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Abstract

Publication selection bias represents one of the most serious challenges to the integrity of empirical economics. We develop Heckman regression methods to solve this potentially persistent problem and apply these meta-regression methods to seventy five empirical estimates from the efficiency-wage literature. Although many researchers find mixed or ambiguous support for the efficiency wage hypothesis (EWH), our meta-analyses give unambiguous confirmation of the EWH. After correcting for publication selection bias, we estimate the wage elasticity of output to be 0.32, much smaller than what the neoclassical version of the efficiency wage hypothesis demands. This wage elasticity also depends significantly upon whether the researchers' model accounts for the simultaneity of wages and productivity and whether their empirical model includes capital. In both cases, the 'correct' specification increases the magnitude of the wage elasticity of production, thereby further corroborating the EWH.

JEL Classification Codes: J3, C2, E24.

I. Introduction

> "(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).

Publication selection bias is widely acknowledged as another fundamental threat to valid empirical economic inference (DeLong and Lang, 1992; Card and Krueger, 1995; Ashenfelter *et al*., 1999; Roberts and Stanley, 2005). Like debasing the coin of the realm, publication selection bias can greatly inflate the estimated magnitude of key empirical economic parameters. Such inflation can be as high as 100, 200 or many hundreds of percent (Stanley, 2005). Hence, it becomes imperative to investigate the severity of publication selection and to correct empirical findings for the distorting effects of selectivity.

Efficiency wage theory has long captured the attention as well as the imagination of researchers and policy analysts. No fewer than two Nobel prizes were awarded, at least in part, for work on the efficiency wage hypothesis (EWH) (Akerlof, 2002; Stiglitz, 2002). For decades and in hundreds of papers, the EWH has been employed to rationalize involuntary unemployment (Solow, 1979; Akerlof, 2002), to explain the seemingly inexplicably wide variation of wages across industries and firms (Krueger and Summers, 1988), and to question the conventional neoclassical characterization of employers (Akerlof, 1982; Chen and Edin, 2002). The efficiency wage hypothesis argues that firms pay higher than market-clearing wages in order to increase productivity. But does the empirical research record confirm or deny this labor market complication? And, is the empirical evidence consistent with the neoclassical formulation of the EWH?

Although empirical research generally supports the EWH, it also contains ambiguous findings with a large variation among the reported elasticities. Furthermore, the neoclassical formulation of the EWH contains very specific implications about the magnitude of the wage elasticities of effort and production (often referred to as the elasticity of output or effort with respect to wages). In particular, the Solow condition requires that effort elasticity be unity (Akerlof and Yellen, 1986, p.14), and the wage elasticity of production must equal labor's share (see section 2, below). Does the research record confirm these specific magnitudes for wage elasticities? We conduct the first meta-regression analysis of the efficiency-wage literature and find confirmation of the existence of an efficiency-wage effect as well as publication selection bias. Once publication bias is filtered from the estimates of the wage elasticity of production, the evidence clearly rejects the neoclassical version of the efficiency wage hypothesis.

The paper is set out as follows. Section 2 provides a brief discussion of efficiency-wage theory. The techniques for detecting publication selection and correcting estimates for the distorting effects of publication selection are developed in section 3, while the meta-analysis of the EWH is reported and discussed in section 4. Section 5 concludes the paper.

II. Theoretical Considerations

The most referenced and empirically tested version of the efficiency wage hypothesis is the 'shirking model' (Shapiro and Stigliz, 1984). In competitive markets, firms offer workers with similar skills the market wage rate. There is no cost to 'shirking' on the job because another job can easily be found with a competing employer at the same market rate. Although employers can monitor their employees, monitoring is costly and sometimes infeasible. Thus, it might be advantageous to pay their workers an 'efficiency wage'; that is, one that is higher than the market-clearing wage rate and reduces shirking. Such an efficiency wage implies a connection between the effort (and therefore productivity) of a given worker and her wage. An efficiency wage premium buys loyalty, hard work, and lessens turnover with all its associated costs. In this way, effort and productivity become dependent on the level of wages.

There are several other justifications of efficiency wages, including the 'adverse selection' model and the 'gift-exchange' model (Stiglitz, 1976; Akerlof, 1982). However, regardless of which model is used, the wage-productivity channel has been central to the efficiency wage hypothesis, and it is the dimension of the EWH most frequently estimated and tested by empirical research. Thus, the wage-productivity connection is the focus of our meta-regression analysis.

Conventional neoclassical profit maximization requires that the wage elasticity of production (or the output elasticity with respect to wages) be equal to labor's share. This is essentially equivalent to the Solow condition that the wage-effort elasticity is unity, but in different units of measurement. To see this, recall the conventional formulation of production and profit functions. When efficiency wages are modeled, the textbook production function includes labor enhanced with an explicit channel from wages to effort, *e(w)*.

$$Q = f(e(w)L,K) \tag{1}$$

where Q is, of course, some measure of production or output and L and K denote labor and capital, respectively. Otherwise, production behaves in the neo-classical manner, and profit (π) takes on the textbook form.

$$\pi = PQ - wL - rK \tag{2}$$

For simplicity and generality across varying market structures, let *Y* be production in monetary units *(i.e., Y=PQ)*. First order conditions give:

$$\partial Y / \partial w = L \tag{3}$$

If we multiply by *w* and divide by *Y*, then we find that profit maximization forces the wage elasticity of production to equal labor's share, $\mathcal{L}$.

$$\partial Y/Y \mid \partial w/w = wL / Y = \mathcal{L} \tag{4}$$

This condition is quite intuitive. When wages increase by *x* percent, total costs will go up by $x\mathcal{Z}$ percent, at the margin; thus, production in monetary units must increase by $x\mathcal{Z}$ percent to 'break even' at equilibrium. In practice, this wage elasticity of production, the left side of equation (4), is the most frequently reported empirical magnitude in the efficiency-wage literature and therefore the most appropriate empirical magnitude for our focus. Next, we consider how to best model the wide variation in reported wage elasticities.

