Do robots boost productivity? A quantitative meta-study

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Abstract

This meta-study analyzes the productivity effects of industrial robots. More than 1800 estimates from 85 primary studies are collected. The meta-analytic evidence suggests that robotization has so far provided, at best, a small boost to productivity. There is strong evidence of positive publication bias. These findings are observed across all measures of productivity used in the primary literature and are robust to several modern meta-analytic estimators. My analysis of the drivers of heterogeneity among the findings of primary studies points to diminishing returns to robot adoption. I also find evidence that econometric methods, the level of analysis, as well as the choice of control variables and robot data can influence the effect size. 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, Solow paradox

JEL codes**:** O11, O12, O14, O33, O47

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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), and 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.

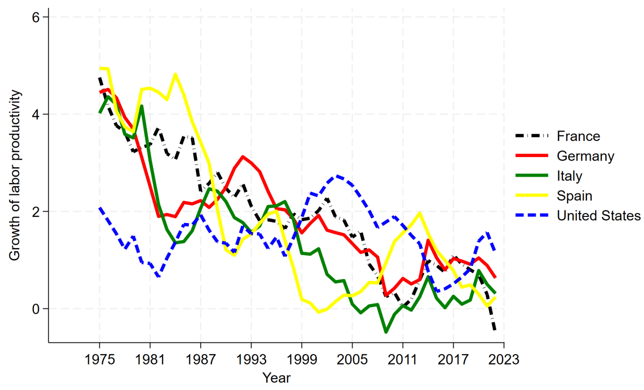
First, this meta-study contributes to the longstanding academic debate on the so-called "productivity paradox" of ICT (Brynjolfsson, 1993). Solow (1987) foundationally expressed in his well-known quote: "You can see the computer age everywhere but in the productivity statistics." Despite the enormous 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; Goldin et al., 2024). 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 reduced 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.

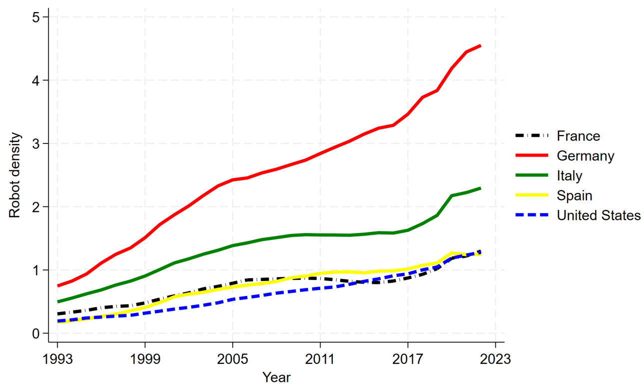
Fourth, by examining whether there are different magnitudes of productivity increases through robotization between advanced and developing/emerging economies, I contribute to explaining economic convergence. For instance, the findings of Eder et al. (2023) suggest that the contribution of robots to labor 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,



(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

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 empirical 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, I identified 85 studies with 1849 estimates for the impact of robots on productivity. Most of these papers are examining the impact of robot use on labor productivity and TFP. I compute partial correlation coefficients ($PCCs$) as comparable effect size and employ several meta-regression models. The meta-analytic evidence suggests that robotization has so far provided, at best, a small boost to productivity. I find strong evidence of positive publication bias. These findings are observed across all measures of productivity used in the primary literature and robust to several modern, meta-analytic estimators. My analysis of the drivers of heterogeneity in the primary literature points to diminishing returns to robot adoption. This can be harmonized with the notion of 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). I also find evidence that econometric methods, the level of analysis, as well as the choice of control variables and robot data can influence the effect size.

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, several robustness checks are implemented. Section 8 discusses explanatory factors that may contribute to 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 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. 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 effects for developing/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 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, thereby possibly improving TFP. 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; Fornino and Manera, 2022). 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, Gechert and Heimberger (2022) the impact of corporate tax cuts, Ridhwan et al. (2022) the role of health, Heimberger (2023) the influence of public debt levels, 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. (2025) meta-analyze the labor-market effects of robots and both find only negligible effects.

