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NJ and PA Once Again: What Happened to Employment When the PA–NJ Minimum Wage Differential Disappeared?

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Card and Krueger's analysis of the impact of the 1992 increase in the New Jersey (NJ) state minimum wage on employment in fast-food restaurants in NJ and Pennsylvania (PA) is very well known. In 1996 and 1997, the federal minimum wage was increased from \$4.25 to \$5.15, thereby increasing the minimum wage by \$0.90 in PA but by just \$0.10 in NJ. We use CPS data to examine the impacts of this increase on employment of likely minimum wage workers in the two states, using *DID* and *DIDID* estimators that exploit within-state and between-state comparisons. We find consistent evidence that employment of "at-risk" groups was negatively affected in PA relative to other groups in PA and to comparable groups in NJ.
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INTRODUCTION

Card and Krueger's analysis of the impact of the 1992 increase in the New Jersey (NJ) minimum wage is very well known [Card and Krueger 1994; 2000]. In that year, the NJ minimum wage increased \$0.80 to \$5.05 per hour, while the minimum wage in Pennsylvania (PA) remained at \$4.25, the federal statutory rate. Card and Krueger treated the NJ increase as a kind of natural experiment and then used their own survey data to examine how employment in fast-food restaurants in PA and NJ was affected. Their result — that employment increased in NJ relative to PA — has been both widely cited and controversial since its first report.[1] President Clinton cited it in his 1995 State of the Union address as evidence that "a modest increase does not cost jobs and may even lure people into the job market" [Clinton 1995]. Other economists have critiqued its survey methodology and approach [Hamermesh 1995; Welch 1995] and its data quality [Welch 1995; Neumark and Wascher 2000].

Interestingly, the 1992 PA–NJ case was followed by another one involving the same two states. The federal minimum wage was increased in two steps, from \$4.25 to \$4.75 on October 1, 1996 and to \$5.15 on September 1, 1997. As a result, the NJ–PA minimum wage difference that had prevailed from 1992 to 1996 was eliminated. This provides an opportunity to examine the impact of the minimum wage on low-wage employment in these two states again, now using NJ as the control state for PA — just the opposite of the original Card–Krueger research. Rather than focusing on employment in a particular industry, as in Card and Krueger,[2] we examine the impact on different groups of workers, following the approach of Deere et al. [1995] who compared aggregate-level changes in employment following the 1990–1991 federal minimum wage increase for demographic groups identified by age, race, and

education. In effect, we are combining the geographic objects and case-study approach of Card–Krueger with the analytical approach of Deere, Murphy, and Welch. We are agnostic about what to expect. Traditional economic theory certainly predicts that employment in PA will fall relative to NJ for workers more likely to be affected by the minimum wage, but the Card–Krueger findings, if they could be generalized from the fast-food industry to broader employment, suggest that it will increase or at least not decrease.

After analyzing the impact of the 1996 and 1997 minimum wage increases, we use the same methodology to re-examine the impact of the original 1992 NJ minimum wage increase to see whether the employment gains found by Card and Krueger for the fast-food industry were found for other groups of likely affected workers. Finally, using both minimum wage episodes, we look, to the extent possible in CPS data, at how employment of younger and less educated workers in the restaurant occupation and industry in PA and NJ was affected by the increases in the minimum wage. Our data come from the CPS-ORG files for NJ and PA in the years before and after the two minimum wage episodes.

To analyze the impact of the increase in the minimum wage, we examine a series of "difference-in-difference" style comparisons. We look at cross-state comparisons for workers likely to be potentially affected by the increase in the minimum wage, within-state comparisons for workers likely to be differently affected by a change in the minimum wage, and a difference-in-difference-in-difference comparison that looks at impacts both within and across the two states. This estimate, which we regard as our most compelling, examines whether "at-risk" workers in PA were more affected relative to PA workers largely not at-risk than were at-risk workers in NJ relative to workers in NJ not at-risk.

Our results are quite consistent. Our NJ–PA comparisons show that the 1996 and 1997 minimum wage increases had a negative effect on employment rates in PA for groups most likely to be affected by the increase. While the effects are not always large and are not always statistically significant, they are always in the same direction and they are often stronger for more narrowly defined groups that are arguably more likely to be affected. This finding holds for all three comparison approaches. Interestingly, PA had slightly higher employment growth over this time period for groups unlikely to have been affected. Our analysis of the 1992 NJ minimum wage increase shows mixed positive and negative impacts. Finally, we also find some evidence of an age and educational attainment distribution change in employment in the restaurant industry and occupation that is suggestive of a minimum wage effect following both the 1992 and the 1996 and 1997 increases.

