Do robots boost productivity? A quantitative meta-study

Schneider, Florian

Goethe University Frankfurt

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Florian Schneider[b]

Goethe University Frankfurt

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Abstract

This meta-study analyzes the productivity effects of industrial robots. More than 1800 estimates from 81 primary studies are collected. There is strong evidence that the empirical literature on the productivity effect of robots suffers from a substantial positive publication bias. This finding is observed across all measures of productivity used in the primary literature and is robust to several modern meta-analytic estimators. Beyond publication bias, there is only limited evidence for a productivity-increasing effect of robots, which so far have exerted at best a marginal boost. My analysis of the drivers of heterogeneity among the findings of primary studies points to adjustment costs at low intensities of robot use as well as diminishing returns at more advanced levels of robotization. My findings are robust to addressing model uncertainty through Bayesian model averaging. Finally, several explanatory factors for the emergence of a productivity paradox in the context of robotics are discussed.

***Keywords*:** robots, technology, IFR, meta-analysis, publication bias, productivity, growth
***JEL codes*:** O11, O12, O14, O33, O47,

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[b]Goethe University Frankfurt and VDMA e.V., Lyoner Str. 18, 60528 Frankfurt am Main, email: florian.schneider@vdma.org

1 Introduction

One of the key lessons in economics is that increases in productivity are the prerequisite for long-term economic growth (Solow, 1956; Swan, 1956; Kaldor, 1961). Nobel laureate Paul Krugman (1997, p. 11) famously wrote: "Productivity isn't everything, but in the long run it is almost everything." He further elaborates: "A country's ability to improve its standard of living over time depends almost entirely on its ability to raise its output per worker." In line with this, continuous growth of labor productivity is the key for sustained economic growth, and a large body of the economic literature deals with productivity and its determinants (Kim and Loayza, 2019). Within this body of literature, a strong focus has been placed on the role of innovation, R&D, and new technologies for economic growth (e.g., Romer, 1990; Grossman and Helpman, 1991; Aghion and Howitt, 1992; Bresnahan and Trajtenberg, 1995; McGuckin et al., 1998; Jones, 2002; Edquist and Henrekson, 2006; Jones and Liu, 2024). The influence of investments in information and communication technologies (ICT) on productivity has attracted particular interest (Stiroh, 2005; Cardona et al., 2013; Polák, 2017; Stanley et al., 2018; Inklaar et al., 2020; Nordhaus, 2021; Growiec, 2023). Within the last decade, the pioneering studies of Jäger et al. (2015), Jäger et al. (2016), Graetz and Michaels (2018) extended the scope of examined technologies by fueling a new strand of literature that focuses on the productivity effects of industrial robots. Industrial robots are fully autonomous machines that do not require a human operator and can be re-programmed to perform several tasks such as handling or processing operations (e.g. assembling, painting, welding, cutting, or grinding) (Jurkat et al., 2022). Graetz and Michaels (2018) showed a clear positive impact of robot use on the growth in labor productivity, caused by an increase in value added not accompanied by a reduction in hours worked. Since then, many further articles have scrutinized the relationship between robot adoption and productivity, using various methods and levels of analysis, covering different countries, and reporting varying effect sizes. My quantitative meta-study on the average effect and the drivers of heterogeneity within this field of research contributes to several strands of the literature dealing with the determinants of productivity and economic growth.

First, this meta-study contributes to the longstanding academic debate on the so-called "productivity paradox" ICT (Brynjolfsson, 1993). Solow (1987) foundationally expressed in his well-known quote: "You can see the computer age everywhere but in the productivity statistics." Despite the enormous innovation, investments, and usage of ICT, little productivity growth was observed. The wealth of studies examining the growth and productivity effects of ICT has paved the way for several meta-studies in this field: Stanley et al. (2018, p. 716) analyze the effect of different types of ICT on economic growth at the country-level and conclude that ICT has made a positive contribution on average, while this effect is "unexpectedly weak". Similarly, Polák (2017) finds only a small productivity effect of ICT at the firm-level, which is more than ten times lower than the result of Stiroh (2005)'s meta-analysis after updating the literature and additionally correcting for publication bias. The link between ICT and robots consists in the fact, that the installation of robots typically involves designing a completely revised production system with a significant amount of complementary investment in ICT equipment (IFR, 2020, p. 49). Thus, my meta-analysis addresses the question whether robots can help overcome the productivity paradox of ICT.

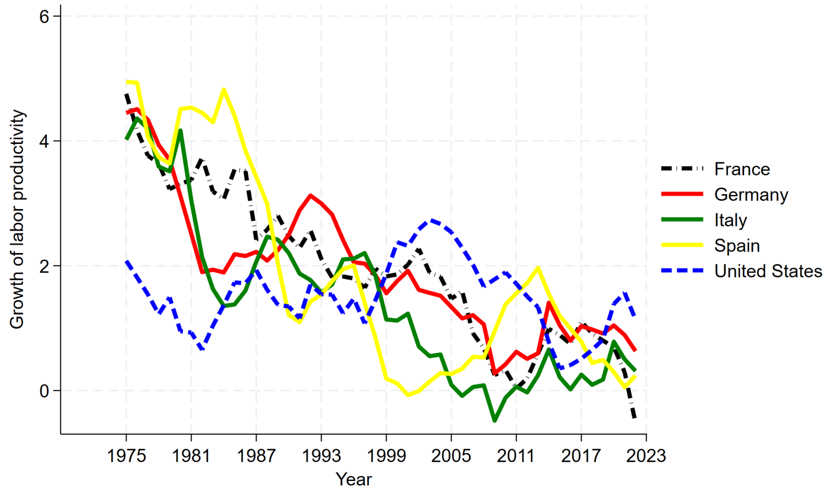
The second strand deals with the clear slowdown in productivity growth in recent decades that many advanced economies have suffered (Cette et al., 2021a; Cette et al., 2021b; Eder et al., 2023). This pattern is illustrated in Figure 1. The sluggish productivity development will likely be exacerbated by the projected reduction in the workforce due to the demographic decline in these countries (Park et al., 2021). For instance, Leitner and Stehrer (2019) estimate that the growth of labor productivity must be more than doubled to keep real GDP growth constant in face of the shrinking workforce. At the same time, robot usage has significantly increased (see Figure 1). My meta-analysis helps answer the question if robot deployment can sufficiently raise productivity to cushion the declining growth potential of economies challenged by a demographic decline.

Third, technological progress, typically measured by total factor productivity (TFP), is essential for the successful climate-neutral transformation of economies without cutting prosperity. Robotized production systems may increase efficiency enabling a reduction in material and energy inputs for the same output, thereby reducing the emission of pollutants (Chen et al., 2022; Huang et al., 2022; Li et al., 2022; Liu et al., 2022a; Liu et al., 2022b; Wu, 2023). However, efficiency gains may also be associated with reduced productions costs, increasing demand, and a upscaling of production, leading to a rebound effect that adversely affects environmental outcomes (Luan et al., 2022). Including articles in my study that focus on the "green productivity" effects of robotization contributes to the empirical question whether robots enable sustainable economic growth not only by increasing TFP and labor productivity but also through improved environmental efficiency.

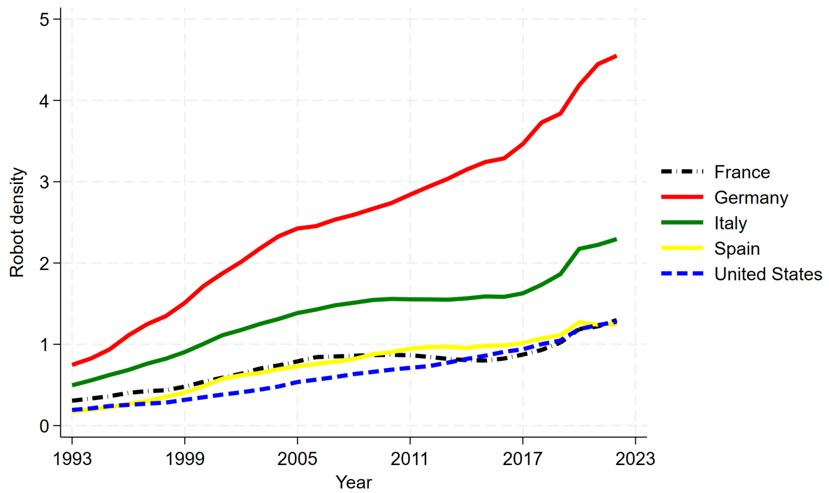
Fourth, by examining whether there are different magnitudes of productivity increases through robotization between advanced and developing or emerging economies, I contribute to explaining economic convergence. For instance, the findings of Eder et al. (2023) suggest that the contribution of robots to labor productivity growth is higher for emerging countries than for developed economies, supporting the idea of varying productivity impacts across different economic contexts.

Fifth, the multi-faceted literature on the labor market effects of automation distinguishes three main transmission channels (Acemoglu and Restrepo, 2019; Acemoglu and Restrepo, 2020; Hötte et al., 2024): 1) a displacement effect that reduces labor demand and thus wages as robots perform tasks previously done by workers; 2) a reinstatement effect comprising the creation of new tasks and occupations, as well as the reallocation of labor within and across industries or firms; 3) a productivity effect as the replacement of less cost-effective labor and technological progress increases productivity in automated tasks as well as the value-added by workers performing tasks complementary to robots. My meta-analysis enables an evaluation of the strength of this productivity effect.

In light of the rapid technological progress in artificial intelligence (AI), synthesizing the empir-



(a) Labor productivity growth (GDP per hour worked), 5-year moving average. Source: OECD (2024c), own calculations.



(b) Robot density (stock per million hours worked). Source: IFR (2023), OECD (2024a), OECD (2024b), own calculations.

Figure 1: Evolution of labor productivity growth and robot density

ical literature on the productivity effects of robots to provide answers to these five fundamental aspects of recent economic research is particularly pressing. This progress has the potential to enormously expand the scope of technological feasibility of automating tasks through AI-based robots (IFR, 2023, pp. 5). Some authors view robotization as part of a "fourth industrial revolution" (e.g., Schwab, 2016; Philbeck and Davis, 2018). Others, in turn, emphasize that robot adoption is a normal pattern of industrial development (Fernández-Macías et al., 2021) or a continued structural change towards a service and information society that was initiated by digital technologies ("third industrial revolution") (Vermeulen et al., 2018; Cetrulo and Nuvolari, 2019; Matthess and Kunkel, 2020). Assessing the strength of the productivity boost robots have exerted so far, contributes to the discussion on how revolutionary this technology may be.

My meta-study closely follows the guidelines as suggested by Havránek et al. (2020) and Irsova et al. (2023). By means of a systematic literature research in Google Scholar, JSTOR and IDEAS/RePEc using the keywords "industrial + robot + productivity", I identified 81 studies with 1813 estimates for the impact of robots on productivity. Most of these papers are examining the impact of robot use on labor productivity and TFP. Computing partial correlation coefficients ($PCCs$) as comparable effect size and employing several meta-regression models, I attempt to correct the original econometric research for publication, misspecification, and omitted-variable biases. Additionally, I scrutinize the drivers of heterogeneity in the estimated productivity effects by coding a set of moderator variables. I obtain strong evidence that the empirical literature on the productivity effect of robots suffers from a substantial to severe positive publication bias. This finding is observed across all measures of productivity used in the primary literature and robust to several modern, meta-analytic estimators. Beyond publication bias, I find only limited evidence for a productivity-increasing effect of robots. My analysis of the drivers of heterogeneity in the findings of primary studies points to adjustment costs at low intensities of robot use as well as diminishing returns at more advanced levels of robot adoption. On the one hand, this suggests that in the initial phase of robot adoption, adjustment processes like complementary investments in organizational or human capital impede the realization of productivity effects (Brynjolfsson et al., 2021). On the other hand, it implies that after passing the threshold of adjustment costs, any productivity effects of additional automation become smaller over time, i.e., result in smaller output gains or less efficiency-enhancing (or even inefficient) processes. These two aspects can be harmonized with the notion of a productivity J-curve as suggested by Brynjolfsson et al. (2021) as well as an increasing level of difficulty in automating further tasks (Acemoglu, 2024) and the deployment of "so-so technologies" with advancing automation, or inefficiencies from excessive automation as mentioned by Acemoglu and Restrepo (2019).

The remainder of my paper is structured as follows: Section 2 provides an overview of the existing empirical literature on the productivity effects of robotization and some related meta-studies in economics. Section 3 presents the process of data collection and Section 4 describes the methodology. Section 5 studies the overall effect of robot adoption on productivity and tests for publication bias. Section 6 analyzes the heterogeneity of empirical results by means of a multivariate meta-regression framework including a matrix of moderator variables. In Section 7, a number of robustness checks are implemented. Section 8 discusses several explanatory factors for the productivity paradox of robots. Finally, Section 9 concludes and provides some recommendations for future research.

2 Literature review

The strand of literature on the productivity effects of robotization was pioneered by Graetz and Michaels, 2018. Using data from the International Federation of Robotics (IFR) for 17 industrialized countries in 14 sectors from 1993-2007, they identified a clear, positive relationship between robot use and growth in labor productivity, which is caused by an increase in value added and not by a reduction in hours worked. According to Graetz and Michaels (2018), the increase in value added by the robotization of production results in price reductions on the goods markets that benefit consumers. They also show a positive link between robotization and the development of total factor productivity (TFP). Using a counterfactual analysis, the authors calculate that labor productivity would have been 5.1 percent lower on average without robotization, which implies an annual productivity growth of 0.36 percentage points for the period under investigation.

Building upon the pioneer study of Graetz and Michaels (2018), many further studies examined the productivity effects of robot adoption. The results are predominantly positive but reveal a more nuanced picture. Jungmittag and Pesole (2019) and Kromann et al. (2020) model robot usage as capital-augmenting technological progress that increases the quality of the non-ICT capital input and find a positive effect on productivity growth at the country-industry level. Acemoglu et al. (2020a), Alguacil et al. (2022), Bettiol et al. (2024), Bonfiglioli et al. (2024), Duan et al. (2023), and Koch et al. (2021) provide evidence for positive productivity effects of robot adoption at the firm level. By dividing their samples into SMEs and large enterprises, Jäger et al. (2016), Ballestar et al. (2020), and Starovatova (2023) do not find any productivity-increasing effect from robot usage among large enterprises. Bekhtiar et al. (2024) criticize the identification strategy of Graetz and Michaels (2018) and replicate their study by focusing on those industries with a significant use of industrial robots (manufacturing + mining). In the reduced sample, the effect on labor productivity is only half as large as the results published by Graetz and Michaels (2018) and the price effects lose their statistical significance. Almeida and Sequeira (2023) fail even to find any significant productivity-enhancing effect in the manufacturing sector. Almeida and Sequeira (2024) extend Graetz and Michaels (2018)'s empirical analysis by covering more countries and a longer timer period from 1997 to 2017. They find that the productivity effects from robotization have markedly decreased in the more recent period from 2008 to 2017.

According to Fu et al. (2021), the positive effect of robots on labor productivity is limited to industrialized countries and cannot be proven in developing countries. In contrast, Eder et al.

(2023) find that the contribution of robots to labor productivity growth is higher for emerging countries than developed economies, thereby fostering economic convergence. Also, quite a number of studies report positive productivity effect for developing or emerging countries, e.g., Rodrigo (2021) for Brazil, Calì and Presidente (2022) for Indonesia, Starovatova (2023) for the Russian Federation, as well as Wu (2023), Zhang et al. (2023b), Zhao et al. (2024), and Wang et al. (2024) for China. Moreover, Capello et al. (2022) do not detect any influence of the use of robots on labor productivity in 260 regions of 24 European countries in the period from 2013 to 2017. Following the "Solow paradox" (Solow, 1987) in the ICT context, they therefore speak of a "modern Solow paradox". As the main reason behind this paradox, Capello et al. (2022) identify a negative reinstatement effect: labor is reallocated from the manufacturing sector to less productive sectors. Similarly, Park et al. (2021) do not find evidence that robot adoption directly raises productivity in the Republic of Korea.

As Stiebale et al. (2024) show at the industry level, robotization increases the productivity, profit margins, and total profits of firms with the highest initial productivity, profit margins, or profitability, but has insignificant or negative effects on the other firms in the respective industry. They conclude that the use of robots primarily drives market concentration through the emergence of "superstar firms" and thus prevents possible price reductions. Almeida and Sequeira (2024), however, employ a quantile regression approach and find a stronger, positive effect of robotization on labor productivity in country-industry pairs with lower productivity. Almeida and Sequeira (2023) additionally estimate the productivity effects for different intensities of robot use and document significantly negative effects for the lowest intensity and null effects for the highest intensity, while the strongest link between robotization and productivity gains is found in the middle of the distribution.

From a theoretical point of view, several mechanisms linking robot usage and productivity can be distinguished. In a standard Cobb-Douglas production function with constant returns to scale, changes in output are determined by variations in the production factors, labor and capital, or a change in TFP. Dividing output by the labor volume (hours worked) results in labor productivity, which can be increased through three main mechanisms (GCEE, 2016, pp. 284–85). Firstly, it can result from raising capital per hour worked, i.e., capital deepening. This illustrates the direct link between investments in equipment such as ICT or robots and labor productivity. If robots displace workers from tasks previously performed by human labor, the capital deepening effect will be particularly pronounced and may allow firms to benefit from cost-savings by substituting relatively more expensive labor (Acemoglu and Restrepo, 2019). In a task-based model, one can additionally capture a productivity-enhancing reinstatement effect as automation creates new tasks that exploit the comparative advantage of labor (Acemoglu and Restrepo, 2019; Acemoglu and Restrepo, 2018c; Acemoglu and Restrepo, 2018b). Secondly, an increase in TFP can boost labor productivity. TFP is supposed to measure technological progress as a result of process and product innovation that enables producing more with the same amount of input factors. Robotic manufacturing systems may be used to increase the efficiency of production processes (e.g., by increasing accuracy, avoiding human errors, or improving management efficiency), to realize product innovations, or to spend more on R&D by saving labor costs (Deng et al., 2024; Duan et al., 2023; Liu et al., 2020; Zhang et al., 2023b; Zhao and Yang, 2022; Dixon et al., 2021). Thirdly, improving the skills of the workforce can contribute to a rise in labor productivity. If robots primarily replace less-skilled workers, create incentives for investment in training, or facilitate knowledge spillovers, this can raise labor productivity by improving the human capital structure as suggested by Graetz and Michaels (2018), Duan et al. (2023), Zhang et al. (2023b), Zhang et al. (2024), and Zhao and Yang (2022)). Moreover, these three mechanisms are closely related to each other. For example, a higher skill-level of employees may increase innovation activity and incentivize further investment in advanced equipment, i.e., further capital deepening (mechanism 1), thereby possibly improving TFP (mechanism 2). A mismatch between required skills and the needs of automation technologies may hamper technology adoption or reduce the productivity gains after adoption (Acemoglu and Restrepo, 2018a). Applying the empirical estimates of the productivity increase through robot usage from Graetz and Michaels (2018) to a growth accounting framework, Cette et al. (2021a) and Cette et al. (2021b) find empirical evidence for the first and second theoretical mechanism, i.e., a contribution of robot usage to productivity growth via capital deepening and TFP. However, they also show that robots are not a main driver of labor productivity growth in the period from 1960 to 2019 (Cette et al., 2021a) and conclude that "robotization does not appear to be the source of a significant revival in productivity" (Cette et al., 2021b).

