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Publication Selection Bias in Minimum-Wage Research? A Meta-Regression Analysis

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Abstract

Card and Krueger's meta-analysis of the employment effects of minimum wages challenged existing theory. Unfortunately, their meta-analysis confused publication selection with the absence of a genuine empirical effect. We apply recently developed meta-analysis methods to 64 US minimum-wage studies and corroborate that Card and Krueger's findings were nevertheless correct. The minimum-wage effects literature is contaminated by publication selection bias, which we estimate to be slightly larger than the average reported minimum-wage effect. Once this publication selection is corrected, little or no evidence of a negative association between minimum wages and employment remains.

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

1. Introduction

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 article also created its own controversy (e.g. Burkhauser *et al.* 2000; Card and Krueger 2000; Neumark and Wascher 1998, 2000). Second, it stimulated a reassessment of the underlying theory, and models were developed that could accommodate C-K's results (e.g. Adam and Moutos 2006; Azam 1997; Bhaskar and To 1999; Bhaskar

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et al. 2002; De Fraja 1999; Deltas 2007; Manning 1995; Walsh 2003). 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 selection with the absence of an empirical effect (Stanley 2005). 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. Doucouliagos and Laroche 2003; Görg and Strobl 2001; Mookerjee 2006).

The purpose of this article is to replicate and extend C-K's meta-analysis of the minimum-wage effect employing valid meta-analytic methods that differentiate genuine empirical effects from publication selection bias. We show that although there are problems with C-K's meta-analysis, their conclusion regarding the existence of publication selection in this literature is largely correct. More importantly, once the effects of publication selection are filtered out, an adverse employment effect is *not* supported by this large and rich research record on the employment effects of minimum-wage regulation. This conclusion is drawn from an extensive meta-analysis of 64 minimum-wage studies that combined offer 1,474 estimates of the employment elasticity.[1]

2. Filtering publication selection bias from minimum-wage research

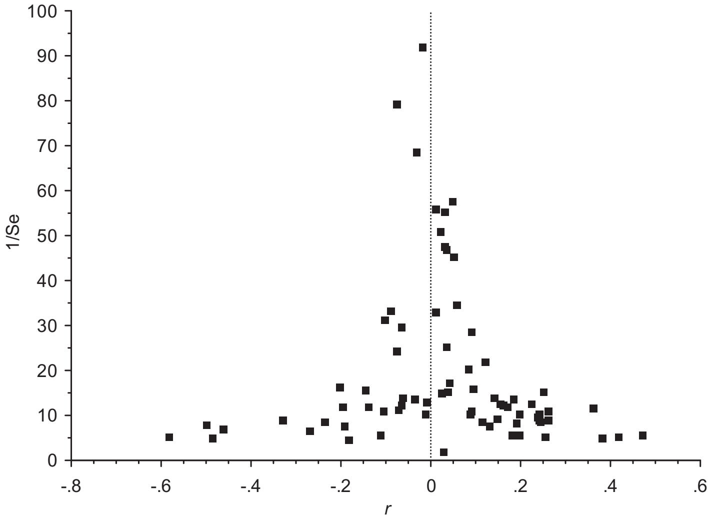
> However, even a careful review of the existing published literature will not provide an accurate overview of the body of research in an area if the literature itself reflects selection bias. (De Long and Lang 1992: 1258)

The key research questions for this article are whether newer methods of meta-analysis also find publication selection in labour research after the research base is extended and updated, and whether meaningful minimum-wage employment effects remain after likely publication selection is filtered from this research literature. In order to address these and related questions, recent developments in meta-regression analysis (MRA) must be briefly surveyed.

'The simplest and most commonly used method to detect publication selection is an informal examination of a funnel plot' (Sutton *et al.* 2000a: 1574). A funnel graph is a scatter diagram of precision versus estimated effect (such as estimated elasticities, regression coefficients or partial correlation coefficients). Precision is best measured by the inverse of the standard error ($1/Se$).

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 selection (see Figure 1). *Note that symmetry is still*

FIGURE 1
Funnel Plot, Union-Productivity Partial Correlations (r).



Source: Doucouliagos and Laroche (2003).

possible even when all estimates have the same sign, whether positive or negative.

Should the plot be overweighted 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 selection (Figure 1) and a 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.

Because C-K's meta-analysis contains so few estimates, its funnel graph is more difficult to interpret — see Figure 3. Nonetheless, it should be clear that it is not symmetric and most likely represents the bottom, left portion of a funnel (compare Figures 2 and 3). Thus, a casual inspection of these funnel graphs reveals selection for negative minimum-wage effects.

This casual inspection is further confirmed when C-K's sample of 15 studies and estimates is extended to include 1,474 estimates in 64 studies (see Figure 2).[2] With this extended sample, the funnel shape is much clearer. Although positive elasticities are reported in this literature, the asymmetry of the funnel graph becomes even clearer after over a thousand estimates are added and fill in the funnel. Note that all of C-K's estimates fall into the densest, lower left side of the funnel graph (Figure 2).

Graphs are, unfortunately, vulnerable to subjective interpretation. An objective statistical test for modelling publication selection involves the

FIGURE 2
Trimmed Funnel Graph of Estimated Minimum-Wage Effects (n = 1,424).

