Technological innovation and employment in derived labour demand models: A hierarchical meta-regression analysis

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6 July 2016

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Abstract

The effect of technological innovation on employment is of major concern for workers and their unions, policy-makers and academic researchers. We aim to provide a quantitative synthesis of the evidence base and the extent of heterogeneity therein. Analysing 567 estimates from 35 primary studies that estimate a derived labour demand model we report the following findings: (i) the effect on employment is positive but small and highly heterogeneous; (ii) publication selection bias reflects a tendency to support the twin hypotheses that process innovation is associated with job destruction whereas product innovation is associated with job creation; (iii) the effects of process and product innovations do not conform to theoretical predictions or narrative review findings after selection bias is controlled for; (iv) only a small part of the residual heterogeneity is explained by moderating factors; (v) country-specific effect-size estimates are related to labour-market and product-market regulation in six OECD countries in a U-shaped fashion; and (vi) OLS estimates reflect upward bias whereas those based on time-differenced or within estimators reflect a downward bias. Our findings bridge the evidence gap in the research field and point out to data quality and modeling issues that should be considered in future research.

Keywords: Innovation, employment, technological change, labour demand, meta-analysis

The research for this paper is part of an ESRC Project titled *Evaluation of Research and Development (R&D) Expenditures, Firm Survival, Firm Growth and Employment: UK Evidence in the OECD Context.* Reference no ES/K004824/1. We thank the funders for their support, subject to usual disclaimer: the views expressed here are those of the authors only, who are responsible for any errors or omissions.

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1. Introduction

The effect of technological change on employment has always divided opinions. Since the Luddite riots of the early 19th century in England, workers and their unions have emphasized the risks of de-skilling and technological unemployment. At the opposite end of the debate, business managers and policy makers tended to argue that technological change is essential for growth and job creation. In between, economic theory distinguishes between the short- and long-run effects: technological innovation may be associated with worker displacement in the short run, but the adverse effect is likely to be reversed as compensation mechanisms induce higher demand for labour.

Several narrative reviews of the extant literature exist. Whilst Chennells and van Reenen (2002) discuss the variation in the evidence base in the light of modeling, estimation and data-related issues others pay attention to additional sources of heterogeneity such as compensation mechanisms, levels of development, and types of technological innovation among others (Spiezia et al., 2002; Piva, 2003; Pianta, 2004; and Vivarelli (2014). The narrative review findings inform three general conclusions. First, the effect of technological innovation on employment is contingent on a wide range of moderating factors, including labour market flexibility, product market competition, types of innovation, national innovation systems, and international trade. Second, the balance of evidence does not point out a negative effect on employment, but process innovation is more likely to be associated with job destruction whereas product innovation is more likely to be associated with job creation. Finally, the effect is more likely to be negative when the data relates to unskilled labour.

We have identified a number of issues that justify a novel review. First, the existing reviews are aware of the sources of heterogeneity in the evidence base, but their conclusions concerning the sources of heterogeneity require quantitative verification. Secondly, the existing reviews do not take into account the risk of publication selection bias, which may arise when authors or editors choose to publish findings that support or reject a given hypothesis more often than contradictory or insignificant findings. Third, the existing reviews do not allow for inference about the magnitude of the average 'effect size' and whether the latter is robust to data dependence that may arise when primary studies draw on the same dataset or overlapping datasets. Finally, and despite the significant role accorded to labour- and product-market flexibility in the theoretical models, existing reviews do not evaluate the relationship between country-specific labour or product market institutions and primary-study estimates.

We aim to address these issues through meta-regression analysis, a quantitative method of literature review that has been used extensively in medical research and has gathered momentum in economics research (Stanley and Doucouliagos, 2012; Stanely et al., 2013). Focusing on primary studies informed by a *derived labour demand model (DLDM)*, we report the following findings: (i) the extent of between-study heterogeneity that cannot be explained by sampling variations is high (over 75%) in the full sample and in some of the sub-samples that reflect specific combinations of innovation and skill types; (ii) the effect-size is positive but small in the full sample and in subsamples that capture different combinations of innovation and skill types; (iii) the effect on the demand for unskilled labour is smaller than skilled or mix-skills labour demand, but there is no evidence of negative effect on unskilled labour demand; (iv) there is evidence of moderate positive publication selection bias in the overall evidence base, but the bias is large and reflects selection in favour of theoretical predictions in the case of process and product innovation subsamples; (v) the evidence based on firm/industry data from six OECD countries reveals a U-shaped relationship between the 'effect-size' estimates and labour/product market regulation; and (vii) although the effect is larger in primary studies published after 2000, it is relatively smaller when the primary studies use panel data and instrumental variable estimation methods, draw on data related to high-innovation-intensity firms/industries, and they measure innovation with intellectual property assets.

The rest of the paper is organised as follows. In section 2, we summarise the existing review findings and highlight the issues that cannot be addressed effectively in narrative reviews in general. Section 3 introduces the derived labour demand model (DLDM) and discusses why we restrict our sample to primary studies that draw on this model only. In section 4 we report the systematic review rules and provide an overview of the evidence base with respect to dimensions such as study type/date, model specification, sample characteristics, and estimation methods. In section 5, we introduce the bivariate and multivariate meta-regression models and discuss how we choose the appropriate estimators. Section 6 reports the meta-regression findings and compare the latter with theoretical predictions and narrative review findings. In the conclusions, we discuss the implications for future research.

2. Technological innovation and employment: what do we know?

Writing only a few years after the Luddite Riots, Riccardo was of the view that the "substitution of machinery for human labour is often very injurious to the interests of the class of labourers." (quoted in Mokyr et al, 2015: 33). Marx went further by arguing that "the machine can only be employed profitably, if it … is the product of far fewer men than it replaces" (quoted in Vivarelli, 2014: 127). Mokyr et al. (2015) demonstrate that the 'technology anxiety' reflected in these statements has emerged repeatedly – mainly at times of rapid technological change and/or deep economic recessions (Mokyr et al., 2015). At other times, both economists and policy-makers have taken the view that job losses caused by technological change are temporary and would be reversed as a wide range of compensation mechanisms trigger new demand for labour.

Yet the multiplicity of the compensation mechanisms (e.g., occupational reallocation, lower product prices, output expansion, higher investment, etc.) has made it difficult to derive non-contingent conclusions. As Vivarelli (2014) has indicated, the compensation mechanisms require strict assumptions, overlook the secondary adverse demand effects that may result from falling wages, and may not all work in tandem. Therefore, Vivarelli (2014: 121) argue that "…economic theory does not have a clear-cut answer regarding the employment effect of innovation." Therefore, attention should "… focus on aggregate, sectoral, and microeconomic empirical analyses that take into account the different forms of technical change … the various compensation mechanisms and the possible hindrances they face."

The call for empirical research is justified, but the empirical evidence has also proved inconclusive. Even though a negative relationship between technological innovation and employment cannot be established, the positive relationship tend to be reported when R&D and/or product innovation [1] are used as proxies for technological change and when the estimations are based on high-tech industry/firm data (Vivarelli, 2014). In contrast, process innovation is reported to have a negative effect on employment and the adverse effect may be exacerbated as trade openness increases (Spiezia et al., 2002; Piva, 2003; Pianta, 2004). A third conclusion is that national innovation systems affect not only the countries' technological opportunities and innovation capabilities, but also the effects of the resulting technological change on employment (see also, Hall et al, 2007). Fourth, the structure of labour and product markets also matter: labour market flexibility and higher levels of product-market competition are usually associated with positive or less adverse innovation effects on employment (See also Benavente and Rodolfo, 2008). Finally, technological innovation may be skill-biased, with the implication that job creation for skilled labour may be at the expense of job destruction for unskilled labour (Berman et al., 1998; Machin, 2001).

Chennells and van Reenen (2002) draw attention to methodological issues as additional sources of heterogeneity in the evidence base. For example, a positive relationship between technological innovation and employment is more likely to be reported in primary studies based on cross-section data. However, estimates based on cross-section data may be biased due to correlated fixed effects. Furthermore, the choice of technological innovation may be endogenous to changes in skill supply or changes in labour-market institutions. The use of time-differenced data may eliminate the fixed effects, but it may also exacerbate the measurement problems as time-differencing requires the assumption that investment in innovation has a constant weight over its estimated life. The measurement error introduced by time-differencing is known to cause downward bias in estimated parameters and the latter may be exacerbated when differencing is based on short time periods such as subsequent years (Draca et al., 2007).

As technological change is un-observable, Chennells and van Reenen (2002) also draw attention to measurement issues that arise when researchers use various innovation proxies such as research and development (R&D) investment, intellectual property assets (IPAs) such as patents and trademarks, investment in information and telecommunications technology (ICT), or knowledge spillovers captured by knowledge capital pools at the industry, regional or national levels. Whereas R&D investment has the advantage of being measured by a comparable unit of account (money), the effect-size estimates based on this measure may be biased due to existence of spillover effects. On the other hand, knowledge spillovers may allow for capturing technology diffusion but they are difficult to measure and the lag-structure in the relationship between spillovers and employment is not known.[2]

The brief summary above indicates that the effect of technological innovation on total employment (employment of skilled, mix-skills and unskilled labour) is likely to be positive but highly heterogeneous. Of the sources of heterogeneity, product innovation is more likely to be associated with positive employment effects compared to process innovation. Secondly, the overall positive effect may be at the expense of a negative effect on the demand for unskilled labour. Third, the reported effect-size estimates are vulnerable to imperfections in the measurement of technological innovation as a proxy for the un-unobservable technological change. Finally, the effect-size estimates are likely to be contaminated with biases due to correlated fixed effects or endogeneity in the relationship between technological innovation and employment.

Although the existing reviews provide informative and valuable insights, they leave a number of issues unresolved. One set of issues arises from heterogeneity and the risk of publication selection bias in the evidence base. Whilst heterogeneity limits the extent to which summary measures can be generalised into other contexts, publication selection bias leads to incorrect inference. If exists, selection bias leads to truncated samples that, in turn, leads to distorted averages and confidence intervals for effect-size estimates (Doucouliagos and Laroche, 2009). Hence summary measures such as mean/median or vote-counting exercises cannot be relied upon for correct inference. Secondly, the existing reviews acknowledge the sources of heterogeneity but provide no quantitative estimates either for the level of heterogenenity itself or for the extent to which it can be explained by study characteristics. Third, the existing reviews do not address between- and within-study data dependence, which is a major concern in synthesizing the evidence based on observational data. Finally, and in spite of the theoretical predictions about the mediating roles of the labour-market flexibility and product-market competition, the existing reviews do not provide a systematic evaluation of how estimates based on data from different OECD countries relate to labour- and product-market institutions in those countries. In the paragraphs below we will try to address these gaps in the knowledge base by drawing on bivariate and multivariate meta-regression techniques.