Upchurch (2018) and Nordhaus (2021) discuss whether we are approaching an "economic singularity", i.e., a situation in which super-intelligent machines are able to innovate production processes, thereby enabling rapid technological progress and unbounded economic growth, while leaving the relative performance of humans negligible. In a related approach, Growiec (2023) develops a growth model with hardware ("brawn input", comprising physical capital and human physical labor) and software ("brain input", comprising pre-programmed software and human cognitive work) as input factors instead of traditional labor and capital. In this framework, a fraction of physical capital is programmable hardware like industrial robots. He outlines a scenario of full automation where the pace of accumulation of programmable hardware will determine the pace of economic growth, i.e., digital performance indicators (e.g., computing power, storage capacity, bandwidth) and the abilities of robotic hardware would become the engine of economic growth. If additionally technological progress is assumed to be partly "hardware-augmenting", for example by increasing the energy efficiency of computers and robots, this would lead to an ever-increasing long-run growth rate of GDP, i.e., an "economic singularity". Based on theoretical and empirical arguments, Nordhaus (2021) concludes that such a singularity will, if at all, only be seen in the distant future. Similarly, Acemoglu (2024) predicts only modest TFP gains from AI over the next 10 years of less than 0.53% in total.

In general, leveraging the full potential from adopting new technologies requires complementary investments and innovations in the realm of business organization, workplace practices, intangible capital, and human capital (Bresnahan et al., 2002; Brynjolfsson and Hitt, 2000; Dixon et al., 2021; Vrontis et al., 2021; Brynjolfsson et al., 2021). This argument also suggests that it takes time until the productivity effects of robotization can fully materialize. In the short-run, adjustment costs associated with complementary measures may even explain a decline in productivity (Basu et al., 2001; Brynjolfsson et al., 2021). This view is supported by Du and Lin (2022) who find evidence in favor of a U-shaped relationship between robot use and TFP in China. An "economic singularity" as discussed by Nordhaus (2021) and Growiec (2023) would require an exponential relationship between robots and productivity after crossing a U-shaped turning point, leading to ever-increasing economic growth. Calì and Presidente (2022), however, find evidence for diminishing productivity returns to robot adoption at the plant-level in Indonesia. A concave relationship between automation and productivity gains is also in line with Nordhaus (2021) who concludes that "the Singularity is not near". Capello et al. (2022), however, do not find evidence that robot adoption is characterized by decreasing or increasing productivity returns.

A significant number of studies included in my meta-study view a positive productivity effect of robot use as a mechanism for other relationships examined, especially when the focus is on the impact of robot adoption on indicators of environmental sustainability. In a sample of 17 manufacturing sectors in 38 countries Wang et al. (2022) find that industrial robots improved energy intensity between 2000 and 2014, while an increase in TFP is shown to be one of the mechanisms. Similarly, Liu et al. (2021), Huang et al. (2022), and Li et al. (2023) document that productivity gains are a mechanism for improving energy performance through robot adoption. Zhang and Shen (2023), Li et al. (2022), Zhu et al. (2023), and Song et al. (2022) show that robot use can reduce the intensity of industrial emissions by raising productivity. Moreover, there are studies that use "green TFP" (GTFP) as dependent variable, i.e., a special concept of TFP additionally accounting for undesired output in the form of emissions, pollutants, or waste. For instance, Zhang et al. (2022), Zhao et al. (2022), Wu (2023), and Chen et al. (2024) find that robotization can contribute to sustainable or "green" growth. Yang and Liu (2024) restrict that robot use only improves GTFP if strong environmental regulation is in place.

My paper is further related to the growing literature of meta-studies in economics. Several of these meta-studies deal with the determinants of economic growth: Doucouliagos and Ulubaşoğlu (2008) analyze the relationship between democracy and economic growth, Klomp and Valckx (2014) examine the influence of natural disasters, Valickova et al. (2015), Bijlsma et al. (2018), and Iwasaki and Kočenda (2024) study the significance of financial development, Havranek et al. (2016) the effect of natural resources, Baskaran et al. (2016) the impact of government decentralization, Cazachevici et al. (2020) scrutinize the role of remittances, Afonso et al. (2020) the effect of the shadow economy, Ridhwan et al. (2022) the role of health, and Ridhwan et al. (2024) meta-analyze the real exchange rate-growth nexus. Further, Doucouliagos and Laroche (2003) examine the effect of unions on productivity, Ugur et al. (2020) meta-analyze the productivity effects from R&D spillovers, and several meta-studies consider productivity spillovers from FDI (Gorg and Strobl, 2001; Meyer and Sinani, 2009; Wooster and Diebel, 2010; Havranek and Irsova, 2010; Iršová and Havránek, 2013; Mebratie and Bergeijk, 2013; Iwasaki and Tokunaga, 2016; Demena and Bergeijk, 2017; Bruno and Cipollina, 2018).

A strand of literature closely related to the productivity effects of robots is the long-standing debate on the productivity effects of ICT as well as technology adoption in general. Cardona et al. (2013), Schweikl and Obermaier (2020), and Vu et al. (2020) provide a review of the empirical literature on the relationship between ICT, productivity, and growth. Foster and Rosenzweig (2010), Mondolo (2021), Filippi et al. (2023), Montobbio et al. (2023), Hötte et al. (2023), and Restrepo (2023) take a broader perspective by reviewing the economic effects of technology adoption. A comprehensive review of the economic and social effects of robot adoption can be found in Klump et al. (2021); a review of the economic effects of AI is available in Lu and Zhou (2021). Going beyond descriptive evidence, Kohli and Devaraj (2003), Stiroh (2005), Polák (2017) and Stanley et al. (2018) meta-analyze the effect of ICT on economic performance. Although these meta-studies reject the Solow-paradox in its strongest form, i.e., a null effect of ICT on productivity, they support it in its less strict form, i.e., an economically weak effect, especially after correcting the primary literature for publication bias. There are already a few meta-studies on the economic effects of robots and automation: Pinheiro et al. (2023) find evidence for a positive link between automation and reshoring; Jurkat et al. (2023) and Guarascio et al. (2024) meta-analyze the labor-market effects of robots and both find only negligible total effects. Jurkat et al. (2023) show that the impact of robotization on wages is more negative in the manufacturing sector and more positive in the non-manufacturing sector.

3 Data collection

3.1 Literature research and selection criteria

My systematic literature research was conducted at the end of December 2023. I employed Google Scholar, JSTOR, and IDEAS/RePEc as search engines using the keywords "industrial + robot + productivity" and 2018 as start year. This research resulted in more than 2,000 hits in total.[1] I chose 2018 as the start year because it marks the year in which the first article on the productivity effects of robots by Graetz and Michaels (2018) was published in a scientific journal. To identify relevant studies, I used machine learning by means of the software ASReview. This AI tool sorts the literature records by relevance after specifying prior knowledge on relevant and irrelevant records. After labeling 25 relevant and 30 irrelevant records, I used the default settings for the active learning model based on Naïve Bayes and reviewed the suggested records until an uninterrupted sequence of 50 irrelevant records was observed. Furthermore, I checked the references of all eligible studies to find additional relevant studies. The collection of primary studies was updated for meanwhile published studies until the end of June 2024.

I selected all primary studies which meet the following criteria: The paper must deal with industrial robots according to ISO standard 8373:2012 (§ 2.9) and their effect on an appropriate measure of productivity by applying econometric methods. The IFR (2020) defines an indus-

[1]From Google Scholar, the first 1,000 hits (sorted by relevance) were retrieved.

trial robot according to the ISO standard 8373:2012 (§ 2.9) as an “automatically controlled, reprogrammable, multipurpose manipulator programmable in three or more axes, which may be either fixed in place or mobile for use in industrial automation applications”. I adhere to this definition and exclude papers with a broader measure of automation comprising, for instance, service robots, numerically controlled machines, digital technologies, or AI (e.g., Xia et al., 2024; Zhu et al., 2024; Lyu and Liu, 2021). Likewise, estimates based on a purely theoretical measure of automation (e.g., based on the intensity of routine tasks) are excluded (Bonfiglioli et al., 2024). Industrial robots differ significantly from other types of automated capital in the sense that they can truly substitute for human labor. The precise definition of industrial robots in the ISO standard avoids confusing the productivity effects of robots with other automation technologies as well as a significantly varying scope of technologies across studies. Nevertheless, data on the usage of industrial robots can be understood as a proxy for automation in general (Jurkat et al., 2022).

Measures of productivity considered suitable are continuous measures of labor productivity, TFP, or GTFP.[2] Within these three categories, different measures and computational methods were used in the primary literature. Labor productivity encompasses varying measures that divide output (value added, revenue, or GDP) by labor input (workers, employees, hours worked, or labor expenses). The simplest method to compute TFP is using the ”Solow-residual” from an OLS regression of output on the capital stock, labor input, and intermediate inputs (e.g., Hötte et al., 2024; Deng et al., 2024; Acemoglu et al., 2020a). OLS estimates of production functions, however, are suspected of producing biased parameters and thus biased estimates of productivity, owing to a potential correlation between unobserved productivity shocks and input levels (Olley and Pakes, 1996; Levinsohn and Petrin, 2003). As a result, several methods have been developed to overcome this endogeneity issue. The most frequently employed method for estimating TFP are the Olley-Pakes (OP) and Levinsohn-Petrin (LP) method. Olley and Pakes (1996) develop a semi-parametric two-step estimation procedure that uses investment as a proxy for unobservable productivity shocks (for a practical implementation see Yasar et al. (2008)). Levinsohn and Petrin (2003) extend the OP estimation framework and suggest using intermediate inputs instead of investment as a proxy for unobservable shocks (for a practical implementation see Petrin et al., 2004). A further refinement of the two-step estimation procedure was introduced by the control function approach of Ackerberg et al. (2015) (for a practical implementation see Manjón and Manez (2016)). Wooldridge (2009) instead proposed a one-step estimation based on generalized method of moments.

[2] A very low number of estimates is reported for a single-factor productivity other than labor productivity: Hötte et al. (2024) report estimates for capital productivity; three studies use varying measures of energy productivity: Chen et al. (2024) report three estimations with energy productivity (ratio of GDP to energy consumption) as dependent variable; the study of Li et al. (2023) contains one estimation with the ratio of output to fuel coal consumption as outcome variable; Zhou et al. (2024) uses the ratio of GDP to electricity consumption as dependent variable. For the sake of comparability, these estimates are excluded from my meta-study.

Furthermore, quite a number of authors use a distance function approach that evaluates the distance of decision-making units from the production efficiency frontier and results in a Malmquist productivity index as a measure of TFP (Malmquist, 1953; Carlaw and Lipsey, 2003). In this approach, the production frontier is typically constructed non-parametrically by means of Data Envelopment Analysis (DEA) (Du and Lin, 2022; Li and Zhou, 2024; Liu et al., 2022b); for a practical implementation of DEA see Ji and Lee (2010). GTFP is always measured by variants of the Malmquist index, while output is decomposed into desirable output (GDP or value of industrial output) and undesirable output (emissions or waste) (Oh, 2010; Zhao et al., 2022; Zhang et al., 2022; Yang and Liu, 2024). All these methods for measuring TFP and GTFP were deemed eligible for inclusion in my meta-analysis. A more detailed discussion of the different methods for TFP estimation can be found in Carlaw and Lipsey (2003), Rovigatti and Mollisi (2018), and Fragkandreas (2021).

Moreover, primary studies must be published in English with public access (via a paid journal or open access). I did not restrict my search to peer-reviewed papers as the economic research on the productivity effects of robots is still a young and rapidly evolving field, resulting in a significant number of working papers. Within a few studies, I omitted some estimates that were based on an event study design, providing several estimates for different points in time and typically presented as a figure without exact numbers (Bonfiglioli et al., 2024; Wang, 2022; Huang et al., 2023). The studies by Cette et al. (2021b) and Eder et al. (2023) were not eligible for inclusion as they measured the percentage contribution of robots to productivity growth in a growth accounting/ decomposition framework, without directly regressing the respective measure of productivity on robot usage.[3] My sample of primary estimates also focuses on the direct effect of robots on productivity, while estimates of spillover effects (e.g., the impact of robot adoption in other firms on firms without robot use) are excluded (Lin et al., 2022; Li et al., 2024; Venturini, 2022). The study by Zhou and Zhang (2024) was dropped due to a very specific study sample focusing on firms with financial difficulties (”zombie firms”), making it non-comparable to other firm-level studies.

As will be described in Section 4, I need coefficients and their respective standard errors (or t-values) as well as the degrees of freedom in order to calculate a comparable effect size. Sometimes, the reported statistical information was incomplete or required methodological information remained unclear. I contacted the authors whenever I faced lacking information or uncertainty on how to interpret the information provided in the respective study. In rare cases, when no suitable information was provided by the authors, I had to exclude the concerned primary estimates from my meta-analysis. In total, I was able to code 81 primary studies with 1813 estimates. A comprehensive list of all primary studies included is available in the appendix (see Table A2). The number of estimates per study ranges from 1 to 549, with a mean of 22 and a median of 6 estimates per study. The systematic literature research is illustrated in a PRISMA flow diagram

[3]Stanley et al. (2018) also exclude growth accounting studies from their meta-analysis of the effect of ICT on economic growth.

in the appendix (see Figure A1).

3.2 Moderator variables

To examine the drivers of heterogeneity in the primary literature's findings, a matrix of moderator variables is coded. This matrix captures study-dependent or estimation-specific characteristics and targets potential biases from omitted variables and misspecification in the primary literature. The analysis of the determinants of heterogeneity in the primary studies comprises five main groups of moderator variables: (1) data and estimation characteristics, (2) measure of productivity, (3) omitted control variables, (4) subpopulations, and (5) publication quality/ status (s. Tables A1). Most of the moderator variables are constructed as binary indicators taking on a value of 1 if the estimate fits the category and 0 otherwise.

(1) *Data and estimation characteristics* include the number of countries (c_num) and years (sample_years) included in the estimation, the level of analysis, the period of analysis, the econometric specification as well as characteristics of the independent and dependent variable. If the number of *sample_years* is equal to 1, the respective estimation is cross-sectional, which holds true for 29% of the primary estimates. 60 studies, roughly accounting for 31% of the estimates, focus on a single country. Among these single-country estimates, the largest share comes from developing and emerging countries, with 71% derived from 41 different studies, while China clearly dominates this group of estimates. The second largest group of single-country estimates is made up of European countries with 17% from 16 studies. Four articles (representing 2% of the estimates) focus on the USA. With regard to the level of analysis, a moderator variable called *firm_lev* signals that the estimate is of micro-economic nature at the firm-level, observing the productivity effect of robots at the level where they are actually deployed. 41 studies, accounting for 21% of the estimates, are at the firm-level. The remainder of estimates comes from more aggregated levels at regional (18 studies, 11% of the estimates), industry (11 studies, 60%), or country (11 studies, 8%) level.

Estimation characteristics are captured by moderator variables related to econometric techniques and functional forms. The moderator variable *log_log* accounts for a specification in which both productivity and robots are in logarithmic terms such that the estimate can be interpreted as elasticity. Quantile regressions are indicated by *quant_reg*. Moderator variable *marginal_comp* indicates that a marginal effect was computed to include the respective estimate in my meta-analysis because the robot variable entered multiple times in the same regression through an interaction term or quadratic term. Moreover, I code whether the primary estimate was based on cluster-robust standard errors (*secl*).

The moderator variable *msms* captures estimates that move closer to estabishing causality by employing econometric methods meeting the criteria of the "Maryland Scientific Method Scale" (WWCLEG, 2016) and reach a score of 3 or 4. These methods include instrumental variable (IV) estimations, difference-in-difference (DID) estimations, as well as panel estimations with year effects, fixed effects at the unit of observation, and appropriate control variables)[4]. Treating endogeneity by such methods may be important for several reasons: (i) unobserved shocks can affect both robot adoption and productivity, e.g., a local recession or industry-specific institutional changes (Acemoglu and Restrepo, 2020); (ii) certain industries, regions, or firms may select into robot adoption and fundamentally differ from non-adopting industries/ regions/ firms, thereby following different trends in productivity evolution even absent automation; (iii) simultaneity or reverse causality can be at work, if only more productive firms are able to afford the costs of robot adoption or if higher labor productivity is associated with higher labor costs that incentivize robot adoption (Koch et al., 2021; Almeida and Sequeira, 2023). The most frequently applied IV approach in the primary literature is to instrument robot adoption in the country/region under research by robot adoption in other, comparable countries/regions (Acemoglu and Restrepo, 2020; Stiebale et al., 2024) or by a measure of task replaceability (Graetz and Michaels, 2018;Wang et al., 2024).