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

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

the first article on the productivity effects of robots by Graetz and Michaels (2018) was published in a scientific journal. To identify relevant studies, my literature research was assisted by machine learning using the software ASReview (van de Schoot et al., 2021). 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 started screening titles and abstracts. Defining the criterion for stopping the screening process is left to the reviewer and needs to balance the costs of continued screening with the risk of not including relevant records (Boetje and van de Schoot, 2024). To comply with the guidelines of Irsova et al. (2023) for meta-analyses and the recommendations of Ros et al. (2017), van de Schoot et al. (2021), Campos et al. (2024), and Boetje and van de Schoot (2024) for active learning-based screening, I apply a mixed strategy: screening stops when at least 500 records had been screened AND an uninterrupted sequence of 5% irrelevant records was observed (i.e., a row of $2152 \times 0.05 \approx 108$ irrelevant records).[2] Furthermore, I applied "snowballing" by checking the references of all eligible studies to find additional relevant studies (Irsova et al., 2023). 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: (1) 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 industrial 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., Benassi et al., 2022; Dinlersoz and Wolf, 2024; Horvat et al., 2019; Lyu and Liu, 2021; Xia et al., 2024; Zhu 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). For the sake of comparability, estimations within primary studies

[2] van de Schoot et al. (2021) show that ASReview enables to find 95% of the eligible studies among the records obtained from the literature research after screening between only 8% to 33% of the studies. Information on the reproducibility of ASReview-assisted literature research is provided in Lombaers et al. (2024). An extensive discussion on stopping rules in AI-assisted screening is available in König et al., 2024.

that further split the robot measure into certain robot types (e.g., Deng et al., 2024 and Li et al., 2024) or only consider robots in specific industries to estimate the productivity effects in the full study sample are excluded (e.g., Chang et al., 2023; Zhao et al., 2022). (2) Measures of productivity considered suitable are continuous measures of labor productivity, TFP, or GTFP.[3] 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).[4] 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 methods 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.

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-

[3] 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.

[4] Studies (or estimations within studies) that only use output as the dependent variable are excluded (e.g., Acemoglu et al., 2023; Dixon et al., 2021).

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).

(3) 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.

(4) The study design must provide valid and comparable estimates of the relationship between robots and productivity. Within a few studies, I omitted event study designs that are only 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.[5] My sample of primary estimates also focuses on the direct effect of robots on productivity, while estimates of pure 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 85 primary studies with 1849 estimates. A comprehensive list of all primary studies included is available in the appendix (see Table A1). The number of estimates per study

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

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 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. It captures study-dependent or estimation-specific characteristics and targets potential biases from omitted variables and misspecification. The heterogeneity analysis comprises five main groups of moderator variables: (1) data and estimation characteristics, (2) the measure of productivity, (3) omitted control variables, (4) subpopulations, and (5) publication quality/ status (see Table 1). 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. The description of moderator variables is available in Table 1.

Three levels of analysis are distinguished. 43 studies are of micro-economic nature at the firm level, observing the productivity effect of robots at the level where they are actually deployed. The remainder of estimates comes from more aggregated levels: 12 studies are at the industry level and 30 studies are at the level of geographic units (countries, regions, or cities).

The sophistication of econometric methods is captured through the variable *non_msms*. It indicates estimates that fail to move closer to establishing causality by employing econometric methods meeting the criteria of the "Maryland Scientific Method Scale" (WW-CLEG, 2016) for a score of 3 or 4. Methods satisfying these criteria are instrumental variable (IV) estimations, difference-in-difference (DID) estimations, as well as panel estimations that include year effects, fixed effects at the unit of observation and appropriate control variables).[6] 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

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

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).

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).