3. The derived labour demand model (DLDM)

This meta-analysis is based on primary studies that draw on various DLDM specifications and utilize firm or industry data for estimation. Following Van Reenen (1997) and Chennells and van Reenen (2002), the *industry-level* production function with constant elasticity of substitution between capital and labour can be written as:

$$Q = T[\,(AL)^{(\sigma-1)/\sigma} + (BK)^{(\sigma-1)/\sigma}\,]^{\sigma/(1-\sigma)} \tag{1}$$

Here, Y is output, L is employment, and K is capital stock. Of the technology parameters, T, A and B represent Hicks-neutral, Harrod-neutral and Solow-neutral technological change, respectively. Hicks-neutral technology leaves the relative factor shares constant for a given capital-output ratio (*K/L*) ratio. Harrod-neutral technology is labour-augmenting (i.e., it leaves the relative factor shares constant at any capital-output ratio). Solow-neutral technology is capital-augmenting (i.e., it leaves relative factor shares constant at any labour-output ratio). Finally, σ is the non-unitary constant elasticity of substitution between capital and labour.

Industry profits are maximised in accordance with (2) below, where P is price level, W is nominal wage and R is cost of capital.

$$\pi = \underset{L,K}{max}(PQ - WL - RK) = \underset{L,K}{max}\{PT[(AL)^{(\sigma-1)/\sigma} + (BK)^{(\sigma-1)/\sigma}] - WL - RK\} \tag{2}$$

Taking derivatives with respect to labour (*L*) and assuming capital (*K*) is constant, we can solve for the level of employment that satisfies the first-order condition for profit-maximisation:

$$logL = logQ - \sigma\log(W/P) + (\sigma - 1)logA \tag{3}$$

From (3), the elasticity of employment with respect to labour-augmenting Harrod-neutral technology is:

$$\frac{\partial logL}{\partial logA} = \frac{\partial logQ}{\partial logA} + \sigma - 1 = \left(\frac{\partial logQ}{\partial logP}\right)\left(\frac{\partial logP}{\partial \log(MC)}\right)\left(\frac{\partial \log(MC)}{\partial logA}\right) + \sigma - 1 \tag{4a}$$

In the second part of the equality on the right, the elasticity of output with respect to labour-augmenting technology $\left(\frac{\partial logQ}{\partial logA}\right)$ is decomposed into 3 components: the price elasticity of the demand for the industry's output $\left(\frac{\partial logQ}{\partial logP}\right)$; the price-cost margin or market power $\left(\frac{\partial logP}{\partial \log(MC)}\right)$

where *MC* is the marginal cost; and the 'size' of the innovation as measured by its effect on marginal cost $\left(\frac{\partial \log(MC)}{\partial logA}\right)$. Denoting these terms with η_p, μ, and θ respectively, the elasticity of employment with respect to technology can be written briefly as:

$$\frac{\partial logL}{\partial logA} = \eta_p \mu\theta + \sigma - 1 \qquad (4b)$$

Assuming perfect competition ($\mu = 1$), the effect of technological innovation on employment will be positive if the price elasticity of product demand (η_p) and the size of innovation (θ) are large enough to counter the effect of substituting capital for labour at rate greater than 1. If product-market competition is imperfect ($\mu > 1$) and the firm shares the monopoly rents with labour via higher wages, the effect on employment will be negative as higher product prices lead to lower equilibrium output. If $\mu > 1$ and rents are shared with labour through a mixture of labour hoarding and wage increases, the effect will depend on the mixture that results from bargaining with the unions.

If we allow for capital to vary too, we can substitute for output (Q) in (3) using the capital stock (K) and the cost of capital (R). Then the DLDM can be written as follows:

$$logL = (\sigma - 1)\log(A/B) - \sigma\log(W/P) + logK + \sigma logR \qquad (5)$$

Finally, replacing the unobserved technology variables (A and B) with an appropriate measure of innovation, and assuming that the cost of capital is constant across industries but varies over time, the stochastic version of the DLDM can be written as:

$$logL_{it} = \gamma\log(Tech\_Innov)_{it} + \beta_1\log(W/P)_{it} + \beta_2 logK_{it} + \tau_t + \varepsilon_{it} \qquad (6)$$

where i is industry; τ_t is a set of time dummies that capture the cost of capital over time; ε_{it} is a white noise error term; β_1 is the elasticity of substitution between capital and labour in response to change in real wages; and *Tech_Innov* is an technological innovation measure (e.g., R&D intensity, patent or trade-mark counts, ICT, knowledge spillovers, etc.) that proxies for technological change.

Equation (6) is an industry-level DLDM, but it can also be used for estimations with firm-level data. The difference between industry- and firm-level estimates depends on whether or not technological diffusion is immediate and whether innovation by a given firm has a strong creative destruction effect on its competitors. If diffusion is slow and the creative destruction effect is strong (i.e, if innovation by a given firm renders the technology of its competitors obsolete at fast rates), the firm level estimates of the innovation-employment relationship can be expected to be larger than industry-level estimates. This is because in both cases the innovative firm will enjoy increased market share and hence its demand for labour will be higher for a given increase in innovation.

Drawing on Chennells and van Reenen (2002) and other reviews, the theoretical predictions from the model can be stated as follows:

1. The higher is the price elasticity of the demand for products/services (i.e., the lower is the level product-market regulation), the more likely it is to observe a positive relationship between technological innovation and employment.

2. The higher is the monopoly power of the firms in an industry, the less likely it is to observe a positive relationship between technological innovation and employment at the industry level. This is because firms with high market power will set prices above marginal costs and the level of output will be depressed.

3. However, the effect may be reversed if low market power (high competition) reduces job security and the workers demand higher wages as a compensation for reduced job security (Amable and Gati, 2004).

4. The higher is the rate of substitution of capital for labour, the more likely it is to observe a negative relationship between labour-augmenting technological change and employment.

5. The relationship between technological innovation and employment at the firm level is more likely to be positive if innovation does not diffuse immediately and the innovative firm increases its market share at the expense of its competitors.

6. Process innovation is associated with reduced demand for labour whereas product innovation is more likely to be associated with output expansion and hence higher demand for labour.

In the meta-regression analysis, we will include empirical studies that draw on a complete version of the DLDM specified in (3) or (5) above; an uncompensated labour-demand version where at least wages and technological innovation are controlled for (van Reenen, 1997); or any variant in between where capital or output is controlled for. Our aim is to establish where the balance of the evidence lies and test whether the predictions from the theoretical model and the conclusions reported in narrative reviews are supported by the existing evidence.

4. Systematic review rules and overview of the research field

Given the focus on primary studies that draw on an identifiable version of the DLDM, studies using a variant of the skill/wage share models or innovation-decomposition models are excluded. This is because skill/wage share models estimate the effect of innovation on the share of skilled (unskilled) labour in total wage bill or in total employment (Berman et al, 1994; 1998). On the other hand, the innovation-decomposition model of Harrison et al (2008) and Hall et al. (2008) allows for estimating the employment generated by the increase in the output of new products.

Although the three models allow for some inference about the innovation-employment relationship, the inference has different meanings across models. The DLDM allows for estimating the effect of one-unit change in technological innovation (however measured) on employment (which may or may not be broken down by skill). In skill/wage share models, the inference is about whether innovation is skill-biased or not – and not about change in the overall demand for employment.

The difference between the former models and the innovation-decomposition model is even more pronounced. Although the model allows for estimating the effect of innovation on employment growth, innovation (hence technological change) is measured differently. On the one hand, process innovation is usually proxied with a dummy variable that indicates whether the firm has introduced new processes or machinery. On the other hand, product innovation is measured through the rate of increase in the output of new products. This model structure poses two issues for meta-analysis. First, the effect technological innovation on employment is the sum of two coefficients on process and product innovation terms. The overall effect can be meta-analysed only if the covariance between the two coefficients were reported. Secondly, the coefficient on the product innovation term may capture not only the impact of technological innovation on employment but also the impact of marketing strategies on output, and through the latter, on employment.

In our literature search, study inclusion and exclusion decisions, and data extraction we follow the best-practice recommendations in Stanley et al. (2013). We have conducted title and abstract searches in eight electronic databases, using 21 key search terms and their extensions. The search was restricted to the period 1980-2013. The initial year is chosen on the basis of information from existing reviews, in which included empirical studies published before 1980 do not feature. The final year was determined by the start of the research project in the last quarter of 2013. Although we restricted the search to studies published in English, we did not impose any restriction on the country of origin for the data.

Two independent reviewers read the titles and abstracts of all studies captured in the electronic searches, using a range of first-stage inclusion criteria designed to ascertain if the study: (i) investigates the effect of technological innovation however measured on employment of skilled, unskilled or mix-skills labour; (ii) has an empirical dimension as opposed to a theoretical focus only; and (iii) is NOT a review only. In the second stage, again two independent reviewers read the full text of the included studies and used second-stage inclusion criteria. The latter are designed to ensure that the included study: (i) draws on a variant of the DLDM as opposed to wage/skill share models or innovation decomposition models; (ii) discusses and documents the data used; (iii) discusses and documents the estimation methodology in the light of theoretical and econometric literature; and (iv) reports 'effect-size' estimates together with standard errors or t-values and associated sample sizes. The process led to inclusion of 27 primary studies. The number eventually increased to 35 as a result of discovering new studies through snowballing and manual search. The latter was guided by information from existing reviews and our reading of the studies in the second stage of study selection.