To measure the degree of robotization, most studies use data from the IFR (86% of the estimates are based on IFR data), which collects data on the annual installations and stock of industrial robots at country-industry level. However, there are also some studies relying on other data sources for the usage of industrial robots. Alternative data sources include trade and customs data as well as firm-level surveys (e.g., Acemoglu et al. (2020a), who compile firm-level data compiled from several sources in France). These alternative datasets partially entail information on robot adoption at a finer level of aggregation than country-industry level, e.g., at firm-level or regional level. To check for potential data dependence in the primary literature, a moderator variable *non_ifr* signals estimates based on alternative data sources for robot use. Additionally, *lag_rob* indicates that the robot variable was lagged in the estimation by at least one period. Data coverage on industrial robots mainly begins in the 1990s (Jurkat et al., 2024). However, in the initial years, robot adoption rates were rather low. To check if the returns to robotization change with an increasing intensity of robot adoption, I collect the sample period for each estimation and calculate its mid-year. I use 2007 as the cutoff to separate estimates with a mid-year after 2007 from those with a mid-year before or equal to 2007. The year 2007 is chosen because it is the year before the Financial Crisis and is frequently used as the final year in primary estimations (e.g., Graetz and Michaels, 2018; Acemoglu and Restrepo, 2020; Kromann et al., 2020; Bekhtiar et al., 2024).

(2) I distinguish three types of *productivity measures* in the primary studies: labor productivity, TFP, and GTFP. 65% of the primary estimations (39 studies) use labor productivity as dependent variable, 27% TFP (42 studies), and 7% use GTFP (12 studies). Additionally, the moderator variable *growth* indicates whether an estimation uses growth rates of productivity or productivity changes over time instead of levels.

(3) *Omitted control variables*: Many primary studies control for general capital input and specif-

[4] The maximum score of 5 is reserved for randomized control trials (RCT), which are not available in the primary literature under research.

ically ICT capital, for labor input and labor costs, for initial or lagged productivity (or output), for demographic shifts in the working age population, for the economic structure (e.g., the manufacturing employment share), for human capital, for research and development, as well as for measures of economic openness and trade. These control variables are considered as relevant explanatory variables in the estimation of production functions and productivity. To examine potential sources of omitted variables bias, I code binary indicators for the omission of these control variables in primary estimations.

(4) *Subpopulations* in the primary literature are specific samples in terms of the development status of countries, the economic sector, firm size, parts of the productivity distribution, or the intensity of robot usage. The moderator variable *dev_country* indicates estimates for developing or emerging countries.[5] Estimates that are solely based on the secondary sector, i.e., mainly manufacturing, are captured by the variable *secondary_sec*. Estimates that decompose the units of analysis along the distribution of productivity are assigned to four indicator variables for the quartiles of productivity. Analogously, estimates for samples with specific intensities of robot use are represented by means of four binary variables corresponding to the quartiles of the distribution of robot use. The moderator variable *excl_high_exposure* refers to estimations that exclude the entities with the highest intensity of robot adoption from the sample. The variables *sme* and *large* indicate estimates for only small and medium sized or large enterprises, respectively.

(5) *Publication quality/status*: To assess the quality of the publications, I follow Picchio and Ubaldi (2024) and use the SCImago Journal Ranking (SJR) index for the respective publication year.[6] Unpublished studies are assigned a SJR value of zero. 91% of the primary estimates come from 65 articles published in academic journals with a mean SJR index of 1.9. 16 studies are unpublished, i.e., mainly working papers and some dissertations. Another approach to measure quality could be the number of citations of the primary study (e.g., used in Ugur et al. (2020) and Pinheiro et al. (2023)). This is, however, not feasible for my meta-analysis since many papers have been published very recently (21 studies in 2023 and 19 studies in 2024). Using the number of citations as a quality measure would simply give more weight to older studies regardless of their actual quality. The most recent primary studies are from June 2024 and, in consequence, cannot have been cited by many other studies.

[5] The classification follows the World Bank (2024) and labels high-income economies as developed economies and the remainder as developing/ emerging economies.

[6] Three journals are not indexed in SCImago: Starovatova (2023) and Zhao et al. (2024) are assigned a SJR index of zero; for Acemoglu et al. (2020a), the impact factor according to CitEc was used as SJR index. Articles published in 2024 are assigned the SJR index for year 2023 since the SJR index for 2024 was not yet available.

4 Methodology

4.1 Partial correlation coefficients

A meta-regression analysis (MRA) requires comparable effect sizes to assess the strength and direction of empirical estimates (Stanley and Doucouliagos, 2012, pp. 22). The aim is to summarize the primary literature in terms of a potential true overall effect and to identify the drivers of heterogeneity among primary studies. Usually, the empirical literature in economics is characterized by varying definitions of dependent or independent variables as well as different econometric specifications and techniques. This is also valid for the primary literature analyzed in this meta-study. Thus, to make the relationship between robots and productivity comparable across diverse specifications and alternate measures of productivity, I follow the recent literature on economic meta-analyses (Stanley et al., 2018; Duan et al., 2020; Cazachevici et al., 2020; Pinheiro et al., 2023; Picchio and Ubaldi, 2024) and convert each estimated coefficient to a PCC as common effect size. $PCCs$ are a unitless measure ranging from -1 to $+1$ for the strength and direction of the association between two variables, while holding other variables constant (Stanley and Doucouliagos, 2012, pp. 24-25; Gustafson, 1961).

The PCC_{is} is defined by the following equation:

$$PCC_{is} = \frac{t_{is}}{\sqrt{t_{is}^2 + df_{is}}} \tag{1}$$

where t denotes the t-statistic and df the degrees of freedom from estimate i in study s. Following Stanley et al. (2024), the degrees of freedom of primary estimations are calculated by $df = N - k - 1$, where N is the number of observations and k is the number of regressors and/or fixed effects included in the estimation. The $PCCs$ are quite robust to imprecise measures of df (Stanley and Doucouliagos, 2012, p.156).[7] Table 1 presents summary statistics for the effect size variables. Doucouliagos (2011) gives guidelines for the relative strength of $PCCs$ based on 22,000 $PCCs$ drawn from various meta-studies. A partial correlation coefficient above ± 0.33 can be considered large, while a PCC between ± 0.17 and ± 0.33 constitutes a moderate effect, and a PCC between ± 0.07 and ± 0.17 is considered as a small effect. $PCCs$ close to ± 1 are very strong effects that rarely occur in MRA in economics.

My meta-study also includes primary estimations that employ an interaction or quadratic term for the robot variable and thus report more than one coefficient for the relation of interest.[8] To make interaction terms fit into my meta-analysis, I follow Cazachevici et al. (2020) by computing

[7] In a few cases, the degrees of freedom had to be approximated by making assumptions on the number of fixed effects included because the exact number was neither provided within the primary study nor obtained upon request.

[8] Estimations with more than one interaction term are excluded from the meta-analysis, e.g., in Du and Lin (2022), Koch et al. (2021), or Li et al. (2024). Likewise, Tobit estimates are excluded since the corresponding marginal effect would depend on the value of ALL explanatory variables and the variance of the error term (relevant for Liu et al. (2022b), Zhang et al. (2022), and Qian and Wang (2022))

Table 1: Descriptive statistics for effect size variables

Measure	Obs.	Mean	Median	SD	Min	Max
t-value	1813	3.28	2.29	5.28	-12.42	48.96
df	1813	33479.28	936	227124.11	9	6227990
PCC	1813	0.10	0.06	0.16	-0.69	0.98
SE_{PCC}	1813	0.05	0.03	0.06	0.00	0.33

the marginal effect at the mean and using the delta method to approximate the associated standard error[9]:

$$\beta = \hat{\beta_1} + \hat{\beta_2}\bar{x}; \qquad SE(\beta) = \sqrt{SE(\hat{\beta_1})^2 + SE(\hat{\beta_2})^2\bar{x}^2} \tag{2}$$

where β_1 denotes the estimate of the robot coefficient for the linear term, β_2 is the estimate of the coefficient for the interaction term, $\bar{x}$ is the sample mean of the variable interacted[10] with the robot measure , $SE(\beta_1)$ is the standard error of the reported coefficient for the linear term, and $SE(\beta_2)$ is the standard error of the reported coefficient for the interaction term.[11]

In case of a quadratic term, the effect of robots on productivity is linearized using the following formula for the marginal effect (Zigraiova and Havranek, 2016):

$$\beta = \hat{\beta_1} + 2\hat{\beta_2}\bar{x}; \qquad SE(\beta) = \sqrt{SE(\hat{\beta_1})^2 + 4SE(\hat{\beta_2})^2\bar{x}^2} \tag{3}$$

where β_1 denotes the estimate of the robot coefficient for the linear term, β_2 is the estimate of the robot coefficient for the quadratic term, $\bar{x}$ is the sample mean of the robot measure, $SE(\beta_1)$ is the standard error of the reported coefficient for the linear term, and $SE(\beta_2)$ is the standard error of the reported coefficient for the quadratic term. The marginal effects and their standard errors computed by Equation (2) or Equation (3) are subsequently transformed into the PCC in line with Equation (1).

4.2 Meta-analytic estimators

Using the $PCCs$ derived from the primary studies, I can now aggregate the information to an overall mean effect of robots on productivity. This is traditionally done by computing simple weighted averages using either fixed effects (FE) or random effects (RE) weights.[12] The weight

[9]Since the original datasets used in the respective primary studies are not available, I omit the co-movement between the estimated coefficients from the formula for SE(β) by assuming the covariances to be zero.

[10]If the interacted variable is a binary variable, $\bar{x}$ is set equal to 1 to compute the marginal effect for the respective subpopulation indicated.

[11]As the original datasets used in the respective primary studies were not available, I omit the co-movement between the estimated coefficients from the formula for $SE(\beta)$ by assuming the covariances to be zero.

[12]The traditional FE and RE estimator are estimated by the STATA command "meta reg".

should reflect the quality of the estimate. More precise estimates with smaller standard errors are likely better estimates of the true effect. Following the meta-analytic literature, I primarily use precision as quality measure of the reported coefficients. For each PCC_{is}, the associated standard error of the effect size is calculated according to (Stanley and Doucouliagos, 2012, p. 25):

$$SE_{PCC_{is}} = \sqrt{\frac{1 - PCC_{is}^2}{df_{is}}} = \frac{PCC_{is}}{t_{is}} \tag{4}$$

where t_{is} is the t-value of estimate i in study s and thus identical to the t-value of the respective PCC. The precision weight is then equal to the inverse of the estimator's standard error:

$$FE1 = \frac{1}{SE_{PCC_{is}}} \tag{5}$$

Assigning these precision weights to primary estimates to summarize an overall effect is known as the fixed effect estimator (FE) in MRA (Stanley and Doucouliagos, 2012, p. 46).[13] The assumption underlying the FE estimator is that all primary estimates are drawn from a single population and as a result measure one true effect of robots on productivity. The primary estimates (PCC_{is}) sampled from studies $i = 1, ..., N$ are assumed to deviate from the true effect only due to sampling error. However, primary studies use different populations and diverse econometric methods. Thus, assuming a uniform effect may be incorrect. The random effects estimator (RE), in contrast, explicitly allows for heterogeneity of primary estimates beyond sampling error; i.e., each study can have a different underlying true effect, randomly drawn from a common distribution with a constant heterogeneity variance of τ^2. The RE assumes additive error variances and the weights additionally account for between-study heterogeneity (Duan et al., 2020; Stanley and Doucouliagos, 2012, pp. 46-47):

$$RE1 = \frac{1}{\sqrt{SE_{PCC_{is}}^2 + \tau^2}} \tag{6}$$

While information on SE_{PCC} is available in primary studies, τ^2 must be estimated from the meta-dataset (Feld and Heckemeyer, 2011).[14] A larger τ^2 indicates a stronger degree of heterogeneity between primary studies. Including τ^2 in the weighting scheme, reduces the relative impact of the precision weight and creates a more uniform weighting scheme.

While the traditional FE estimator imposes a heterogeneity variance of zero, the traditional RE estimator forces the heterogeneity variance to be constant and independent of the sampling errors (Stanley et al., 2023). Stanley and Doucouliagos (2015) and Stanley and Doucouliagos (2017) argue that the most appropriate meta-analytic estimator is neither fixed nor random.

[13] This FE estimator should not be mistaken for a panel-fixed effect estimator in econometric terms. The intuition behind it is that a more precise estimate (as measured by a small standard error) allows a smaller estimated effect to retain a statistically significant coefficient (Jurkat et al., 2023). Therefore, more weight is attributed to $PCCs$ with a higher precision and a lower standard error.

[14] This is done by means of the restricted maximum-likelihood (REML) estimator (Raudenbush, 2009).

Instead, they recommend an unrestricted WLS (UWLS) estimator by using precision weights in the following basic regression:

$$PCC_{is} = \mu + \epsilon_{is} \tag{7}$$

where, in the absence of any bias, μ represents the overall true effect, and the error term $\epsilon_{is} \sim N(0, \sigma^2_{\epsilon_{is}})$ describes the primary sampling error.[15] This leads to identical point estimates, as obtained from the traditional FE estimator. However, the UWLS standard errors and confidence intervals differ from FE because the UWLS heterogeneity variance is proportional to the sampling error variance and thereby, like RE, adjusts for heterogeneity (Stanley et al., 2023). Analogously, Equation 7 can be estimated by WLS using RE1 weights to obtain the UWLS version of RE1, as done by Hong and Reed (2024). In the next section, I will present estimates for both the traditional FE1 and RE1 estimators and their UWLS counterparts to make the potential impact on statistical inference transparent.

As typically done in economic meta-studies, I include all estimates provided by a primary study if they fit the selection criteria.[16] As mentioned in Section 3, the distribution of the number of estimates per study is highly right-skewed. To avoid an overinflated influence of a few studies with a large number of estimates, I employ FE and RE weights, additionally adjusting for the number of estimates per study:

$$FE2 = \frac{1}{SE_{PCC_{is}} \cdot \sqrt{n_{i \in s}}} \tag{8}$$

$$RE2 = \frac{1}{\sqrt{SE^2_{PCC_{is}} + \tau^2} \cdot \sqrt{n_{i \in s}}} \tag{9}$$

where n is the number of estimates of study s. In doing so, I assign equal weight to studies rather than to estimates for a given precision (Duan et al., 2020). Multiple estimates per study further raise the concern of study and author dependence. Estimates from one primary study usually rely on the same data and similar econometric techniques. This could undermine the basic assumption of regression analysis that the error terms are independently and identically distributed. Therefore, I always use robust standard errors clustered at the study-level to address within-study dependence (Stanley and Doucouliagos, 2012, p. 71). Moreover, in the

[15] Measuring (7) by OLS is not appropriate since the error term in Equation (7) is a function of the sampling error and therefore heteroskedastic, i.e., not independently and identically distributed (Stanley and Doucouliagos, 2012, p. 61). Thus, Equation (7) must be estimated by weighted least squares (WLS) to receive unbiased, consistent and efficient estimates.

[16] In other disciplines, it is often argued that only one estimate per study should enter the meta-analysis to avoid study dependence. This could be achieved by selecting the "best estimate" per study as preferred by the authors of the primary study or by the meta-analyst; alternatively, one could use an average effect size per study. In both cases, this would prevent leveraging within-study variation, leading to a loss of information that impedes detecting the drivers of heterogeneity. Moreover, results could suffer from an arbitrary selection of estimates with the most favorable sign or effect size. Choosing only one estimate per study is especially misleading if studies provide estimates for several subpopulations. See Stanley and Doucouliagos (2012, pp. 32-33) for a discussion of this issue.

multivariate MRA my moderator matrix will capture varying estimation characteristics used and subpopulations examined within a given primary study. Like before, FE2 and RE2 will be used as analytical weights in a WLS regression of Equation (7) to obtain their corresponding UWLS versions.

Applying alternate weighting schemes requires some judgment of what I deem preferable. In view of the highly right-skewed distribution of the number of estimates per study, it is necessary to adjust the weighting scheme for the number of estimates per study to avoid an inflated impact of a few studies with a high number of estimates. Thus, FE2 and RE2 are preferred over FE1 and RE1. There are particular concerns that RE is biased in the presence of publication selection, as the random effects might be correlated with the standard error, as shown in simulation studies (Stanley and Doucouliagos, 2012, p. 82-84; Stanley and Doucouliagos, 2015; Stanley and Doucouliagos, 2017; Stanley et al., 2023; Henmi and Copas, 2010; Poole and Greenland, 1999). As I obtain strong evidence for publication bias, I prefer using FE2 weights in a WLS regression. This also avoids the additional uncertainty involved in estimating τ^2.

5 Overall mean effect and publication bias

A simple vote counting based on the conventional 5% significance level reveals that 57% of the estimates show a positive and statistically significant effect, while 3% report a significantly negative effect. Forty percent are statistically insignificant, with 29% showing a positive sign and 11% with a negative sign. However, as described by Stanley and Doucouliagos (2012, pp. 43-45), the loss of information involved in the categorical approach of vote counting can be misleading. Therefore, I calculate further summary measures of the effect size and formally test for publication selection bias in the literature on the productivity effects of robots.

Table 2 presents summary measures for the *PCCs* computed for all primary estimates collected, while Figure 2 demonstrates the frequency distribution of *PCCs* by means of a histogram and a Kernel density estimator.[17] All estimators for the overall mean effect of robots on productivity are statistically significant. In terms of economic significance, only the unweighted mean and the RE estimators indicate a small positive effect slightly above the threshold of 0.07 (Doucouliagos, 2011). Comparing the traditional FE and RE estimates with their UWLS counterparts (UWLS FE1 and UWLS RE2, respectively), shows a substantial increase in the standard errors of UWLS due to their adjustment for heterogeneity as well as clustering at study level. Statistical inference based on UWLS is thus significantly more conservative. With an overall effect size of 0.01, both FE1 and FE2 suggest an effect that is close to zero. This is in line with the highest frequencies of *PCCs* being centered around small positive values as shown in Figure 2. The

[17]When aggregating the effect sizes from all primary studies, 12 primary estimates are dropped to avoid double counting. These estimates come from Almeida and Sequeira (2024), who report 12 estimates with cluster-robust standard errors in addition to only robust standard errors. These estimates will be included in the multivariate MRA of Section 6 as the moderator variable *secl* captures whether primary estimates are based on clustered standard errors.

