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Publication Bias in Minimum-Wage Research? Card and Krueger Redux

T.D. Stanley and Hristos Doucouliagos

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Abstract:

Card and Krueger's (1995) meta-analysis of the employment effects of minimum wages challenged existing theory. Unfortunately, their meta-analysis confused publication bias with the absence of a genuine empirical effect. Recently developed meta-analysis methods corroborate that Card and Krueger's findings were nevertheless correct. The minimum wage effects literature is contaminated by publication bias. Once this publication bias is corrected, no time-series evidence of a negative association between minimum wages and employment remains. JEL: J20, C12, C13.

"(P)ublication bias is leading to a new formulation of Gresham's law—like bad money, bad research drives out good." – Bland (1988, p. 450)

A decade ago, Card and Krueger (1995b) created a schism within economics by reporting quasi-experimental and econometric evidence that minimum wage increases do *not* decrease employment. One part of Card and Krueger's (C-K) empirical evidence is a meta-analysis of the time-series studies on minimum-wage effects Card and Krueger (1995a). This meta-analysis had three effects. First, predictably, their 1995 paper also created its own controversy (e.g. Neumark and Wascher, 1998; *The Economist*, 2001; Card and Krueger, 2000; Neumark and Wascher, 2000; and Burkhauser *et al.* 2001). Second, it stimulated a reassessment of the underlying theory, with models developed that could accommodate C-K's results (e.g. Manning, 1995; Azam, 1997; and De Fraja, 1999). Third, many economists adopted C-K's meta-analytic methods. Unfortunately, regardless of the validity of underlying theoretical considerations, C-K's methods mistake publication bias with the absence of an empirical effect. This error has been repeated by others following C-K's methodology, and there is a growing risk that it will become standard practice (*e.g.*, Görg and Strobl, 2001; Doucouliagos and Laroche, 2003; Mookerjee, 2006).

The purpose of this paper is to correct a few methodological liberties taken during this debate, to re-meta-analyze the time-series evidence on minimum-wage effects, and to offer valid meta-analytic methods that differentiate genuine empirical effects from publication selection bias. We show that although there are problems with Card and Krueger's meta-analysis approach, their conclusion regarding the existence of publication bias in this literature is largely correct. More importantly, once the effects of publication bias are filtered out, the evidence points to either a zero or a *positive* association between minimum wages and employment. Regardless, a negative employment effect is not supported by the research record.

I. The Card and Krueger Approach

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The purpose of C-K's meta-analysis is to assess the weight of time-series evidence on the minimum wage's reduction in employment, "One of the best-known predictions of standard economics" (Card and Krueger, 1995a, p. 238). To do so, they concentrate on the relation of a study's *t*-value to its sample size (or degrees of freedom). The idea is simple and based on the well-known property of statistical power—namely, that power rises with the sample size. However, power and the magnitude of the *t*-value will rise with sample size, *ceteris paribus*, only when the null hypothesis under test is false. In fact, one can be more precise about this relationship. "(T)he absolute value of the *t* ratio should vary proportionately with the square root of the degrees of freedom, and a regression of the log of the *t* ratio on the log of the square root of the degrees of freedom should yield a coefficient of 1" (Card and Krueger, 1995a, p. 239). The problem is that C-K neglect to condition this relationship on the existence of a genuine empirical effect (i.e., H_1: $\beta \neq 0$, where β represents the elasticity of minimum wage in the teen employment equation).

When the null hypothesis under test is true, E(t)=0 for all sample sizes. Hence, there will be no relationship between a study's sample size (or degrees of freedom) and its *t*-value. The simple answer to C-K's rhetorical question: "What might prevent the *t* ratio from rising with sample size?"(1995a, p. 239) is that the minimum wage has no employment effect. Or, if there is an effect, is it either too small to be of economic significance or it is ambiguous. Any of these empirical facts can explain the absence of an empirical relation between reported *t*-ratios and sample sizes. C-K interpret the nonappearance of an observed relationship between a study's *t*-ratio and its degrees of freedom as evidence of selection bias in minimum-wage research. Although they also recognize that an absence of a relationship between *t* and *df* may be alternatively explained by fortuitous structural change that lessens the minimum-wage effect over time, they emphasize the role of specification searching and publication selection bias (Card and Krueger, 1995a, p. 242). Unfortunately, this oversight has been repeated several times by economists.[1]

Of course, the absence of a relationship between a study's *t*-ratio and its degrees of freedom may indeed be caused by publication selection. For instance, if there were no genuine effect and yet papers are selected entirely on the basis of their statistical significance, then the reported *t*-ratios would likely vary around two or so, regardless of the degrees of freedom. Furthermore, should there be an effect, publication bias will attenuate the empirical trace of statistical power that this relationship between *t* and *df* represents—potentially erasing it altogether (Stanley, 2005). The point is that an *observed absence* of a significant relationship between a study's reported standardized test statistic and its degrees of freedom *does not imply publication bias*. Rather such a finding is consistent with either publication selection or the simple nonexistence of the investigated empirical effect. To differentiate between these potential causes requires further investigation.