III. Identifying and Correcting Publication Bias

A. Identifying publication selection

Research is generated through a knowledge production process. Researchers transform their labor and knowledge (as well as the labor of others, data, expertise, economic theory, information technology and past research) into research output. In empirical economics research, the research output is typically estimates of the direction of an effect (whether it is positive, neutral or negative), and the magnitude of key elasticities, or some other standardized measure of association, such as elasticities and partial correlations. Denote the 'true' wage elasticity as η and the estimated elasticity reported in the i[th] study as $\hat{\eta}_i$.[1] In general,

$$\hat{\eta}_i = \eta + \omega_i \tag{5}$$

where ω_i is an error term associated with the complex research production process. This error term is more complicated than the usual random sampling error because it is likely to contain various misspecification and selection biases that may be systematically related to observable research characteristics. When this complexity is taken into account, equation (1) may be modeled broadly as:

$$\hat{\eta} = \beta_0 + \alpha_0 SE + Z\beta + \varepsilon \tag{6}$$

[1] This assumes that there is a single population parameter. It is a simple matter to introduce a distribution of population parameters—see equation (6) below.

where ε is a random error vector, i.i.d. *Z* is a vector of moderator variables that acknowledge key dimensions in the variation of the 'true' elasticity (heterogeneity) or identify large-sample biases that arise from model misspecification. *SE* contains the reported standard errors of the estimated elasticities. Lastly, β_0 denotes the 'true' elasticity, which can be estimated by meta-regression analysis (MRA).

However, the most widely used estimate of the 'true' elasticity is a simple weighted mean:

$$\hat{\eta}_w = \sum N_i \hat{\eta}_i / \sum N_i \tag{7}$$

where N_i is sample size associated with the i[th] study.[2] This weighted elasticity will be an unbiased estimate of the population elasticity, as long as the studies included in the calculation of equation (7) are a random sample from the population of all estimates, whether reported or not (see Hunter and Schmidt 2004). In the presence of publication selection effects, the pool of available studies from which inferences can be drawn will be truncated. When only a selected subset of estimates is used to calculate $\hat{\eta}_w$, then the literature is said to be contaminated by publication selection bias. Publication bias means that a non-random sample of the estimates remain unpublished and unreported; hence, the pool of research available is a biased representation of the population of estimates.

Publication bias can arise from both sides of the 'market' for research. On the supply side, it is possible that in the course of their empirical analysis, authors may find results that are opposite to their expectations (*e.g.*, negative wage elasticities of production) and decide to file these results away, leaving them unpublished and unknown to other researchers.[3] Authors may modify their methodology, data, model specification, and estimation procedure until the expected "correct results" emerge (*e.g.*,

[2] Alternative weights such as the inverse of the estimate's variance are also used, but the general results are the same.

[3] This idea of filing away unwelcome findings is called the 'file drawer problem' and is widely discussed in the meta-analysis literature (Rosenthal, 1979).

positive wage elasticities of production).[4] Alternatively, authors may simply be seeking statistical significance because it is perceived to be a necessary condition for scientific importance. The issue of publication selection bias has recently received heightened scrutiny among medical researchers as a consequence of the Vioxx and Paxil scandals. As a result of these purposeful suppressions of unfavorable clinical evidence, most leading medical journals now require the prior registration of all clinical trials as a condition for the later publication of their findings (Krakovsky, 2004).

On the demand side, journals may not publish results that are contrary to the editors' or referees' expectations or that are not statistically significant. Or, editors and referees may encourage researchers to modify their methodology until statistical significance or the expected result is found. To the extent that anyone in the complex process of research and publication has a preference for results of a given type, then publication selection bias will be present in that literature. The knowledge (or the mere belief) that editors and reviewers have a preference for statistically significant results is sufficient to cause authors to engage in selection in order to satisfy their customers' perceived demand.

It would be a great endorsement of academic research if publication selection bias were absent from academic research. Sadly, the large majority of meta-analytic investigations of publication selection have, thus far, found the statistical traces of bias. Examples include: Card and Krueger (1995) on minimum wage effects; Ashenfelter *et al.* (1999) on returns to education; Görg and Strobl (2001) on the productivity effects of multinationals; Stanley (2005) on the price elasticity of water; Abreu *et al.* (2005) on economic growth convergence; Nijkamp and Poot (2005) on the wage curve; Rose and Stanley (2005) on the trade effects of common currency; Doucouliagos (2005) on the growth effects of economic freedom; and Mookerjee (2006) on the growth effects of exports.[5]

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[4] It is also possible that some authors will even falsify results, although this is hopefully a very rare event.

[5] The literature on the impact of unions on productivity levels is a rare exception to this pattern (Doucouliagos and Laroche 2003).

But then much of the publication selection found in economics may be the result of an understandable desire to report findings that are statistically significant (hence noteworthy) and of the 'correct' sign (hence perceived to be 'valid'). For example, all economists know that there is an inverse relationship between price and quantity demanded—the 'Law' of demand. Thus, when estimating the price elasticity of water demand, some researchers will suppress aberrant positive estimates in the belief that they are correcting some data or specification error and are thereby reporting more accurate and reliable elasticity estimates. Likewise, a finding that raising wages causes workers to be *less* productive seems to be an obvious error; some important consideration must have been omitted. Such publication selection is clearly seen among estimates of water price elasticities, and it causes a three or four-fold exaggeration of the magnitude of this elasticity (Stanley, 2005). Even when selection is for the best of scientific reasons, it can greatly distort any summary or average (whether weighted or not) of a research literature.

B. Heckman regression

Because we do not have access to all of the original findings, manuscripts, their revisions, and the associated referee reports, we have no effective way of directly detecting or modeling the selection process. Available estimates constitute the incidental truncation of the population of estimates (Wooldridge, 2006). The problem of publication selection bias is much the same as sample selection bias in general (Heckman, 1977), but with a twist. Widely used econometric models that correct for sample selection biases typically employ Heckman's two-equation system:

$$\hat{\eta} = Z\beta + \varepsilon \tag{8}$$

$$P^* = K\alpha + u \tag{9}$$

where P^* represents the probability that an estimated effect is published (or reported), hence observable, and *K* is a matrix of variables that influence the probability of being published or reported

(Davidson and MacKinnon, 2004, pp.486-89).[6] Typically, equation (9) is estimated by a probit on the observable *P* = {0,1} using the entire sample of selected *and* non-selected observations. However, in the case of publication selection, we do not generally have access to the non-selected (unreported) estimates.[7] Thus, step one in the conventional Heckman two-step method cannot be estimated for the publication selection of empirical economic research.

