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Erzo F.P. Luttmer*

*Harvard University, erzo_luttmer@harvard.edu

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Does the Minimum Wage Cause Inefficient Rationing?*

Erzo F.P. Luttmer

Abstract

By not allowing wages to clear the labor market, the minimum wage could cause workers with low reservation wages to be rationed out while equally skilled workers with higher reservation wages are employed. This paper exploits overlapping CPS panels to more precisely identify those most affected by the minimum wage, a group I refer to as the "unskilled." I test for inefficient rationing by examining whether the reservation wages of employed unskilled workers in states where the 1990-1991 federal minimum wage increase had the largest impact rose relative to reservation wages of unskilled workers in other states. I find that proxies for reservation wages of unskilled workers in high-impact states did not rise relative to reservation wages in other states, suggesting that the increase in the minimum wage did not cause jobs to be allocated less efficiently. However, even if rationing is efficient, the minimum wage can still entail other efficiency costs (e.g., from employment reductions).

KEYWORDS: rationing, minimum wage, job allocation, misallocation cost, allocative efficiency

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*J.F. Kennedy School of Government, Harvard University, Cambridge, MA 02138, National Bureau of Economic Research, and IZA. E-mail: erzo_luttmer@harvard.edu. I would like to thank David Cutler, Thomas DeLeire, Martin Feldstein, Edward Glaeser, Caroline Hoxby, Lawrence Katz, Ellen Meara, two anonymous referees, and seminar participants at Harvard and Chicago for suggestions and helpful discussions. I would like to thank Andra Hibbert for excellent research assistance. Financial support from the Olin Foundation and the National Institute on Aging through Grant Number T32-AG00186 is gratefully acknowledged. All errors are my own.

1. Introduction

Debates about the minimum wage tend to center on the relative magnitudes of its redistributive benefits and unemployment costs. The debate about the employment effects of the minimum wage has been especially heated (see, Burkhauser et al., 2000; Card, 1992; Card and Krueger, 1994, 1995; Card et al., 1994; Deere et al., 1995; Katz and Krueger, 1992; Neumark and Wascher, 1992, 1994, 2000; and Singell and Terborg 2007). Brown (1999) and Neumark and Wascher (2007) give comprehensive overviews of the debate on the possible employment cost of the minimum wage.

However, the costs of the minimum wage are not necessarily limited to unemployment. The minimum wage also interferes with the allocative function of the labor market, which could lead to an inefficient allocation of workers to jobs (Friedman and Stigler, 1946; Lott, 1990). Even if the increase in the minimum wage has no impact on total employment, it can cause inefficient rationing. This happens if workers who were unwilling to work at the old and lower minimum wage end up in jobs that otherwise would have been filled by workers of the same skill level who are willing to work at the old minimum wage.[1] In this case, workers with reservation wages below the old minimum wage are displaced by workers with reservation wages above the old minimum wage.[2] The deadweight loss of such inefficient rationing is equal to the difference between the reservation wages of these two types of workers and, as Glaeser and Luttmer (2003) point out, this deadweight loss is typically a first-order loss. In practice, of course, the minimum wage can also affect the level of employment. As explained in more detail in Section 2 below, the extent to which the minimum wage induces inefficient rationing can be inferred from changes in reservation wages after controlling for the change in reservation wages that can be accounted for by the change in employment.

Theory cannot tell us *a priori* whether rationing will be efficient or not. Rationing is efficient if, for each group of people with the same marginal product,

[1] Since the market for minimum wage jobs has a lot of turnover, the rationing process could also operate through the job retention process in addition to the hiring process. For example, a minimum wage increase could reduce the efficiency of rationing by increasing the job retention of those with relatively high reservation wages more than the job retention of those with relatively low reservation wages.

[2] I use the term reservation wage to denote the wage that exactly compensates the employee for the disutility of effort because the welfare consequences of a job allocation are based on the disutility of effort in that job allocation. If a person has a one-shot choice of accepting a job or not, the lowest wage at which this person will accept the job equals the disutility of effort. However, if the person can continue to search and wage offers are stochastic, the lowest wage at which a job will be accepted is equal to the disutility of effort *plus* the option value of continued search.

all non-employed people have a reservation wage that equals or exceeds the reservation wage of any employed person. In other words, it must be impossible to increase social welfare by interchanging employed and non-employed individuals with the same skill level. This concept of efficient rationing takes employment at each level of skill as given and therefore does not include the efficiency cost of the minimum wage possibly leading to a substitution of higher-skilled workers for lower-skilled workers.[3] In addition, it does not include potentially important costs of the rationing process itself such as queuing costs (see Barzel, 1974, and Suen, 1989). Rationing may result in an efficient job allocation if those with the lowest reservation wages are willing to spend more effort in activities that increase their chance of getting a job. However, rationing will be inefficient if employers randomly select employees with heterogeneous reservation wages from an excess supply of potential employees of the same skill. Inefficient rationing also results if, for some reason, those with high reservation wages have an edge in obtaining jobs over persons with low reservation wages.[4,5]

This paper examines empirically whether higher minimum wages reduce the efficiency of the allocation of jobs.[6] Misallocation and rent-seeking costs of price controls have been empirically estimated in other settings, most notably for the gasoline market by Deacon and Sonstelie (1989) and Frech and Lee (1987), for the housing rental market by Glaeser and Luttmer (2003), and for the natural gas market by Davis and Kilian (2007). Linneman (1982) and Lang and Kahn (1998) give evidence that minimum wages affect the composition of employment, hinting that the rationing process may favor certain types of workers. Palda (2000) simulates the deadweight loss of rationing under the assumption that rationing is random. To my knowledge, however, this is the first study that empirically examines the relationship between the minimum wages and inefficient job rationing.

Because the minimum wage is only binding for a relatively small group of unskilled individuals, it is necessary to identify these individuals accurately in

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[3] The benefit of using a rationing concept that takes employment at each skill level as given is that it eliminates the need to estimate the effect of a change in employment at one skill level on the marginal product of workers of other skill levels who are substitutes or complements in the production function.

[4] For example, this could be the case if persons who live in a household with another working adult both have a relatively high reservation wage and have an edge in getting a job due to the connections of the working member in their household.

[5] To the extent that we know from other sources that some of these inefficient rationing mechanisms are being used, we know that rationing cannot be perfectly efficient. The empirical approach of this paper focuses instead on the effect of *increasing* the minimum wage on the efficiency of the resulting job allocation.

[6] Holzer, Katz and Krueger (1991) find that minimum wage jobs attract more applicants than jobs that pay either slightly more or slightly less. This constitutes the most direct evidence that the minimum wage leads to rationing.

order to obtain precise estimates of minimum wage effects. This paper develops a novel measure of skill to identify these individuals. Rather than measuring skill by characteristics such as age, education, industry or occupation, this paper uses market wages. More specifically, for a given state and year, a person's wage is a reflection of her skill as valued by the market. Over time, however, the wage that a person with a constant skill level earns may change for many reasons, such as changes in technology or in the minimum wage. Thus, to use the wage as a measure of skill, it is important to net out these state-specific changes in the returns to skill. I use the overlapping panel nature of the Current Population Survey (CPS) to estimate state-specific changes in returns to skill and then use these estimates to infer skill from wages in different years. Using these wage-based skill measures, I divide the working population into four skill groups: *unskilled*, *low-skilled*, *semi-skilled* and *skilled*. The skill levels of these groups correspond respectively to the first, second, third, and top seven wage deciles of the 1989 state wage distribution.

The 1990/91 federal minimum wage increase (from \$3.45 to \$4.25) had a greater impact in some states than in others because of differences in state minimum wages, skill composition, and nominal returns to skill. I measure the degree of the impact by the fraction of workers in each state earning from \$3.35 to \$4.24 per hour in 1989. The impact of the federal minimum wage increase varied not only across states but also across skill groups within a given state. I use these differences across skill groups as a specification check on the estimates of the efficiency of job rationing.

Using data from the merged outgoing rotation groups of the Current Population Survey of 1989 and 1992, I find that employment for the unskilled group fell significantly in states where the minimum wage impact was large compared to states where it was relatively small. This is consistent with findings on the effects of the 1979-80 minimum wage increases by Currie and Fallick (1996), who also use a wage-based measure of skill. However, this employment reduction among the unskilled seems to be largely offset by increased employment among low-skilled workers, who are likely to be close substitutes for the unskilled. This finding implies that, for a more broadly defined group of less skilled workers (which includes both unskilled and low-skilled workers), no large negative employment impact can be found. The prior evidence on the effects of the minimum wage on more broadly defined groups of less skilled workers (such as teenagers) is mixed and controversial. For example, using cross-state comparisons, Card (1992) finds no effect of the 1989 minimum wage increase on teenage employment, while Neumark and Wascher (1992) find significant disemployment effects using state-level panel data.

Because reservation wages are not available in the CPS and because self-reported reservation wages may themselves be affected by the minimum wage, I instead use proxies that are likely to be correlated with reservation wages within skill groups, such as the potential income of other household members. In a scenario with inefficient rationing, the average reservation wage of unskilled workers rises as the minimum wage enables individuals with higher reservation wages to displace workers with lower reservation wages.[7] I find, however, that the average reservation wage of unskilled workers fell between 1989 and 1992 in the states where the minimum wage had the greatest impact relative to other states. This fall in the average reservation wage is statistically significant for two of the four reservation wage proxies and cannot be explained by changes in the level of employment of unskilled workers. This finding suggests that the minimum wage increase did not lead to additional inefficient rationing. The average reservation wage proxies did not fall significantly for any of the other skill groups in the high-impact states but showed a significant increase in some cases. It therefore seems unlikely that exogenous state-specific shocks to reservation wage proxies can explain the relative decrease in the average reservation wage of unskilled workers, unless these shocks only affected unskilled workers but not other types of workers. Additional alternative explanations are explored in section 4.2, but I conclude these are unlikely to explain the findings.

A caveat to these results is that they are based on a relatively modest minimum wage increase and only examine changes in the allocation of jobs over a four-year period. They do not rule out the possibility that the minimum wage could lead to inefficient rationing over a longer time period or that a larger minimum wage increase could cause inefficient rationing. In addition, I do not measure the cost of any effort that workers might undertake in order to get the rationed minimum wage jobs (such as queuing costs). Finally, I only estimate the efficiency of rationing *within* skill groups and thus do not estimate the efficiency cost of any changes in the skill composition of the work force due to the minimum wage.

While the lack of evidence of inefficient rationing bears favorably on using the minimum wage as an instrument for income redistribution, the estimates of the employment effects of the minimum wage suggest caution. I find that employment among the unskilled drops significantly in states where the minimum wage had the greatest impact. This loss in employment seems to be largely offset by a gain in employment among the low-skilled. Hence, while total employment may not be significantly affected, some of the least skilled members of society are likely to be hurt by the minimum wage.