II. Filtering Publication Bias from Minimum-Wage Research

"However, even a careful review of the existing published literature will not provide an accurate overview of the body of research in an area if the literature itself reflects selection bias."

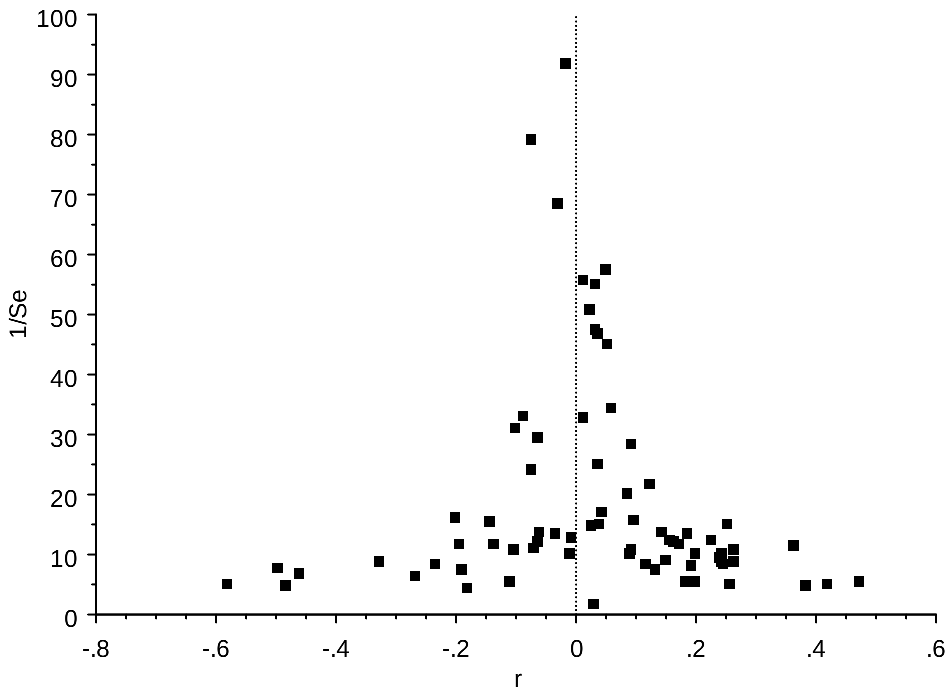
—De Long and Lang (1992, p. 1258)

The key research issue is whether newer methods of meta-analysis also find publication bias in labor research, and whether a meaningful minimum-wage effect remains after likely publication bias is filtered from this research literature. In order to address these and related questions, recent developments in MRA must be surveyed briefly.

"The simplest and most commonly used method to detect publication bias is an informal examination of a funnel plot" Sutton *et al.* (2000, p.1574). A funnel graph is a scatter diagram of precision vs. estimated effect (such as estimated elasticities, regression coefficients, correlation coefficients, *etc*.). Precision is best measured by the inverse of the standard error (*1/Se*). Figures 1 and 2 provide two economic illustrations.

As the name suggests, the expected shape is an inverted funnel—*in the absence of publication selection*. When there is no publication selection, estimates should vary randomly and *symmetrically* around the 'true' population effect. Because small-sample studies with typically less precision form the base of the graph, the plot will be more spread out there than at its top. However, it is the graph's symmetry (or its absence) that is crucial for assessing publication bias. Note that symmetry is still possible even with all estimates in one direction (positive or negative).