Table 2: Summary measures of PCCs for the effect of robots on productivity

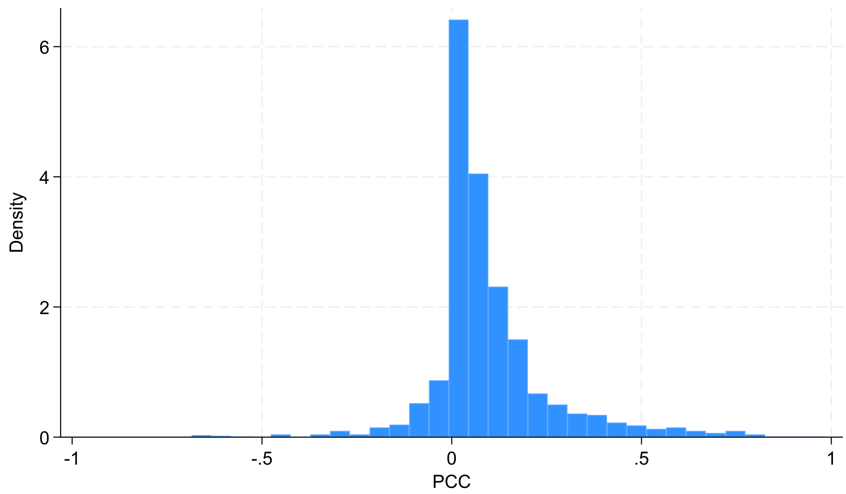
	Obs.	Estimate	SE	95% CI
Mean	1801	0.0946	0.0149	[0.0650, 0.1242]
FE	1801	0.0114	0.0001	[0.0111, 0.0116]
UWLS FE1	1801	0.0114	0.0036	[0.0042, 0.0186]
UWLS FE2	1801	0.0108	0.0019	[0.0071, 0.0145]
RE	1801	0.0901	0.0032	[0.0839, 0.0962]
UWLS RE1	1801	0.0901	0.0114	[0.0675, 0.1126]
UWLS RE2	1801	0.0736	0.0121	[0.0494, 0.0977]

Note: Standard errors are clustered at study level, except for FE and RE.

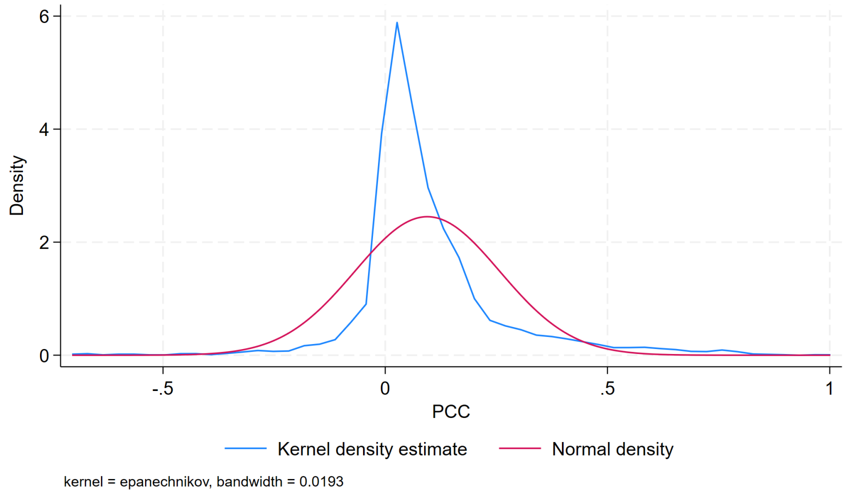
substantial decline in the mean effect caused by assigning larger weights to more precise studies, may point to a publication selection bias. Thus, no reliable inference can be drawn from such summary measures. In case of publication selection, all averages, weighted or not, can be biased. Furthermore, under systematic heterogeneity, any measure of average effect size may blur the picture of the economic phenomenon under research. To capture systematic heterogeneity, I will include my moderator matrix in Section 6.

Publication selection describes a process of choosing research results according to statistical significance or compliance with conventional theories (Stanley and Doucouliagos, 2012, pp. 51). Many existing meta-analyses in economics show evidence for a publication bias (Doucouliagos and Stanley, 2013, Ioannidis et al., 2017), including meta-studies related to the determinants of economic growth (e.g., Havranek and Irsova, 2011; Gunby et al., 2017) and, in particular, on the impact of ICT on productivity (Polák, 2017; Stanley et al., 2018). Thus, it is essential to formally test for and correct publication selection bias in the literature on the productivity effects of industrial robots. Usually, economists assume that the adoption of advanced technologies boosts productivity and economic growth, and this belief is supported by many well-published theoretical and empirical articles (e.g., Romer, 1990; Parente and Prescott, 1994; Carlaw and Lipsey, 2003; Acemoglu and Restrepo, 2020; DeCanio, 2016). As a result, researchers might selectively report positive effects of robots on productivity because they expect such findings to align with the prevailing view and, therefore, be more likely to be published.

The presence of a publication bias can be visually inspected by a so-called funnel plot in Figure 3: a scatterplot of the effect sizes ($PCCs$, x-axis) against their precision (inverse of the standard error of $PCCs$, y-axis). Without publication bias (and between-study heterogeneity), the primary estimates should be distributed symmetrically around the overall effect size (using FE2 and RE2 in Figure 3) as the sampling error is random. The typical funnel shape emerges from the measure of precision on the y-axis. Imprecise estimates at the bottom of the graph are widely dispersed because they have larger standard errors. More precise estimates with lower standard errors, shown in the upper part of the illustration, by contrast, are more compactly distributed.



(a) Histogram of $PCCs$



(b) Kernel density of $PCCs$

Figure 2: Distribution of $PCCs$

The funnel plot shown in Figure 3 is quite asymmetric with more imprecisely estimated $PCCs$ to the right of the overall effects as measured by FE2 and RE2. The most precise estimates show an effect size close to zero, in line with the estimators FE1 and FE2 in Table 2. This points to a positive publication selection bias. Furthermore, I plot a Kernel density estimate of the t-values reported in the primary literature. As shown in Figure 4, the density peaks just above the cutoff value for the 5% significance level (t-value = 1.96, dashed line). This pattern strongly suggests p-hacking, i.e., selecting estimates for their statistical significance (Andrews and Kasy, 2019).

In case of publication selection bias, the reported effect size is correlated with its standard error. To formally test for publication bias, I perform the so-called funnel-asymmetry test (FAT) by estimating the following regression (also called Egger's regression (Egger et al., 1997):

$$PCC_{is} = \beta_0 + \beta_1 SE_{PCC_{is}} + \epsilon_{is} \tag{10}$$

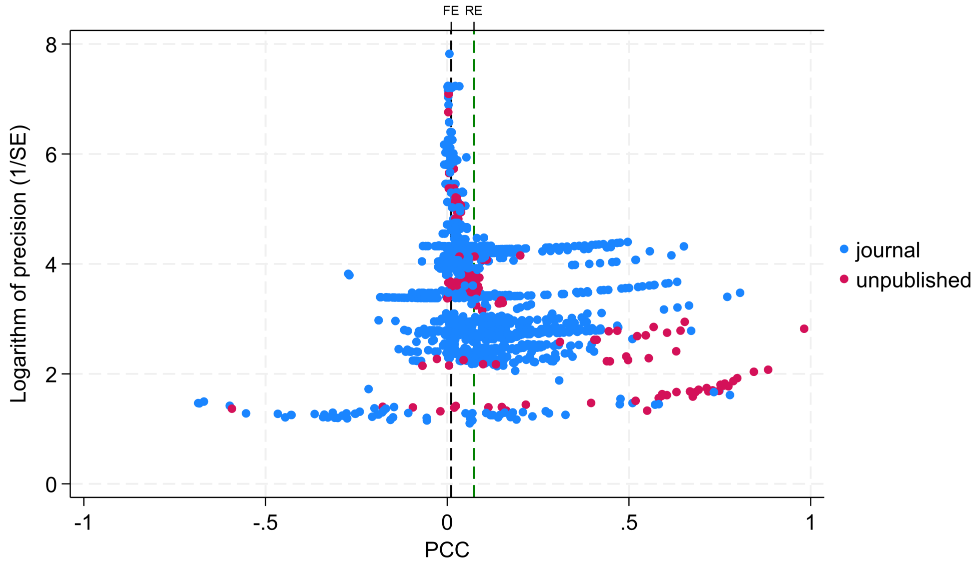


Figure 3: Funnel plot of $PCCs$ (N = 1801, from 81 studies).

where PCC and SE_{PCC} are the effect size and its associated standard error of estimate i in study s as previously defined, and ϵ_{is} is the regression error term. The coefficient β_0 measures the true empirical effect corrected for potential publication bias, and testing for its statistical significance is referred to as precision-effect test (PET). Coefficient β_1 estimates the direction and magnitude of publication bias (FAT). Equation (10) exhibits heteroskedasticity by construction because the independent variable is the standard deviation of the dependent variable. Therefore, it must be estimated by WLS using the weights given in Equations (5), (6),(8), and (9).

Table 3 presents the FAT-PET results. I find strong evidence for publication selection. The FE estimators point to a "severe" magnitude of selectivity ($\beta_1 > 2$) according to the practical guidance of Doucouliagos and Stanley (2013). This finding is robust to the "Meta-Analysis Instrumental Variable Estimator" (MAIVE) as suggested by Irsova et al. (2024). MAIVE instruments the reported variance (SE^2_{pcc}) by the inverse of sample size to treat potential endogeneity coming from spurious precision due to p-hacking in primary estimations. This reflects that the authors of primary studies may not only exaggerate effect sizes (e.g., by changing control variables) to achieve statistical significance but also select for smaller standard errors, for instance, by choosing different clustering or bootstrapping approaches (Irsova et al., 2024). Only the traditional RE estimator and its UWLS counterpart show weaker evidence for publication selection; however, these estimators do not adjust for the number of estimates per study and, thus, appear less reliable. When adjusting for the number of estimates per study, UWLS RE2 indicates a substantial magnitude of selectivity ($1 < \beta_1 < 2$) according to the practical guidance provided by Doucouliagos and Stanley (2013). The "true" effect corrected for publication bias shows statistical significance in 5 out of 7 specifications. However, the effect beyond bias always lacks economic significance (PET), with estimates well below the threshold of 0.07. This

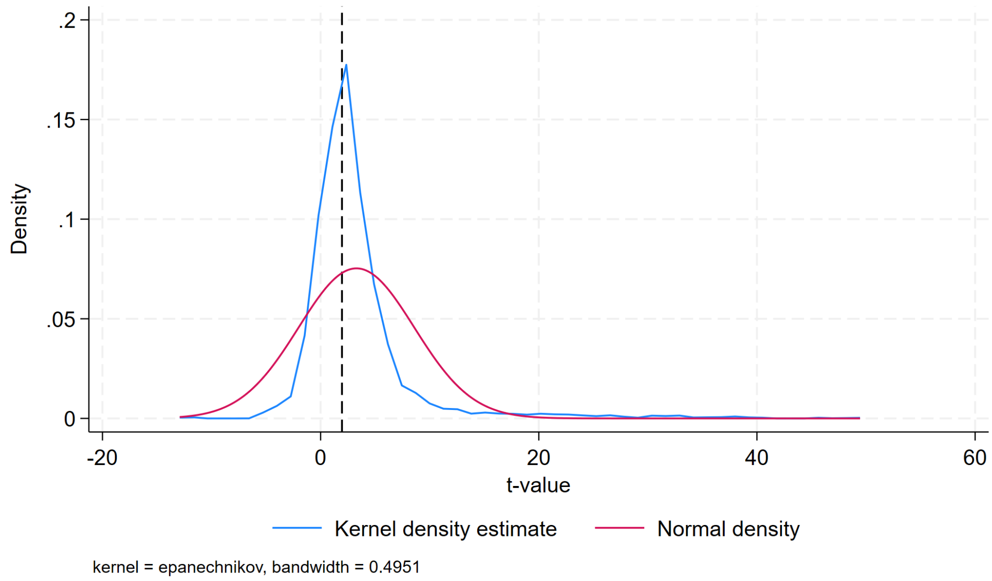


Figure 4: Kernel density estimate of t-values (N = 1801, from 81 studies).

Table 3: FAT-PET

	(1) FE1	(2) UWLS FE1	(3) UWLS FE2	(4) RE1	(5) UWLS RE1	(6) UWLS RE2	(7) MAIVE
Publication bias (β_1)	2.9353***	2.9353***	2.1973***	.9145***	.9145	1.8707***	2.321***
	(.0263)	(.679)	(.3864)	(.0857)	(.5825)	(.4641)	(.5277)
True effect (β_0)	.0043***	.0043	.0078***	.0577***	.0577***	.0216*	.0061
	(.0001)	(.0027)	(.0026)	(.0043)	(.0195)	(.0118)	(.0175)
Observations	1801	1801	1801	1801	1801	1801	1801
No. of studies	81	81	81	81	81	81	81

Notes: Standard errors are in parentheses. They are clustered at study level, except for columns (1) and (4). The weights used in the WLS estimation are indicated in the column header. $^{***}p < .01$,$^{**}p < .05$, $^{*}p < .1$

suggests that industrial robotization has, so far, had only a marginal effect on productivity.

These findings are confirmed by non-linear techniques correcting for publication bias in Table 4.[18] Using the squared standard error of PCC in Equation (10) is called "precision effect estimate with standard error" (PEESE). MAIVE-PEESE refers to the PEESE version where SE^2_{pcc} is instrumented by the inverse of the sample size (Irsova et al., 2024). Both estimators suggest a statistically significant "true" effect that, however, lacks economic significance. WAAP means "weighted average of the adequately powered". This estimator employs a UWLS weighted average using FE1 weights that is calculated only on the adequately powered estimates (Ioannidis et al., 2017). Adequate power in social science research has been conventionally set at 80% or larger. This corresponds to a probability of a Type II error which is not larger than four times the

[18] I also employed the "endogenous kink" model of Bom and Rachinger (2019). This model, however, reduces to UWLS FE1, i.e., column (2) of Table 3.

probability of the Type I error. As explained by Ioannidis et al. (2017), this implies the following relationship between the estimate of the "true" effect (β_0) and its standard error (SE_{PCC}):

$$SE_{PCC_{is}} <= |\beta_0|/2.8 \tag{11}$$

"Top 10%" is the unweighted average among the estimates in the top decile of precision. Both WAAP and "Top 10%" point to a marginal impact of robotization on productivity. The evidence for publication bias and a negligible effect beyond publication bias is also robust across several subsets of primary estimates. As shown in Table 5, it holds among estimates that treat endogeneity, i.e., estimates that achieve a score of at least 3 or 4 on the MSMS, as well as only among IV estimates (MSMS score of 4).[19] The pattern of publication bias is also evident for all productivity measures employed in the primary literature (see Table 6). It remains consistent when focusing solely on estimates in published articles that fall in the top quartile of the SJR index in my meta-dataset (i.e., SJR >= 2.063; columns (1) and (2) of Table 7) as well as for estimates at the firm level (columns (3) and (4) of Table 7).

Focusing on the subset of primary estimates with a log-log specification allows the use of elasticities instead of $PCCs$ as effect size. This means that the reported coefficient and its associated standard error can be directly used in the MRA (Stanley and Doucouliagos, 2012, pp. 26). The results are shown in columns (5) and (6) of Table 7. Using elasticities as an alternative effect size measure still provides evidence of a positive publication selection bias and only a marginally positive effect of robots on productivity beyond publication bias.

Examining publication bias and the "true" effect beyond that bias across several subsets of primary estimates leads into the heterogeneity analysis. The very homogeneous findings with respect to the treatment of endogeneity, different productivity measures, journal quality, level of analysis, and elasticity specifications, suggests that the moderator variables capturing these research dimensions (*msms*, *tfp*, *gtfp*, *sjr*, *firm_lev*, *log_log*) are not important drivers of heterogeneity in the primary literature. This assumption will be checked in the following section by means of a multivariate MRA framework.

[19] IV estimates that suffer from a weak instrument are excluded. Moreover, IV estimates using the "reaching & handling" instrument proposed by Graetz and Michaels (2018) are excluded due to a violation of the monotonicity assumption as shown by Bekhtiar et al. (2024).

Table 4: Non-linear techniques correcting for publication bias

	(1) PEESE	(2) MAIVE-PEESE	(3) WAAP	(4) Top 10%
SE^2_{pcc}	14.9539*	9.3801		
	(8.6355)	(5.6512)		
True effect (β_0)	.011***	.0421**	.0069***	.0078**
	(.0034)	(.0176)	(.0019)	(.0031)
Observations	1801	1801	149	181
No. of studies	81	81	14	19

Notes: Standard errors clustered at study level are in parentheses. $^{***}p < .01$,$^{**}p < .05$, $^{*}p < .1$

Table 5: FAT-PET in the subset of estimates that treat endogeneity

	MSMS		IV estimates	
	(1) UWLS FE2	(2) UWLS RE2	(3) UWLS FE2	(4) UWLS RE2
Publication bias (β_1)	2.7541***	2.991***	2.5228***	1.5309**
	(.4265)	(.6953)	(.5661)	(.7516)
True effect (β_0)	.0048**	.0045	.0069**	.0245*
	(.0023)	(.0105)	(.0027)	(.0134)
Observations	659	659	254	254
No. of studies	58	58	34	34

Notes: Standard errors clustered at study level are in parentheses. $^{***}p < .01$,$^{**}p < .05$, $^{*}p < .1$

Table 6: FAT-PET by productivity measures

	Labor productivity		TFP		GTFP	
	(1) UWLS FE2	(2) UWLS RE2	(3) UWLS FE2	(4) UWLS RE2	(5) UWLS FE2	(6) UWLS RE2
Publication bias (β_1)	2.243***	1.7742***	2.3376***	2.3652**	3.2825**	2.1886
	(.6172)	(.4789)	(.5004)	(1.0244)	(1.2979)	(1.3431)
True effect (β_0)	.0031**	.0167	.0088***	.0172	-.0167	.017
	(.0014)	(.0225)	(.0026)	(.0105)	(.0328)	(.0361)
Observations	1170	1170	496	496	135	135
No. of studies	39	39	42	42	12	12

Notes: Standard errors clustered at study level are in parentheses. $^{***}p < .01$,$^{**}p < .05$, $^{*}p < .1$

Table 7: FAT-PET for subsets

	Top 25% SJR		Firm-level estimates		Elasticities	
	(1)	(2)	(3)	(4)	(5)	(6)
	UWLS FE2	UWLS RE2	UWLS FE2	UWLS RE2	UWLS FE2	UWLS RE2
Publication bias (β_1)	2.648***	2.0822***	1.7819***	1.6013***	3.2269***	1.334*
	(.3871)	(.2445)	(.4705)	(.4397)	(1.1629)	(.7157)
True effect (β_0)	.0031*	.0122*	.0083***	.0103***	.0031***	.0291*
	(.0016)	(.0063)	(.0026)	(.0023)	(.0006)	(.0159)
Observations	330	330	389	389	975	975
No. of studies	17	17	41	41	30	30

Notes: Standard errors clustered at study level are in parentheses. $^{***}p < .01$, $^{**}p < .05$, $^{*}p < .1$

6 Drivers of heterogeneity in the primary literature

6.1 Multivariate MRA-model

A univariate regression, as applied in Eq. 10, may result in biased estimates since potential drivers of systematic heterogeneity are ignored. For example, a specific method chosen by the authors of primary studies may affect both the standard error and the effect size. In such cases, the standard error as explanatory variable will be correlated with the error term, resulting in a biased estimate of β_1 (Havránek, 2015). Hence, I add a matrix of moderator variables, as described in Section 3.2, to Eq. (10) and estimate the following multivariate MRA model to identify the drivers of heterogeneity in the primary literature:

$$PCC_{is} = \beta_0 + \beta_1 SE_{PCC_{is}} + \sum \beta_k Z_{kis} + \epsilon_{is} \quad (12)$$

for estimate i in study s, where k represents the number of moderator variables, β_k is the coefficient of the corresponding moderator variable, and ϵ_{is} denotes the error term. My moderator matrix Z is composed of 38 variables to account for as many research characteristics as possible and alleviate omitted variable bias concerns. In MRA, the number of moderator variables is usually reduced by means of a variable selection procedure to mitigate potential multicollinearity (Stanley and Doucouliagos, 2012, p. 91). I therefore follow Stanley et al. (2018)'s approach in their meta-analysis on the growth effects of ICT and employ a general-to-specific (GTS) procedure that sequentially removes the variables with the largest p-value from the moderator matrix until all p-values are below or equal to 0.05.[20] The next subsection will present the drivers of heterogeneity identified by this procedure.