[7] Using self-reported reservation wages from another data set, the Panel Study of Income Dynamics, I find empirical support for the reservation wage proxies.

2. Theoretical Framework

It has long been known that one of the potential costs of imposing wage or price controls is that jobs or goods may no longer be allocated to those who value them most (Friedman and Stigler, 1946). Various authors have presented theoretical analyses of misallocation and rent-seeking costs of controls that prevent prices from clearing the market (Barzel, 1974; Weitzman, 1977; Suen, 1989; and Palda, 2000). Building on this work, I present a simple theoretical framework that generates predictions, which in turn enable us to draw inferences from the empirical results about the efficiency of rationing.

The efficiency of rationing is a concern in markets in which there is heterogeneity across individuals in their valuation of the good (or job). If all individuals had the same valuation, then any allocation across individuals would be equally efficient. In the labor market, the degree to which an individual values a job is given by her reservation wage for that job. For simplicity, I assume that jobs are homogeneous. This implies that an individual's reservation wage is the same for all jobs, which rules out efficiency losses from misallocation within a given set of working individuals and a given number of jobs.[8]

Consider a segment of the labor market in which all individuals have the same level of skill, i.e., they are equally productive from the perspective of a firm.[9] Let there be a continuum of these individuals, who are indexed by their reservation wage θ.[10] Their cumulative distribution is given by $G(\theta)$ and their density by $g(\theta)$. Each individual either works full-time or is not employed. To measure the efficiency of the allocation of these individuals to a *given* number of jobs, it is useful to introduce an allocation function $p(\theta)$. This function denotes for each value of θ the fraction of individuals with that reservation wage holding a job. An efficient allocation of jobs requires (and implies) that every person holding a job values this job more than any individual without a job. Hence:

$$\text{Efficient job allocation} \quad \Leftrightarrow \quad p(\theta) = \begin{cases} 1 \text{ for } \theta \leq \theta^* \\ 0 \text{ for } \theta > \theta^* \end{cases} \quad \text{for } \theta^* = G^{-1}(L) \qquad (1)$$

[8] In the case of heterogeneous jobs, even if the people with the lowest reservation wages obtain jobs, they might not be matched to the jobs that they value most. This type of misallocation can be an additional efficiency cost of price controls. I cannot test if the allocation of workers across jobs is efficient because that also requires information about job characteristics, which is not available in the data used. However, Glaeser and Luttmer (2003) find that this type of misallocation constitutes a large fraction of the misallocation costs of rent-control.

[9] This assumption will be relaxed later to analyze how the imposition of a minimum wage can affect skill groups for which the minimum wage is not binding.

[10] I assume that the private reservation wage equals the social reservation wage. In other words, there is no externality from a job being taken by one individual over another.

with L denoting total employment. In a competitive market, θ^* would equal the market-clearing wage.

If the market is distorted, the welfare loss per working person from a misallocation of jobs is found by comparing social welfare for the observed job allocation, $p(\theta)$, to the efficient allocation:

$$\frac{DWL(p(.))}{L} = \frac{1}{L}\int_{-\infty}^{\infty} \theta p(\theta) g(\theta) d\theta - \frac{1}{L}\int_{-\infty}^{G^{-1}(L)} \theta g(\theta) d\theta \qquad (2)$$

Equation 2 shows that the deadweight loss per job from an inefficient job allocation can be decomposed into two terms: (i) the actual average reservation wage of all working individuals and (ii) the average reservation wage of working individuals under the efficient allocation. This second term does not depend on the allocation function, only on total employment (L) and the population distribution of reservation wages ($g(.)$). Rearranging and totally differentiating (2) shows that the change in the average reservation wage of individuals is given by:

$$d\begin{pmatrix} \text{average reservation wage} \\ \text{of working individuals} \end{pmatrix} = d\left(\frac{DWL}{L}\right) + \alpha dL \quad \text{where} \quad \alpha = \frac{1}{L^2}\int_{-\infty}^{G^{-1}(L)} (G^{-1}(L)-\theta) g(\theta) d\theta > 0 \qquad (3)$$

According to equation 3, the changes in the average reservation wage of working individuals that cannot be explained by changes in employment indicate a change in the efficiency with which jobs are allocated. To investigate whether the minimum wage causes inefficient rationing, the change in the average reservation wage of working individuals is regressed on the impact of the minimum wage and a control for changes in employment. A positive coefficient on the impact of the minimum wage on reservation wage indicates that a minimum wage increase leads to a higher deadweight loss of job rationing.

In a labor market with multiple segments, each consisting of workers with roughly the same level of skill, this analysis can be extended to apply to each segment. Because labor of a certain skill level may be a complement or substitute for labor of other skill levels, employment changes in one segment may affect labor demand in the other segments. Through this mechanism, the effects of an increase in the minimum wage will not remain limited to those skill segments where the increase is binding. If the minimum wage reduces employment in the unskilled segment and labor demand is not completely inelastic, then labor demand will increase for those skill levels that are substitutes for the unskilled workers. It is plausible that the closest substitutes for the unskilled workers are low-skilled workers. In this case, demand for low-skilled workers would increase, possibly increasing their wages such that the minimum wage is no longer binding for them.

If nearby skill groups are sufficiently close substitutes, the links between the different skill segments of the labor market have two important implications. First, rationing only takes place among unskilled workers. Therefore, it is important to identify the unskilled accurately when testing for changes in the efficiency of rationing. Second, the employment effects of the minimum wage are likely to be of opposite signs for unskilled and low-skilled workers. Hence, the measured employment effects of the minimum wage on a group of workers that includes both unskilled and low-skilled workers are likely to be less pronounced. This, too, suggests that it is necessary to have a precise measure of skill in order to estimate whether the minimum wage causes a loss of jobs among the unskilled individuals.

3. Data and Empirical Methodology

3.1 Data

The empirical methodology consists of two main steps. The first step estimates a so-called wage evolution curve for each state. These curves will allow us to identify workers with the same level of skill, as measured by the wage they receive and adjusted for the fact that the relationship between wages and skill may change over time. This enables us to identify the group of workers that is most affected by the minimum wage (the "unskilled"). The first step is explained in more detail in Section 3.2 below. The second step estimates how the increase in the federal minimum wage affected employment and the efficiency of rationing of unskilled individuals, and this step is explained in detail in Section 3.4 below. Because only the first step requires that individuals appear in two consecutive years, different samples are used for these two steps.

The data used to estimate the wage evolution curves come from the NBER extracts of the merged outgoing rotation groups of the Current Population Survey for 1989 to 1992. Because approximately half the individuals who are in the outgoing rotation group in one year reappear in the next year's outgoing rotation group, it is possible to construct three overlapping panels by matching individuals who appear in two consecutive years. Each panel contains approximately 115,000 persons, consisting of individuals that are matched in two consecutive years. Because many of these individuals, especially those over 65, have a missing or allocated wage in one or both years, the sample on which the wage evolution curves are based consists of approximately 45,000 individuals in each of the three overlapping panels. Appendix A describes the matching procedure.

Table 1: Summary statistics

	1989		1992	
	Mean	Std. dev.	Mean	Std. dev.
Other earnings in household (percentile)	0.500	0.289	0.500	0.289
No. of other employed persons in household	0.766	0.715	0.744	0.708
No. of other adults in household	0.944	0.682	0.955	0.684
Age < 30	0.361	0.480	0.335	0.472
Male	0.463	0.499	0.466	0.499
Black	0.126	0.332	0.127	0.333
Married	0.564	0.496	0.558	0.497
High school dropout	0.232	0.422	0.213	0.409
High school graduate	0.354	0.478	0.348	0.476
Some college	0.237	0.425	0.256	0.436
College and higher	0.178	0.382	0.183	0.387
wage < 3.35	0.015	0.122	0.011	0.103
3.35 ≤ wage < 4.25	0.061	0.240	0.009	0.094
4.25 ≤ wage < 5.00	0.039	0.194	0.055	0.229
5.00 ≤ wage < 6.00	0.072	0.258	0.067	0.251
6.00 ≤ wage < 7.00	0.064	0.244	0.061	0.239
7.00 ≤ wage < 8.00	0.058	0.234	0.055	0.227
8.00 ≤ wage < 9.00	0.053	0.224	0.052	0.222
9.00 ≤ wage < 10.00	0.035	0.184	0.036	0.186
10.00 ≤ wage < 15.00	0.158	0.365	0.160	0.366
15.00 ≤ wage < 20.00	0.068	0.252	0.081	0.274
20.00 ≤ wage	0.049	0.216	0.070	0.254
Not working	0.327	0.469	0.344	0.475
Number of observations	233,251		238,901	

1) The universe for the NBER extracts of the CPS Merged Outgoing Rotation Groups consists of all non-institutionalized persons of age 16 and older. The 1989 sample was selected as follows (with 1992 figures between parentheses): 1,822 (1,594) observations were dropped because their sampling weight is missing. Last, 597 (496) observations are dropped because their wage is lower than 1.00$/hr (which is probably measurement error) and 39,067 (39,209) observations are dropped because they are working but their wage is missing or allocated.

2) The variable *Other earnings in household* measures the total labor income (usual weekly earnings) of other household members divided by the total number of household members in the sample. This variable is expressed as a percentile in each state-year cell. Adults are defined as persons of age 20 and older. The variables *No. of other employed persons in household* and *No. of other adults in household* are topcoded at 2 to prevent outliers from driving results. All the remaining variables are dummy variables.

The CPS merged outgoing rotation groups also provide the sample used to examine the effects of the minimum wage on employment and the efficiency of rationing. This sample combines the outgoing rotation groups from 1989 with those from 1992. The sample is limited to individuals between the ages of 16 and 65 and excludes working people with missing wages, allocated wages, or wages below $1.00/hr (which are likely to be measurement error). The wage is measured as the hourly wage for hourly workers and as usual weekly earnings divided by usual weekly hours for salaried workers. The sample size for 1989 and 1992 combined is 472,152 observations. Table 1 shows the means and standard deviations for the variables in this sample.

