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Do State Minimum Wage Laws Reduce Employment? Mixed Messages from Fast Food Outlets in Illinois and Indiana

Joseph Persky# and Ron Baiman*
University of Illinois at Chicago#, Center for Tax and Budget Accountability – USA*

Abstract. In January 2004 and January 2005 the state of Illinois increased its minimum wage to $5.50 and then $6.50, well above the national minimum of $5.15. This study, comparing the impacts on Illinois fast food outlets to a control group of Indiana outlets, was conceived as a repetition of the Card-Krueger study of a similar situation in New Jersey. The central question is whether the Illinois outlets demonstrated a substantial reduction in employment in response to the higher legislated wage rates. We conclude that the Illinois-Indiana data lack the power to differentiate between a "zero employment effect" and a "small negative employment effect." Furthermore, we question the welfare significance of such a determination even if it could be convincingly made.

1. Introduction

Starting with the work of Card and Krueger (1994, 1995) changes in state minimum wages have become key quasi-experiments to explore the shape of labor demand curves for low wage workers. The debate launched by Card and Krueger's study of a New Jersey increase in the minimum wage (Newmark and Washcer, 2000) has continued for more than a decade now without a decisive resolution. A recent review by Newmark and Wascher (2008) concludes that the traditional view of labor demand curves has been maintained with the bulk of evidence suggesting that higher minimum wages significantly reduce employment. On the other hand Dube et al. (2010) suggest that the evidence supports no effect of minimum wages on low wage employment.

The Illinois-Indiana Study (Powers, Baiman and Persky, 2007)[1] was originally conceived as a repetition and improvement on the Card and Krueger effort to study fast food outlets. Illinois in 2003 passed legislation that raised the state's minimum wage from the national figure of $5.15 an hour to $5.50 in January 2004 and $6.50 in January 2005. At the time Indiana left its minimum at the national figure. This natural quasi-experiment suggested the possibility of a new geographical field to be studied. In addition the researchers hoped to extend the Card-Krueger surveys to include information on hours of work, a key criticism of earlier studies. We find the results of the Illinois-Indiana study lack the power to differentiate between a "zero employment effect" and a "small negative employment effect." The present paper puts forth that reading and contrasts it with Powers (2009) who concludes that the Illinois-Indiana data show sizable negative effects of minimum wage increases on employment in Illinois fast food outlets.[2] Whatever the truth of the matter, we argue below that from the perspective of minimum wage workers the regional welfare implications of the contesting viewpoints are only modestly different.

[1] This study will be referred to throughout the current paper as the "Illinois-Indiana Study." Elizabeth Powers and the present authors cooperated in the collection and processing of the survey data in the two states. As will become clear, our analysis and interpretation of those data differ substantially from Powers'.

[2] The issue here is clearly of some moment. Neumark and Wascher (2008) in their survey interpret the Illinois-Indiana Study (Powers, Baiman and Persky, 2007) in a manner heavily influenced by Powers (2009).

2. Study Background, Data and Methodology

The Illinois-Indiana minimum wage study identified a list of 410 fast-food outlets on the Indiana-Illinois border. The study area was chosen to avoid the relatively high wage labor markets of Chicago and northwest Indiana. The area is defined in Figure 1 from the study report. Particular attention was paid to match both population growth and income levels in the two states with the hope of minimizing any systematic differences. In aggregate, population in the Illinois counties grew 4.27% between 1990 and 2000, while the Indiana counties grew 4.30% over the same period. In 1999 per capita income in the Illinois counties was $18,605, while the Indiana counties had a per capita income of $18,365. The similarity of the two areas is well maintained in the sampling period of the study. Between 2003 and 2005 population in the Illinois counties grew 0.34%, while in the Indiana counties population grew 0.20%.