The next section briefly reviews the empirical literature on the new economics of the minimum wage. Our data and methods are presented in the third section. Results are presented in the fourth section.

THE NEW MINIMUM WAGE RESEARCH

The traditional time-series aggregate approach to identifying employment impacts of the federal minimum wage [Brown et al. 1983] gave way in the early 1990s to studies that exploited other sources of minimum wage variation, including state minimum wages, and examined case studies of employment in particular industries or states in response to state or federal minimum wage increases.[3] Card and Krueger's study, based on the increase in the NJ state minimum wage from $4.25 to

$5.05 in April 1992, is a very well-known and widely cited contribution to this new literature. Card and Krueger surveyed fast-food restaurants in NJ and in eastern PA in February and March 1992 (one month before the minimum wage increased) and again in November and December of that year. Approximately 400 restaurants were interviewed in both waves. Based on reported counts of FTE workers (number full-time + 0.5 × number part-time), Card and Krueger found that employment increased in NJ by 0.6 FTE workers, while falling by 2.1 FTE workers for the "control" group of restaurants in PA. These results imply that the NJ increase in the state minimum wage actually increased average fast-food employment by 2.7 FTE workers, with an implied elasticity of 0.8.[4] These results are robust to a variety of sensitivity tests. Card and Krueger suggested that monopsony issues could be part of the unexpected positive impact of a higher wage floor, but did not develop this very fully or convincingly. This literature has remained primarily empirical.[5]

Subsequent analysis by Neumark and Wascher [2000] questioned these results, placing particular emphasis on concerns about data quality and the very large standard deviations of employment in the survey data. Using payroll data, which they argued were more reliable than the survey data collected by Card and Krueger, they report a 3.9–4.0 percent decline in employment in fast-food restaurants in NJ relative to PA.[6] The corresponding demand elasticity was −0.21. Card and Krueger's [2000] own subsequent reanalysis, based on an analysis of BLS data on employment in NJ and PA in fast-food restaurants, shows a small positive effect of employment in NJ, but with standard errors that are two to four times the coefficient estimate. They concluded that "Based on all the evidence now available…the increase in the New Jersey minimum wage…had little or no systematic effect on total fast-food employment in the state, although there may have been individual restaurants where employment rose or fell in response to the higher minimum wage" [p. 1398]. Earlier criticisms by Welch [1995] and by Hamermesh [1995] focused on data quality, on timing issues, and on the exclusive focus on employment in a single industry. Welch noted that economic theory does not make an unambiguous prediction about how employment in one industry will be affected by an increase in the minimum wage if there are close substitutes in consumption (e.g., sandwich shops) that are more heavily affected by the increase because they are even more labor intensive. Hamermesh especially questioned whether the time frame of the CK analysis was long enough to capture long-run effects.

An alternative line of research looks at the impact of the minimum wage not by industry (i.e., fast-food as in not only Card and Krueger, but also Katz and Krueger [1992]), but by demographic group, usually at a national level. Deere et al. [1995] is a prominent example of this approach. They examined the employment effect of the 1990–1991 federal minimum wage increase by comparing changes in employment across groups as a function of the proportion of low-wage workers in that group. This is nearly a natural experiment approach in the sense proposed by Card [1992] in his earlier analysis of the state-by-state impact of the minimum wage: "The imposition of a national minimum wage standard provides a natural experiment in which the treatment effect varies across states depending on the fraction of workers initially earning less than the new minimum" [p. 22]. In the work of Deere, Murphy, and Welch, the "treatment effect" varies not across states but across demographic groups.[7]

Deere, Murphy, and Welch classify workers by age, race, marital status, Spanish ethnicity, and educational attainment and then compare the percentage of low-wage

earners in each sub-group category with the subsequent change in the employment rate in the 12 months following the minimum wage increase. They find a consistent negative relationship: groups with higher proportions of low-wage workers have less favorable employment growth. For example, 11 percent of black workers, 7.2 percent of white workers, and 5.4 percent of Asian workers were classified as "low-wage workers" by Deere, Murphy, and Welch. The corresponding employment changes are −4.8, 3.1, and 0.7 percent. This kind of pattern holds for all groupings considered but one and for both men and women.[8]

In a recent and extensive review of the minimum wage research literature, Neumark and Wascher [2007] note that negative employment effects are more often obtained in analyses focused on less-skilled groups and less often or rarely obtained in analyses focused on particular industries and shorter time periods. We use that guidance here, focusing on groups likely to be affected by the minimum wage and permitting sufficient response time.