To reveal the consequences of this limitation, consider the 'Heckman regression', which is the second step of Heckman's method:

$$\hat{\eta} = Z\beta + \rho\sigma I(K\hat{\alpha}) + e \tag{10}$$

where $I(K\hat{\alpha})$ is the inverse Mills ratio, ρ is the correlation between ε_i and u_i, and σ is the standard error of ε_i (Davidson and MacKinnon, 2004, pp.486-88). Unfortunately, the usual Heckman method is unavailable to us because the first-step probit is needed to estimate $\hat{\alpha}$ and hence to identify the inverse Mills ratio, $I(K\hat{\alpha})$. Without this first step, $I(K\hat{\alpha})$ becomes an omitted variable in the estimation of β from $\hat{\eta} = Z\beta$.

Fortunately, empirical economic research is 'blessed' with heteroscedasticity. That is, the standard error of $\hat{\eta}_i$, SE_i, is likely to vary greatly from one study to the next. Like the nonlinearity of the inverse Mills ratio that permits the identification of equation (10) in the typical economic application (Davidson and MacKinnon, 2004, p.489), it is this heteroscedasticity of empirical economic estimates that makes possible the identification and estimation of Heckman regression for publication selection, equation (10). Replacing the inverse Mills ratio term in Heckman regression with $\alpha_0 SE_i$ gives our

[6] Although *K* was used previously for capital, following conventional practice, it is used here and in the remainder of this paper to represent any variable related to publication selection.

[7] Of course, we all know the values of all the estimates for our own research but not the unreported findings of others. Even unpublished working papers and dissertations may contain only selected results in anticipation of future peer review.

previously reported equation (6). In this equation, $\alpha_0 SE_i$ represents systematic selection for statistical significance. Studies with smaller samples and hence larger standard errors (SE_i) will need to run and re-run their models more intensely to achieve a statistically significant result. More precise studies (i.e., those with a smaller $1/SE_i$) will require less searching and less selection to obtain the desired significant result. Thus, the telltale signal of publication selection is a systematic relation of reported effects with their standard errors revealed by a meta-regression analysis (Stanley 2005).

However, the meta-regression equation (6) will itself contain obvious heteroscedasticity, because SE_i is the estimated standard deviation of $\hat{\eta}_i$, which will vary from observation to observation. The obvious remedy is to divide equation (6) by SE_i to obtain the weighted least squares (WLS) version of this MRA.

$$\hat{\eta}_i / SE_i = t_i = \alpha_0 + \beta_0 / SE_i + \Sigma\beta_k Z_{ki} / SE_i + \nu_i \tag{11}$$

where t_i is the reported t-value of the ith estimated elasticity. It is this meta-regression model and its extensions that form the basis of our investigation of the efficiency-wage literature.

Without making the connection to Heckman regression, medical researchers have widely used the simple MRA version of model (11) as a test for publication bias:

$$t_i = \alpha_0 + \beta_0 / SE_i + \nu_i \tag{12}$$

(Egger *et al.*, 1997; Sutton *et al*., 2000). Testing whether α_0 = 0 becomes the test for the presence of publication bias. This method for identifying publication selection has been called a funnel-asymmetry test (FAT) due to its relation to funnel graphs (Stanley, 2005).

A funnel graph plots precision, the inverse of the standard error ($1/SE_i$), against the reported estimated effects. In our case, we use the wage elasticity of production. Figure 1 plots these estimated elasticities and their precision.[8] As can be seen in Figure 1, efficiency-wage elasticities are almost entirely positive and skewed to the right, indicating publication selection for positive wage elasticities. Even clearer, and more importantly, is the asymmetry of the efficiency-wage funnel graph. This asymmetry becomes more apparent when compared to what the absence of publication is expected to look like—see Figure 2. However, any such speculation quickly reveals the underlying weakness of using graphs to identify publication bias. Visual inspection is inherently subjective and must remain somewhat ambiguous; hence, the motivation for a formal statistical test such as: FAT (H_0: α_0 =0).

FIGURE 1 ABOUT HERE

FIGURE 2 ABOUT HERE

Simulations show that the MRA estimate of β_0 in equation (12) also serves as a test for genuine empirical effect corrected for publication bias (Stanley, 2008). Because $1/SE_i$ is the precision of this estimate of empirical effect, this test (H_0: β_0 =0) has been called the 'precision-effect test' (PET), which makes the meta-regression model (12) a FAT-PET-MRA. These simulations also reveal that PET is very powerful and robust to the intensity of publication selection (Stanley 2008). However, there is also a downward bias in the FAT-PET-MRA estimate of β_0 when there is an authentic empirical effect (*i.e.*, β_0 ≠0). Although there are several reasons for such a bias, the fact that the inverse Mills ratio term in equation (10) is a nonlinear function of *SE* is likely a contributing factor.[9]

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[8] See section IV for the details about how this data was collected.

[9] Below, we will introduce a nonlinear MRA that simulations show greatly reduces this bias. See the Appendix of this paper for simulations of the bias of this PET estimate relative to several other estimates, including an improved nonlinear version of PET—PEESE. Another source of the attenuation in the estimate of β_0 is the fact that SE_i is an estimate and, hence, has

Equations (6) and (11) may be expanded to capture the dependence of reported effects on the omitted selection variables, *K*.

$$\hat{\eta}_i = \beta_0 + \alpha_0 SE_i + \Sigma\beta_k Z_{k\,i} + SE_i \Sigma\alpha_j K_{j\,i} + \varepsilon_i \tag{13}$$

$$t_i = \alpha_0 + \beta_0 / SE_i + \Sigma\beta_k Z_{k\,i} / SE_i + \Sigma\alpha_j K_{j\,i} + \nu_i \tag{14}$$

for i= 1, 2, . . ., I; where I is the number of estimates reported in a literature. The variables, *Z*, explain the heterogeneity in the true estimated effects or large sample bias that arises from misspecification, and the *K*-variables identify factors that influence the propensity that an estimate gets published or reported. Because the inverse Mills ratio is a nonlinear function of $K\hat{\alpha}$, higher order terms of *K* might be needed in the FAT-PET-MRA equation (14). However, in our application to the efficiency-wage literature as well as many other meta-regression analyses, the *K*-variables will be dummy variables, making *K* perfectly collinear to K^2 or any other power of *K*. Thus, the potential nonlinearity of *K* is irrelevant for our application. As simulations indicate, equations (6) and (13) may also be nonlinear in SE_i. This issue is addressed explicitly in the next section.