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.[7] Unpublished studies are assigned a SJR value of zero. 91% of the primary estimates come from 68 articles published in academic journals with a mean SJR index of 1.9. 17 studies are unpublished, i.e., mainly working papers and some dissertations.

[7]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.

Table 1: Moderator variables included in the multivariate MRA

Variable	Description	Mean	SD	W-Mean	W-SD
Data and estimation characteristics					
firm_lev	= 1, if the analysis is at the firm level (omitted reference category)	0.229	0.420	0.506	0.500
geo_lev	= 1, if the analysis is at the level of geographical units (countries, regions, cities, etc.)	0.186	0.389	0.353	0.478
ind_lev	= 1, if the analysis is at the industry level	0.586	0.493	0.141	0.348
period_2007	= 1, if the midpoint of the sample period is smaller or equal to year 2007	0.626	0.484	0.374	0.484
time_span	= ln(final year − initial year + 1), a value of zero indicates a cross-sectional estimation for only one year	2.631	0.627	2.292	0.870
non_ifr	= 1, if robot data are drawn from another source than IFR	0.154	0.361	0.412	0.492
lag_rob	= 1, if the robot variable is lagged by at least one period	0.090	0.286	0.131	0.337
quant_reg	= 1, if a quantile regression is emlpoyed	0.357	0.479	0.030	0.171
non_msms	= 1, if the estimation fails to meet the criteria for a MSMS score of 3 or 4 according to WWCLEG (2016)	0.574	0.495	0.420	0.494
log_log	= 1, if both dependent and independent variable are logarithmically transformed	0.528	0.499	0.301	0.459
non_clse	= 1, if inference is based on non-clustered standard errors	0.367	0.482	0.524	0.500
iq_spec	= 1, if the estimation includes a quadratic or interaction term in addition to the linear robot variable	0.130	0.337	0.096	0.295
Omitted control variables					
hc_omit	= 1, if the estimation does not control for human capital (e.g. level of education)	0.939	0.240	0.821	0.384
prod_omit	= 1, if the estimation does not control for initial/ lagged productivity or output	0.813	0.390	0.675	0.469
labor_omit	= 1, if the estimation does not control for labor input	0.908	0.290	0.818	0.386
openness_omit	= 1, if the estimation does not control for economic openness (e.g. trade, offshoring, FDI)	0.739	0.439	0.559	0.497
ict_omit	= 1, if the estimation does not control for ICT capital	0.514	0.500	0.777	0.416
capital_omit	= 1, if the estimation does not control for overall capital (e.g. total capital stock, non-ICT capital, capital intensity)	0.393	0.489	0.670	0.470
rd_omit	= 1, if the estimation does not control for research, development, or innovation (e.g. R&D expenditures)	0.882	0.323	0.775	0.418
lc_omit	= 1, if the estimation does not control for labor costs	0.792	0.406	0.830	0.376
demograph_omit	= 1, if the estimation does not control for demographic developments (e.g. working age population share, avg. age of population, population growth)	0.910	0.286	0.918	0.275
structure_omit	= 1, if the estimation does not control for the economic structure (e.g. shares of certain economic sectors like manufacturing)	0.946	0.226	0.837	0.369
Subpopulations					
dev_country	= 1, if the sample comprises only developing/emerging countries, i.e., no high-income economies following the classification in World Bank (2024).	0.222	0.416	0.490	0.500
sme	= 1, if the sample comprises only small and medium-sized enterprises (¡250 employees)	0.007	0.084	0.034	0.182
p_quartile_1	= 1, if the sample comprises only the first quartile of the productivity distribution	0.092	0.289	0.011	0.102
p_quartile_2	= 1, if the sample comprises only the second quartile of the productivity distribution	0.136	0.343	0.013	0.112