We extracted all 'effect-size' estimates (567 in total) reported in 35 primary studies, coding each estimate with respect to four dimensions of the research field: (i) Publication type (journal article, working paper, book chapter, etc.) and date; (ii) model specification (full/uncompensated DLDM, differenced or level specification, inclusion of time/industry dummies, etc.); (iii) sample characteristics (country of origin for the data, firm/industry data, high/low/mixed levels of innovation intensity, panel/cross-section data, small/large firms, etc.); and (iv) estimation methods (OLS, Fixed effect or within estimators, and instrumental variable estimators such as GMM, 2SLS or 3SLS). We included all reported estimates to make full use of existing information and avoid the risk of sample selection that arises when reviewers use only some estimates 'preferred' on the basis of reviewer-set criteria.[3]

Given that the unit of measurement for the dependent and independent variables differs within and between studies, we calculate partial correlation coefficients (PCCs) to ensure that the estimates are comparable. The PCC and its standard errors are calculated in accordance with (7) below, where t_i and df_i are the t-values and degrees of freedom reported in the primary studies.

$$pcc_i = t_i / \sqrt{t_i^2 + df_i^2} \qquad \text{and} \qquad se\_pcc_i = \sqrt{(1 - pcc_i^2)/df_i} \tag{7}$$

The standard error of the PCC represents variations due to sampling error and its inverse is used as a weight in the meta-regression models in sections 5 and 6 below. The PCC itself measures the strength of the association between technological innovation and employment - after controlling for other determinants of the demand for labour in the DLDM. Doucouliagos (2011) suggests that a partial correlation that is less than ±0.07 can be regarded as small, even if it is statistically significant. The partial correlation indicates strong association (large effect) if it is greater than ±0.33. A PCC in between indicates moderate effect.

Table A1 in the Appendix provides an overview of the included studies, with information on a range of study characteristics. The latter include publication type, country origin of data, unit of analysis (i.e., whether firm, industry or sector level data is used), number of firms/industries, first and last year of data, innovation type (whether process or product innovation), skill type (whether unskilled or skilled labour), estimation method, median of the estimates reported in the study, median t-value and number of reported estimates. The majority of the primary studies included in this meta-analysis are published journal articles (71%), followed by working papers (26%). 74% of the studies utilised firm-level data, 14% utilised industry-level data, and the remainder utilised sector-level data.

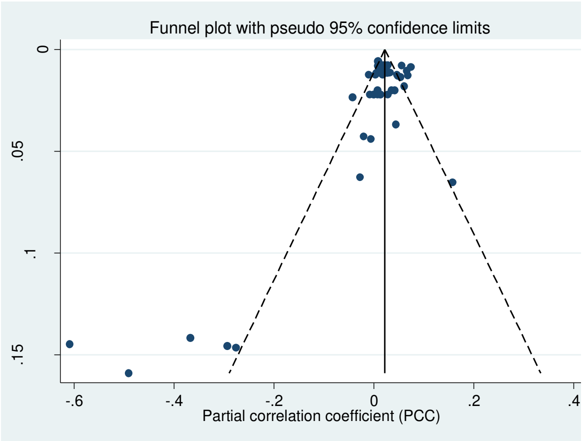
The number of estimates reported by each primary studies varies between 2 and 105. Median values of the within-study estimates are all positive except for three studies (Piva and Vivarelli, 2004a; Rottmann and Ruschinski, 1998; Yochum and Rhiel, 1990), and the median estimate and t-value for all reported effect sizes are 0.036 and 1.850, respectively. The overview of the evidence base points to positive and significant median estimates, but the latter vary considerably between studies. The firm-level median estimates vary between -0.017 (Piva and Vivarelli, 2004a) and 0.155 (Westermann and Schaeffer, 2001). At the industry level, the estimates vary between -0.550 (Yochum and Rhiel, 1990) and 0.257 (Berndt et al., 1992). Finally, the sector-level estimates vary between 0.062 (Luchesse and Pianta, 2012) and 0.242 (Mastrostefano and Pianta, 2009).

The funnel graphs in Figure 1 provide more information about the extent of heterogeneity and the risk of publication selection bias in the evidence base. The graphs are based on six evidence pools, distinguished by different combinations of innovation and skill types for which evidence exists: (1) process innovation and mixed-skills labour; (2) product innovation and mixed-skills labour; (3) undifferentiated innovation and skilled labour; (4) undifferentiated innovation and unskilled labour; (5) undifferentiated innovation and mixed-skills labour; and (6) full sample.

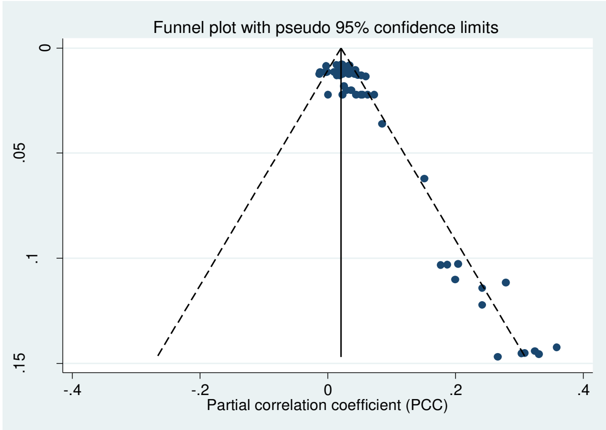
The mean-effect (represented by the vertical line) is positive in all evidence pools, with the exception of evidence pool (4) that reflect the estimates for unskilled labour demand. The distribution of the estimates around the vertical line indicates a moderate positive selection bias in graphs (3) to (6). This is evident from the relatively larger number of estimates above the mean compared to those below the mean. The two exceptions are evidence pools (1) and (2). In evidence pool (1), we observe a *strong negative* selection bias with respect to process innovation, whereas in (2) we observe *strong positive* selection bias with respect to product innovation. These visual indicators will be tested formally through meta-regression below. If confirmed, they indicate that summary measures or vote-counting results cannot be relied upon for correct inference about the effects of technological innovation on employment.

Moreover, a significant number of estimates are beyond the 95% pseudo confidence intervals – indicating heterogeneity that cannot be explained by sampling errors (Sterne and Harbord, 2004). Using the random-effect meta-regression estimator proposed by Harbord and Higgins (2008), we find that residual heterogeneity that cannot be explained by sampling differences is excessive (75% and over) in three evidence pools (3, 5 6), but it is moderate or low in evidence pools (1, 2 and 4).

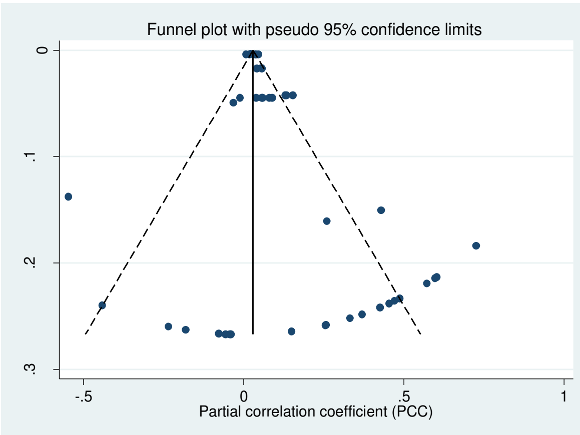
Figure 1: Funnel plots: potential selection bias and heterogeneity[4]



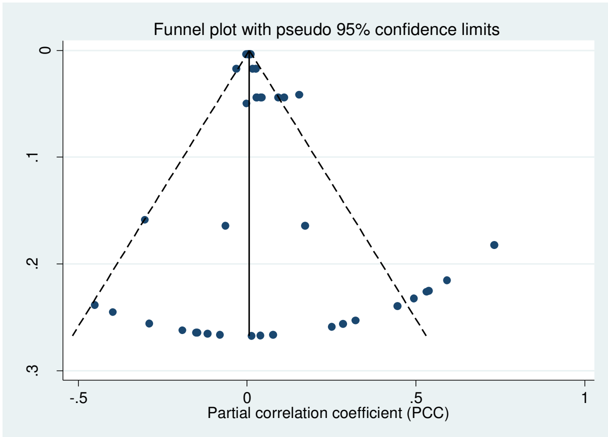
1. Process innovation and mixed-skills labour.
Heterogeneity: 68%



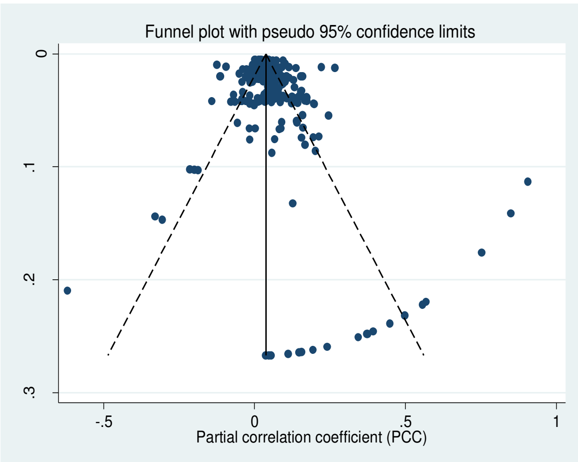
2. Product innovation and mixed-skills labour
Heterogeneity: 29%



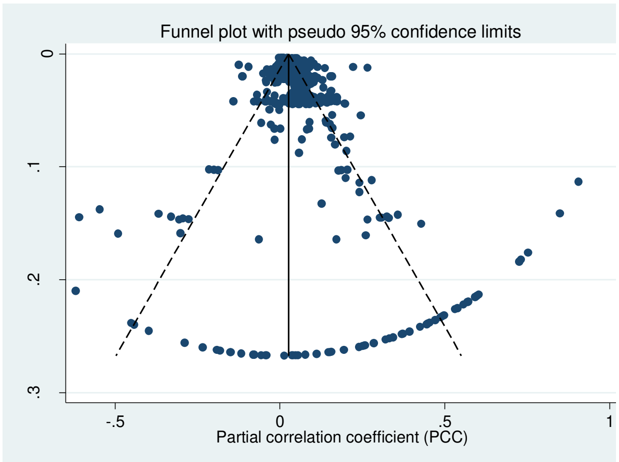
3. Undifferentiated innovation and skilled labour
Heterogeneity: 75%



4. Undifferentiated innovation and unskilled labour
Heterogeneity: 59%



5. Undifferentiated innovation and mixed-skills
Heterogeneity: 88%



6. Full sample
Heterogeneity: 85%

5. Why hierarchical meta-regression?

Given these indicators heterogeneity, summary measures (particularly those based on evidence clusters with high-heterogeneity) cannot be generalised to other contexts. In addition, selection bias can potentially lead to incorrect inference in narrative reviews. That is why we conduct meta-regression analysis to quantify the sources of heterogeneity and take account of selection bias.