C. Correcting publication selection—PEESE

Thus far we have discussed how publication selection may be identified and how genuine empirical effect 'beyond' publication selection can be tested. However, empirical researchers and policy makers also need an accurate estimate of the magnitude of the underlying effect, corrected for possible publication bias. If the inverse Mills ratio were constant and independent of SE_i, then MRA model (12) would be correctly specified and could provide consistent estimates of the true effect

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sampling error. Such errors in the independent variable (errors-in-variables) cause a well-known downward bias. See Stanley (2005) for a possible remedy.

corrected for publication bias. Unfortunately, neither is true, which explains the low power of using H_0: α_0=0 as a test for publication bias and the downward bias in the estimate of β_0 (Egger et al., 1997; Stanley, 2008, and see the Appendix). The inverse Mills ratio depends on SE_i.[10] Thus, the relationship between the observed effect and its standard error will be nonlinear in the presence of publication selection.

This nonlinearity with respect to SE_i becomes the basis for a new approach to estimation, corrected for publication bias—precision-effect estimate with standard error (PEESE). Typically, nonlinear relations are estimated using a power series. Thus, PEESE begins with the square of SE_i (*i.e.*, the variance of each estimated elasticity) as the second term of MRA model (6).

$$\hat{\eta}_i = \beta_0 + \alpha_0 SE_i^2 + \varepsilon_i \tag{15}$$

However as before, this model has obvious heteroscedasticity; thus the WLS version would divide equation (15) by SE_i giving:

$$t_i = \alpha_0 SE_i + \beta_0 / SE_i + \nu_i \tag{16}$$

Note that there is now an additional independent variable SE_i, compared with equation (12), and that there is no intercept. In this framework, $\hat{\beta}_0$ is the estimated value of η corrected for

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[10] An exception to this rule is for the case when there is no genuine effect, T_e=0. In general, it can be shown that the derivative of inverse Mills ratio with respect to SE_i is: $[T_e I(T_e/SE_i - c) / SE_i^2][(T_e/ SE_i - c) + I(T_e/ SE_i - c)]$; where c is the critical value from the t-distribution. See Davidson and MacKinnon (2004) and Wooldridge (2006, p. 598). Because this derivative is generally nonzero when $T_e \neq 0$, the inverse Mills ratio is not constant with respect to SE. But conversely, when T_e =0, this derivative is zero, and the relationship between the observed effect and its standard error will be linear.

publication selection or the precision-effect estimate with standard error (PEESE). The Appendix reports the results of Monte Carlo simulations of this corrected estimate of η, and shows that PEESE greatly reduces the potential bias of publication selection, and compares its bias to several other overall estimates of elasticity.

Our approach in this paper is to explore the efficiency-wage literature for evidence of publication selection and to correct the overall estimate of wage elasticity for publication selection using (16) and variants of equation (14). In this framework, $\hat{\beta}_0$, from equation (16), is the estimated value of η, corrected for publication selection. From equation (14), $\hat{\alpha}_0$ is the direct measure of publication bias, $\hat{\beta}_k$ are the coefficients on other variables that affect the magnitude of η, and $\hat{\alpha}_j$ are the coefficients on other variables that affect the propensity for publication selection. In the more complex multivariate meta-regression analysis (14), publication selection is modeled by the combination of $\hat{\alpha}_0$ and the $\hat{\alpha}_j K_j$ terms.

This framework is flexible enough to facilitate the testing of several hypotheses. First, it enables us to test whether η varies: (a) across country *(UK* or *US*), (b) by the level of aggregation (*Firm* or *Industry*), (c) through the choice of wage and output measures (*RelWage* for relative wages*, ValAdded,* for value added*, and PerWorker* for output per worker), (d) by allowing potential simultaneity between wages and productivity (*Simul*), and (e) by including other relevant variables (a measure of capital stock, *Cap*, union membership, *Union*, and human capital, *HumCap*).

IV. Meta-Analysis of the Efficiency Wage Hypothesis

A. The Data

A comprehensive search of the literature was conducted of several databases, including ECONLIT for: ‘efficiency wage_‘ and (‘empirical’ or ‘ test_‘ or ‘estimate_‘). This search produced over 200 ‘hits.’ After culling those that had any chance of containing an applied or empirical finding, we obtained over

100 working papers, books, dissertations, and journal articles. In all cases, we erred on the side of inclusion and did not exclude any paper because of language.[11] This literature search netted 75 estimates of the wage elasticity of production contained in 14 studies. Given the immense interest in efficiency wage theory, it is surprising to find only 14 empirical studies that offer comparable estimates of the efficiency-wage elasticity. A complete reference list is available from the authors.

Although the efficiency-wage literature contains several different empirical outcome measures, the most commonly reported measure of the efficiency-wage effect is the wage elasticity of production, $\hat{\eta}_i$. The average reported wage elasticity is 0.63 (p<.001). This magnitude is approximately equal to what the first-order conditions of neoclassical theory require. Recall that profit maximization, equation (4), predicts that the wage elasticity of production should be equal to labor's share. Labor's share is one of the few macroeconomic 'stylized facts' thought to be stable. Traditionally, labor's share is assumed to be 2/3, although it is recognized to vary by industry and over time. More recent estimates report values around 0.6 or much higher (Krueger, 1999). Superficially, therefore, this simple average of reported elasticity estimates seems to confirm the neoclassical version of the efficiency wage hypothesis.

However, economic research usually contains publication bias and considerable heterogeneity, both of which call into question the validity of using any average elasticity estimate. First, we turn to the issue of whether there is publication bias and how best to correct it. After that, we will use MRA to explain any excess heterogeneity.