3.2 Formation of Skill Groups

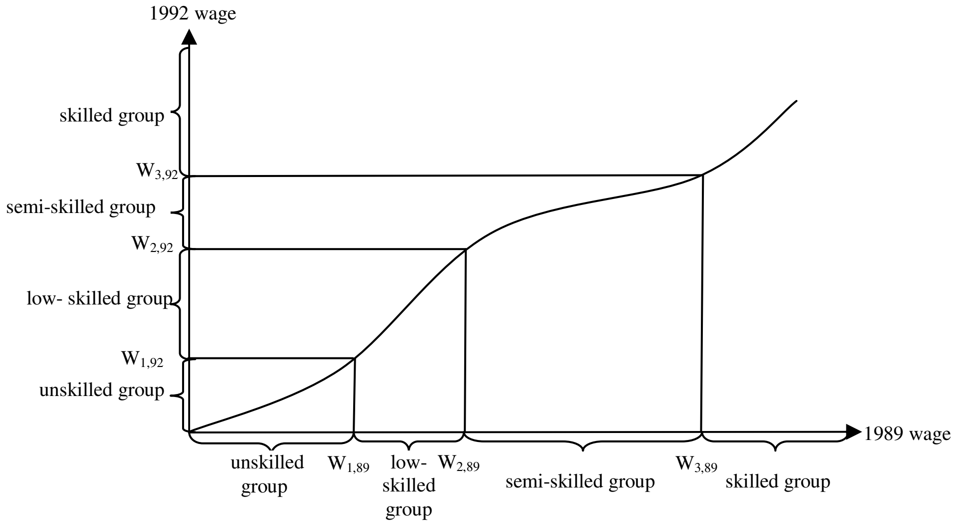
Because the minimum wage mainly affects low-wage workers, the accuracy of estimates of minimum wage effects increases with the precision with which these individuals are identified. Researchers often use demographic characteristics, such as education or age, to identify these individuals. In the context of this paper, however, using age to identify low-wage workers limits our ability to use age as a proxy for reservation wages (since in a group defined by age, the average age cannot, by definition, vary much). Therefore, I only use education as the demographic characteristic to identify low-wage workers and create three education groups: high school dropouts, high school graduates and those with some college or more.

In order to identify those affected by the minimum wage more precisely than is possible with education, this paper develops a methodology that exploits wage data to identify these individuals.[11] If we define skill such that wages are strictly increasing in skill within each state-year cell, then the working population can be divided into skill groups based on their observed wages in a given state before the minimum wage increase. However, due to the increase in the minimum wage and other shocks to the labor market, the wages that correspond to each skill level likely change over time. Thus, for each skill group, we must identify those individuals, in the period after the minimum wage increase, who would have earned the same wage as members of that skill group in the period before the minimum wage increase. In order to create groups with constant skill levels over time, one needs to know the 1989 wage and the 1992 wage that correspond to each skill level. Plotting these two wages for all skill levels in a given state yields a so-called "wage-evolution" curve for that state. As illustrated in Figure 1, the wage evolution curve tells us how the boundary wages between the skill groups in 1989 ($w_{1,89}$, $w_{2,89}$, and $w_{3,89}$) correspond to boundary wages in

[11] Indeed, results presented in Section 4.1 indicate that education is a fairly imprecise indicator for those most affected by the minimum wage.

1992 ($w_{1,92}$, $w_{2,92}$, and $w_{3,92}$). These boundary wages in 1992 can then be used to construct the skill groups in 1992 that have the same level of skill as the corresponding group in 1989.

Figure 1: Example of a state-specific wage evolution curve



To construct a wage evolution curve, we need two consecutive years of wage observations for individuals with a constant skill level. It seems likely, however, that the skill level of workers observed in two consecutive years of the CPS increases because their work experience increases by one year. To correct for the increase in the wage that results from the increase in experience, I estimate cross-section regressions of log wage on a spline in age. I run separate regressions with state fixed effects for each sex-race-education cell.[12] The sample is restricted to workers with wage observations in both years to ensure compatibility with the sample used for the wage evolution curves. Hence, the following OLS regressions are run for each sex-race-education cell (indexed by k):

$$\ln wage_{isk} = \delta_{sk} + \sum_{j=15,19,23,\ldots,67} \max(age_{isk} - j, 0)\beta_{kj} + \varepsilon_{isk} \quad \text{for } k=1, 2, 3, \ldots, 16 \qquad (4)$$

where i indexes individuals and s indexes states. The dependent variable is the log wage in the base year and the independent variables are a full set of state

[12] There are two sex categories, two race categories (black and non-black) and four education categories (high school dropout, high school graduate, some college, and college or more) yielding 16 sex-race-education cells.

dummies (δ_{sk}) and a spline in age with breakpoints every four years.[13] The error term is denoted by ε_{isk}.

The estimates $\hat{\beta}_{kj}$ are used to adjust the second year wage for the increase in skill associated with one extra year of experience. Therefore, we have two wage observations for worker i: $wage_{i,t}$, the actual wage earned in year t, and $\hat{wage}_{i,t+1}$, an estimate of the wage the worker would have earned in year t+1 if his skills had remained constant.[14] These wage pairs are used to estimate a separate wage evolution curve for each state. The functional form used for the wage evolution curve is an 11-segment spline in the log of wages:

$$\ln \hat{wage}_{i,s,t+1} = \alpha_{st} + \sum_{j=1}^{11} \max(\ln wage_{ist} - \ln \kappa_j, 0)\gamma_{jst} + \eta_{ist} \tag{5}$$

where i indexes individuals, s indexes states and t indexes years. The breakpoints of the spline are denoted by ln κ_j and the error term is given by η_{ist}.[15]

The slope of the wage evolution curve may be too flat because of attenuation bias caused by measurement error in the wage in the base year. To mitigate this bias, I ran exactly the same procedure in reverse, which yields an estimate of the inverse of the wage evolution curve. This estimate is biased towards zero as well. Under the assumption that the distribution of measurement error is constant over time, the biases have the same size, and it is possible to obtain an unbiased estimate of the wage evolution curve by combining the biased estimate of the wage evolution curve with the biased estimate of its inverse.[16] Using the procedure described above, three wage evolution curves are estimated for each state: for 1989-90, 1990-91 and 1991-92. For each state, these three curves are combined to form a single wage evolution curve for 1989 and 1992, as shown in Figure 1.

[13] In cases with fewer than 25 observations between two breakpoints, breakpoints were removed such that each spline segment was based on at least 25 observations.

[14] Specifically, $\hat{wage}_{i,t+1}$ is calculated as $\hat{wage}_{i,t+1} = wage_{i,t+1} * \exp(-\hat{\beta}_{kj})$ where $wage_{i,t+1}$ is the observed wage for individual i in year t+1 and $\hat{\beta}_{kj}$ is the estimated regression coefficient in equation (4) for the age category and sex-race-education cell of individual i.

[15] The breakpoints in the spline, ln κ_j, occur at hourly wages of $0.99, $ 3.34 $4.24, $5.49, $6.99, $8.49, $9.99, $12.49, $14.99, $17.49, $19.99 and $25.00. If necessary, spline segments are combined to ensure at least 25 observations in each segment. However, the breakpoints at $3.35 and $4.25 are never removed.

[16] If the attenuation bias in the wage evolution curve and its inverse are the same, any point (x,y) on the *unbiased* curve will show up as $(x,y+v)$ on the biased wage evolution curve and as $(x+v,y)$ on the biased inverse wage evolution curve, where v is the attenuation bias at point (x,y). The unbiased wage evolution curve can therefore be found by numerically determining the set of points (x,y) for which there exists some v such that $(x,y+v)$ lies on the biased wage evolution curve and $(x+v,y)$ on the biased inverse wage evolution curve.

Table 2: Minimum wage impact measures and group boundaries

State	Impact of min. wage (fraction of workers in 1989 with: $3.35 \leq wage < 4.25$)	Wage boundary between unskilled and low-skilled workers		Wage boundary between low-skilled and semi-skilled workers		Wage boundary between semi-skilled and skilled workers	
		1989	1992	1989	1992	1989	1992
AK	0.017	5.50	5.30	7.00	8.72	8.50	9.75
CT	0.022	5.49	5.64	6.83	7.08	8.00	9.89
CA	0.024	4.50	4.54	5.50	6.28	6.75	7.54
MA	0.031	5.00	5.16	6.50	6.92	7.50	8.49
NH	0.032	5.00	5.09	6.00	6.15	7.00	7.07
RI	0.033	4.75	5.59	5.50	6.18	6.50	7.52
NJ	0.035	5.00	5.84	6.25	7.38	7.50	8.34
DC	0.036	5.00	5.77	6.00	6.71	7.15	7.65
VT	0.051	4.50	5.17	5.50	6.54	6.27	7.27
MD	0.052	4.75	5.29	6.00	6.75	7.14	8.43
DE	0.053	4.50	4.80	5.50	6.17	6.50	7.42
ME	0.054	4.50	4.59	5.41	5.76	6.00	6.25
NY	0.062	4.50	5.07	5.63	6.20	7.00	7.98
HI	0.068	4.50	4.99	5.50	6.62	6.67	8.39
WA	0.074	4.35	5.08	5.40	7.11	6.51	7.73
NV	0.077	4.50	5.24	5.28	6.44	6.50	7.71
MN	0.079	4.25	4.80	5.25	5.59	6.25	7.06
PA	0.082	4.10	4.39	5.10	5.71	6.22	7.42
VA	0.082	4.05	4.40	5.10	5.84	6.27	6.86
OR	0.087	4.10	4.93	5.21	6.34	6.25	7.74
IL	0.092	4.00	4.57	5.00	5.67	6.25	7.43
FL	0.096	4.00	4.51	5.00	5.77	5.74	6.55
GA	0.097	4.00	4.86	5.00	5.56	5.95	6.46
MI	0.098	4.00	4.60	5.00	5.90	6.15	7.28
NC	0.107	4.00	4.64	5.00	5.53	5.63	6.17
CO	0.109	4.00	5.35	5.00	6.01	6.00	6.73
OH	0.109	4.00	4.74	5.00	5.69	6.00	7.10
AZ	0.111	4.00	4.88	5.00	5.92	5.95	7.00
UT	0.113	4.00	4.55	4.90	5.79	5.63	6.74

Table 2, continued

State	Impact of min. wage (fraction of workers in 1989 with: 3.35≤wage<4.25)	Wage boundary between unskilled and low-skilled workers 1989	1992	Wage boundary between low-skilled and semi-skilled workers 1989	1992	Wage boundary between semi-skilled and skilled workers 1989	1992
WI	0.113	4.00	4.41	4.83	6.06	5.75	6.76
IN	0.117	4.00	4.79	4.75	5.51	5.50	6.23
MO	0.122	3.65	4.17	4.65	4.98	5.50	6.29
KA	0.131	3.90	4.40	4.61	5.09	5.60	6.19
WY	0.134	3.60	4.20	4.50	5.29	5.27	6.68
IA	0.136	3.80	4.73	4.50	5.45	5.32	6.34
TX	0.141	3.65	4.06	4.50	5.31	5.25	6.35
SC	0.142	3.75	3.88	4.50	4.99	5.25	6.17
ID	0.149	3.65	4.48	4.25	5.02	5.00	5.72
TN	0.150	3.80	4.64	4.50	5.37	5.25	6.08
NE	0.154	3.75	4.47	4.46	5.37	5.10	6.06
OK	0.155	3.65	4.32	4.50	5.45	5.25	6.54
AL	0.158	3.75	4.18	4.38	4.66	5.00	5.54
MT	0.163	3.57	4.10	4.30	5.02	5.13	5.95
ND	0.163	3.75	4.46	4.40	5.16	5.00	5.89
LA	0.166	3.50	4.20	4.13	4.85	5.00	5.66
KY	0.169	3.50	3.74	4.15	5.63	5.00	6.17
NM	0.171	3.50	2.99	4.22	4.73	5.00	5.11
AR	0.182	3.50	4.13	4.15	4.92	5.00	6.24
SD	0.185	3.50	3.38	4.00	4.68	5.00	6.07
WV	0.185	3.35	4.30	4.00	4.40	5.00	5.35
MS	0.203	3.35	3.62	4.00	4.93	4.63	5.57
Mean:	0.105	4.10	4.63	5.01	5.79	5.95	6.88
Std.dev.	0.051	0.53	0.58	0.72	0.81	0.88	1.03

1) The variable *Impact of minimum wage* is a measure of the impact of the 1990/91 federal minimum wage increase on a state. It is measured as the fraction of workers between the ages of 16 and 65 whose wage in 1989 was greater than or equal to $3.35/hr and strictly less than $4.25/hr. The wage is measured as the hourly wage for hourly workers and as usual weekly earnings divided by usual weekly hours for salaried workers.
2) The wage boundary between unskilled and low-skilled workers in 1989 is the wage at the 10th percentile of the wage distribution in the relevant state. The wage boundary between these two skill categories in 1992 is found by estimating what the wage in that state would be in 1992 for a worker of the same skill as a worker earning the boundary wage in 1989. This estimation procedure is described in detail in section 3.2.

