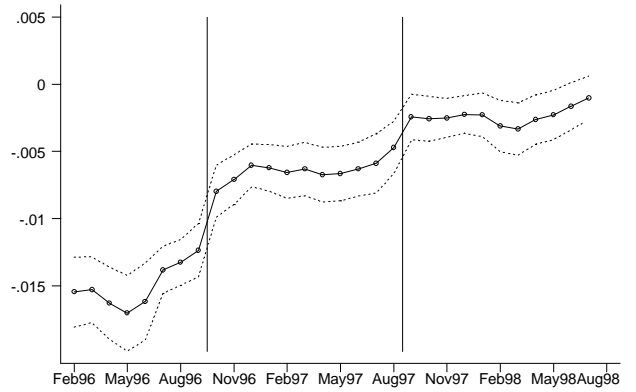


FIGURE 2E. TEENAGE EMPLOYMENT SHARE

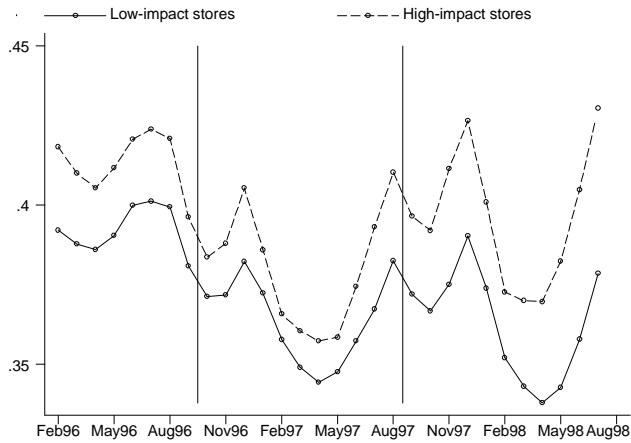
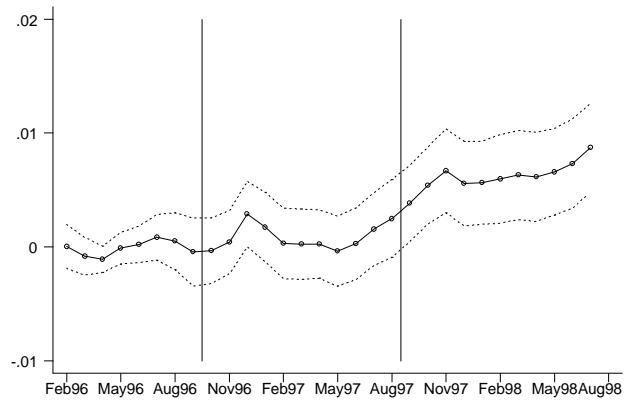
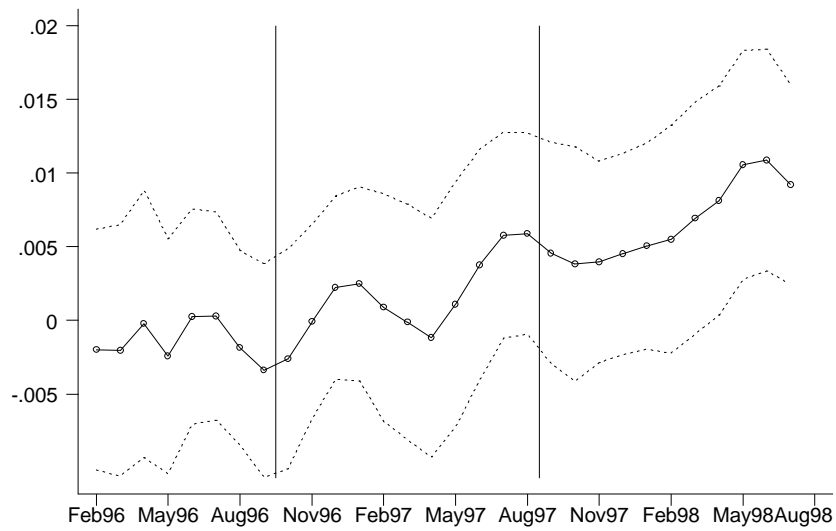


FIGURE 2F. EFFECT OF A .01 INCREASE IN RELATIVE WAGE GAP ON TEENAGE EMPLOYMENT SHARE



Note: Vertical lines indicate dates of October 1996 and September 1997 federal minimum wage increases. Dotted lines in Fig. 2d, 2f show 95 percent confidence interval.

FIGURE 3. ESTIMATED EFFECT OF .01 INCREASE IN RELATIVE WAGE GAP ON FRACTION OF TEENAGERS WHO ARE FROM HIGH-INCOME ZIP CODES



Note: Vertical lines indicate dates of October 1996 and September 1997 federal minimum wage increases. Dotted lines show 95 percent confidence interval.