Does intellectual property protection deliver economic benefits?
A multi-outcome meta-regression analysis of the evidence

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Abstract

Both theoretical and empirical work reports contingent and conflicting findings on how intellectual property (IP) protection affects related outcomes such as innovation, technology diffusion, productivity, or growth. To establish where the balance of the evidence lies, we conduct a multi-outcome meta-regression analysis to synthesize findings form 91 primary studies that report 1,626 effect-size estimates for one or more outcomes. Controlling for unobserved heterogeneity and selection bias only, we find that the effect on innovation, technology diffusion, productivity, and economic growth is statistically or practically insignificant. The effect remains insignificant when we control for observed sources of heterogeneity and estimate meta-effects based on different scenarios for 'best-practice' research. Our work contributes to the existing research effort by extending the application of the multi-outcome meta-regression analysis into evidence synthesis in economics. It also provides verifiable/replicable evidence indicating that the sanguine claims about the economic benefits of IP protection voiced in some legal studies and the advocacy literature are misleading.

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1. Introduction

In line with the empirical studies included in this meta-analysis and the theoretical models that underpin the empirical research effort, we subscribe to a definition of intellectual property (IP) that emphasizes the existence of an exclusive right to own and market an invention for a fixed time period (Gallini and Scotchmer, 2002). Hence, intellectual property protection (IP protection) refers to protection of the fixed-term but exclusive right through copy rights, patents, licences, trademarks or other of protection (Gallini and Scotchmer, 2002; Scotchmer, 2004; Granstrand, 1999). From the perspective of the theory incentives, IP protection is one of several incentive-correcting mechanisms for tackling three potential sources of market failure in knowledge production: (i) uncertainties about the outcomes of the research process; (ii) positive externalities associated with produced knowledge; and (iii) near-zero marginal cost of imitation.[1] The policy objective is to maximize social welfare by minimising the gap between socially and privately optimal levels of innovation.

Yet, even in a static setting, IP protection entails a trade-off between the market power associated with the protection of the time-limited exclusive right and the correction of the incentive for innovation (Hall & Harhoff, 2012). In a dynamic setting, the trade-off becomes even more complex. Here, the effect is contingent on whether IP protection increases the cost of subsequent and complementary innovations (Bessen & Maskin, 2009) and whether the quality of the inventive activity falls due to patent races (Dasgupta & Stiglitz, 1980, 1988). The economic consequences of IP protection become even more difficult to ascertain if innovators choose to frustrate the objective of the policy by using the intellectual property rights (IPR) instruments strategically (Hall & Harhoff, 2012).

These theoretical sources contingency notwithstanding, there has been a staggering increase in the number of IPR instruments and in the volume of research investigating their economic implications. A Google Scholar search using the "intellectual property rights protection" as a search term yields 5,640 studies published in 1990, with the number increasing at 10% annually

[1] Other incentive-correcting mechanisms include direct and indirect public support for private investment in innovation, government investment in basic research, and public procurement and/or outsourcing policies that create predictable demand for private innovation outputs. For reviews of the literature on these mechanisms, see Becker (2015) on public support for business research and development (R&D) investment; Salter and Martin (2001) on publicly funded research; and Chicot and Matt (2018) on public procurement and innovation.

to reach 42,600 studies in 2018. Furthermore, the number of patents and trademarks in 2018 stood at 14 million and 49 million, respectively. The stock in 2018 has been underpinned by respective annual growth rates of 5.2% and 15.5% since 2013, with China and India emerging as major contributors to the growth in patent and trademark registrations.[2]

Against this background, we observe a stark discrepancy between the pessimistic/cautious conclusions derived in most narrative reviews by economists (Boldrin & Levine, 2013; Chang, 2001; De Beer, 2016; Gallini, 2002; Hall & Harhoff, 2012) and the optimistic/sanguine claims about the benefits of IP protection encountered in some work by legal scholars (e.g., Acri, 2016; Haber, 2016; Lehman, 1996) or in reviews/reports sponsored by business interest groups or official IP bodies (e.g., Dixon, 2011; OECD, 2015; WIPO, 2015). Whereas work in the former category draws attention to the trade-off between the incentive-correction and monopoly-power effects of IP protection; work in the latter category tends to emphasize the incentive-correction effects and conclude that IP protection is a significant driver of innovation, knowledge diffusion, and economic growth.

We acknowledge the valuable contribution that narrative reviews make to the research field by identifying the current state of the art and deriving implications for future research. However, the tools available for narrative reviewers (summary measures, tabulations, etc.) are inadequate for settling the conflicting claims in the field, where the reported effect-size estimates are heterogenous and may reflect publication selection. Hence, one aim of this study is to propose and implement a multi-outcome meta-regression model that allows for obtaining verifiable/replicable effect-size estimates for the effects of IP protection on four closely related economic outcomes (innovation, technology diffusion, productivity, and economic growth) after taking account of heterogeneity, publication selection bias, and data dependence. The second aim is to inform evidence-based policy debate by establishing whether a true effect exists and how the effect varies by outcome, by sampling and estimation choices, and by differences in the IP protection environment.

To the best of our knowledge, most meta-analysis studies in economics have so far addressed within-study dependence and between-study heterogeneity through clustered standard errors (e.g., Alptekin & Levine, 2012; Awaworyi Churchill & Yew, 2018; Lichter et al., 2015) or two-level hierarchical meta-regression models (e.g., Awaworyi Churchill et al., 2017a; Balima

[2] See, https://www.wipo.int/about-ip/en/.

et al., 2020; Ugur et al., 2018; Ugur et al., 2020). Nevertheless, issues of dependence and heterogeneity that may arise at higher levels have not been addressed. This is particularly the case with respect to the economic benefits of IP protection, which are estimated in the context of different but related outcomes such as innovation, technology diffusion, productivity, and growth. Hence, the third aim of this study is to address higher levels of dependence and heterogeneity at the outcome level, which occur when primary studies report evidence on one or more outcomes and the outcome-level effects may be correlated. To achieve this aim, we draw on recent developments in meta-analysis methodology in education, psychology, and healthcare where researchers synthesize the effect-size estimates for multiple but related outcomes of a given intervention (Dessie et al., 2020; Moeyaert et al., 2017; Van den Noortgate et al., 2015). The resulting multi-outcome meta-regression model is a multi-level model where effect-size estimates (level-1) are nested within primary studies (level-2) that, in turn, are nested within one or more of the four IP protection outcomes they investigate (level-3).

The proposed method offers two advantages: (i) it takes account of correlation between related outcomes; and (ii) makes use of the sampling covariances information even if the latter is not reported by primary studies (Van den Noortgate et al., 2015). A third advantage is that the effect-size estimate for a particular outcome 'borrows strength' from observed effect sizes for other outcomes, resulting in more accurate and precise effect-size estimates (Jackson et al., 2011).

The rest of the paper is organised in four sections. In section 2, we first provide a summary of the theoretical models that analyse the effects of IP protection on the four economic outcomes stated above. We then discuss the measurement and estimations issues faced by the empirical studies and the way in which we model them in this meta-analysis. Finally, we highlight the contested nature of the IP protection and the need for verifiable/replicable evidence synthesis.

In section 3, we describe the search strategy and the inclusion/exclusion criteria we used to identify the eligible studies. Here, we also explain the extraction and coding procedures we followed in constructing the meta-analysis dataset, and provide a detailed overview of the evidence base, including visual and econometric evidence on the extent of heterogeneity and publication selection. This is followed by the discussion of the meta-analysis methodology in section 4, where we introduce the rationale for and advantages of the multi-outcome meta-regression approach. As a sensitivity check, we also provide evidence from separate meta-analyses of the individual outcomes.

Section 5 presents our findings. The first set of results takes account of between-outcome dependence, publication selection and unobserved heterogeneity. They indicate that IP protection has no significant effect on innovation, productivity, or economic growth. The effect on technology diffusion is positive but small, ranging from 0.044 to 0.049. In the second set of results, we take account of observed sources of heterogeneity such as publication type, sample characteristics, data characteristics, estimation methods, etc. Once these observable sources of heterogeneity are considered, the meta-effect based on different scenarios for 'best practice' in the research field indicates that IP protection has no significant effect on any of the four economic outcomes considered in this study. We conclude by summarising the main findings and discussing their implications for policy and future research.

2. The theory and empirics of IP protection and its economic consequences

The theoretical work on economic consequences of IP protection builds on early contributions to the economics of innovation, which have highlighted three characteristics of the inventive activity: (i) high degree of uncertainty concerning the outcome of knowledge production; (ii) in-appropriability of the innovation outcomes; and (iii) near-zero marginal cost of imitation or knowledge spillovers (Arrow, 1962; Jewkes et al., 1962). Nordhaus (1969) is the first attempt at incorporating the insights from this early work into a neo-classical growth model augmented with patent life, patent value and other dimensions of the intellectual property rights protection. Focusing on patent protection for a single invention, Nordhaus (1969) demonstrates that stronger intellectual property protection induces more investment in innovation; and consequently higher levels of output and productivity growth.

Later work extended the initial model in several directions. Some studies have considered innovation as a cumulative process that requires attention to how IP protection affects follow-up innovation. As indicated in an extensive review by Gallini (2002), the dynamic approach to IP protection qualifies the predictions of the Nordhaus (1969) model in two ways. If the patent provides sufficiently strong protection, the patent holder can "hold up" future innovations by threatening litigation. In addition, the patent holder could also be held up by previous innovators. Therefore, the effect of IP protection on future innovation is ambiguous: it depends on the balance between the incentive and deterrence effects of protection (Merges & Nelson, 1990).

Others have considered the implications of entry. For instance, longer patent protection may increase an entrant's incentives to imitate and hence reduce the value of the protective patent as an incentive-correcting instrument (Gallini, 1992). Even if imitation occurs only after the patent expires, the relationship between patent life and the rate of innovation will have an inverted-U shape (Horowitz & Edwin, 1996). The hump-shaped relationship is due to opposing effects of patent life. On the one hand, a longer patent life induces the patent holder to develop larger (more prominent) inventions. On the other hand, the frequency of innovation falls as patent life increases. If the low-frequency effect dominates (is dominated by) the size effect, the rate of innovation declines (increases) in patent life.

Another strand of research investigated the extent to which licensing can mitigate or correct for the adverse effects of IP protection on innovation. For example, Green and Scotchmer (1995) demonstrate that the innovation incentives of both licensors and licensees would increase if an inventor could license the use of their innovation for a fee, which emerges as a market solution that resolves the incentive problem by compensating the pioneer with a share of the rents associated with the follow-on discovery. Nevertheless, licensing contracts may remain sub-optimal because it is difficult to identify the subsequent innovators. Secondly, subsequent innovators must invest in innovation before they can commit to conclude a licensing contract. Third, the licensor and the licensee may have diverging expectations about the value of the invention, leading to inefficient bargaining on the license fee. Finally, transaction costs of negotiating contracts may be high, particularly when the follow-on innovation draws on multiple patents (Gallini, 2002).

As can be seen from the summary above, the effect of patent protection on subsequent innovation is ambiguous at best. The ambiguity is placed in sharper relief by Heller and Eisenberg (1998), who draw attention to a possible 'anti-commons tragedy' under the conditions of strong patent protection. The authors highlight the complex obstacles that arise in biomedical research, where a downstream innovator needs access to multiple patented inputs to create a single useful product. Because each upstream patent allows their owners to restrict access, the cost of downstream innovation increases and hence the pace of innovation decreases.