B. Identifying and correcting publication selection

Table 1 reports MRA models (12) and (16). Recall that FAT is a test for publication selection, and it tests H_0: $\alpha_0 = 0$ in equation (12). The efficiency-wage literature shows clear evidence of

[11] We were unable to obtain only two papers that might have contained estimates. Both were published in narrowly circulated journals—one in the Netherlands and the other in Turkey. The search for studies was ended July 2006. Any studies subsequent to that date have not been included in our dataset.

publication selection (equation (12); t= 2.67; p<.01). In spite of this selection, equation (12) also contains strong evidence of a genuine efficiency-wage effect (PET; t= 18.41; p<.0001)—recall the precision-effect test of Section 2 and see Stanley (2005) and Stanley (2008).[12] These results again confirm the relevance of the EWH. Thus far, we have clear evidence of both publication bias and a real efficiency-wage effect.

Given that a genuine efficiency-wage effect exists, how large is it? Is it consistent with the neoclassical version of EWH? The coefficient on precision (1/SE) in equation (16) Table 1 is 0.32. This is the PEESE estimate of the efficiency-wage elasticity, corrected for publication selection bias. Note that this correction nearly halves the previous simple average. Even with modest publication selection,[13] it can induce a bias that has practically important implications (estimated bias= 0.63 - 0.32= 0.31). Our corrected estimate of the efficiency-wage elasticity rejects of any labor's share of 0.4 or larger. Because PEESE is so much less than any credible estimate of the aggregate labor's share, our meta-analysis of the efficiency wage literature calls into question the neoclassical version of the EWH.

TABLE 1 ABOUT HERE

If the neoclassical view is not valid, what is? "(I)n its most general form, efficiency wage theory is virtually tautologous" (Akerlof and Yellen, 1988, p.44). Taking this risk of turning the EWH into a tautology, one might observe that turnover costs are the most obvious omission from our profit function and add turnover costs, *T(w)*, explicitly into it. First order conditions would then mandate that:

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[12] We do not use the MRA coefficient on 1/SE from equation (12), *as an estimate of wage elasticity*, because it is known to be biased downward when there is an effect. See the Appendix. This is the main reason that we prefer to use PEESE instead. Nonetheless, testing whether this coefficient on 1/SE from equation (12) is or is not zero, (*i.e.*, H_0: $\beta_0 = 0$) remains a valid and robust test for a genuine empirical effect beyond publication bias (Stanley, 2008).

[13] The magnitude of $\hat{\alpha}_0$, which is 0.98 in equation (12) Table 1, serves as a good proxy for the severity of publication selection (Doucouliagos and Stanley, 2007).

$$\eta = \partial Y/Y \mid \partial w/w = \mathcal{Z} + (T/Y)E_t \tag{17}$$

where E_t is the wage elasticity of turnover costs.

Yet this amended formulation of profit maximization gives little support to neoclassical theory. Now, either turnover costs need to be a large proportion of output or turnover costs need to be highly elastic, or both. The empirical magnitudes revealed by our meta-analysis require that higher wages reduce turnover costs by roughly an amount equal to the value of the increased productivity. At a minimum, our results give additional credence to the labor turnover model of the efficiency wage hypothesis (Stiglitz 1974; Salop 1979).

C. Z and K variables and Multivariate MRA

Surely, more than publication selection is responsible for the wide variation we observe among reported wage elasticities? To explain this variation in observed elasticities, we coded a number of research characteristics that were thought to be potentially related to either the magnitude of η or to its publication selection.

Our first set of explanatory variables contains country dummies (US and UK). These are included as part of the *Z*-vector to capture any underlying differences in the efficiency-wage effect across countries. They are included also in the *K*-vector to test whether the selected country influences the publication process. Recall the distinction between *Z* and *K*- variables made in Section III. In our meta-regression analysis, we allow country composition to affect both the genuine effect and publication selection bias. We also explore whether including other relevant independent variables (capital stock, *Cap*, union membership, *Union*, and human capital, *HumCap*) affects reported elasticities. The last *Z*-variable is *Simul*, which denotes whether the researcher's model or methods allowed for the potential simultaneity between wages and productivity. An obvious criticism of the efficiency wage hypothesis is that higher productivity leads to higher wages through rent-sharing, rather than the other way around (Blanchard and Summers, 1986; Wadhwani and Wall, 1991). Thus, it is important to account for whether researchers took precautions for the likely simultaneity between wages and productivity. All of these *Z*-variables are divided by the standard error, SE, and denote genuine effects on the underlying wage elasticity—recall equation (14).

The *K*-variables are those that might be correlated with publication selection and some are conceptually linked to the first step probit of Heckman's selection method. Along with dummies for countries (*UK* and *US*), they include the level of aggregation (*Firm* or *Industry*) and the choice of wage and output measures used (*RelWage* for relative wages*, ValAdded* for value added*, and PerWorker* for output per worker). Perhaps editors or referees have a professional preference about which level of aggregation is most appropriate or which measures are the best?

Table 2 presents the results of estimating a more complex multivariate MRA model, by adding the K and Z-variables. The specific moderator variables included in Table 2 were selected using the 'general to specific' (GTS) approach; that is, all the above variables were included in the meta-regression model and the insignificant ones were omitted, one at a time. The results reported in Table 2 are quite consistent with our simple MRA findings, equation (12) Table 1. The efficiency wage effect is now contained in the Z-variables: *Simul/SE*, *Cap/SE*, and *UK/SE.* Note how the coefficients of *Simul/SE* and *Cap/SE* sum approximately to the same value as the simple MRA coefficient for 1/SE (equation (12) Table 1). Studies that include a measure of capital in their estimated production functions, which all should, produce significantly higher wage-productivity elasticities. Together, these two variables, Simul/SE and Cap/SE, represent the genuine positive efficiency-wage effect and are jointly statistically significant ($F_{(2,68)}$= 314.7; p<.001).