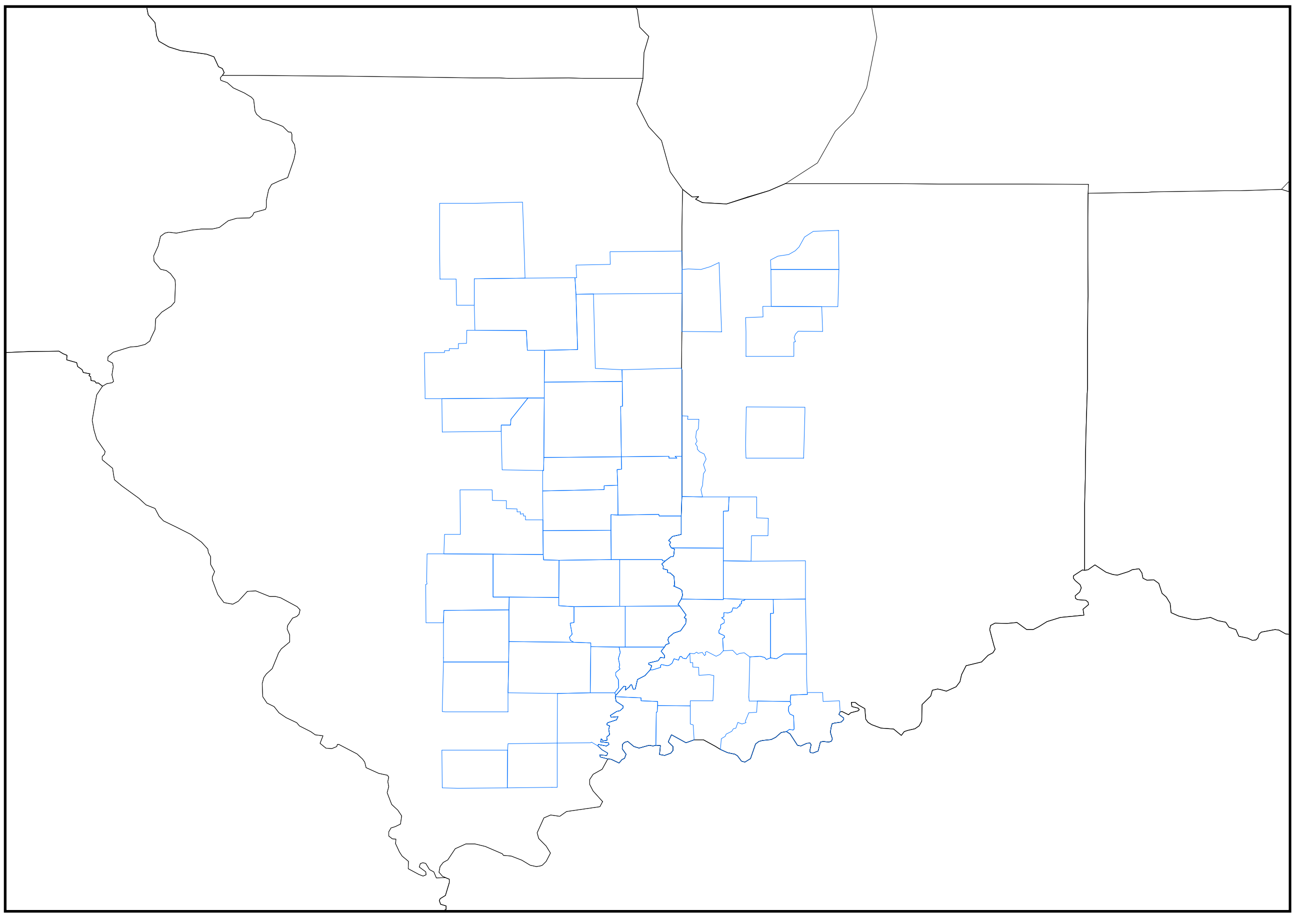

Figure 1. Illinois-Indiana study area.

In the first wave of the study in fall 2003, 252 outlets provided basic employment responses. The final sample of 2005 allows for a matched sample to measure changes of 190 outlets. For the hourly data the matched sample is somewhat smaller at 169. The data were collected by student interviewers using telephones, with only a few interviews conducted face-to-face.

The questionnaire for the survey draws heavily on that used by Card and Krueger in their New Jersey-Pennsylvania study. In addition to information on employment it provides data relevant to a range of sub-hypotheses concerning possible impacts of an increase in minimum wages. The present paper focuses on the nonsupervisory employment and hours questions in the survey.[3]

[3] Other survey data suggest little support for various secondary hypotheses. On these matters we agree with Powers' general assessment: "While entry-level wages of Illinois establishments rose substantially in response to the mandated increases, there is little evidence that Illinois establishments ameliorated wage increases by delaying scheduled raises or reducing fringe benefit offerings. There is also little evidence of 'labor-labor' substitution in favor of women, better educated, or teenage workers, or increased worker tenure at the new wage. There is weak evidence of increased food prices" (Powers, 2009, p. i).

Given some of the details to be considered in this paper it is useful to be clear about the wording of the most important questions. The key items are:

1. How many people including both managers and nonsupervisory employees were on your restaurant's payroll during the last pay period?

1a. About what number or what percentage of these were nonsupervisory employees?

1b. How long is your payperiod?

2. About what number or percentage of the nonsupervisory employees were part time?

7. In your last pay period (or other specified period: ___________) how many hours did all nonsupervisory employees work in total?

These questions represent a compromise between Card and Krueger's questions and those used in the Current Employment Statistics survey by the Bureau of Labor Statistics. Most important is the inclusion of question (7) on the hours of nonsupervisory employees. We explicitly used the term "nonsupervisory" for workers who aren't managers or assistant managers. This is the terminology of the Bureau.

Card and Krueger didn't ask a question about hours in their survey. They assumed that all managers and assistant managers were full-time workers. They went on to construct their central measure of employment, full-time-equivalents, as the sum of 0.5 times part-time nonsupervisory workers and 1.0 times full-time workers (nonsupervisory and all managers and assistant managers.) As can be seen above our wording allows us to reproduce this full-time equivalent measure under the same assumptions that Card and Krueger used. However, in what follows we concentrate on nonsupervisory workers since this is the group for whom the minimum wage is most relevant. Our estimate of full-time equivalent nonsupervisory workers (FTENS) excludes all managers.[4] This seems much the better measure since it involves no assumption about the hours of supervisory employees. Indeed, casual empiricism from talking to fast-food-outlet management suggests that many assistant managers work part time. More importantly, the minimum wage is binding only for nonsupervisory workers and not for management workers. Hence throughout this paper we focus on nonsupervisory workers.[5]