The working population is divided into four skill groups: in 1989, the *unskilled,* the *low-skilled*, the *semi-skilled* and the *skilled* group consist respectively of workers in the first, second, third and in the top seven deciles of the 1989 state wage distribution. The wage evolution curve is used to find the 1992 wages that separate these four skill levels. In 1992, workers are classified into the four skill groups based on their 1992 wage and these boundary wages. Table 2 shows the boundary wages that separate these four skill groups in 1989 and in 1992 for each state.[17] These four skill groups are an input into the method of estimating whether an increase in the minimum decreases the efficiency of the job allocation, i.e., they are needed for the second step of the estimation procedure, which is explained below in more detail in Section 3.4.

3.3 Reservation wage proxies

For jobs to be allocated efficiently within a group of equally skilled workers, they must be allocated to those individuals who have the lowest reservation wages. The CPS does not ask about reservation wages, and even if it had asked about them, we would be concerned that the minimum wage directly affects a worker's reported reservation wage, even if that worker's disutility of effort remains unchanged. Thus, to test for inefficient job rationing, I instead use variables that are correlated with reservation wages conditional on skill level. Based on theoretical considerations, I identify the following four reservation wage proxies:

(1) *Labor earnings of other household members*. This proxy variable is likely to be positively correlated with reservation wages because the hardship of non-employment tends to be lower if other household members have more

[17] One might be concerned about the ability of the wage evolution curve approach to handle spikes in the wage distribution, in particular spikes at the minimum wage. While spikes cause a concentration of observations at particular wage levels in a diagram with this year's wage on one axis and next year's wage on the other axis, such a concentration would not bias a regression of next year's wage on the current year's wage. In other words, wage spikes will not bias the estimated wage evolution curves. The main complication of spikes in the wage distribution comes at the point where the labor force in 1992 is divided into skill groups based on the boundary wages, as estimated by the wage evolution curves. While there is hardly ever a spike at these 1992 boundary wages itself (it therefore hardly matters whether the boundary is inclusive or not), small variations in the estimated boundary wage can cause the whole spike to fall either in the skill group below the boundary or above the boundary. While there is no reason to believe that there is a systematic relationship between the impact of the minimum wage on a state and inaccuracies in the wage boundary causing wage spikes to fall on a particular side of the boundary in that state, I recognize that the estimates of the effect of the minimum wage on employment would be sensitive to any such a systematic relationship. Thus the employment effect results should be interpreted with caution (as I also stress in section 4.1). The estimates of the effect of the minimum wage on the efficiency on rationing are relatively insensitive to any bias in the size of the unskilled group because these estimates also include a control for the size of the unskilled group. They are therefore the primary focus of this paper.

income. Ideally non-labor earnings of other household members would also be included, but they are not available in the CPS outgoing rotation groups.

(2) *The number of other employed persons in the household.* This proxy variable is expected to be positively correlated with reservation wages because the value of household production is likely to be higher if other household members are spending time at work. Moreover, the hardship of non-employment is likely to be lower because the other household members provide earnings. This proxy will be stronger if the variation comes from additional employed adults in the household rather than employed children, who likely contribute relatively little to household income.

(3) *The number of other adults in the household*, where adults are defined as people aged 20 and older. Because other adult household members could potentially find a reasonably paying job, this variable is likely to be positively correlated with reservation wages for the reasons mentioned above. This proxy will be stronger if the other adults are not too old to work productively.

(4) *Younger than 30.* Teenagers and people in their twenties are likely to have a higher reservation wage (for a given skill level) because of schooling opportunities available to them, the possibility of parental support and fewer financial commitments such as mortgages.[18]

A number of surveys have asked individuals about their reservation wage; these responses can be used to test the validity of these proxies.[19] In practice, three important issues complicate validating the reservation wage proxies. First, some skepticism seems justified concerning the degree to which reservation wages can be measured by the answer to *"What is the lowest wage or salary you would accept on any job?"* Individuals may not know the answer, may engage in wishful thinking, or may understate their reservation wage to show they are truly unemployed and rightfully claim unemployment benefits (especially if they believe the government may obtain their answer). Moreover, the questions are usually vague about the job characteristics and opportunities for further search. Second, the question is typically asked of a select sample such as unemployed individuals who are actively seeking work. Third, care must be taken to control adequately for skill, otherwise reservation wage proxies may not reflect the

[18] The choice of the exact age cutoff is admittedly arbitrary. Rather than trying to fine-tune this cutoff, I chose to use a round number.

[19] Other studies have used self-reported reservation wages to analyze search behavior and the effects of government actions on unemployment. These studies include Kiefer and Neumann, 1979 (using a survey conducted by Pennsylvania State University), Feldstein and Poterba, 1984 (using a 1976 CPS supplement), Holzer, 1986 (using the NLSY) and Jones, 1989 (using a 1982 survey conducted by the Economist Intelligence Unit). As an alternative approach, Hofler and Murphy (1994) use a stochastic frontier regression technique to infer reservation wages from a sample of employed workers. None of these studies confirm or reject the proposed proxies for reservation wage conditional on skill.

individual's time and effort costs of working but merely reflect the earnings opportunities corresponding to the unobserved skill level of that individual.

Despite these caveats, it remains valuable to examine whether my reservation wage proxies receive empirical support. Using the self-reported reservation wages from the Panel Study of Income Dynamics, I find that, conditional on skill, each of the four proxies is positively correlated with self-reported reservation wages. Moreover, the correlation is statistically significant at the 5% level for all but the third proxy (*No. of other adults in the household*). The validation results are described in detail in appendix B.

3.4 Inferring Rationing from Differential Changes in Reservation Wages

The federal minimum wage increased from \$3.35/hr to \$3.80/hr in April 1990 and was raised to \$4.25 in April 1991. I measure the *impact* of the federal minimum wage increases on a state by the fraction of workers in that state who earn an hourly wage between \$3.35 and \$4.24 in 1989. This impact measure is listed for each state in the first column of table 2 and varies from 1.7% for Alaska to 20.3% for Mississippi. The measure is similar to the one used by Card (1992), except that he only considered teenage workers, whereas my sample includes all workers.

The impact of the federal minimum wage differs, not only across states, but also across skill groups. In particular, the impact is greatest on unskilled workers. This variation in the impact of the minimum wage increase across states and skill groups is used to test the effect of minimum wages on the efficiency of rationing.

First, for expositional ease, consider an increase in the minimum wage in a state where employment remains constant (e.g., because labor demand is perfectly inelastic). Under efficient rationing, the average reservation wage of the employed should not be affected. Under inefficient rationing, however, we would expect the average reservation wage of workers to increase because some non-employed people with high reservation wages take jobs that otherwise would have been held by people with lower reservation wages. We would expect the increase in average reservation wages to show up most strongly for unskilled workers because the increase in the minimum wage affects them most. Next, consider a minimum wage increase in a state where employment also changes. Under efficient rationing, a decrease in employment should decrease the average reservation wage of workers because those with the highest reservation wages are rationed out. Inefficient rationing, however, leads to reservation wages that are higher than what one would have expected the reservation wages to be given the change in employment in the unskilled group.

To examine whether an increase in the minimum wage leads to inefficient rationing, the following regressions are run:

$$\Delta(reservation\ wage)_{sk} = \alpha_k + impact_s \beta_k + \Delta groupsize_{sk} \gamma_k + v_{sk} \quad (6)$$

where s indexes states and k indexes wage-based skill groups or education groups. A separate regression is run for each of these groups. The dependent variable is the change between 1989 and 1992 in the average reservation wage (as measured by one of the four proxies) in group k in state s. The key explanatory variable is $impact_s$, which measures the impact of the federal minimum wage increase on state s by the fraction of workers in that state earning between \$3.35 and \$4.24 in 1989. I instrument $impact_s$ by its own value lagged one year to rule out the possibility that any of the results are driven by random over or under sampling in 1989 of certain subgroups of the population.[20] Random over or under sampling of subgroups that constitute a disproportionate share of low-wage workers and have a higher or lower than average reservation wage would create a mechanical correlation between $impact_s$ and changes in the reservation wage proxies. The second independent variable is $\Delta groupsize_{sk}$, which measures the change between 1989 and 1992 in the size of group k in state s as a fraction of the working-age population in that state. This variable controls for any changes in the average reservation wage that can be attributed to changes in the group's employment rate. The error term is denoted by v_{sk}.

Theory predicts that γ_k should be positive for the employed groups and negative for the non-employed groups if the efficiency of rationing remains constant, but because omitted variables could both affect $\Delta groupsize$ (by affecting the employment rate) and changes in reservations wages, I do not interpret this coefficient causally. If the minimum wage increase has no impact on the efficiency of rationing, theory predicts that β_k should be zero for all groups. If the minimum wage increase exacerbates inefficient rationing, β_k is positive for the unskilled group but equal to zero for the other employed groups for whom the minimum wage is not binding. Under inefficient rationing, the average reservation wage of non-employed unskilled individuals decreases. However, because non-employed unskilled individuals constitute only a fraction of the non-employed group, it is doubtful that this effect can be tested using the estimate of β_k for the non-employed group. Following from these predictions, estimates of β_k that are only positive for unskilled workers but not for the other groups would indicate that a minimum wage increase reduces the efficiency of the allocation of jobs.

[20] In particular, when I calculate the lagged value of $impact_s$, I only use observations in the 1988 CPS sample that are in their second interview year, which ensures that they are not part of the 1989 CPS sample.

