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Unions, Innovation, and Technology Adoption: New insights from the cross-country evidence

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Unions, Innovation, and Technology Adoption:
New insights from the cross-country evidence

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Abstract

There is currently no consensus regarding the effect of unions on technology. We apply meta-regression analysis to the extant econometric studies and find that unions depress investment in new technology. However, this adverse effect has been declining over time and is moderated by country differences in industrial relations and regulations: The adverse effect appears to *increase* with labor market flexibility. Unions also have an adverse effect on technology adoption. The paper considers both the direct and indirect effects of unions and shows that their effect on technology is larger than their effect on profitability and physical capital. The size of the union effect on technology is compared to the effects of human capital, industry concentration, firm size, growth, profitability, and physical capital.

Keywords: unions, R&D, innovation, technology adoption, regulation, meta-regression analysis

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Unions, Innovation, and Technology Adoption: New insights from the cross-country evidence

> "... cross-country differences in the R&D impact of unions could represent either unsolved econometrics problems or genuine institutional differences between nations in union attitudes and ability to bargain. We suspect the latter is the main reason."
>
> Lommerud *et al.* (2006, p. 21)

> "At the very least the technological revolutions in the US would have been seriously impeded if the labor market environment would have been more like in Europe, namely with stronger unions and with more regulation."
>
> Alesina and Zeira (2006, p. 5)

1. Introduction

Intangible forms of capital, such as knowledge and R&D, are widely deemed to be significant drivers of economic growth and cross-country differences in incomes (Mankiw, Romer and Weil, 1992; Lederman and Maloney, 2003; OECD, 2008). Technology also shapes labor market outcomes, such as unemployment and the returns to skill. Consequently, much research has been directed to identifying the determinants of innovation and the adoption of new technology.

One important line of research is the effects of industrial relations and unions on inter-firm and inter-industry differences in innovation (Freeman and Medoff, 1984; Addison and Hirsch, 1989; and Hirsch 1991).[1] This literature currently consists of 38 econometric studies (see section 3 below) and a very large number of case studies. There is much ambiguity about the effects of unions on innovation and relatively little is known about their effects outside the US and the UK. Menezes-Filho, Ulph and Van Reenen (1998a, p. 929) note that there is: "still relatively little empirical work on the important issue of the effect of labor market

[1] In this paper we abstract from macroeconomic performance issues. See Flanagan (1999) and Storm and Naastepad (2009) for reviews of this parallel literature.

institutions on growth and R&D.” Unfortunately, the comparatively poor availability of unionization, R&D, and innovation data, means that the supply of new studies will be rather limited, making it imperative to draw as much information as possible from the extant evidence.

This paper offers the first quantitative research synthesis of the literature through a systematic review, or meta-regression analysis (MRA), of the evidence (Stanley, 2001; Hunter and Schmidt, 2004). Our MRA has three aims. First, we wish to provide a statistical integration of the existing empirical studies on the net impact of unions on innovation (hereafter the U-I effect) and the adoption of technology (U-A effect), with a view to quantify the magnitude of these associations: Are these effects negative, positive, or neutral? Is the size of the U-I and U-A effect of practical significance?[2] Our second aim is to test whether there are observable differences in the effects of unions between countries and over time. We are especially interested in exploring the extent to which the differences in findings can be explained by country differences in regulations: Is U-I larger when labor markets are flexible or regulated? What is the effect of trade liberalization? Our third aim is to identify the sources of differences in reported effects between studies. For example, how important are differences in econometric specification and the measurement of innovation and unionization?

Our MRA is not a substitute for the existing reviews. Rather it revisits the issues using a different, and statistically based, methodology. We draw inferences from two sets of empirical studies. First, we analyze the partial correlations and elasticities between unions and innovation (U-I) from 29 econometric studies that report 208 estimates. Second, we

[2] Our focus here is only on the effects of unions on technology rather than the reverse causality: Unions themselves might be affected by technological change.

analyze the marginal effects from nine probit studies that report 51 estimates of the U-A effect. The first group of studies analyzes the effects of unions on innovation (using measures of R&D or counts of the number of innovations), while the second group looks at the adoption of technological change. By drawing upon this diverse evidence, we are able to *comprehensively* analyze the effects of unions on technology and are also able to compare the union effect across industries, countries, and over time.

The paper is set out as follows. The theoretical background is reviewed briefly in section 2. The data used in the MRA are discussed in section 3. In section 4 we apply MRA to quantify the *direct* effects of unions on technology. Section 5 investigates the heterogeneity in the reported U-I and U-A effects. Section 6 explores the relative importance of unions by comparing their effect to six other determinants of innovation: Human capital, industry concentration, firm size, growth, profitability, and physical capital. We then use some of this information to estimate the *indirect* effects of unions on innovation. The paper is concluded in section 7.

2. Theoretical Considerations

Excellent reviews of the theoretical arguments can be found in Booth (1995), Menezes-Filho and Van Reenen (2003), and Hirsch (2007). Here we present only a summary of the main arguments.

There are several dimensions to the effects of unions on innovation and technology. Unions can affect the *level of investment* in R&D, which in turn can affect the rate of innovation. Unions can affect the *adoption* of technological change in the workplace and, hence, productivity growth. Unions can affect the firm's ability to *gain* fully from the benefits of new technology, and where projects vary in the degree to which they are vulnerable to rent appropriation, unions might affect the *type* of investment project undertaken (Schnabel and

Wagner, 1992b). Our focus in this paper is on the first two dimensions, as they have drawn most of the attention in the empirical literature.

As in many areas of labor economics and industrial relations, there are competing and contrasting views on the effect of unions on technology. Several perspectives can be identified.

A tax on capital and labor monopoly: Union wage demands can serve as a tax on labor, with an uncertain impact on investment (Denny and Nickell, 1991, 1992; Hirsch, 1991). On the one hand, they raise production costs reducing the optimal level of output and, hence, reducing capital requirements. Lower profits also make it more difficult to finance new investment. On the other hand, higher wages are an incentive to substitute labor with the relatively cheaper capital. There are also non-wage effects. For example, restrictive work practices and resistance to the introduction of new technology increase the cost of investing in both tangible and intangible assets. Unions can devote their monopoly power to rent-seeking, capturing some of the firm's quasi-rents from long lived investments. This discourages investment and decreases dynamic efficiency (see Grout, 1984; Hirsch and Link, 1984; Fitzroy and Kraft, 1990).

Collective Voices: Freeman and Medoff (1984) argued that unions have two faces – the labor monopoly and the collective voices aspect. Unions might be receptive to organizational change, creating a climate conducive to investment, and they may help to retain highly trained staff who can contribute to innovation. Unions may also enable firms to increase the speed of diffusion of technology and, hence, increase the firm's incentive to invest (see Menezes-Filho *et al*, 1998b). Higher levels of productivity (static efficiency) resulting from unionization might increase the attractiveness of investment. Hence, the *net* effect on investment in intangible assets might be positive.

Bargaining: The disincentive effects on investment may disappear with 'efficient' bargains, where unions bargain over wages and other aspects of the employment relationship, (see Menezes-Filho *et al*, 1998b). In a series of papers, Ulph and Ulph (1989, 1994 and 1998) focus on the strategic aspects of R&D rivalry between firms and argue that technology outcomes depend on the form of bargains (e.g. right-to-manage versus efficient bargaining) and union preferences for employment and wages. Depending on the competitive setting, it is possible that stronger unions cause firms to increase innovation. Tauman and Weiss (1987) show that unionized firms might adopt labor-saving technologies, especially if wages and technology are simultaneously determined and product demand is high.

In practice, firms usually have the prerogative to decide what technology to adopt, when to adopt it, the purpose to which it is to be used and who shall use it. Negotiations with unions often occur after these decisions have already been made.[3] Unions might be involved in negotiations prior to key technology decisions being made, but this in itself does not mean that technology choices will be affected: Union preferences need not prevail. In this case, the effect on technology is likely to be neutral. Unions might embrace technological change purely because they realize that they have no choice – resistance might be useless. On the other hand, even if there is no formal negotiation, it is possible that union preferences might shape management preferences. This would make it difficult to econometrically identify the effects of unions.

Are unions necessarily hostile to new technology? Union attitudes to technology are shaped by many factors, especially the structure and organization of unions,[4] their bargaining

[3] For example, firms in the US are not required to bargain over the introduction of new technology, but they are required to bargain over the effects of technological change (Abraham and Finzel, 1997).

[4] For example, industrial unions may find that technological change affects only a small fraction of their membership or that it affects different parts of the union in different ways (e.g. hurts some members but benefits many others) so that the union does not oppose the change. In contrast, craft unions maybe more exposed and resist technological change. This distinction is rarely made in the econometric studies.

strength, the form that bargaining takes, the nature of product market competition, and the nature of the technological change itself. Two major issues for unions are the effect of technology on employment (and, hence, union membership) and the pace of work.[5] Unions often seek to prevent or limit employment displacement. Technology's effect on employment depends, in part, on the elasticity of the demand for labor.[6] Events that reduce this elasticity might trigger union opposition, e.g. trade liberalization (Lommerud *et al.,* 2006).[7]

Dowrick and Spencer (1994, p. 50) argue that there is: "no systematic relationship between union bargaining strength and union attitudes to innovation. Rather, it is the structure of union organization and the underlying preferences toward wages and employment that is important." They also argue that the relationship might be non-monotonic, with high levels of innovation associated with firm-level unions and then again with peak union bodies that take a more global view, while industry unions might resist innovation (see also Haucap and Wey, 2004, for a similar story). Calabuig and Gonzalez-Maestre (2002) argue that higher levels of union centralization can stimulate innovation, especially if innovation is sufficiently drastic.

All these varying model predictions indicate that the net impact of unions is theoretically unclear. Hence, empirical investigations are essential. Unfortunately, econometric studies have produced a wide variation of results (reviewed below) and this variation is rather difficult to digest using the framework of a traditional review. Prior reviews of the empirical literature have in general concluded that U-I is negative in the US

[5] The effect on skills can also be of concern.

[6] Our focus here is at the firm and industry level. At the macroeconomic level, technology and new markets can create millions of new jobs. See Hornstein *et al.* (2007) on how the interaction between labor market institutions and technological change can shape the demand for labor.