A major criticism of the Card and Krueger study was its failure to generate data on hours. The full-time equivalent measure described above assumes that part-time workers put in hours equal to 50% of full-time workers. Several commentators have pointed out that this is a strong assumption, one that is difficult to establish. Hence, our questionnaire explicitly asked for hours for nonsupervisory workers. These hours data allow a second (and more direct) estimate of full-time equivalent nonsupervisory workers. We divide weekly nonsupervisory hours by 35 to calculate full-time equivalent nonsupervisory employment based on hours, henceforth referred to as FTENSH.[6]

What then is the impact of increases in the Illinois minimum wage on the employment of nonsupervisory workers in typical Illinois fast-food outlets? We approach the problem as one of estimating establishment-level demand curves for nonsupervisory labor.[7] The dataset includes only outlets with nonsupervisory information for both 2003 and 2005.[8] The basic null hypothesis to be tested is the proposition that the wage elasticity of this demand curve is zero against the alternative that this elasticity is not equal to zero.[9] We start with a set of simple difference in difference calculations. We then move on to consider responses adjusted for outlets' initial wages using so called "gap" variables. These are done both by entering the gap variables directly and, alternatively, entering them as instruments to predict wage changes. Finally we consider the effect of outlets' chains on demand.

3. Difference in difference

As of 2003, fast-food outlets in the two states look well matched, with the Indiana outlets only slightly smaller (13.10 vs. 13.29 in Illinois). The difference-in-difference test asks whether the change in average establishment employment occurring from before to after the introduction of the minimum wage increase in Illinois was significantly different from the change in Indiana over the same period. Between 2003 and 2005 the average Indiana fast food outlet (N=68) went from 13.1 full-time equivalent nonsupervisory workers to 12.9 such workers. The Illinois average (N=121) fell from 13.3 to 12.2. Thus a simple difference-in-

[4] Card and Krueger also estimated such a measure but did not emphasize it in their analysis.

[5] Results for full-time equivalents including managerial workers are quite similar to those presented below.

[6] The choice of 35 hours is arbitrary. However, since division of the dependent variable by a scalar doesn't affect the significance of coefficients, alternative denominators would yield the same pattern of results.

[7] We do not know how many of the missing establishments in 2005 were in fact out of business. Nor do we know what other general equilibrium effects the minimum wage might produce. We suspect both these impacts are negligible.

[8] More formally, for each outlet estimated full-time equivalent nonsupervisory employment is greater than zero for each year.

[9] Notice that we use two-sided tests because of the possibilities generated by Card and Krueger's original observation of a positive effect of minimum wages on employment.

difference test shows the Illinois outlets falling 0.88 more full-time equivalents than the Indiana outlets. However, we can put little confidence in this figure since the standard error of the difference in difference is 1.20. The value of -0.88 fails the 10% two-tail test, and we cannot reject the null hypothesis.

This difference-in-difference test is equivalent to estimating the following simple regression equation:

$$\Delta FTENS_i = \alpha + \beta_1 Illinois\ Dummy_i + \varepsilon_i \qquad (1)$$

where $\Delta FTENS_i$ is the change in full-time equivalent nonsupervisory workers in outlet i and Illinois $Dummy_i$ takes the value of 1 if outlet i is in Illinois and 0 otherwise. For the full sample estimation yields[10]:

$$\Delta FTENS_i = -0.17 - \underset{(1.20)}{0.88}\ Illinois\ Dummy_i \qquad N=189;\ R^2 = 0.00 \qquad (2)$$

The Illinois dummy variable explains virtually none of the variance in the change in full-time equivalent nonsupervisory workers, yielding an R^2 of 0.00. All the R^2 values for this paper are very low and will not be presented in most of the remaining equations.

Applying White's test for heteroscedasticity to the residuals of this equation generates a chi-square statistic of 1.43 for one degree of freedom. Accepting the null hypothesis in this simple case is equivalent to asserting that the variances of the error generating processes in the two states are not statistically different.

The story looks very much the same if we take as the dependent variable gFTENS, the outlet growth rates in full-time equivalent nonsupervisory employment.[11] Full-time equivalent nonsupervisory employment in Illinois outlets grew slower on average than Indiana outlets, but the difference is not significant:

$$gFTENS_i = 0.002 - \underset{(0.08)}{0.113}\ Illinois\ Dummy_i \qquad (3)$$

Again a White test for heteroscedasticity is not significant. Indeed, White tests are for the most part not significant in the equations reported below. Where, however, there are problems (defined as a Chi square significant at the 10% level or better) we have re-estimated the equations using robust estimates of the standard errors.[12] In the results reported below such standard errors are indicated by #. Almost always these are somewhat larger than the errors generated assuming homoscedasticity. However, such changes are quite minor. In no case does the re-estimation of the standard errors change the significance of estimated coefficients.