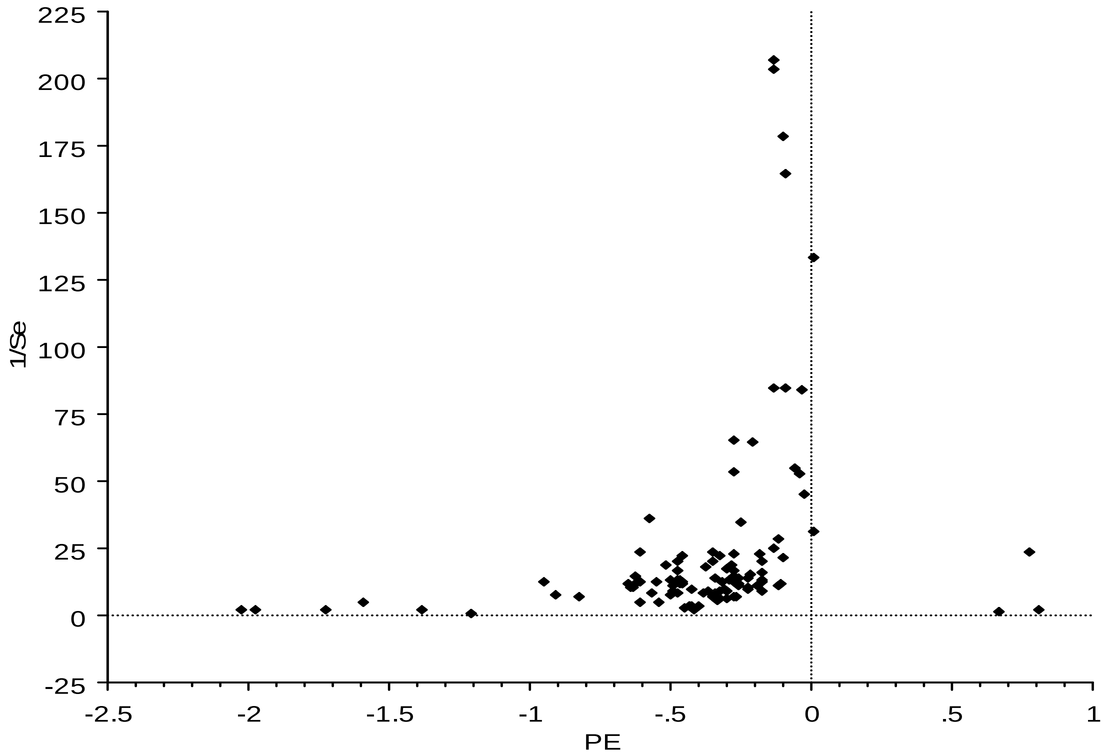
Figure 1: Funnel Plot, Union-Productivity Partial Correlations (r)



Source: Doucouliagos and Laroche (2003)

Should the plot be over-weighted on one side or the other, this is taken as evidence of publication selection. In Figures 1 and 2, we have an obvious example of a symmetric funnel graph and thus the absence of publication bias (Figure 1), and an equally obvious skewed diagram that reflects publication selection (Figure 2). There are theoretical reasons supporting both positive and negative effects of union membership on worker productivity (Doucouliagos and Laroche, 2003). Thus, the apparent absence of asymmetry in Figure 1 is consistent with accepted theoretical presuppositions.

Figure 2: Funnel Graph of Price Elasticities (PE) for Water Demand

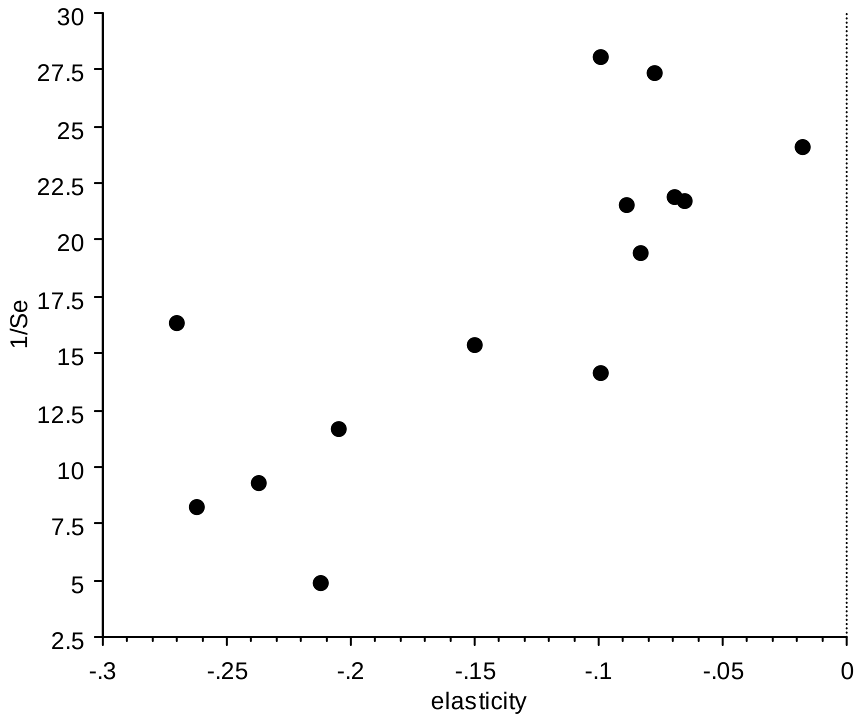


Source: Dalhuisen *et al.* (2003)

In contrast, economists expect the own price elasticity to be negative (*i.e*., the ‘Law’ of demand). Hence, the funnel graph of estimated price elasticities of water demand (Figure 2) is clearly skewed to the left, illustrating evident publication/reporting selection. Undoubtedly, many economists use the sign of price elasticity as a specification test and would regard the discarding of a clearly ‘erroneous’ estimate (*i.e.,* a positive one) as simple quality control.[2] Doing so, however, greatly distorts the average estimate of price elasticity, by a factor of three or four, and thereby has important policy implications (Stanley, 2005).