Our methodology is informed by Stanley (2005, 2008) and Stanley and Doucouliagos (2012). The underpinning theoretical framework is that of Egger et al. (1997), who postulate that researchers search across model specifications, econometric techniques and data measures to find sufficiently large (hence statistically-significant) effect-size estimates. This theoretical framework implies that reported estimates are correlated with their standard errors. Denoting the effect size with e_i and the standard error with SE_i, and assuming that the error term (u_i) is independently and identically distributed (i.i.d.), the selection process can be modelled as a bivariate meta-regression model in (8):

$$pcc_i = \beta + \alpha se\_pcc_i + u_i \tag{8}$$

However, model (8) raises four estimation issues. First, the model is heteroskedastic because effect-size estimates have widely-different standard errors. To address this issue, we estimate a weighted least squares (WLS) version (9), where precision ($1/SE_i$) is used as weight (Stanley, 2008; Stanley and Doucouliagos, 2012):

$$t_i = \beta\left({}^{1}/{}_{se\_pcc_i}\right) + \alpha + v_i \tag{9}$$

Here t_i is the t-value associated with the reported estimate and the error term v_i is the error term in (8) weighted by precision. OLS estimation of (9) yields minimum-variance linear unbiased estimates if the Gauss-Markov conditions are satisfied. Testing for $\alpha = 0$ is a test for publication selection bias or funnel asymmetry test (FAT), whereas testing for $\beta = 0$ is a 'genuine effect' test or precision-effect test (PET) after controlling for selection bias. The selection bias is considered as *substantial* if $|\alpha| \geq 1$ or as *severe* if $|\alpha| \geq 2$ (Doucouliagos and

Stanley, 2009; 2012). Testing for selection bias is justified given the evidence about its prevalence in both social-scientific and medical research (Card and Krueger, 1995; Dickersin and Min, 1993; Ioannidis, 2005; and Simmons et al., 2011).[5]

The second issue is about which estimator is better-suited for the data at hand. Most meta-analysis studies tend to estimate (9) with ordinary least squares (OLS). However, OLS estimates from (9) would be biased in the presence of *data dependence*, which arises when primary studies using a particular dataset report multiple estimates or when different studies use the same dataset at overlapping time intervals (Doucouliagos and Laroche, 2009). Data dependence may be an issue here as several studies make use datasets from the same country of origin several times – albeit at different time periods. (See Tables A1 in the Appendix).

Data dependence can be taken into account by: (i) obtaining bootstrapped standard errors; (ii) conducting clustered data analysis; and (iii) using hierarchical models (Doucouliagos and Laroche, 2009). The first two methods correct the standard errors for within-study dependence. Hierarchical models (HM), however, allow for heteroscedasticity-robust standard and take account of both within-study and between-study dependence explicitly. An added feature is that HMs allow for a range of likelihood ratio (LR) tests to choose between least-square and HM estimators and between the latter themselves with respect to how dependence should be modelled. Therefore, we estimate model (9) as a HM – provided that the choice is justified by LR tests. Finally, we model data dependence by allowing for between-cluster random variation in reported estimates, which may be due to cluster-specific intercepts and/or slopes (Demidenko, 2004; McCulloch et al., 2008).

We nest the primary-study estimates within six evidence pools that correspond to unique pairs of innovation and skill types, which constitute the higher-level clusters of the HM.[6] The lower-level clusters consists of primary studies reporting evidence within any of the higher-level clusters. We also allowed for reported estimates to vary between clusters and studies either because they share a common intercept or a common intercept and slope at the study level. The choice between OLS and HM and between different specifications for the latter is based on likelihood ratio (LR) tests, with the null hypothesis that the compared model is nested within the preferred HM.[7]

The random-intercept-only and random-intercept-and-slope versions of the HM can be stated as follows:

$$t_{ijk} = \alpha + \beta\left({}^{1}/_{pcc\_se_{ijk}}\right) + v_{0j} + v_{0k} + \varepsilon_{ijk} \qquad (10a)$$

$$t_{ijk} = \alpha + \beta\left({}^{1}/_{se\_pcc_{ijk}}\right) + v_{0j} + v_{0k} + v_{1k}\left({}^{1}/_{se\_pcc_{ijk}}\right) + u_{ijk} \qquad (10b)$$

Here, subscripts *i* and *j* and *k* refer to individual estimates (PCC), analytic clusters, and primary studies, respeticley; and ε_{ijk} and u_{ijk} are normally distributed error terms with zero mean and fixed variance. The random effects v_{0j}, v_{0k} and v_{1k} are not estimated directly, but their variances are. Finally, parameters α and β are as defined above and estimated with maximum likelihood (ML). The choice between the random-intercept-only (10a) and random-intercepts-and-slopes (10b) models will be guided by LR tests.

The third issue in estimating the Egger model is that the relationship between primary-study estimates and their standard errors may be non-linear. Indeed, Moreno et al. (2009) provide evidence that a quadratic specification is superior if 'genuine effect' exists beyond selection bias, i.e., if the PET in (9), (10a) or (10b) rejects the null hypothesis of zero effect. Then, the correct specification is obtained by weighting both sides of (8) with precision-squared instead of precision. This inverse-variance weighting is referred to as precision-effect estimation corrected for standard errors (PEESE).

The random-intercept-only and random-intercept-and-slope versions of the hierarchical PEESE models are given below in (11a) and (11b) respectively:

$$t_{ijk} = \alpha se\_pcc_{ijk} + \beta\left({}^{1}/_{se\_pcc_{ijk}}\right) + v_{0j} + v_{0k} + \lambda_{ijk} \qquad (11a)$$

$$t_{ijk} = \alpha se\_pcc_{ijk} + \beta\left({}^{1}/_{se\_pcc_{ij}k}\right) + v_{0j} + v_{0k} + v_{1k}\left({}^{1}/_{se\_pcc_{ijk}}\right) + w_{ijk} \qquad (11b)$$

All subscripts, random effects, error terms and parameters are as defined above.

The final issue is that some studies report disproportionately large numbers of estimates compared to the rest. For example, four studies (van Reenen, 1997; Berndt et al., 1992; Lachenmaier and Rottmann, 2011; and Yang and Lin, 2008) account for 43% of the total estimates in the evidence pool. Even though the HM takes account of between- and within-study dependence, the sheer number of estimates reported by such studies may dominate the informational content of the evidence base and the meta-regression estimates. Therefore, we estimate the bivariate and multi-variate meta-regression models by also weighting the primary-study estimates with the inverse of the number of estimates reported by each study. This weighting scheme ensures that the weight of each study in the sample is equal to one.

The 'average' employment effect in the bivariate meta-regression (β) is estimated after controlling for selection bias. This is more reliable than other summary measures that do not account for selection bias; but its out-of-sample generalizability may still be limited due to excessive heterogeneity. Therefore, we obtain quantitative measures of heterogeneity using a random-effect meta-regression model proposed by Harbord and Higgins (2008).

Then we verify the sources of heterogeneity by augmenting (10a) or (10b) with a range of dummy variables (*Z*) that capture the dimensions of the research field. The random-intercepts-only and random-slopes-and-intercepts versions of the multivariate meta-regression model (MVMRM) are given in (12a) and (12b), respectively:

$$t_{ijk} = \alpha + \beta(1/pcc\_se_{ijk}) + \textstyle\sum_m \theta_m Z_m(1/se\_pcc_{ijk}) + v_{0j} + v_{0k} + \epsilon_{ijk} \qquad (12a)$$

$$t_{ijk} = \alpha + \beta(1/pcc_{se_{ijk}}) + \textstyle\sum_m \theta_m Z_m(1/se\_pcc_{ijk}) + v_{0j} + v_{0k} + v_{1k}(1/pcc\_se_{ijk}) + \mu_{ij} \qquad (12b)$$

The *mx1* vector of covariates (Z_m) are defined in Table 2 below; and the corresponding summary statistics are in Table A2 in the Appendix.

We estimate the PET/FAT/PEESE models for 6 evidence clusters indicated above. Estimation results (Table 1) are reported together with model diagnostics such as LR test statistics, log-likelihood values for hierarchical and the comparator models, variance inflation factors, and levels of heterogeneity. The MVMRM is estimated with the full sample, controlling for skill and innovation types as additional sources of heterogeneity. To avoid multicollinearity and overfitting, we follow a general-to-specific model-estimation routine; and present the general model results in Table A4 in the Appendix. The specific model is obtained by omitting the most insignificant covariates (those with the largest p-value) one at a time until all remaining covariates are statistically significant. Results from the specific model are presented in Table 2 in the main text, with further robustness checks with sampling weights in Table A5 in the Appendix.

6. Hierarchical meta-regression results

Table 1 consists of two panels. In panel A, we present the PET/FAT results for the level of selection bias (α) and for the 'effect-size' estimates (β) after controlling for selection bias. In panel B, we control for quadratic relationship between primary-study estimates and their standard errors if the PET/FAT results indicate significant effect beyond selection bias. Clusters 1 to 3 and cluster 6 are estimated with the appropriate HM estimators, whereas clusters 4 and 5 are estimated with OLS. LR test Chi-square and log-likelihood values justify the HM specification in four out six evidence pools. Finally, the robustness of the results to equal study weights is checked and the results, which are consistent with Table 1, are reported in Table A3 in the Appendix.

Results in Table 1 indicate that selection bias is moderate or insignificant in four evidence clusters (3a – 6a), but substantial in two clusters (1a and 2a).[8] In the latter, the bias is negative in 1a (process innovation and demand for mixed-skills labour) but positive in 2a (product innovation and demand for mixed-skills labour). This finding is interesting because it reveals selection bias in favour of the hypotheses informed by theory, which posit a negative effect on employment when technological change is measured with process innovation but a positive effect when the measure is product innovation (Katsoulacos, 1984; Harrison et al, 2008)[9]. This finding demonstrates that the matching conclusions reported in the narrative reviews may be misleading as they are based on highly-selected evidence.