For the education groups, theory predicts that inefficient rationing will lead to a positive β_k for the employed and that β_k will be larger for less educated workers than for more educated workers. Under inefficient rationing, β_k will be negative for the non-employed and β_k will be more negative for less educated individuals. Finally, one can control for reservation-wage shocks that are unrelated to the minimum wage increase and specific to education groups and states by taking the difference between the β_k of employed and non-employed individuals in each education group. Hence, under inefficient rationing, this difference ($\Delta\beta_k$) should be positive and decreasing with education.

4. Empirical Results

4.1 Effects on Wages and Employment

Before turning to the main question of whether the increase in the minimum wage led to inefficient rationing, I present estimates of the impact of the minimum wage on wages and employment. For wages, these estimates serve as a joint check on the impact measure and the estimates of the wage evolution curves. For employment by skill level, these estimates of the impact of the minimum wage are of direct interest to policy.

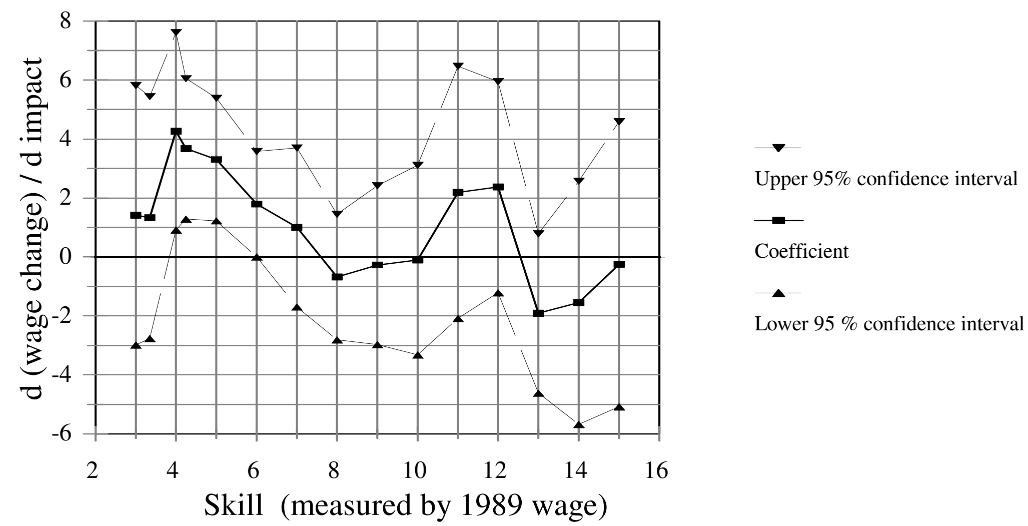
To examine whether wages evolved differently in states where the impact of the federal minimum wage increase was relatively large, the following regression was run for each skill group:

$$\Delta wage_{sk} = \alpha_k + impact_{\mathrm{s}}\, \beta_k + \varepsilon_{sk}\ , \tag{7}$$

where $\Delta wage_{sk}$ is the change in the wage (in \$) for a worker of skill k (as measured by the 1989 wage) in state s and $impact_{\mathrm{s}}$ is the impact of the federal 1990/91 minimum wage increase on state s (as measured by the fraction of workers in 1989 with wages between \$3.25 and \$4.24). For each skill level, a separate cross-section IV regression is run with 51 observations. The impact measure is instrumented by its own lag of one year to rule out the possibility of a mechanical correlation due to sampling variation.

Figure 2 graphs the coefficients β_k and the corresponding confidence intervals, which are based on Huber/White robust standard errors. The figure shows that the coefficients corresponding to skill levels of \$3.00 to \$3.35 are not significantly different from zero. This result should come as no surprise because the wages of workers in these skill levels must be raised to the new legal minimum of \$4.25 independently of the fraction of workers who are affected by this change. The coefficients corresponding to skill levels from \$4.00 to \$5.00 are significantly positive. This finding is consistent with the case that workers with skill levels in the \$4.00 to \$5.00 range are close substitutes for workers with skill levels around \$3.35. In states where a larger fraction of workers are affected by the minimum wage increase, one would expect a bigger drop in employment among unskilled workers, which increases the wages of workers who are close substitutes for them. The magnitude of the coefficient, about 4, implies that the wages of workers with skill levels between \$4.00 and \$5.00 increased by about 75 cents more in the highest impact state, Mississippi, than in the lowest impact state, Alaska. The wages of workers of skill levels corresponding to \$6.00 and up are not significantly affected by the impact of the minimum wage on their state.

Figure 2: Impact of minimum wage on wage evolution



This graph depicts the differential impact of the federal minimum wage increase on the wage distribution across states. The graph plots the coefficients β_k and the corresponding confidence intervals of the following regressions:

$\Delta wage_{\mathrm{sk}} = \alpha_{\mathrm{k}} + impact_{\mathrm{s}}\, \beta_k + \varepsilon_{s,}$

where $\Delta wage_{sk}$ is the change in the wage (in \$) for a worker of skill k (as measured by the 1989 wage) in state s and $impact_{\mathrm{s}}$ is the impact of the federal 1990/91 minimum wage increase on state s as measured by the fraction of workers in 1989 with wages between \$3.25 and \$4.24. For each skill level, a separate cross-section IV regression is run with 51 observations. The impact measure is instrumented by its own lag of one year. The confidence interval is based on Huber/White robust standard errors.

To examine the effect of the minimum wage increase on employment, I classified individuals into wage-based skill groups and into education groups, as described in section 2. Table 3a shows the summary statistics for the four wage-based skill groups and for the group of non-employed individuals. This table

Table 3a: Summary statistics by wage-based skill group

	Wage-based skill groups:				
	Unskilled workers	Low-skilled workers	Semi-skilled workers	Skilled workers	Non-employed Persons
1989					
Other earnings in household (percentile)	0.514	0.522	0.519	0.518	0.463
No. of other employed persons in household	0.978	0.962	0.893	0.756	0.673
No. of other adults in household	1.139	1.094	1.022	0.894	0.934
Age < 30	0.644	0.563	0.484	0.266	0.379
Male	0.373	0.384	0.422	0.582	0.333
Black	0.132	0.149	0.141	0.098	0.158
Married	0.306	0.398	0.480	0.654	0.531
High school dropout	0.406	0.310	0.222	0.103	0.373
High school graduate	0.316	0.395	0.432	0.359	0.329
Some college	0.224	0.238	0.265	0.260	0.199
College and higher	0.054	0.056	0.081	0.278	0.100
1992					
Other earnings in household (percentile)	0.522	0.514	0.513	0.522	0.462
No. of other employed persons in household	0.967	0.908	0.849	0.737	0.657
No. of other adults in household	1.149	1.094	1.007	0.897	0.954
Age < 30	0.609	0.537	0.431	0.220	0.371
Male	0.403	0.415	0.426	0.570	0.363
Black	0.127	0.147	0.139	0.095	0.162
Married	0.313	0.402	0.503	0.665	0.508
High school dropout	0.352	0.282	0.181	0.079	0.354
High school graduate	0.343	0.393	0.441	0.338	0.331
Some college	0.246	0.266	0.280	0.285	0.212
College and higher	0.059	0.060	0.097	0.297	0.102

1) The formation of skill groups is described in detail in Section 3.2.
2) The variable *Other earnings in household* measures the total labor income of other household members divided by the total number of household members. This variable is expressed as a percentile (on a 0 to 1 scale) in each state-year cell. Adults are defined as persons of age 20 and older. The variables *No. of other employed persons in household* and *No. of other adults in household* are topcoded at 2 to prevent outliers from driving results. All the remaining variables are dummy variables.

Table 3b: Summary statistics by education group

	Employed			Non-employed		
	High school dropout	High school graduate	Some college or more	High school dropout	High school graduate	Some college or more
1989						
Other earnings in household (percentile)	0.489	0.509	0.534	0.437	0.461	0.502
No. of other employed persons in household	0.899	0.816	0.776	0.704	0.633	0.678
No. of other adults in household	1.116	0.952	0.889	1.046	0.855	0.880
Age < 30	0.419	0.337	0.340	0.465	0.273	0.388
Male	0.599	0.505	0.519	0.394	0.263	0.332
Black	0.139	0.121	0.093	0.209	0.143	0.110
Married	0.497	0.616	0.580	0.392	0.661	0.561
Unskilled	0.223	0.077	0.053	n/a	n/a	n/a
Low-skilled	0.193	0.109	0.063	n/a	n/a	n/a
Semi-skilled	0.138	0.119	0.074	n/a	n/a	n/a
Skilled	0.446	0.694	0.809	n/a	n/a	n/a
1992						
Other earnings in household (percentile)	0.490	0.506	0.539	0.441	0.450	0.495
No. of other employed persons in household	0.883	0.794	0.762	0.707	0.613	0.645
No. of other adults in household	1.143	0.962	0.898	1.090	0.883	0.875
Age < 30	0.402	0.297	0.306	0.487	0.271	0.345
Male	0.599	0.510	0.507	0.411	0.316	0.359
Black	0.142	0.122	0.091	0.207	0.157	0.115
Married	0.489	0.609	0.592	0.366	0.614	0.557
Unskilled	0.215	0.081	0.051	n/a	n/a	n/a
Low-skilled	0.259	0.140	0.082	n/a	n/a	n/a
Semi-skilled	0.140	0.133	0.080	n/a	n/a	n/a
Skilled	0.386	0.645	0.786	n/a	n/a	n/a

See notes to Table 3a.

shows that unskilled workers are disproportionately young, female, single, less educated and live in households with more other adults. While unskilled workers have the highest rate of high school dropouts, the other skill groups still have substantial high school dropout rates. The same holds for the other dimensions along which unskilled workers are over-represented. This implies that skill groups based on any of these other variables would span a relatively large range of skills as measured by the wage employers are willing to pay these individuals. Table 3b shows the summary statistics for the education groups. It shows that employed high school dropouts are disproportionately young, male, black, single and from households with more other adults. This reveals some striking differences between the high school dropouts and the unskilled. High school dropouts are disproportionately male whereas unskilled workers are disproportionately female. Unskilled workers are also much younger and more often single than high school dropouts. Finally, the fact that less than 25% of the high school dropouts earn wages in the bottom wage decile implies that even high school dropouts span a considerable range of skills as measured by wages.

Table 4 shows how the level of employment for each skill group is affected by the impact of the federal minimum wage increase. Panel A shows the results for the wage-based skill groups. It shows that the unskilled group experienced a large and significant decline in employment in states where more workers were covered by the federal minimum wage increase compared to other states. Employment among the low-skilled, in contrast, showed a significant increase in those states relative to the other states.[21] This is the expected result if the low-skilled are close substitutes for unskilled workers. Employment in the other two skill groups also shows a relative increase in the high-impact states, but this increase is not significant. It seems very unlikely that the large relative decrease in the fraction of non-employed persons in high-impact states can be explained by the minimum wage. Rather, it seems that high-impact states happened to experience favorable economic shocks relative to low-impact states. This makes the drop in employment among the unskilled even more striking. Apparently this drop occurred in spite of relatively favorable economic conditions in those states. These results indicate that, while the minimum wage may only have a minor employment impact on a broadly defined group of less skilled workers, it has a large negative impact on employment among the least skilled workers. The apparent coincidence of relatively favorable macro conditions in

[21] This may be one of the reasons why studies that identified the unskilled by demographics, education or occupation, which are likely to be noisy measures of skill, did not tend to find negative employment effects (e.g., see Card, 1992; Katz and Krueger, 1992; Lang and Kahn, 1998). Studies that did find negative employment effects often based skill groups on wage information, which may identify the unskilled and most affected individuals more precisely. (e.g., see Abowd et al., 1999; Currie and Fallick, 1996; and Linneman, 1982).