[7] Lommerud *et al.,*(2006) argue that trade liberalization increases union opposition to technology when it decreases the elasticity of labor demand and when there exists a larger home market and a technological advantage.

but not necessarily elsewhere. According to Menezes-Filho, Ulph and Van Reenen (1998b, p. 46): “There is as much evidence of a positive correlation as of a negative one”.

Several moderating factors might explain the wide variation in empirical findings. One potential contextual factor is the environment in which unionization has been examined. The effect of unions may depend, at least in part, on the research setting (industry, country and time period) in which unions are investigated. Further, industrial relations systems and regulatory regimes vary from one country to another. Measurement differences are another methodological feature that might moderate U-I findings. There are several ways in which technological change and innovation can be measured, such as investment in R&D, the number of patents and innovations, and the number of employees involved.

It is possible that this wide range of operational definitions and contextual differences contributes to the variation in estimates of the effects of unions on innovation. In this paper we use meta-regression analysis to explore whether there are differences between industries, countries, labor market institutions, over time, and the level of aggregation (firms versus industries).

3. Data[8]

3.1 Search criteria

The first step in the meta-analysis was to identify the relevant studies. We conducted a comprehensive search for comparable empirical studies using numerous search engines, including EconLit and Google Scholar. We also pursued references cited in the empirical studies themselves, as well as the existing literature reviews. We restricted our search to the published literature (book chapters and journals) in the areas of economics, industrial organization, industrial relations, and management. We excluded unpublished dissertations,

[8] All the data used in this paper are available from the authors.

manuscripts and working papers. The search focused on studies published in English or French.[9] This extensive literature search identified more than 60 articles and book chapters. However, many of these studies were not appropriate for the MRA. We eliminated literature reviews and studies that did not provide original empirical analysis. We also eliminated several studies where unions were included as a control variable but where key results, such as a t-statistic or a coefficient and its standard error, or marginal effects were not reported (e.g. Chennels and Van Reenen, 1997). Those studies that involve the same author(s) and used the same data were combined together (e.g. Acs and Audretsch, 1987, 1988; Addison and Wagner, 1994a, 1994b; Schnabel and Wagner, 1992a, 1992b; and Menezes-Filho *et al.*, 1998a, 1998b). From this process of literature search and elimination, we arrived at two groups of studies.[10]

The main group consists of 29 studies that estimate the U-I effect and a second group consisting of nine studies that estimate the U-A effect.[11] Some studies include a single estimate while others report several estimates. For the first group of studies, we have 208 comparable estimates of the effect of unions on innovation, while the second group reports 51 estimates. These are the population of estimates that meet our search criteria and they are the data we use for the MRA.[12]

The second step in the MRA is the computation of an effect size for each study. As recommended by Djankov and Murrell (2002) and Doucouliagos and Laroche (2009), we use

[9] The search for studies ended 30th June 2010.

[10] Our list of econometric studies is similar to that used by other reviewers, most notably Menezes-Filho and Van Reenen (2003). We include some newer studies not included in their review and exclude some studies for which it was not possible to calculate a partial correlation or a marginal effect.

[11] Note that there are several other binary studies available. However, we were unable to include these in the MRA because we were unable to calculate marginal effects, e.g. Benvignati (1982).

[12] New technology is often embedded within physical stock (equipment and buildings). However, except for section 6.1 below, we ignore the effects of unions on physical investment in this paper. See Hornstein, Krusell and Violante (2007) for analysis of capital-embodied technical change.

the partial correlation for those estimates using continuous measures of innovation. Partial correlations measure the strength of the association between unions and innovation, holding all other factors constant. They are directly comparable between and within studies. Partial correlations can be combined to estimate the overall average effect size of unions, as well as the average effect for individual countries and time periods. We use the marginal effect for those estimates that use a binary measure of technology adoption: This was either reported in studies or we calculated it directly from information provided in the study.

The studies included in the MRA are listed in Appendix A, together with the country investigated, the measure of innovation, the average sample size, and the average partial correlation (or marginal effect). Of the 208 reported partial correlations, 58 percent use US data and most (64 percent) analyze manufacturing. The most common measure of unionization is union density, while the most common measure of innovation is R&D as a ratio to output (or sales). Of the 208 partial correlations, 47 percent of the reported estimates were of negative U-I effects that were also statistically significant. A further 28 percent were of negative U-I effects that were not statistically significant. Only ten percent of the estimates found a positive and statistically significant effect. With less than half of the estimates reporting statistically significant negative coefficients, it is tempting to conclude that the evidence is weak. However, it is well known that sampling error can distort inferences drawn from individual studies (Hunter and Schmidt, 2004; Kulinskaya, Morgenthaler and Staudte, 2008). Hence, it is important to draw inferences from the *accumulated* evidence from all comparable studies combined. By combining the evidence from numerous studies, meta-analysis increases statistical power.

Most of the studies were published in the 1980s and the 1990s, while the most recent was published in 2009. While this literature does not appear to be attracting many new datasets and studies, it nevertheless continues to be an important body of research. First, it is an important part of the literature on the economic effects of unions: “What do unions do?” Unions affect physical capital, productivity, productivity growth, and profitability. Their effect on innovation is a major part of this configuration of effects. Second, the effect of unions on innovation is important to understanding technology as an endogenous process arising from conscious decisions made by various institutions. Third, it is an important part of the literature that explores the effects of labor market regulations on productivity growth. Fourth, investment in intangible capital and innovation are now arguably more important than they have been in the past. Understanding the factors that obstruct or facilitate innovation is important for policy makers and firms. Hence, it is important to be clear regarding the effects of unions on innovation. This is particular important as most econometric studies on technology do not consider the effects of industrial relations. If industrial relations play a role, then these studies might be mis-specified.

3.2 Comparability of estimates

An important consideration in any review of a literature is the comparability of the studies evaluated. Of particular interest is whether the quality of studies differs significantly and, hence, whether it is meaningful to combine studies. We do not believe that this is a problem for this literature. The majority (86%) of the studies have been published in highly regarded journals, such as *Industrial Relations*, the *Industrial and Labor Relations Review*, the *Review of Economics and Statistics,* the *Economic Journal,* and the *European Economic Review.* Hence, we do not believe that any of the studies warrant exclusion from the database. To test this formally, we regress the precision (the inverse of the standard error) of each estimate against a constant and the Journal Impact Factor (from the 2007 Social Science

Citation Index).[13] If the coefficient on the Impact Factor is positive and statistically significant, it would indicate that higher ranked journals (in terms of their Impact Factor) report estimates with greater precision and, hence, that the studies should not be combined. This test indicates no association between precision and journal quality (coefficient = -0.925, with a t-statistic of -0.28). Thus, for our data, there is no difference in statistical precision on the basis of journal quality. Hence, we conclude that we can meaningfully combine the studies.

In any case, in the MRA we do not assign equal weights to all observations. Instead, we employ 'optimal weighs' and weigh each estimate by the inverse of its variance (Hedges and Olkin, 1985). This means that more precise estimates are given a greater weight.

Since the estimates were chosen to all be directly comparable across studies, they can be pooled together. We calculate three sets of averages for the pooled data. First, we calculate un-weighted averages for each country (see Table 1). Second, we apply MRA by regressing the partial correlations (or the marginal effects from the probit group of studies) upon a set of country dummy variables (see Table 1). These averages are unconditional because they abstract from data, measurement and specification differences between studies and estimates. Third, we use MRA to estimate conditional averages that control for country, time period, data, specification and measurement differences (see Table 2). We rely on these conditional MRA estimates for inference.

[13] Obviously, other rankings of journals can be used. However, Impact Factors are highly correlated with these. For example, the first order correlation between Impact Factors and the Kalaitzidakis, Stengos and Mamuneas' (2003) – KMS - rankings is 0.74 for the studies included in our MRA. That is, higher KMS ranked journals have higher Impact Factors.

4. The Unconditional Average Union-Innovation Effect

Figure 1 is a funnel plot showing the association between estimated U-I partial correlations and their estimated precision, measured here as the inverse of the estimate's (union effect's) standard error (Stanley and Doucouliagos, 2010).[14] The funnel plot illustrates the distribution of the U-I estimates showing that most partial correlations are negative. The continuous vertical line illustrates the position of the weighted average partial correlation of -0.09 (using precision to weigh each estimate), while the dotted vertical line shows the position of a zero partial correlation. The funnel plot appears to be fairly symmetrically distributed, around the precision weighted average. Symmetrical funnel plots suggest that an empirical literature is free of publication selection bias. This feature is important because selection bias has been shown to be a serious obstacle to statistical inference: If it is sufficiently large, selection bias can significantly magnify the association between two variables and distort statistical inference (see Stanley, 2005 and 2008). The funnel plot suggests that the U-I literature is relatively free of any such distortion.

Nearly two-thirds of the estimates use US data (58%), compared to 26% for the UK, 9% for Germany and 7% for Canada. Figure 2 presents the funnel plot for US estimates. For the US, the distribution of the results is similar to that shown in Figure 1, but with fewer positive correlations. The weighted average correlation for the US is -0.14. Figure 3 presents the funnel plot for non-US estimates, which have a weighted average of only +0.01, suggesting no effect on innovation.

FIGURES 1, 2, and 3 ABOUT HERE

[14] The figure draws its name from the expected 'funnel' shape: Econometric estimates should be symmetrically centred around the 'true' effect, which is more closely approximated by the more precise estimates (those at the top of the funnel), with the less precise estimates distributed on either side of the 'true' effect forming the funnel. The concept can be extended to allow a distribution of 'true effects', or genuine heterogeneity (see section 5 below).

The symmetrical appearance of the funnel plots is confirmed by formal statistical tests. We use the Funnel Asymmetry Test - Precision Effect Test (FAT-PET) test proposed by Stanley (2008). This is a test for the expected non-existence of an association between standard errors and the size of the U-I (or U-A) effect, if researchers do *not* strive to report statistically significant effects. In contrast, if publication is biased in favor of statistical significance, then researchers will re-estimate their models until they get a 'desired' result. Thus, the test involves regression the U-I partial correlations upon a constant and the standard error of U-I effects.[15] The test results are available from the authors and show that there is no statistical evidence of publication selection bias in this literature.[16] [17] Below we report multivariate versions of this test which reveals a small degree of selection bias that is of little consequence for inference.