METHODS AND DATA

Our analysis adapts the general methodological approach of Deere, Murphy, and Welch to revisit the NJ–PA minimum wage issue, this time taking advantage of the increases in the Federal minimum wage in 1996 and 1997 that eliminated the NJ–PA minimum wage differential. As in Card–Krueger, we use a comparison of neighboring states, which has the important potential virtue of controlling for common regional influences that might affect employment trends. Prior to the 1996 increase, NJ had the second highest state minimum wage in the country and was one of just 11 states (including Alaska and Hawaii) with a state minimum above the federal level [US DOL 2007]. We compare employment before the increase in 1995 with employment after the increase in 1998. Unlike Card–Krueger, however, we do not focus on employment in fast-food restaurants. Instead, like Deere, Murphy, and Welch, we examine impacts on groups more or less likely to have been affected by the minimum wage increase. The availability of a control group (NJ) is particularly valuable as a way of controlling for otherwise unobservable employment trends, as long as NJ and PA are similarly affected. While there is no certainty that this condition holds, their geographic proximity is a substantial advantage relative to studies at the national level or that involve multiple states that are widely scattered geographically.

We use a "difference-in-difference" approach to examine the impact of the Federal minimum wage increase on employment in PA and NJ. We consider three comparisons involving the employment rate (E) in time periods 1 and 2: (1) between-state comparisons of the form $DID_B = (E^K_{PA2} - E^K_{PA1}) - (E^K_{NJ2} - E^K_{NJ1})$ for workers in "at-risk" demographic group K in time periods 1 and 2; (2) within-state comparisons of the form $DID_W = (E^J_{S2} - E^J_{S1}) - (E^K_{S2} - E^K_{S1})$, where J and K are identifiable groups in state S that differ substantially in terms of the likely proportion of minimum wage workers; and (3) a between-state within-state comparison of the form $DIDID = [(E^J_{PA2} - E^J_{PA1}) - (E^K_{PA2} - E^K_{PA1})] - [(E^J_{NJ2} - E^J_{NJ1}) - (E^K_{NJ2} - E^K_{NJ1})]$. The first estimator ($DID_B$) examines whether similar at-risk groups (e.g., teenagers or less-educated workers) were differentially affected in the two states. The second (DID_W) examines whether different groups with likely different exposure within a state were differentially affected. The last estimator ($DIDID$) examines whether this differential

impacts on groups varied by state. We think this latter estimator is the most interesting and revealing measure.

We use primarily age and education to identify at-risk groups. We present estimates for individuals age 16–19, 16–24, and 30–49, assuming that the likely impact of the minimum wage would fall with age. We also look at employment rates for individuals with less than a high school degree and those with at least a college degree; we further consider non-teens with less than a high school degree and male non-teens with low education.

Our data come from the CPS Outgoing Rotation Group (ORG) samples for 1995 and 1998, one year before and one year after the federal minimum wage increase.[9] The ORG samples are a portion of the CPS monthly survey that includes families who are exiting the sample after either their initial four months or, following an eight-month absence, their final four months. Sample sizes are quite large. At any time, one-fourth of the CPS sample is a member of one of the ORG samples. The annual CPS-ORG samples include all 12 months of ORG interviews, so the weighted total cumulates to three times the total population. We rely on the standard CPS questions to determine employment.[10]

We limited the sample to persons between ages 16 and 59 who resided in NJ or PA. The 1995 sample includes 20,979 persons (10,232 in NJ and 10,747 in PA); the 1998 sample includes 15,667 persons (6,623 in NJ and 9,044 in PA).[11] Sample means for the NJ and PA samples are shown separately in Table 1. For most of the variables, means are very similar across the states. The employment rate for this age group is 74.1 percent in PA and 74.2 percent in NJ. About 9 percent of the sample in both states are age 16–19 and just under 20 percent are age 16–24. About one-seventh of the samples have less than a high school education. NJ has a higher minority proportion — 14.1 percent black and 11.9 percent Hispanic compared to 9.3 and 2.2 percent, respectively, in PA.[12] NJ also has a more-educated population — 28.6 percent college graduates, compared to 22.3 percent in PA. The variable means change very little over time within each state; these results are not shown in Table 1.