These same two equations can be run using the nonsupervisory full-time equivalent measure based on hours, FTENSH:

$$\Delta FTENSH_i = 4.35 - \underset{(1.24)}{3.10^{**}}\ Illinois\ Dummy_i \qquad (4)$$

$$gFTENSH_i = 0.300 - \underset{(0.10)}{0.219^{**}}\ Illinois\ Dummy_i\ . \qquad (5)$$

Sample sizes for hours are smaller for both Indiana (N=60) and Illinois (N=109). Unfortunately, the average number of nonsupervisory full-time equivalents measured on an hours basis for Indiana outlets in 2003 (10.0) is smaller than the figure for Illinois (12.0). Thus for this sample at base line the two sets of outlets appear somewhat different. Between 2003 and 2005 the Indiana outlets report an average expansion of 4.35 full-time equivalent nonsupervisory workers while the average in Illinois was only 1.25. The Indiana outlets averaged 30% growth while the Illinois outlets managed only 8%. The Illinois dummy variables in both the difference-in-difference and the difference-in-growth rates equations are now significant at better than the 5% level.

But something seems a bit odd here. We are not observing Illinois outlets laying off their nonsupervisory employees (the standard theoretical prediction), but rather seeing a boom in Indiana outlet hours employed. One possibility is that there is a difference in the set of outlets in the two measures. A number of outlets gave interviewers position data, but were unwilling or unable to give hours data. What happens to the position estimates if we limit the sample to those with full hours data? Such a constraint actually reduces the difference-in-difference estimate to -0.53, leaving it far from significant. Similarly, the difference in growth falls to -9.2%, again quite far from significance.

$$\Delta FTENS_i = 0.31 - \underset{(1.19)}{0.53}\ Illinois\ Dummy_i \qquad (6)$$

$$gFTENS_i = 0.033 - \underset{(0.07\#)}{0.092}\ Illinois\ Dummy_i \qquad (7)$$

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[10] Note, for all equations: Standard errors of estimate in parentheses. Results significant at the 10% level on a two-tailed test are marked with a single asterisk, those at the 5% level with two asterisks and at the 1% level with three asterisks.

[11] Like Card and Krueger we use the difference over the average value to measure growth rates.

[12] These are Huber/White sandwich estimators.

The results for position data when limited to the same sample as that used for the hours data are not closer to the hourly data, but rather further away.

We are tempted to prefer the hourly data to the position data since, as noted above, the latter makes a rather arbitrary assumption about the ratio of part-time to full-time average hours. But there are measurement problems in the hourly data that must be acknowledged. In analyzing the questionnaires it becomes clear that respondents had some difficulty with reporting the length of the payroll period. If we include a set of dummy variables for the payroll period in the hours difference-in-difference calculation, we find support for this suspicion. The estimated equation is now:

$$\Delta FTENSH_i = \alpha + \beta_1 Illinois\ Dummy + \beta_2\ TW\_2003 + \beta_3\ TW\_2005 + \varepsilon_i. \qquad (8)$$

where TW_2003 is a dummy variable with a value of 1 if the outlet reported a two week payroll period in 2003 and similarly for TW_2005.

Having a two-week pay period in 2003 significantly increases growth, while a two-week pay period in 2005 reduces growth (equations 9 and 10). Indeed, the significance of the two-week effect for 2003 (significant at better than the 1% level) is stronger than that for any other coefficients explored in this paper. We doubt there is any explanation for these significant dummy coefficients other than misclassifications. It is important to include these pay period dummies in the analysis of hourly data since they are not independent of the state effect. Including these dummies drops the coefficient on the state effect to -1.90 and leaves it highly insignificant.

$$\Delta FTENSH_i = -0.65\ - 1.90\ IL\ Dummy + 6.89^{***}TW\_2003 - 2.61^{*}TW\_2005 \qquad R^2{=}0.12 \qquad (9)$$

with standard errors of 1.47#, 1.86#, and 1.74#

$$gFTENSH_i = -0.65\ - 0.125\ IL\ Dummy + 0.54^{***}TW\_2003 - 0.20^{*}\ TW\_2005 \qquad R^2{=}0.17 \qquad (10)$$

with standard errors of 0.10, 0.10, and 0.10

4. Using gaps

While the difference-in-difference approach captures the aura of a quasi-experiment, we know in advance that not all outlets in a state are identical. For one thing, they may differ because of their national chain affiliations. We return to this question in the next section. More primitively, they differ in terms of their initial starting wage. Many outlets in both Illinois and Indiana paid starting nonsupervisory workers more than the national minimum wage. These differences most likely reflect market conditions in an outlet's community, but may also be indicative of personnel strategies adopted by management. In any case, as Card and Krueger noted in their New Jersey-Pennsylvania study, a minimum wage increase may not be binding on many outlets in a state.

Indiana average starting wages in 2003 were $5.72 per hour with a range from $5.15 to $7.00. Illinois' average was $5.79 with a range from $5.15 to $7.75. Over the study period, the Illinois distribution shifted up to a mean of $6.50 by 2005, while the Indiana distribution increased only slightly to $5.83.[13] For the Illinois outlets with 2003 wages greater than $6.50, no adjustment in labor demand was required, while those paying the national minimum of $5.15 presumably faced a serious shock of about 25%. The initial gap at a restaurant is defined as the percentage increase required in its starting wage to reach the required minimum. By definition the Indiana outlets' gap is set at 0.0.