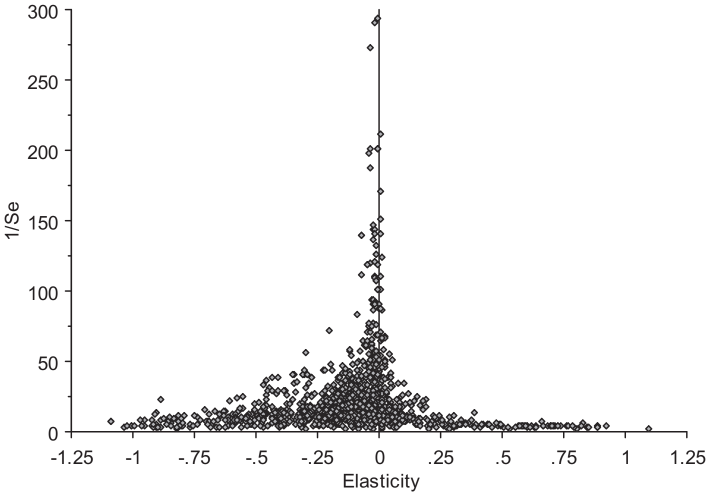
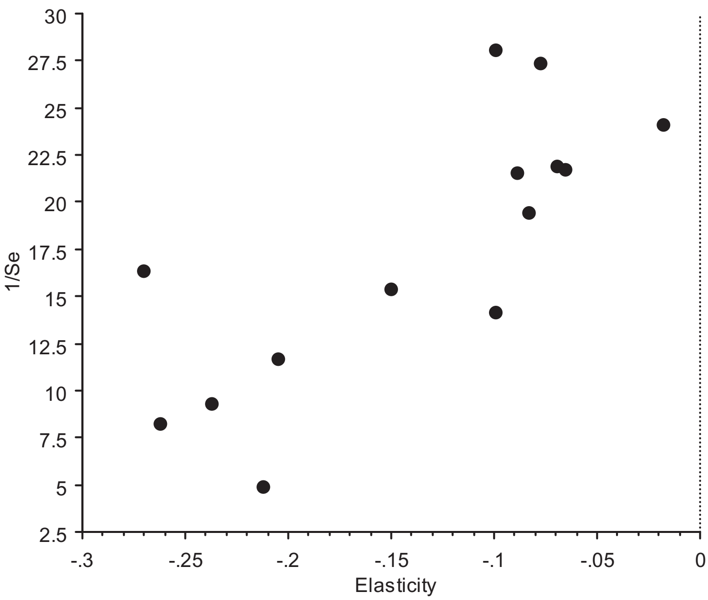


FIGURE 3
Funnel Graph of Card and Krueger's Estimated Minimum-Wage Elasticities.



Source: Card and Krueger (1995a).

simple MRA between a study's reported effect (e.g. estimated elasiticities, partial correlations, etc.) and its standard error (Ashenfelter *et al.* 1999; Card and Krueger 1995a; Görg and Strobl 2001; Mookerjee 2006):

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

where e_i is an estimated elasticity, and Se_i is its standard error. Equation (1) is the explicit representation of C-K's second MRA model for publication selection (Card and Krueger 1995a: 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 selection bias will be proportional to the standard error — $\beta_0 Se_i$.[3] Authors of smaller studies are, on average, more likely to engage in specification searches 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. Correspondingly, the amount of publication selection, $\beta_0 Se_i$, shrinks to zero with the error variance. Studies using larger samples can be expected to report smaller publication biases.

In economics, research studies use different sample sizes and different econometric models and techniques. 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:

$$t_i = \beta_0 + \beta_1(1/Se_i) + v_i \tag{2}$$

where t_i is the conventional *t*-value for the estimated minimum-wage elasticity, e_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 MRA model (1), and it can provide a valid test for both the presence of publication selection and for genuine effect beyond publication selection (Stanley 2005, 2008).

The conventional *t*-test of the intercept of equation (2), β_0, is a test for publication selection, and its estimate, $\hat{\beta}_0$, indicates the direction and magnitude of this bias — see Egger *et al.* (1997), Doucouliagos and Stanley (2008) and Stanley (2008). Thus, testing β_0 may be considered the funnel graph's asymmetry test (FAT).[4]

Column 1 of Table 1 reports FAT for C-K's original data on minimum-wage effects. It contains evidence of publication selection (i.e. selection for negative employment effects of the minimum-wage) in minimum-wage research (reject H_0: $\beta_0 = 0$; $t = -3.49$; $p < 0.01$).[5] Thus, Card and Krueger's (1995a) view and our interpretation of the funnel graph (Figure 3) that there is publication selection in the minimum-wage literature is confirmed by explicit meta-regression tests for publication selection.

TABLE 1
MRA Tests for Publication Selection Using Card and Krueger's Data (Dependent Variable, $Y = t$ or $\ln|t|$)

Moderator variables	*Column 1* $Y = t$ *MRA model (2)*	*Column 2* $Y = \ln|t|$ *MRA model (3)*
Intercept (β_0)[c]	**–2.01** (–3.49)[a]	2.03 (1.39)
$1/Se$[c]	0.002 (0.06)	—
$\ln(df)$	—	–0.40 (–1.01)
n[b]	14	15
k[b]	14	15
R^2	0.0002	0.093
Standard error	0.969	0.510

[a] t-values are reported in parentheses and are calculated from heteroscedasticity-consistent standard errors. Cells in bold denote statistical significance at least at the 5 per cent level.
[b] n denotes the number of elasticity estimates and k denotes the number of independent studies.
[c] If the literature is free of publication bias, the intercept in column 1 should not be statistically significant. The coefficient on $1/Se$ measures the minimum-wage employment effect, corrected for publication selection.
MRA, meta-regression analysis.

This MRA (Table 1, column 1) can also be used to test for a genuine effect beyond publication selection. The coefficient on precision, β_1, can be considered an estimate of empirical effect corrected for publication selection (Stanley 2005, 2008).[6] Applying this precision effect test (PET) to C-K's data finds no evidence of an employment effect from minimum wages (accept H_0: $\beta_1 = 0$; $t = 0.06$; $p > 0.05$). Thus, our FAT-PET-MRA, equation (2), entirely confirms Card and Krueger's (1995a) interpretation of minimum-wage research. There is clear evidence of publication selection bias in C-K's data; yet, there is *no* evidence of any minimum-wage effect on employment.