4.1 Unions and innovation

Table 1 reports the un-weighted and MRA weighted averages for the USA, the UK, Germany, and Canada. It is important to note that Table 1 ignores all other aspects of study design, other than the country of origin of the data. Column 1 reports un-weighted averages for each country separately. This is the raw average of reported results that does not take into account the fact that the precision of the estimates varies. Column 2 reports the MRA weighted results pooling all estimates together and using country dummies to identify country differences. Precision is used to assign weights to individual estimates. In case there are any

[15] For full details and the logic behind the test, see Stanley (2005) and (2008).

[16] For both the U-I and U-A effects, the coefficient for selection bias is statistically insignificant, with p-values of 0.97 and 0.12, respectively.

[17] The finding of no publication selection bias differs sharply to what has been found for most other areas of empirical economics (see Roberts and Stanley, 2005).

country specific selection biases, we include also the standard error as an explanatory variable in the MRA, as recommended by Stanley (2008). Column 2 shows that abstracting from measurement and specification differences, the average U-I effect is negative in the US and Canada, but is positive in the UK and Germany.[18] In section 5 below, we analyze the U-I effect within a multivariate framework controlling for various study differences.

TABLE 1 ABOUT HERE

4.2 Technology Adoption Studies

Columns 3 and 4 of Table 1 report the average marginal effect from the nine probit studies for which we were able to calculate marginal effects. As we were unable to derive standard errors for many of the marginal effects, we used the square root of the sample size as a proxy for precision. When all 51 estimates are pooled together, the simple un-weighted average marginal effect is +0.014 (t-statistic = 0.82) and the equivalent weighted average marginal effect is -0.004 (t-statistic = -0.67). In columns 3 and 4 we split the data into countries. The average marginal effect is small and negative for the US. There is a small positive effect for the UK, but the level of statistical significance is weak. The insignificant dummy for Australia means that the average marginal effect in Australia does not differ from the average marginal effect in the US.

[18] The coefficient for *Canada* is not statistically significant in column 2. This means that Canada is not different to the base, which is the US. The MRA coefficient for the UK is +0.16, while the coefficient for the base (the US) is -0.14. Hence, the U-I effect for the UK is +0.02. Similarly, the MRA coefficient for Germany is +0.20. Hence, the U-I for Germany is +0.06.

5. Explaining heterogeneity

The averages reported in Table 1 ignore the obvious heterogeneity displayed in figures 1 to 3. The aim of this section is to identify the factors that cause heterogeneity in estimates reported within and between studies. Estimates will differ because of differences in samples (different industries, countries and time periods), differences in the measurement of key variables, differences in econometric specification, and they will also differ because of sampling error. To model these features, we use a standard MRA model:

$$r_{ij} = \beta_0 + \sum \gamma_k X_{ijk} + \sum \delta_n K_{ijn} + u_{ij} \qquad (1)$$

where r_{ij} is the ith comparable partial correlation (or, alternatively, the marginal effect) derived from the jth study, β_0 is the constant, the Xs are dummy variables representing characteristics associated with the jth study, the Ks are continuous variables associated with the jth study, γ_k and δ_n are the unknown regression coefficients, and u_i is the disturbance term (Stanley and Jarrell 1989). The X and K variables quantify the effect of key study differences on reported union effects. While the X and K variables will in the main come from information drawn from *within* the econometric studies included in the MRA, they can also include information that is *exogenous* to the studies. For example, below we consider the degree to which labor markets were regulated at the time that the samples were taken. This enables us to model potential sources of between-study variation that were not available to the authors of the primary studies.

The MRA model was initially applied by Stanley and Jarrell (1989), and is gaining widespread use among economists. Recent examples include Görg and Strobl (2001), Jarrell and Stanley (2004), Dobson *et al.* (2006), and Disdier and Head (2008). The meta-regression model has been developed to analyze the multi-dimensional nature of the research process (Stanley and Jarrell, 1989; and Stanley 2001). The MRA of equation 1 helps to quantify the effect of differences in data, specification, measurement, and estimation on reported union effects. Some of these differences are due to economic factors, such as differences between countries and over time, while others are due to research design, such as differences in the econometric specification.

5.1 Explanatory variables

We included 26 potential explanatory variables in the MRA. First, we considered eight variables that capture key data differences. Three of these relate to country differences: *UK*, *Germany*, and *Canada* are binary variables taking the value of 1 if the estimates relate respectively to the UK, Germany, and Canada, with the US used as the base. These dummies provide a test for cross-country differences in the U-I effect.[19]

Industry Level is a binary variable taking the value of 1 if the estimates relate to industry-level innovation data and 0 if they relate to firm/establishment-level data. The use of industry as the unit of analysis can lead to problems of aggregation bias and it is arguably preferable to use firm-level data. Moreover, decisions affecting innovation are made at the firm level and therefore are more likely to be affected by union activities at the firm level rather than by those at the industry level. *Services* is another binary variable taking the value of 1 if the estimates relate purely to services industries (with manufacturing as the base). *Various* is a binary variable taking the value of 1 if the estimates relate to data for several industries (again with manufacturing as the base). These variables are included to test whether the effect of unions on innovations varies across industry sectors as suggested by some studies (Hirsch, 1990; 1991; 1992). *Panel* is a binary variable taking the value of 1 if panel data are used (with cross-sectional data as the base). Cross-sectional data are more likely to capture long-run effects, while panel data capture transitional (medium-term)

[19] Table 1 reported the MRA with just country dummies. Table 2 extends this to include several control variables.

effects. *Average Year* is a continuous variable representing the average year of the data sample used in studies. This variable is included to capture any time patterns in the partial correlations.

Second, we consider six variables for measurement differences. *Industry Union* is a binary variable taking the value of 1 if industry-level union density is used (with firm level unionization as the base).[20] *Union Dummy* is a binary variable taking the value of 1 if a union dummy is used (with a firm level union density as the base). *RD Level* and *Innovations* are binary variables taking the value of 1 if the dependent variable is measured as the dollar value of R&D and the number of innovations, respectively, with R&D/sales as the base. These variables capture different dimensions of innovation. The most widely used measure is the R&D ratio.

Third, we consider twelve variables that reflect differences in control variables: *Firm Size*, *Profitability*, *Concentration*, *Market Share*, *Firm's Age*, *Wages*, *Advertising*, *Skills*, *Industry Dummies*, *Growth*, *High-Tech Firm*, and *Time Trend*. These are all binary variables taking the value of 1 if these variables were included as part of the econometric specification. For example, human capital (*Skills*) may be an important control variable. Some studies suggest that trade unions in high-skilled firms are less likely to challenge technological change because skilled workers are less likely to be affected by the resulting change, as workers are less likely to fear losing their jobs. Likewise, profits (*Profitability*) are important as they provide a major source for the financing of investments. Hence, profits should arguably be included in a well specified primary regression.[21] Finally, *Non-OLS* is included to capture the effect of different estimators, with OLS as the base.

[20] There is some collinearity between *Industry Union* and *Industry Level*, with a first order correlation coefficient of 0.57.

[21] There is, however, an argument for *not* including profits in innovation regressions. Odgers and Betts (1997) found that removing profits from their econometric model increases U-I. This is because unions decrease profits.

The effect of unions on innovation can depend on the institutional structure of collective bargaining, as well as the nature of the industrial relations system. In countries such as Germany, unions and employers regard each other as partners. Such cooperative industrial relations may facilitate technological progress and innovation. That is, the outcome of union rent seeking need not always be inefficient for both unions and firms. As Schnabel and Wagner (1992a: 370) argue: "Firms and unions could be better off by agreeing to maximize the size of the pie and then bargaining over the division of the pie (…) In other words, efficient bargaining would entail no real effect of unions on firms' investment activities". We use two variables to capture differences in bargaining and industrial relations.

First, we used data on the degree to which collective bargaining is centralized, *Central Wage*. These data are reported by the Fraser Institute as a component of their measure of economic freedom.[22] Second, we use a measure of overall labor market regulation, *Labor Regulation*, again reported by the Fraser Institute. This index is constructed by taking into account the existence and size of the minimum wage, the degree of hiring and firing regulation, the degree of centralization of collective bargaining, and mandated cost of hiring and firing. *Labor Regulation* serves as a proxy to which labor markets deviate from the competitive labor market model. Both series range from 1 to 10, with 10 being the most liberal (economically free).[23]

It is important to note that the individual econometric studies included in the dataset do not consider the effects of labor market regulation on the U-I effect. One of the benefits of

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So, including profits picks up only part of the effects of unions. At the same time, not including profits means that the equation may become mis-specified. MRA enables us to quantify the effect of including profits in the econometric specification.

[22] http://www.freetheworld.com/index.html.

[23] The *Labor Regulation* series varies both within and between countries over time. The average value of *Labor Regulation* is 7.22, 6.90, 7.24, and 3.36 for the US, the UK, Canada, and Germany, respectively, while the standard deviation is 0.53, 0.12, 0.04, and 0.06, respectively.

MRA is that it is able to consider factors that were not, or could not, be considered by the original econometric studies, in our case the degree of labor market liberalization. By pooling the estimates from different studies and collecting data on labor market regulation, we can explore the links between these two series. Data on labor market regulation, collective bargaining and the time trends in U-I can be regarded as information that is exogenous to the individual studies.[24]

The multivariate MRA results are presented in Table 2.[25] We use clustered data analysis to adjust standard errors for data dependence arising from multiple estimates reporting within studies. Since all observations are weighted by precision, estimation is by weighted least squares.

TABLE 2 ABOUT HERE

Column 1 presents the results of applying a general-to-specific modeling strategy, whereby we commenced with all 26 potential moderating variables and then sequentially eliminated any that were not statistically significant at least at the 10% level of significance.[26] Of the 26 variables, 15 were found to be statistically significant in explaining heterogeneity in the reported U-I effects.

For sensitivity analysis only, in column 2 we remove any study to which Hirsch was an author or co-author. One reason for checking the sensitivity of the results with respect to the Hirsch studies is that they use a relatively larger sample than most of the other studies,

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[24] While the years of data used can be an endogenous modelling choice, general trends in U-I are exogenous to the individual study.