Table 4: Effect of minimum wage on employment by skill & education groups

Dependent variable:	Impact of minimum wage (fraction in 1989 with: 3.35 ≤ wage <4.25) Coefficient	(S.E.)	Implied percentage point change in skill group size in MS versus AK	R^2	N
Panel A: *By wage-based skill group:*					
Δ Fraction employed unskilled persons	-0.172**	(0.066)	-3.2	0.081	51
Δ Fraction employed low-skilled persons	0.202**	(0.087)	3.8	0.099	51
Δ Fraction employed semi-skilled persons	0.157	(0.097)	2.9	0.058	51
Δ Fraction employed skilled persons	0.085	(0.076)	1.6	0.038	51
Δ Fraction non-employed persons	-0.272**	(0.050)	-5.1	0.399	51
Panel B: *By education group:*					
Δ Fraction employed high school dropouts	0.030	(0.026)	0.6	0.030	51
Δ Fraction employed high school graduates	0.141**	(0.043)	2.6	0.191	51
Δ Fraction employed with some college or more	0.101*	(0.053)	1.9	0.071	51
Δ Fraction non-employed high school dropouts	-0.112**	(0.037)	-2.1	0.171	51
Δ Fraction non-employed high school graduates	-0.093**	(0.031)	-1.7	0.177	51
Δ Fraction non-employed with some college or more	-0.066**	(0.025)	-1.2	0.084	51

1) Huber/White robust standard errors are reported. Significance levels: * 10 percent; ** 5 percent.
2) The dependent variable is the percentage point change between 1989 and 1992 in the size of the group as a fraction of the working-age population (ages 16-65).
3) The independent variable is *Impact of minimum wage*, which is a measure of the impact of the 1990/91 federal minimum wage increase in each state. It is measured as the fraction of workers between the ages of 16 and 65 whose wage in 1989 was greater than or equal to \$3.35/hr and strictly less than \$4.25/hr. The wage is measured as the hourly wage for workers for hourly workers and as usual weekly earnings divided by usual weekly hours for salaried workers.
4) Each line corresponds to a separate cross-section regression with the 51 states as observations. In each regression, *Impact of minimum wage* is instrumented for by its value lagged one year. The lagged value of this impact measure is calculated using only CPS observations that are in their second interview year, i.e., those that cannot appear in the next year.
5) The last column shows the predicted differential effect of the federal minimum wage increase on the size of the various skill groups in Mississippi compared to Alaska.

states most affected by the 1990-91 minimum wage increase may also be part of the explanation why studies using a broader definition of skill groups (e.g., Card, 1992) do not find a negative impact on employment.

These employment results depend on the accuracy of the estimates of the wage evolution curves. If, for some reason, the wage evolution curves systematically underestimate the wage increase for unskilled workers in high-impact states, too few individuals will be classified as unskilled in those states. While figure 2 shows that the wage evolution curves estimate a relatively large wage increase for the unskilled in the high-impact states, one should be aware of the sensitivity of the employment effects to any possible bias in the wage evolution curves. For this reason, the primary contribution of this paper is the evidence on the effect of the minimum wage on the efficiency of rationing rather than the evidence on the employment effects of the minimum wage.

The results for the education groups are shown in panel B. Overall employment in high-impact states rose relative to low-impact states, but this rise should probably be attributed to relatively favorable economic conditions in high-impact states rather than the minimum wage since we also find employment increases for groups unlikely to be affected by the minimum wage. While the relative employment increase in high-impact states was less pronounced for high school dropouts than for high school graduates, much of this difference arises because employed high school dropouts comprise a smaller fraction of the working-age population (10%) than employed high school graduates (25%). It is especially striking that the fraction of non-employed high school dropouts fell sharply in the high-impact states compared to low-impact states. Because this fall is nearly four times as large as the employment increase among high school dropouts in high-impact states, many of the non-employed high school dropouts in high-impact states must either have migrated to low-impact states or obtained high school degrees. Thus, when estimates are based on education groups, no clear negative employment impact of the minimum wage on low-wage workers is apparent. However, this lack of an impact may be due to the fact that low-wage workers cannot be identified with enough precision by education groups.

4.2 Effects on the Efficiency of Rationing

To examine whether the increase in the minimum wage led to a less efficient allocation of jobs among unskilled workers, I regress a proxy for the average reservation wage of unskilled workers in each state on the impact of the minimum wage increase in that state, controlling for changes in the level of employment among the unskilled (see equation 6). The results of this regression are reported for each of the four reservation wage proxies in the first column of panel A of table 5. Similar regressions are run for the other skill groups to check for exogenous state-specific movements in reservation wage proxies that happen to be correlated with the impact measure. These results are reported in the remaining columns.

The table shows that the minimum wage had a negative effect on the average reservation wage in the unskilled group according to all four proxies. This decline is significant at the 5% level for two of the proxies and marginally significant for one.[22] The coefficient on the change in employment among the unskilled (Δ *group size*) is positive, as theory would predict, for three of the four proxies but is not significant for any of them. These movements in the average reservation wage suggest that the allocation of jobs among unskilled workers, if anything, became relatively more efficient in those states where the minimum wage had the largest impact. These results suggest that the increase in the minimum wage increase did not lead to more inefficient rationing.

In order to get some sense of the magnitude of the estimated impact on the reservation wage proxies, it is instructive to convert these proxies into reservations wages. Multiplying the coefficient $\partial proxy/\partial impact$ from table 5 with the coefficient $\partial \ln(reservation\ wage)/\partial proxy$ from appendix table B.1 yields an estimate of $\partial \ln(reservation\ wage)/\partial impact$, or the implied effect of the minimum wage increase on log reservation wages. I calculate this implied effect separately for each of the four reservation wage proxies. Though this conversion exercise relies on rather strong assumptions (in particular, that the whole impact is captured by the proxy), it is reassuring that the implied effects are small and consistent for the four proxies. The four point estimates suggest that compared to Alaska (the lowest impact state), the average reservation wage of unskilled workers in Mississippi (the highest impact state) changed between -0.3 and -0.1 percentage points due to the minimum wage increase, with the four 95% confidence intervals covering a range from -0.7 to 0.2.

An alternative explanation for these results is that the reservation wage proxies showed a relative decline in the high-impact states for some reason unrelated to the minimum wage increase. In this case, however, one would expect to find negative coefficients on the impact measure for all skill groups. Instead for three of the four proxies, these coefficients are positive for the low-skilled group, which is the group that is most similar to the unskilled.[23] Hence, it seems

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[22] Lang and Kahn (1998) find that the employment composition of less skilled workers moved towards teenage and student workers in states where the minimum wage impact was largest. Their finding contrasts with my finding that the minimum wage increased the fraction of individuals older than 30 among the unskilled workers. Perhaps this difference is due to the measure of skill. Lang and Kahn identify less skilled workers as those employed in the eating and drinking establishments and working in food service occupations.

[23] To test whether the reservation wages of unskilled workers fell relatively more in the high impact states after controlling for any common shocks to the lowest two skill groups in each state, I ran the following regression:

unlikely that an exogenous relative decline in reservation wage proxies in the high-impact states can explain the results, unless this decline affected unskilled workers without affecting other skill groups.

Another alternative explanation for these results is a bias in the formation of the skill groups. In particular, the relative reservation wage of unskilled workers could fall in the high-impact states if low-skilled workers have lower reservation wages and a greater proportion of low-skilled workers are misclassified as unskilled in the high-impact states in 1992.[24] Three of the four reservation wage proxies are indeed lower for the low-skilled than for the unskilled (as table 3 shows). It seems unlikely, however, that a greater proportion of low-skilled workers are misclassified as unskilled in the high-impact states in 1992 because, as table 4 shows, these states had a relative increase in the number of individuals classified as low-skilled and a relative decrease in those classified as unskilled.

Panel B of table 5 examines whether the change in the demographic composition of the skill groups is also correlated along other dimensions with the impact of the minimum wage increase.[25] The first column shows that only the fraction of high school dropouts is significantly related (at the 5% level) to the impact of the minimum wage. The negative coefficient on high school dropouts suggests that the minimum wage may have led to a relative increase in the average level of education of unskilled workers in high-impact states. This might be an indication that some variation in skill levels is present even within the unskilled group. If this is the case and the least skilled within the unskilled group lost their jobs due to the change in minimum wage, then the average education level among unskilled workers would indeed increase. Though there is a significant relation between some of the demographic proxies and impact of the minimum wage for the other skill groups, the difference in the impact of the minimum wage on the demographic composition of unskilled and low-skilled workers is insignificant for all demographic characteristics except for the fraction

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$$\Delta(res.wage)_{sk} = \delta_s + unskilled_k\alpha + impact_s * unskilled_k\beta + \Delta groupsize_{sk}\gamma_1 + \Delta groupsize_{sk} * unskilled_k\gamma_2 + v_{sk} \qquad (8)$$

where $unskilled_k$ is a dummy variable for unskilled workers, δ_s is a set of state fixed effects and the remaining variables are the same as in equation (6). The regression is run for unskilled and low-skilled workers combined and thus has 102 observations. The direct effect of $impact_s$ is absorbed by state fixed effects. The coefficient β is significantly negative for all four reservation wage proxies, confirming that the relative drop in the reservation wages of unskilled workers in the high impact states cannot be driven by shocks to the two lowest skill groups that happened to be correlated with the impact of the minimum wage increase.

[24] Note that, for such misclassification to occur, the bias in the estimates of the wage evolution curves would need to be in the opposite direction of the bias needed to explain the employment results.

[25] This was tested using the same specification as in regression (8), except that the reservation wage proxy was replaced by a demographic characteristic.