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

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

but should be interpreted differently (Stanley 2005, 2008).[7] If we can reject H_0: $\alpha_1 \leq 0$, then there is evidence of an empirical effect, regardless of publication selection. Column 2 of Table 1 again finds no evidence of a genuine adverse employment effect from minimum wages (accept H_0: $\alpha_1 \leq 0$, $t = -1.01$; $p > 0.05$).[8]

Card and Krueger (1995a: 239) erroneously use the absence of the relation between a study's reported t-value and its degrees of freedom, both expressed in logarithms, as a statistical indicator of publication bias. However, there is a second, equally valid, cause of an insignificant estimated α_1 in equation (3) above. Perhaps, there is simply no actual empirical effect, no adverse minimum-wage effect on employment. In the absence of an empirical effect,

t-values will not rise with their degrees of freedom, regardless of whether there is or there is no publication selection (Stanley 2005). Unfortunately, this oversight has been repeated several times by economists and industrial relations researchers.[9] The simple answer to C-K's rhetorical question: 'What might prevent the t ratio from rising with sample size?' (Card and Krueger 1995a: 239) is that the minimum wage has no employment effect.[10]

3. An extended meta-analysis of minimum wage's employment effect

Believing is seeing. Demsetz (1974: 164)

Many more minimum-wage studies have been reported since C-K's meta-analysis. Hence, it is important to include these newer studies. Meta-analysis starts with extensive literature searching (Stanley 2001; Stanley and Jarrell 1989). We searched ECONLIT and several other Internet databases for any occurrence of the terms 'minimum wage', 'employment', and 'teenage employment'. We followed up also on references cited in empirical studies and reviews of this literature. After much reading and retrieving, this process eventually yielded 1,474 empirical estimates of the minimum-wage elasticity of employment from 64 comparable studies using US data. Of these, 39 studies report estimates relating to US teenagers, while the rest report estimates either for a specific region (e.g. California or New Jersey), a specific industry (e.g. retail trade) or a specific sub-group (e.g. males and/or non-white employees).[11]

We excluded 31 studies from the meta-analysis because they were incompatible with the main group. Excluded are studies that either: (a) focused on unemployment and not employment; (b) did not offer sufficient information to be included in the MRA; or (c) used the conditional logit model and focused on the probability of employment rather than the elasticity of employment with respect to the minimum wage.[12]

The second step of any meta-analysis is to choose a common metric that best measures empirical effect in the area of research under analysis. Here, we select the elasticity of employment with respect to the minimum wage. In economics, elasticities are often assumed to be relatively stable, yet key, parameters. In this minimum-wage research literature, these elasticities are by far the most frequently reported measure of empirical effect. Thus, we only include those estimates that are elasticities or can be converted to elasticities. Furthermore, to be included in our MRA, the study must also report the associated t-value or the estimated elasticity's standard error.[13] Without one of these additional statistics, publication selection cannot be identified in or filtered from the research literature.

The uncorrected average elasticity is -0.190 ($p < 0.001$) or -0.054 ($p < 0.001$) when weighted by the inverse of each estimate's variance ($n = 1{,}474$).[14] The difference between these averages is largely due to publication selection bias. To see this publication selectivity, recall Figure 2.

As we mentioned above, the interpretation of graphical depictions are inherently subjective and are often ambiguous. This is why MRA is necessary. Table 2 columns 1–4 report the results of the FAT-PET-MRA, equation (2), for our extended database for 39 estimates of the effect of minimum wages on national US teenage employment. These are the 'best-set' of estimates, taking one estimate from each study. That is, we use the estimate preferred by the author(s). Where the author(s) does not reveal a preference, we take the average of those estimates deemed by the author to be valid and reliable. In their reviews of the time-series minimum-wage studies, Brown *et al.* (1982) and Card and Krueger (1995b) use the best-set of the then available studies. Using the 'best' estimates is common practice among economic researchers and literature reviewers. Thus, our meta-analysis would be vulnerable to obvious criticism if we were to ignore this way of identifying what constitutes the relevant research record. However, we also include all the reported employment elasticities in our meta-analysis (Table 3) to ensure that our findings are robust, regardless of how one might identify the research base on minimum-wage employment effects.

Column 1 of Table 2 reports the results of applying ordinary least squares (OLS) to MRA model (2) for this 'best-set' of employment elasticities, while column 2 employs a robust regression. Each of the 39 studies is a separate and distinct study; yet, some studies share the same author. This may potentially introduce a structure of dependence among employment elasticity estimates. That is, a researcher might have the tendency to use an idiosyncratic approach (to modelling or to selecting data) that is not easily identified by reading her research article and yet influences her findings in some systematic way. Two ways of dealing with this potential dependence are the use of clustered data analysis (column 3), and the random-effects multi-level model (REML, column 4). Both approaches allow for dependence within a given author's, or group of authors', reported elasticities. Within-study and/or within-author dependence has long been recognized as a potential estimation problem for meta-regression. Multi-level models (or equivalently, unbalanced panel models) can compensate for any observed within-study dependence (Bateman and Jones 2003; Rosenberger and Loomis 2000). The presence of some within-study dependence is, in this application, revealed by the Durbin–Watson statistic (0.94, when all 1,474 estimates are used). Note that the MRA results are quite robust to different methods and subsets of reported research results. Although there are differences in the MRA coefficients, like C-K's meta-analysis, we find strong evidence of publication selection (reject H_0: $\beta_0 = 0$; $t = (-5.42;\ -6.71;\ -3.95;\ -3.03)$; $p < 0.01$) but no evidence that the minimum-wage raises reduce employment (accept H_0: $\beta_1 = 0$; $t = (-0.89;$ $-0.21;\ -0.91;\ -1.03)$; $p > 0.05$) — columns 1–4 of Table 2.

Columns 5–8 of Table 2 repeat this same MRA using all the available studies, including studies that report region, industry and sub-group specific elasticities. Once again we have strong evidence of publication selection

TABLE 2
MRA Tests for Publication Selection Using our Expanded Research Data, Best-Set (Dependent Variable: t-Value)

Moderator variables	*Column 1: OLS, aggregate studies*	*Column 2: Robust, aggregate studies*	*Column 3: Clustered data analysis, aggregate studies*[b]	*Column 4: REML, aggregate studies*[b]	*Column 5: OLS, all studies*	*Column 6: Robust, all studies*	*Column 7: Clustered data analysis, all studies*[b]	*Column 8: REML, all studies*[b]
Intercept (β_0)[c]	**–2.98**	**–2.01**	**–2.98**	**–2.58**	**–2.81**	**–1.90**	**–2.81**	**–2.69**
	(–5.42)[a]	(–6.71)	(–3.95)	(–3.03)	(–5.64)	(–6.31)	(–4.07)	(–4.29)
$1/Se$[c]	–0.017	–0.002	–0.017	–0.024	–0.002	–0.005	–0.002	–0.009
	(–0.89)	(–0.21)	(–0.91)	(–1.03)	(–0.09)	(–0.49)	(–0.08)	(–0.49)
n	39	39	39	39	64	63	64	64
k	39	39	39	39	64	63	64	64
R^2	0.012	—	—	—	0.001	—	—	—
Employment effect of a 10% increase in the minimum wage	–0.17%	–0.02%	–0.17%	–0.24%	–0.02%	–0.05%	–0.02%	–0.09%