[25] The standard error (*SE*) of the partial correlation was also included in the MRA in order to capture any selection effects. While statistically significant, the coefficient is less than 1, indicating the existence of only a small degree of selection bias.

[26] For the sake of brevity, the results for the general model are not reported here. These results are available from the authors.

and this might affect the results. Most of the results are not affected by the inclusion of the Hirsch group of studies. Moreover, there is no valid theoretical reason to exclude them from the dataset. In column 3 we use only those estimates that use R&D as a measure of innovation, removing any estimates using measures such as the number of innovations and patents data. In column 4 we rerun the MRA after removing the largest 5% and smallest 5% correlations. Finally, in column 5 we use robust regression to estimate the MRA. While this offers MRA coefficient estimates robust to outliers, this comes at the cost of having standard errors that are not corrected for data clustering, and individual data points (estimates from individual studies included in the MRA) assigned equal weights, instead of using precision. As can be seen from Table 2, most of the results are robust, expect for the variables that capture specification differences.

The preferred results are presented in column 1, and the discussion below revolves around these results. In these estimates, the constant is an estimate of the effects of unions on innovation in the US, using cross-sectional data, with firm level innovation and unionization data, with investments measured as R&D as a ratio of sales, estimated using OLS. The MRA does a very good job at explaining the variation in estimates, accounting for 68% of the variation (see the adjusted R-squared, column 1, Table 2). The remaining variation can be attributed to random errors.

5.2 Time effects

The coefficient for *Average Year* is positive indicating that the size of the adverse union effect is *declining* over time. The MRA reported in Table 2 controls for all the main differences between estimates and between studies. Hence, the time series pattern in the partial correlations is unlikely to be an outcome of the way that partial correlations are calculated.[27] The time trend exists for all countries, though it is much more pronounced for non-US estimates: Regressing the partial correlations on *Average Year* for just the US estimates gives a coefficient of +0.010 (t-statistic = 2.79), whilst for non-US estimates it gives a coefficient of +0.028 (t-statistic = 3.53). In his own extensive analysis, Hirsch (1992) found no evidence of time variation in U-I. In contrast, our MRA finds a significant pattern, though a weaker one for the US.

One explanation for this trend is that it reflects structural change, with the adverse U-I effect getting smaller over time, perhaps because of the changing nature of bargaining. The trend could, for example, be driven by the weakening of union strength over time: Weaker unions could have smaller adverse effects on innovation. Further, union preferences might have changed over time, and/or the nature of contracts might have changed over time, with unions and employers negotiating not just over wages, but also bargaining over employment and, possibly, over innovation, so that these are all determined simultaneously. This might be related to declining union strength and increased competitive pressure from other (non-unionized) domestic firms and foreign competitors. If there has been a progressive shift away from non-cooperative bargaining, towards a more cooperative one, then this will lead to the observed time series pattern. An alternative explanation might be that *Average Year* is correlated with a variable not included in the MRA. We have, however, tried to consider all factors relevant in the MRA modeling.

5.3 Country Effects and Labor Regulation

None of the three non-US dummies, (Canada, Germany, and the UK) are statistically significant in the general-to-specific model (column 1, Table 2). This means that the U-I

[27] If *Average Year* is replaced by the actual year of *publication*, we get a similar positive trend: +0.008, with a t-statistic of 3.30.

effect in these countries does not differ from the US, once all other aspects of the research process are considered. The base in the MRA is the US, so that the constant in the MRA measures the U-I effect for the US. Since the non-US country dummies are not different to the US effect, the MRA suggests that *unions have an adverse effect on innovation in all countries included in the dataset.* Note the contrast with the results presented in Table 1, where other aspects of research design were not modeled. Simply taking an average of reported estimates gives the appearance of a positive U-I effect for Germany and a near zero effect for the UK. However, once data, measurement and specification differences are modeled, we find that U-I is negative in all countries.

Panel B of Table 2 adds the three country dummies back into the general-to-specific version of the MRA (the variables initially drop out as part of the general-to-specific modeling strategy). All three country dummies have a negative coefficient, but they are not statistically significantly.[28] Hence, the MRA (column 1 of Table 2) suggests that after controlling for the various differences in the way studies were constructed and holding time and country-specific regulatory differences constant, unions have an adverse effect in all countries studied - the US, the UK, Canada, Germany.

While the country dummies are not statistically significant, the MRA indicates that regulatory differences modify the size of the U-I effect. The negative coefficient on *Labor Regulation* in column 1 indicates that the more liberal are labor markets, the more adverse is the effect of unions.[29] This is an interesting finding to emerge from the MRA. Flexible labor markets improve the allocation of labor. However, the results presented here indicate that labor market flexibility comes at a cost in terms of increasing union resistance to innovation. To see this, consider that the average value of *Labor Regulation* was 7.22 for the US, compared to 3.36 for Germany. This means that, *ceteris paribus*, labor market flexibility adds

[28] The two exceptions are Germany in columns 3 and 5, where it has a *larger negative* effect than the US.

[29] If country and *Labor Regulation* interactions are added, they are not statistically significant.

-0.07 to the negative U-I partial correlation for the US (7.22*-0.01) compared to only -0.03 for Germany (3.36*-0.01). Hence, ignoring all other factors, the MRA estimates that U-I in the US is -0.24 (-0.17-0.07) compared to -0.20 in Germany (-0.17-0.03). These results stand in sharp contrast to what many expect regarding the effects of labor market regulations (see for example the opening quote to this paper from Alesina and Zeira, 2006).

The associations between average year and the reported U-I effects and labor market regulation and the reported U-I effects are illustrated in figures 4 and 5, respectively. The associated partial regression plots from the MRA are presented in figures 6 and 7, respectively. In order to ensure that the results are not driven by a small number of potentially relatively large partial correlations, the MRA was re-estimated after removing the smallest and largest partial correlations (see Table 2, column 4). The results remain unchanged, as can be seen by the associated partial regression plots (Figures 8 and 9).

FIGURES 4, 5, 6, 7, 8, and 9 ABOUT HERE

Schnabel and Wagner (1994, p. 493) note that: "Efficient bargaining and the cooperative behavior it entails depend on the legal framework, the institutional structure of collective bargaining, the design of negotiated contracts, and the style of industrial relations." They argue that adversarial industrial relations (such as those found in the US and UK) are more likely to result in adverse effects on innovation, than cooperative ones (countries such as Germany). The variable *Labor Regulation*, however, reflects more than just collective bargaining differences: It includes the minimum wage and regulations over hiring and firing. We replaced *Labor Regulation* with a variable that focuses purely on collective bargaining (*Central Wage*), but this was not statistically significant. We also used a series on the degree of bargaining coordination (see Flanagan, 1999). This variable is also not statistically significant.

Labor Regulation is actually very highly correlated with Germany (first order correlation of -0.93).[30] Unfortunately, we are unable to find a suitable instrument to redress this correlation. Table 3 explores the sensitivity of the MRA results by comparing the results from Tables 1 and 2, to some additional meta-regressions.[31] Each row reports two sets of results. The first set of entries reports the key regression coefficients from the MRA. The second set of entries reports the average effect on R&D, evaluated by considering the degree of labor regulation and the average year of the data used for the individual country samples. The *Labor Regulation* variable is omitted in row 3 and *Average Year* is also omitted in the results reported in row 4. Rows 5 and 6 repeat the MRA reported in rows 3 and 4, but this time with country dummies added as explanatory variables.[32] Row 6 effectively repeats the results from Table 1 but with the addition of the MRA control variables listed in Table 2 (*Industry Level*, etc.) In row 7 we report the MRA after removing all observations relating to Germany. The results from Table 3 confirm that regardless of the specification, the effect of unions on R&D is negative, in all countries. This table also indicates that the results for *Labor Regulation* are not driven by the inclusion of German data (see column 7 Table 3).

TABLE 3 ABOUT HERE

[30] Of the four countries in the dataset, Germany has the highest degree of labor market regulation. *Labor Regulation* is not correlated with UK (correlation = 0.06) nor Canada (correlation = 0.11), but it does have a moderate degree of correlation with USA (correlation = 0.43).

[31] Recall that Table 1 reports the unconditional averages predicted by MRA, while tables 2 and 3 report the conditional averages.

[32] Recall that these variables drop out of the MRA when a general-to-specific strategy is pursued, meaning that there is no statistical difference between the U-I effect in the US and the other countries.

The possibility that labor market regulation might moderate the effect of unions has received little attention in the literature. The effect of labor market regulation on innovation is itself theoretically ambiguous.[33] The effect of labor market regulation on the U-I effect is likewise ambiguous: Several opposing effects might operate, so that the net effect remains an empirical issue.

On the one hand, labor market *de*regulation can be expected to stimulate innovate. Deregulation benefits firms by increasing flexibility and reducing labor and production cost. Labor market deregulation is also likely to decrease unemployment thereby benefiting 'outsiders'.[34] Greater labor market deregulation should increase incentives to innovate, especially where technological change requires labor adjustment. Technological change that creates opposition from insiders might be easier to implement when labor markets are flexible. Also, flexible labor markets might limit the extent to which unions can capture rents arising from technology.

On the other hand, labor market regulation might stimulate innovation through several channels:

(a) *Higher labor cost*: Tighter labor market regulations are likely to strengthen the position and bargaining power of insiders, increasing their wages. Higher labor costs increase the incentives to increase capital intensity, replacing labor with capital. Alesina and Zeira (2006) extend this line of argument by arguing that labor regulation in Europe keeps wage inequality

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[33] Labor market deregulation does not actually mean that industrial relations become completely unregulated. Rather, the idea behind 'deregulation' is that regulations emphasize internal sources, e.g. managers are given either unilateral say within a workplace over key decisions affecting employees, or in consultation with employees. In contrast, 'regulation' means that at least some key decisions are made externally to the organization.

[34] Technological shocks might create unemployment when generous welfare state provisions (that often co-exist with labor market regulation) make displaced workers prefer unemployment.

low, creating incentives for firms: “to develop and adopt labor saving capital-intensive technologies at the low end of the skill distribution. At the same time technical change in the US has been more skill biased than in Europe, since American skilled wages have been higher” (2006, p. 3).