A similar conclusion is obtained when the effect on technology diffusion is considered. On the one hand, stronger patent protection may encourage technology diffusion through vertical specialization. In this setting, some innovators specialise in large inventions with multiple patentable inventions whilst subsequent innovators purchase the patentable inventions instead of undertaking costly investment in innovation. Hall and Ziedonis (2001) demonstrate that vertical specialisation has been associated with technology transfer/diffusion in the semi-conductor industry. Nevertheless, licensing agreements under vertical specialisation can exacerbate the adverse effects of market power associated with patent protection. For example, technology-dominant firms may set transfer terms that maximise rent extraction instead of technology diffusion. Overall, the effect on technology diffusion depends on the extent to which competition policy is effective in monitoring transfer agreements and reducing their adverse effects on competition in innovative activity (Gallini, 2002).

The sources of ambiguity discussed above notwithstanding, Acemoglu and Akcigit (2012) demonstrate that the effect of IP protection on innovation and technology diffusion also depend on the distance to the technology frontier. The authors develop a dynamic model of interactions between IP protection and competition, where industries and firms undertake step-by-step innovation. They demonstrate that full patent protection for all innovators is sub-optimal. In contrast, IP protection encourages subsequent innovation when it protects only the innovations of technology leaders near or at the technology frontier. These findings are similar to what has been reported by Cornelli and Schankerman (1999), who demonstrate that, under moral hazard and information asymmetry, a uniform IP protection provision for firms with different R&D productivities may be welfare-reducing. This is because the uniform patent protection provides too much R&D incentive to low-productivity firms and too little to high-productivity ones.[3]

The third strand of theoretical work examines the effects of IP protection on productivity and economic growth. In this line of research, IP protection affects the level of productivity or economic growth through its effects on innovation and technology diffusion. For example,

[3] The case for variable IP protection instruments and provision has also been discussed from legal, regional development and international cooperation perspectives. For example, Carroll (2009) is in favour of a uniform regime but the legal instruments can/should be revised depending on changes in innovator incentives and availability of alternative appropriability mechanisms among other factors. Tödtling and Trippl (2005) investigate innovation policy at the regional level and conclude that a “one-size-fits-all model” is unfeasible given the differences in the innovation activities of central, peripheral and old industrial areas. Finally, Maskus (2010) discusses the existing IP protection arrangement in the context of environmentally sound technologies and concludes that there is a case for variable provisions that involve linking patent terms with licensing commitments, investing in patent transparency, and facilitating voluntary patent pools.

Segerstrom et al. (1990) develop a dynamic general equilibrium model of 'North-South' trade.[4] In the model, innovation in the 'North' is an outcome of research and development (R&D) races, in which the probability of winning the race is proportional to R&D resources. The winner of each R&D race earns monopoly profits during the patent life, after which perfect competition prevails. If wages in the 'North' and 'South' are equal, patent protection increases productivity and output in the 'North'. This is achieved through higher levels of employment in R&D activities and higher probability of innovation. However, patent protection is detrimental for productivity and output growth if wages in the 'North' are higher than the 'South'. In this scenario, stricter patent protection reduces the steady-state level of employment in R&D activities and increase the steady-state number of monopolistic firms.

Helpman (1993) also develops a 'North-South' model of IPR enforcement, where the 'North' invents new products and the 'South' imitates them. Taking into account the endogenous nature of innovation, Helpman (1993) demonstrates that the 'South' loses form tighter IP protection. This is because the increase in world output that follows from the temporary increase in innovation due to tighter IP protection in the 'North' is not sufficient to compensate the 'South' for its losses. The effect on the 'North' is also negative: tighter IP protection raises the 'North's welfare through increase in savings and R&D investment rates, but this welfare gain is smaller than the welfare loss caused by the shift in the time profile of available products, including products imported from the 'South'.

Nevertheless, Dinopoulos and Segerstrom (2010) arrive at opposite conclusions when they introduce multinational firms (MNFs) into a model of 'North-South' trade. In the model, Northern firms are innovative and produce higher-quality products, multinational firms transfer technology through foreign direct investment (FDI) in the South, and the Southern firms imitate products produced by local affiliates of the multinational firms. The authors demonstrate that stronger IP protection in the South leads to a permanent increase in the rate of technology transfer to the South, a permanent increase in R&D employment by Southern affiliates of the Northern MNFs, and a temporary increase in the Northern innovation rate. As a result, IP protection in the South is a source of productivity gains and output growth – provided that the

[4] We thank one of our anonymous reviewers for drawing attention to the change in the terminology used to differentiate between developed and developing countries over time. To strike a balance between the evolving terminology and the terminology used in the reviewed sources, we have decided to state the North-South descriptor in inverted commas – to suggest that the description is generic.

MNFs transfer technology from the North to the South. The proviso, however, is difficult to ascertain due to difficulties in measuring the level and quality of the technology transfer.

It must be acknowledged here that the brief review we provide is not extensive enough to reflect all theoretical contributions in the research field. For a more complete evaluation of the theoretical work, we refer the reader to consult the narrative reviews by Gallini (2002), Hall and Harhoff (2012), Maskus (2012) and Rockett (2010). It must also be indicated that the earlier theoretical work on the economic implications of IP protection have been overtaken by a new strand of work that focuses on intellectual property analytics (IPA).[5]

Nonetheless, the theoretical work reviewed above has informed a substantial empirical effort aimed at estimating the effects of IP protection on related outcomes such as innovation, technology diffusion/transfer, productivity, and economic growth. In the meta dataset we have constructed, most primary studies estimate the effect of IP protection on one or more of the outcomes at the country or within-country regional level (59%), followed by firm-level (28%) and industry-level (13%) estimates. In terms of data period, 50% of the studies utilise data ending in 2004, 25% utilise data ending in 2008, and 5% utilise data ending in 2011. Furthermore, 54% of studies haven published in the last decade from 2010-2019 compared to 46% published in the previous two decades from 1900-2009. Hence, the primary study sample can be considered as representative in terms of publication date and time dimension of the data used. In what follows below, we provide a summary of the typical empirical models estimated in the included primary studies.

Studies that investigate the effect of IP protection on *innovation* estimate either a *patent flow model* based on a knowledge production function or an *R&D intensity model* where R&D investment is a function of income (or firm/industry sales) and R&D productivity. Both models are augmented with a measure of IP protection, which enters the models as a determinant of the *unobserved knowledge efficiency* in the patent flow model or *R&D productivity* in the R&D intensity model. The hypothesis is that IP protection, through its effects on the productivity of patentable knowledge or R&D investment, affects innovation at the firm, industry, or country

[5]The IPA literature utilizes big data availability to analyse the trends, patterns and relationships in IP protection activity. This literature is reviewed in Aristodemou and Tietze (2017), who identify four focus areas for the emerging literature: knowledge management, technology management, economic value of information, and the extraction and effective management of information.

level. The patent flow and R&D investment models are stated formally in (1) and (2) below, respectively.

$$lnP_{it} = \alpha_{10} + \beta_{11} lnP_{it-1} + \beta_{12} lnRD_{it} + \beta_{13} lnHC_{it} + \gamma_1 lnIPP_{it} + v_{1i} + \delta_{1t} + \varepsilon_{1it} \quad (1)$$

$$RD_{it} = \alpha_{20} + \beta_{21} Y_{it} + \beta_{2k} \sum_{k=2}^{K} CV_{itk} + \gamma_2 IPP_{it} + v_{2i} + \delta_{2t} + \varepsilon_{2it} \quad (2)$$

Here, i is the unit of analysis (firm, industry, or country), t is time, v is unit-specific heterogeneity, δ represents time effects, ε is the idiosyncratic error term. Of the dependent variables, *P* is patent flow and *RD* is the level of R&D expenditures. The numerical subscripts reflect the model number at hand. In the patent flow model, the coefficient of interest is γ_1, which measures the effect of IP protection on innovation after controlling for previous knowledge (P_{it-1}), R&D investment (*RD*), and human capital (*HC*). In model 2, the coefficient of interest is γ_2, which measures the effect of IP protection on innovation after controlling for sales/income and a set of control variables denoted by CV_k.

The primary studies that estimate the effect on *diffusion* estimate either a *foreign direct investment (FDI)* or a *royalty payments* model. In these studies, FDI or royalties are used as proxies for technology diffusion/transfer, which remains unobservable for the researcher. Studies that estimate the effect on FDI flows usually draw on a gravity model of FDI, whereas others that estimates a royalty payments model usually draw on a model proposed by Branstetter et al. (2004). Typical empirical models estimated in this area of research can be stated as follows:

$$lnFDI_{ijt} = \alpha_{30} + \beta_{31} lnY_{it} + \beta_{32} lnY_{jt} + \beta_{3k} \sum_{k=3}^{K} CV_{itk} + \gamma_3 lnIPP_{jt} + \varepsilon_{3ijt} \quad (3)$$

$$RP_{ilt} = \alpha_{40} + \beta_{41} y_{it} + \beta_{42k} \sum_{k=1}^{K} P_{itk} + \beta_{43k} \sum_{k=1}^{K} H_{jtk} + \beta_{44k} \sum_{k=1}^{K} A_{ilk} + \gamma_4 IPP_{it} + \varepsilon_{4ilt} \quad (4)$$

In (3), i and j denote the home and host countries of FDI, Y_i and Y_j are home and host country GDP, and CV_i is a set of control variables that affect inward FDI investment. The coefficient of interest, γ_3, measure the effect of IP protection on FDI flows into the host country after controlling for other determinants of inward FDI investment. In (4), the dependent variable is royalty payments (*RP*) from affiliate l to its parent company i. Of the remaining variables, y is a vector of country-specific time trends, and *P*, *H* and *A* are parent, host, and affiliate characteristics, respectively. The coefficient of interest, γ_4, measures the effect of IP protection in the host country on royalty payments from the affiliate in the host country to its parent company abroad.

The empirical *productivity* and *growth* models share a common ground in that they are based on a Cobb-Douglas production function where IP protection is the determinant of *unobserved technological change*. Typical productivity and growth models for estimation can be stated as follows:

$$lnY_{it} = \alpha_{50} + \beta_{51}lnK_{it} + \beta_{52}lnL_{it} + \gamma_5 lnIPP_{it} + v_{5i} + \delta_{5t} + \varepsilon_{5it} \qquad (5)$$[6]

$$\Delta(\tfrac{Y}{N})_{it} = \alpha_{60} + \beta_1(\tfrac{Y}{N})_{i0} + \textstyle\sum_{k=2}^{K}\beta_k CV_{itk} + \gamma_6 IPP_{it} + v_{6i} + \delta_{6t} + \varepsilon_{6it} \qquad (6)$$

In (5), *Y* is real output, produced utilizing capital input *K* and labour input *L*, with coefficients $\beta_{51}$and β_{52} as factor shares that reflect the marginal products of capital and labour. The coefficient on *IPP* (γ_5) is the contribution of IP protection to output, after controlling for contributions of the conventional inputs. In (6), the dependent variable $\Delta(Y/N)$ is the growth of per-capita GDP, which depends on the initial level of income, $(Y/N)_0$, and the set of regressors usually included in growth models, which include share of capital, share of human capital, population growth, institutions or financial development, etc. *IPP* enters both models due to its effect on technical change that, in turn, enables countries or firms/industries to produce a higher level of output or achieve faster growth rates at given levels of conventional inputs.

There are several measurement and estimation issues that arise from the representative models introduced above. Foremost among these is the measurement of IP protection, which consists of qualitative legal instruments. Four different measures are utilised to convert the latter into a quantitative indicator: (i) the Ginarte and Park (1997) index of patent protection strength (45%); (ii) other IP protection indexes based on legal provisions and expert opinion surveys (25%); indicator variables reflecting domestic and/or international regime change in favour of stronger IP protection (12%); and patent count or intensity measures (17%).