During the years we examine, the national unemployment rate was slowly trending downward. The unemployment rate was 5.6 percent for most of 1995 and fell a bit more than one full percentage point by 1998. The national employment rate showed a very modest increase, from 63.1 percent in March 1995 to 64.0 percent in March 1998. Unemployment declined in both NJ and PA, with PA's rate falling

Table 1 Sample means, persons age 15–59, NJ and PA, 1995 and 1998 CPS-ORG

	NJ (%)	*PA (%)*
Age 16–19	9.2	8.9
Age 16–24	19.3	19.7
Age 30–49	52.7	51.8
White	69.4	86.3
Black	14.1	9.3
Hispanic	11.9	2.3
Male	48.8	49.0
Not HS graduate	13.1	14.3
College graduate	28.6	22.3
Employment rate	74.1	74.2
Sample size	16,855	19,791

from 5.9 percent in March 1995 to 4.7 percent in March 1998 and NJ's falling from 6.4 to 4.8 percent. This difference should be viewed cautiously since the minimum wage increase could be a possible cause of the difference between the states. The employment rate for 16–59 year olds increased in NJ from 73.4 to 74.7 percent; in PA, it increased from 73.3 to 75.1 percent.

FINDINGS

Findings for our between-state estimator (DID_B) for the period 1995–1998 are presented in Table 2. The figures shown in this table and the others are employment rates; rather than showing standard errors for all of the means and the resulting estimators, we show the appropriate *t*-statistic (in parentheses) for the test of no difference in the relevant means.[13] It is most useful to start at the far right, where we show employment rates for males and females age 30–49. These groups are likely to have a very low proportion of minimum-wage workers and thus provide a good barometer of general employment changes. Baseline employment rates in the two states are very similar to one another and the changes between 1995 and 1998 are quite small. Male employment rates increased 2.24 percentage points in PA and 1.55 percentage points in NJ. Employment rates for women age 30–49 increased in both states, with a slightly larger increase for women in PA.[14] Taken together, these estimates suggest nothing particular is happening in PA relative to NJ that affected employment rates for prime-age males and females. The small differences that do exist suggest more rapid prime-age employment growth in PA than NJ, a difference that will be relevant for our *DIDID* estimates.

Across the other columns of the table, however, we see small, but consistent differences in employment rate changes that favor NJ relative to PA for groups that may have been affected by the minimum wage. Some, but not all, are statistically significant at conventional levels. Consider, first, the estimates for teens. In NJ, the teen employment rate increased about two and a half percentage points, while in PA, it increased just 1.67 percentage points, yielding a DID_B estimate (PA−NJ) of 0.89 percentage points. The effect is slightly larger for age 16–24 year olds, where employment actually fell in PA. Since some of this may involve part-time employment among college students and may reflect multiple causes, we interpret this effect conservatively. The next three columns show impacts for three groups of workers with less than a high school degree: all such workers, including teens; all except the teens; and only non-teen males. The impacts are successively larger, ranging from −0.66 for all workers with less than a high school degree to −2.45 percentage points for all non-teens to −6.31 percentage points for non-teen males without a degree. The effect for the non-teen males is statistically significant, while the effect for all less-educated non-teens is short of conventional levels. These results suggest that the biggest effect may not be on teens, who are obviously inexperienced but who may nevertheless have considerable cognitive abilities, but on non-teens with very low terminal schooling.

Although, as discussed above, employment of prime-age males and females moved quite similarly in NJ and PA during these years, it is nevertheless possible that NJ and PA experienced different employment shocks over this time period and that these shocks could be the source of the employment rate differences. Thus, in Table 3, we show our within-state estimates for PA, where the minimum wage increased wage rates for some groups far more than others. Here we focus on young

Table 2 Between-state *DID* estimates of impact of minimum wage on the employment rate, by age, education, and gender, 1995–1998

Year	*Age 16–19*	*Age 16–24*	*Educ < HS (All)*	*Educ < HS (Non-Teen)*	*Educ < HS (Non-Teen Males)*	*Male, 30–49*	*Female, 30–49*
A. Pennsylvania							
1995	44.42%	59.29%	46.33%	50.83%	39.56%	86.80%	70.20%
1998	46.09%	58.56%	48.64%	53.08%	41.15%	89.04%	72.24%
Difference	1.67 (1.37)	−0.74 (0.91)	2.31** (2.41)	2.25* (1.81)	1.59 (0.93)	2.24** (7.47)	2.05** (3.66)
B. New Jersey							
1995	36.52%	51.69%	45.83%	57.39%	44.38%	89.50%	71.90%
1998	39.08%	53.40%	48.80%	62.09%	52.29%	90.19%	73.51%
Difference	2.56** (2.04)	1.71* (1.83)	2.97** (2.68)	4.70** (3.31)	7.90** (3.81)	0.69** (2.43)	1.62** (2.75)
C. DID (PA–NJ)	−0.89 (0.51)	−2.45** (1.98)	−0.66 (0.45)	−2.45 (1.30)	−6.31** (2.34)	1.55** (3.76)	0.43 (0.53)

Absolute value of *t*-statistic in parentheses.
** Statistically significant at 5% level; *Statistically significant at 10% level.