The positive effect of controlling for simultaneity, 0.18 (t=3.73; p<.01), is especially important. If the critique that it is productivity changes that cause movements in wages were valid, rather than the other way around, then those studies that explicitly allowed simultaneity would have marginally *lower* estimates of the efficiency-wage elasticity. Yet, we find the opposite. It is also important to use capital stock in one's empirical production function to avoid obvious omitted-variable bias, doing so also produces *higher* wage elasticities (Table 2). Therefore, *better* specified estimation models produce *large*r estimates of the efficiency-wage elasticity. All of these MRA results confirm the validity of a positive efficiency-wage effect. Note also that the UK exhibits higher wage elasticities, by approximately 0.20 (t=2.39; $p \leq .01$). But even in the UK, MRA estimates this wage elasticity (0.50= 0.18+0.13 +0.19) to be lower than the accepted stylized fact.

<u>TABLE 2 ABOUT HERE</u>

Table 2 also shows that publication selection depends on several factors: whether production is measured by value added (*ValAdded*), whether wages are measured relatively (*RelWages*) and whether the study measures these wages and production at the industry level (*Industry*). Publication selection is associated with those studies that use industry-level data, value added output measures, and that do not employ relative wages. The joint significance of these variables again confirm the presence of publication selection ($F_{(3,68)}= 6.03$; $p \leq .001$).

Together these moderator variables explain 91% of the variation in the observed t-values and 82% of the estimate-to-estimate variation in reported wage elasticities. Not only are the multivariate MRA results consistent with the simple FAT-PET-MRA (equation (12) Table 1), but they are also quite robust. Column 2 of Table 2 presents the estimated MRA coefficients from a random-effects multi-level model that allows for dependence of the reported elasticities within a given study. Within study dependence has long been recognized as a potential problem for meta-regression, and multi-level models can compensate for any observed within-study dependence (Bateman and Jones, 2003). Column 3 Table 2 reports the robust regression version of this same multivariate MRA model. Note that these MRA results are quite robust to different methods and models. The important coefficients, especially on *Simul/SE* and *Cap/SE*, remain the same. The only noteworthy differences are that the magnitude and significance of *UK/SE* falls in the robust regression, and *ValAdded* is no longer significant. Even if we remove these two variables, all of our previous inferences remain unchanged—including the sign, significance and the sum of the remaining coefficients. Lastly, as a further check for robustness, this MRA model is run again by omitting each study, one at a time. In all cases, we obtained the same general findings concerning the sign and significance of the important MRA coefficients.

V. Summary and Conclusions

This paper extends Heckman's econometric methods for selection correction to a critical research domain—publication selection and its effects on our research base. Econometrically, this is a difficult problem because data from non-selected (hence unpublished) studies are generally unavailable. However, serendipitous heteroscedasticity among reported econometric estimates allows selection effects to be identified from a background of random variation and heterogeneity among genuine empirical effects. In addition to Heckman's methods that identify publication selection and test for genuine effects beyond publication selection, we offer a new MRA model (PEESE, precision-effect estimate with standard error), which reduces the bias caused by publication selection. The resultant meta-regression models are applied to the empirical literature on the efficiency wage hypothesis (EWH).

Our meta-analysis of the wage elasticities of production finds the average reported elasticity to be 0.63, which is roughly equal to the labor's share as required by neoclassical theory. However, after correcting for publication bias, this value goes down to 0.32, practically and statistically very different than labor's share. Thus, these Heckman meta-regression methods call into question the conventional neoclassical version of the EWH. Publication selection has a large practical effect on the efficiency wage literature.

Consistent with this large observed publication bias is evidence of publication selection from the funnel-asymmetry test ($t=2.67$; one-tailed $p<.01$). Nonetheless, the evidence of a genuine efficiency-wage effect, regardless of publication selection, remains very strong ($t=18.4$; $p<.0001$). Like all other meta-regression analyses in economics, we find that the magnitude of the reported wage elasticity depends on other factors. Region, *UK*, using a measure of capital stock, and employing an econometric method that can accommodate possible simultaneity between productivity and wages all have sizable effects on the reported wage elasticity. Those studies that allow for simultaneity report significantly larger wage elasticities ($t=3.73$; $p<.001$), corroborating the validity of the efficiency wage hypothesis. Likewise, including a measure of the capital stock in the researcher's model increases the reported wage elasticity ($t=2.77$; $p<.01$). Together, these effects sum to practically the same elasticity estimate, 0.31, as that given by PEESE's corrected estimate. All of our MRA results are consistent, confirm the presence of an efficiency-wage effect, and are robust to variations in methods and the studies included in the meta-analysis.

Although publication selection need not be unethical or nefarious, its bias often has practical consequences for economic research and policy. Therefore, the effects of publication selection must be understood and filtered from any summary of research findings if we are to have a clear or accurate idea about what research is telling us.

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Appendix: Simulations of Precision-Effect with Standard Error (PEESE)[14]

Our simulations are premised on a research literature that tests a regression coefficient (*i.e.,* H_0:β_1=0). This simple regression estimate is merely illustrative of any statistical estimate. Similar statistical properties for precision-effect estimate with standard error (PEESE), equation (16), are found for other types of statistical estimates.[15]

The basic structure of our meta-regression simulations may be sketched as:

1. Generate the regression variables randomly.
2. Use OLS to estimate and test H_0: β_1=0. Select significant test results. Each selected test of H_0: β_1=0 comprises one study's reported result in our hypothetical empirical literature.
3. Simulate these meta-regression tests by repeating the previous steps either 20 or 80 times. At this stage, meta-regression model (16) is estimated to provide one PEESE estimate.
4. Repeat all of the above steps 10,000 times.

Step 1 is the data-generating process. X_1 is the independent variable and is made to be a random uniform variable (100, 200). Then, the dependent variable is generated by:

$$Y_i = 100 + \beta_i X_{1i} + \beta_2 X_{2i} + 100\, e_i \qquad i=1, 2, \ldots n \qquad \text{(A.1)}$$

e_i ~NID(0,1). β_i is either 0 or 1. When β_i=1, the average R^2 is approximately 9%, and the correlation coefficient is approximately 0.3. The role of $\beta_2 X_{2i}$ is to induce misspecification bias, in general, and omitted-variable bias, in particular.

14 See Stanley (2008) for a more detailed description and discussion of these simulations.

15 A team of medical researchers have corroborated these simulations in the context of estimating the logarithm of the odds ratio (Moreno et al., 2007). The odds ratio is the conventional measure of the effect of a new medical treatment or new drug where there is a binary outcome.