[6] The dependent variable in the productivity model can also be total factor productivity (TFP), which is estimated from the Cobb-Douglas production function with conventional inputs, excluding IP protection. In this case, TFP is regressed on IP protection. The estimate of the IP protection effect should be the same in both specifications if the assumptions of perfect competition and constant returns to scale hold.

The most commonly-used measure, the Ginarte and Park (1997) index, is constructed quinquennially by coding information about the national patent laws. It considers the extent of coverage, membership in international patent agreements, provisions for loss of protection, enforcement mechanisms, and patent life (duration of the protection). Each of these features is assigned a value that ranges between 0 and 1; and the index is calculated as the unweighted sum of the five scores. Hence, the index ranges from zero (no protection) to 5 (maximum protection). Other IP protection indexes follow a similar scoring methodology, but they also include information about patenting activity and/or survey information reflecting the assessment of the business managers about the strength of IPR regime in the country. Of the remaining two, the IPR regime change indicators are usually dummy variables indicating participation in international IPR agreements or the introduction of domestic legislation or both. Finally, the patent-related measures usually reflect one or a combination of the following: patents registered, patents in force, or the ratio of granted patents to the total number of patent applications.

The multiplicity of IP protection measures raises the question of whether it is feasible/permissible to pool estimation results based on different measures of the explanatory (intervention) variable. To address this question, we draw on Lesser (2011) who reports that the correlation between the most-commonly used Ginarte and Park (1997) index and other indexes that measure the strength of trademarks and copyright protection ranges from 0.6 to 0.8. Therefore, Lesser (2011) concludes that alternative indexes can be used for evaluating trends over time, but they should be used for cross-section comparisons with caution. There is no comparative assessment of the correlation between regime change indicators and IP protection indexes. However, the two can be expected to convey correlated information as regime change feeds into the IP protection indexes through construction.

Our conclusion is that the existing measures may be heterogenous but provide overlapping information about the extent of IP protection strength they reflect. Hence, we pool evidence based on different measures and address the issue of heterogeneity by defining a categorical variable of IP protection strength, which takes values from 1 to 4, with 1 corresponding to IPR regime change that reflects the lowest protection strength; 2 corresponding to measures based on patenting activity; 3 corresponding to the Ginarte and Park (1997) index and other indexes; and 4 corresponding to IP protection indexes augmented by enforcement quality indicators such as rule of law or quality of judiciary. We utilise this categorical variable to ascertain if the effect on economic outcomes differs between low (category 1 and 2) and high IP protection strength (category 3 and 4).

Another measurement issue arises from the difference between the *log-log* and *log-linear* specifications in models 1-6 above. In some models, the effect-size estimate of interest, γ, is based on a log-log specification in models 1, 3 and 5, but on a *log-linear* specification in 2, 4 and 6. Furthermore, even when the underlying specification is log-log, primary studies tend to use an IP protection measure in levels. Overall, unit of measurement emerges as another source of heterogeneity that must be tackled before the effect-size estimates can be pooled for meta-analysis. To address this issue, we standardise the effect-size estimates by obtaining a partial correlation coefficient (PCC) and a Fisher's Z transformation of the latter – both of which reflect the strength of the correlation between IP protection and economic outcomes after controlling for other determinants. This is a common practice in meta-analysis (e.g., Stanley et al., 2018; Awaworyi Churchill et al., 2017b; Wang & Shailer, 2015), exercised at the cost of having to rely on a correlation measure instead of a causal effect size.

The third measurement issue relates to the dependent (outcome) variable. As indicated above, the primary studies investigate the effects of IP protection on four related economic outcomes: innovation, technology diffusion, productivity, and output growth. The measurement of the dependent variable in the latter two clusters is straightforward: the logarithm of real output in the productivity cluster and the growth rate per-capita GDP/output in the growth cluster. Therefore, a standardised effect-size measure is comparable within or between studies.

However, the measures used for innovation and technology diffusion differ between studies. Studies investigating the effect of IP protection on innovation utilise five types of innovation measures: (i) level or intensity of R&D expenditures as measures of innovation inputs (24%); (ii) other innovation input measures such as innovation investment dummy or probability (2%); (iii) patenting activity as a measure of innovation outputs (47%); (iv) other innovation output measures such as forward patent citations, follow-on innovations or product innovation (21%); and (v) economic complexity index that measures the countries' productive capabilities (6%). In the case of technology diffusion, the outcome is measured as FDI inflows (45%), royalty payments (15%) or international licensing probability or bilateral trade (40%).

This heterogeneity reflects the difficulty in measuring innovation and technology diffusion; and the lack of consensus on which measure is preferable/appropriate. As stated by Nelson et al. (2014), the variation in the measures does not necessarily call into question the empirical efforts. It does, however, indicate that further research on the measurement of innovation and diffusion is necessary – at least to develop a ranking of the competing measures with respect to their informational content. More to the point, however, it also indicates the need for appropriate modelling and coding of the evidence base. To address this requirement, we rely on multi-level models that allow for both dependence and heterogeneity within outcomes; and control for input and output measures of innovation as one of the outcome variables.

3. Identifying eligible studies and overview of the evidence base

Our search for eligible studies, inclusion and exclusion decisions, and data extraction protocols all follow the best-practice recommendations in Stanley et al. (2013). We began with finding primary studies from existing narrative reviews. This sample was then augmented through electronic search in Scopus, Google Scholar, the Science & Technology Management Bibliography (STMB) and EBSCO Information Services (EBSCO). We used a pre-specified list of keywords, including: "intellectual property rights", "intellectual property protection", "property right protection", "patent protection", "trademarks" combined with "economic growth", "economic development", "productivity", "innovation", "technology diffusion", and "technology transfer". The search produced 1,187 studies published between 1990 and 2019 (see, the PRISMA flow diagram in Figure 1A in the Appendix).

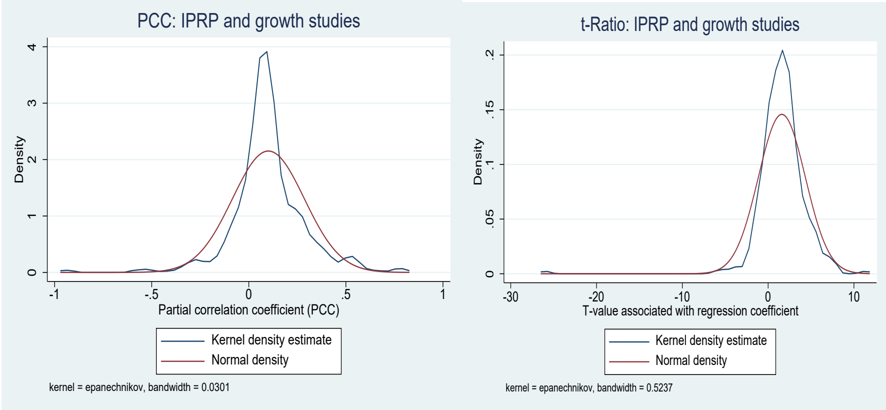
Screening the title/abstract information, we have identified 311 studies for critical evaluation based on full-text information. We excluded all theoretical papers or papers with no empirical estimates (e.g., Chu & Peng, 2011; Keely, 2001), studies with non-tractable empirical models (e.g., Bielig, 2015), and studies that report starred coefficients without standard errors or t-values (Allred & Park, 2007; Gold et al., 2019).

At the end of the full-text evaluation, we obtained a sample of 91 primary studies that reported 1,626 estimates for the effects of IP protection on at least one of the four economic outcomes (economic growth, productivity, innovation, and technology diffusion). Of these, 10 studies report multiple effect-size estimates for more than one economic outcome. For instance, Cho et al. (2015) and Falvey et al. (2006) provide estimates on both growth and innovation; Branstetter et al. (2007) on productivity and diffusion; and Hall and Sena (2017) on productivity and innovation. Hence the total number of study IDs for nesting at level 2 of the multi-level model is 101. Finally, the primary studies report multiple effect-size estimates, which range from 2 in Varsakelis (2001) to 69 in Sakakibara and Branstetter (2001).

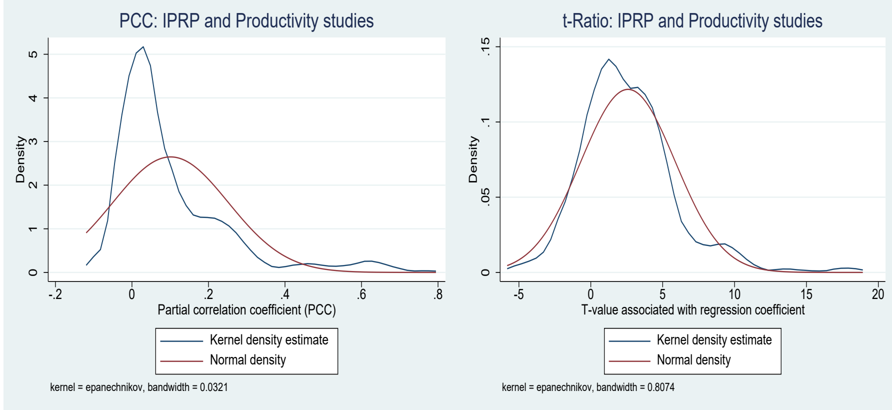
We have extracted all reported effect-size estimates to ensure complete use of existing information and avoid the risk of reviewer-induced selection bias. In order to control for observed sources of heterogeneity, we coded each estimate with several indicators that capture the following dimensions of the research field: (i) publication type and characteristics (journal article or working paper or book chapter, journal quality, multi-authors publication, funded research, etc.); (ii) publication date; (iii) model specification (underlying theoretical model, modeling for linear or interaction effects, use of time or , industry/country dummies, etc.); (iv) data and sample characteristics (whether the unit of analysis is firm, industry or country; the country origin of the data, midpoint of the data's time dimension, etc.); (v) estimation methods (fixed/random effect estimators, GMM, 2SLS, OLS, non-linear probability estimators, etc.); (vi) the economic outcome to which the effect-size estimates pertain (growth, productivity, innovation and technology diffusion); and (vii) IP protection strength measures (patent-based measures; Ginarte and Park (1997) index or equivalent, domestic regime change dummies or international regime change dummies such adoption of the Trade-Related Intellectual Property Rights -TRIPS- agreement, etc.).

Tables A1 to A4 in the Appendix provide bibliographic and content information about included primary studies, including the number of cross-section units, data period, IP protection measure(s) used, estimation method, and the mean and median of the reported effect-size estimates and associated t-values. Most studies (82%) are journal articles. The remaining includes working papers and reports (14%), and book chapters and thesis (4%). Whilst the data is at the country level in 51 studies, 11 studies draw on industry-level data and 23 studies are based on firm or affiliate data. The rest include 1 study at region level and 5 studies using different innovation outcome units, such as citation of article or drug discovery.