Table 1 continued from previous page

Variable	Description	Mean	SD	W-Mean	W-SD
p_quartile_3	= 1, if the sample comprises only the third quartile of the productivity distribution	0.097	0.297	0.010	0.102
p_quartile_4	= 1, if the sample comprises only the fourth quartile of the productivity distribution	0.090	0.286	0.009	0.093
r_above	= 1, if the sample comprises only robot intensities above the median	0.075	0.264	0.027	0.163
secondary_sec	= 1, if the sample comprises only the secondary sector	0.380	0.486	0.626	0.484
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)	0.021	0.142	0.020	0.141
Productivity measure					
lp	= 1, if labor productivity is the dependent variable (omitted reference category)	0.653	0.476	0.429	0.495
tfp	= 1, if TFP is the dependent variable	0.274	0.446	0.438	0.496
green_tfp	= 1, if GTFP is the dependent variable	0.073	0.260	0.133	0.340
growth	= 1, if the estimation uses growth rates of productivity or productivity changes	0.408	0.492	0.306	0.461
Publication status/ quality					
sjr	= ln(1 + SCImago Journal Ranking index for the respective publication year)	0.748	0.464	0.713	0.572

Note: This table presents the description of all moderator variables. "Mean" gives the unweighted sample average of each moderator variable. "SD" is the unweghted standard deviation. "W-Mean" and "W-SD" are the weighted version of "Mean" and "SD" using the inverse number of estimates per study as weights.

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). 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 and convert each estimated coefficient to a PCC as common effect size (Stanley et al., 2018; Duan et al., 2020; Cazachevici et al., 2020; Pinheiro et al., 2023; Picchio and Ubaldi, 2024). $PCCs$ 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).[8] Table 2 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.

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.[9] To make interaction terms fit into my meta-analysis, I follow Cazachevici et

[8]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. Specifically, the number of entity fixed effects was approximated by dividing the number of observations by the number of sample years.

[9]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 2: Descriptive statistics for effect size variables

Measure	Mean	Median	SD	Min	Max
t-value	3.25	2.61	5.03	-12.42	48.96
df	112200.95	3033	495309	5	6227990
PCC	0.07	0.04	0.20	-0.91	0.98
SE_{PCC}	0.04	0.02	0.05	0.00	0.33

Notes: N= 1849. Mean, median, and standard deviation are weighted using the inverse number of estimates reported per study as weights.

al. (2020) by computing the marginal effect at the mean and using the delta method to approximate the associated standard error[10]:

$$\beta = \hat{\beta}_1 + \hat{\beta}_2 \bar{x}; \qquad SE(\beta) = \sqrt{SE(\hat{\beta}_1)^2 + SE(\hat{\beta}_2)^2 \bar{x}^2} \tag{2}$$

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[11] 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.

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).

[10] 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 $SE(\beta)$ by assuming the covariances to be zero.

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

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 by computing weighted averages. Meta-analytic theory and simulations show that the optimal meta-analytic weighting scheme is based on precision, i.e., the inverse of the estimate's standard error (Stanley and Doucouliagos, 2012, p. 46 ff.Irsova et al., 2023). 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 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).[12] Stanley and Doucouliagos (2015) and Stanley and Doucouliagos (2017) argue that the most appropriate meta-analytic estimator is an unrestricted weighted least squares (UWLS) estimator using precision weights in the following basic regression:

$$PCC_{is} = \mu + \epsilon_{is} \tag{6}$$

where, in the absence of any bias, μ represents the overall effect, and the error term $\epsilon_{is} \sim N(0, \sigma^2_{\epsilon_{is}})$ describes the primary sampling error.[13]

As typically done in economic meta-studies, I include all estimates provided by a primary

[12]This FE estimator should not be mistaken for a panel-fixed effect estimator in econometric terms. The intuition is that a more precise estimate (as measured by a smaller standard error) allows a smaller estimated effect to retain a statistically significant coefficient. Therefore, more weight is attributed to more precise $PCCs$.