Traditional labor demand theory suggests that the change in full-time equivalent nonsupervisory employment would be influenced by the gap.[14] Using the gap as an independent variable tests whether the "treatment effect" is proportional to the stimulus. Notice in particular that regressing the growth in full-time equivalents on the gap is roughly equivalent to estimating a labor demand elasticity. This equation would simply be:

$$gFTENS_i = \alpha + \gamma_1\, Gap + \varepsilon_i. \qquad (11)$$

Table 1 exhibits the gap coefficients for the same set of dependent variables discussed in equations 1-10 above. All the coefficients in equations 12-15 have negative signs. However, none of them is significant at the 10% level. If we follow our procedures from the last section and restrict the position sample to the same observations as the hours sample we get the results of equations 16 and 17. In the first of these the gap coefficient on ΔFTENS has actually gone positive, although far from significant. If we introduce the payroll period controls into the hours-based equations (equations 18 and 19) these are again very significant. For these two equations neither gap coefficient is significant, although here the effect on gFTENS (equation 19) is actually positive.

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[13] Somewhat curiously, eleven Illinois outlets in 2005 report starting wages below the minimum wage.

[14] Notice that the gap is computed in terms of the nominal wage at each outlet. While local real wages may differ across communities, the gap for real wages would give the same percentage changes. Implicitly the modeling assumption here is that percentage changes in labor demand are proportional to percentage changes in wages.

Table 1: Gap coefficients.

	N	Variable	Coefficient	Equation
Indiana and Illinois				
ΔFTENS from Position Data	189	Gap	-5.35	(12)
			(6.95#)	
gFTENS from Position Data	189	Gap	-0.55	(13)
			(0.42)	
ΔFTENSH from Hours Data	169	Gap	-3.39	(14)
			(10.10#)	
gFTENSH from Hours Data	169	Gap	-0.06	(15)
			(0.53)	
ΔFTENS from Position Data Limited to Hours Sample	166	Gap	0.44	(16)
			(6.77#)	
gFTENS from Position Data Limited to Hours Sample	166	Gap	-0.25	(17)
			(0.428#)	
ΔFTENS from Hours Data with pay period dummies	169	Gap	-1.35	(18)
			(9.17#)	
		TW_2003	7.22***	
		TW_2005	-2.82*	
gFTENS from Hours Data with pay period dummies	169	Gap	0.10	(19)
			(0.49)	
		TW_2003	0.56***	
		TW_2005	-0.22**	
Illinois Only				
ΔFTENS from Position Data	121	Gap	-4.46	(20)
			(9.26#)	
gFTENS from Position Data	121	Gap	-0.30	(21)
			(0.62)	
ΔFTENSH from Hours Data	109	Gap	16.25	(22)
			(13.86)	
gFTENSH from Hours Data	109	Gap	1.51**	(23)
			(0.71)	
ΔFTENS from Position Data Limited to Hours Sample	107	Gap	4.91	(24)
			(9.62)	
gFTENS from Position Data Limited to Hours Sample	107	Gap	0.18	(25)
			(0.64)	
ΔFTENS from Hours Data with pay period dummies	109	Gap	11.05	(26)
			(13.40#)	
		TW_2003	7.01***	
		TW_2005	-4.29**	
gFTENS from Hours Data with pay period dummies	109	Gap	1.17*	(27)
			(0.67)	
		TW_2003	0.47***	
		TW_2005	-0.17	

Standard errors of estimate in parentheses; # indicates significant White test and robust estimate of standard errors.
Results significant at the 10% level on a two-tailed test are marked with *, those at the 5% level with ** and at the 1% level with ***.

An alternative approach to introducing the gap variable is to restrict the sample to Illinois outlets only, allowing higher wage Illinois fast food establishments to act as controls for the lower wage establishments. In principle this shouldn't affect the coefficient on the gap variable. For some of our samples, however, it has a very substantial effect on that coefficient. The simple nonsupervisory position sample stays fairly constant (equations 20 and 21 in Table 1), but the hours sample changes dramatically with the coefficients in both the difference and growth equations actually going positive (equations 22 and 23 in Table 1). The last of these is quite significant. Restricting the position data to the hours sample also yields positive coefficients on the gap variable (equations 24 and 25). Adding pay period dummies to the hours data increases the (positive) coefficients, with the growth equation coefficient reaching significance at the 10% level.

Presumably the gap variable works as an exogenous shift in the wage rate. Normally we wouldn't be able to determine whether a change of wages results from a shift in demand or of supply. Thus a theoretically appealing model is to treat our problem as one of estimating a demand curve using the gap variable as an instrument to identify the effect of wage increases on quantity demanded. Thus in the first stage we regress percentage wage change on gap and then used the predicted wage changes as the independent variable explaining percentage change in either the position-based full-time equivalent nonsupervisory workers (FTENS) or the hours-based full-time equivalent workers (FTENSH).

The first stage results of this exercise as reported in equations 28 are quite plausible and essentially identical whether we use the position sample (*N*=171) or the smaller (*N*=154) hours-based sample (The R^2 equals 0.51 for the larger sample and 0.57 for the smaller.):

$$g_w = 0.02 + \underset{(0.06)}{0.78^{***}}\, gap. \qquad (28)$$

However the second stage results vary in a somewhat embarrassing manner between the position and hours data. The key elasticity in the former case appears as -1.0 (significant at the 10% level), while in the latter case with pay period dummies it is a very insignificant -0.1. Limiting the sample to Illinois only doesn't change the position result, but drives the hours result (with dummies) to a surprisingly positive (although not significant) value of 1.09.