6.2 Estimated effects for the drivers of heterogeneity

Table 8 shows the results of my multivariate MRA. Employing the GTS approach for the WLS regression with FE2 weights results in a selection of 12 moderator variables that are drivers

[20]The STATA command *stepwise* is used with standard errors clustered at study level.

Table 8: Multivariate MRA

	UWLS FE2				UWLS RE2	
	(1) Full model	(2) GTS	(3) No GTFP	(4) No marginal effects	(5) Full model	(6) GTS
SE_{PCC}	2.9981***	2.8722***	2.8572***	2.9792***	1.6**	1.8465***
	(.3779)	(.4042)	(.4286)	(.4246)	(.6311)	(.4585)
c_num	-.0006	-.0011***	-.0011***	-.0011***	-.001	
	(.0005)	(.0003)	(.0003)	(.0003)	(.0007)	
dev_country	.0103*	.0067***	.0067***	.0066***	.0285	
	(.0057)	(.0023)	(.0023)	(.0022)	(.0278)	
period_2007	.0177***	.0156***	.0156***	.0156***	.0236	
	(.0036)	(.0047)	(.0047)	(.0047)	(.0229)	
lc_omit	.0171	.0083***	.0083***	.0082***	.0686*	
	(.0107)	(.0025)	(.0025)	(.0025)	(.035)	
large	-.1569***	-.1392***	-.1384***	-.1456***	-.1067**	-.1108**
	(.0303)	(.0335)	(.0346)	(.0346)	(.0424)	(.0435)
r_quartile_1	-.2177***	-.2231***	-.2035***	-.2419***	-.1901***	-.1839***
	(.0251)	(.025)	(.0232)	(.0266)	(.0318)	(.0229)
secondary_sec	-.0295	-.0203**	-.0187**	-.0194**	-.0435	-.0529**
	(.0188)	(.0078)	(.0074)	(.0077)	(.0313)	(.0235)
non_ifr	-.0168***	-.0142***	-.0141***	-.0142***	-.0138	
	(.0046)	(.0042)	(.0042)	(.0042)	(.0238)	
green_tfp	-.0363	-.0367**		-.0345**	-.0419	
	(.0228)	(.0142)		(.0153)	(.0411)	
growth	.0108	.0079**	.0085**	.0069*	-.0039	
	(.0075)	(.0039)	(.004)	(.0039)	(.0308)	
quant_reg	.0845**	.0577**	.0363	.0771**	-.0133	
	(.0352)	(.0253)	(.0223)	(.0296)	(.0412)	
marginal_comp	-.0233**	-.0195***	-.0166**		-.0549***	-.062**
	(.0113)	(.0068)	(.0063)		(.0186)	(.0274)
_cons	.005	.0126	.0112	.012	.0675	.0637***
	(.0249)	(.0081)	(.0075)	(.0079)	(.0862)	(.0237)
Observations	1813	1813	1678	1602	1813	1813
No. of studies	81	81	70	78	81	81
R-squared	.4814	.4334	.4452	.4583	.3363	.2368

Notes: Standard errors clustered at study level are in parentheses. Columns (1)-(4) employ the UWLS FE2 estimator, columns (5) and (6) UWLS RE2. GTS refers to the moderator variables selected in the general-to-specific procedure. Only the moderators selected in UWLS FE2 are shown. Column (3) excludes primary estimates with GTFP as dependent variable. Column (4) excludes primary estimates where marginal effects had to be computed. $^{***}p < .01$,$^{**}p < .05$, $^{*}p < .1$

of heterogeneity at least at the 5% significance level. Several estimated coefficients of these moderators point to diminishing productivity returns to robotization. First, the coefficient of *period_2007* suggests that primary estimates with a data midpoint before year 2008 are somewhat higher than estimates for more recent periods. Robot adoption and the technological progress involved in robotic systems, however, have continued or even accelerated since 2008 (IFR, 2023, pp. 54). The growing number of industrial robots, therefore, had a decreasing impact on productivity over time. Second, estimates focusing on the secondary sector exhibit smaller productivity effects from robotization, although industrial robots are primarily used in manufacturing industries (IFR, 2023, pp. 74; Fernández-Macías et al., 2021). Third, and in the same vein as the second point, large companies show significantly smaller effects, even though robot adoption is concentrated in larger companies, as shown by Deng et al. (2024), Zolas et al. (2021), and Acemoglu et al. (2022). Moreover, developing and emerging countries tend to benefit more from robot adoption relative to advanced economies. This may be attributed to a higher scope for productivity improvements inherent to their catch-up process compared to advanced economies. It could also be interpreted as a further indicator of diminishing returns to robot adoption, as advanced economies are the front-runners in robotization (IFR, 2023, pp. 74).

In addition to diminishing returns, there is also evidence for adjustment costs at low levels of robot adoption. The coefficient of the moderator variable *r_quartile_1* suggests significantly smaller productivity effects from robot adoption for users in the bottom quartile of robotization intensity. In this context, it must also be considered that estimates based on a quantile regression (*quant_reg*) are associated with somewhat more positive findings on average.

The negative coefficient for *green_tfp* suggests that the impact of robot use on GTFP is lower compared to labor productivity and TFP. This means that additionally accounting for environmental outcomes, such as emissions and waste, can reduce the productivity effect. That is in line with a rebound effect associated with robot adoption: despite potentially reduced scrap rates and improved energy efficiency through high precision robots, an expansion of production might weaken or even outweigh any beneficial sustainability effects (Luan et al., 2022; Zhang et al., 2022). As robustness check, all primary estimates with GTFP as dependent variables are dropped in column (3) of Table 8. This leaves my findings largely unchanged.

With regard to control variables, I find some evidence that primary estimations omitting a control variable for labor costs (*lc_omit*) tend to slightly overestimate the productivity effects from robotization. This omitted variable bias is in line with a positive relationship between labor costs and robot adoption (Jung and Lim, 2020; Fan et al., 2021). If labor costs and productivity are also positively correlated, this results in a positive omitted variable bias.

Furthermore, I find some evidence for data dependence: estimates that are not based on the most frequently used IFR dataset tend to result in somewhat smaller productivity effects. This may be attributed to less clear-cut definitions of industrial robots in firm-level surveys and trade data. Moreover, authors applying data on robot adoption from firm-level surveys are typically reliant on a binary indicator for robot usage in surveyed firms.

Lastly, the coefficient for *c_num* implies a slightly dampening influence of including more countries in the sample of the primary estimation, while the coefficient of *growth* suggests that specifications in terms of growth rates or changes in productivity may result in slightly higher estimates. The coefficient of *marginal_comp* indicates lower effect sizes among estimates where a marginal effect had to be computed due to an interaction or quadratic term for the robot variable in the primary estimation following Equations (2) and (3). To rule out a distorting influence of these marginal effects, I exclude them from my estimation in column (4) of Table 8. This leaves my findings unchanged.

Apart from the drivers of heterogeneity, it is important to mention, that the evidence for a "severe" (or at least "substantial") positive publication selection bias remains strong in the multivariate MRA, as visible in the coefficient of SE_{PCC} (Doucouliagos and Stanley, 2013). The constant (*_cons*) of my multivariate MRA reflects the mean effect when all moderator variables and SE_{PCC} are equal to zero and thus corresponds to the mean effect for the reference categories beyond publication bias. As an estimation with zero countries does not exist, I need to adjust the constant for the average number of countries (= 6.97) used among the primary estimates.[21] Using the coefficient estimates from the selected and preferred specification in column (2) of Table 8, results in an estimated mean effect of 0.005 that is statistically insignificant. Other moderator variables that may be deemed important for quantifying a "best practice estimate", i.e., a treatment of endogeneity (*msms*), using clustered standard errors (*secl*), or estimations at the firm level (*firm_lev*), are neither statistically significant in the full model of column (1) nor selected by the GTS procedure in column (2) of Table 8. Thus, they are assumed to have no relevant influence on the estimated mean effect. Primary estimations that address endogeneity do not obtain results that are different to estimates without any adjustment for endogeneity. This suggests either that endogeneity issues are of minor relevance in the primary literature or that the econometric methods used to treat endogeneity are insufficient. To further illustrate the influence of the drivers of heterogeneity, Figure 5 shows the estimated mean effects and the associated 95% confidence intervals for several subgroups of primary estimates. These effects are computed from the following formula:

$$\text{Meta effect} = \beta_0 + \beta_1 \cdot 0 + \beta_{c\_num} \cdot 6.97 + \beta_k \tag{13}$$

where β_k is the coefficient of the relevant moderator variable(s) in column (2) of Table 8 for the respective subgroup. The subgroup "Low robot intensity" uses the coefficients of both *r_quartile_1* and *quant_reg* as the effects for different quartiles of robot adoption are mainly estimated by quantile regressions. Figure 5 shows that only the meta-effect for primary estimates with a data midpoint before year 2008 show a statistically significant effect with a positive sign whose magnitude is, however, well below the threshold of economic significance. The meta-effects for the specification in growth rates or productivity changes, for developing or emerging

[21] In line with the WLS weighting scheme adjusted for the number of estimates per study, I use a weighted average for the number of countries, with the inverse of the number of estimates per study as weights.

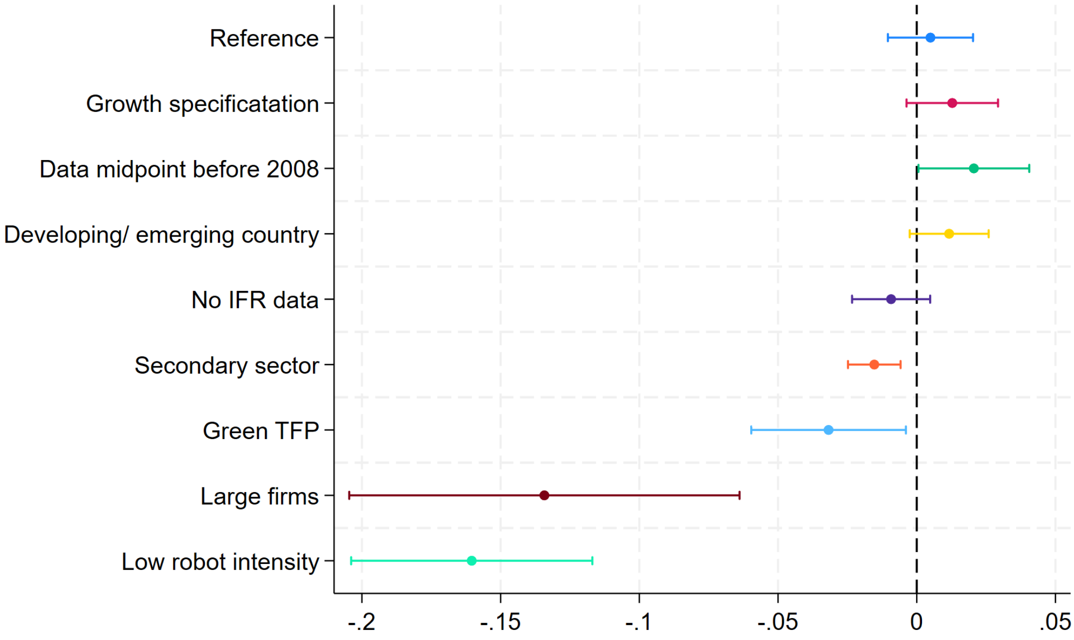


Figure 5: Heterogeneous meta effects

countries, and for estimations based on non-IFR data are not statistically significant at the 5% level. Very small negative effects are found for estimates that focus solely on the secondary sector, as well as for estimates that use GTFP as the dependent variable. Somewhat stronger negative effects are found for estimates of large enterprises and for the bottom quartile of robotization intensity.[22] Nevertheless, the strength of these effects is still low (Doucouliagos, 2011, Stanley et al., 2018).

[22] All the heterogeneous meta-effects, except for "Data midpoint before year 2008", are valid for data midpoints after 2007. For earlier time periods, i.e., if *period_2007* is additionally assumed to be equal to 1, they would be slightly higher.

7 Robustness checks

7.1 Alternative estimators

For the sake of robustness, I implement further meta-analytic estimators. First, Stanley et al. (2024) discuss that meta-analyses of $PCCs$ may be biased, especially if primary studies rely on small samples ($n < 200$). To reduce this potential bias, they propose an adjustment to the degrees of freedom in the calculation of $PCCs$ by adding 3 and employing UWLS, i.e.:

$$PCC_{adj_{is}} = \frac{t_{is}}{\sqrt{t_{is}^2 + df_{is} + 3}} \tag{14}$$

This meta-analytic estimator is called $UWLS_{+3}$. Stanley et al. (2024) admit that this adjustment may be not a "notable factor" in economic meta-analyses where primary studies are econometric studies which typically involve at least hundreds of observations. Thus, I expect that the results from $UWLS_{+3}$ will be in line with the previously presented findings.

Second, Hong and Reed (2024) suggest a "smooth estimator" which may perform better than the previously used UWLS estimators based on Equations (1) and (4). Their adjustment affects the weighting scheme by using the meta-analytic sample mean of $PCCs$ in the formula for the standard error of PCC:

$$SE_{PCC_{is}} = \sqrt{\frac{1 - \overline{PCC_{is}}^2}{df_{is}}} \tag{15}$$

Third, a drawback of the PCC is that its distribution is not normal when its value is close to -1 and $+1$ (Stanley and Doucouliagos, 2012, p. 25). To address this issue, I follow van Aert (2023) and apply a Fisher's z transformation to $PCCs$:

$$z = \frac{1}{2} \times \ln\left(\frac{1 + PCC_{is}}{1 - PCC_{is}}\right) \tag{16}$$

The sampling variance of the Fisher's z-transformed PCC is equal to:

$$s^2 = \frac{1}{N - 3 - (k - 1)} = \frac{1}{df_{is} - 1} \tag{17}$$

This Fisher's z-transformation also addresses the issue that the sampling variance (and standard error) of the PCC is a function of the PCC itself (van Aert, 2023).

Fourth, I follow Picchio and Ubaldi (2024) and apply a winsorization of the effect size variables at the 5th and 95th percentiles of their distributions; i.e., t-statistics and degrees of freedom below the 5th percentile and above the 95th percentile are replaced with the values of the 5th and 95th percentiles, respectively. I then compute the $PCCs$ using the winsorized t-statistics and degrees of freedom. This robustness check controls for an undue influence of extreme values in the effect size variables.

Table 9: FAT-PET with alternative estimators

	UWLS+3		Smooth estimator		Fisher's z		Winsorization	
	(1) FE2	(2) RE2	(3) FE2	(4) RE2	(5) FE2	(6) RE2	(7) FE2	(8) RE2
Publication bias (β_1)	2.1967***	1.8852***	2.0297***	1.8199***	2.1037***	2.1246***	2.4109***	2.2048***
	(.3865)	(.4632)	(.3662)	(.42)	(.3749)	(.4565)	(.3204)	(.4163)
True effect (β_0)	.0078***	.021*	.008***	.0171*	.0079***	.0121	.0074***	.0143**
	(.0026)	(.0116)	(.0025)	(.0097)	(.0025)	(.0106)	(.0024)	(.0071)
Observations	1801	1801	1801	1801	1801	1801	1801	1801

Notes: Standard errors clustered at study level are in parentheses. The respective estimator and weights used in the unrestricted WLS estimation are indicated in the column header. $^{***}p < .01$,$^{**}p < .05$, $^{*}p < .1$

7.1.1 Publication bias

Table 9 shows that my FAT-PET findings reported in Section 5 are robust to the alternative meta-analytic estimators presented in Section 7.1. There is still clear evidence for a substantial to severe publication selection bias in favor of positive results and little evidence for a meaningful effect of robots on productivity beyond publication bias.

7.1.2 Drivers of heterogeneity

For the sake of robustness, I also implement the alternative meta-analytic estimators for the multivariate MRA with the 12 selected moderator variables from Section 6.2. Additionally, I employ WLS with precision weights only (i.e., not adjusted for the number of estimates per study) and MAIVE with an instrumented SE_{PCC}. Table 10 shows the results of these robustness checks. A positive publication selection bias with a substantial to severe magnitude is still evident. Likewise, the main drivers of heterogeneity identified in Section 6.2 are robust to the alternative meta-analytic estimators: evidence is still found for diminishing returns to robotization, as well as adjustment costs at low levels of robot adoption. As before, the magnitude of the estimated regression coefficients and the constant term is rather small in terms of $PCCs$ (Doucouliagos, 2011).