Because Card and Krueger’s meta-analysis contains so few estimates, its funnel graph is more difficult to interpret—see Figure 3.[3] Nonetheless, it should be clear that it is not symmetric and most likely represents the bottom, left half of a funnel (see the previous figures). Thus, a casual inspection of a funnel graph reveals selection for negative minimum-wage effects.

Figure 3: Funnel Graph of Estimated Minimum-Wage Elasticities



Source: Card and Krueger (1995a)

Graphs are, unfortunately, vulnerable to subjective interpretation. An objective statistical test for modelling publication selection involves the simple MRA between a study's reported non-standardized effect (*e.g.,* estimated elasiticities, partial correlations, *etc.*) and its standard error (Card and Krueger, 1995a; Ashenfelter *et al*., 1999; Görg and Strobl, 2001; Monkerjee, 2006):

$$effect_i = \beta_1 + \beta_0 Se_i + \varepsilon_i \tag{1}$$

Equation (1) is the explicit representation of C-K's second MRA model for publication bias (Card and Krueger, 1995a, p. 241). In the absence of publication selection, observed effects should vary randomly around the 'true' value, β_1, independently of the standard error. When all studies are selected for statistical significance, publication bias will be proportional to the standard error—$\beta_0 Se_i$.[4] Authors of smaller studies are more likely to engage in specification searchers, on average, to find the sufficiently large estimated effects needed to compensate for their associated larger standard errors

With increased observations, *Se* will become smaller, approaching zero as the sample size grows indefinitely, and the reported effects will approach β_1, the 'true' effect (Sutton *et al*., 2000, Macaskill *et al*., 2001). Correspondingly, the degree of publication bias, $\beta_0 Se_i$, shrinks to zero with the error variance. Larger samples can be expected to contain smaller publication biases.

Studies use different sample sizes and modelling variations. Hence, the random estimation errors of this MRA model, ε_i in equation (1), are likely to be heteroscedastic. In an unusual econometric twist, the independent variable, Se_i, is a sample estimate of the standard deviation of these meta-regression errors. Dividing equation (1) by this measure of the heteroscedasticity (Se_i) gives:

$$t_i = \beta_0 + \beta_1(1/Se_i) + e_i \qquad (2)$$

where t_i is the conventional *t*-value for $effect_i$. The intercept and slope coefficients are reversed, and the independent variable becomes the inverse of its previous incarnation. Equation (2) is the WLS version of the MRA model (1), and it provides valid tests for both the presence of publication bias and for genuine effect beyond publication bias (Stanley, 2005; Stanley, 2007).

The conventional *t*-test of the intercept of equation (2), β_0, is a test for publication bias, and its estimate, b_0, indicates the direction of this bias—see (Egger *et al.* 1997). Thus, testing β_0 may be considered the funnel graph's asymmetry test (FAT) (Sutton *et al*., 2000).[5]

Column 1 Table 1 reports FAT for Card and Krueger's original data on minimum-wage effects.[6] It contains evidence of publication bias (that is, selection for the unemployment effects of the minimum-wage) in minimum-wage research (reject H_0: $\beta_0=0$; $t=-3.49$; $p<0.01$).[7] Thus, Card and Krueger's (1995a) view and our interpretation of the funnel graph (Figure 3) that there is publication bias in the minimum-wage literature is confirmed by explicit meta-regression tests for publication bias.

Table 1: MRA Tests for Publication Bias and Genuine Empirical Effect
(Dependent Variable, Y= t or ln|t|)

Moderator Variables:	*Column 1: Y= t MRA Model (2)*	*Column 2: Y=ln|t| MRA Model (3)*	*Column 3: Y=t MRA Model (4)*
Intercept	-2.01(-3.49)***	2.03(1.39)	-2.37 (-6.01)***
1/Se	0.002 (0.06)	–	0.192 (3.45)***
ln(df)	–	-0.40 (-1.01)	–
Year/Se	–	–	0.007 (3.24)***
Lag/Se	–	–	-0.074 (-3.71)***
Auto/Se	–	–	-0.089 (-2.93)***
Un/Se	–	–	-0.190 (-4.94)***
n	14	15	14
R^2	0.0002	0.093	0.759
Standard Error	0.969	0.510	0.583

t-values are reported in parenthesis and are calculated from heteroscedasticity-consistent standard errors. *** denotes statistically significant at the 1 percent level.