5. Chains and the size of outlets

It has been argued that smaller fast food outlets may be more restricted in their response to rising minimum wages than their larger rivals. In particular, the Subway chain is uniformly smaller than others included in this study. On this basis Powers (2009) argues for dropping Subway outlets from the Illinois-Indiana sample. Such an argument seems to suggest that the traditional theory doesn't hold for smaller chains.[15] Still, it seems fair to ask whether excluding Subway outlets from the sample makes a substantial difference in the estimated effects of the minimum wage.

The samples in equations 29 and 30 are the same as those in equations 2 and 3, but they exclude Subway outlets. The average outlet size rises by about three full-time equivalent nonsupervisory workers. However there is only a modest increase in the difference measures. Neither summary measure is significant, as the standard errors have risen with the estimates.

$$\Delta FTENS_i = -0.00 - \underset{(1.72)}{1.33}\, IL\ Dummy_i \qquad R^2 = 0.00 \qquad (29)$$

$$gFTENS_i = 0.02 - \underset{(0.10\#)}{0.14}\, IL\ Dummy_i \qquad R^2 = 0.01 \qquad (30)$$

If we now subject the sample without Subway to the same set of tests using the Illinois dummy or the gap variable, we do find somewhat stronger indications of a negative employment response (see equations 31-46 of Table 2). Four out of sixteen coefficients reach negative significance. One of the sixteen, however, is positive. The only significant coefficients are for the state dummy with hourly data. But, as before, the differences here are not the results of reported declines in Illinois, but rather are produced by increases in Indiana, increases which are difficult to attribute to the increase in the minimum wage in Illinois. Illinois-reported hourly positions in non-Subway outlets actually grew by 1.25, while Indiana positions grew by 4.35. Table 3 substitutes the instrumental model for the simple regression on the gap variable.

Finally we limit the gap test to non-Subway outlets in Illinois. Out of eight tests reported in Table 4, two are negative (neither significantly) and six are positive (one significantly), with similar results from the instrumental approach (not reported).

6. Discussion

Perhaps the best way to summarize the present paper is to contrast our findings with those of Elizabeth Powers (2009). In that article, based on the same survey of Illinois-Indiana fast-food outlets, Powers argues that her "study is the first 'local' one to find evidence

[15] In the context of theory, the question of heterogeneity in size suggests that average percentage change measures of impact might be more fitting than average absolute changes. Similarly, changes in hours might be more sensitive to demand conditions than changes in positions.

of large adverse employment effects" (Powers, 2009, p. 392). The results presented above suggest that Powers has seriously overstated the case.

Both studies focus on nonsupervisory workers and their hours. This is the group most directly influenced by minimum wages. Throughout her study Powers uses the change in outlet positions or hours. While we have reproduced such estimates above, a much more relevant endpoint is the *growth rate* in outlet positions or hours. Outlets differ in size, and impacts are expected to be proportional to size. Throughout we have included estimates based on these growth rates. The use of the growth rate as the dependent variable is particularly useful in gap equations. In this situation the gap coefficient can be interpreted as an estimate of a demand elasticity.

The key equation then becomes our equation 15 where the relevant elasticity (i.e., the percentage change in nonsupervisory hours for every 1% change in the gap) is estimated as an insignificant -0.06, virtually indistinguishable from zero. This result suggests no effect on hours, not a "large adverse" one.

Many of Powers' most significant negative findings are obtained using as a dependent variable the change in part-time nonsupervisory positions. This is measured across outlets of varying sizes and staffing patterns. The variable in question is somewhat suspicious since results for it can't be easily duplicated for full-time equivalent nonsupervisory positions.

Table 2. Key coefficients excluding Subway chain.

Dependent Variable	N	Variable	Coefficient	Equation
ΔFTENS from Position Data	130	IL Dummy	-1.33	(31)
			(1.72)	
gFTENS from Position Data	130	IL Dummy	-0.14	(32)
			(.11)	
ΔFTENSH from Hours Data	115	IL Dummy	-4.83**	(33)
			(2.14)	
gFTENSH from Hours Data	115	IL Dummy	-0.28**	(34)
			(0.12)	
ΔFTENS from Position Data Limited to Hours Sample	112	IL Dummy	-0.94	(35)
			(1.74)	
gFTENS from Position Data Limited to Hours Sample	112	IL Dummy	-0.11	(36)
			(0.08#)	
ΔFTENSH from Hours Data with Pay Period Dummies	115	IL Dummy	-3.68*	(37)
			(1.95#)	
gFTENSH from Hours Data with Pay Period Dummies	115	IL Dummy	-0.22*	(38)
			(0.12)	
ΔFTENS from Position Data	130	Gap	-7.41	(39)
			(8.46)	
gFTENS from Position Data	130	Gap	-0.75	(40)
			(0.55#)	
ΔFTENSH from Hours Data	115	Gap	-10.93	(41)
			(13.25#)	
gFTENSH from Hours Data	115	Gap	-0.52	(42)
			(0.61)	
ΔFTENS from Position Data Limited to Hours Sample	112	Gap	0.10	(43)
			(9.14#)	
gFTENS from Position Data Limited to Hours Sample	112	Gap	-0.30	(44)
			(0.52#)	
ΔFTENSH from Hours Data with Pay Period Dummies	115	Gap	-4.07	(45)
			(11.80#)	
gFTENSH from Hours Data with Pay Period Dummies	115	Gap	-0.14	(46)
			(0.59)	

Standard errors of estimate in parentheses; # indicates significant White test and robust estimate of standard errors.
Results significant at the 10% level on a two-tailed test are marked with *, those at the 5% level with ** and at the 1% level with ***.