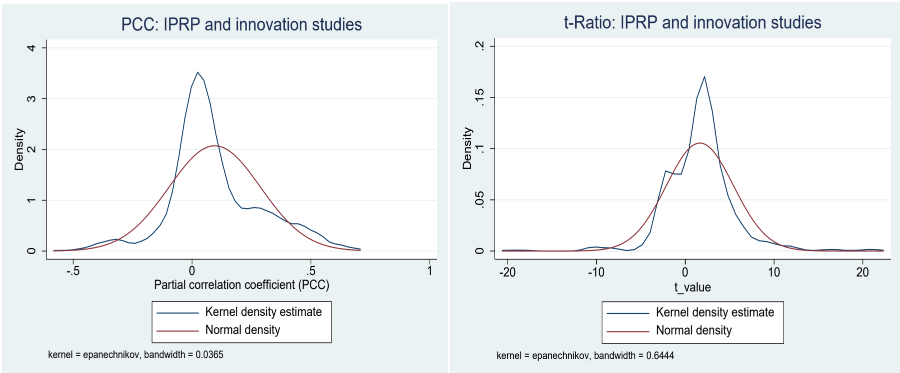
Figure 1: Kernel densities of effect-size estimates and associated t-values



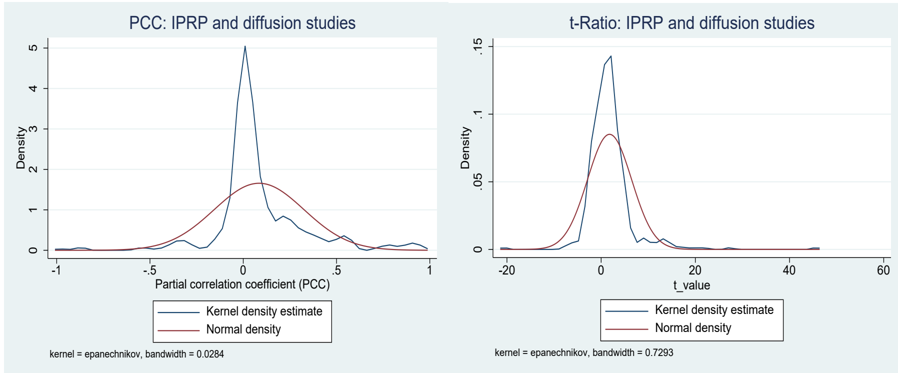
Panel A: Evidence base for IP protection and per-capita GDP growth.
Median PCC: 0.089; Median t-value: 1.590



Panel B: Evidence base for IP protection and productivity.
Median PCC: 0.049; Median t-value: 2.189



Panel C: Evidence base for IP protection and innovation.
Median PCC: 0.055; Median t-value: 1.796



Panel D: Evidence base for IP protection and technology diffusion.
Median PCC: 0.025; Median t-value: 1.448

Kernel densities of the effect-size estimates and associated t-values are displayed in *Figure 1*. Most effect-size estimates are positive, but their distribution has long tails, either to the right (for productivity and technology diffusion - *Panel B* and *D*), or to the left (for growth - *Panel A*). With regards to the t-values, the highest density is often around the value of 2 – near the cut-off point associated with the 5% level of significance. This might be a signal of publication selection, which is revealed as a sudden increase in the frequency of the reported effect-size estimates that just pass the 5% significance level.[7]

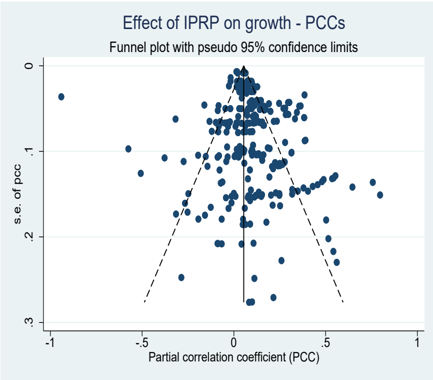
Funnel plots in *Figure 2* provide visual information about the extent of heterogeneity and the risk of publication selection bias. The graphs are based on four evidence pools for four economic outcomes of IP protection: growth, productivity, innovation, and technology diffusion. The standardised effect-size estimate consists of two measures: the partial correlation coefficient (PCCs) and the latter's Fisher's Z transformation.[8] The vertical line indicates the fixed-effect weighted mean (FEWM), estimated with weights equal to the reciprocal of the squared standard error. Asymmetric distribution around the vertical line may be an indication of publication selection bias (Egger et al., 1997; Stanley & Doucouliagos, 2012). Effect-size estimates beyond the 95% pseudo confidence interval limits (dashed lines) reflect the degree of residual heterogeneity that cannot be explained by sampling variations (Harbord & Higgins, 2008).[9]

The funnel graphs suggest that the fixed-effects weighted means (FEWMs) of the effect sizes are usually positive but very close to 0. Besides, the extent of residual heterogeneity that cannot be explained by sampling variation is quite high in all four sub-samples (around 90%).[10] Finally, the distribution of the effect sizes around the FEWM is asymmetric across all four clusters, with evident concentration to the right. This is an indication of publication selection bias, which needs to be verified further using meta-regression and other estimation methods.

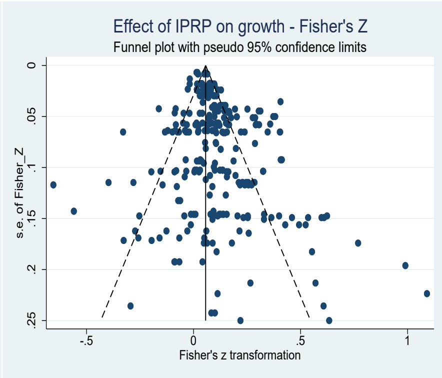
[7] See Andrews and Kasy (2019), who consider the jumps in the density of the reported estimates around the cut-off points for significance as potential indicators of selection bias.
[8] While the use of partial correlations has several advantages, its distribution might not be normal when its value gets close to +1 or -1. Fisher's z-transformation is the most common method in dealing with this problem (Stanley et al., 2018). Thus, we presented the results for both partial correlation and Fisher's z-transformation, in order to increase the robustness and reliability of our results.
[9] The funnel graphs are based on data that excludes outliers using the *dfbeta* routine in Stata.
[10] The residual heterogeneity statistic is the residual weighted sum of squares from the fixed-effects meta-regression model and is the generalization of Cochran's Q from meta-analysis to meta-regression (Harbord & Higgins, 2008).

Figure 2: Funnel graphs for effect-size distributions: PCC and Fisher's Z

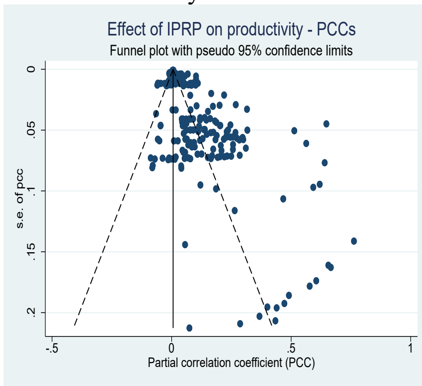
Growth effects: PCC



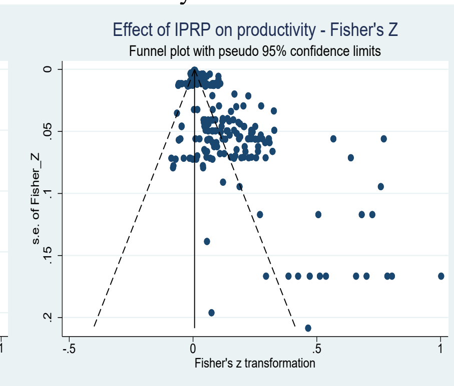
Growth effects: Fisher's Z



Productivity effects: PCC

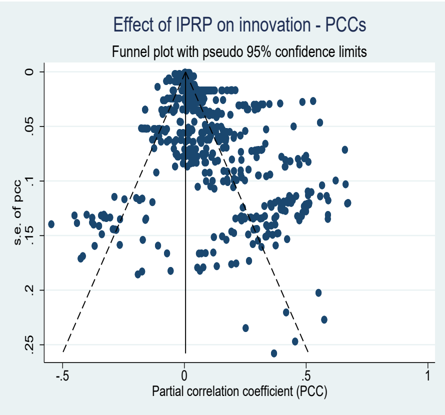


Productivity effects: Fisher's Z

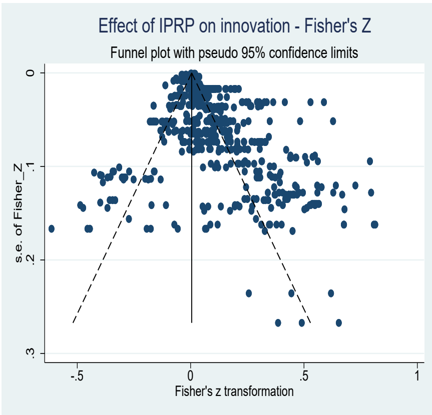


Residual heterogeneity: 85.66% 81.8% 93.4% 93.42%

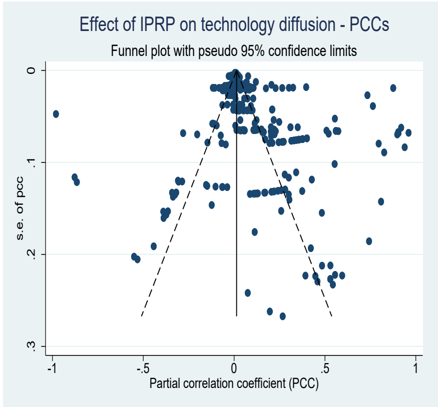
Innovation effects: PCC



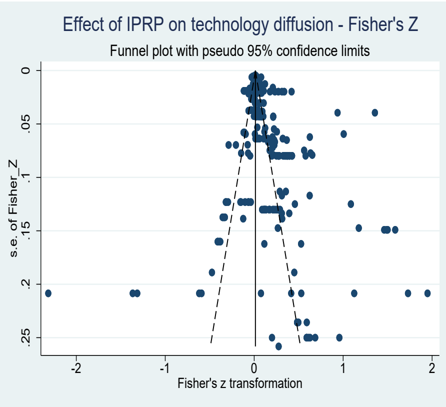
Innovation effects: Fisher's Z



Diffusion effects: PCC



Diffusion effects: Fisher's Z



Residual heterogeneity: 92.97% 93.02% 95.49% 94.03%

We begin exploring the presence of publication selection bias following Card and Krueger (1995), who regress the logarithm of the absolute t-value associated with each primary-study estimate on the corresponding logarithm of the square root of the degrees of freedom (LSRDF). The assumption here is that estimations based on larger samples should produce higher t-ratios (i.e. more precise estimates). The null hypothesis is that there is no potential publication selection if the coefficient on LSRDF is 1. Results are presented in *Table 1*, where the null hypothesis of *β=1* is rejected strongly.

Table 1: Card and Krueger (1995) test for publication selection bias

Outcomes	Coefficient (β)	SE	Wald test statistic H_0: β=1	p-value
Panel A: Estimations taking account of between-outcome dependence				
1. OLS estimation				
Growth	0.5603	0.0917	F(1,100)= 23.00	0.0000
Productivity	-0.0006	0.1563	F(1,100) = 41.00	0.0000
Innovation	0.1611	0.0765	F(1,100) = 120.14	0.0000
Diffusion	-0.1777	0.0770	F(1,100) = 234.21	0.0000
2. Multi-level model estimation with unstructured covariance				
Growth	0.4514	0.1180	chi2(1)=21.62	0.0000
Productivity	0.1071	0.1120	chi2(1)=63.54	0.0000
Innovation	0.1130	0.0806	chi2(1)=121.16	0.0000
Diffusion	-0.0805	0.0783	chi2(1)=190.59	0.0000
Panel B: Estimation based on separate outcomes				
OLS estimation				
Growth	0.5603	0.0932	F(1,22)= 22.24	0.0001
Productivity	-0.0006	0.1602	F(1, 16) = 39.00	0.0000
Innovation	0.1611	0.0771	F(1, 36) = 118.40	0.0000
Diffusion	-0.1970	0.0836	F(1, 23) = 204.82	0.0000

Notes: The dependent variable is the logarithm of the absolute value of the t-ratio, and the explanatory variable is the logarithm of the square root of the degrees of freedom. Robust standard errors are clustered at the study level. Multi-level RIS3 where the random-effect covariances are independent provides similar results, which are not reported here to save space, but are available upon request.