Table 3 Within-state *DID* and *DIDID* estimates of impact of minimum wage on the employment rate, 1995–1998 by age and education

	Age 16–19	*Age 16–24*	*Age 30–49*	*Educ < High School (Non-Teen)*	*Educ ⩾ College*
A. Pennsylvania					
1995	44.42%	59.29%	78.49%	50.83%	86.33%
1998	46.09%	58.56%	80.34%	53.08%	86.80%
Difference	1.67	−0.74	1.86**	2.25*	0.47
	(1.37)	(0.91)	(5.78)	(1.81)	(1.36)
	Age 16–19 vs 30–49	*Age 16–24 vs 30–49*			*Educ < High Sch vs ⩾ Coll*
DID estimates	−0.18	−2.59			1.78
	(0.15)	(2.98)			(1.38)
	Age 16–19	*Age 16–24*	*Age 30–49*	*Educ < High School (Non-Teen)*	*Educ ⩾ College*
B. New Jersey					
1995	36.52%	51.69%	80.52%	57.39%	85.70%
1998	39.08%	53.40%	81.59%	62.09%	85.87%
Difference	2.56**	1.71*	1.08**	4.70**	0.17
	(2.04)	(1.83)	(3.27)	(3.31)	(0.49)
	Age 16–19 vs 30–49	*Age 16–24 vs 30–49*			*Educ < High Sch vs ⩾ Coll*
DID estimates	1.49	0.63			4.53**
	(1.14)	(0.64)			(3.09)
C. DIDID Estimates (PA–NJ)	−1.67	−3.22**			−2.75
	(0.92)	(2.45)			(1.41)

Absolute value of *t*-statistic in parentheses.
** Statistically significant at 5% level; *Statistically significant at 10% level.

workers *vs* prime-age workers, and workers with less than a high school degree compared to those with at least a college degree. The raw data used to construct the estimates is presented for PA in panel A, rows 1–2, with differences in row 3 and the *DID* estimates in row 4. Again, *t*-statistics for the test of no difference in means are shown in parentheses.

In PA, the employment rate increased 1.67 percentage points for teens and 1.86 percentage points for persons age 30–49. This comparison suggests that the increase in the minimum wage accounted for a very small decrease (−0.18) in the teen employment rate. The *DID* impact on employment for 16–24 year olds is larger, greater than 2.5 percentage points. This finding is somewhat unexpected, but not impossible. There is no effect in PA on the employment of non-teen workers with less than a high school degree relative to employment of college-educated workers. Employment of less educated workers increased more in PA than employment of college-educated workers.

In our view, the most credible estimator of the minimum wage impact on employment combines the between-state and within-state comparisons. The NJ within-state comparisons, shown in Panel B, are almost certainly not the result of any change in the minimum wage, which increased only 10 cents over this time period. Thus, they provide control for broader impacts that could affect different

groups differently. We use the NJ estimates in Panel B in conjunction with those for PA in Panel A to construct the *DIDID* estimates shown in Panel C.

The *DIDID* estimates tell a consistent story. Employment for teens fell slightly relative to older workers in PA (−0.18), but rose in NJ (1.49), yielding a *DIDID* minimum wage estimate of −1.67 percentage points; this effect is not large enough to be precisely estimated and it is not statistically significant at conventional levels. A similar pattern holds for 16–24 year olds, yielding a *DIDID* estimate that is twice as large and is statistically significant. Employment of less-educated non-teens rose quite sharply relative to more-educated workers in NJ. The resulting *DIDID* estimate is −2.75 percentage points, suggesting that the minimum wage increase did reduce employment of less-educated non-teens in PA relative to what would otherwise have occurred. This effect is substantial, but is not estimated precisely.

In general, these results are consistent with a negative employment impact of the minimum wage on workers in PA relative to NJ. Consistently, workers in arguably affected demographic groups do worse in PA than in NJ, while workers in arguably unaffected demographic groups do better in PA than in NJ. That distinction is the source of the *DIDID* estimates.