Table 3. IV estimates excluding Subway chain.

Dependent Variable	N	Variable	Coefficient	Equation
gFTENS from Position Data	121	Gap	-0.74	(47)
			(0.65)	
gFTENSH from Hours Data	109	Gap	-0.66	(48)
			(0.76)	
gFTENS from Position Data Limited to Hours Sample	107	Gap	-0.36	(49)
			(0.61)	
gFTENS from Hours Data with pay period dummies	109	Gap	0.00	(50)
			(0.75)	

Standard errors of estimate in parentheses; # indicates significant White test and robust estimate of standard errors.
Results significant at the 10% level on a two-tailed test are marked with *, those at the 5% level with ** and at the 1% level with ***.

Table 4. Key coefficients excluding Subway chain, Illinois only.

Dependent Variable	N	Variable	Coefficient	Equation
ΔFTENS from Position Data	121	Gap	-5.71	(51)
			(13.89)	
gFTENS from Position Data	121	Gap	-0.51	(52)
			(0.85)	
ΔFTENSH from Hours Data	109	Gap	16.53	(53)
			(19.79#)	
gFTENSH from Hours Data	109	Gap	1.23	(54)
			(0.89)	
ΔFTENS from Position Data Limited to Hours Sample	107	Gap	8.39	(55)
			(14.60)	
gFTENS from Position Data Limited to Hours Sample	107	Gap	0.26	(56)
			(0.88)	
ΔFTENS from Hours Data with pay period dummies	109	Gap	22.32	(57)
			(17.26)	
gFTENS from Hours Data with pay period dummies	109	Gap	1.47*	(58)
			(0.86)	

Standard errors of estimate in parentheses; # indicates significant White test and robust estimate of standard errors.
Results significant at the 10% level on a two-tailed test are marked with *, those at the 5% level with ** and at the 1% level with ***.

Powers introduces an ad hoc adjustment factor to explain this inconsistency. It seems better to focus on growth (not absolute change) in all nonsupervisory hours, and especially so since the major motivation for the survey was to include hours data in the Card-Krueger framework.

We have also emphasized in our study the importance of pay period dummy variables for all equations using hours data. Powers' paper includes a set of payroll dummies in only two out of many equations, she never presents estimated coefficients for these dummies, and she fails to take account of them for the bulk of her analysis. Throughout this has the effect of increasing the absolute size of the negative coefficients she obtains in her hours equations. As suggested above these dummies are clearly correcting for important errors in reporting. The pay period dummies are consistently the most significant variables in our analysis. A two week pay period in 2003 was worth about 40% growth in hours. And the inclusion of pay period dummies clearly affects estimates of coefficients for both Illinois dummies and gap variables. Thus, the equation we consider most relevant (equation 19, above) regresses the growth rate in hours on the gap variable and payroll dummies. This gives an insignificant *positive* coefficient of 0.10, effectively zero. Although small and insignificant, the sign here agrees with the Card-Krueger suspicion of positive effects, not Powers' claim of large negative effects.

When used in the equations for just Illinois outlets, the same set of payroll variables produce a result that is almost embarrassing for traditional theory. As observed above, Indiana growth in fast food employment is difficult to explain within the model. Restricting the sample to Illinois outlets, focusing on the interaction between gap and growth in hours and including pay period dummies yields a significant demand elasticity estimate of *positive* 1.17. Excluding

Subway chains from the sample raises this estimate to 1.47. These are both formulations that do not appear in Powers' paper.

We do not claim that these analyses of the Illinois-Indiana data provide definitive evidence of large positive employment effects. The evidence is clearly mixed. There are many, indeed, a majority of negative signs in our tables, although not as many as in Powers' analysis of these same basic data. The point is that the evidence is highly mixed. More often than not coefficients on key variables are insignificant and the data overall lack the power to imply a strong conclusion.

It is our judgment that Powers' eagerness to support the traditional view led her to overstate the message contained in the Illinois-Indiana survey. Our reading of these data suggests that a more accurate conclusion is that the impact of the recent Illinois state minimum wage increase on fast food outlets was not statistically different from zero. However, it would be unreasonable to use these results as a serious challenge to the traditional view of labor demand curves. Just as they are too weak to strongly support that view, they are too weak and uncertain to be turned around as an attack. The range of confidence intervals is just too broad for such a conclusion.