[13]Measuring Equation (6) by OLS is not appropriate since the error term 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 (6) must be estimated by weighted least squares to receive unbiased, consistent, and efficient estimates.

study if they fit the selection criteria.[14] 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 weights additionally adjusted for the number of estimates per study:

$$FE2 = \frac{1}{SE_{PCC_{is}} \cdot \sqrt{n_{i \in s}}} \tag{7}$$

where n is the number of estimates of study s. In doing so, I assign equal weight to studies rather than estimates for a given precision (Duan et al., 2020). Multiple estimates per study further raise the concern of study 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 multivariate MRA my moderator matrix will capture varying estimation characteristics and subpopulations examined within a given primary study. In view of the highly right-skewed distribution of the number of estimates per study, it appears 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 weights are preferred over FE1 weights.

5 Overall mean effect and publication bias

A simple vote counting based on the 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

[14]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. 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.

formally test for publication bias.

5.1 Summary measures

Table 3 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.[15] The summary measures are statistically significant. In terms of economic significance, only the unweighted mean indicates a small positive effect slightly above the threshold of 0.07 (Doucouliagos, 2011). With an overall effect size of 0.01, both FE1 and FE2 suggest a very small effect. This is in line with the highest frequencies of *PCCs* being centered around small positive values as shown in Figure 2. The substantial decline in the mean effect caused by assigning larger weights to more precise studies, may point to publication bias. 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. In the following subsections, I will investigate the evidence of publication bias.

Table 3: Summary measures of PCCs

Measure	Obs.	Estimate	SE	95% CI
Mean	1837	0.0926	0.0147	[0.0634, 0.1217]
UWLS FE1	1837	0.0104	0.0031	[0.0043, 0.0165]
UWLS FE2	1837	0.0102	0.0019	[0.0063, 0.0140]

Note: Standard errors are clustered at the study level.

5.2 Publication bias

Publication selection describes the 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 of publication bias (Doucouliagos and Stanley, 2013; Ioannidis et al., 2017), including meta-studies related to the determinants of economic growth (e.g., Gechert and Heimberger, 2022; Gunby et al., 2017; Havranek and Irsova, 2011) and on the impact of ICT on productivity (Polák, 2017; Stanley et al., 2018). Thus, it is essential to formally test and correct for

[15] 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 *non_clse* captures whether primary estimates are based on clustered standard errors.

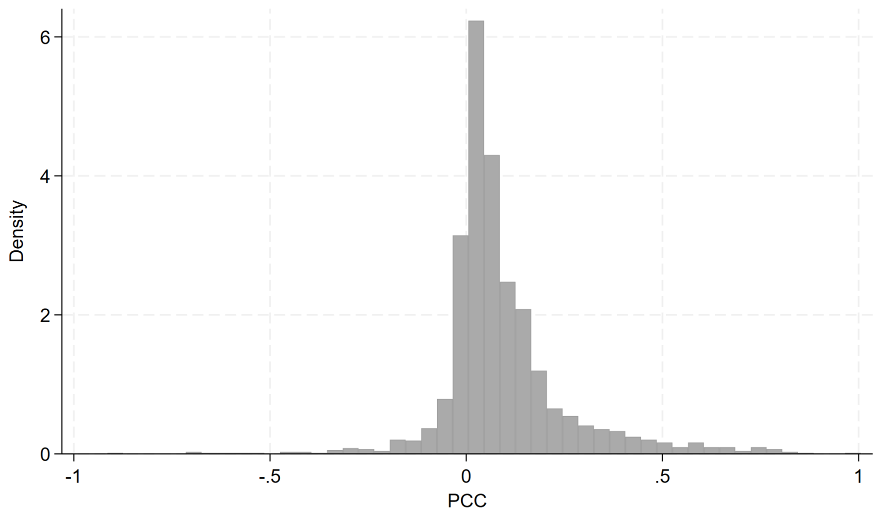


Figure 2: Histogram of $PCCs$

Notes: N = 1837, from 85 studies.

publication 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. Different forms of publication bias can be distinguished: Authors might decide not to publish insignificant results or findings that contradict theoretical conventions (Andrews and Kasy, 2019); or authors "manipulate" or "p-hack" their findings by searching for and selectively reporting specifications that deliver the desired outcome (Brodeur et al., 2016; Simonsohn et al., 2014). Accordingly, there are also different approaches to tackle publication bias in meta-studies that can be grouped into two categories: selection models and funnel-based techniques (Irsova et al., 2023). Complying with the guidelines in Irsova et al. (2023), the following subsections implement several models from both categories.