The implied elasticities are reasonable and consistent with other findings [see Neumark and Wascher 2007], although not with Card–Krueger. The minimum wage increased 21 percent between 1995 and 1998. The *DIDID* estimates imply a percentage decline in employment of 3.8 percent for 16–19 year olds and 7.9 percent for non-teens with less than a high school degree.[15] The resulting elasticity estimates are −0.178 and −0.372, respectively. Neumark and Wascher [2000, p. 1390] report elasticities that range from −0.10 to −0.25 for employment in the fast-food industry for the original NJ minimum wage increase. Since the demographic groups are not composed exclusively of minimum wage workers, these elasticity estimates implied larger effects for affected workers.

The original NJ minimum wage increase

The same technique used to examine the impact of the 1996 and 1997 minimum wage increases can be applied to the 1992 NJ minimum wage increase that was the subject of Card and Krueger's paper. We do that to examine whether the positive effects they report for a single industry hold broadly and in the kind of labor market data more typically used in minimum wage analyses.[16] We use the 1991 and 1993 CPS-ORG samples for this purpose. Sample size for persons age 16–59 is 43,946 (21,794 in NJ, 22,151 in PA). Over this time period, the employment rate for the NJ sample fell from 73.9 to 72.7 percent, while the corresponding rate in PA increased from 72.8 to 73.4 percent.

Rather than repeat the full between-state and within-state analysis presented above, we present in Table 4 our within-state *DID* estimates and our *DIDID* estimates for PA and NJ for the 1991–1993 period. The results are mixed. The employment rate for 16–19 year olds fell by an essentially identical amount in both states. But the employment rate for 30–49 year olds was unchanged in PA, while it fell by 1.49 percentage points in NJ, a change presumably unrelated to the minimum wage increase. Thus the DID_W estimate for 16–19 year olds is −1.95 in PA and −0.46 in PA, which suggests a positive effect of the minimum wage increase. This is seen in the *DIDID* row, where the PA–NJ estimate is −1.48 percentage points. (Note that in this table, a positive *DIDID* sign reflects a negative effect of the minimum wage on a particular group.) For non-teens with less than a high school degree,

Table 4 Within-state *DID* and *DIDID* estimates of impact of minimum wage on the employment rate, 1991–1993 by age and education

	Age 16–19	*Age 16–24*	*Age 30–49*	*Educ < High School (Non-Teens)*	*Educ ⩾ College*
A. Pennsylvania					
1991	45.66%	57.89%	79.09%	55.18%	85.61%
1993	43.71%	59.03%	79.10%	53.22%	85.94%
Difference	−1.94*	1.14	0.01	−1.96*	0.33
	(1.71)	(1.55)	(0.02)	(1.79)	(0.91)
	Age 16–19 vs 30–49	*Age 16–24 vs 30–49*			*< High Sch (Non-Teen) vs Coll+*
DID estimates	−1.95*	1.14			−2.29**
	(1.65)	(1.42)			(1.99)
	Age 16–19	*Age 16–24*	*Age 30–49*	*Educ < High School (Non-Teens)*	*Educ ⩾ College*
B. New Jersey					
1991	35.80%	53.30%	80.61%	62.05%	86.85%
1993	33.84%	52.23%	79.12%	54.56%	86.40%
Difference	−1.96*	−1.07	−1.49**	−7.50**	−0.45
	(1.83)	(1.38)	(4.94)	(7.24)	(1.50)
	Age 16–19 vs 30–49	*Age 16–24 vs 30–49*			*Educ < High Sch vs ⩾ Coll*
DID estimates	−0.46	0.42			−7.05**
	(0.42)	(0.62)			(3.01)
C. DIDID estimates (PA–NJ)	−1.48	0.71			4.76**
	(0.92)	(0.62)			(3.01)

Absolute value of *t*-statistic in parentheses.
** Statistically significant at 5% level; *Statistically significant at 10% level.

however, the results are consistent with a large negative minimum wage effect in NJ relative to PA. Employment of these workers in NJ fell 7.5 percentage points between 1991 and 1993, compared to about two points in PA. Adjusting further for the change in employment for college-educated workers yields DID_W estimates of −7.05 in NJ and −2.29 in PA. The resulting *DIDID* estimator, shown in the last row, is 4.76 percentage points, an effect that is precisely estimated. Thus, our findings here provide evidence for teen workers that is broadly consistent with the positive effects found by Card–Krueger for the fast-food industry, but estimates for less educated workers that are in quite the opposite direction.