[a] All estimates relate to equation (2). t-values are reported in parentheses. Cells in bold denote statistical significance at least at the 5 per cent level.
[b] Author identifiers are used to cluster elasticities and for the REML estimation.
[c] If the literature is free of publication bias, the intercept should not be statistically significant. The coefficient on $1/Se$ measures the minimum-wage employment effect, corrected for publication selection effects.
Notes: n denotes the number of elasticity estimates and k denotes the number of independent studies. Column 3 uses clustered data analysis to account for within-study dependence. REML denotes the random-effects multi-level model. Columns 1 to 4 use only elasticities relating to teenagers for the USA as a whole. Columns 5 to 8 include regional, sub-industry, and sub-group specific elasticities.
OLS, ordinary least squares.

TABLE 3
MRA Tests for Publication Selection Using our Expanded Research Data, All-Set
(Dependent Variable: t-Value)

Moderator variables	*Column 1: MRA model*	*Column 2: Robust*	*Column 3: Clustered data analysis*[b]	*Column 4: REML*[b]	*Column 5: Omitting all positive elasticities*
Intercept (β_0)[c]	**–1.60**	**–1.31**	**–1.60**	**–1.71**	**–2.63**
	(–17.36)[a]	(–16.94)	(–4.49)	(–5.62)	(–27.80)
$1/Se$[c]	**–0.009**	–0.004	–0.009	**–0.010**	–0.006
	(–3.15)	(–1.91)	(–1.09)	(–4.17)	(–1.88)
n	1,474	1,474	1,474	1,474	1,125
k	64	64	64	64	61
R^2	0.009	—	—	—	0.004
Employment effect of a 10% increase in the minimum wage	**–0.09%**	–0.04%	–0.09%	**–0.10%**	–0.06%

[a] All estimates relate to equation (2). t-values are reported in parentheses. Cells in bold denote statistical significance at least at the 5 per cent level.
[b] Study identifiers are used to cluster elasticities and for the REML estimation.
[c] If the literature is free of publication bias, the intercept should not be statistically significant. The coefficient on $1/Se$ measures the minimum-wage employment effect, corrected for publication selection effects.
Notes: n denotes the number of elasticity estimates and k denotes the number of independent studies. Column 3 uses clustered data analysis to account for within-study dependence. REML denotes the random-effects multi-level model. Columns 1 to 4 use all reported elasticities. Column 5 uses only negative minimum-wage elasticities.
MRA, meta-regression analysis.

(reject H_0: $\beta_0 = 0$; ($t = (-5.64; -6.31; -4.07; -4.29)$; $p < 0.01$)). And again, after this publication selection is filtered, no evidence of a minimum-wage effect remains (accept H_0: $\beta_1 = 0$; ($t = (-0.09; -0.49; -0.08; -0.49)$; $p > 0.05$)). Note further that the magnitude of the estimated intercept, $\hat{\beta}_0$, in MRA model (2) represents rather 'severe' publication selection. Simulations show a strong correlation (0.977) between the magnitude of $\hat{\beta}_0$ and the publication selection bias (Doucouliagos and Stanley 2008).

Because Table 2 uses only the 'best-set' of minimum-wage elasticities, we also report the FAT-PET-MRA for the 'all-set' of 1,474 estimated elasticities to ensure that these results are robust to the definition of the research record (Table 3). Columns 1 to 4 of Table 3 report the OLS, robust, clustered data and REML estimates, respectively. As we now include more than one estimate from each study, any potential dependence among estimates is best captured by using study identifiers. Alternatively, using author identifiers does not alter the results in any significant fashion. The research record, however defined, provides robust evidence of publication selection in the minimum-wage literature.

The one difference between the best-set and the all-set MRA results is that the latter suggests the existence of a very small, but statistically significant, negative minimum-wage effect. A 10 per cent increase in the minimum wage reduces employment by about 0.10 per cent (see column 4 of Table 3). But even if this adverse employment effect were true, it would be of no practical relevance. An elasticity of –0.01 has no meaningful policy implications. If correct, the minimum wage could be doubled and cause only a 1 per cent decrease in teenage employment.

We have further reason to question this small negative minimum-wage effect. First, the precision-effect test can be biased in favour of rejecting H_0: $\beta_1 = 0$ when there is a large amount of unexplained heterogeneity and a very high incidence of publication selection (i.e. type I error inflation) (Stanley 2008).[15] Second, when there is significant systematic heterogeneity among the underlying employment elasticities (i.e. if elasticity changes over time or varies by the type of measures or data used), this systematic variation needs to be included explicitly in a multivariate MRA. Otherwise, the simple MRA reported in Tables 2 and 3 can suffer from omitted variable bias. This systematic variation is explored below and reported in Table 5. As revealed in the next section, there are strong statistical reasons to believe that the employment elasticity of minimum wage is indeed heterogeneous.

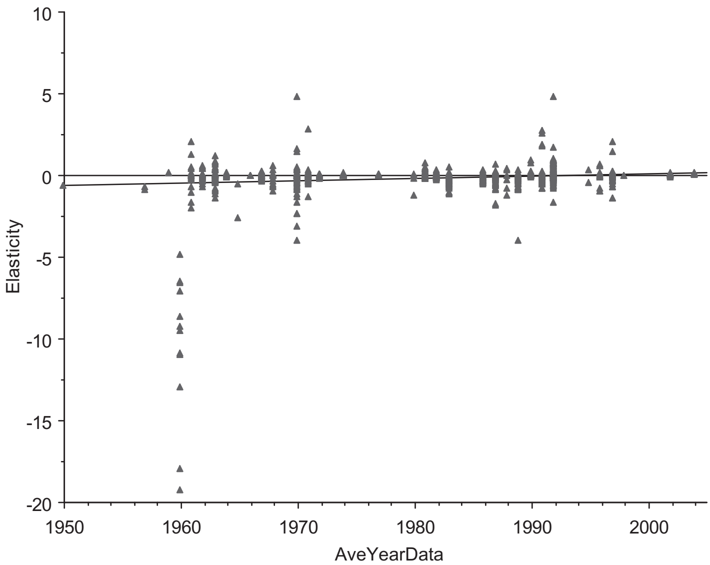
For robustness sake, it might be interesting to investigate whether the above findings of no adverse employment effect would persist if the skewed research record were subjected to even further sample selection. That is, if one were to insist that all estimated positive elasticities, which are inconsistent with neoclassical theory, must be in error and therefore removed, then surely we must find evidence of minimum wage's negative employment effect. Column 5 of Table 3 reports the FAT-PET-MRA when all positive elasticities are omitted from the sample, leaving 1,125 negative ones.[16] Unsurprisingly, the strong signal of publication selection for negative elasticities becomes stronger still ($t = -27.80$; $p < 0.001$). More remarkable is that there is little statistical evidence of any adverse employment effect, even if one were to insist that elasticities must be zero or negative to be considered 'admissible.'