The findings in Table 1 suggest the t-ratio (i.e., the precision) of the reported effect-size estimates is not increasing with sample size. Although Card and Krueger (1995) indicate that the absence of a positive relationship between the t-ratio and the sample size is due to specification searching and publication selection bias, the test does not allow for direct verification of either. It just implies that, when the null hypothesis is rejected, the distribution of the reported effects in a research field may be driven by specification searching and publication selection among other factors. To eliminate this ambiguity, the test for publication selection should establish whether the effect-size estimate is related to its standard error in such a way that it reveals preference for reporting statistically significant results. Publication selection in this sense implies that statistically significant effects would be reported with a higher probability compared to the probability to be expected when the effect-size estimates are distributed randomly. Despite its mainly suggestive nature, however, the Card and Kruger (1995) test indicates that the risk of publication selection should be taken seriously and addressed appropriately in the evidence base at hand. We address the issue in section 5, where test for publication selection bias formally utilising the funnel asymmetry test (FAT) proposed by Stanley (2005; 2008) and Stanley and Doucouliagos (2012).

4. Meta-regression methodology

We propose a multi-outcome meta-regression model where effect-size estimates (level-1) are nested within studies (level-2) that, in turn, are nested within one or more of the four outcomes they investigate (level-3). This multi-level setup allows for taking account of heterogeneity and data dependence at study and outcome levels (levels 2 and 3). It has been applied in meta-analysis studies in education, psychology, and healthcare (Baldwin et al., 2014; Dessie et al., 2020; Moeyaert et al., 2017; Van den Noortgate et al., 2015). The model for estimation is based on three effect-size equations that reflect the 3-level nesting in the evidence base. Denoting the outcome pools by *k*, the primary studies by *j*, and the effect-size estimates by *i*, the three equations can be stated as follows:

Level 1: $$d_{ijk} = \beta_{jk} + \varepsilon_{ijk} \text{ , where } \varepsilon_{ijk} \sim N(0, \sigma^2_{\varepsilon_{ijk}}) \qquad (7)$$

Level 2: $$\beta_{jk} = \theta_{jk} + u_{jk} \text{ , where } u_{jk} \sim N(0, \sigma^2_{u_{jk}}) \qquad (8)$$

Level 3: $$\theta_{jk} = \gamma_k + v_k \text{ , where } v_k \sim N(0, \sigma^2_{v_{0k}}) \qquad (9)$$

Equation (7) states that the i^{th} observation of the effect-size estimates reported by study *j* for outcome *k* is equal to the population estimate β_{jk} plus a random residual ε_{ijk}. The level-2 equation (eq. 8) states that the effect-size estimate for each outcome can be decomposed into a study-specific mean (θ_{jk}) and study-specific random residual (u_{jk}). Finally, the equation for level 3 (eq. 9) states that the effect-size estimate for outcome *k* in study *j* is equal to the population mean for the outcome, γ_k, which varies randomly around a random error of v_k. All random residuals are assumed to be distributed normally with mean 0 and a variance of $\sigma^2_{\varepsilon_{ijk}}$, $\sigma^2_{u_{jk}}$ or $\sigma^2_{v_k}$, respectively. Substituting equations (8) and (9) into (7), we can see that the observed effect-size estimates for an outcome, d_{ijk}, is equal to an outcome-specific mean (γ_k), subject to an idiosyncratic error (ε_{ijk}) and random variations between studies (u_{jk}) and between outcomes (v_{0k}).

$$d_{ijk} = \gamma_k + u_{jk} + v_k + \varepsilon_{ijk} \qquad (10)$$

This specification in (10) is valid only if publication selection bias is assumed away. This assumption, however, is not realistic given the preliminary evidence on publication selection presented above. To verify whether publication selection exists, we build on Egger et al. (1997) who postulate that researchers search across model specifications, econometric techniques and data samples to find sufficiently large (hence statistically-significant) effect-size estimates. If publication selection exists, the observed effect-size estimates reported in primary study j for outcome k (d_{ijk}) will deviate from the 'true effect' due to a random error (ω_{ijk}) and the confounding effect of the standard error – as stated in (11) below.

$$d_{ijk} = \beta_k + \alpha_k SE_{ijk} + \omega_{ijk} \qquad (11)$$

Here β_k is the outcome-specific estimate of the average effect over j studies, SE_{ijk} is the standard error reported in the primary studies, and ω_{ijk} is the idiosyncratic error term. The test for publication selection has the null hypotheses that $\alpha_k = 0$. If the null hypothesis is rejected, we infer publication selection in that the reported effect-size estimates are correlated with the standard error. In other words, there is ground to conclude that the reported effect-size estimates become larger in magnitude as the standard error gets larger so that the effect is statistically significant. Model (11) can also be used to test for the average effect in each outcome by testing $\beta_k = 0$. Nevertheless, the Egger et al. (1997) model suffers from heteroskedasticity as the variance of the error term is dependent on the standard error term, SE_{ijk}. This inherent heteroskedasticity is addressed through a weighted least squares (WLS) estimator, using precision-squared ($1/SE_i^2$) as analytical weight. This is equivalent to dividing both sides of (11) with the standard error (Stanley, 2008; Stanley & Doucouliagos, 2012; Stanley, 2005), leading to:

$$t_{ijk} = \alpha_k + \beta_k \left(1/_{SE_{ijk}} \right) + \varphi_{ijk} \qquad (12)$$

Here t_{ijk} is the t-value associated with the effect-size estimate i reported in primary study j for outcome k; β_k is the outcome-specific average effect over j studies, and φ_{ijk} is the error term in (11) divided with the standard error. Testing for 'true effect' (*H₀:* $\beta = 0$) is referred to as precision-effect test (*PET*), whilst testing for (*H₀:* $\alpha = 0$) is the funnel asymmetry test (*FAT*) for publication selection. According to Doucouliagos and Stanley (2013, p. 320), the selection bias is modest if $|\alpha| < 1$; it is substantial if $1 \leq |\alpha| \leq 2$; and severe if $|\alpha| > 2$.

Most meta-analyses studies of economics research and related research fields focus only on a single outcome (e.g., Alptekin & Levine, 2012; Awaworyi Churchill & Mishra, 2018; Awaworyi Churchill & Yew, 2018; Doucouliagos & Ulubasoglu, 2008; Ugur et al., 2018). Hence within-outcome dependence and between-outcome heterogeneity does not arise as issues to be addressed. Moreover, most meta-analysis studies focusing on a single outcome either overlook the issue of within-study dependence or take account of the latter through clustered standard errors (e.g., Alptekin & Levine, 2012). Although some meta-analysis studies take account of within-study dependence and between-study heterogeneity through multi-level modelling (e.g., Awaworyi Churchill et al., 2017a; Ugur et al., 2018; Ugur et al., 2020; Ugur et al., 2016), the two-level meta-regression models in these studies are inadequate for synthesizing the evidence on the economic benefits of IP protection, where we have multiple effect-size estimates for multiple outcomes that are theoretically related.

To address this issue, we augment the *PET-FAT* model in (12) with random-effect components that reflect the random variation and dependence between the effect sizes at the primary study and/or outcome levels. This is formalized in Model (13) below, where we have a three-level mixed-effect model specified in such a way as to allow for outcome-specific average ('fixed') estimates for publication selection (α_k) and for the 'true effect' effect (β_k). These 'fixed' estimates are obtained after study-specific and outcome-specific 'random effects' are estimated (see below). The three-level *PET-FAT* model for k outcomes can be stated as follows:

$$t_{ijk} = \delta_k' \alpha_k + Precision_{ijk}' \beta_k + u_{0k} + u_{1k} Precision_{ijk} + u_{0jk} + u_{1jk} Precision_{ijk} + \vartheta_{ijk} \quad (13)$$

In (13), δ_k' is a set of dummy variables that identify the four outcomes in the research field. Of the random-effect components, u_{0jk} and u_{1jk} are random effects modelled as random intercepts and random slopes at the study level. Similarly, u_{0k} and u_{1k} are random effects modelled as random intercepts and random slopes at the outcome level. The random-effect components are distributed with zero mean and constant variances as follows:

$u_{0jk} \sim N\left(0, \sigma^2_{u0_{jk}}\right)$; $u_{0k} \sim N\left(0, \sigma^2_{u0_k}\right)$; $u_{1jk} \sim N\left(0, \sigma^2_{u1j_k}\right)$; $u_{1k} \sim N\left(0, \sigma^2_{uj_k}\right)$

Of the coefficient estimates, α_k is the 'fixed' estimate for publication selection bias in the evidence pool for outcome *k*; and β_k is the 'fixed' average effect-size estimate for outcome *k*. These 'fixed' parameters of interest are estimated by assuming that the outcome-specific and study-specific random effects are at their zero mean. On the other hand, the random-effect components are estimated separately and stored for the purpose of post estimation tests. One such test involves the best linear unbiased predictions (BLUPs) of the selection bias and/or effect-size estimate by outcome and/or by study. Such post-estimation exercises allow for establishing the extent to which the 'fixed' selection-bias and effect-size estimates (α_k and/or β_k) vary by outcome and/or by study when the random effects are taken into account.

This three-level hierarchical model is estimated without constant to obtain estimates for *publication bias* (α_k) and *average effect-size* (β_k) estimates for each outcome, taking account of the information on remaining outcomes. The proposed model minimizes the risk of Type I error (incorrect inference) by taking account of within-outcome dependence and between-outcome variation, which are ignored when meta-analysis is conducted with separate data for each outcome (Becker, 2000).

The multi-level estimator allows for 4 different covariance structures between the random-effect components: independent, exchangeable, identity, and unstructured. The independent covariance structure allows for a distinct variance for each random effect and assumes that all covariances are 0. The exchangeable structure specifies one common variance for all random effects and one common pairwise covariance. In the case 'identity' structure, all variances are equal and all covariances are 0. Finally, in the 'unstructured' variance-covariance case, all variances and covariances are distinct. The choice of the covariance structure needs to be verified through likely-hood ratio (LR) tests, where the null hypothesis is that the 'more restricted' specifications such as 'identity' or 'independent' is nested within the less restricted specification such as 'exchangeable' or 'unstructured'. Rejection of the null indicates preference for the less restrictive covariance structure.

We also conduct likelihood ratio (LR) tests to verify whether random intercepts and random slopes are appropriate at the outcome or study level or both. The null hypothesis is whether the variances of all or part of the random-effect parameters (u_{0jk}, u_{0k}, u_{1jk}, and u_{1k}) are significantly different than zero. In other words, the LR tests allow for deciding whether random intercepts and slopes exist at the study or outcome-pool levels or both.[11]

The proposed modeling strategy makes full use of the information with respect to four outcomes before estimating average effect sizes for each outcome pool separately. According to Van den Noortgate et al. (2013), one advantage of the multi-level meta-regression model we adopt in this study is the flexibility it allows for modeling more or fewer levels of nesting in the data.[12] Secondly, the proposed approach allows for comparison between an intervention's effects on separate but related outcomes. This comparability is ensured by taking account of all available information about the investigated outcomes. Third, the multi-level model does not require prior knowledge of the sampling covariance, in contrast to multi-variate or Bayesian meta-regression models that require prior knowledge (see, also Moeyaert et al., 2017).In addition, simulation work by Van den Noortgate et al. (2013) indicates that the multi-outcome meta-regression model provided the most accurate standard errors and interval estimates of treatment effects in two scenarios where the sampling covariance is zero or large.

Nevertheless, the proposed model could yield biased estimates unless two sources of endogeneity are addressed. We conduct two tests to verify whether Type-1 and/or Type-2 endogeneity exists. Whereas Type-1 endogeneity may exist due to correlation between the random-effects at the study level (u_{0jk} or u_{1jk}) and the regressor ($Precision_{ijk}$); Type-2 endogeneity may exist if the idiosyncratic error (ϑ_{ijk}) is correlated with the regressor ($Precision_{ijk}$). To address Type-1 endogeneity, we followed Mundlak (1978) by augmenting the model with within-study means of the regressor.[13] To address the Type-2 endogeneity, we instrumented $Precision_{ijk}$ with the square-root of the sample size. When Type-2 endogeneity is established, i.e., when *Precision* is endogenous, we have used the predicted value (*Precison_hat*) obtained from the first stage of a two-stage least squares (2SLS) estimator.