Omitting a relevant variable from a regression model causes the estimate of β_l to be biased and inconsistent. Such bias remains in large samples and does not lessen with the standard error. Therefore, it can be mistaken for a genuine effect, potentially confusing any statistical estimate that tries to identify genuine effect. Small-sample bias causes little or no difficulty for MRA methods. Although only omitted-variable biases are simulated here, they serve as a paradigm of any type of large-sample, misspecification bias. Because large-sample bias is the most problematic for these MRA methods, it is the only type of bias used in these simulations. Cases where there are no misspecification biases have also been simulated but are not reported here because they are even more favorable to PEESE.

Random misspecification bias is induced by making β_l in equation (A.1) a random normal variable, N(0,.25). This random misspecification bias acts as 'heterogeneity,' which has been recognized as a key parameter by meta-analysts (Sutton et al., 2000; Moreno et al., 2007). The difference is largely a matter of interpretation. Here, we assume that variation in the expected estimated effects is caused by some weaknesses in our empirical methods; whereas, heterogeneity is usually viewed as variation in the 'true' effect.

The most important magnitude for the performance of meta-regression methods is the typical size of the misspecification bias, relative to the sampling error. The magnitude of publication bias increases with the ratio of the standard deviation of these misspecification biases (σ_{bias}) to the standard deviation of the sampling errors (σ_{bl}). The average observed variation among reported effects found among previous meta-analyses implies a standard deviation of this misspecification bias of approximately 0.1. To be conservative, this value is multiplied by two and half times, σ_{bias}=.25, and this value is again doubled to explore the robustness of our MRA methods.[16]

We further assume that there are either 20 or 80 studies. Twenty is chosen because it is a rather small sample size for any regression estimate, while eighty is selected because many meta-analyses in economics contain eighty or more estimates. A MRA sample size of eighty is both

[16] See Stanley (2008) for greater details. There, the size of these misspecification biases is doubled yet again.

practically feasible and gives these MRA methods high power (Stanley, 2008). Statistical power depends in the expected way on the MRA sample size, σ_{bias}, the magnitude of the effect, and σ_{bl}. Sample sizes for the original studies are {30, 50, 75, 100, 200}.

Publication bias is simulated as selecting a statistically significant positive b_1. That is, if the random estimate does not provide a significantly positive t-value, a new sample is taken and the original regression is run again with different random errors and random biases until a significant t-value is obtained by chance. For example, the 50% publication selection condition assumes that exactly half of the studies estimate and re-estimate their regression models until a random, yet statistically significant, estimate is found and reported. For the other half, the first random estimate, significant or not, is reported and used.

In practice, not all reported results that are published will have been selected for statistical significance. Among previous economic meta-analyses, the proportion of statistically significant results varies from 29% to 79%. 100% publication selection is not simulated, because we have observed no such case in economics. Economic research is too contentious to permit unanimous agreement. Therefore, it is assumed that the incidence of publication selection is either: 0%, 25%, 50% or 75%.

The conventional estimates of an overall effect (the simple average, fixed-effects and random-effects) have large biases when there is publication selection, and in all cases PEESE greatly reduces this bias. Recall that fixed-effects and random-effects estimates are weighted averages, where the weights are the inverse of each reported estimate's variance. The fixed-effects estimates assumes that the estimates come from a common population mean; whereas the random-effects estimate does not (Sutton et al., 2000).

When there are genuine effects and publication selection, Appendix Table 1 shows that PEESE has the smallest bias. However, the simple MRA estimate of β_0 from equation (12) has the smallest bias when there is no underlying empirical effect (*i.e.*, β_1=0). The superiority of $\hat{\beta}_0$ when T_e=0 is an implication of our interpretation the MRA model of publication selection as a Heckman regression. When T_e=0, the relation between an estimated effect and its standard error will be linear, and MRA model (12) is correctly specified (recall note 10). Thus, precision-effect estimate with standard error (PEESE) should be used only when then there is evidence of a genuine effect (*i.e.*, we can reject H_0: β_0=0 in equation (12)).

APPENDIX TABLE 1: MEANS OF PEESE AND CONVENTIONAL ESTIMATORS

Hetero-geneity	True effect	Selection Incidence	Sample Size	Simple Average	Fixed-effects	Random-effects	$\hat{\beta}_0$	PEESE
σ_{bias}=.25	0	0%	20	-.0005	.0002	.0003	.0033	.0001
	0	0%	80	-.0012	.0007	.0007	.0029	.0002
	0	25%	20	.2334	.1953	.2140	.0415	.1274
	0	25%	80	.2345	.1973	.2158	.0391	.1314
	0	50%	20	.4676	.3927	.4304	.0608	.2528
	0	50%	80	.4677	.3920	.4300	.0619	.2518
	0	75%	20	.7004	.5846	.6273	.0699	.3580
	0	75%	80	.7016	.5857	.6288	.0738	.3624
	1	0%	20	1.0012	.9980	.9983	.9969	1.0005
	1	0%	80	1.0001	.9997	.9996	1.0002	.9987
	1	25%	20	1.0660	1.0361	1.0429	.9289	.9897
	1	25%	80	1.0653	1.0385	1.0450	.9272	.9882
	1	50%	20	1.1320	1.0761	1.0876	.8495	.9722
	1	50%	80	1.1318	1.0762	1.0871	.8489	.9756
	1	75%	20	1.1988	1.1150	1.1297	.7693	.9576
	1	75%	80	1.1971	1.1133	1.1253	.7711	.9580
σ_{bias} =.50	0	0%	20	.0027	-.0025	-.0022	.0033	.0030
	0	0%	80	.0011	.0001	-.0003	-.0019	.0016
	0	25%	20	.2701	.2267	.2546	.0449	.1443
	0	25%	80	.2706	.2258	.2545	.0391	.1463
	0	50%	20	.5393	.4486	.5043	.0874	.2992
	0	50%	80	.5399	.4510	.5062	.0847	.2936
	0	75%	20	.8115	.6802	.7461	.1373	.4472
	0	75%	80	.8095	.6803	.7478	.1358	.4436
	1	0%	20	.9996	.9996	.9999	.9943	1.0004
	1	0%	80	.9991	.9993	.9994	.9993	1.0012
	1	25%	20	1.0962	1.0667	1.0815	.9388	1.0147
	1	25%	80	1.0966	1.0652	1.0810	.9438	1.0122
	1	50%	20	1.1958	1.1301	1.1590	.8827	1.0231
	1	50%	80	1.1933	1.1298	1.1596	.8798	1.0205
	1	75%	20	1.2901	1.1937	1.2317	.8171	1.0294
	1	75%	80	1.2909	1.1937	1.2331	.8096	1.0263