Our simple FAT-PET-MRA results are consistent with C-K's findings. Even after adding 49 studies and more than 1,000 estimated elasticities, Card and Krueger's (1995a) interpretation of the minimum-wage literature still stands.

4. 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 minimum-wage effect on employment over time.[17] There is a significant linear trend, which suggests that elasticity estimates are getting 0.14 larger (or less negative) every decade ($t = 5.92$; $p < 0.0001$).[18] Because most research in this area uses the Kaitz index that explicitly accounts for the effective magnitude of the minimum wage, this decline of adverse minimum-wage effect is not the result of a falling real minimum wage. However, this decline could be due to an actual lessening of minimum wage's impact over time — that is, 'structural change'. To explore this and broader questions concerning what

FIGURE 4
Time Trend of Employment Elasticities for Minimum-Wage Raises (n = 1,492).



does explain the variation in reported estimates of the minimum-wage effect, we turn to multivariate MRA.

Like any regression model, the estimates of MRA's coefficients can become biased when important explanatory variables are omitted. MRA model (2) can be expanded to include moderator variables, Z_k, that explain variation in elasticities and other factors, K_j, that are correlated with the publication selection process itself.

$$t_i = \beta_0 + \Sigma\gamma_j K_{ij} + \beta_1(1/Se_i) + \Sigma\alpha_k Z_{ik}/Se_i + v_i \quad (4)$$

Table 4 lists the potential *Z-K* variables that we code and investigate. This list of MRA control variables is driven purely by the type of data at hand and by debates in the literature. Since the all-set includes estimates for specific industries, we include controls (*Agriculture*, *Retail* and *Food*) for estimates relating to agriculture, retail and food (mainly restaurants). Sub-group estimates typically relate to differences between males and females and/or whites and non-whites. Hence, we add controls for these, *Male* and *Non-White*. Some of the estimates relate to a specific region and the variable *Region* is included to control for any differences between region-specific and US-wide elasticities. We also include estimates relating to young adults (*Adults*) and compare these to teenagers. Most estimates relate to the contemporaneous employment effects of the minimum wage. Nonetheless, many estimate the lagged effect of minimum wage rises. The coefficient on *Lag* is used to investigate the extent of any such difference.

TABLE 4
Potential Explanatory Variables for Meta-Regression Analysis

K & Z variable	*Definition*	*Mean (standard deviation)*
t-statistic	the dependent variable in the FAT-PET regressions	–1.69 (2.83)
1/*Se*	is the elasticity's precision; it is used to test for a genuine effect, PET	22.76 (28.32)
Panel	= 1, if estimate relates to panel data with time series as the base	0.45 (0.50)
Cross	= 1, if estimate relates to cross-sectional data with time series as the base	0.13 (0.34)
Adults	= 1, if estimate relates to young adults (20–24) rather than teenagers (16–19)	0.14 (0.35)
Male	= 1, if estimate relates to male employees	0.07 (0.26)
Non-white	= 1, if estimate relates to non-white employees	0.05 (0.22)
Region	= 1, if estimate relates to region-specific data	0.10 (0.30)
Lag	= 1, if estimate relates to a lagged minimum-wage effect	0.13 (0.34)
Hours	= 1, if the dependent variable is hours worked	0.07 (0.25)
Double	= 1, if estimate comes from a double log specification	0.42 (0.49)
AveYear	is the average year of the data used, with 2000 as the base year	–19.17 (11.90)
Agriculture	= 1, if estimates are for the agriculture industry	0.01 (0.11)
Retail	= 1, if estimates are for the retail industry	0.08 (0.27)
Food	= 1, if estimates are for the food industry	0.13 (0.34)
Time	= 1, if time trend is included	0.37 (0.48)
Yeareffect	= 1, if year-specific fixed effects are used	0.30 (0.46)
Regioneffect	= 1, if region/State fixed effects are used	0.34 (0.47)
Un	= 1, if a model includes unemployment	0.56 (0.50)
School	= 1, if model includes a shooling variable	0.15 (0.35)
Kaitz	= 1, if the Kaitz measure of the minimum wage is used	0.40 (0.49)
Dummy	= 1, if a dummy variable measure of the minimum wage is used	0.17 (0.38)
Published	= 1, if the estimate comes from a published study	0.85 (0.35)

Notes: *K* variables may affect the likelihood of being selected for publication. *Z* variables may affect the magnitude of the minimum-wage elasticity. All variables are included as *Z* and *K* variables in a general-to-specific modelling approach.
FAT-PET, funnel asymmetry-precision-effect testing.

There is some debate in the literature about the need to control for cyclical effects (*Un*) and school enrolment (*School*). We include the variables *Un* and *School* to account for the effects of including these variables in the researchers' demand for labour equation. The vast majority of estimates relate to employment, but some relate to hours worked. *Hours* captures this difference in the measurement of the dependent employment variable. The effects of including a time trend in the specification are explored through the *Time* variable. A large group of estimates (696) come from studies that use panel data, while a smaller group use cross-sectional data (210). *Panel* and *Cross* are included to control for differences in the type of data, with time-series studies as the base. Two related variables are *Yeareffect* and *Regioneffect*, which control for the inclusion of period and cross-section (region/State) fixed effects, respectively.