Table 5: Effect of minimum wage on employment composition by skill group

Dependent variable:	Independent variables:	Wage-based skill groups: Unskilled workers	Low-skilled workers	Semi-skilled workers	Skilled workers	Non-employed persons
***Panel A:** Reservation wage proxies:*						
1) Δ Other earnings in household (percentiles, on 0-1 scale)	Impact of min. wage	-0.216**	0.114	0.148*	-0.013	-0.006
		(0.088)	(0.076)	(0.083)	(0.029)	(0.058)
	Δ Group size	-0.127	-0.164	-0.089	-0.075	0.098
		(0.190)	(0.191)	(0.141)	(0.053)	(0.102)
2) Δ No. of other employed persons in household	Impact of min. wage	-0.150	0.693**	0.548*	0.256*	-0.199
		(0.269)	(0.222)	(0.322)	(0.139)	(0.155)
	Δ Group size	0.005	0.222	0.196	0.131	-0.719**
		(0.531)	(0.588)	(0.556)	(0.228)	(0.311)
3) Δ No. of other adults in household	Impact of min. wage	-0.709**	-0.102	-0.150	-0.093	-0.400*
		(0.305)	(0.236)	(0.321)	(0.106)	(0.229)
	Δ Group size	0.314	0.171	0.176	0.149	-0.727
		(0.587)	(0.509)	(0.547)	(0.138)	(0.499)
4) Δ Fraction with age < 30	Impact of min. wage	-0.341*	0.076	-0.011	-0.064	-0.124
		(0.180)	(0.166)	(0.151)	(0.059)	(0.079)
	Δ Group size	0.533	0.248	0.401	0.420**	-0.315*
		(0.326)	(0.268)	(0.252)	(0.123)	(0.188)

See notes on next page.

Table 5, continued

Dependent variable:	Independent variables:	Wage-based skill groups: Unskilled workers	Low-skilled workers	Semi-skilled workers	Skilled workers	Non-employed persons
***Panel B:** Other demographic characteristics*						
5) Δ Fraction male	Impact of min. wage	-0.237*	-0.240	-0.248*	0.120**	-0.235**
		(0.133)	(0.157)	(0.132)	(0.057)	(0.082)
	Δ Group size	0.023	0.225	0.218	-0.082	-0.002
		(0.249)	(0.329)	(0.244)	(0.102)	(0.174)
6) Δ Fraction black	Impact of min. wage	-0.008	0.072	-0.016	-0.028	-0.138
		(0.089)	(0.143)	(0.123)	(0.065)	(0.116)
	Δ Group size	-0.064	0.187	-0.272	0.043	-0.146
		(0.304)	(0.201)	(0.202)	(0.101)	(0.265)
7) Δ Fraction married	Impact of min. wage	0.313*	-0.389**	-0.240**	0.020	-0.158
		(0.160)	(0.192)	(0.097)	(0.078)	(0.121)
	Δ Group size	-0.186	-0.221	-0.240	-0.527	-0.250
		(0.358)	(0.257)	(0.211)	(0.150)	(0.278)
8) Δ Fraction high school dropout	Impact of min. wage	-0.322**	0.048	-0.095	0.133**	-0.005
		(0.160)	(0.125)	(0.090)	(0.032)	(0.136)
	Δ Group size	-0.380	0.629**	0.075	0.212**	0.261
		(0.396)	(0.225)	(0.263)	(0.085)	(0.279)

1) Huber/White robust standard errors are reported. Significance levels: * 10 percent; ** 5 percent.
2) The dependent variable is the change in the characteristic listed in the first column between 1989 and 1992 for each skill group.
3) The independent variable are *Impact of minimum wage*, which is a measure of the impact of the 1990/91 federal minimum wage increase in each state, and *Δ Group size*, which is the change between 1989 and 1992 in the size of the relevant skill group as a fraction of the working-age population (ages 16-65). *Impact of minimum wage* is measured as the fraction of workers between the ages of 16 and 65 whose wage in 1989 was greater or equal than \$3.35/hr and strictly less than \$4.25/hr.
4) For each skill group and dependent variable, a separate cross-section regression is estimated with the 51 states as observations. In each regression, I instrument the *Impact of minimum wage* by its value lagged one year. The lagged value of this impact measure is calculated using only CPS observations that are in their second interview year, i.e., those that cannot appear in the next year. Observations are weighted by the harmonic mean of the number of observations in the two years of each state-skill group cell.

married. Therefore, it seems unlikely that the differential change in the reservation wage proxies for the unskilled in the high-impact states can be explained by random shocks that happened to affect unskilled but not low-skilled workers in those states.

The results from the wage-based skill groups therefore indicate that the allocation of employment seems to have improved for unskilled workers in states where the impact of the minimum wage was high relative to other states. This finding provides evidence against the hypothesis that the increase in the federal minimum wage led to an increase in the amount of inefficient rationing in the labor market.

To test the sensitivity of the results to the definition of skill groups, table 6 shows the results for education groups. The first column tests the effect of the minimum wage change on reservation wage proxies for high school dropouts, the group most likely to be affected by the minimum wage. There are no significant effects for any of the four proxies, though the estimate is positive for three of the proxies and marginally significantly so in two of the cases. Thus, while these estimates are not inconsistent with the findings using the wage-based skill groups, the point estimates generally hint at an increase in average reservation wages among high school dropouts, which would indicate more inefficient rationing. An advantage of using education groups is that it allows one to compare the impact of the minimum wage on employed persons to the impact on non-employed persons. In this way, one can control for exogenous shocks to reservation wages that are common to employed and non-employed individuals. The last three columns show the impact of the minimum wage on the reservation wage proxies of the employed compared to the non-employed. Among high school dropouts, this differential impact is significantly positive for two of the proxies. The evidence from the education-based skill groups on the effect of the minimum wage on the efficiency of rationing is therefore more mixed, with some evidence suggesting that the minimum wage has an adverse effect on the efficiency of rationing. As argued earlier based on evidence from table 3, being a high school drop out is a relatively noisy proxy for having skill level corresponding to wage that is affected by the minimum wage. This may temper one's confidence that the estimated effects on the reservation wage proxies of high school dropouts are causal effects of the minimum wage.

Table 6: Effect of minimum wage on employment composition by education

Dependent variable:		Independent variables:	Employed: High school dropout	Employed: High school graduate	Employed: Some college or more	(Employed) – (Non-employed): High school dropout	(Employed) – (Non-employed): High school graduate	(Employed) – (Non-employed): Some college or more
Reservation wage proxies:								
1)	Δ Other earnings in household (percentiles, on 0-1 scale)	Impact of min. wage	0.023	0.005	-0.005	0.133	0.047	-0.029
			(0.059)	(0.043)	(0.043)	(0.105)	(0.066)	(0.108)
		Δ Group size	0.629	0.011	-0.107	0.433	0.010	-0.087
			(0.559)	(0.152)	(0.094)	(0.765)	(0.327)	(0.274)
2)	Δ No. of other employed persons in household	Impact of min. wage	0.516*	0.372**	0.119	0.593**	0.378*	-0.039
			(0.261)	(0.181)	(0.136)	(0.285)	(0.189)	(0.191)
		Δ Group size	2.290	0.415	0.313	1.874	-1.217	0.691
			(1.877)	(0.408)	(0.396)	(1.960)	(1.023)	(0.485)
3)	Δ No. of other adults in household	Impact of min. wage	-0.194	-0.137	-0.096	-0.017	0.133	0.023
			(0.226)	(0.177)	(0.145)	(0.261)	(0.201)	(0.186)
		Δ Group size	0.168	0.620**	-0.170	0.140	-2.190*	0.358
			(1.497)	(0.307)	(0.364)	(1.964)	(1.192)	(0.473)
4)	Δ Fraction with age < 30	Impact of min. wage	0.237*	-0.101	-0.036	0.306**	0.015	0.090
			(0.123)	(0.086)	(0.090)	(0.150)	(0.107)	(0.173)
		Δ Group size	0.001	0.264	0.239	0.039	-0.471	-0.352
			(0.861)	(0.335)	(0.203)	(1.297)	(0.406)	(0.401)

1) Huber/White robust standard errors are reported. Significance levels: * 10 percent; ** 5 percent.
2) The dependent variable is the change in the characteristic listed in the first column between 1989 and 1992 for each education group.
3) The independent variable are *Impact of minimum wage*, which is a measure of the impact of the 1990/91 federal minimum wage increase in each state, and *Δ Group size*, which is the change between 1989 and 1992 in the size of the relevant skill group as a fraction of the working-age population (ages 16-65). *Impact of minimum wage* is measured as the fraction of workers between the ages of 16 and 65 whose wage in 1989 was greater or equal than \$3.35/hr and strictly less than \$4.25/hr.
4) For each skill group and dependent variable, a separate cross-section regression is estimated with the 51 states as observations. In each regression, I instrument the *Impact of minimum wage* by its value lagged one year. The lagged value of this impact measure is calculated using only CPS observations that are in their second interview year, i.e., those that cannot appear in the next year. Observations are weighted by the harmonic mean of the number of observations in the two years of each state-education group cell.

5. Conclusion

In this paper, I examine the impact of the minimum wage on the level and composition of employment. Because the minimum wage primarily affects individuals whose skill level is sufficiently low so that the minimum wage is binding for them, it is important to identify these individuals accurately and precisely. I present a new methodology to identify these unskilled workers. Using the overlapping panel nature of the CPS, I estimate how the wages corresponding to a constant skill level changed over time in each state. I then use this information to infer each worker's skill from the actual wage paid to this individual.

Using this methodology to identify workers' skills, I find suggestive evidence that the 1990/91 increase in the federal minimum wage reduced employment among unskilled workers. However, their employment reduction seems to be largely compensated for by increased employment among the next skill group, which is likely to be a close substitute. Hence, for a more broadly defined group of less skilled workers, I do not find evidence of a large negative employment impact. This may help to explain why estimates relying on relatively imprecise measures of skill such as age, education, or occupation may not find large employment effects from minimum wage increases.

Contrary to what theory would predict if rationing were inefficient, I do not find significant increases in reservation wage proxies in states where the minimum wage increase had a large impact relative to states where the minimum wage increase had little impact. Thus, my findings do not suggest that the minimum wage increase led to a more inefficient rationing of jobs among unskilled workers. If anything, the allocation of jobs seems to have become relatively more efficient in states where the impact of the federal minimum wage increase was larger. In other words, those who valued their job least, as measured by four reservation wage proxies, appear to have become less likely to hold a minimum wage job.

An advantage of the wage-based skill measure is that it reflects firms' valuations of skills. By construction, the unskilled group identifies only low-wage workers, precisely those who are most affected by a minimum wage increase. However, the methodology used to construct these wage-based skill groups is relatively complicated and one might be concerned that the results could be driven by some bias in the formation of the skill groups. I show that the finding that minimum wages do not adversely affect the efficiency of rationing can only be explained by a bias in the skill groups if relatively more low-skilled workers are misclassified as unskilled in high-impact states. This, however, seems unlikely because the number of unskilled workers fell relative to low-skill workers in high-impact states.

These results have mixed implications for the desirability of the minimum wage as a policy instrument. The absence of evidence of inefficient rationing suggests that there is no need to add the deadweight loss of job misallocation to the other costs associated with minimum wages (such as any effort cost of trying to obtain the rationed jobs or the efficiency costs of employment reductions). On the other hand, the results do indicate that an increase in the minimum wage reduces the employment rate of unskilled workers. This means that policy makers who are concerned about people at the very bottom of the wage distribution should be cautious about advocating the minimum wage as an instrument for income redistribution.