Restaurant industry and occupation findings

Thus far, we have shown that employment of teens and of less educated workers fell in PA relative to NJ between 1995 and 1998 and that between 1991 and 1993, employment of teens in NJ rose relative to PA, while employment of less educated workers fell. While it is not possible to replicate the kind of analysis that Card and Krueger did with CPS data, it is possible to identify workers in the Eating and Drinking Industry and also in several common restaurant occupations. This allows

the testing of some related hypotheses about how the minimum wage affected employment of these workers and suggests whether this industry/occupation is representative of broader trends.

With the CPS data, we can identify workers who are employed in the Eating and Drinking industry[17] and in food service occupations, which includes cooks; waiters/waitresses; food counter, fountain and related occupations; kitchen workers; waiters'/waitresses' assistants; and miscellaneous food preparation occupations. It is not possible to further refine either the industry or occupation categories to include only fast-food restaurants, because specific employment sites are not identifiable in the CPS. The industry and occupation categories are not fully overlapping. In 1995 and 1998, for example, about two-thirds of workers in the Eating and Drinking Industry were in restaurant occupations. Sizeable occupations included in the Eating and Drinking Industry that are not part of the restaurant occupation include managers, cashiers, supervisors, and truck drivers. Similarly, many workers in food service occupations worked in schools and nursing homes, rather than restaurants.

Despite these limitations, it is instructive to see what can be learned about changes in employment in the broader restaurant industry and occupation from the CPS. One way to infer the impact of the minimum wage is to see how the age and education distribution of workers changed in this industry and occupation in NJ and PA over this time period. If the minimum wage had a negative effect on employment of less-skilled workers, it is likely that their relative employment would fall.

Table 5 provides information on how the employment share of teen workers and workers with no more than a high school education who reported working in the restaurant industry and the restaurant occupation changed following changes in the minimum wage in NJ and PA. The top seven rows show the data for 1995–1998 and the bottom seven rows for 1991–1993.

Between 1995 and 1998, the employment share for 16–19 year olds increased 3.5 percent points in NJ and 2.0 points in PA, while the corresponding share for less educated workers increased 7.1 percent points in NJ and 4.3 points in PA. Both *DID* estimates are negative, although they are quite small. These findings are thus broadly consistent with a negative impact of the minimum wage increase on

Table 5 Employment share of teens and less-educated workers in the restaurant industry and occupation, NJ and PA, 1995–1998 and 1991–1993

	Teens	*Education* $\leqslant$ *HS*
NJ, 1995 ($N=215$)	0.243	0.625
NJ, 1998 ($N=112$)	0.278	0.696
Change (1998–1995)	0.035	0.071
PA, 1995 ($N=248$)	0.325	0.685
PA, 1998 ($N=198$)	0.345	0.728
Change (1998–1995)	0.020	0.043
DID (PA–NJ)	−0.015	−0.028
NJ, 1991 ($N=185$)	0.209	0.700
NJ, 1993 (N−204)	0.187	0.692
Change (1993–1991)	−0.022	−0.008
PA, 1991 ($N=274$)	0.317	0.715
PA, 1993 ($N=247$)	0.309	0.741
Change (1993–1991)	−0.008	0.026
DID (PA–NJ)	0.014	0.034

employment of teens and less-educated workers in the restaurant industry and occupation in PA following the 1995–1998 increases.

The corresponding data for 1991 and 1993 tell exactly the opposite story. Between 1991 and 1993, the employment share of teens in the restaurant industry/occupation fell 2.2 percentage points in NJ and less than one point in PA. The employment share for less-educated workers fell less than a point in NJ, but rose 2.6 points in PA. Thus, both *DID* estimates are positive, suggesting a small negative effect of the minimum wage increase on employment for these low-wage workers in NJ. While none of the *DID* effects in Table 5 are very large, it is interesting that the signs reverse between the two time periods in ways that are consistent with a negative employment effect of the minimum wage.

SUMMARY AND DISCUSSION

Our analysis of a second interesting NJ–PA minimum wage experiment comes to a different conclusion than did Card and Krueger based on the first one. The 1996 and 1997 increases in the Federal minimum wage raised the minimum in PA by more than 20 percent but hardly increased it in NJ. Their analysis was focused on employment in a single industry, while ours examines the impact across demographic groups. Using data on employment rates in PA and NJ in 1995 and 1998, we examined how this differential increase in the minimum affected potentially at-risk groups. Our results do not contradict theirs: we are looking at impacts across different populations. Arguably, because our sample is broader and because it is based on widely used CPS data, our findings are more relevant to the policy debate.