Perhaps it is not surprising that the data collected here cannot resolve the debate launched by Card and Krueger. For all the heat that has been generated, the two sides of that broader debate at their frankest do not differ that much in terms of substantive predictions. Both sides have become more careful in their selection of language. By the end of 2000, Card and Krueger concluded: "The increase in New Jersey's minimum wage probably had no effect on total employment in New Jersey's fast-food industry, and possibly had a small positive effect" (p. 1419). At the same time Neumark and Wascher came to the conclusion: "New Jersey's minimum-wage increase did not raise fast-food employment in that state" (p. 1391). The latter authors' preferred estimate for the wage elasticity of fast food restaurants is about -0.2, a number we suspect Powers would endorse in the Illinois case. If we take Card and Krueger's preferred estimate as 0.0, a number we would find plausible for Illinois, the fact is that there is not that much difference between them. Figure 2 shows two alternative labor demand curves in the relevant range for the Illinois minimum wage increase, one with ε = -0.2 and one with ε = 0.0. It is hard to believe that this difference has motivated the intensity of the debate.

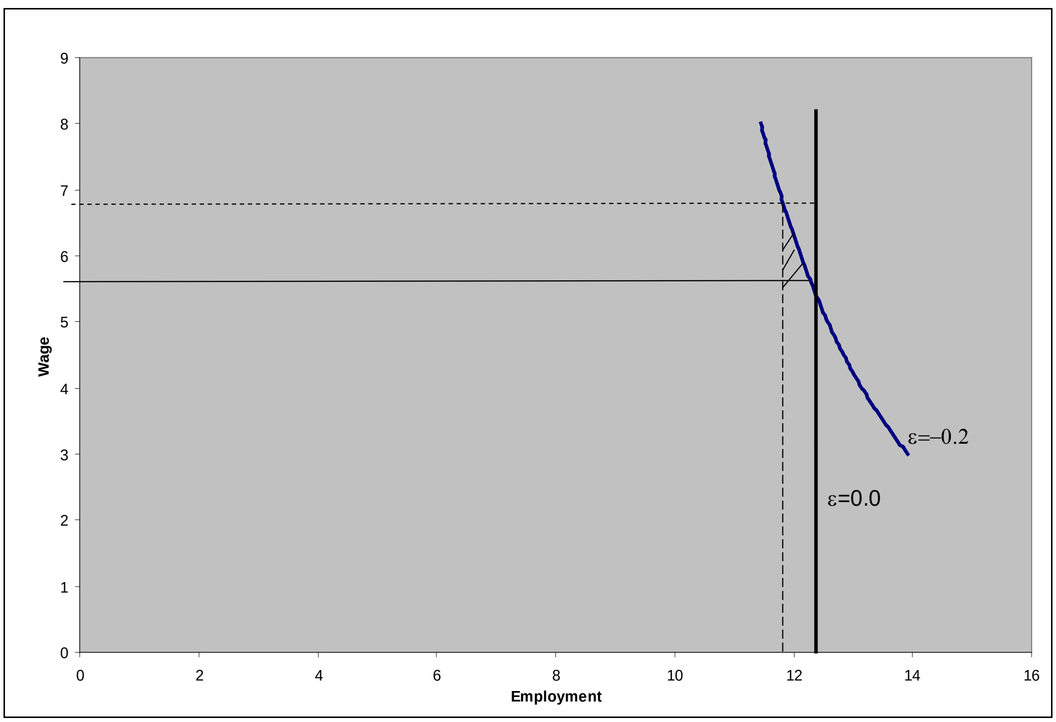

Figure 2. Alternative establishment labor demand curves. An increase in the minimum wage for a typical establishment with a highly inelastic labor demand curve (labeled ε=-0.2 in the figure) generates only modestly less surplus for workers than the same increase in an establishment with a zero-elasticity labor demand curve (labeled ε=0.0 in the figure). The dead weight loss in the first case amounts to only 2.2% of the transfer in the second.

The zero-elasticity curve predicts a welfare gain for fast-food workers in a typical outlet of about $30,700 a year, exactly equal to the loss of surplus born by employers. If, however, the demand elasticity is -0.2, then the minimum wage increase still generates a net increase in the wage bill of about $24,000 or about 78% of the gain with a zero elasticity. If, in addition, workers had any opportunity cost on the hours lost, then the difference is even smaller. For example, if workers value their time at a relatively conservative 30% of the initial minimum wage,[16] the welfare gain to workers rises to $25,600 or about 83% of the gain with a zero elasticity. And at a 50% opportunity cost workers capture $26,700 net or about 87% of the gain with a zero elasticity. From the point of view of employers, now over and above their transfer to workers they suffer a relatively small deadweight loss equal to the shaded area in Figure 2, amounting to about $700 or 2.2% of the initial transfer to workers with a zero elasticity demand curve. Put somewhat differently, the workers' net gain of $26,700 is achieved for an efficiency loss of $700, amounting to an excess burden on the transfer of only 2.3%, far lower than estimated excess burdens from funds originating in income tax transfers.

Where hypothesized values are close, researchers may interpret even the best quality data quite differently. When data like ours is clearly subject to numerous and difficult-to-model measurement errors, perhaps there is no wonder that readings may differ substantially. In the end, however, we suggest that at least with respect to fast food establishments, the welfare implications of one side or the other are not that different.

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[16] See Persky, Felsenstein, and Carlson (2004) for a discussion of the opportunity cost of low wage workers.