This MRA (Column 1 Table 1) can also be used to test for a genuine effect beyond publication bias. The coefficient on precision, β_1, can be considered an estimate of empirical effect corrected for publication selection (Sutton *et al.*, 2000, Macaskill *et al.*, 2001, Stanley, 2005).[8] Applying this precision effect test (PET) to C-K's data finds no evidence of an employment effect from minimum wages (accept H_0: $\beta_1=0$; $t=0.06$; $p>>0.05$).

There is a second MRA model that can be used to test for an empirical effect beyond publication bias. Meta-significance testing (MST) uses the same model as do C-K,

$$E(ln|t_i|) = \alpha_0 + \alpha_1 lndf_i \tag{3}$$

but is interpreted differently (Stanley, 2001, Stanley 2005). If we can reject $H_0{:}\alpha_1 \leq 0$, then there is evidence of an empirical effect irrespective of publication selection. Column 2 of Table 1 again finds no evidence of a genuine negative employment effect from minimum wages (accept $H_0{:}\alpha_1 \leq 0$, $t=-1.01$; $p>>0.05$).

III. Can Structural Change Explain the Absence of an Employment Effect?

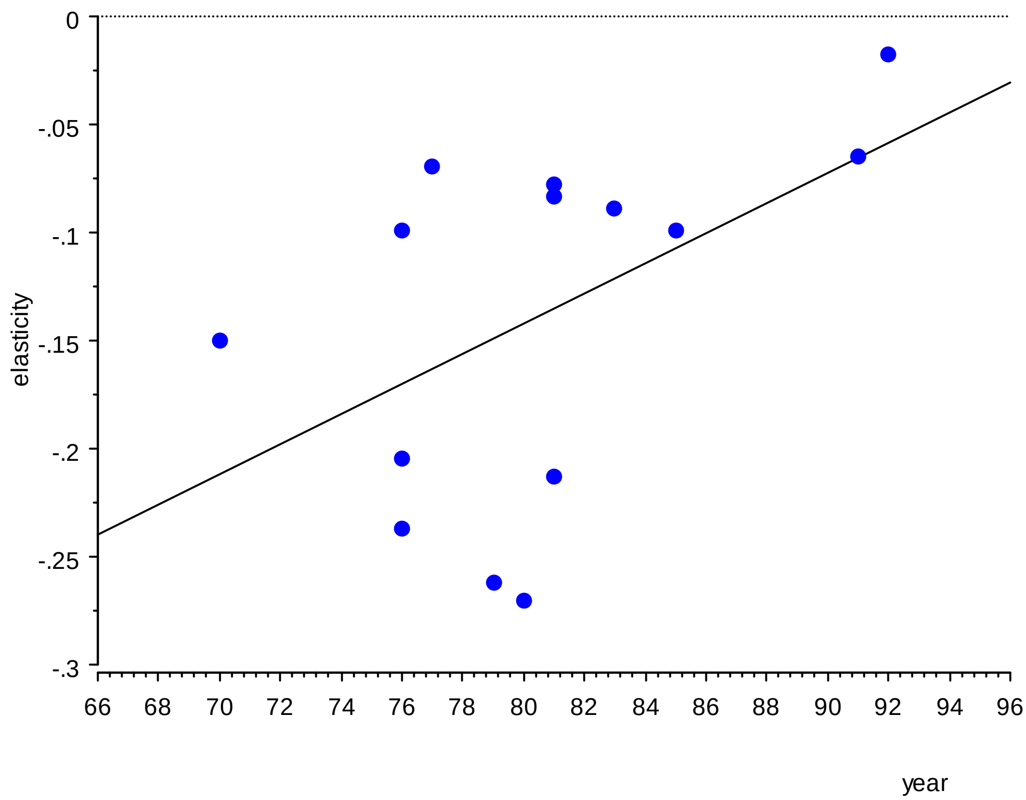
Figure 4 presents an alternative way to look at this literature, tracing changes in the reported minimum-wage effect on teen employment over time. Note the upward trend. The negative employment effects from raising the minimum wage are reported to have moderated over time. Because most research in this area uses the Katz index that explicitly accounts for the effective magnitude of the minimum wage, this fall in minimum wage's effect is not the result of a declining real minimum wage. However, this decline could be due to an actual lessening of minimum wage's impact over time—*i.e.*, 'structural change.' Or perhaps, having established minimum wage's negative effects, it becomes more novel, hence more publishable, to report modest or insignificant effects?