To uncover how observable sources of heterogeneity may influence the effect-size estimate, we augment the bivariate model in (13) with a set of M binary moderating variables (Z_m) multiplied with

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[11] In the results section, we report that the LR tests indicate random intercepts at the study and outcome levels, but random slopes only at the study level only.

[12] For example, the proposed model can allow for four levels of nesting if the meta-analyst wishes to take account of dependence and heterogeneity that may be due to the country or institution or funding sources of the primary-study authors.

[13] In the results section, we report that Mundlak tests indicate no correlation between the study-level random effects and the regressor (*Precision*). Had correlation existed, correct estimation would have been obtained by including the within-study mean of the regressor as an additional regressor in the model.

$Precision_{ijk}$. The coefficient (γ_m) on the interaction term (Z_m*$Precision_{ijk}$), if significant, indicates the extent to which the dimension of the research field captured by moderating variable is associated with larger or smaller effect size compared to the average effect size. The resulting model is a *multi-outcome multi-variate meta-regression model (MO-MRM)*, which allows for potential dependence between related outcomes and can be stated as follows:

$$t_{ijk} = \delta_k' \alpha_k + Precision_{ijk}' \beta_k + u_{0jk} + u_{1jk} Precision_{ijk} + u_{0k} + u_{1k} Precision_{ijk} + \sum_m Z_m * Precision_{ijk}' \gamma_{km} + \epsilon_{ijk} \quad (14)$$

In (14), the moderating variables (Z_m) are binary indicators that indicate whether the underlying primary-study evidence is reported in a journal article or based on a specific IP protection measure or obtained through a particular estimation method (e.g., instrumental variable estimation). The coefficient on the interaction term (γ_{km}) is estimated for each outcome and each moderating variable. We have coded for 18 moderating variables, which capture different dimensions of the research field as described in *Table A5* in the *Appendix*. The construction of the moderating variables, on the other hand, is explained in *Box A1* in the *Appendix.*

Given model uncertainty (i.e., lack of prior knowledge about which of the moderating variables should enter the MO-MRM) and the potential for high levels of multi-collinearity, we have adopted a model averaging routine based on weighted-average least squares (WALS) to identify the *specific MO-MRM* to be estimated. WALS is a combination of Bayesian and frequentist approaches to model selection (De Luca & Magnus, 2011; Havranek et al., 2017), where covariates are included in the specific MO-MRM if they are associated with absolute t-values of |1| or greater in the WALS estimation. As discussed in Ugur et al. (2020), WALS performs as well as the Bayesian model averaging (BMA) routine and requires much less computation time. In the model selection process, we use *Precision* as the focus regressor and allow for model uncertainty by treating all moderating variables as auxiliary regressors. For comparison purposes, however, we also report estimation results based on the *general MO-MRM* in *Table A9* in the *Appendix*.

After estimating the specific MO-MRM, we have explored several specifications for the random effects and their variance-covariance structures. Relying on likelihood ratio (LR) tests, we have chosen random intercepts at the study and outcome levels, combined with random slopes at the study level; and an unstructured variance-covariance structure as the preferred specifications. The coefficient estimates from the MO-MRM are then used to obtain meta-effects under different scenarios for sources of heterogeneity and 'best practice' in the research field. The meta-effect for each outcome k is obtained in accordance with (15) below, where $m = 1, 2, 3, .. M' < M$ is the sub-set of moderating variables that delineate the sources of heterogeneity and/or best-practice scenario.

$$Meta\_Effect_k = \partial t_{ijk} \Big/ \partial Precision_{ijk} = \beta_k + \sum_m \gamma_{km}, where\ m = 1, 2, 3, .. M' < M \quad (15)$$

We verify the significance of the meta-effect through a Wald test, where the null hypothesis is that the sum of the coefficient estimates is zero.

5. Meta-regression results

In this section, we first present the average effect-size estimates for four related outcomes (economic growth, productivity, innovation and technology diffusion), based on the bivariate multi-outcome meta-regression model (13). These estimates take account of publication selection bias, *unobserved* heterogeneity, and within-study as well as within-outcome correlation (dependence) between primary-study findings. This will be followed by evidence from the MO-MRM (eq. 14) that, in addition to the above, also takes account of *observable sources* of heterogeneity by controlling for the variations in study characteristics. Evidence from the MO-MRM allows for qualified (scenario-specific) inference – i.e., it allows for addressing questions about what the effect size would be when certain dimensions of the research field are considered as observable sources of heterogeneity.

5.a Bivariate meta-regression evidence

Bivariate meta-regression model estimates for the effects of IP protection on economic growth, productivity, innovation and technology diffusion are presented in *Table 2*. *Panel A* reports average effect-size and publication selection bias estimates, using partial correlation coefficients (PCCs) as the standardised measure of the effect sizes reported in primary studies. *Panel B* reports the results based on Fisher's Z transformation of the PCC. All results obtained from a 3-level hierarchical model with random intercepts at the outcome cluster level, and random intercepts and slopes at the study level – as justified by LR tests. In each panel, columns A1 and B1 report estimates based on an unstructured covariance matrix for the random effects, whereas in columns A2 and B2 the random-effect variance-covariance matrix is independent.[14] The log likelihood statistics and other fit statistics such as AIC and BIC favour the estimates in panel B, where the underlying effect size is the Fisher's Z transformation of the partial correlation coefficient (PCC).

Table 2 – Effects of IP protection on growth, productivity, innovation, and technology diffusion: Bivariate estimates from the multi-outcome meta-regression model

	Panel A: PCCs		Panel B: Fisher's Z	
Dependent variable: t-value	(A1)	(A2)	(B1)	(B2)
Effect-size estimate (β) for:				
Growth (Nesting 23 study IDs that report 289 effect-size estimates)	0.0359	0.0365	0.0371	0.0370
	(0.0241)	(0.0240)	(0.0236)	(0.0236)
Productivity (Nesting 17 study IDs that report 279 effect-size estimates)	0.0161	0.0159	0.0142	0.0142
	(0.0195)	(0.0194)	(0.0191)	(0.0191)
Innovation (Nesting 38 study IDs that report 673 effect-size estimates)	-0.0052	-0.0052	-0.0042	-0.0042
	(0.0142)	(0.0141)	(0.0141)	(0.0141)
Diffusion (Nesting 23 study IDs that report 381 effect-size estimates)	0.0444***	0.0437***	0.0487***	0.0488***
	(0.0166)	(0.0165)	(0.0164)	(0.0164)
Publication bias estimate (α) for:				
Growth (Nesting 23 study IDs that report 289 effect-size estimates)	0.5571	0.5494	0.6411	0.6435
	(0.8294)	(0.8363)	(0.6989)	(0.6975)
Productivity (Nesting 17 study IDs that report 279 effect-size estimates)	2.4820**	2.4888**	2.7485***	2.7478***
	(1.0916)	(1.1009)	(0.9173)	(0.9155)
Innovation (Nesting 38 study IDs that report 673 effect-size estimates)	1.9853***	1.9799***	1.9768***	1.9776***
	(0.7163)	(0.7207)	(0.6000)	(0.5992)
Diffusion (Nesting 23 study IDs that report 381 effect-size estimates)	2.2010***	2.2200***	1.6127**	1.6097**
	(0.8340)	(0.8413)	(0.6958)	(0.6943)
Total observations	1622	1622	1622	1622
Total studies (study IDs)	91(101)♣	91(101)♣	91(101)♣	91(101)♣
Log-likelihood (LL)	-4100.2	-4100.2	-3965.2	-3965.2
LR test: comp. mod. Preferred ($p > chi^2$)	0.000	0.000	0.000	0.000
AIC	8224.4	8224.5	7954.4	7954.4
BIC	8289.1	8289.1	8019.1	8019.1

Notes: All columns report estimates based on a 3-level hierarchical model with random intercepts at the outcome level (level 3) and random intercepts and slopes at the study level (level 2). The variance-covariance structure of the random effects is unstructured in columns (A1) and (B1), which are preferred by the likelihood ratio (LR) test. Columns (A2) and (B2) report results based on an independent variance-covariance structure as sensitivity checks. ♣The estimation sample is based on effect-size estimates reported by 91 individual studies, of which 10 studies report estimates for more than one outcome. Hence, the total number of study IDs is 101. The number of study IDs and effect-size estimates nested within each outcome cluster (growth, productivity, innovation and technology diffusion) are reported in column 1 next to each outcome. Robust standard errors are clustered at the study level. ***, **, * indicates significance at 1%, 5% and 10%, respectively.

[14] The preferred variance-covariance structure is unstructured (columns A1 and B1), but we also report estimates based on independent variance-covariance structure (columns A2 and B2). Unstructured covariance structure allows for distinct variances and covariances for all random effects. Independent covariance structure allows for distinct variances for but 0 covariance between random effects.

Results in *Table 2* indicate that publication selection bias (α) is substantial in the evidence pool on innovation; and severe in the productivity and technology diffusion pools (see, Doucouliagos and Stanley (2013, p. 320)). In contrast, publication selection is modest but statistically insignificant in the evidence pool concerning the effect of IP protection on per-capita GDP growth. We are of the view that the latter result may be due to low statistical power of the funnel asymmetry test (FAT) based on the bivariate meta-regression model, as indicated in Stanley (2008). Complementing the findings in *Table 2* with visual evidence from the funnel graphs in *Figure 2* and with suggestive evidence based on Card and Krueger (1995) tests in *Table 1*, we conclude that selection bias is a pervasive issue in this field of research. Therefore, summary measures or vote-counting exercises typically used in narrative/qualitative reviews or advocacy literature cannot be relied upon to establish the 'true' effect of IP protection on any of the related outcomes.[15]

With respect to effect-size estimates (β), *Table 2* indicates that the effects on innovation, productivity and per-capita GDP growth are statistically insignificant after controlling for selection bias. The effect on technology diffusion, which ranges from 0.044 to 0.049, is statistically significant but too small to be practically significant.[16] Hence our second conclusion is that, after controlling for selection bias, the existing evidence lends support to IP protection pessimism reflected in narrative reviews (Boldrin & Levine, 2013; Chang, 2001; De Beer, 2016; Gallini, 2002; Hall & Harhoff, 2012). It also indicates that the IP protection optimism reflected in the advocacy literature (e.g., Dixon, 2011; OECD, 2015; WIPO, 2015) is not warranted.

Before controlling for moderating factors (i.e., before taking account of observable sources of heterogeneity) in accordance with model (14), we have investigated whether the small effect on *technology diffusion* varies between different samples that compare countries with respect to the level of development and patenting intensity. Our findings, reported *in Table A6* in the Appendix*,* indicate that the positive effect on technology diffusion is driven by primary-study evidence based on data from *non-OECD*, *developing* and *low-patenting-intensity* countries.[17] In contrast, the effect on technology diffusion is insignificant; and the effect on innovation is mostly negative when the

[15] It must be noted that the potential bias in summary measures does not decrease as the number of effect-size estimates evaluated increases (Stanley, 2008: 104).
[16] This evaluation is based on criteria proposed by Doucouliagos (2011), according to which IP protection explains less than one-fourth of one percent of the variance in technology diffusion after controlling for the latter's other determinants.
[17] Country classification in terms of development is based on the United Nations classifications whereas classification in terms of patenting intensity is based on World Intellectual Property Indicators published annually by the World Intellectual Property Organization (WIPO) (WIPO, 2015). Effect-size estimates reported in primary studies are coded in accordance with the level of development or patenting intensity that holds in the year that corresponds to the mid-point of the time horizon in the primary-study data.

evidence relates to OECD countries, developed countries including OECD and non-OECD countries, and high patenting-intensity countries.