The majority of estimates have been reported in published academic journals. There are, however, estimates of minimum-wage elasticities that come from working papers (e.g. NBER working papers) and have yet to be published in an academic journal. Many of these working papers have been cited in the literature and, hence, need to be included in the meta-analysis. Two final controls relate to differences in the measurement of the minimum wage — the use of the Kaitz index (*Kaitz*) and the use of dummy variables (*Dummy*).

All variables are listed under both categories because they might potentially affect the expected elasticity of employment with respect to minimum wage (the *Z*-vector) and also the propensity of a study being selected for publication (the *K*-vector). The advantage of our MRA approach is that publication selection (*K*-variables), structural change (*AveYear*) and any other potential influence upon estimated minimum-wage effect (*Z*-variables) can be explicitly modelled, equation (4), and each influence may be separately accounted for, identified and estimated. Estimation of model (4) is further useful in identifying the source of the heterogeneity among the reported elasticities, but here we focus on publication selection — its identification and correction.

Table 5 presents the MRA results of general-to-specific modelling (Charemza and Deadman 1997), applied to the all-set of 1,474 elasticity

TABLE 5
Multivariate, General-to-Specific, MRA Model Using Our All-Set (Dependent Variable: *t*-Value)

Moderator variables	*Column 1: Clustered data analysis*	*Column 2: REML*
Genuine empirical effects (Z-variables)		
1/Se	0.120 (4.39)[a]	0.107 (7.00)
Panel/Se	–0.182 (–4.72)	–0.155 (–12.31)
Double/Se	0.064 (3.20)	0.044 (5.96)
Region/Se	0.040 (0.92)	0.087 (6.37)
Adult/Se	0.024 (2.68)	0.021 (3.76)
Lag/Se	0.026 (1.60)	0.012 (2.05)
AveYear/Se	0.004 (4.34)	0.003 (7.40)
Un/Se	–0.042 (–3.04)	–0.041 (–6.14)
Kaitz/Se	0.052 (3.06)	0.033 (4.48)
Yeareffect/Se	0.069 (1.98)	0.068 (7.83)
Published/Se	–0.041 (–2.69)	–0.039 (–5.60)
Time/Se	–0.022 (–2.08)	–0.020 (–3.07)
Publication bias (K-variables)		
Intercept (β_0)	–0.359 (–0.11)	–1.222 (–3.82)
Double	–1.482 (–3.23)	–1.091 (–4.31)
Un	–0.840 (–1.87)	0.852 (2.64)
n	1,474	1,474
k	64	64

[a] *t*-values are reported in parentheses and are calculated from heteroscedasticity-consistent standard errors.
Notes: *n* denotes the number of elasticity estimates and *k* denotes the number of independent studies. REML denotes the random-effects multi-level model. Columns 1 and 2 use study identifiers.
MRA, meta-regression analysis.

estimates. That is, all *Z*- and *K*-vector variables listed in Table 4 were included in a general meta-regression model estimated using OLS, and then the statistically insignificant ones were removed, one at a time, to derive a specific model.[19] Column 1 reports the MRA results using clustered data analysis which is one way to account for dependence within the same study. An alternative approach to modelling this intra-study dependence is reported in column 2 — REML.

There is, of course, some variation in the estimated MRA coefficients across the estimation approaches, but there is only one sign reversal and most individual effects are robust.[20] Note that in this framework, both genuine effect and publication bias are more complicated. Genuine effects (and/or large-sample biases) are now captured by the combination of all the *Z*-variables (i.e. those divided by *Se*), while the *K*-variables (i.e. those *not* divided by *Se*), along with the intercept, together represent publication selection. With such a rich dataset, many research dimensions can be identified as statistically significant, whether or not they are practically important or even meaningful. For our purposes, the details of the multiple MRA that are represented by Table 5 are not important. What truly matters is whether our central findings about the existence of publication selection and the absence of a genuine minimum-wage effect on employment remains after any other reasonable research factors, including structural change, is accounted for.

Publication Selection

Clear evidence of publication selection remains in this multivariate MRA ($F_{(3,\ 1459)} = 84.2$; $p < 0.0001$).[21] The intercept, by itself, is no longer a measure of the magnitude of the average publication bias. Rather, it is the combination of the intercept and all the *K-variables* (*Un* and *Double*). Estimated MRA coefficients from these variables can be used to calculate the average estimated publication bias for the minimum-wage literature as –0.231 — compared with a –0.273 for the simple MRA, column 1 of Table 2.[22] Subtracting the estimated publication bias from the reported minimum-wage elasticities converts the average minimum-wage elasticity (–0.190) to a *positive* value (+0.041). Nonetheless, this small positive value is so small that it is of little practical import. The only robust factor associated with publication selection is whether a double logarithmic model is used to estimate the minimum-wage elasticity. Apparently, selection for a negative elasticity is associated with choosing the double-log form of the employment equation.

Effects on Minimum-Wage Elasticities

Now, we turn to identifying variations in the actual responsiveness of employment to minimum-wage rises, irrespective of publication bias. Rather than some single overall effect, minimum-wage effects on employment are the combination of several factors (*1/Se, Panel/Se, Double/Se, Region/Se, Adults/Se, Lag/Se, AveYear/Se, Un/Se, Kaitz/Se, Yeareffect/Se, Published*/Se and *Time/Se*) ($F_{(11,\ 1459)} = 50.8$; $p < 0.0001$). When all of these Z-variables are zero (this implies, among other things, that time-series data are used and that the average year of data used in the study was 2000), the minimum-wage effect is predicted to have a contemporaneous *positive* effect (0.120; 0.107) on employment ($t = 4.39$; 7.00; $p < 0.0001$). Allowing for the lagged effect of minimum wages slightly increases this positive employment effect (by 0.01–0.026). This positive minimum-wage elasticity is due largely to structural change, which estimates elasticity to increase by 0.034 each decade (using the REML MRA; $t = 7.40$; $p < 0.0001$). By 2008, our estimated MRA model (4) predicts a positive minimum-wage elasticity for teenage employment of +0.146, using coefficients from column 2 for *1/Se*, *Lag/S* and *AveYear/Se*, and thereby correcting for publication selection bias.