Indeed incorrect inference is evident from the effect-size estimates (β) after controlling for selection bias. In the case of process innovation (column 3a), the average PCC is positive (0.037) and significant; and it remains significant in Panel B where we also control for quadratic relationship between primary-study estimates and their standard errors. In contrast, the average PCC for product innovation (column 2a) is insignificant! This is because selection bias in this evidence pool is the highest (α = 1.895), and unsurprisingly, the effect-size estimate becomes insignificant when selection bias is taken into account. Our conclusion is that, in the

Table 1: Technological innovation and employment: Effect-size estimates by innovation and skill type

	Panel A						Panel B				
	(1a)	(2a)	(3a)	(4a)	(5a)	(6a)	(1b)	(3b)	(4b)	(5b)	(6b)
Dependent variable: t-value	PET/FAT	PET/FAT	PET/FAT	PET/FAT	PET/FAT	PET/FAT	PEESE	PEESE	PEESE	PEESE	PEESE
β	0.037***	0.004	0.037***	0.025***	0.004*	0.025***	0.027***	0.039***	0.029***	0.007***	0.028***
	(0.011)	(0.004)	(0.007)	(0.006)	(0.002)	(0.006)	(0.007)	(0.006)	(0.005)	(0.002)	(0.005)
α	-1.405**	1.895***	0.210	0.937***	0.712**	0.461					
	(0.683)	(0.141)	(0.378)	(0.298)	(0.287)	(0.392)					
Std. error							-17.959***	-9.073***	3.531**	2.053	-1.187
							(3.407)	(1.997)	(1.380)	(1.289)	(2.090)
Observations	66	69	344	42	43	567	66	344	42	43	567
Studies	14	13	21	7	7	35	14	21	7	7	35
LR Test chi^2	11.927	0.719	22.317	231.916	2.765	20.420	36.239	47.714	372.342	10.633	33.207
P> chi^2	0.000	0.397	0.000	0.000	0.096	0.000	0.000	0.000	0.000	0.005	0.000
Log-likelihood (HM)	-107.500	-105.052	-825.242	-87.664	-79.054	-1289.587	-104.536	-825.802	-89.189	-81.236	-1290.414
Log-likelihood (Comp. model)	-130.225	-108.570	-848.867	N.A.	N.A.	-1348.930	-125.722	-849.161	N.A.	N.A.	-1359.481
Heterogeneity#	68%	29%	88%	75%	59%	85%	N.A.	N.A.	N.A.	N.A.	N.A.
Estimator	HM2-RIS	HM2-RI	HM2-RI	OLS	OLS	HM3-RIS	HM2-RIS	HM2-RI	OLS	OLS	HM3-RIS

Notes: Estimators: OLS (Ordinary least squares); HM2-RI (two-level hierarchical estimation with random intercepts only); HM2-RIS (two-level hierarchical estimation with random intercepts and slopes); HM3-RIS (three-level hierarchical estimation with random intercepts and slopes). Clusters: (1a) - process innovation and demand for mixed-skills labour; (2a) - product innovation and demand for mixed-skills labour; (3a) - undifferentiated innovation and demand for mixed-skills labour; (4a) - undifferentiated innovation and skilled labour; (5a) - undifferentiated innovation and demand for unskilled labour; (6a) - full sample. Robust standard errors (in brackets) are clustered at the study level. Observations with undue influence are excluded, using the DFBETA influence statistics. # indicates the proportion of residual between-study variation due to heterogeneity, as opposed to within-study sampling variability. *, **, *** indicate significance at 10%, 5% and 1%, respectively. N.A.: Not applicable.

presence of selection bias, summary measures or vote-counting results cannot be relied upon for correct evidence synthesis and inference.

The evidence in Table 1 also indicates that the consistent 'effect-size' estimates in Panel B are smaller than the benchmark of +0.07 suggested by Doucouliagos, (2011) and the earlier benchmark of 0.10 suggested by Cohen (1988). Given the confidence interval around the point estimates, the small but positive effect of technological innovation on the demand for labour may well be practically insignificant. This is irrespective of the evidence pool one focuses on. Furthermore, the evidence indicates a degree of skill bias as the positive effect on skilled labour demand (0.029 in column 4b) is larger than unskilled labour (0.007 in column 5b). However, it must also be noted that the effect on skilled labour demand is smaller than the effect on mixed-skills labour (0.039 in column 3b). This anomaly reflects the relatively higher level of positive selection in the evidence pool for skilled-labour demand (0.937 and significant in column 4a) compared to mixed-skills labour demand (0.210 but insignificant in column 3a). Hence, we conclude that more but less-selected evidence from DLDM estimations is required to make correct inference about the extent of skill-biased technical change.

Finally, the evidence in Table 1 also points out a trade-off between selection bias and residual heterogeneity. The latter is higher (between 75% - 88%) when selection bias is small or insignificant. On the other hand, when heterogeneity is low (between 29% - 68%), the selection bias is substantial (greater than one) in two out of three estimations. This evident trade-off indicates the need not only for less selected estimates, but also for better data quality and estimation methods that would reduce the level of residual heterogeneity between primary-study estimates. In what follows, we conduct multi-variate meta-regression analysis to shed light on the sources of heterogeneity listed in Table 2.

We organise the potential sources of heterogeneity in four categories: (i) publication type/date to verify if journal articles and work published after 2000 report systematically different estimates; (ii) variations in DLDM specification to verify if econometric specifications matter; (iii) characteristics of the samples used in primary studies to verify if data type, innovation type and measure, skill type, and country of origin for the firm/industry data are conducive to different estimates; and (iv) estimation methods to verify if controlling for endogeneity and time-invariant fixed effects lead to different estimates. The covariates within each category are dummy variables that take the value of 1 if the primary-study estimate is associated with the controlled characteristic and zero if it is associated with the excluded characteristic(s). They are all interacted with precision to capture their effects on ‘effect-size’ estimates reported in primary studies.

Table 2: Sources of heterogeneity and expected effect sign

Sources of variation in the evidence base	Controlled category	Reference category	Expected sign
A. Publication type and date			
Journal article, working paper, book chapter	Journal article	Working paper, book chapter	+/-
Publication date after 2000	Yes	Publications 2000 and before	+
B. Model specification			
Informed by theoretical DLDM	Yes	Ad hoc DLDM	n.a.
Dynamic specification	Yes	No	-
Time dummies included	Yes	No	+/-
Industry or sector dummies included	Yes	No	-
Wage included in model	Yes	No	n.a.
Output included in model	Yes	No	n.a.
Capital included in model	Yes	No	n.a.
Long-term effect (3 lags or more)	Yes	No	-
C. Sample characteristics			
Panel data	Yes	Cross-section, time-series	-
Industry or sector data	Yes	Firm	-
Innovation measure: R&D			+
Innovation measure: Intellectual property assets (IPAs)	Yes	No	n.a.
Innovation measure: ITC	Yes	No	+/-
Innovation measures: R&D+IPA	Yes	No	n.a.
Innovation type: Process	Yes	No	-
Innovation type: Product	Yes	No	+
Newness of Innovation: First to industry or country	Yes	First to firm	+
Skill type: Unskilled labour	Yes	Mixed skills and skilled labour	-
Sector: Manufacturing	Yes	Other sectors	+/-
Country: Canada, France, Germany, Italy, Netherlands, Norway, Spain, Sweden, UK, USA, OECD Countries	Yes	Data from Non-OECD countries and other Country	n.a.
High innovation intensity	Yes	No	+
Firm size: Large	Yes	Small and mixed-size firms	+/-
D. Estimation method			
GMM	Yes	OLS and all others	-
Differenced / within	Yes	No	-

Coefficients on the covariates should be interpreted as follows: a positive (negative) and significant coefficient indicates that primary-study estimates characterised by the control dummy are larger (smaller) than those associated with the reference category. A non-significant coefficient indicates no systematic difference between the primary-study estimates associated with the controlled and reference categories. The expected signs on the coefficients are informed by conclusions reported in the narrative reviews discussed above and by our reading of the studies included in the meta-analysis.

Estimation results from the general model (Table A4 in the Appendix) indicate that the moderating variables reduce the residual heterogeneity only marginally – from 85% in the full-sample bivariate model in Table 1 to 79% in general MVMRM in Table A4. Another observation is that the specification of the DLDM, the measure for innovation (with the exception of ICT and intellectual property assets), the country of origin for the data (with the exception of Canada and the US) are insignificant in explaining heterogeneity. Finally, the results indicate that the effects of innovation types (insignificant in the case of process innovation, negative and significant in the case of product innovation) are the opposite of what the theory predicts. However, we do not use the general model findings as a basis for inference as the coefficient estimates may be unstable due to multicollinearity, with a VIF value of 13.41.

The specific-model estimation results, presented in Table 3, are obtained by dropping the covariates with the largest *p-value* one at a time until all remaining covariates are significant. Then precision is added to the model to verify if the sign/significance of the covariates remain stable. Finally, to account for heteroscedasticity, the specific model is estimated with two different types of heteroskedastic residual-error structures at the innovation-type level (column 2) and at the skill-type level (column 3). The preferred model is (3), given the lower magnitude of the log-likelihood value. We have also estimated model (3) by weighting the primary-study estimates with the inverse of the total number of estimates reported in each study (column 4).