We have also checked whether the full-information meta-regression results in *Table 2* differ from separate-outcome estimations. Comparing *Table 2* with *Table A7* in the Appendix, we observe that the absence of significant effects remains the norm in both Tables. This is the case irrespective of whether the separate-outcome estimations are based on OLS, fixed effects or two-level hierarchical model estimators. Despite the similarity, we prefer the multi-outcome hierarchical model estimations in Table 2 as they are based on full information about four economic outcomes of IP protection and, in contrast to separate-outcome estimations, allow for comparison between effect-size estimates across outcomes.

The evidence so far indicates that IP protection is not effective in spurring economic growth, productivity, or innovation. The effect is small in the case of technology diffusion, and this is driven by larger effects in non-OECD, low patent-intensity and developing countries. Furthermore, the informational value of the effect on technology diffusion is limited as the latter is proxied either by FDI flows that may or may not reflect 'true' technology diffusion (Eaton & Kortum, 1996).[18] Hence, our conclusion is that IP protection delivers practically insignificant or no economic benefits after taking account of *unobserved heterogeneity* and selection bias in the evidence base. In what follows, we will investigate whether the meta-effect differs when we account for *observable sources of heterogeneity* explicitly through a multivariate meta-regression model.

5b. Multivariate meta-regression evidence

Multivariate meta-regression results are reported in *Table 3*, using the Fisher's Z transformation of the partial correlation coefficient as the standardised effect size. While column (1) reports estimates based on an *unstructured* variance-covariance structure, estimates column (2) reports estimates based on an *independent* variance-covariance structure as sensitivity checks. The preferred results are in column (1), based on LR tests and better model fit in terms of log-likelihood, AIC, and BIC. However, we also take account of the findings in column (2) given that the model fit in column (1) is only

[18] Eaton and Kortum (1996: 401) draw attention to the following: "Inferring the pattern of international technology diffusion implied by the pattern of international patenting and other data requires a number of specific modeling assumptions about production technologies, market structure, and inventor behavior, none of which is easy to verify directly."

fractionally better. Hence, we consider a research dimension to have a significant effect on the variation in effect-size estimates if the corresponding moderating variable is significant in both columns.

Table 3: Three-level Multivariate Meta-regression Results based on Fisher's Z

		(1) Unstructured covariance structure	(2) Independent covariance structure
Dependent variable: t-value			
	Precision		
	Growth cluster	0.0192	0.0196
		(0.0991)	(0.0990)
	Productivity cluster	0.0191	0.0161
		(0.0345)	(0.0356)
	Innovation cluster	0.00418	0.00373
		(0.0162)	(0.0168)
	Diffusion cluster	0.0757	0.0817
		(0.0618)	(0.0626)
	Publication bias		
	Growth cluster	-0.00604	0.00915
		(0.929)	(0.900)
	Productivity cluster	2.172**	2.231**
		(1.000)	(0.964)
	Innovation cluster	1.582**	1.616**
		(0.713)	(0.693)
	Diffusion cluster	0.737	0.741
		(0.770)	(0.742)
Moderating variables			
	Primary study is journal article		
	Growth cluster	0.00636	0.00624
		(0.0531)	(0.0532)
	Productivity cluster	-0.0114	-0.0103
		(0.0269)	(0.0278)
	Innovation cluster	-0.00251	-0.00222
		(0.0163)	(0.0168)
	Diffusion cluster	-0.0194	-0.0224
		(0.0136)	(0.0150)
	Funded research		
	Growth cluster	0.00982	0.00878
		(0.0652)	(0.0648)
	Productivity cluster	-0.0245	-0.0236
		(0.0267)	(0.0277)
	Innovation cluster	-0.00184	-0.00520
		(0.00862)	(0.0102)
	Diffusion cluster	0.00632	-0.00485
		(0.0576)	(0.0586)
	Empirical model informed by theory		
	Growth cluster	-0.0208	-0.0213
		(0.0535)	(0.0535)
	Productivity cluster	-0.00331	-0.00547
		(0.0155)	(0.0162)
	Innovation cluster	-0.00119	-0.00695
		(0.0186)	(0.0199)

Diffusion cluster	-0.0652	-0.0590
	(0.0434)	(0.0438)
Innovation input measure		
Innovation cluster	-0.0187**	-0.0191**
	(0.00883)	(0.00889)
Data mid-point after 1996		
Growth cluster	-0.00478	-0.00507
	(0.0260)	(0.0260)
Productivity cluster	-0.0100	-0.00683
	(0.0155)	(0.0163)
Innovation cluster	-0.00220	-0.00183
	(0.0143)	(0.0148)
Diffusion cluster	-0.0160	-0.0170
	(0.0173)	(0.0185)
Unit of analysis: country/region		
Growth cluster	0.0278	0.0288
	(0.0413)	(0.0407)
Productivity cluster	0.109***	0.110**
	(0.0422)	(0.0429)
Innovation cluster	0.0795*	0.0758*
	(0.0410)	(0.0409)
Diffusion cluster	0.200***	0.196***
	(0.0260)	(0.0265)
Data averaging		
Growth cluster	0.0125	0.0119
	(0.0531)	(0.0532)
Productivity cluster	0.0552	0.0528
	(0.177)	(0.175)
Innovation cluster	0.0153	0.0127
	(0.0305)	(0.0311)
Diffusion cluster	0.0124	0.0145
	(0.0120)	(0.0126)
Geographic origin of data: China		
Productivity cluster	0.0299	0.0278
	(0.0269)	(0.0281)
Innovation cluster	0.0283**	0.0277*
	(0.0141)	(0.0149)
Diffusion cluster	0.119*	0.110*
	(0.0634)	(0.0639)
Geographic origin of data: India		
Innovation cluster	0.0211	0.0204
	(0.0143)	(0.0149)
Diffusion cluster	-0.429**	-0.429**
	(0.201)	(0.201)
Stronger IP protection measure		
Growth cluster	0.0350	0.0339
	(0.0459)	(0.0460)
Productivity cluster	-0.000114	-0.000161
	(0.00233)	(0.00233)
Innovation cluster	-0.0270***	-0.0211**
	(0.00881)	(0.0105)
Diffusion cluster	-0.0613	-0.0659
	(0.0574)	(0.0576)
Estimator: IV		
Growth cluster	0.00904	0.00911
	(0.0176)	(0.0176)
Productivity cluster	0.000979	0.00101
	(0.00325)	(0.00325)
Innovation cluster	0.00660	0.00667

	(0.0102)	(0.0103)
Diffusion cluster	-0.0274	-0.0154
	(0.116)	(0.115)
N	1619	1619
Primary studies	91♣	91♣
p-value	0.000	0.000
Log-likelihood (LL)	-3901.6	-3906.8
AIC	7903.1	7913.6
BIC	8172.6	8183.1

Notes: ***, **, * indicates significance at 1%, 5% and 10%, respectively. Table A8 in the Appendix presents sensitivity checks based on PCC. The number of study IDs and effect-size estimates in each cluster is the same as indicated in Table 2 above.

Following this rule, we report that a large majority of the moderating variables are statistically *insignificant* sources of heterogeneity in the evidence base. This remains to be the case when the estimations are based on PCC, as reported in *Table A8* in the Appendix.[19] Therefore, we report that observable moderating factors in this research field (i.e., variations in publication, data, estimation, and sample characteristics) are *insignificant* predictors of the residual heterogeneity observed in the funnel plots (*Figure 2*), which is high and ranging between 81-95 per cent. This contrasts with findings in other meta-analysis studies in economics, where observable research dimensions are reported to have some explanatory power (e.g., Alptekin & Levine, 2012; Awaworyi Churchill & Mishra, 2018; Awaworyi Churchill & Yew, 2018; Doucouliagos & Ulubasoglu, 2008; Ugur et al., 2018). Given this contrast, we argue that residual heterogeneity in this research field is potentially due to other factors that have not been controlled for given the potentially limited set of moderating variables that are included in the specific model. Eaton and Kortum (1996) provide some insights as to what these other factors may be: differences in production technologies, market structure, and inventor behaviour that are difficult to trace and control for - both in primary studies and meta-analysis studies. Our findings suggest that the effects of these unobservable factors can explain part of the residual heterogeneity.

The statistically insignificant coefficients on the publication type suggest that journal articles are not associated with any systematic difference in effect sizes. Thus, we conclude that the 'winner's curse' highlighted by Costa-Font et al., (2013) does not hold in this research field. Stated differently, there is no evidence to suggest that journal editors accommodate more selected findings in this research

[19] Observable sources of heterogeneity remain insignificant when the multivariate meta-regression model is estimated separately for each IP protection outcome on its own. Results from separate-outcome estimations of the multivariate meta-regression model are not reported here to save space, but they are available on request. Results based on the general model are reported in Appendix Table A9 while Table A10 reports results for OLS and FE models for comparison only.

field by 'exploiting' the journals' reputation as 'research vetting' institutions. Similarly, there is no statistically significant evidence to suggest that funded research is associated systematically different effect-size estimates in this research field. This is a 'comforting' finding because it indicates the absence of what we can describe as a 'funded-research curse', which may arise when researchers tend to report selected findings that reflect the expectations of funding institutions.

We find that only a small number of moderating variables are significant in explaining effect-size heterogeneity. Of these, data at the country level, compared to data at the firm or industry level, is associated with larger IP protection effects on productivity, innovation, and diffusion. This finding raises the question of why the effects are larger when country-level is used for estimation. We are of the view that this is due to the 'small N problem' in country-level panel data. In the evidence base, the median N is 47 in primary studies based on country-level data, as opposed to a median of 716 in studies based on firm/industry data. Furthermore, 73% of the country-level effect-size estimates are based on conventional panel-data estimators, which require large number of cross-section units (N) relative to the number of time periods (T).

As demonstrated by Pesaran and Smith (1995), Eberhardt and Teal (2011) and Eberhardt et al. (2013), conventional panel data methods yield potentially inconsistent estimates in the presence of correlation between cross-sectional units. Eberhardt et al. (2013) also report that the productivity effects of knowledge capital are biased upward if cross-sectional correlation is not accounted for. Therefore, we conclude that the relatively larger IP protection effects at the country level may well be due to cross-sectional dependence, which remains unaddressed in the country-level studies that rely on panel-data methods better suited for micro-econometric evidence with large N relative to time periods (T). Therefore, the relatively larger effects associated with country-level data should be considered as a reflection of upward bias rather than genuinely larger IP protection effects.

We also find some evidence indicating that the effects of IP protection on innovation is relatively larger when the underlying data is related to China as opposed to other countries in the sample. We interpret this finding as an indication of larger IP protection effects on innovation at lower levels of development. Furthermore, we find that primary studies using stronger measures of IP protection (composite indices or indices weighted by enforcement quality) tend to report smaller effects on innovation. These findings are in line with predictions from theoretical models, which indicate that economic benefits of IP protection are more likely when the initial levels of protection or development are low (Furukawa, 2010; Hudson & Minea, 2013). They are also in line with non-linear (quadratic) effects reported in empirical studies by Kanwar and Evenson (2003), Papageorgiadis and Sharma (2016), and Qian (2007).

Two further findings from Table 3 deserve some discussion. One is the effect IP protection on technology diffusion, which is relatively larger when the underlying data is at the country level or when the data relates to China. These findings provide additional support to our conclusion stated earlier: IP protection is more likely to spur technology diffusion in developing countries and/or when the initial level of IP protection strength is low. However, they also indicate that the seemingly larger effects on technology diffusion may be just a reflection of the upward bias caused by the 'small N problem' in country-level panel data in general. Therefore, we probe the issue of diffusion and country-level data again in Table 4 below, where report meta-effects based on various research scenarios.