Like any regression model, the estimates of MRA's coefficients can become biased when important explanatory variables are omitted. Adding moderator variables, $\Sigma\alpha_k Z_{ik}$, to equation (1) to explain variation in true effect lead to:

$$t_i = \beta_0 + \beta_1(1/Se_i) + \Sigma\alpha_k Z_{ik}/Se_i + e_i \tag{4}$$

in the place of equation (2). Accordingly, Column 3 Table 1 reports the MRA model for the minimum-wage effects on teen employment after adding several explanatory variables to allow for heterogeneity in the minimum-wage effect. These moderator variables reflect whether a study uses a lagged value of the dependent variable in the original model of teen employment (*Lag/Se)*, divided by the standard error of the estimated elasticity, if the researchers made a technical correction for autocorrelation (*Auto/Se*), used the unemployment rate as a cyclical indicator (*Un/Se*), or the year in which the study was published (*year/Se*). This latter term may be used to allow for 'structural change' in the effects of minimum wages on employment.

Figure 4: Time Series Pattern of Employment Elasticities for Minimum Wage Raises



The results presented in Column 3 are revealing. First, note that once again there is strong evidence of publication selection bias (t=-6.01; p<0.001). Next, the upward trend is statistically significant, even after other factors affecting minimum-wage effects are accounted for (t=3.24; p=0.01).

Second, there appears to be evidence of a genuine *positive* employment effect after correcting for publication bias and allowing for structural change (t=3.45; $p<0.01$). This is, of course, the opposite of what conventional economics demands, but it is consistent with alternative explanations that predict positive employments—including efficiency wages (Manning, 1995 and Azim, 1997), contracts involving working conditions (De Fraja, 1999), non-wage compensation (Simon and Kaestner, 2004) and oligopsony and market power resulting from heterogenous worker preferences (Bhaskar *et al.,* 2002). Thus Card and Krueger's most controversial claim, namely that minimum wages raises have positive employment effects (Card and Krueger, 1995b), is corroborated by a meta-regression analysis that corrects for publication bias. However, with heterogeneity, genuine effect is not so simple. Now, it will depend on the combination of MRA coefficients for specific values of the moderator variables.

It is well known that the time series of employment (or unemployment), teenage or otherwise, is nonstationary. Not properly accounting for the dynamics of employment will almost certainly bias all of these reported estimates of the minimum-wage employment effect. The only exception is the highly unlikely event that employment and the Kaitz index, which is typically used to measure the effective minimum wage rate, happen to be cointegrated. At a minimum, researchers in this area of labor economics need to include a lagged value of the dependent variable as an approximation to teen employment's dynamics.[9] Incorporating a lagged value of teen employment into a researcher's estimation model has a significantly negative impact on the reported minimum wage effects (t=-3.71; $p<0.01$). At a minimum, we should add coefficients on both *1/Se* and *Lag/Se*) to serve as our estimate of the minimum-wage employment effect after correcting for publication bias. However, this sum (.117) is not significantly different from zero ($F_{(1,8)}= 3.28$; $p>0.05$)— implying no genuine employment (positive or negative) effect from minimum wages. If we consider the use of the unemployment rate as a cyclical indicator as part of the 'best practice' in this area of research, then its coefficient should also be added as part of the minimum wage effect. Doing so lowers the overall estimate to -.072, which is still not different than zero ($F_{(1,8)}= 3.70$; $p>0.05$).

Then there is also the issue of structural change. *Year/Se* is also statistically significant; thus the best estimate of the magnitude of the minimum-wage effect increases by .007 each year after 1970. Including structural change can bring this overall estimate back into the positive territory for the later years in C-K's sample, but not significantly so. Lastly, we would argue that it is better not to make a technical adjustment (*Auto/Se=0*) for autocorrelation (*Auto/Se=0*) because doing so only masks the misspecified employment dynamics. Regardless of what one considers to be the best practice of labor research, correcting for publication bias reveals either a positive or an insignificant employment effect.

IV. Conclusion

This paper re-evaluates the time-series evidence of a minimum-wage effect on teen employment. Several meta-regression tests corroborate Card and Krueger's overall finding of an insignificant (both practically and statistically) employment effect from minimum-wage raises. Recently developed tests for publication selection bias confirm its presence in this area of labor research. The time-series research on minimum-wage effects contains the clear trace of selection for negative employment effects.

No evidence of a genuine disemployment effect can be found among time series estimates of minimum-wage elasticities of teen employment, but they contain a clear indication of publication bias. Recall that quasi-experimental evidence corroborates minimum wage's insignificant (both practically and statistically) employment effect (Card and Krueger, 1995b). There never was much empirical evidence of a negative employment effect from minimum-wage increases (Leonard, 2000). In any case, there seems to be a consensus among labor economists that if there is a negative employment effect it is a small one (*The Economist,* 2001), and our meta-analytic re-assessment of Card and Krueger's data corroborates this.

In reviewing Card and Krueger's meta-analysis, we identified problems in their use of meta-analysis. In the place of their MRA methods, we offer alternatives that are validated through Monte Carlo simulations and by extensive applications in other fields of economic research (Roberts and Stanley, 2005, Stanley, 2005; Stanley, 2007). Funnel asymmetry (FAT) and precision-effect testing (PET) offers great promise for the rigorous empirical study of economic research.