FIGURE 1: FUNNEL GRAPH OF EFFICIENCY-WAGE ELASTICITIES

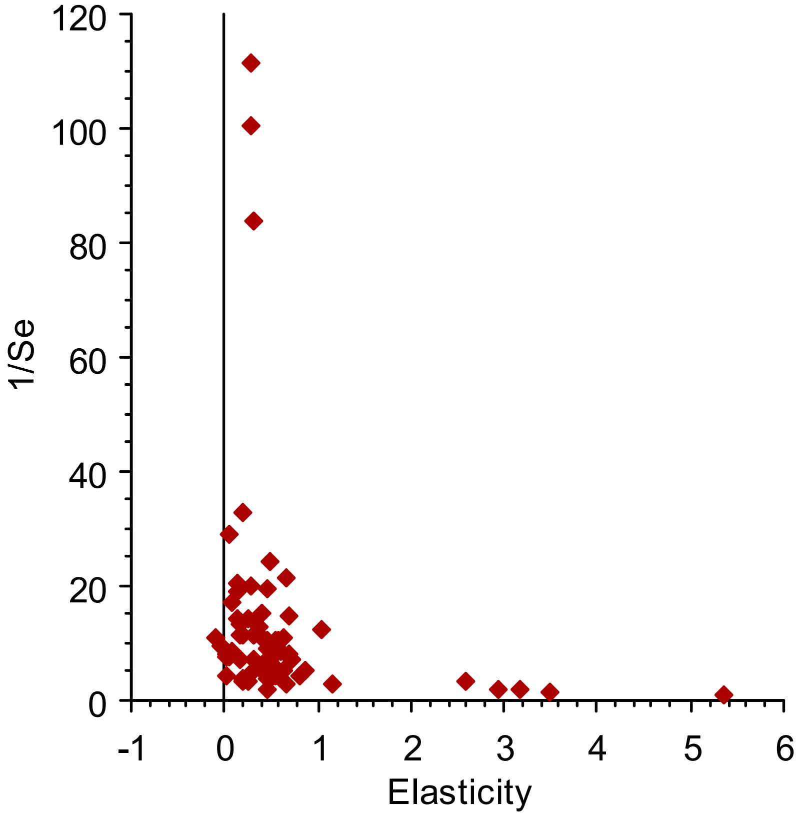
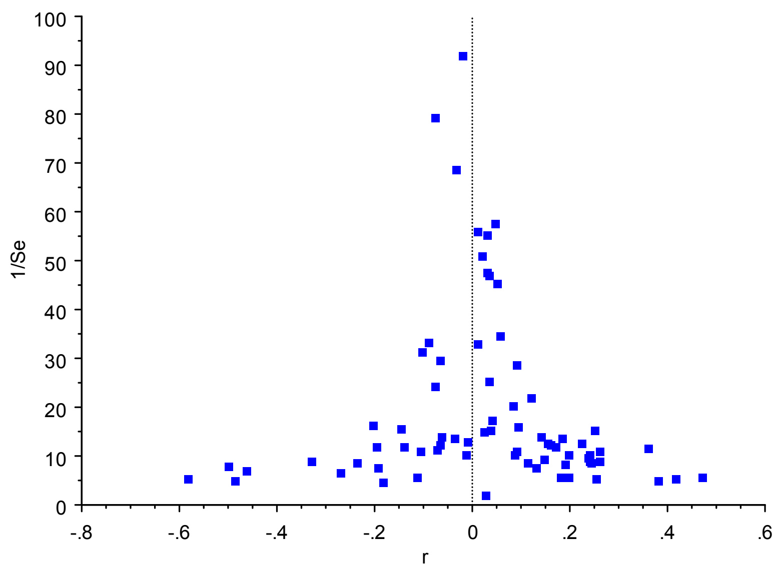


FIGURE 2: FUNNEL GRAPH OF UNION-PRODUCTIVITY PARTIAL CORRELATIONS (r)



Source: Doucouliagos and Laroche (2003)

TABLE 1: PUBLICATION SELECTION AND EFFICIENCY-WAGE ELASTICITIES

Independent Variable	Simple FAT-PET-MRA Equation (12)	PEESE-MRA Equation (16)
Constant	0.98 (2.67)**	-----
1/SE	0.30 (18.41)**	0.32 (23.41)**
SE	-----	2.45 (2.82)**
Standard Error	2.65	2.63
R-squared	0.82	0.89

Dependent variable: reported t-statistics. ** denotes statistically significant at the 1% level. t-statistics are in parentheses.

TABLE 2: MULTIVARIATE MRA OF THE EFFICIENCY WAGE HYPOTHESIS

Moderator Variables	Column 1 WLS (14)	Column 2 REML (14)	Column 3 Robust (14)
Intercept	1.87 (3.59)**	1.87 (3.12)**	1.84 (4.85)**
Simul/Se	0.18 (3.73)**	0.18 (3.56)**	0.18 (5.25)**
Cap/Se	0.13 (2.77)**	0.13 (2.82)**	0.12 (3.62)**
UK/Se	0.19 (2.39)**	0.20 (2.19)*	0.09 (1.61)
Industry	1.29 (2.39)**	1.24 (1.94)*	1.05 (2.69)**
RelWage	-1.90 (-3.42)**	-1.90 (-2.87)**	-1.59 (-3.94)**
ValAdded	1.39 (2.20)*	1.54 (2.11)*	0.64 (1.38)
Standard Error	1.97	1.92	_
R-squared	.91	_	_

Dependent variable: reported t-statistics. * and ** denote statistically significant at the 5% and 1% one-tailed level, respectively. t-statistics are in parentheses. All regressions relate to equation 14.