7.2 Bayesian Model Averaging

To address model uncertainty and multicollinearity in light of the 38 moderator variables as potential drivers of heterogeneity, I follow several recent meta-analyses, as well as the guidelines from Irsova et al. (2023), and employ Bayesian model averaging (BMA) (Havranek et al., 2017; Havranek et al., 2018b; Havranek et al., 2018a; Cazachevici et al., 2020; Duan et al., 2020; Zigraiova et al., 2021; Iwasaki and Kočenda, 2024; Malovaná et al., 2024). The sequential t-testing involved in the stepwise GTS procedure implemented in Section 6.2 risks excluding an important variable at some step because it does not take into account the conditionality of the results on the previous t-tests (Zigraiova et al., 2021). BMA, instead, does not select a specific

Table 10: Multivariate MRA with alternative estimators

				Smooth	Fisher's z	Winsorized
	(1)	(2)	(3)	(4)	(5)	(6)
	UWLS FE1	MAIVE	UWLS+3 FE2	UWLS FE2	UWLS FE2	UWLS FE2
SE_{PCC}	2.1131***	1.8008***	2.8719***	2.7047***	2.793***	2.8664***
	(.4465)	(.4465)	(.4045)	(.3863)	(.394)	(.3376)
dev_country	.0016	.0044	.0067***	.0066***	.0067***	.0083***
	(.0028)	(.0061)	(.0023)	(.0023)	(.0023)	(.003)
period_2007	.0159***	.0116	.0156***	.0154***	.0155***	.0093**
	(.004)	(.0092)	(.0047)	(.0048)	(.0048)	(.0038)
large	-.1107**	-.0925**	-.1385***	-.1285***	-.1353***	-.1467***
	(.0429)	(.0386)	(.0334)	(.0324)	(.033)	(.0299)
secondary_sec	-.0228***	-.0291***	-.0202**	-.0191***	-.019***	-.0128*
	(.0086)	(.0101)	(.0078)	(.0072)	(.0072)	(.0071)
r_quartile_1	-.2171***	-.2003***	-.2225***	-.2057***	-.2132***	-.1611***
	(.006)	(.0054)	(.0251)	(.0215)	(.0226)	(.0123)
lc_omit	.0049***	.0103**	.0083***	.008***	.0081***	.0073***
	(.0014)	(.0046)	(.0025)	(.0024)	(.0024)	(.0026)
non_ifr	-.018***	-.0241***	-.0142***	-.014***	-.0141***	-.0072
	(.0028)	(.0083)	(.0042)	(.0042)	(.0042)	(.0046)
green_tfp	-.0131	-.0088	-.0367**	-.0355**	-.0364**	-.0341**
	(.0125)	(.0121)	(.0142)	(.0136)	(.0138)	(.0137)
growth	.0032	-.0136*	.0078**	.0078*	.0078*	.0055
	(.004)	(.0075)	(.0039)	(.004)	(.004)	(.0042)
quant_reg	.0724***	.0483***	.0577**	.0447**	.0493**	.0237**
	(.0137)	(.0158)	(.0253)	(.0188)	(.021)	(.0102)
c_num	-.002***	-.0014*	-.0011***	-.001***	-.0011***	-.0007**
	(.0008)	(.0008)	(.0003)	(.0003)	(.0003)	(.0003)
marginal_comp	-.0308*	-.0451**	-.0195***	-.0186***	-.0193***	-.0199***
	(.0176)	(.0184)	(.0069)	(.0064)	(.0066)	(.0066)
_cons	.0269***	.0341**	.0126	.012	.0117	.0058
	(.0092)	(.0153)	(.0081)	(.0074)	(.0075)	(.0076)
Observations	1813	1813	1813	1813	1813	1813
R-squared	.4078	.3109	.4333	.4575	.4454	.4045

Notes: Standard errors clustered at study level are in parentheses. The respective estimator and weights used in the unrestricted WLS estimation are indicated in the column header. $^{***}p < .01$, $^{**}p < .05$, $^{*}p < .1$

model but estimates many regressions using different subsets from the list of moderator variables. Then, a weighted average of all the estimated regression coefficients and the associated standard errors is computed, with weights equal to the posterior model probability (PMP) according to Bayes' theorem. The PMP indicates how well the respective model fits the data. As I consider 38 moderator variables in addition to SE_{PCC}, the model space is represented by 2^{39} possible models. Since it is not feasible to estimate all possible models, BMA samples model specifications from the model space through Monte Carlo Markov Chain (MCMC) sampling, which only considers models with a relatively high PMP (Zeugner and Feldkircher, 2015).[23] BMA further computes a posterior inclusion probability (PIP) for each moderator variable, which is equal to the sum of the PMPs of all the models that include the respective moderator variable. Thus, the PIP measures the probability that a moderator variable belongs to the "true" model (Cazachevici et al., 2020). I implement the BMA approach using the BMS package in R (Zeugner and Feldkircher, 2015). BMA requires specifying priors regarding model size (model prior) and regression coefficients (Zellner's g-prior). In the baseline setting, I opt for a uniform model prior and the unit information prior (UIP) on Zellner's g to represent my lack of prior knowledge, thereby complying with the recommendation of Eicher et al. (2011). The UIP sets $g = N$ for all possible models, i.e., it attributes the same information to the prior as is contained in one primary estimate. The uniform model prior implies a prior expected model size of $k/2$. For a detailed review of model averaging and its applications in economics, the reader is referred to Steel (2020).

Since BMA is based on the OLS estimator and does not offer a weighting option, one cannot precisely recreate the UWLS-FE2 estimator of Section 6.2 in the BMA framework. This is only possible for the UWLS-FE1 estimator of Eq. 12 because an OLS regression with weighted variables (i.e., multiplied by precision) and an intercept is equivalent to the UWLS counterpart with FE1 weights:[24]

$$t_{is} = \beta_0 / SE_{PCC_{is}} + \beta_1 + \sum \beta_k Z_{kis} / SE_{PCC_{is}} + \epsilon_{is} \tag{18}$$

Therefore, the BMA approach does not use weights that adjust for the number of estimates per study. As such, this robustness check does not only address model uncertainty but also the sensitivity to mere precision weighting. BMA automatically includes a constant, and this

[23] I use 2 million iterations with 1 million burn-ins to achieve convergence.

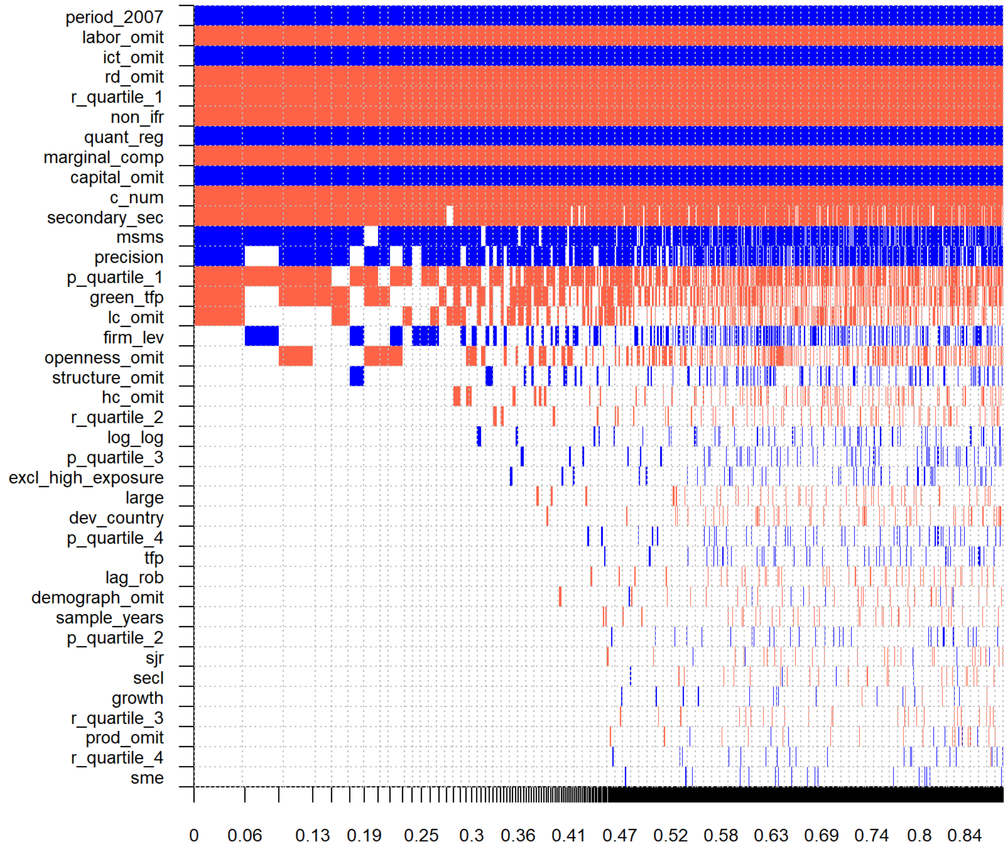
[24] This does not hold true for FE2 weights as multiplying SE_{PCC} by the FE2 weight does not recreate the intercept. An OLS regression with FE2-weighted variables, therefore, requires to suppress the constant term and, instead, to include a FE2-weighted constant among the independent variables. The BMA framework, however, automatically estimates a constant. Nevertheless, applying FE2-weighted variables in the BMA framework with an unweighted constant would support the evidence of the 12 selected moderator variables presented in Section 6.2. Each of the 12 moderator variables would be assigned a PIP above 0.5, and mostly above 0.75. Due to the computational inconsistency, these BMA results are not shown here but are available upon request.

constant corresponds to the coefficient of SE_{PCC} in UWLS-FE1, while the estimated coefficient of *precision* corresponds to the constant in UWLS-FE1. Moreover, BMA does not allow a clustering of standard errors and, thus, entails a less conservative statistical inference, since it does not address the potential dependence of primary estimates from the same study. Figure 6 illustrates the inclusion of moderator variables in the BMA framework. The moderator variables are listed on the vertical axis, sorted by their PIPs in descending order. Blue color indicates a positive coefficient, red signals a negative coefficient, and white corresponds to non-inclusion. On the x-axis the 5,000 best models are shown, scaled by their PMPs. I will focus only the moderator variables with a PIP of above 0.5, following the classification described in Malovaná et al. (2024), based on Jeffreys (1961) and Kass and Raftery (1995): the evidence of an effect is deemed weak if the PIP is between 0.5 and 0.75, substantial if the PIP is between 0.75 and 0.95, strong if the PIP is between 0.95 and 0.99, and decisive for a PIP above 0.99. 14 moderator variables are found to pass the threshold of a PIP of at least 0.5. Eight of these variables align with the drivers of heterogeneity identified by the GTS approach in Section 6.2. The variables *dev_country*, *lc_omit*, *large*, and *growth*, however, seem to be sensitive to the weighting scheme, as they are not found to be important variables in the BMA approach with mere precision weighting. Instead, the precision-weighted BMA indicates that *labor_omit*, *ict_omit*, *rd_omit*, *capital_omit*, *msms*, and *p_quartile_1* are relevant variables, which were not included in the GTS selection in Section 6.2.

Table 11 shows the BMA results in more detail. In addition to the PIP, the posterior mean (Post Mean) displays the variable's estimated coefficients averaged over all models, including the models wherein the respective variable was not contained. Analogously, the posterior standard deviation (Post SD) reports the weighted average of that variable's estimated standard errors. Additionally, "Cond. Pos. Sign" indicates the "sign certainty" of the variable's coefficient by displaying the "posterior probability of a positive coefficient expected value conditional on inclusion" (Zeugner and Feldkircher, 2015, p. 5). A value of 1 suggests that the variable's coefficient always has a positive sign. The BMA findings corroborate the evidence of diminishing returns to robot adoption: *period_2007* has a PIP of 1 and always exhibits a positive coefficient; *secondary_sec* has a PIP close to 0.95 and always shows a negative coefficient. Likewise, the evidence of adjustment costs at low intensities of robot use is confirmed: *r_quartile_1* has a PIP of 1 and is always assigned a negative coefficient. The relevance and sign of the variables *c_num*, *non_ifr*, *quant_reg*, *marginal_comp,* and *green_tfp* are also reinforced. Beyond that, the BMA results suggest that a treatment of endogeneity (*msms*) in primary estimations may slightly increase the estimated effects. Moreover, not controlling for labor input and R&D may cause a small negative omitted variables bias, while not controlling for general capital input and ICT capital may overestimate the effects. The result for *p_quartile_1* suggests that it can be somewhat more difficult to realize productivity gains for entities at the lower end of the productivity distribution. As before, the magnitude of the moderator variables' estimated coefficients is quite small, with posterior means that are mostly close to zero.

As a frequentist check, I run a UWLS-FE2 regression with all moderator variables that are

Figure 6: Inclusion of moderator variables in BMA



Notes: This figure presents the BMA results, based on Eq. 18. Columns represent individual models, and the moderator variables are listed on the y-axis, sorted by their PIPs in descending order. The x-axis shows cumulative PMPs for the 5,000 best models.

assigned a PIP above 0.5 by BMA. This reveals that only those moderator variables attain statistical significance, which have already been identified as drivers of heterogeneity by the GTS approach in Section 6.2. Last but not least, the BMA approach reinforces the evidence of a severe positive publication bias.

To rule out a sensitivity of the BMA results to the choice of priors, Figure 7 shows the moderator variables' PIPs, based on alternative model and g-priors. The PIPs of the variables with the highest PIPs in the baseline setting is very robust to alternative priors. The hyper g-prior generally results in larger PIPs with lower PIPs in the baseline setting.

8 Discussion

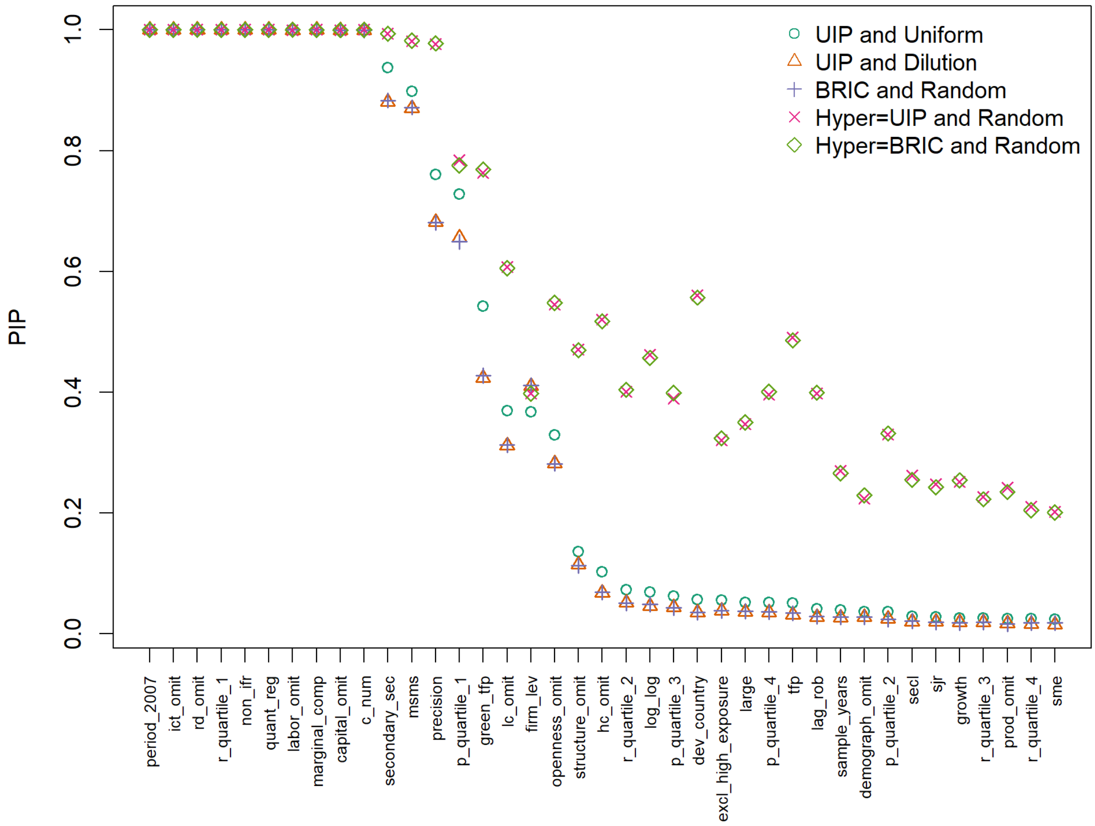
The central finding of a negligible meta-analytic effect of industrial robot deployment on productivity, beyond publication selection bias, points to a potential continuation of the Solow paradox and may be referred to as a "modern productivity paradox" (Brynjolfsson et al., 2019). This raises the question of what are the reasons for the absence of a productivity boost. In the existing literature on the productivity paradox and technology adoption, several arguments