We derive two sets of conclusions from the results in Table 3: (a) conclusions supported by highly-robust evidence if the sign/significance of the coefficient estimates is congruent across columns (3) and (4) – marked **bold**; and (b) conclusions supported by moderately-robust evidence if the coefficient estimates are significant only in column (3). Where relevant, we will compare our findings with theoretical predictions and narrative review findings discussed in section 3 above,

Table 3: Sources of heterogeneity: Specific model estimations

Dependent variable: t-value	(1)	(2)	(3)	(4)
Precision	0.004	0.003	0.006	0.005
	(0.013)	(0.013)	(0.012)	(0.013)
Publication date after 2000	0.023***	0.023**	**0.020*** **	**0.011** **
	(0.009)	(0.009)	**(0.007)**	**(0.006)**
Data type: Panel	-0.023**	-0.023**	-0.015*	-0.004
	(0.010)	(0.010)	(0.008)	(0.010)
Output included in model	-0.023***	-0.022***	**-0.024*** **	**-0.016* **
	(0.008)	(0.008)	**(0.008)**	**(0.009)**
Industry or sector data	0.039**	0.036**	0.030*	0.026
	(0.017)	(0.016)	(0.018)	(0.017)
Innovation measure: IPA	-0.010***	-0.009***	**-0.010*** **	**-0.009** **
	(0.003)	(0.003)	**(0.003)**	**(0.004)**
Innovation measure: ICT	0.114**	0.106**	0.116**	-0.020
	(0.049)	(0.042)	(0.054)	(0.109)
Skill type: Unskilled labour	-0.020***	-0.020***	**-0.019*** **	**-0.017*** **
	(0.005)	(0.005)	**(0.006)**	**(0.004)**
Sector: Manufacturing	0.043***	0.043***	**0.036*** **	**0.023** **
	(0.010)	(0.010)	**(0.009)**	**(0.011)**
Canada data	-0.048**	-0.049**	**-0.042* **	**-0.032** **
	(0.021)	(0.021)	**(0.023)**	**(0.013)**
UK data	0.022*	0.023*	0.016	0.011
	(0.013)	(0.013)	(0.013)	(0.018)
US data	0.075***	0.075***	**0.063*** **	**0.058*** **
	(0.014)	(0.014)	**(0.014)**	**(0.021)**
OECD countries data	0.016***	0.015***	**0.015** **	**0.012*** **
	(0.005)	(0.005)	**(0.006)**	**(0.004)**
High innovation intensity	-0.035***	-0.035***	**-0.027*** **	**-0.029** **
	(0.006)	(0.006)	**(0.005)**	**(0.013)**
Long-term effect (3 years or more)	-0.016***	-0.016***	**-0.017*** **	**-0.026*** **
	(0.006)	(0.006)	**(0.004)**	**(0.005)**
Differenced / within estimation	-0.012***	-0.011***	**-0.011*** **	**-0.009* **
	(0.003)	(0.003)	**(0.003)**	**(0.005)**
Constant	-0.151	-0.138	-0.037	0.599
	(0.314)	(0.307)	(0.329)	(0.483)
Observations	567	567	567	567
Studies	35	35	35	35
LR Test chi^2	215.618	216.082	216.077	4319.460
P> chi^2	0.000	0.000	0.000	0.000
Log-likelihood (HM)	-1221.714	-1219.098	-1176.717	-80.321
Log-likelihood (Comp. model)	-1252.818	N.A.	N.A.	N.A.
VIF	8.05	8.05	8.05	8.05
Heterogeneity[#]	80%	80%	80%	80%
Estimation	HM3-RI	HM3-RI	HM3-RI	HM3-RI

Notes: HM3-RI indicates three-level hierarchical model with random intercepts. # indicates the proportion of residual between-study variation due to heterogeneity, as opposed to within-study sampling variability. Homoscedastic residual-error structures (column 1); followed by heteroskedastic residual-error structures by skill type (column 2) and Heteroskedastic residual-error structures by innovation type (column 3). *, **, *** indicate significance at 10%, 5% and 1%, respectively. N.A.: mot available with heteroskedastic error structure.

Our findings supported by highly-robust evidence are in partial agreement only with two narrative review conclusions. First, the effect of technological innovation on unskilled labour demand is *smaller* compared to the effect on skilled or mixed-skills labour demand. This is also in line with the skill bias reported in skill/wage share literature. Our contribution here is to combine both bivariate and multivariate meta-regression results and reveal that the effect on unskilled labour demand is positive but too small to be practically significant. Secondly, we confirm the conclusion in Vivarelli (2014) and report that primary studies published after 2000 tend to report relatively *larger* employment-effect estimates compared to previous studies.

However, our strongly-robust findings are either incongruent with narrative review findings or shed new light on a number of moderating factors that they are unable to evaluate in a conclusive manner. For example, we find that estimates based on high-innovation or high-tech firm/industry data in primary studies are relatively *smaller* than those associated with the reference category. This is in contrast to the conclusion in Vivarelli (2014). Secondly, we find no systematic difference between estimates based on process and product innovation data as both are insignificant in the specific MVMRM and their effects in the bivariate meta-regression are the opposite of the consensus view. Third, we find that the effect of technological innovation on labour demand is relatively *smaller* in the long run. This is in contrast to theoretical predictions that worker displacement in the short run may be reversed as compensation mechanisms trigger new demand for labour in the long run. However, this findings is in line with van Reenen (1997) who reports a declining effect in the long run – and with the creative destruction argument in Schumpeterian models where firm/indusrty innovation becomes obsolete over time as competitors introduce new technology (Aghion et al., 2014).

The strongly-robust findings that shed new light on the role of the moderating factors can be listed as follows:

1. Inclusion of output in the empirical model is associated with relatively *smaller* estimates compared to models that do not control for output. This is due to the fact that the firm optimisation implied by the underlying theoretical model may not hold every period. Our finding suggests that firm/industry employment may be responding not only to capital and labour costs but also to demand shocks, the exclusion of which may

lead to omitted variable bias. Indeed, when output is included in the model to correct for the omitted variable bias, the effect of technological innovation on employment is dampened.

2. Technological innovation measured by intellectual property assets (IPAs) is associated with *smaller* estimates compared to all other measures of innovation. This finding can bridge the evidence gap for two reasons. First, both primary studies and existing narrative reviews are silent about the effect of patents and trademarks on the demand for labour *relative* to other measures of technological innovation. Secondly, it may indicate that the number of IPAs may not reflect the true quality of the technological innovation they protect.

3. The effect on manufacturing employment is *larger* compared to non-manufacturing employment. This is despite the fact that the unit of analysis (firm versus industry) is distributed evenly between manufacturing and non-manufacturing sectors. This finding goes some way towards bridging the evidence gap as narrative reviews do not compare the employment effects in manufacturing and non-manufacturing sectors systematically. The finding also indicates that manufacturing firms/industries that innovate register *higher* demand for labour even though the share of manufacturing in total employment is declining in OECD and non-EOCD countries.

4. Effect-size estimates based on differenced data or fixed-effect estimators are *smaller* than those based on level data. This is in line with econometric theory, which clearly indicates that differenced or demeaned data is associated with attenuation bias. It also bridges the gap in evidence synthesis for this research field, as narrative reviews are silent on the trade-off between the need to correct for correlated fixed effects (which differencing and fixed0effect estimations do) and the attenuation bias in the latter when the variables are likely to be mismeasured.

5. There is evidence that the effect is *larger* in OECD compared to non-OECD countries and in the USA compared to all other countries. However, most country data is not associated with significantly different employment-effect estimates. This pattern does not conform to theoretical predictions that different levels of labour-

market flexibility and product-market competition may be associated with different innovation effects on employment. We will probe this issue further by estimating independent bivariate meta-regressions for individual countries.[10]

Results in Table 3 also allow for some conclusions based on moderately-robust evidence. Two of these are worth highlighting here. First, and in contrast to the conclusion in Vivarelli (2014), we find moderately-robust evidence that panel data is associated with relatively smaller effect-size estimates compared to cross-section and time-series data. Secondly, and in contrast to the suggestion in Chenneles and van Reenen (2002), we find that industry/sector level data is associated with *larger* estimates compared to firm-level data. These findings suggests that the informational content of the existing estimates may be hampered by data quality, for which econometrics can provide only partial solutions.[11]

The MVMRM results above are useful in accounting for a given source of variation, after controlling for all other sources for which evidence exists in the full sample. In what follows, we will provide further bivariate meta-regression estimates based on isolated evidence pools with a view to examine heterogeneity between countries and between different estimation methods. We undertake this exercise because country dummies and OLS/instrumental variable dummies have turned out to be insignificant in explaining heterogeneity in the overall evidence base. To save space, we present only the PEESE results that take account of the quadratic relationship between primary-study estimates and their standard errors.[12] In panel A of Table 4, we present the findings for some OECD countries; whereas in Panel B we present the findings for different estimation methods.

The results in Panel A indicate that the effect of technological innovation on employment do not vary in a monotonic fashion as the level of employment protection and product-market regulation changes in the six OECD countries for which more than 10 observations exist. The countries in Panel A are listed in *decreasing* order of employment protection legislation (EPL) and product market regulation (PMR) indices over the period 1998-2003 (OECD, 2004: 117; OECD, 2013: 29).[13] Yet, and in contrast to theoretical predictions, the country-specific effect of technological innovation on labour demand is relatively higher at both ends of the labour- and product-market rigidity indices constructed by the OECD. The effect is relatively larger in France and Sweden and in the US at the higher and lower ends respectively, compared to Germany, Italy and the UK in the middle.

Table 4: Indicators of heterogeneity through bivariate meta-regression estimates

Panel A: Heterogeneity by country (PEESE)

VARIABLES	(1b) France	(2b) Sweden	(3b) Germany	(4b) Italy	(5b) UK	(6b) US
β	0.070***	0.038**	0.029***	0.033***	0.029***	0.057***
	(0.019)	(0.014)	(0.007)	(0.008)	(0.004)	(0.017)
Std. error	-208.155	-154.473	-8.166	-16.023	5.781	-25.853***
	(167.880)	(157.674)	(12.339)	(20.498)	(9.020)	(8.644)
Observations	11	23	95	11	171	95
Number of groups	2	2	8	3	4	5
LR Test chi^2	41.263	6.648	15.921	22.966	44.333	20.529
P> chi^2	0.000	0.036	0.000	0.000	0.000	0.000
Log-likelihood (HM)	-26.321	-65.781	-182.228	-20.337	-310.868	-241.610
Log-likelihood (Comp. model)	-26.321	-66.013	-192.098	-20.337	-312.035	-250.223

Panel B: Heterogeneity by estimator (PEESE)

Dependent variable: t-value	(1b) OLS	(2b) GMM-All	(3b) GMM-Sys	(4b) 2SLS - 3SLS
β	0.035***	0.015***	0.014***	0.012***
	(0.004)	(0.004)	(0.005)	(0.004)
Std. error	-6.764*	51.931**	67.531**	56.347***
	(3.804)	(20.360)	(27.545)	(19.647)
Observations	297	111	64	120
Studies	21	8	6	11
LR Test chi^2	82.515	39.975	31.927	42.382
P> chi^2	0.000	0.000	0.000	0.000
Log-likelihood (HM)	-670.639	-202.834	-126.691	-221.097
Log-likelihood (Comp. model)	-681.760	-208.530	-129.786	-227.030

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All are estimated as two-level hierarchical models with random intercepts.