The other (and final) findings we want to discuss in the context of Table 4 relates to the effect of IP protection on input measures of innovation such as R&D investment as opposed to output measures such as patents. We observe that the effect is smaller when innovation is measured with input measures. This finding is in line with theoretical predictions about patent races (Dasgupta & Stiglitz, 1980, 1988) and indicates that IP protection is more likely to encourage patenting rather than additional investment in innovation – a practice that also raises questions about the quality of the registered patents.[20]

'Best-practice' meta-effect estimates

We obtain meta-effect estimates by taking the linear combination of the effect-size estimates as indicated in equation (15) above. To do this, we have defined a 'best-practice' research scenario where the effect-size estimates control for endogeneity through instrumental variable (IV) estimators, use relatively more recent data with data-mid-point in 1996 or after, and are reported in journals. We consider this combination as 'best practice' on the grounds that journal articles are subject to an external review process, data with mid-point in 1996 or after is more likely to be harmonised across countries after the TRIPS agreement, and IV methods are preferable when potential endogeneity may

[20] It is important to also note the potential limitations of measuring innovation. For instance, patents are recognized legal instruments and are increasingly identified as a strategic tool rather than an innovation tool. We are grateful to an anonymous reviewer for prompting us about this.

exist due to sample selection or simultaneity in the relationship between IP protection and estimated outcomes. Scenario (2) for 'best-practice' research augments scenario (1) with an additional criterion: primary-study estimations on based on data averaged over a period of five years or more. We consider

Table 4 – Conditional meta effects

'Best practice' research scenarios 1 to 4	Conditional meta effect - PCC	Conditional meta effect - Fisher's Z
1. Journal article, data mid-point in 1996 and after, instrumental variable (IV) estimation		
Meta IP protection effect on Growth	0.0217	0.0298
	(0.1086)	(0.0920)
Meta IP protection effect on Productivity	-0.0014	-0.0014
	(0.0051)	(0.0046)
Meta IP protection effect on Innovation	0.0112	0.0061
	(0.0191)	(0.0164)
Meta IP protection effect on Diffusion	-0.1942	0.0130
	(0.1373)	(0.1217)
2. ***Journal article, data mid-point in 1996 and after, IV estimation, and data is averaged over 5 years or more***		
Meta IP protection effect on Growth	0.0462	0.0423
	(0.0736)	(0.0630)
Meta IP protection effect on Productivity	0.0779	0.0538
	(0.1971)	(0.1776)
Meta IP protection effect on Innovation	0.0299	0.0214
	(0.0436)	(0.0359)
Meta IP protection effect on Diffusion	-0.1695	0.0254
	(0.1382)	(0.1219)
3. ***Journal article, data mid-point in 1996 and after, IV estimation, data is averaged over 5 years or more, and funded research***		
Meta IP protection effect on Growth	0.0680	0.0521
	(0.1026)	(0.0871)
Meta IP protection effect on Productivity	0.0536	0.0293
	(0.1992)	(0.1789)
Meta IP protection effect on Innovation	0.0245	0.0196
	(0.0454)	(0.0375)
Meta IP protection effect on Diffusion	-0.2075	0.0317
	(0.1415)	(0.1266)
4. ***Journal article, data mid-point in 1996 and after, IV estimation, data is averaged over 5 years or more, funded research, and theoretically informed model***		
Meta IP protection effect on Growth	0.0444	0.0313
	(0.0940)	(0.0792)
Meta IP protection effect on Productivity	0.0520	0.0260
	(0.2014)	(0.1807)
Meta IP protection effect on Innovation	0.0140	0.0184
	(0.0484)	(0.0412)
Meta IP protection effect on Diffusion	-0.2785**	-0.0334
	(0.1353)	(0.1212)
5. ***'Best-practice research in scenario (4) + country-level data***		
Meta IP protection effect on Growth	0.0675	0.0592
	(0.0896)	(0.0770)

Meta IP protection effect on Productivity	0.1715	0.1354
	(0.2121)	(0.1901)
Meta IP protection effect on Innovation	0.1010	0.0979*
	(0.0632)	(0.0549)
Meta IP protection effect on Diffusion	-0.0723	0.1668
	(0.1404)	(0.1260)

this as an additional criterion for 'best-practice' research because data averaging reduces business cycle noise and mitigates the 'small N problem' by increasing the ratio of cross-section units (N) to the time periods (T).

In scenario (3), we add funded research as an additional criterion. We consider funded research findings as a potential contributor to 'best practice' on the grounds that most of the acknowledged funders in the primary studies are public funders or charities not directly connected with the national or international IP protection institutions. The assumption here is that research funders are motivated to secure good returns to their investment and use funding competitions as a filter to identify projects with better research design. In scenario (4), we add another criterion, which requires that the empirical model used for estimation is tractable and informed by the theoretical models discussed in section 2. In scenario (5), we apply the 'best-practice' research scenario (4) to country-level data to test whether the meta-effect differs as a result of relatively larger effect-size estimates reported by country-level studies.

The conditional meta effects based on these scenarios are reported in *Table 4*. Using both PCC and the Fisher's Z transformation of the latter as standardised effect-size measures, we find that IP protection has no statically significant effect on innovation, productivity, or growth under any of the four scenarios for 'best-practice' research. The only exception in scenarios 1 – 4 is the *negative* and significant meta-effect on technology diffusion in scenario 4. This contrasts with the positive effect in Table 2, where observed sources of heterogeneity are not accounted for. The contrast lends further support to our earlier conclusion that the effect of IP protection on technology diffusion is imprecise – most probably due to poor informational content of the proxies with which it is measured. In scenario 5, where we restrict the data to the country-level, the meta-effect for innovation is positive, but highly imprecise in terms of significance and consistency between PCC and Fisher's Z measures of the effect size.

Combining all the findings above, it is safe to argue that there is no empirical support for the sanguine claims about economic benefits of IP protection encountered in the advocacy literature. Our findings indicate that IP protection may be necessary to enable innovators to appropriate the benefits of private innovation but is not sufficient to deliver higher levels of innovation, technology diffusion, productivity, or income growth.

6. Conclusions

The aim of this meta-analysis study was to establish whether IP protection delivers economic benefits by increasing innovation, technology diffusion, productivity, and economic growth as related outcomes. To achieve this aim, we extended the application of the multi-outcome multi-level meta-regression model to evidence synthesis in economics. The proposed model allows for obtaining effect-size estimates that take account of publication selection, heterogeneity, and correlation between related outcomes. It also yields effect-size estimates that can be used to compare and/or rank the effects of a given intervention on multiple but related outcomes. Our findings can be summarised as follows: (i) the effect-size estimates reported in the primary studies are highly heterogenous and contaminated with publication selection bias; (ii) effect-size heterogeneity is mainly due to unobservable factors that remain beyond the range of publication, sampling and estimation characteristics usually controlled for in meta-analysis; (iii) both bivariate and multivariate meta-regression estimates indicate that IP protection does not spur innovation, technology diffusion, productivity or income growth; and (iv) the sanguine claims about the economic benefits of IP protection voiced in the advocacy literature or some legal research are not supported by the existing evidence.

In addition to establishing where the balance of the evidence lies with respect to economic benefits of IP protection, we distil three recommendations for future research and evidence-based policy debate in this field. With respect to evidence synthesis in economics, we recommend expanding the tool kit by adopting a multi-outcome and multi-level approach to meta-regression, particularly when the evidence relates to the effects of a given intervention on multiple but related outcomes. One property of the proposed approach is that it minimizes the risk of incorrect inference by taking account of dependence and heterogeneity that may exist at the outcome or study levels or both. Secondly, it yields more accurate and precise effect-size estimates for each outcome by 'borrowing strength' from the information about other outcomes. Finally, the outcome-specific effect-size estimates are comparable and can be used for ranking the effects of the intervention across related outcomes.

Our second recommendation relates to future empirical research on economic consequences of IP protection. Our study has demonstrated that the level of residual heterogeneity that cannot be explained by sampling, publication, and estimation characteristics of the primary studies is much higher compared to other empirical research fields. We are of the view that this is in large part due to two limitations in current research: (i) inadequate modeling of the trade-off between incentive-correction and market-power effects of IP protection; and (ii) high levels of 'noise' in the informational content of the IP protection and technology diffusion measures used.

As indicated in the introduction and section 2, the potential effect of IP protection on market power of the innovator is a central issue in theoretical models, but market power remains outside the empirical models in the research field. The omission of market power from the empirical models is a potential source of endogeneity and heterogeneity, both of which can be mitigated partly by augmenting the existing models with a quadratic market-power term, which reflects the non-monotonic effects of market power on innovation in Schumpeterian models (Aghion et al., 2014; Aghion et al., 2005). Another augmentation entails interaction between market-power and IP protection measures. These 'innovations' that the relationship between IP protection and innovation, technology diffusion or growth is non-linear. Allowing for non-linearities reduces the scope for simple (but also simplistic) policy recommendations informed by evidence from mis-specified models. From the perspective of research practice, however, the non-linear specifications enable researchers to provide more information about market power as a source of heterogeneity and contingency in the economic effects of IP protection.[21]

It is also necessary to improve the informational contents of the existing IP protection measures – particularly the index measures that allow for further refinement. As indicated above, the existing indices are designed to reflect higher levels of protection the stronger are the provisions of the IPR regime in terms of coverage, compensation, patent duration, and compliance with international patent agreements. For example, in the Ginarte and Park (1997) index, each of these dimensions of the IP protection regime is assigned a value between 0 to 1; and the index is calculated as the unweighted sum of the five scores.

[21] It is important to note here that existing rightsholders and innovators significantly derive benefits from the existence or expansion of IP systems. This is evident at various levels and has been shown to be the case even for bilateral and multilateral trade agreements such as those with the US which typically include provisions for IP expansions that benefit American firms. We are grateful to an anonymous reviewer for prompting us about this argument.

Although the IP protection indices are comparable between themselves, their effects on relevant outcomes may not be comparable between countries and over time due to variations in the 'optimal' level of protection for different countries and for different years within the same country. To ensure comparability, we suggest two refinements: (i) assigning variable weights to the constituent components; and (ii) augmenting the indices with information on other dimensions of the IPR regime such as disclosure and competition policy quality. The weights for the constituent components should be higher the more likely that the provision tilts the incentive-correction and market-power trade-off in favour of the former. Alternatively, the weights can be linked to the 'effect' of each component on research and development (R&D) investment relative to its effect on patenting activity. Combining a weighting scheme with evolving information on disclosure and competition policy ensure that the IP protection indices take account of the country-specific trade-off between the incentive-correcting and market-distorting effects of intellectual property protection. This suggestion is in line with the search for optimal patent terms in Roin (2013) and Williams (2017).

The implication of our findings for evidence-based policy debate is that a 'one size fits all' IP protection policy is not optimal. This is because the effects of IP protection on related outcomes such as innovation, technology diffusion, productivity, or growth reflect a high degree of heterogeneity across countries, industries, or firms. Furthermore, positive effects in some industries, countries or time periods does not justify the sanguine claims about economic benefits of IP protection for two reasons. First, IP protection effects on related outcomes are highly heterogenous and the sources of heterogeneity are largely unobservable. Secondly, the overall effects on all outcomes are statistically or practically insignificant after controlling for unobserved or observable sources of heterogeneity. Therefore, we conclude with a call for evidence-based debate on policy reform, which should aim to introduce more flexibility and contingency in the design of the future IP protection policy.

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