(b) *Firm’s bargaining power*: Innovation improves productivity, increases profits and make new capital more attractive. It might also increase a firm’s bargaining power, enabling it to capture a greater share of available rents. Labor market regulations might ‘shock’ firms into investing in technological change that actually shifts bargaining power in their favour.

(c) *Motivation effect:* Storm and Naastepad (2009) argue that regulations might increase innovation if they increase worker motivation and commitment to the firm, making it easier to introduce labor-saving technical change. That is, regulations might increase the likelihood that labor will cooperate with management. Labor deregulation might even worsen industrial conflict, especially if it lowers wages. Consistent with this argument, Storm and Naastepad (2009) find that for OECD countries, labor market regulation increased productivity growth. Francois and Roberts (2003) argue that the more regulated European labor markets might enable firms to extract work effort at a lower cost, compared to US type labor markets.[35]

(d) *Rent seeking:* By creating frictions that restrict firing (and hiring), unions might have greater opportunities to extract rents from firms. They may thus encourage firms to innovate, so that they can subsequently extract rents from the investments. This effect would, at best, occur only in the short term.

[35] Francois and Roberts (2003) argue that while firms operating in highly regulated labor markets face higher hiring and firing costs, they also enjoy lower costs of extracting effort. Thus, the net effect on incentives to innovate is unclear.

5.4 *Measurement, Data, and Econometric Specification Differences*

The MRA results indicate that data differences are a robust determinant of heterogeneity in reported U-I effects. The use of industry level innovation data (*Industry Level*) produces larger adverse effects, compared to firm level data (increases the negative partial correlation by 0.19, on average). That is, the adverse effects of unions are magnified at the industry level. This result might be an outcome of aggregation bias. However, it could also reflect negative externalities, as the adverse effects spillover from one firm unto another. In contrast, the use of industry level unionization data (*Industry Union*) results in smaller adverse effects compared to firm level union data. This appears to be inconsistent with the idea of a non-monotonic relationship between unions and technology discussed in section 2 above (Dowrick and Spencer, 1994), although there is not a strong association between the use of industry level union data and industry unions. Measuring unionization as a binary variable (*Union Dummy*), instead of a more informative continuous variable (typically union density) also results in smaller adverse U-I effects.

The MRA did not detect any industry differences in U-I effects: The two industry dummies (*Services* and *Various*) were not statistically significant, indicating that the U-I effect in these industries does not differ from manufacturing.

The measurement of innovation is important. Compared to R&D based measures, studies that use innovations data (e.g number of patents and number of innovations) find much smaller adverse effects (*Innovation*). That is, using R&D data (investment flows) results in larger adverse U-I effects, compared to measures of actual innovation. This indicates that while unions depress investment in R&D (an indicator of innovation), actual innovation is not as adversely affected.[36]

The MRA shows that econometric specification differences are important, though the results are not always robust (compare columns 1 through to 5). Controlling for the firm's age, advertising, and wages all result in larger negative U-I effects. For example, studies that include the firm's age in the primary regression find, on average, a 0.06 larger negative U-I effect than those that do not. In contrast, the inclusion of industry dummies, a time trend, market share, human capital (skills), and profitability in the econometric specification are also important, resulting in smaller adverse effects.[37] For example, studies that include human capital in the primary regression find, on average, a 0.07 *smaller* negative U-I effect than those that do not.

Several variables did not appear to be important in explaining the differences between estimates. Controlling for industry concentration, firm size, industry dummies, the firm's growth, and whether a firm is a high-tech firm, all make no difference to the reported U-I estimates. The MRA also suggests that there is no difference in U-I estimates from cross-sectional and panel data. Further, estimator does not appear to matter once other dimensions of research are considered: Estimates derived from OLS are essential the same as those derived from other estimators. It is not the estimator but the data that affects estimates of U-I.

[36] We rerun the MRA using country dummies interacted with *Innovation*, to test whether there are country differences with respect to innovation counts. None of the interactions were statistically significant, either individually or jointly (Wald test = 0.49, p-value = 0.62).

[37] Note that the time trend referred to here is a time trend in the primary regression model capturing trends in intangible capital formation. It is *not* a measure of a time trend in the union effect: This is captured by *Average Year* in the MRA. The only other variable that is statistically significant is *Book Chapters*. Studies published as book chapters tend to report much smaller adverse effects.

5.5 *Robustness*

For sensitivity analysis, we re-estimated column 1 (of Table 2) using author ids to cluster the observations, instead of using study ids. The results are essentially unchanged. Also in unreported regressions, we explored the effects of other variables. First, we considered the effects of trade liberalization. Lommersund *et al.* (2006) argue that trade liberalization might increase resistance to technology, if it reduces the elasticity of the demand for labor. As a proxy for trade liberalization, we use the Freedom House's index of Freedom to Trade Internationally.[38] This was never statistically significant, suggesting that for the industries and time periods examined, economic openness was not an important influence on the size of the U-I effect.

Dowrick and Spencer (1994), argue that the existence of unemployment insurance might make unions more willing to accept technological change. To explore this, we use three alternative measures. First, we included transfers and subsidies as a percent of GDP in the MRA. Second, we included the per capita value of social public expenditures (including unemployment compensation and health care) expressed in constant prices PPP (in 2000 US prices). Third, we included social public expenditures as a percent of GDP.[39] We found no link between these proxies of welfare state provisions and the size of the U-I effect. It appears that it is not welfare provisions that shape U-I. Rather, it is the combined effect of hiring and firing regulations, minimum wages, and collective bargaining that moderates union's response to investment in new technology.

[38] This is component Area 4b of the index. The series ranges from 1 to 10.

[39] The data on transfers comes from The World Bank, while the data on social public expenditures comes from the OECD.

5.6 *Technology adoption (the U-A effect)*

With only nine studies and 51 estimates with marginal effects, we are unable to estimate an MRA with the same number of variables as the MRA for the U-I effect. Instead, we considered an MRA with the effects of regulations, time, and country differences.[40] Only two variables emerge to be statistically significant. We find that *Labor Regulation* has a negative coefficient (-0.05, t=-1.90), confirming the results for U-I effects: Unions operating in countries with more flexible labor markets are more likely to resist the adoption of new technology. We also found a larger negative marginal effect in Australia, compared to the US (-0.11, t=-1.87). No difference was detected between the UK and the US in terms of technology adoption (0.04, t=0.84).[41] Finally, in contrast to the U-I effect, there was no noticeable pattern in union resistance to technology adoption over time (-0.011, t=-0.30).

5.7 *Size of effect*

How large is the effect of unions on innovation? Our key measure of effect has been the partial correlation. When assessing the effect of a first order correlation, most authors use Cohen's (1988) guidelines, and these have been used also for partial correlations. According to Cohen, a correlation of 0.2 is a small effect, 0.50 is a medium effect and anything larger than 0.8 is large. However, partial correlations can be larger or smaller than first order correlations. Doucouliagos (2010) derives guidelines for partial correlations, showing that: a partial correlation less than 0.02 is trivial; less than 0.07 is small; less than 0.16 is medium; and greater than 0.30 is large.

According to the MRA reported in Table 2, the effects of unions on innovation vary depending on the measures of innovation and unionization used. When firm level data are

[40] This is not as limiting as it might seem. All the U-A estimates are derived from firm level innovation and unionization data, so that there is no issue here regarding firm versus industry level data.

[41] We had no observations for Germany for which we could calculate marginal effects and their standard errors.

used and innovation is measured as R&D, the U-I partial correlation for the US is estimated to be -0.22, rising to -0.34 when industry level data are used.[42] If the measure of innovation is actual innovation counts instead of R&D, the U-I partial correlation for the US is estimated to be -0.08, rising to -0.20 when industry level data are used. This means that the effect of unions on R&D in the US is moderate to large and is economically significant, while the effect on the number of innovations is small to medium.

For a sub-set of 128 estimates we were able to calculate elasticities (the percentage change in innovation resulting from a one percentage change in unionization). Evaluated at the sample means, we find that unionization has directly reduced innovation by about 7%. This we regard to be of practical economic significance.[43]

6. Other determinants of innovation

The partial correlations measure the union effect, holding all other factors constant. It is also of some interest to investigate what other (non-union) determinants of innovation are established in this literature, and to compare the size of their effects. Our analysis of the effects of unions was based on a *systematic review* of all the evidence. That is, we compiled a population of the available estimates. Since the main focus in this paper is on unions, we have not undertaken a similar systematic search for all studies that report estimates for other determinants of innovation. Hence, we are unable to offer a systematic review of these. However, we are able to provide a *partial review* of the relevant literature.

The partial review is conducted on only those estimates of other determinants of innovation that are published as part of the U-I effect literature. That is, we include here only

[42] For these calculations we use the sample averages for average year and labor market regulation, we assume that unionization is measured as a continuous variable and that the econometric model includes all the variables identified as significant by the MRA in column 1 of Table 2.

[43] We were able to calculate 72 elasticities for the US which have an average value of -6.4, and 56 elasticities for the rest of the world which have an average value of -8.4.

those estimates of the determinants of innovation that are reported in the 38 studies that investigate the U-I effect. Those studies that did not investigate the effects of unions are, obviously, not included in our sample. Hence, the results presented in Table 4, should be considered in this light. For example, Comin and Hobijn (2004) find that cross-country differences in technology adoption are driven by income, human capital, and openness. They find that two institutional variables are also important (executive and legislative power) but they do not explore the effects of industrial relations.

We collected information on six other determinants of technological change: human capital, industry concentration, firm size, growth, profitability, and physical capital. Table 4 reports the direct effect of these six non-union determinants of innovation. Both human capital and industry concentration are negatively correlated with innovation. Firm size, growth, profitability and physical capital all have a positive effect on innovation.