Table 11: BMA results

	BMA				Frequentist check, UWLS FE2	
	PIP	Post Mean	Post SD	Cond.Pos.Sign	Coef	SE
period_2007	**1**	0.0170	0.0022	1	0.0159***	0.0044
labor_omit	**1**	-0.0186	0.0038	0	-0.0025	0.0052
ict_omit	**1**	0.0497	0.0046	1	0.007	0.0047
rd_omit	**1**	-0.0522	0.0051	0	-0.0023	0.0067
r_quartile_1	**1**	-0.2121	0.0187	0	-0.2157***	0.0287
non_ifr	**1**	-0.0241	0.0024	0	-0.0191***	0.0036
quant_reg	**1**	0.1276	0.0119	1	0.0629**	0.028
marginal_comp	**1**	-0.0307	0.0055	0	-0.0153*	0.0086
capital_omit	**1**	0.0121	0.0034	1	-0.0035	0.0054
c_num	**0.9999**	-0.0022	0.0005	0	-0.0012***	0.0003
secondary_sec	**0.9432**	-0.0219	0.0084	0	-0.0238**	0.01
msms	**0.9068**	0.0051	0.0023	1	0.0025	0.0032
precision	**0.7623**	0.0349	0.0228	1	0.0326***	0.0106
p_quartile_1	**0.7409**	-0.0066	0.0046	0	-0.0122	0.0073
green_tfp	**0.5555**	-0.0189	0.0188	0	-0.0285*	0.0147
lc_omit	0.3744	-0.0029	0.0042	0		
firm_lev	0.3548	0.0128	0.0187	0.9997		
openness_omit	0.3207	-0.0023	0.0037	0		
structure_omit	0.1234	0.0011	0.0034	1		
hc_omit	0.0919	-0.0011	0.0040	0		
r_quartile_2	0.0574	-0.0016	0.0078	0		
log_log	0.0565	0.0001	0.0007	1		
p_quartile_3	0.0483	0.0002	0.0011	1		
excl_high_exposure	0.0440	0.0007	0.0039	1		
large	0.0404	-0.0056	0.0351	0		
dev_country	0.0398	-0.0002	0.0011	0.0014		
p_quartile_4	0.0376	0.0001	0.0010	1		
tfp	0.0341	0.0001	0.0010	0.9990		
lag_rob	0.0292	-0.0001	0.0006	0.0045		
demograph_omit	0.0278	-0.0001	0.0008	0.2089		
sample_years	0.0272	0.0000	0.0000	0.0055		
p_quartile_2	0.0250	0.0001	0.0006	1		
sjr	0.0199	0.0000	0.0002	0.1535		
secl	0.0188	0.0000	0.0004	0.1724		
growth	0.0182	0.0000	0.0004	0.7926		
r_quartile_3	0.0178	-0.0001	0.0024	0		
prod_omit	0.0174	0.0000	0.0005	0.2828		
r_quartile_4	0.0172	0.0001	0.0018	0.9736		
sme	0.0161	0.0001	0.0035	0.9829		
Publication bias	**1**	2.3215	NA	NA	2.5480***	0.4425
Observations	1813				1813	

Notes: BMA is based on FE1 weights. The frequentist check is a UWLS estimation with FE2 weights and includes variables that have a PIP greater than 0.5, according to BMA. PIPs above 0.5 are highlighted in bold. Standard errors (SE) in the frequentist check are clustered at the study level. $^{***}p < .01$, $^{**}p < .05$, $^{*}p < .1$

Figure 7: Sensitivity of PIPs to alternative priors



Notes: UIP and Uniform = baseline setting, recommended by Eicher et al. (2011). UIP and Dilution = priors proposed by George (2010). The dilution model prior penalizes models with highly collinear regressors (Zeugner and Feldkircher, 2015). BRIC and Random = "BRIC" sets $g = max(N, k^2)$ according to Fernández et al. (2001) and "random" implements the binomial-beta model prior according to Ley and Steel (2009). Hyper sets a hyper g-prior (Liang et al., 2008; Feldkircher and Zeugner, 2009), with a prior expected shrinkage factor either equal to the UIP prior or to the BRIC prior (Feldkircher and Zeugner, 2012).

and mechanisms have been put forward that can also be applied in the context of robotization. These aspects can be grouped into eight overarching categories:

(1) *Compensation mechanisms*: If robot adoption increases (or decreases) value added and employment in the same proportion, this will leave labor productivity unchanged (Capello et al., 2022). The compensating effects originate from two channels through which robot adoption can influence labor productivity. On the one hand, it is frequently assumed that robots displace workers, thereby reducing the quantity of labor input (Acemoglu and Restrepo, 2018c; Acemoglu and Restrepo, 2020). On the other hand, it is typically expected that robotization enables the expansion of output and higher market shares by increasing competitiveness compared to competitors without robot deployment, i. e., a market size effect (Koch et al., 2021; Graetz and Michaels, 2018). Taken together, these two effects would reinforce each other and boost labor productivity, which is why Capello et al. (2022) dismiss compensation mechanisms as an explanation for a productivity paradox of automation. However, the economic literature is far from reaching consensus on the labor market effects of robots. The meta-analyses of Jurkat et al. (2023) and Guarascio et al. (2024) find negligible effects of robot adoption on wages and employment. Moreover, the labor market effects are likely very heterogeneous across different occupational and demographic groups of workers as well as economic sectors (Albinowski and Lewandowski, 2024; Dauth et al., 2021; Adachi, 2024). If the productivity or scale effect of robots were strong enough to dominate the displacement effect, employment could rise as a consequence of robot adoption (Acemoglu and Restrepo, 2018c; Acemoglu and Restrepo, 2018b). This align with Koch et al. (2021), who report a net job creation for robot-adopting firms. Thus, it cannot be ruled out a priori that growing labor demand may counteract output gains, thereby dampening measured productivity.

Another compensatory mechanism may be that robotic capital is merely a continuation of former mechanization and automation technologies (Fernández-Macías et al., 2021). In the context of information technology (IT), Dewan and Min (1997) found that IT capital is a net substitute for other forms of capital, such that an increase in IT capital per employee is accompanied a reduction in non-IT capital per employee, leading to an offsetting effect (Schweikl and Obermaier, 2020). Analogously, one could argue that, while robotic capital has been more frequently used, other forms of capital may have been withdrawn.

(2) *Reallocation effect*: Automation may be accompanied by significant reallocation effects across sectors. Dauth et al. (2021) show that negative employment effects of robot adoption in the manufacturing sector is compensated by positive employment effects in the service sector. Similarly, Dottori (2021) finds that labor is reallocated toward less robot-intensive industries. More negative wage effects in manufacturing, mirrored by more positive wage effects in non-manufacturing, are also evident in the meta-analysis by Jurkat et al. (2023). In the ICT context, Autor and Dorn (2013) observe a reallocation of low-skill labor into service occupations. Capello et al. (2022) argue that a reallocation of workers from more productive, robot-adopting manufacturing industries towards less productive industries is the main reason for a productivity paradox of robotization.

(3) *Concentrated robot adoption and productivity gains*: If productivity gains are concentrated in a few highly innovative and productive firms or sectors with a limited share of the overall economy, they will have little influence on the aggregate productivity dynamics (Capello et al., 2022). Recent research points to increasing productivity differences between firms at the frontier and average firms in the same industry (Andrews et al., 2016; Furman and Orszag, 2018), while a small number of superstar firms are expanding their market share (Autor et al., 2017; Autor et al., 2020). Stiebale et al. (2024) provide evidence of the superstar phenomenon in the context of robot adoption. It is, however, not reflected in my meta-analysis, since the moderator variables for the productivity quartile of analyzed entities in the primary literature are not identified as important drivers of heterogeneity. A significantly higher productivity effect of robot adoption in the the top quartile of the productivity distribution (*p_quartile_4*) would have pointed to concentrated productivity gains among the most productive entities.

Apart from a potentially concentrated distribution of productivity gains, the distribution of robot adoption must also be taken into account. Fernández-Macías et al. (2021) and Deng et al. (2024) show that robot adoption is highly concentrated in certain industries (especially automotive) and applications (particularly handling operations and machine tending), as well as among a small share of firms that are typically larger and more productive. Based on Hulten's theorem, one can argue that the effect of robots on TFP growth hinges on the GDP share of tasks impacted by robots (Acemoglu, 2024; Hulten, 1978). However, this share is limited due to the concentrated distribution of robot usage across industries, applications, and firms. Moreover, a concentrated adoption of robots limits the disruptiveness of robotization what concerns network and spillover effects. Deng et al. (2024) report that only 1.55% of plants in Germany used robots in 2018 (even the manufacturing sector had a share of only 8.22%), despite Germany being one of the countries with the highest robot density in the world (IFR, 2023, p. 74). Such a small share of robot users is naturally far from unleashing the full potential of network effects through harmonized production systems along or across value chains (Katz and Shapiro, 1994; Birke, 2009). It further clarifies that potential knowledge spillovers in implementing robotized productions systems cannot have yet materialized to a great extant (Agarwal et al., 2010; Schweikl and Obermaier, 2020). This leads over to another important explanatory factor for the productivity paradox, namely implementation lags and adjustment costs.

(4) *Adjustment delays*: The economic history has already brought forth several industrial revolutions enabled by general purpose technologies such as the steam engine, electrification, or

ICT (Bresnahan and Trajtenberg, 1995).[25] It is well-documented that it took many decades for these technologies to diffuse and for their benefits to fully unfold. For instance, Crafts (2004) shows that it took roughly 100 years after the invention of the steam engine for its contribution to economic growth to peak. This can be attributed to comprehensive adjustment processes, co-inventions, and the complementary investments required to fully leverage the productivity-boosting potential of a new breakthrough technology (Capello et al., 2022; Brynjolfsson et al., 2019). The adjustment delay may be illustrated as a "productivity J-curve" (Brynjolfsson et al., 2021). This pattern emerges from investments in intangible assets such as organizational capital (e.g., business strategies, corporate culture, decision processes, branding), R&D and intellectual property, as well as human capital (e.g., training, learning processes, experience), all of which are required to make productive use of the new technology. As these intangible assets remain largely unmeasured in economic statistics, the adaptation phase of introducing a new technology can be described as a situation where measurable capital and labor input produce a significant share of unmeasured or poorly measured intangibles (Brynjolfsson et al., 2020; Brynjolfsson et al., 2021). Since input factors are employed without producing anything of measurable value, this will result in a drop in productivity growth. Later, when the intangible investments begin to pay off, the situation is reversed: unmeasured intangible assets generate measured output, and productivity growth may be overestimated (Brynjolfsson et al., 2021). The small negative productivity effect found for the bottom quartile of robot intensity (*r_quartile_1*) in my multivariate MRA may be attributed to these adjustment processes and summarized as adjustment costs associated with robot adoption. Such adjustment costs can also take the form of consultancy services from robot integrators to redesign the production system (Leigh and Kraft, 2018).

Moreover, the reinstatement effect of automation technologies is assumed to create new tasks or completely new occupations in which workers have a comparative advantage over robots (Acemoglu and Restrepo, 2018c; Hötte et al., 2023). Workers who are reinstated in new tasks and jobs enter learning processes, need to gain experience, and may possibly acquire entirely new skills. Here, also institutional aspects like the efficiency of labor markets and educational systems come to play. A mismatch between the skills of labor force and the requirements associated with robotic production systems will hamper productivity growth (Schweikl and Obermaier, 2020). Furthermore, Acemoglu (2024) argues that AI may also create new "bad tasks" like cyber at-

[25] According to Schwab (2016) and Skilton and Hovsepian (2018, pp. 3-24), four industrial revolutions can be distinguished. The 'first industrial revolution' spans the period from the 1780s century to the 1870s and is characterized by mechanization through steam engines. The 'second industrial revolution' extends from the 1880s to the 1930s and is marked by electrification on an industrial scale and the development and use of internal combustion engines. The 'third industrial revolution' describes the period from the 1950s to the 1990s, characterized by the emergence of ICT through the development of microelectronics and semiconductors, as well as the beginnings of the internet. The 'fourth industrial revolution' began at the start of the 21st century and refers to the development of new technologies that merge the physical, digital, and biological spheres (e.g., robotics, super and quantum computing, genetic engineering, micro- and nanotechnology). Industrial production production is increasingly based on cyber-physical systems, in which robots play a decisive role.

tacks. In the context of AI-based robotics (or cyber-physical production systems in general), adopting firms may be compelled to invest more in cyber-security to avoid malicious attacks or industrial espionage. Such protective measures will likely not improve the efficiency of the production process but limit any cost savings achievable from automation.

(5) *Diminishing returns to robot adoption*: As described in Section 6.2, my multivariate MRA points to decreasing productivity returns from robotization.[26] Following up on the task-based modeling of automation, diminishing returns to robot adoption can be attributed to an increasing level of difficulty and effort required to automate further tasks. Acemoglu (2024) differentiates between "easy-to-learn tasks" and "hard tasks" for AI applications. Likewise, one can argue that repetitive manual, routine tasks can easily be taken over by robots, whereas more complex tasks are much more challenging to implement in robotic production systems and do not provide significant productivity gains compared to human labor (Vries et al., 2020). Similarly, Acemoglu and Restrepo (2018b) and Acemoglu and Restrepo (2019) worry about "so-so technologies" that are just good enough to replace human workers without significantly boosting productivity. Fully unleashing the potential network externalities and spillover effects of intelligent automation may help to overcome the diminishing returns to robot adoption in the future (Wu et al., 2024).

(6) *Measurement issues*: In the context of the Solow paradox, many researchers viewed measurement problems as one explanatory factor (Baily et al., 1988; Brynjolfsson, 1993; Triplett, 1999). An exact measurement of productivity requires that both the output quantity and quality are adequately measured. Concerning the output measurement, there is a well-known challenge of adjusting prices for inflation and quality changes in order to obtain comparable real values that enter the numerator of labor productivity (growth) at constant prices (Schweikl and Obermaier, 2020; Capello et al., 2022). On the one hand, robot adoption may lead to product differentiation or improved product quality, allowing for price increases (e.g., customized products) (Capello et al., 2022; DeStefano and Timmis, 2024). On the other hand, robots may be used as a cost-cutting technology that allows for price decreases (Acemoglu and Restrepo, 2018c; Graetz and Michaels, 2018; Hötte et al., 2024). Even more challenging may be estimating the value of completely new goods and services that emerge from the deployment of modern technologies like robots, if they have no comparable predecessors (Brynjolfsson, 1993). If the mismeasurement of the "new economy" becomes worse over time, this would contribute to a slowdown in measured productivity growth (Brynjolfsson et al., 2020). Recent articles, however, suggest that this source of mismeasurement is likely insufficient to explain a modern productivity paradox because mismeasurement has always been an issue that also affected past innovations (Brynjolfsson et al., 2020; Byrne et al., 2016; Syverson, 2017). Another measurement issue has already

[26] As explained in Section 6.2, diminishing returns are suggested by the coefficients of *period_2007*, *secondary_sec*, *large*, and *dev_country*. Another obvious indicator of diminishing returns would have been a negative and significant coefficient for the top quartile of robot intensity (*r_quartile_4*). This moderator variable, however, seems to be dominated by the other moderator variables that already capture the heterogeneity between primary estimates with regard to diminishing productivity returns.

been described above in the context of intangible investments.
In addition to difficulties in properly measuring productivity, there might also be measurement problems involved in robot capital. Jurkat et al. (2022) discuss the limitations of the IFR dataset, the most frequently used source for robot data. In particular, the IFR's construction of the operational stock of robots is not adjusted for quality changes due to technological progress. Thus, a robot installed today is assumed to have the same quality as a robot installed in 1993, as both are simply counted as one unit installed in the respective year. Assuming that technological progress improves the quality of robots over time, a quality-adjusted measure of the robot stock would, ceteris paribus, grow faster than the number of units reported by the IFR. This would imply that the true value of robots as an input factor is underestimated in more recent time periods and overestimated in earlier periods (Kromann et al., 2020), thereby potentially contributing to an overestimation of TFP in recent years. Thus, the diminishing returns to robotization found in my multivariate MRA cannot be attributed to ill-measured robot capital but would have been even stronger with a quality-adjusted measure of robots.
(7) *Exaggerated expectations*: It could be the case that industrial robots are simply not as productivity enhancing as expected. One could argue that earlier breakthrough innovations, such as steam power, internal combustion engines, electricity, or computers, have had a much more far-reaching impact than industrial robots such that after previous industrial revolutions, productivity growth is simply returning to its more modest long-term trend (Brynjolfsson et al., 2019, pp. 40-41; Brynjolfsson et al., 2020; Schweikl and Obermaier, 2020). In this vein, Fernández-Macías et al. (2021) soberly view industrial robots as the "latest iteration of the long-term process of industrial mechanisation and automation rather than a radical departure."
(8) *Mismanagement*: A last argument put forward by Schweikl and Obermaier (2020) is that managers have not succeeded in effectively implementing and utilizing modern technologies such as robots. If managers primarily focus on cost-cutting and neglect required investments in intangible assets, robots will likely not be used efficiently, and the company may even lose innovative power (Antonioli et al., 2024). Moreover, exaggerated expectations (a "robot hype"), overconfidence, pressure from shareholders (Lim et al., 2013), or tax incentives (Acemoglu et al., 2020b) may tempt managers to excessively invest in automation. A famous example is the production of Tesla Model 3, which was characterized by too many robots in the assembly line (Büchel and Floreano, 2018, Acemoglu and Restrepo, 2019). This caused Elon Musk (2018) to admit: "Yes, excessive automation at Tesla was a mistake. To be precise, my mistake. Humans are underrated." This is especially true in complex environments that require flexible adaptability to unforeseen situations (Büchel and Floreano, 2018). Mismanagement, and especially excessive automation, might also explain the small negative productivity effect found among large enterprises in my multivariate MRA.

9 Conclusion

In this study, I meta-analyze for the first time the relationship between the adoption of industrial robots and productivity. Through a systematic literature review, supported by machine learning, I identify 81 relevant primary studies with more than 1800 estimates of the productivity effect of robots. I find strong evidence that this empirical literature suffers from a substantial to severe positive publication bias. This finding is observed across all measures of productivity used in the primary literature and is robust to several modern, meta-analytic estimators, as well as subsets of the primary literature (firm-level estimates, estimates with a treatment of endogeneity, elasticity estimates, and estimates from higher-quality journals). Beyond publication bias, there is only limited evidence for a positive effect of robots on productivity. At best, robotization has so far exerted only a marginal boost to productivity.

My multivariate MRA of the drivers of heterogeneity in the primary literature points to adjustment costs at low intensities of robot use, as well as diminishing returns at more advanced levels of robotization. Diminishing returns are indicated by (i) smaller, more recent estimates despite accelerated robot adoption, (ii) smaller productivity effects in the secondary sector, although industrial robots are primarily used in manufacturing, and (iii) significantly smaller effects among large enterprises, even though robot adoption is concentrated in larger companies. Somewhat less robust is the evidence that developing and emerging countries tend to benefit more from robot adoption relative to advanced economies. Likewise, I find some evidence that estimates with green TFP (i.e., adjusted for the emission of pollutants) as the dependent variable are slightly negative, possibly pointing to a rebound effect. Further, I obtain some evidence for data dependence, as estimates relying on data sources other than the IFR are somewhat smaller. However, all the heterogeneous meta-effects that can be computed from my multivariate MRA framework are rather small in magnitude. My findings are quite robust to addressing model uncertainty through a BMA framework.