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Appendix

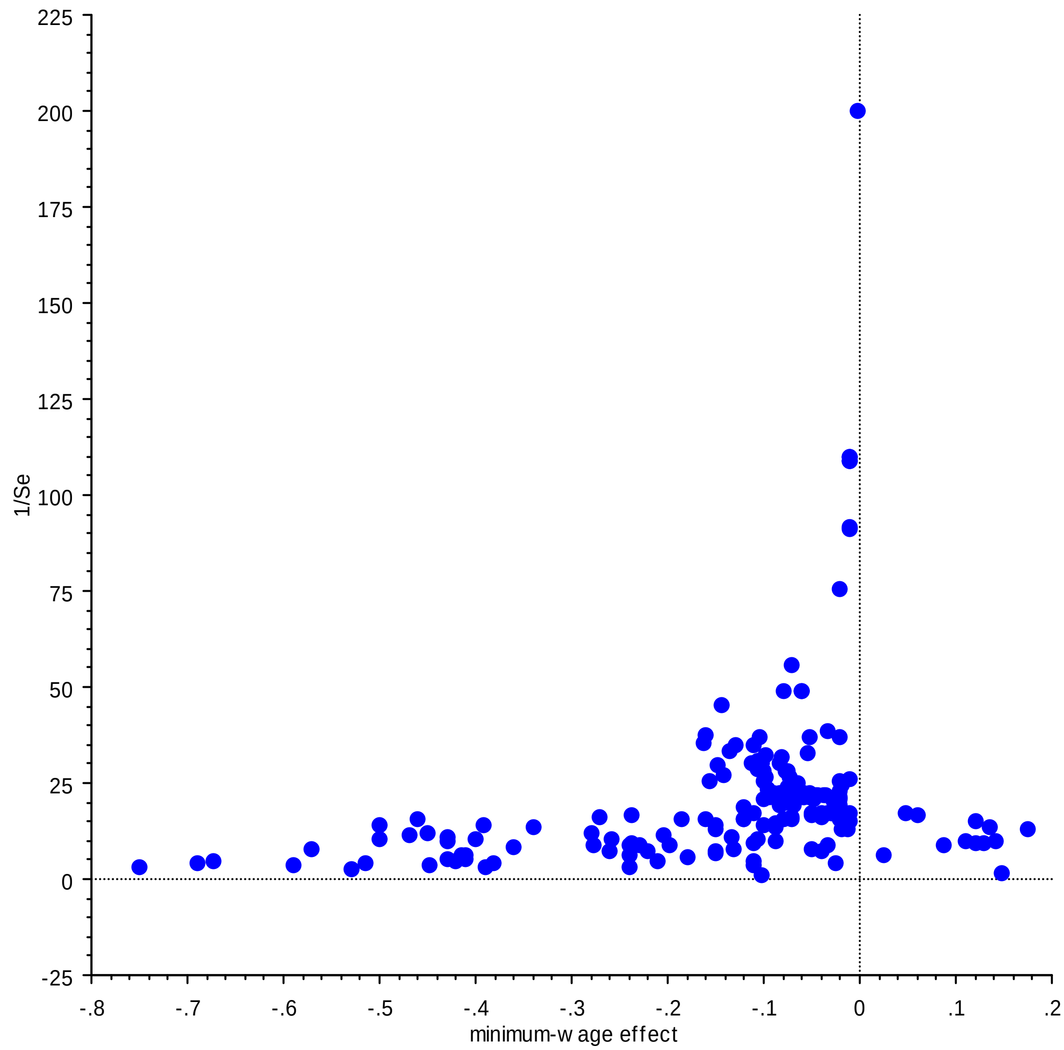
In this appendix, we replicate the meta-analyses reported in Section 3 of this paper for the larger sample of minimum-wage effects reported by fifteen studies that Card and Krueger (1995a) identified. We have made the effort of fully coding the original studies to insure that our findings are robust, to avoid adding to the selection bias that we have already identified in this area of research, and to increase our sample size and thereby the reliability of our meta-regression results. We used the most aggregate estimates available. However, some studies reported disaggregated minimum-wage effects by either age, or race, or gender. As a result, we have 192 estimates with sufficient information to use in our meta-regression analyses.

Figure 4 is the funnel graph for this larger set of minimum-wage effects. The largest and smallest four estimates were deleted to make the shape of the remaining 184 effects clear. Again, the asymmetry of the funnel graph is obvious. This selection preference for significantly negative minimum-wage effects is also confirmed in the MRAs found in Appendix Table 1.