Appendix A: Matching individuals in two consecutive years of the CPS

This appendix describes the procedure used to match individuals who appear in two consecutive years of the CPS. This matching procedure is not straightforward because the CPS samples residential units and does not contain person identifiers. Therefore a single residential unit identifier may correspond to two different households in cases where a household moves. The idea behind the matching procedure is to use information about the sex, race, age and relationship codes of individuals within each household to (i) assess the likelihood that a single household identifier indeed identifies the same household in both periods and (ii) to match individuals within households. Madrian and Lefgren (2000) explain the rotating design of the CPS and examine several different matching procedures. My approach is consistent with their recommendations.

The first column of table A.1 describes the matching process between 1989 and 1990. The CPS interviews individuals for four consecutive months, then waits eight months, and interviews them again for four consecutive months. The outgoing rotation groups only contain individuals in their 4^{th} or 8^{th} interview month. Therefore, only observations in their 4^{th} interview month in 1989 or in their 8^{th} interview month in 1990 need to be considered for a match. As rows 1a and 1b show, there are 335,012 such observations with 95,309 distinct household identifiers. However, dropping households that appear only in one year leaves 297,712 observations in 75,245 households (see rows 2a and 2b). Within each household, the maximum number of matches is the lowest number of observations in that household over the 2 years. As shown in row 3, this reduces the number of potential matches to 139,862 or 279,724 person-year observations.

Of these 139,862 potential matches, 122,452 unique matches were identified. Matched observations have the same household identifier, race and gender, and the age of the person must have increased by two or fewer years. Some multiple matches are resolved by requiring that the age of the person increased by exactly one year and by using the relationship codes. Multiple matches that cannot be resolved are discarded.

Even if a unique match is identified, it is unclear that the matched observations refer to the same individual. First, the same household identifier might be used for a different household if the original household could not be located by the interviewers. The matching routine rates each matched household on a five-point scale indicating the likelihood that the same household identifier refers to same household. This rating is based on the number of matched individuals in the household, the number of non-matched individuals, their ages and relationship codes.[26] Second, within the same household, the wrong individuals could be matched to each other. For example, this could happen if one person moved out and a different person of the same age, race and gender moved in. Hence, the quality of the individual matches within each household is also rated. This rating is based on the method used to resolve any multiple matches, on the relationship codes and on whether the person aged by exactly one year.

These subjective quality ratings are validated using education for the 1989/90 and the 1990/91 matches. The education validation is not possible for 1991/92 because the education definitions changed. As expected, the validations show that matches with higher quality ratings are less likely to have incompatible education levels, where incompatible education levels are those that decrease over time or increase by more than two years. These validations are used to determine the quality ratings of the household and individual match required for a "high-quality" match. As rows 4a and 4b show, 92.6% of the unique matches in 1989/90 are considered high-quality. Of these high-quality matches, 2% have incompatible education levels, whereas nearly 10% of the low-quality matches have incompatible education levels (see rows 5a and 5b). Of the high-quality matches, even the most highly rated matches still have a rate of incompatible education levels of 1.9% (not shown in the table). This raises the suspicion that the base rate of misreporting education levels may be close to 1% per year. In this case, the number of false positive matches may be substantially lower than the 2% suggested by the education validation. On the other hand, some false positive matches may not be detected by the education validation because both individuals happen to have the same level of education.

[26] The routine that rates match quality was constructed to mimic my subjective assessment. I subjectively rated a large number of matches and created an algorithm that best captured these assessments. This algorithm is too complicated to describe in full detail but is available on request.

For the estimation of the wage evolution curves, I only use high-quality matches. Row 6 shows that 45,687 matches of the 113,429 high-quality matches have non-missing wage data in both years. While this fraction may seem low, one should realize that the matches also include the elderly population and that imputed or allocated wages are treated as missing.

Table A.1: Match quality in the CPS Merge

		1989/90	1990/91	1991/92
1)	In 4^{th} sample month in 1^{st} year or in 8^{th} sample month in 2^{nd} year			
1a)	Households	95,309	95,398	93,630
1b)	Person-year observations	335,012	340,855	334,242
2)	Of which the same household identifier occurs in both years			
2a)	Households	75,245	77,947	76,728
2b)	Person-year observations	297,712	309,031	303,520
3)	Potential matches (based on no. obs. in matched households)	139,862	145,154	142,675
4)	Unique matches (based on sex, race, age and relationship codes)	122,452	127,135	125,281
4a)	Of which match quality is low	9,023	9,197	9,053
4b)	Of which match quality is high	113,429	117,938	116,228
	(as % of potential matches)	(81.1 %)	(81.3 %)	(81.5 %)
	(as % of unique matches)	(92.6 %)	(92.8 %)	(92.8 %)
5)	Validation based on education			
5a)	Low-quality matches: % incompatible education	9.7 %	8.7 %	n/a
5b)	High-quality matches: % incompatible education	2.0 %	1.9 %	n/a
6)	High-quality matches with non-missing wages in both years	45,687	46,526	45,704

Appendix B: Validation of Reservation Wage Proxies

In this appendix, I examine the empirical validity of the reservation wage proxies using the self-reported reservation wage measure from the Panel Study of Income Dynamics. The National Longitudinal Study of Youth also has a self-reported reservation wage, but the PSID was used because its sample is more representative. The reservation wage in the PSID is measured as the response to the question: *"What is the lowest wage or salary you would accept on any job?"* This question was only asked of family heads who are not working, not temporarily laid off and who have "done anything in the last four weeks to find a job."[27] Moreover, the question was only asked from 1980 to 1987, and the sample was restricted to individuals between the ages of 16 and 64 for comparability with the CPS sample. This leaves a sample of 3308 person-year observations. Eliminating individuals with missing education information reduces the sample to 3275 person-year observations on 1861 distinct individuals.

We would expect a valid proxy to explain the self-reported wage after controlling for ability. Hence, a proxy is judged empirically valid if we find a significantly positive β in a regression of the following form:

$$\textit{self-reported reservation wage} = \alpha + (\textit{reservation wage proxy})\,\beta + (\textit{skill measures})\,\gamma + \varepsilon$$

The reservation wage proxies from PSID data were constructed to resemble as much as possible the four CPS reservation proxies used elsewhere in the paper: *Other earnings in the household (as a percentile), No. of other employed persons in household, No. of other adults in household* and *Age less than 30.* However, a number of differences were unavoidable. First, the PSID proxies are measured at the family level whereas the CPS ones are measured at the household level. Second, *Other earnings in the family* includes half of the family's asset income if a wife is present and reflects taxable income in the past calendar year, whereas in the CPS no asset income is included and earnings are based on usual weekly earnings. Like the CPS, *Other earnings in the family* excludes any income of the person to whom the reservation wage proxies apply and is scaled for family size by dividing by the number of 16-64 year olds in the family. Third, *Other earnings in the family* is expressed as a percentile in each year rather than as a percentile in each state-year cell because of sample size considerations.

To ensure that the reservation wage proxy reflects the individual's time and effort cost of working rather than an unobserved component of skill, the controls include two skill measures. The first is the average hourly earnings of the individual in the past calendar year. A dummy variable is included for the 438

[27] In 1985, the question was also asked of non-employed job-searching spouses of family heads. These individuals are not included in the analysis.

observations with missing average hourly earnings. The second control for skill is the individual's predicted wage. This prediction is based on the demographic characteristics of this individual, including the reservation wage proxies, year dummies and characteristics of the local labor market. It is important that the reservation wage proxies are included in order to control for those components of the proxies that reflect skill rather than effort and time costs of working. To predict wages, I first run an OLS regression of log average hourly earnings on these individual demographics, year dummies and local labor market conditions for a sample of 41,246 working family heads aged 16 to 64.[28] This sample is also drawn from the 1980 to 1987 waves of the PSID but does not overlap with the sample of unemployed individuals with self-reported reservation wages. Next, the coefficients of this regression are applied to the demographic and labor market characteristics of the sample with self-reported reservation wages to generate a predicted log wage.

The results of the OLS regressions of log self-reported reservation wages on the reservation wage proxies and skill measures are reported in table B.1. All four reservation proxies have a positive effect on the self-reported reservation wage and the effect is significant for all proxies except the third proxy, *No. of other adults in the family*. Hence, these proxies are not only plausible on theoretical grounds but also confirmed empirically. When all four proxies are entered jointly in the regression (not reported), the fourth proxy remains significant but the first three become insignificant. This is not surprising because the first three proxies are highly collinear. The controls for skill are highly significant and the results are as expected: more highly skilled individuals have higher reservation wages.

[28] The demographics and controls include sex, race dummies (black, white and other), marital status dummies (married/cohabitating, single, widowed and divorced), education dummies (high school dropout, high school graduate, some college, college degree or more), a six-segment linear spline in age (breakpoints at 20, 30, 40, 50 and 60), seven dummies for the number of kids under 17, nine dummies for family size, a ten-segment spline in the first reservation wage proxy (other earnings), a full set of dummy variables for the other three reservation wage proxies, year dummies and seven dummies for the county unemployment rate. The adjusted R^2 of this regression is 0.229.

Table B.1: Validation of Reservation Wage Proxies

Dependent Variable: Log self-reported reservation wage

	(1)	(2)	(3)	(4)
Reservation wage proxies:				
Proxy 1: Other earnings in the family (as percentile)	0.079			
	(0.029)			
Proxy 2: No. of other workers in the family		0.033		
		(0.013)		
Proxy 3: No. of other adults in the family			0.018	
			(0.013)	
Proxy 4: Age < 30				0.038
				(0.016)
Measures of skill				
Predicted log wage (based on demographics)	0.338	0.344	0.343	0.392
	(0.025)	(0.025)	(0.026)	(0.029)
Log average hourly earnings (in past calendar year)	0.263	0.264	0.265	0.265
	(0.016)	(0.016)	(0.016)	(0.016)
Dummy for missing average hourly earnings	-0.130	-0.130	-0.132	-0.128
	(0.015)	(0.015)	(0.015)	(0.015)
R^2	0.3323	0.3318	0.3307	0.3317
Number of observations	3275	3275	3275	3275

1) Huber/White robust standard errors in parentheses. Standard errors are corrected for group error terms for repeated observations of the same individual. The 3275 observations come from 1861 distinct individuals.
2) The dependent variable is the self-reported reservation wage, which is the answer to the question: "*What is the lowest wage or salary you would accept on any job?*"
3) The data are from the PSID. The reservation wage question was asked from 1980 to 1987 to unemployed family heads who have been looking for work in the last four weeks. For consistency with the CPS reservation wage proxies, the sample is limited to individuals aged from 16 to 64. Finally, 33 individuals with missing education information were dropped.
4) The log average hourly earnings of individuals for whom this variable was initially missing was set equal to the sample mean.

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