Eight explanatory factors for the emergence of a productivity paradox in the context of robotics are discussed, namely: (1) compensation mechanisms from an increase in labor input or the crowding out of other forms of capital, (2) an adverse reallocation effect of labor to less productive industries, (3) a concentrated distribution of robot adoption and productivity gains, (4) adjustment delays and costs from complementary investments, (5) diminishing returns to robot adoption, (6) measurement issues related to productivity and robot capital, (7) exaggerated expectations, and (8) mismanagement.

My meta-study provides some directions for future avenues of research. More research is required on the potential reasons for the reoccurrence of the Solow paradox in the context of modern automation technologies, such as industrial robots. Especially, empirical research on adjustment costs associated with robot adoption and intangible investments required for the efficient use of robots is a largely untouched field. Ploughing this field of research might provide answers regarding what kinds of adjustment costs exist, how large they are, and how long they persist. In addition to more empirical evidence on intangible investments that complement robot adoption, this would also contribute to the question of whether robotics can meet the criteria for a general-purpose technology. Another focus could be placed on the causes for the occurrence of diminishing productivity returns to robot installation. Likewise, more empirical research on the productivity effect of robots in developing and emerging countries other than China would enrich the evidence on whether robotization can contribute to economic convergence. Ideally, all research on the productivity effects of robots would complement the IFR data with other data sources. And optimally, the data on robot adoption would include some quality-adjustment to differentiate the current trend to AI-based robotics from earlier waves of industrial robots. Referring to Hulten's theorem, one could argue that AI may significantly increase the share of tasks that can be taken over by robots, thereby potentially scaling up the impact of robots on productivity in the future (Acemoglu, 2024; Hulten, 1978).

Table A1: Moderator variables included in the multivariate MRA

Variable	Description	Mean	SD
Data and estimation characteristics			
firm_lev	= 1, if the analysis is at the firm level; 0 for more aggregated levels of analysis	0.215	0.411
c_num	Number of countries in the sample	11.989	9.273
period_2007	= 1, if the midpoint of the sample period is smaller or equal to year 2007	0.621	0.485
sample_years	Number of years in the sample	13.072	9.142
non_ifr	= 1, if robot data are drawn from another source than IFR	0.138	0.345
lag_rob	= 1, if the robot variable is lagged by at least one period	0.077	0.266
quant_reg	= 1, if a quantile regression is employed	0.365	0.481
msms	= 1, if the criteria for a MSMS score of 3 or 4 are met	0.427	0.495
log_log	= 1, if both dependent and independent variable are logarithmically transformed	0.538	0.499
secl	= 1, if inference is based on clustered standard errors	0.631	0.483
marginal_comp	= 1, if the effect size is based on a marginal effect (non-linear or interaction term)	0.116	0.321
Omitted control variables			
hc_omit	= 1, if the estimation does not control for human capital (e.g. level of education)	0.939	0.240
prod_omit	= 1, if the estimation does not control for initial/ lagged productivity or output	0.819	0.385
labor_omit	= 1, if the estimation does not control for labor input	0.906	0.292
openness_omit	= 1, if the estimation does not control for economic openness (e.g. trade, offshoring, FDI)	0.749	0.434
ict_omit	= 1, if the estimation does not control for ICT capital	0.505	0.500
capital_omit	= 1, if the estimation does not control for overall capital (e.g. total capital stock, non-ICT capital, capital intensity)	0.382	0.486
rd_omit	= 1, if the estimation does not control for research, development, or innovation (e.g. R&D expenditures)	0.880	0.325
lc_omit	= 1, if the estimation does not control for labor costs	0.793	0.406
demograph_omit	= 1, if the estimation does not control for demographic developments (e.g. working age population share, avg. age of population, population growth)	0.908	0.288

structure_omit	= 1, if the estimation does not control for the economic structure (e.g. shares of certain economic sectors like manufacturing)	0.945	0.228
Subpopulations			
dev_country	= 1, if the sample comprises only developing countries	0.220	0.414
sme	= 1, if the sample comprises only small and medium-sized enterprises (<250 employees)	0.007	0.084
large	= 1, if the sample comprises only large enterprises	0.004	0.066
p_quartile_1	= 1, if the sample comprises only the first quartile of the productivity distribution	0.094	0.292
p_quartile_2	= 1, if the sample comprises only the second quartile of the productivity distribution	0.138	0.345
p_quartile_3	= 1, if the sample comprises only the third quartile of the productivity distribution	0.099	0.299
p_quartile_4	= 1, if the sample comprises only the fourth quartile of the productivity distribution	0.092	0.288
r_quartile_1	= 1, if the sample comprises only the first quartile of robot intensity	0.035	0.185
r_quartile_2	= 1, if the sample comprises only the second quartile of robot intensity	0.035	0.185
r_quartile_3	= 1, if the sample comprises only the third quartile of robot intensity	0.041	0.199
r_quartile_4	= 1, if the sample comprises only the fourth quartile of robot intensity	0.042	0.200
secondary_sec	= 1, if the sample comprises only the secondary sector	0.368	0.483
excl_high_exposure	= 1, if the sample excludes the most exposed entities (e.g. plants with highest robot intensity or most robotized industries like automotive)	0.020	0.141
Productivity measure			
lp	= 1, if labor productivity is the dependent variable – omitted category	0.652	0.476
tfp	= 1, if TFP is the dependent variable	0.274	0.446
green_tfp	= 1, if GTFP is the dependent variable	0.074	0.263
growth	= 1, if the estimation uses growth rates of productivity or productivity changes	0.409	0.492
Publication status/ quality			
sjr	SCImago Journal Ranking index for the respective publication year	1.391	1.786

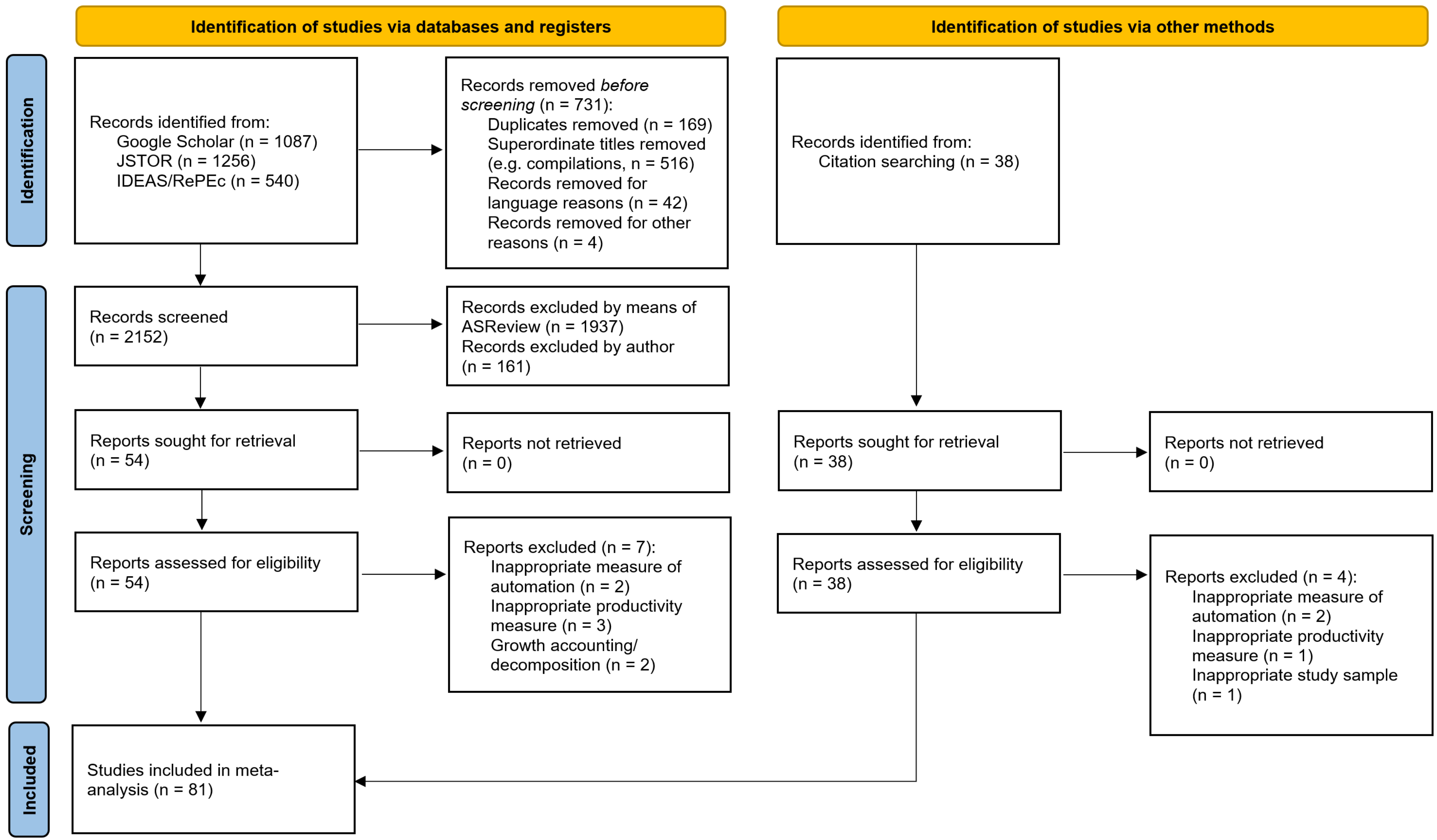


Figure A1: PRISMA 2020 flow diagram following Page et al. (2021).

Table A2: Overview of studies included in the meta-analysis

Study	No. of estimates	Mean of PCC	SD of PCC	Countries	Time span	Robot data	Level of analysis
Acemoglu and Restrepo, 2020	4	0.553	0.132	USA	1992-2007	IFR	Industry
Acemoglu et al., 2020a	14	0.011	0.008	France	2010-2015	Survey by the French Ministry of Industry, SYMOP, customs data, and fiscal files	Firm
Acemoglu et al., 2022	3	0.017	0.017	USA	2018	Annual Business Survey	Firm
Alguacil et al., 2022	1	0.109	.	Spain	1994-2010	Encuesta Sobre Estrategias Empresariales (ESEE)	Firm
Almeida and Sequeira, 2023	549	0.091	0.136	Europe + USA	1995-2017	IFR	Industry
Almeida and Sequeira, 2024	134	0.193	0.133	Europe + USA	1997-2017	IFR	Industry
Antonietti et al., 2023	3	-0.003	0.003	Italy	2008-2017	IFR	Regional
Ballestar et al., 2020	6	0.008	0.038	Spain	2008	ESEE	Firm
Ballestar et al., 2021	4	0.073	0.009	Spain	2000-2016	ESEE	Firm
Bekhtiar et al., 2024	184	0.125	0.102	OECD	2010-2015	IFR	Industry
Bettiol et al., 2024	1	0.027	.	Northern Italy	2010-2017	Own survey	Firm
Bonfiglioli et al., 2024	2	0.012	0.010	France	1994-2013	French Customs Authority DOUANE	Firm

Table A2: Overview of studies included in the meta-analysis

Study	No. of estimates	Mean of PCC	SD of PCC	Countries	Time span	Robot data	Level of analysis
Calì and Presidente, 2022	3	0.010	0.008	Indonesia	2008-2015	IFR	Firm
Camiña et al., 2020	4	0.086	0.008	Spain	1991-2016	ESEE	Firm
Cao et al., 2021	2	0.018	0.000	China	2011-2013	IFR	Firm
Capello et al., 2022	8	0.040	0.091	Europe	2013-2017	IFR	Regional
Chang et al., 2023	19	0.131	0.121	China	2005-2018	China Robot Industry Yearbook	Regional
Chen et al., 2024	30	0.083	0.149	China	2007-2019	IFR	Regional
Cui et al., 2024	1	0.021	.	China	2011-2019	IFR	Firm
Dauth et al., 2018	1	0.102	.	Germany	2004-2014	IFR	Regional
Deng et al., 2024	15	0.025	0.062	Germany	2018	IAB Establishment Panel Survey	Firm
Díaz-Chao et al., 2021	1	0.081	.	Spain	2009-2016	ESEE	Firm
Du and Lin, 2022	30	0.210	0.244	China	2006-2019	IFR	Regional
Duan et al., 2023	56	0.029	0.012	China	2007-2019	IFR	Firm
Fu et al., 2021	12	0.107	0.098	World-wide developed	2004-2016	IFR	Country
Gong et al., 2023	3	0.100	0.027	World-wide	1993-2019	IFR	Country
Graetz and Michaels, 2018	67	0.141	0.036	OECD	1993-2007	IFR	Industry
He et al., 2024	1	0.006	.	China	1998-2013	China Customs Database	Firm
Hötte et al., 2024	56	-0.014	0.054	Europe	1995-2007	IFR	Country

Table A2: Overview of studies included in the meta-analysis

Study	No. of estimates	Mean of PCC	SD of PCC	Countries	Time span	Robot data	Level of analysis
Huang et al., 2022	9	0.015	0.015	China	2001-2010	China Customs Database	Firm
Huang et al., 2023	11	0.010	0.006	China	2000-2013	China Customs Database	Firm
Huang et al., 2024	9	0.160	0.085	China	2006-2019	IFR	Regional
Jäger et al., 2015	2	0.047	0.053	Europe	2008	European Manufacturing Survey	Firm
Jäger et al., 2016	16	0.024	0.041	Europe	2011	European Manufacturing Survey	Firm
Jungmittag and Pesole, 2019	48	0.076	0.033	EU	1995-2015	IFR	Industry
Koch et al., 2021	2	0.022	0.007	Spain	1990-2016	ESEE	Firm
Kromann et al., 2020	17	0.160	0.046	Europe + Japan	2004-2007	IFR	Industry
Leitner and Stehrer, 2019	32	0.509	0.317	EU	2012-2017	IFR	Country
Leone, 2022	2	0.103	0.007	Spain	1993-2014	ESEE	Firm
Li and Zhou, 2024	9	0.107	0.024	China	2003-2017	IFR	Regional
Li et al., 2022	2	0.220	0.001	World-wide	1993-2017	IFR	Country
Li et al., 2023	1	0.008	.	China	2005-2014	IFR	Firm
Li et al., 2024	40	0.019	0.016	China	2000-2013	China Customs Database	Firm

Table A2: Overview of studies included in the meta-analysis

Study	No. of estimates	Mean of PCC	SD of PCC	Countries	Time span	Robot data	Level of analysis
Lin et al., 2022	1	0.041	.	China	2000-2013	China Customs Database	Firm
Liu et al., 2021	3	0.429	0.291	China	2006-2016	IFR	Industry
Liu et al., 2022b	9	0.033	0.012	China	2014-2015	IFR	Firm
Park et al., 2021	60	-0.089	0.328	Republic of Korea	2006-2015	IFR	Industry
Pisková et al., 2024	2	-0.271	0.002	Europe	2002-2021	IFR	Country
Qi and Zhang, 2023	1	0.009	.	China	2000-2015	China Customs Database	Firm
Qian and Wang, 2022	2	-0.001	0.001	China	2011-2019	IFR	Regional
Ren et al., 2018	1	0.080	.	China	2014	China Employer-Employee Survey	Firm
Rodrigo, 2021	3	0.103	0.087	Brazil	2006-2013	Customs data	Regional
Shen and Zhang, 2023	1	0.059	.	China	2006-2020	IFR	Regional
Soliman, 2021	16	0.455	0.159	Europe	2005-2015	IFR	Country
Somohano-Rodríguez and Madrid-Guijarro, 2022	4	-0.021	0.003	Spain - Region Cantabria	2005-2018	I4.0 survey, Regional Ministry of Industry of the Government of Cantabria	Firm
Song et al., 2022	7	0.014	0.009	China	2000-2013	China Customs Database	Firm
Stapleton and Webb, 2023	28	0.030	0.005	Spain	1990-2016	ESEE	Firm

Table A2: Overview of studies included in the meta-analysis

Study	No. of estimates	Mean of PCC	SD of PCC	Countries	Time span	Robot data	Level of analysis
Starovatova, 2023	6	0.089	0.020	Russian Federation	2017	Competitiveness of the Russian Industry database	Firm
Stiebale et al., 2024	94	0.003	0.004	EU	2004-2013	IFR	Firm
Sun et al., 2023	1	0.080	.	Worldwide	1997-2020	IFR	Country
Venturini, 2022	8	-0.071	0.107	Worldwide	1990-2014	IFR	Country
Wang, 2022	3	0.004	0.001	USA	1992-2012	Customs data	Firm
Wang et al., 2022	4	0.007	0.020	Worldwide	2000-2014	IFR	Indsutry
Wang et al., 2023	1	0.037	.	China	2011-2017	IFR	Firm
Wang et al., 2024	9	0.024	0.004	China	2000-2016	China Customs Database	Firm
Weyerstrass, 2018	4	0.533	0.096	EU	1996-2016	IFR	Country
Wu, 2023	9	0.026	0.012	China	2008-2017	IFR	Regional
Xie and Yan, 2024	2	0.236	0.054	China	2006-2019	IFR	Regional
Yang and Liu, 2024	21	0.012	0.014	China	2004-2015	China Customs Database	Regional
Yang and Shen, 2023	18	0.124	0.058	China	2006-2020	IFR	Regional
Zhang and Deng, 2023	3	0.020	0.014	China	2002-2007	IFR	Firm
Zhang and Shen, 2023	1	-0.023	.	China	2011-2019	IFR	Firm
Zhang et al., 2022	10	0.061	0.010	China	2006-2019	IFR	Regional
Zhang et al., 2023b	8	0.007	0.006	China	2000-2012	China Customs Database	Firm

Table A2: Overview of studies included in the meta-analysis

Study	No. of estimates	Mean of PCC	SD of PCC	Countries	Time span	Robot data	Level of analysis
Zhang et al., 2023a	3	0.026	0.011	China	2000-2012	China Customs Database	Firm
Zhang et al., 2024	12	-0.002	0.001	China	2013-2017	IFR	Firm
Zhao et al., 2022	19	0.110	0.116	China	2007-2019	IFR	Regional
Zhao et al., 2024	12	0.175	0.076	China	2013-2021	IFR	Industry
Zhou et al., 2023	9	0.269	0.089	China	2006-2019	IFR	Regional
Zhu and Zhang, 2021	3	0.313	0.814	China	2006-2019	IFR	Country
Zhu et al., 2023	1	0.036	.	China	2000-2012	China Customs Database	Firm

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