Our interpretation of this U-shaped pattern is that labour-market flexibility (one of the necessary conditions for job creation under technological innovation) may be high in countries with high and low EPL and PMR. In low EPL and PMR countries such as the US, labour-market flexibility follows from relatively easier hiring and firing aided by product-market competition. In the high EPL and PMR countries, on the other hand, labour-market flexibility may result from labour unions' agreements to wage flexibility in exchange for job security. This interpretation is in line with non-linear relationships reported by Calmfors and Driffill (1988) and Amable and Gatti (2004).

On the other hand, the results in Panel B confirms the expected upward bias in OLS estimations, which do not take account of endogeneity and correlated fixed effects. The difference between different instrumental variable estimators (GMM difference and system, GMM and 2SLS/3SLS) is quite small.

7. Conclusions

The analysis above demonstrates that meta-analysis is an effective method of synthesizing the evidence on the relationship between technological innovation and employment in general and by skill type. The method has enabled us to verify the qualitative conclusions put forward in existing reviews and to shed new light on the effects of moderating factors with respect to which they are either silent or inconclusive. We provide partial empirical support to two conclusions reported in prior reviews: (i) technological innovation increases the demand for skilled labour more than unskilled labour; and (ii) primary studies published after 2000 tend to report relatively larger 'effect-size' estimates. However, the empirical support to these conclusions is qualified in that the effect on skilled-labour demand is not larger (in fact it is smaller) than the effect on mixed-skills labour demand; and that the increased availability of panel data after 2000 is not necessarily the driver of larger estimates reported in more recent studies. If anything, estimates based on panel-data are relatively smaller than those based on cross-section or time-series data.

Our findings demonstrate that narrative review inferences may be incorrect when they draw on highly selected estimates. This was evident with respect to the effects of process and product innovation on mixed-skills labour demand. The selection bias in these evidence pools is in the direction of theoretical predictions. Furthermore, the level of selection is so high that the effect-size estimates based on process and product innovation turn out to be the opposite of what the narrative reviews report. The meta-analysis findings were also in contradiction to narrative review conclusions concerning the employment effects at the industry level (which turns out to be relatively larger than firm-level effect) and in high-innovation-intensity firms or industries (which turns out to be smaller than the excluded categories).

Finally, we have also shed new light on some moderating factors with respect to which the narrative reviews are either silent or inconclusive. Specifically, we found that: (i) the inclusion of output in the DLDM is associated with smaller innovation effects on employment; (ii) measuring technological innovation with R&D investment has no systematic effect on reported estimates, but the reported estimates are relatively smaller when innovation is measured with patents or trademarks and relatively larger when innovation is measured with investment in ICT; (iii) the effect of labour- and product-market regulation on the relationship between innovation and employment is more nuanced than the narrative review conclusions in that both high and low regulation countries may derive larger employment gains from innovation as labour unions trade off job security with wage flexibility in high-regulation countries; and (iv) a wide range of study characteristics explain only a small part of the heterogeneity in the evidence base.

Persistent heterogeneity and lack of conformity between meta-analysis findings and some theoretical predictions (particularly those related to process and product innovation) suggest that the informational content of the existing evidence may be constrained by data quality and modeling issues. Chennells and van Reenen (2002) provide an authoritative account of the difficulties involved in measuring innovation as a proxy for the unobservable technological change. Given those observations and our findings here, we suggest that investment in better-quality data is necessary to reduce the risk of mismeasurement. In our view, the transition to capitalisation of R&D expenditures is a step in the right direction because it will bring a common approach to R&D deflators and to the building of R&D capital stock from R&D investments. We also think that the R&D capital should be augmented with other intangible assets to create a measure of knowledge capital as suggested by Clayton et al. (2009).

Irrespective of the innovation measure adopted, knowledge diffusion remains a central issue for modeling. In the literature on R&D and productivity (e.g., Griliches, 1979; Hall et al., 2010; Hall, 2011) knowledge diffusion is modelled as a separate source of productivity because it is considered as complement rather than substitute to own knowledge capital. In the innovation and employment literature reviewed here none of the studies control for knowledge diffusion as a separate source of technological change. Although constructing the external knowledge pools poses additional measurement issues (Griliches, 1992), its exclusion from the theoretical and empirical models is rather ad hoc – and may be a source of omitted variable bias.

Another modeling issue is the lag structure in the relationship between the knowledge stock (both own and external knowledge stock) and employment. Fifty percent of the included studies use contemporaneous values of employment and innovation and 31% use between 1 and 3 lags for technological innovation, with the remaining 19% using more than 3 lags. The variation in the lag choices appears to be driven by empirical concerns rather than justifications on theoretical grounds. Therefore, we suggest better linkage with the literature on innovation and growth/productivity with a view to highlight not only the need for taking account of the lag structure in the relationship between technological innovation and employment, but also to acknowledge the difficulties in identifying the lag structure in firm-level as opposed to industry-level data.

A final modeling issue concerns the need for explicit incorporation of market power and creative destruction into the theoretical and empirical models. The Schumpeterian growth literature (Aghion et al., 2014) provides useful insights about the rationale for their inclusion in the growth models and their implications for growth. One way in which the Schumpeterian insights can be incorporated into the derived labour demand model is to allow for interactions between technological innovation and market power. Another way is to treat innovation intensity in the industry or the region not only as a source of knowledge spillovers but also as a source of creative destruction that makes the firm's or the industry's own technology obsolete.

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Appendix

Table A1: Technological innovation and employment: Overview of the evidence base

Study	Publication type	Country	Unit of analysis (count)	Data period	Innovation measure	Innovation type	Skill type	Estimation method	Median PCC	Median t-value	Reported estimates
Akcigit and Kerr (2012)	Working paper	US	Firm (n.a.)	1978-1992	IPA	Undifferentiated	Mixed	OLS	0.060	7.250	11
Araujo et al (2011)	Working paper	Non OECD	Firm (10810)	1997-2005	ICT, IPA	Undifferentiated	Unskilled	GMM	0.008	2.188	4
Berndt et al (1992)	Working paper	US	Industry (20)	1968-1986	ICT	Undifferentiated	Mixed	OLS	0.257	0.995	60
Blanchflower and Burgess (1995)	Working paper	OECD Mixed	Firm (889)	1989	ICT	Undifferentiated	Mixed	OLS	0.072	1.910	34
Blechinger et al (1998)	Working paper	Mixed	Firm (16374)	1992	R&D, Process innovation, Innovation count	Undifferentiated	Mixed	OLS	0.008	0.471	21
Bogliacino and Vivarelli (2012)	Journal article	OECD mixed	Sector (25)	1996-2005	R&D	Undifferentiated	Mixed	GLS	0.067	3.000	11
Bogliacino et al (2012)	Journal article	OECD mixed	Firm (677)	1990-2008	R&D	Undifferentiated	Mixed	LSDVC	0.042	2.300	5
Brouwer et al (1993)	Journal article	Netherlands	Firm (771)	1983-1988	R&D, Product innovation	Product	Mixed	OLS	0.008	0.225	2
Buerger et al (2012)	Journal article	Germany	Industry (270)	1999-2005	IPA, R&D	Undifferentiated	Mixed	LAD	0.022	0.540	24
Carlsson and Smedsaas (2007)	Journal article	Sweden	Firm (1516)	1989-1996	ICT	Undifferentiated	Mixed	FE WG	0.027	2.321	8
Coad and Rao (2010)	Journal article	Sweden	Firm (1577)	1973-2004	R&D	Undifferentiated	Mixed	LAD	0.035	3.560	15
Coad and Rao (2011)	Journal article	US	Firm (527)	1963-1998	R&D	Undifferentiated	Mixed	LSDVC	0.086	5.560	16
Conte and Vivarelli (2011)	Journal article	Non OECD	Industry (28)	1980-1991	ICT	Undifferentiated	Unskilled	GMM	0.034	2.004	6
Cozzarin (2004)	Journal article	Canada	Firm (5212)	1999	IPA, R&D, Innovation count	Undifferentiated	Mixed	GLS	0.009	0.678	9
Evangelista and Vezzani (2011)	Journal article	OECD mixed	Firm (57856)	2002-2004	Process Innovation	Process	Mixed	3SLS	0.008	1.453	3
Giuliodori and Stucchi (2010)	Working paper	Spain	Firm (2350)	1991-2005	Process and Product Innovation	Both product and process	Mixed	FE WG	0.021	2.333	28
Greenan and Guellec (2000)	Journal article	France	Firm (13126)	1985-1991	Process and Product Innovation, Innovation count	Undifferentiated	Mixed	OLS	0.051	3.885	10
Greenhalgh et al (2001)	Journal article	UK	Firm (151)	1987-1994	IPA, R&D	Undifferentiated	Mixed	FE WG	0.082	2.140	28
Greenhalgh et al (2011)	Working paper	UK	Firm (7038)	2000-2006	IPA	Undifferentiated	Mixed	FE WG	0.022	4.119	11
Lachenmaier and Rottmann (2006)	Working paper	Germany	Firm (4567)	1982-2003	Product and Process Innovation	Both product and process	Mixed	OLS	0.057	4.063	8
Lachenmaier and Rottmann (2011)	Journal article	Germany	Firm (690)	1982-2002	R&D, Process and Product Innovation	Undifferentiated	Mixed	GMM	0.019	1.633	40
Lucchese and Pianta (2012)	Journal article	OECD mixed	Sector (21)	1995-2007	Process and Product Innovation	Both product and process	Mixed	WLS	0.062	1.003	2
Mastrostefano and Pianta (2009)	Journal article	OECD mixed	Sector (10)	1994-2001	Product innovation	Product	Mixed	OLS	0.242	2.050	4