Against such an economically meaningful, positive effect of minimum-wage increases are considerations of what might constitute the 'best practice' in labour research. In particular, a case could be made that the use of panel data (including year fixed effects) and the Kaitz index represent 'best practice'. When doing so, our MRA model predicts a reduction of the above-positive employment effect to +0.065 (or +0.092 for 2008). What constitutes 'best practice' is, however, controversial. For example, Burkhauser *et al.* (2000) argue against the use of including fixed year effects in minimum-wage studies. If these are removed from the best practice calculation, the MRA model predicts a trivial negative employment elasticity of −0.003 for 2000 (or +0.024 for 2008). On the other hand, Card and Krueger (1995b) argue for the inclusion of fixed effects but against the use of the Kaitz index. Doing so, our MRA model predicts a positive employment effect of +0.032 for 2000 (or +0.059 for 2008). Regardless, no practically significant, adverse employment effect remains for the US labour market in the twenty-first century, after correcting for publication selection bias.[23]

Neoclassical theory predicts a negative employment response in the long run. Indeed, a zero contemporaneous employment effect is possible, depending on the fixity of inputs. The statistical significance of *Lag/Se* suggests that employers do not fully anticipate minimum-wage increases and, hence, do not fully adjust input levels in advance of regulatory changes. Moreover, *Lag/Se* has a positive coefficient in the MRA, suggesting that the long-run elasticity is actually less negative (more positive), in contrast to the neoclassical prediction. In any case, the size of the lagged effect is very small.

A more complex MRA of minimum-wage research reveals a heterogeneous effect on employment, which gets less negative (or more positive) over time and depends on several research choices. Overall, no adverse US-wide minimum-wage effect remains after publication selection is filtered from the reported estimates.

Discussion

How can it be that there is no adverse employment effect from raising the minimum wage? Every economics and business student has been taught for many decades that minimum-wage hikes cause unemployment. This and rent control are the quintessential textbook illustrations of the allocative inefficiency that results through market interference or from government regulation. Card and Krueger (1995a), among others, have suggested that significant monopsonistic power in the labour market would explain this observed inelasticity (or even the positive wage elasticity) of labour demand. Alternatively, efficiency-wage theory can provide a plausible explanation of the absence of any adverse employment effect (Akerlof 1982, 2002). Higher wages are observed to lead to higher productivity, which, theoretically, could compensate for the higher labour costs.

An entirely separate meta-analysis of the efficiency-wage literature finds the clear trace of an authentic efficiency-wage effect after correcting for publication selection (Stanley and Doucouliagos 2007; Krassoi-Peach and Stanley 2009). Like the current study, the efficiency-wage MRA results are very robust. The presence of an economically meaningful efficiency-wage effect is corroborated by both simple and multiple meta-regressions and by using more sophisticated estimation techniques that are not vulnerable to the dependence across estimates (REML) or to extreme estimates (robust regression).

5. Conclusion

This article re-evaluates the empirical evidence of a minimum-wage effect on employment. Several meta-regression tests corroborate C-K's overall finding of an insignificant employment effect (both practically and statistically) from minimum-wage raises. Recently developed tests for publication selection bias confirm its presence in this area of labour research. The research on minimum-wage effects contains the clear trace of selection for adverse employment effects.

No evidence of a genuine adverse employment effect can be found among time-series estimates of minimum-wage elasticities used by C-K, but they contain a clear indication of publication selection. Recall that quasi-experimental evidence corroborates minimum wage's insignificant employment effect (Card and Krueger 1995b). Our analysis confirms that there never was much accumulated empirical evidence of a negative employment effect from minimum-wage regulation (Leonard 2000).

Our meta-analysis of 1,474 estimated minimum-wage elasticities only confirms this view and Card and Krueger's (1995a) results. We still find strong evidence of publication selection for significantly negative employment elasiticites, but no evidence of a meaningful adverse employment effect when selection effects are filtered from the research record. Even after accounting for structural change in this area of research, very strong evidence of publication selection for negative employment elasticities remains.

In the minimum-wage literature, the magnitude of the publication selection bias is as large or larger, on average, than the underlying reported estimate. Overall, correcting for publication bias would transform a modestly negative average elasticity to a small *positive* employment elasticity. However, our MRA identifies several factors, including structural change, that affect the magnitude of the minimum-wage elasticity. Thus, no single estimate can adequately summarize the minimum-wage effect on employment. Rather, estimated employment effects are dependent upon research choices and time. Even under generous assumptions about what might constitute 'best practice' in this area of research, little or no evidence of an adverse employment effect remains in the empirical research record, once the effects of publication selection are removed.

Two scenarios are consistent with this empirical research record. First, minimum wages may simply have no effect on employment. If this interpretation were true, it implies that the conventional neoclassical labour model is not an adequate characterization of the US labour markets (especially the market for teenagers). It also implies that other labour market theories, such as those involving oligopolistic or monopsonistic competition, or efficiency wages or heterodox models, are more appropriate (see Lester 1946 and Card and Krueger 1995b).

Second, minimum-wage effects might exist, but they may be too difficult to detect and/or are very small. Perhaps researchers are 'looking for a needle in a haystack' (Kennan 1995: 1955). In any case, with 64 studies containing approximately 1,500 estimates, we have reason to believe that if there is some adverse employment effect from minimum-wage raises, it must be of a small and policy-irrelevant magnitude.

One objection to the first inference is that the standard competitive labour market model does not predict that employment will fall for *all* sub-sectors. Employment need not fall for all establishments, and it might rise for some establishments. The model does, however, predict some adverse effect on employment across the board, on average, and the research record is not consistent with the prediction of an adverse effect at any level. Moreover, researchers do use the extant evidence to make inferences. For example, Neumark and Wascher (2007) provide a conventional descriptive review of most of the studies included in our meta-analysis. In their review of this same literature, Neumark and Wascher (2007: 123) see the evidence as: 'largely solidifying the conventional view that minimum wages reduce employment among low-skilled workers, and as suggesting that the low-wage labor market can be reasonably approximated by the neoclassical competitive model.' However, the contrast between their subjective narrative review and meta-analysis is quite striking.