Firm size has the largest positive effect on innovation: Larger firms invest more in innovation. Growth is more important than profitability, while physical capital has a smaller effect. Table 4 indicates that physical capital and intangible capital (innovation) are complements in the production process, while human capital and intangible capital are substitutes.[44]

It is interesting to compare the size of these effects with the size of the effect of unions. The size of the unconditional U-I effect in the US (-0.14, column 2, Table 1) is greater than the adverse effect on innovation arising from human capital (-0.10, column 1, Table 4) and industry concentration (-0.08, column 2, Table 4). The MRA suggests that

[44] We expected to find complementarity between innovation and human capital. The negative correlation with human capital might simply be a specific result found by the group of studies under review, rather than a universal effect: Only of full meta-analysis of the entire literature on the effects of human capital on innovation can uncover the real relationship between the two. The result is, however, consistent with vintage human capital theories, where firm specific human capital accumulates for a specific vintage of capital, reducing the incentive to adopt new technology (Jovanovic and Nyarko, 1996; Comin and Hobijn, 2004).

market power reduces the incentive to innovate, but this is smaller than the effect from unions.

TABLE 4 ABOUT HERE

6.1 Indirect Effects

Table 4 can be used to identify some of the indirect effects of unions on innovation. Table 4 shows that profitability has a direct positive effect on innovation, with a partial correlation of about +0.13.[45] Doucouliagos and Laroche (2009) found that, on average, US unions had a negative effect on profits, with an average partial correlation of -0.09. This means that unions have an *indirect* negative effect on innovation working through their adverse effect on profitability of -0.012 $(= 0.13 \times -0.09)$. Similarly, Table 4 shows that the direct effect of physical capital on innovation is +0.05. Doucouliagos and Laroche (2003) found that unions had a negative effect on physical capital, with an average partial correlation of -0.08. This means that unions have an indirect negative effect on innovation working through their adverse effect on physical capital, of -0.004 $(= 0.05 \times -0.08)$. These two indirect effects are small, adding about -0.02 to the direct U-I effect.

7. Conclusion

This paper provides a systematic review of the econometric evidence on the effects of unions on innovation and technology adoption. We apply meta-regression analysis to 208 estimates reported in 29 technology impact studies and 51 estimates from nine technology adoption studies. We draw five robust conclusions from these data.

[45] There is a causality issue here, as higher profits enable more investment to be financed and investing in new capital increases future profits.

First, when the existing estimates are scrutinized using meta-regression analysis, we find that there is in fact little disagreement between studies. All the available evidence indicates that unions depress the level of innovation and they also depress the adoption of technology. This finding is consistent with both the tax on capital and labor monopoly theories of union behavior (outlined in section 2), which appear to dominate any collective voice effects. Unions have a more depressing effect on R&D then they do on actual innovations.

Second, country differences in the degree to which unions impact upon technology are driven largely by the degree of labor market regulation. Holding labor market regulation constant, unions have an adverse effect on investment in all countries of a similar magnitude. However, industrial relations and regulatory regimes differ between countries, and these differences matter for technology outcomes. More regulated labor markets experience less union resistance to technology. Consequently, unions have a larger negative effect on innovation in the US and Canada, then they do in Germany.

Third, the U-I effect has been declining over time in all countries, though this trend is less pronounced in the case of the US. This could reflect change in the nature of bargaining processes.

Fourth, the size of the direct effect of unions on technology in the US is much larger than the effect of unions on profits and the effect on physical capital: Unions have a more noticeable adverse effect on innovation. Further, by depressing profits and physical capital, unions have a small second round, or indirect, effect on innovation.

Fifth, MRA shows that most of the variation in reported estimates can be explained by differences in the data used (firm versus industry), the measurement of technology (R&D versus patents) and the econometric specification. MRA quantifies the effect of these differences.

The meta-regression analysis indicates that unions affect both infra-marginal and marginal decisions: They affect the decision to adopt technology and how much innovation is undertaken. Many econometric studies have been carried out ignoring the effects of industrial relations on technology. Such studies might very well be mis-specified, as the evidence indicates that industrial relations do matter.

The meta-analysis presented here focused only on econometric studies. There exists a very large number of case studies on the effects of unions on technology. Analysis of this vast literature using the tools of meta-analysis might also reveal important insights on the economic effects of unions.

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Table 1: Average Union Effects,
Innovation and Technology Adoption

Innovation (U-I)			Technology Adoption (U-A)		
Country	Un-weighted Average Partial Correlations (1)	MRA, Precision Weighted Average Partial Correlations (2)	Country	Un-weighted Average Marginal Effects (3)	MRA, Precision Weighted Average Marginal Effects (4)
US (N=120, K=15)	-0.120 (-8.88)***	-0.140 (-5.45)***	US (K=16, N=2)	-0.039 (-1.52)	-0.030 (-3.44)***
UK (N=54, K=5)	-0.021 (0.66)	0.160 (5.44)***	UK (K=24, N=5)	0.067 (1.22)	0.095 (1.85)
Germany (N=19, K=5)	-0.057 (0.70)	0.201 (11.28)***	Germany	na	na
Canada (N=15, K=2)	-0.207 (-3.12)***	-0.054 (-1.12)	Canada	na	na
Australia	na	na	Australia (K=11, N=2)	-0.026 (-1.51)	0.004 (0.25)

Notes: N denotes the number of estimates and K denotes the number of studies. Columns 1 and 3 report averages using data for each country separately. Columns 2 and 4 combine all data and use country dummy variables, so that the total effect is calculated relatively to the base (the US). The MRA in columns 2 and 4 includes also the partial correlations' standard error and the marginal effects' inverse of square root of the sample size, respectively. Figures in brackets are t-statistics using clustered data analysis to correct for any data dependence arising from the inclusion of several estimates per study. **, *** indicates statistical significance at the 5% and 1% levels, respectively. na denotes insufficient estimates.

Table 2: Determinants of Heterogeneity in U-I effects
(Dependent variable is partial correlations)

Explanatory Variable	Mean [SD]	All data, WLS (1)	Without Hirsch studies, WLS (2)	R&D studies only, WLS (3)	Without top and bottom 5%, WLS (4)	Robust regression (5)
A. General to Specific Models						
Constant (USA)	0.58 [0.50]	**-0.17** (5.45)	**-0.22** (5.24)	**-0.15** (5.80)	**-0.16** (5.34)	**-0.14** (4.21)
Data differences						
Industry Level	0.25 [0.43]	**-0.19** (7.14)	**-0.16** (7.80)	**-0.20** (6.56)	**-0.19** (6.76)	**-0.21** (9.06)
Industry Union	0.26 [0.44]	**0.06** (2.89)	**0.05** (2.82)	**0.05** (2.15)	**0.05** (2.76)	**0.04** (2.28)
Union Dummy	0.44 [0.50]	**0.05** (5.14)	**0.04** (2.75)	**0.05** (4.28)	**0.05** (5.03)	**0.04** (2.08)
Innovation	0.13 [0.34]	**0.14** (6.99)	**0.16** (6.76)	**-**	**0.13** (5.41)	**0.13** (5.32)
Specification differences						
Profitability	0.32 [0.47]	**0.01** (2.51)	0.01 (0.67)	**0.01** (2.99)	**0.01** (2.53)	0.02 (1.73)
Human capital	0.15 [0.36]	**0.07** (2.69)	-0.05 (1.39)	**0.10** (6.65)	**0.08** (3.33)	**0.11** (5.31)
Advertising	0.06 [0.23]	**-0.11** (3.99)	-0.05 (1.03)	**-0.16** (5.57)	**-0.09** (3.05)	**-0.08** (2.31)
Market share	0.08 [0.27]	0.04 (1.91)	**0.06** (4.03)	**0.07** (3.51)	0.04 (1.50)	0.02 (0.72)
Wages	0.40 [0.49]	-0.04 (1.74)	**0.08** (2.22)	**-0.07** (3.90)	-0.03 (1.28)	0.01 (0.32)
Firm's Age	0.40 [0.49]	**-0.06** (3.09)	-0.01 (0.65)	**-0.04** (2.12)	**-0.06** (2.39)	**-0.10** (4.22)
Industry Dummies	0.24 [0.43]	**0.01** (2.55)	**0.03** (2.74)	0.02 (1.97)	0.01 (0.86)	-0.01 (0.72)
Time Trend	0.35 [0.48]	**0.02** (2.87)	0.01 (0.23)	**0.03** (4.46)	**0.02** (2.85)	0.02 (1.18)
Exogenous Data						
Average Year	19.80 [4.63]	**0.01** (5.44)	**0.01** (5.24)	**0.01** (5.79)	**0.01** (5.34)	**0.01** (4.25)
Labor Regulation	6.79 [1.17]	**-0.01** (2.38)	0.01 (0.75)	0.01 (0.19)	**-0.01** (3.45)	**-0.03** (4.55)
SE	0.07 [0.06]	**0.80** (2.41)	**0.75** (2.48)	**0.98** (2.78)	**0.83** (3.08)	**0.51** (4.21)
N		208	129	181	188	208
K		25	21	18	25	25
Adjusted R^2		0.68	0.71	0.38	0.80	-
B. Specific MRA Models With Country Dummies						
Canada	0.07 [0.26]	-0.02 (0.84)	-0.03 (1.85)	-0.03 (1.76)	-0.02 (0.92)	0.03 (1.14)
Germany	0.09 [0.29]	-0.09 (1.30)	-0.09 (0.62)	-0.35 (5.31)	-0.03 (0.40)	-0.21 (2.13)
UK	0.26 [0.44]	-0.01 (0.63)	-0.01 (0.12)	0.01 (0.13)	-0.01 (0.08)	-0.07 (2.09)

Columns 1 to 4 estimated by weighted least squares, using precision as weights. Column 5 uses robust regression. Figures in round brackets are *absolute* values of t-statistics, using clustered data analysis to adjust standard errors. Bold indicates statistically significant at least at the 5% level. *SE* is the standard error of the individual U-I effects. N and K denote the total number of estimates and studies, respectively. Results in shaded cells are robust to alternative sub-samples. Panel B repeats the MRA models after adding back the country dummies.