Osterman (1986)	Journal article	US	Industry (40)	1972-1978	ICT	Undifferentiated	Skilled	OLS	0.054	0.331	6
Pianta (2000)	Book chapter	OECD mixed	Sector (49)	1989-1993	R&D, Process and Product Innovation	Undifferentiated	Mixed	OLS	0.177	1.720	17
Piva and Vivarelli (2004a)	Journal article	Italy	Firm (488)	1989-1997	R&D	Undifferentiated	Skilled	SUR	-0.017	-0.335	2
Piva and Vivarelli (2004b)	Journal article	Italy	Firm (318)	1992-1997	R&D	Undifferentiated	Mixed	FE WG	0.019	0.775	6
Rottmann and Ruschinski (1998)	Journal article	Germany	Firm (1982)	1980-1992	Product and Process Innovation	Both product and process	Mixed	IV	-0.003	-0.252	4
Smolny (1998)	Journal article	Germany	Firm (2405)	1980-1992	Product and Process Innovation	Both product and process	Mixed	OLS	0.021	2.550	2
Smolny (2002)	Journal article	Germany	Firm (2405)	1980-1992	Product and Process Innovation	Both product and process	Mixed	OLS	0.029	2.800	2
Srour et al (2013)	Working paper	Turkey	Firm (17462)	1980-2001	R&D, IPA	Undifferentiated	Unskilled	OLS	0.011	3.024	12
van Reenen (1997)	Journal article	UK	Firm (598)	1977-1982	IPA, Innovation count, Product and Process Innovation	Undifferentiated	Mixed	OLS	0.027	1.251	105
Westermann and Schaefer (2001)	Journal article	Germany	Firm (450)	1981-1993	ICT	Undifferentiated	Mixed	OLS	0.155	2.779	12
Yang and Lin (2008)	Journal article	Non OECD	Firm (492)	1997-2003	IPA, R&D, Process and Product Innovation	Undifferentiated	Mixed	GMM	0.036	1.801	37
Yochum and Rhiel (1990)	Journal article	US	Industry (1)	1946-1983	Process Innovation	Process	Mixed	OLS	-0.550	-3.642	2
All									**0.036**	**1.850**	**567**

Table A2: Summary statistic for moderating variables

Moderating variables	Obs	Mean	Std Dev	Min	Max
Effect indicators					
PCC	567	0.058	0.152	-0.619	0.906
Standard error of PCC	567	0.051	0.072	0.004	0.267
Precision	567	60.282	56.107	3.742	284.462
Publication type and date					
Journal article	567	0.637	0.481	0	1
Publication date after 2000	567	0.536	0.499	0	1
Model specification					
Informed by theoretical DLDM	567	0.760	0.427	0	1
Dynamic specification allowed	567	0.295	0.456	0	1
Time dummies included	567	0.356	0.479	0	1
Industry or sector dummies	567	0.236	0.425	0	1
Wage included in labour demand model	567	0.515	0.500	0	1
Output included in labour demand model	567	0.598	0.491	0	1
Capital included in labour demand model	567	0.388	0.488	0	1
Long-term effect (3 lags or more)	567	0.127	0.333	0	1
Sample characteristics					
Data type: Panel	567	0.714	0.452	0	1
Industry or sector data	567	0.233	0.423	0	1
Innovation measure: R&D	567	0.217	0.413	0	1
Innovation measure: IPA	567	0.159	0.366	0	1
Innovation type: Process	567	0.118	0.323	0	1
Innovation type: Product	567	0.122	0.327	0	1
Innovation measure: ICT	567	0.122	0.327	0	1
Innovation measures: R&D + IPA	567	0.249	0.433	0	1
Newness of Innovation: First to country or industry	567	0.002	0.042	0	1
Skill type: Unskilled labour	567	0.076	0.265	0	1
Sector: Manufacturing	567	0.873	0.333	0	1
Canada data	567	0.016	0.125	0	1
France data	567	0.019	0.138	0	1
Germany data	567	0.168	0.374	0	1
Italy data	567	0.019	0.138	0	1
Netherlands data	567	0.004	0.059	0	1
Norway data	567	0.005	0.073	0	1
Spain data	567	0.053	0.224	0	1
Sweden data	567	0.041	0.197	0	1
UK data	567	0.302	0.459	0	1
US data	567	0.168	0.374	0	1
OECD Countries data	567	0.917	0.276	0	1
High innovation intensity	567	0.120	0.325	0	1
Firm size: Large	567	0.028	0.166	0	1
Estimation method					
GMM	567	0.196	0.397	0	1
Differenced/within estimators	567	0.713	0.453	0	1

Table A3: Robustness check 1: PET/FAT/PEESE results using sampling weights

	Panel A						Panel B				
	(1a)	(2a)	(3a)	(4a)	(5a)	(6a)	(1b)	(3b)	(4b)	(5b)	(6b)
Dependent variable: t-value	PET/FAT	PET/FAT	PET/FAT	PET/FAT	PET/FAT	PET/FAT	PEESE	PEESE	PEESE	PEESE	PEESE
β	0.032**	0.007	0.031***	0.026***	0.004*	0.016***	0.020***	0.035***	0.027***	0.005**	0.021***
	(0.014)	(0.005)	(0.008)	(0.003)	(0.002)	(0.006)	(0.006)	(0.007)	(0.003)	(0.002)	(0.006)
α	-1.686	1.979***	0.556	0.357	0.170	0.857**					
	(1.243)	(0.158)	(0.535)	(0.552)	(0.317)	(0.428)					
Std. error							-21.466***	5.228	2.242	0.558	0.886
							(2.871)	(4.866)	(1.470)	(1.640)	(5.593)
Observations	66	69	344	44	43	567	66	344	42	43	567
Studies	14	13	21	7	7	35	14	21	7	7	35

Notes: The bivariate meta-regression models are estimated with sampling weights to ensure that the weight of each study in the sample is equal to one. See notes under Table 1 in the main text for model diagnostics and description of the evidence clusters.

Table A4: Multivariate meta-regression results: General model

Dependent variable: t-value			
Precision	-0.014	Sector: Manufacturing	0.045***
	(0.023)		(0.012)
Publication type and date		Canada data	-0.053**
Journal article	0.005		(0.024)
	(0.008)	France data	0.003
Publication date after 2000	0.023*		(0.023)
	(0.012)	Germany data	-0.001
Model specification			(0.013)
Informed by theoretical DLDM	-0.003	Italy data	-0.002
	(0.010)		(0.017)
Dynamic model utilized	0.005	Netherlands data	-0.017
	(0.008)		(0.065)
Time dummies included	-0.007	Norway data	0.001
	(0.007)		(0.047)
Industry or sector dummies included	0.006	Spain data	-0.008
	(0.007)		(0.017)
Wage included in model	0.003	Sweden data	-0.016
	(0.006)		(0.012)
Output included in model	-0.016	UK data	0.022
	(0.014)		(0.019)
Capital included in model	0.001	US data	0.074***
	(0.008)		(0.020)
Long-term effect (3 lags or more)	-0.015**	OECD countries data	0.026*
	(0.006)		(0.015)
Sample characteristics		High innovation intensity	-0.034***
Data type: Panel	-0.014		(0.006)
	(0.016)	Firm size: Large	-0.012
Industry of sector data	0.035*		(0.013)
	(0.019)	**Estimation method**	
Innovation measure: R&D	-0.003	Estimator: GMM	-0.002
	(0.006)		(0.004)
Innovation measure: IPA	-0.012**	Differenced / within	-0.013***
	(0.005)		(0.004)
Innovation measure: ICT	0.098**	Constant	0.048
	(0.049)		(0.399)
Innovation measures: R&D + IPA	-0.005	Observations	567
	(0.006)	Studies	35
Innovation type: Process	-0.009	LR Test chi^2	242
	(0.007)	P> chi^2	0.000
Innovation type: Product	-0.013*	Log-likelihood (HM)	-1218.331
	(0.007)	Log-likelihood (Comp. model)	-1226.925
Innovation is first to industry or country	-0.044	VIF	13.41
	(0.030)	Heterogeneity[#]	79%
Skill type: Unskilled labour	-0.024***	Estimation	HM2-RI
	(0.004)		

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Notes

[1] See Katsoulacos (1984) for a detailed theoretical exposition of the difference between the employment effects of process and product innovation.

[2] See Griliches (1992) on the measurement of knowledge spillovers and the lag structure in the relationship between R&D spillovers and productivity.

[3] In the estimations, we exclude the outliers from estimation if they are found to have undue influence (i.e., if they are associated with a *dfbeta* statistic greater than one in magnitude). We also control for the effects of including multiple estimates from primary studies by weighting each estimate with the inverse of the total number of estimates reported in the study.

[4] The heterogeneity measure is a generalization of Cochran's Q and indicates the proportion of residual between-study variation due to heterogeneity, as opposed to within-study sampling variability (Harbord and Higgins, 2008). Higgins et al. (2003) suggest that heterogeneity is low if the measure is between 25%–50%, moderate if it is between 50%–75%, and high if over 75%.

[5] There is a mistaken presumption that the Egger et al. (1997) model makes the detection of publication selection bias almost inevitable because of the positive association between effect-size estimates and their standard errors (or because of the negative association between effect-size estimates and their precision). On the contrary, simulation results in Stanley (2008) indicate that the funnel asymmetry test based on Egger et al (1997) has low power - i.e., it tends to fail detecting publication selection when the latter actually exists.

[6] The evidence pools are the same as those that underpins the funnel graphs above.

[7] As a further check, we also compare the log-likelihood ratio for alternative estimators and for different HM specifications. A smaller log-likelihood value in magnitude provides additional evidence in favour of the estimator/specification at hand.

[8] The effect-size estimate after controlling for selection bias is preferable to alternative summary measures even if the selection bias is insignificant.

[9] Katsoulacos (1986: 12) reports that his theoretical results lend support to the "often quoted empirical observation . . . that product innovation is more likely to have a favourable employment effect than process innovation."

[10] This exercise will enable us to verify if the bivariate meta-regression results may be related to different levels of employment protection in OECD countries

[11] As Griliches and Mairesse (1995: 22) have noted in the context of R&D productivity literature, much of the work "has been guided … by what 'econometrics' as a technology might be able to do … rather than focusing on the more important but technically less tractable problems of data quality and model specification."

[12] PEESE estimates are presented only if the PET/FAT estimates indicate significant effect-size estimate beyond selection bias. PET/FAT estimates are available on request. Furthermore, we report country-specific estimates only if the number of observations based on a given country data is larger than 10.

[13] The ranking is based on 2003 data for labour-market flexibility and on 1998, 2003 and 2008 data for product-market competition.