In updating and extending C-K's meta-analysis, we offer alternative meta-regression methods that are validated through Monte Carlo simulations and by extensive applications in other fields of economic research (Doucouliagos

2005; Gemmill *et al.* 2007; Roberts and Stanley 2005; Stanley 2005, 2008; Stanley and Doucouliagos 2007). FAT and PET offer great promise for the empirical study of economic research. Our meta-analysis of minimum-wage regulation and the associated MRA models are robust to variations both in the meta-data used and to variation in the econometric approaches employed. Thus, it seems safe to conclude that minimum-wage research exhibits much publication selection, regardless of which additional factors one considers.

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Notes

1. Our focus in this article is exclusively on the employment effects of minimum wages. A sizeable literature exists exploring other dimensions of minimum wages, such as their effect on unemployment, labour force participation, enrolment rates, prices and profitability. Analysis of these is beyond the scope of this article.
2. First, we include all reported estimates in the 15 time-series studies that Card and Krueger (1995a) sampled. This alone produced an additional 260 estimates, because C-K *selected* only one estimate per study. Second, we update and extend their meta-analysis to include any estimate of the minimum-wage elasticity, whether produced using time-series, cross-sectional or other types of data. Doing so identifies an additional 49 studies. See the next section for greater details about how we arrived at these 1,474 estimates of the minimum-wage elasticity. A few elastic estimates, whether positive or negative, are trimmed from the funnel graph to permit the pattern in the remaining 1,424 to be seen. However, all estimates are included in our below meta-regression analyses.
3. See Stanley (2005, 2008) 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 ($\beta_1 = 0$). Should $\beta_1 \neq 0$, the second term of equation (1) will not be linear (Stanley and Doucouliagos 2007).
4. To understand the relation of equation (2) with the funnel graph, first invert the funnel by plotting *Se* versus *effect*. Next, rotate the funnel 90 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).
5. When coding the standard errors for minimum-wage effects, the standard error of one study could not be calculated (Ragan 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 $1/Se_i$.

6. 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 2008). The validity of this test needs to be qualified. If there is large unexplained heterogeneity and a high incidence of publication selection, it can suffer from type I error inflation. Simulations show that the failure to reject H_0: $\sigma_v^2 \leq 2$ serves as an effective means to limit these potential type I errors (Stanley 2008), where σ_v^2 is the error variance.
7. However, simulations show that MST often has inflated type I errors and is thereby not as reliable as FAT-PET (Stanley 2008).
8. MST's weakness is that it finds an empirical effect that is not there too often (i.e. type I error inflation). This weakness cannot explain our current findings, because here we find no minimum-wage effect.
9. 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: 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, Mookerjee (2006) uses these same methods in his meta-analysis of the export growth hypothesis.
10. Perhaps given the history of minimum wage research in economics, Card and Krueger were hoping that the reader would answer their rhetorical question in precisely this way. If there is no relationship between the reported *t*-values and sample size, then either there is no minimum wage effect or there is publication bias. Either way, their critique of orthodox minimum wage theory is valid.
11. The full reference list can be found at http://www.deakin.edu.au/meta-analysis.
12. The list of the excluded studies and the reasons for exclusion is available from http://www.deakin.edu.au/meta-analysis.
13. This causes the loss of 142 observations, resulting in the 1,474 remaining elasticities with their standard errors.
14. Weighting by the inverse of the variance is standard practice among meta-analysts — the so-called 'fixed-effects' estimate (Sutton *et al.* 2000b).
15. Because we have evidence of a large amount of unexplained heterogeneity (reject H_0: $\sigma_v^2 \leq 2$ at any significance level; $\chi^2_{(1472)} = 10{,}441{,}368$) and a clear indication of substantial or severe publication selection, we cannot rule out a type I error as a likely cause of this significant PET result.
16. We, in no way, sanction any such selection of reported results. Quite the contrary, we consider all reported research as sacrosanct. However, to anticipant a possible neoclassical criticism and to test the limits of our methods, we throw out all positive elasticities only as a hypothetical exercise.
17. Note that all reported elasticities are shown in Figure 4, whether or not they have an associated standard error. No doubt, many readers will be struck by the obvious erroneous values of some of these reported minimum-wage elasticities. Most would agree that minimum-wage elasticities greater than 5, plus or minus, are not plausible. Also, some might argue that we should remove these obvious outliers from our meta-analysis entirely. Throwing out these potential outliers is unnecessary. One of the beauties of FAT-PET-MRA methods is that imprecise

estimates, which all of these potential outliers are, have little or no effect on the MRA results, aside from helping to identify the presence of publication selection. In any case, we report a robust regression that searches for and removes potential outliers.

18. When trend is estimated along with other factors that affect the reported employment elasticity and its publication selection, the magnitude of this positive trend is greatly reduced but remains statistically significant (see Table 5). It is a robust feature of this research literature, even after we control for differences in research methods and approaches over time.
19. OLS was used in the general-to-specific modelling strategy. The specific model was then re-estimated using clustered data analysis and REML. These results are reported in Table 5.
20. There is evidence of a high degree of multicollinearity in the MRA model reported in Table 5. Several variables have variance-inflation factors exceeding 10. In complex FAT-PET-MRAs, where many variables are being divided by the estimates' standard errors, multicollinearity is nearly inevitable. However, because we are only investigating this research area's overall pattern of publication selection and underlying employment effect, our meta-analysis will likely be robust to any unreliability in estimating the individual effect of any moderator variable.
21. This is calculated from the cluster-robust MRA reported in column 1 of Table 5. However, a likelihood ratio test based on the REML MRA gives the same general assessment.
22. The average publication bias in terms of the minimum-wage elasticity is calculated from the average estimated value of $(\beta_0 + \Sigma\gamma_j K_j)Se$, or $\beta_0 Se$ for the simple MRA, using the coefficients estimated by REML.
23. Only by insisting that 'best practice' also includes only those elasticities from *published* articles that use *time* trends and the *unemployment* rate in the employment equation will the corrected elasticity become negative. But even here, the adverse employment effect is again too small (–0.009) to be of any economic relevance. Besides, there are major problems with this notion of 'best practice'. Although one could reasonably argue that published papers contain better estimates, the observed negative effect of *Published/Se* might also be another reflection of publication selection bias. Moreover, Neumark and Wascher (1998, n. 10) show how the 'benchmark' specification used by Solon (1985) and many others and which contains a quadratic time trend has residuals that are an $I(1)$ process, implying that this benchmark employment equation is a spurious regression.

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