Table 3: Country Differences in U-I Effects

	USA	UK	Germany	Canada	Labor Regulation	Average Year
			Unconditional averages			
Univariate model (Table 1) (1)	-0.14 (-5.45)	0.16 (5.44)	0.20 (11.28)	-0.05 (1.12)	-	-
Average effect	*-0.14*	*+0.02*	*+0.06*	*-0.14*		
			Conditional averages			
General-to-specific model (Column 2, Table 2) (2)	-0.17 (5.45)	-	-	-	-0.01 (2.38)	0.01 (5.44)
Average R&D effect	*-0.17*	*-0.10*	*-0.07*	*-0.16*		
(3)	-0.18 (5.27)	-	-	-	-	0.01 (5.23)
Average R&D effect	*-0.17*	*-0.10*	*-0.10*	*-0.16*		
(4)	-0.09 (6.42)	-	-	-	-	-
Average R&D effect	*-0.09*	*-0.09*	*-0.09*	*-0.09*		
(5)	-0.20 (3.62)	-0.01 (0.63)	-0.09 (1.30)	-0.02 (0.84)	-	0.01 (3.59)
Average R&D effect	*-0.16*	*-0.10*	*-0.06*	*-0.14*		
(6)	-0.13 (6.63)	0.06 (5.99)	0.12 (5.15)	-0.02 (0.60)	-	-
Average R&D effect	*-0.13*	*-0.07*	*-0.01*	*-0.13*		
(7)	-0.18 (6.67)	-	-	-	-0.02 (3.77)	0.01 (6.62)
Average R&D effect	*-0.19*	*-0.12*	-	*-0.18*		

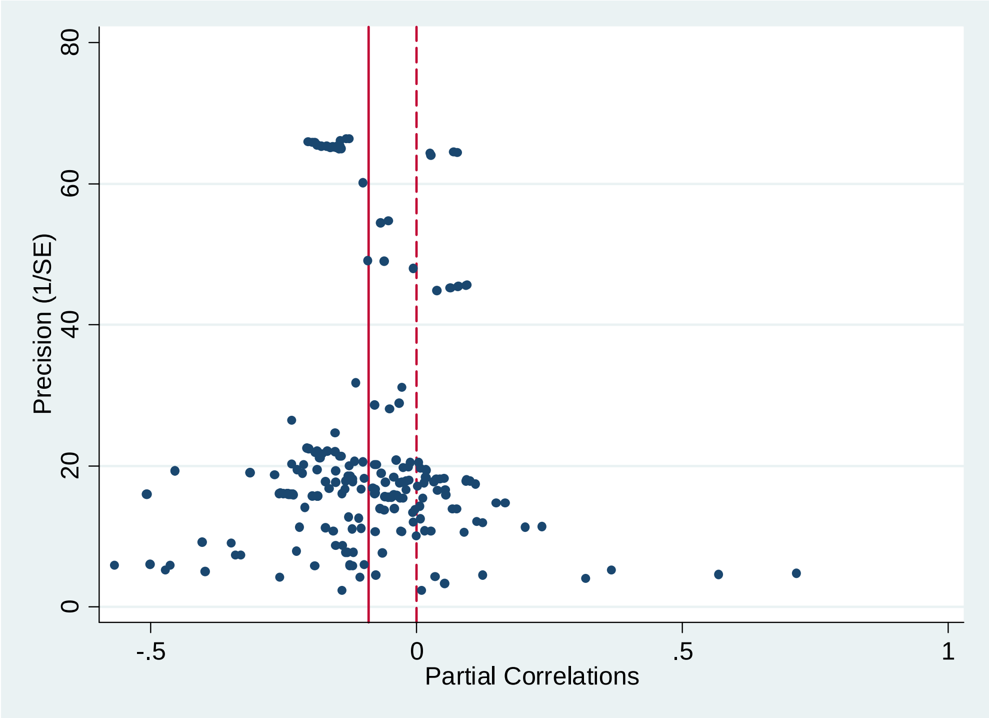
Estimation is by weighted least squares, using precision as weights. Figures in brackets are *absolute* values of t-statistics, using clustered data analysis. Except for row 1, all rows include the same set of control variables as in Table 2. Row 1 reproduces the results from Table 1, column 2. Row 2 reproduces the results from Table 2, column 2. All German data are excluded from the results reported in row 7. Average R&D effects are evaluated using country specific sample means for average year and labor market regulation.

Table 4: Non-Union Determinants of Innovation
(Weighted average partial correlations)

	Human capital (1)	Industry concentration (2)	Firm size (3)	Growth (4)	Profitability (5)	Physical capital (6)
Non-union determinants	-0.10 (-2.13)**	-0.08 (-3.56)***	0.28 (8.86)***	0.17 (3.27)***	0.13 (8.87)***	0.05 (1.99)**
N	22	73	61	47	37	56
K	8	18	14	8	10	13

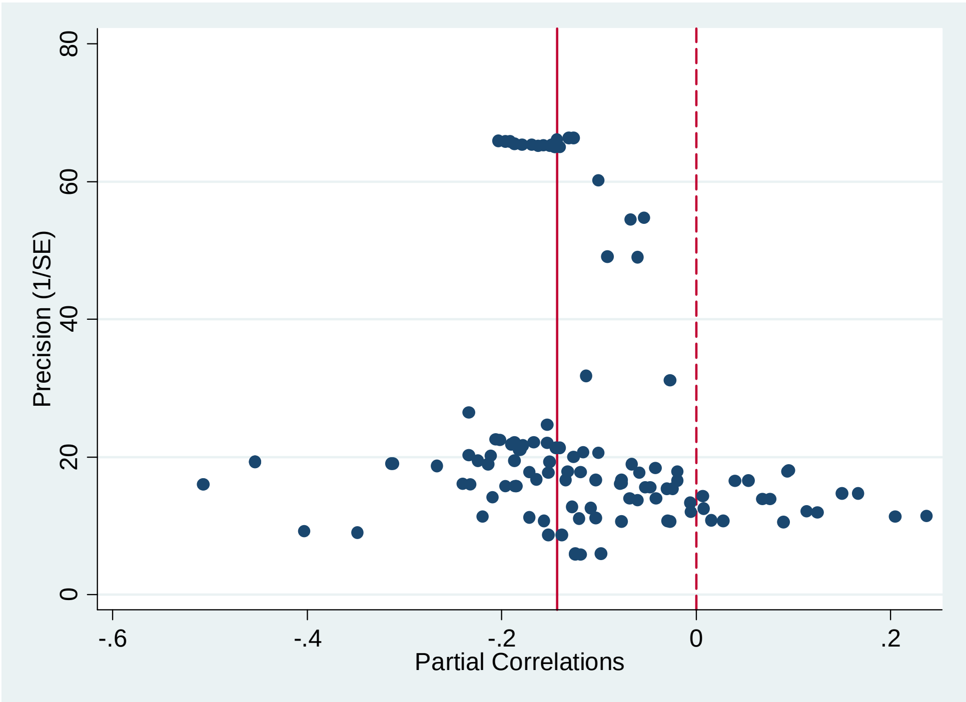
Notes: N and K denote the total number of estimates and studies, respectively. Figures in brackets are t-statistics derived using standard errors that are robust to data clustering. These estimates are based on a partial meta-analysis of the data. **, *** indicates statistical significance at the 5% and 1% levels, respectively. ns denotes insufficient number of observations for between-country analysis: Results are for all countries combined.

Figure 1: Funnel Plot of Union-Innovation Effects, All Estimates (N = 208)



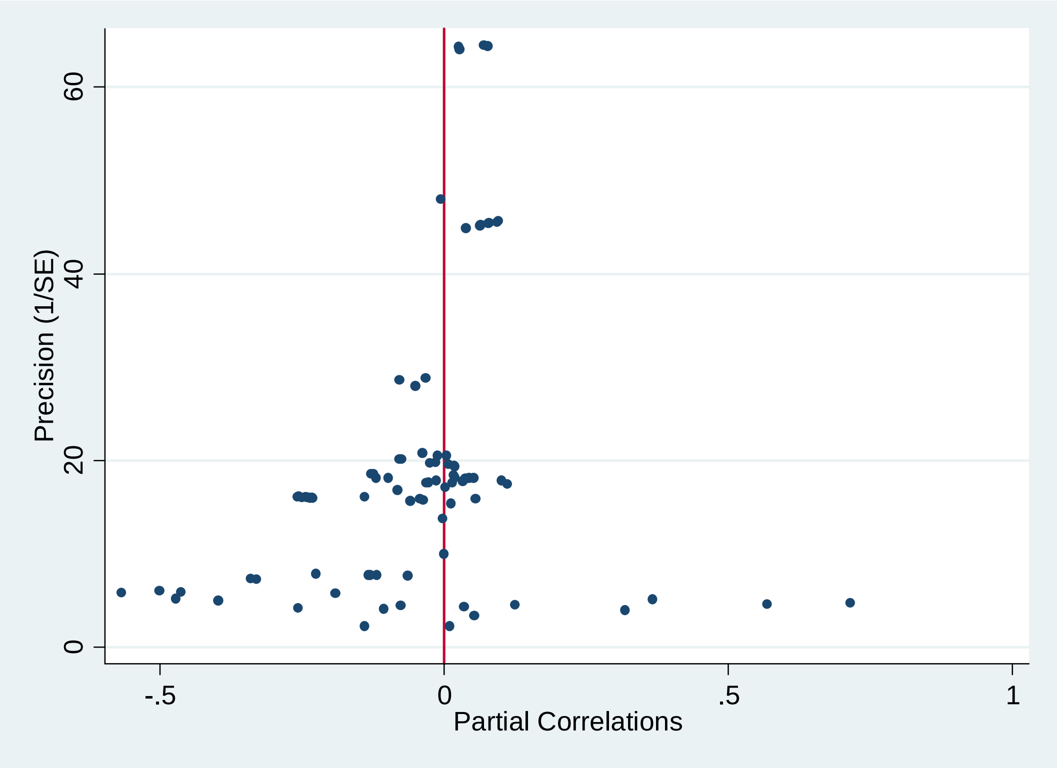
Simple average = -0.09. Precision weighted average = -0.09. Continuous line located at the weighted average.

Figure 2: Funnel Plot of Union-Innovation Effects, US estimates (N = 120)



Simple average = -0.12. Precision weighted average = -0.14. Continuous line located at the weighted average.

Figure 3: Funnel Plot of Union-Innovation Effects, Non-US estimates (N = 88)



Simple average = -0.06. Precision weighted average = +0.01. Continuous line located at the weighted average.