Appendix Table 1 reports the same MRA models and tests as those presented in Table 1 but now for the much larger sample of effects. Appendix Table 1 corroborates our previous findings. In particular, the intercepts of both MRA model (2) and (4) are significantly negative, indicating funnel asymmetry and a selection preference for negative minimum-wage effects. Thus, FAT confirms our previous results (reject H_0: $\beta_0=0$; $t=-8.67$ & -6.71; $p<0.0001$). Precision-effect tests (PET), on the other hand, concern the existence of genuine minimum-wage effect beyond publication bias. They too corroborate previous findings. For MRA model (2), PET is again insignificant. For the multivariate MRA model (4), the estimate of β_1 is again positive, but now insignificantly so (accept H_0: $\beta_1=0$; $t=1.32$; $p>0.05$). As discussed in Section 3, with heterogeneity no single coefficient represents the 'true' minimum-wage effect corrected for publication bias. Thus, we only emphasize how there is no statistical support for a negative minimum-wage effect regardless of how one views the best research practice. This interpretation of minimum-wage research is also confirmed by the larger dataset. Regardless of what values we use for the moderator variables in column 3 Appendix

Table 1, the estimated minimum-wage effect will be positive, but not statistically significant.

Figure 4: Funnel Graph of Estimated Minimum-Wage Effects (n=184)



Appendix Table 1: MRA Tests for Publication Bias and Genuine Empirical Effect

(Dependent Variable, Y= t)

Moderator Variables:	*Column 1: Y= t MRA Model (2)*	*Column 2: Y=t MRA Model (4)*
Intercept	-1.63(-8.67)***	-1.37 (-5.78)***
1/Se	-0.0056 (-0.82)	0.183 (1.19)
ln(df)	_	_
Year/Se	_	-0.002 (-1.22)
Lag/Se	_	-0.015 (-0.79)
Auto/Se	_	0.039 (2.58)**
Un/Se	_	-0.039 (-3.29)***
n	192	192
R^2	0.006	0.096
Standard Error	1.69	1.63

t-values are reported in parenthesis and are calculated from bootstrapped standard errors using 1000 replications with replacement. **, *** denote statistically significant at the 5 and 1 percent levels, respectively.

[1] For example, Görg and Strobl (2001) assess the spillover effects of multinational corporations, using the same meta-regression model to identify publication selection, and incorrectly interpret the nonexistence of the expected statistical relationship between degrees of freedom and a study's *t*-value as evidence of publication bias (p. F735). Likewise, citing Card and Krueger (1995a), Doucouliagos and Laroche (2003, p. 670) regress the logarithm of the absolute value of the study's *t*-ratio and the logarithm of the square root of its degrees of freedom as a test of publication bias among studies of union-productivity effects. More recently, Monkerjee (2006) uses these same methods in his meta-analysis of the export growth hypothesis.

[2] Not reporting a positive estimated price elasticity on the grounds that doing so will increase the accuracy of his reported (negative) estimate actually decreases the accuracy of the estimated price elasticity. If everyone possesses this same selection bias, the research base itself will greatly exaggerate the magnitude of the estimated price elasticity, on average. This is another example of the fallacy of composition.

[3] Card and Krueger (1995a) selected only one estimate from each study for their meta-analysis. Yet, these same studies contain many more estimates of the minimum wage effect, n=192. We have included an

appendix that analyzes this larger set of estimates to make sure that Card and Krueger (1995a) did not inadvertently introduce their own selection bias and to increase the sample size for our meta-regression analysis.

[4] See Stanley (2005 and 2007) for a more comprehensive discussion of these MRA models and their statistical properties. This strict proportionality will hold only when there is no empirical effect (β_1=0). Should $\beta_1 \neq 0$, the second term of equation (1) will not be linear.

[5] To understand the relation of equation (2) with the funnel graph, first invert the funnel by plotting *Se* vs. *effect*. Next, rotate the funnel ninety degrees, reversing the axes. Equation (1) results from inverting, rotating and interpreting the funnel graph as a regression relation. As discussed above, equation (2) is merely the WLS version of equation (1).

[6] Appendix Table 1 reports the corresponding results for the 192 estimates of minimum wage effect that are contained in these same studies.

[7] When coding the standard errors for minimum-wage effects, the standard error of one study could not be calculated, Regan (1981). Thus, one observation is lost. It should also be noted that we get all of the same MRA test results when the square root of degrees of freedom is used as a proxy for $1/Se_i$.

[8] Unfortunately, this MRA coefficient is a biased estimate when $\beta_1 \neq 0$. Nonetheless, testing H_0: $\beta_1=0$ provides a valid and powerful test for genuine effect beyond publication selection bias (Stanley, 2007).

[9] Researchers should also have thoroughly tested for stationarity and cointegration and used these results to choose the order of differencing. But then none do so in this area of labor economics. Thus, including lags is the best control for employment's dynamics observed in the actual research record.