We find consistent evidence that workers likely to be more affected by the minimum wage in PA were more negatively affected than similar workers in NJ. This conclusion holds when we compare similar workers between states, different groups of workers within a state, and, most convincingly in our opinion, the difference across states in the differential impact on different groups of workers. We estimate particularly large effects on non-teens with less than a high school degree. Our attempt to apply the same approach to the 1992 NJ minimum wage increase yields mixed evidence of a negative minimum wage employment effect — no negative effect for teens, but a substantial negative effect on older workers with less than a high school degree. We also find some tentative evidence that the share of employment in the restaurant industry and occupation for teens and less-educated workers grew more rapidly in NJ than in PA between 1995 and 1998, while exactly the opposite occurred in the 1991–1993 period. A recent summary of the minimum age literature by Neumark and Wascher concludes that traditional economic theory is right far more often than it is wrong when it comes to the employment effects of the minimum wage. They cite 102 studies, of which "nearly two-thirds give a relatively consistent (although by no means always statistically significant) indication of negative employment effects… while only eight give a relatively consistent indication of positive employment effects" [p. 121]. They further note that 28 of the 33 studies they regard as most credible and most studies focusing on least-skilled groups find negative impacts. Our evidence on the 1996–1997 PA–NJ minimum wage experiment is consistent with this body of literature.

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Notes

1. The effect reflects a decline in employment in PA and essentially no change in NJ. This implies that employment would have fallen in NJ had it not been for the minimum wage increase.
2. Card and Krueger (2000, pp. 1406–7) very briefly examine the impact of only the 1996 increase on employment in fast-food restaurants in PA counties adjacent to NJ using BLS aggregate data on employment in firms covered by UI programs. Their analysis indicated that employment in PA rose relative to NJ between October 1996 and September 1997 for the seven-county PA sample that was part of their original analysis and increased very slightly in a broader 14-county PA sample.
3. Many of the earliest contributions to this new literature appear in a special issue of *The Industrial and Labor Relations Review*, October 1992, including Card [1992] and Katz and Krueger [1992]. See Neumark and Wascher [2007] for a very thorough review.
4. That most of the quantitative change in employment comes from the negative experience of the control state is not problematic as long as PA is considered an appropriate control.
5. For an explanation in terms of rescheduling effects, see Michl [2000], who argues that the minimum wage increase could cause firms to increase employment, while reducing the workweek.
6. The payroll data were collected with the assistance of the Employment Policy Institute, an industry-based group. This has created some concerns about possible biases in the data.
7. Deere, Murphy, and Welch do not use the language of natural experiments nor make any explicit difference-in-difference calculations.
8. The only group not following this pattern is states classified into high-wage, middle-wage, and low-wage. This parallels the finding of Card [1992]. Deere, Murphy, and Welch argue that this reflects employment growth by state in low-wage states that overwhelms the impact of the minimum wage increase.
9. Data files were obtained from the CEPR data archive at http://www.ceprdata.org/cps/org_index.php.
10. We use the recoded variable LFP, which includes codes for Not in Labor Force, employed full-time or part-time for economic or non-economic reasons, and unemployed.
11. The 40 percent decrease in the NJ sample size between 1995 and 1998 reflects a reduction in the overall CPS sample in 1996 that affected primarily states that had higher than average sampling rates. See BLS [2002] Table H-1 for further information. Prior to the sample decrease, sample weights for PA were 40 percent higher than NJ, which means that NJ was sampled at a substantially higher rate than PA. Sample weights in 1998 for NJ and PA are within 4 percent of each other.
12. These differences in race and Hispanic composition are confirmed in official state population estimates for NJ and PA (see PA Division of Health Statistics 1998 and New Jersey Department of Labor and Workforce Development, undated).
13. Sample sizes in Table 2 range from 2,918 (females, PA, 1995, age 30–49) to 224 (non-teen males, education < high school, NJ, 1998). A table with sample sizes and standard errors for each group for this table and all others is available from the authors.
14. Nationally, the employment rate for women aged 16 and older increased from 55.6 percent in 1995 to 57.1 percent in 1998.
15. The elasticity estimates assume that employment in the control group was completely unaffected by the change in the minimum wage. As such, the computed elasticities are upper-bound estimates.
16. We gratefully acknowledge the suggestion of an anonymous referee to apply the methods of this paper to the earlier NJ–PA episode.
17. This is the broad industry code analyzed by Card and Krueger in their analysis of BLS firm data [Card and Krueger 2000], although they focus on employment only in fast-food restaurants within this industry.

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