Figure 4: Average Year and U-I Effects

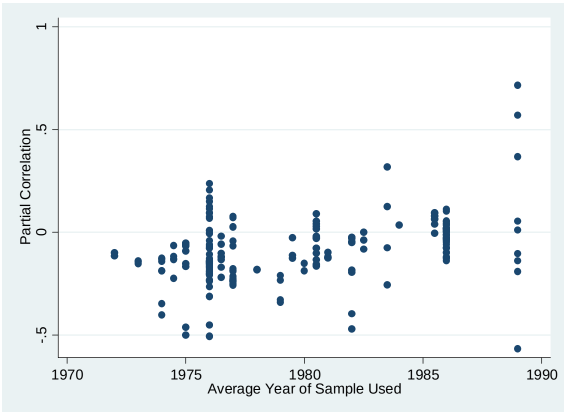


Figure 5: Regulation and U-I Effects

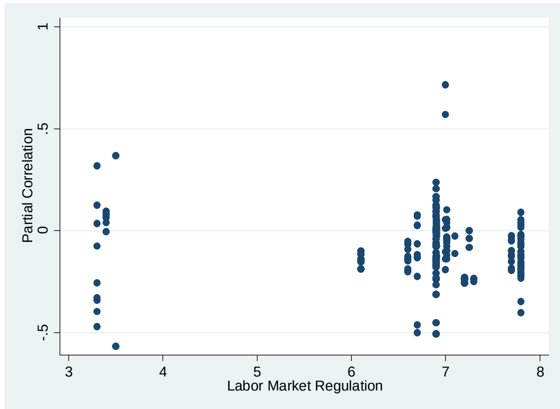


Figure 6: Partial Regression Plot, Average Year

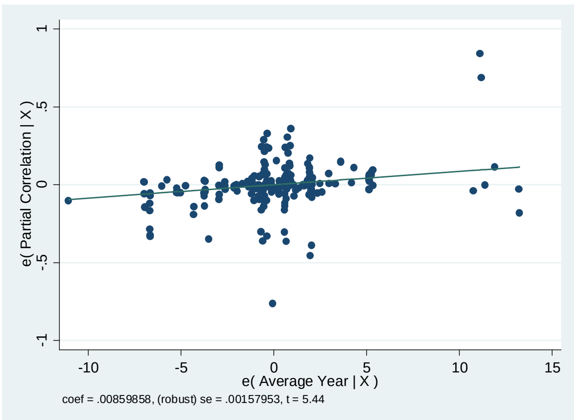


Figure 7: Partial Regression Plot, Regulation

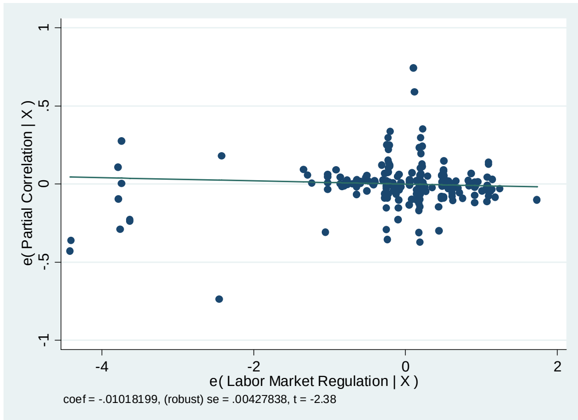


Figure 8: Partial Regression Plot, Average Year (without bottom and top 5% estimates)

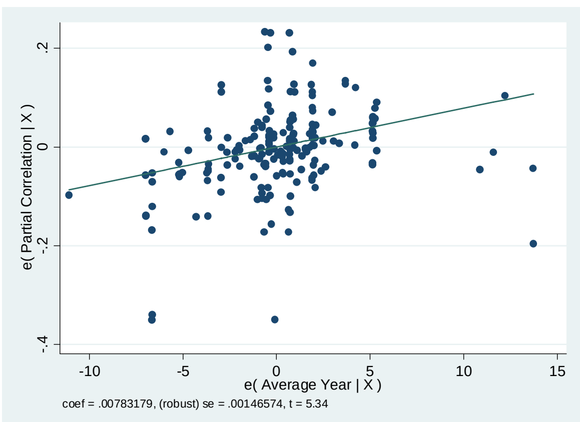
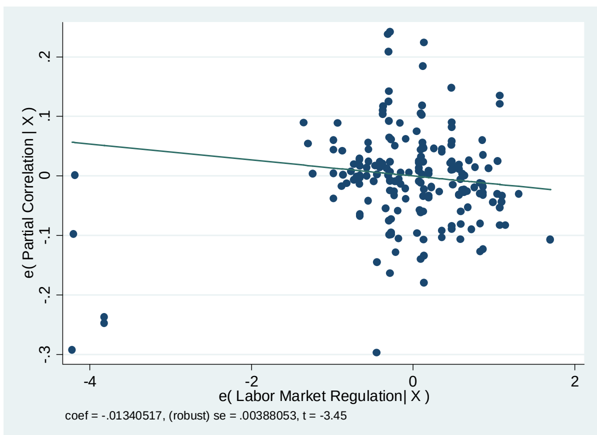


Figure 9: Partial Regression Plot, Regulation (without bottom and top 5% estimates)



Appendix A: Econometric Studies Included in the Meta-Analysis (K=38)

Author(s) (Year)	**Main Country Analyzed**	**Measure of Innovation**[1]	**Average Sample Size**[2]	**Weighted Average partial correlation**[3]
Innovation studies				
Acs & Audretsch (1987 &1988)	US	Number of innovations	247	-0.191***
Addison & Wagner (1994a & 1994b)	UK	R&D ratio	15	0.036
Allen (1988)	US	R&D ratio	74	-0.376***
Audrecht & Schulenburg (1990)	US	Number of innovations	246	-0.185***
Betcherman (1991)	Canada	Expenditures on innovation	294	-0.047
Betts, Odgers & Wilson (2001)	Canada	R&D ratio	247	-0.241***
Blumenfeld (2002)	US	R&D ratio	1,011	-0.070
Blundell, Griffith and Van Reenen (1999)	UK	Number of innovations	4,125	0.050**
Bronars & Deere (1993)	US	R&D ratio	660	-0.153***
Bronars, Deere & Tracy (1994)	US	R&D ratio	209	-0.069
Connolly, Hirsch & Hirschey (1986)	US	R&D ratio	367	-0.137***
Fitzroy & Kraft (1990)	Germany	Innovation rate	57	-0.335**
Geroski (1990)	UK	Number of innovations	73	-0.134
Hirsch (1990)	US	R&D expenditure	2,692	-0.067***
Hirsch (1991)	US	R&D expenditure	4,327	-0.133***
Hirsch (1992)	US	R&D expenditure	4,176	-0.165***
Koeller (1996)	US	Number of innovations	246	-0.050
Kraft, Stank and Dewenter (2009)	Germany	Number of innovations	2,062	0.064***
Menezes-Filho, Ulph & Van Reenen (1998a & 1998b)	UK	R&D ratio	469	-0.037
Nair-Reichert & Pomery (1999)	US	R&D ratio	419	-0.126***
Schnabel & Wagner (1992a & 1992b)	Germany	R&D ratio	27	0.023
Schnabel & Wagner (1994)	Germany	R&D ratio	29	-0.060
Schulenburg & Wagner (1990)	Germany and US	Number of innovations	138	-0.070
Taymaz (1991)	US	Innovation rate	42	-0.116
Ulph & Ulph (1989)	UK	R&D/sales	33	-0.482**
Technology adoption studies				
Drago and Wooden (1994)	Australia	Introduction of technical change	802	-0.07**
Hirsch and Link (1987)	USA	Advantage in product-related technological innovation / Leader in developing innovative new product	315	-0.14**
Keefe (1991)	USA	Adoption of CADCAM, Numerically machine tool (NMT), Computer Numerically controlled (CNC), etc.	821	-0.02
Latreille (1992)	UK	Using new technology	418	+0.17**
Lintner, Pokorny, Woods & Blinkhorn (1987)	UK	Adoption of CADCAM, CNC, NMT, etc.	123	+0.02
Machin and Wadhwani (1991)	UK	Introduction of conventional and advanced technical change		+0.07
Michie and Sheehan (1999)	UK	Introduction of advanced technological change	374	-0.07
Michie and Sheehan (2003)	UK	Adoption of any product and process innovation	242	+0.19
Rogers (2004)	Australia	Innovator	920	-0.02

Notes: [1] Broad type of measure used. See studies for exact measures used. Some studies use several measures. **, *** indicates statistical significance at the 5% and 1% levels, respectively. [2] Average sample size and weighted average partial correlation are the averages of all estimates used, with precision used to weigh the individual correlations. [3] The average marginal effect is reported for the technology adoption studies. The MRA uses the individual partial correlations and individual marginal effects. Bibliographic references are available from the authors.

Appendix B: Studies included in the meta-analysis

1. Acs, Z. and Audretsch, D.B. 1987. Innovation in Large and Small Firms. *Economic Letters*, 23: 109-112
2. Acs, Z.J. and Audretsch, D.B. 1988. Innovation in Large and Small Firms: An empirical analysis. *American Economic Review*, 78(4):678-690.
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17. Hirsch, B.T. 1990. Innovative Activity, Productivity Growth and Firm Performance: Are labor unions a spur or a deterrent? *Advances in Applied Micro-Economics*, vol.5, Greenwich, Connect.: JAI Press, 69-104.
18. Hirsch, B.T. 1991. *Labor Unions and the Economic Performance of Firms*, WE Upjohn institute, Kalamazoo, Michigan.
19. Hirsch, B.T. 1992. Firm Investment Behavior and Collective Bargaining Strategy. *Industrial Relations*, 31: 95-121.

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21. Keefe, J. 1991. Do Unions Influence the Diffusion of Technology? *Industrial and Labor Relations Review*, 44(2): 261-274.
22. Koeller, C.T. 1996. Union Membership, Market Structure, and the Innovation Output of Large and Small Firms. *Journal of Labor Research*, XVII (4): 683-699.
23. Kraft, K., Stank, J. and Dewenter, R. 2009. Co-determination and Innovation. *Cambridge Journal of Economics*, December, 23.
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