5.2.1 Distribution of t-statistics and selection models

To illustrate the distribution of t-statistics reported in the primary literature, I plot a histogram and Kernel density estimate in Figure 3. The histogram is suggestive of jumps at zero and above the critical value for the 5% significance level (t-value = 1.96). The density also peaks closely above this value. This pattern may point to selective reporting or p-hacking, i.e., selecting or "manipulating" estimates for their sign and statistical sig-

nificance (Andrews and Kasy, 2019; Brodeur et al., 2016; Simonsohn et al., 2014).
To investigate a potential bunching of primary estimates above the 5% significance level, I follow Cattaneo et al. (2018, 2020) and Picchio and Ubaldi (2024) and test for a discontinuity of t-statistics at the value of 1.96, using a nonparametric local polynomial density estimator. A potential pitfall in examining such a discontinuity is that collecting coefficients and standard errors from reported regression tables to compute t-statistics is subject to rounding errors because the number of decimal points shown is limited. This could generate a bunching of t-statistics at a value of 2 merely as an artifact (Brodeur et al., 2016). The range of variation of the original t-statistic might even go from insignificant values to highly significant ones (Kranz and Pütz, 2022). To overcome this issue, Brodeur et al. (2016) propose a de-rounding approach that draws missing digits from a uniform distribution and Kranz and Pütz (2022) suggest a method that drops observations which are too coarsely rounded.[16] Figure 4 illustrates the evidence of a significant discontinuity in the reported t-statistics at a value of 1.96.
A related approach is formed by selection models. They aim to capture researchers' decisions whether or not to submit empirical results, and reviewer decisions whether or not to publish submissions. In doing so, primary estimates with different significance levels (or sign) are assumed to have different probabilities of publication (Andrews and Kasy, 2019; Irsova et al., 2023). It is important to note that the presence of multiple estimates per study complicates the implementation of selection models. The model of Andrews and Kasy (2019) (henceforth AK), however, allows to cluster standard errors at the study level. AK model the publication process through a truncated sampling process. They assume a population of latent (published or unpublished) studies within an empirical literature. The result of each study is drawn from a normal distribution, conditional on the true effect (denoted by Θ) and standard error (denoted by Σ), which are both allowed to vary across studies. Only the truncated sample of published studies, comprising journal articles and uploaded working papers, is observable. The probability of publication is modeled as a function of the t-statistics (denoted by the random variable T with realizations t), i.e.,

[16]The rounding issue and the discontinuity test is further complicated by the inclusion of marginal effects in my meta-dataset according to Equations 2 and 3. In specifications with interaction terms, it might be less clear which term (the linear robot variable or the interaction term?) receives the most focus from the authors. Moreover, computing marginal effects automatically results in estimates and standard errors with a higher number of decimal points, compared to the other primary estimates. These marginal effects are, however, still computed from rounded numbers reported in the respective study. They might even introduce an additional measurement error as they are based on two coefficients and two standard errors. Therefore, I follow Picchio and Ubaldi (2024) and exclude specifications with interaction or quadratic terms from my meta-dataset before performing the discontinuity test.

the ratio of the study's estimate and standard error:

$$\Theta^* \quad \sim \quad \bar{\theta} + t(\tilde{\nu}) \cdot \tilde{\tau}, \quad p(T) \quad \propto \quad \begin{cases} \beta_{p,1} & if \quad T < -1.96 \\ \beta_{p,2} & if \quad -1.96 \leq T < 0 \\ \beta_{p,3} & if \quad 0 \leq T < 1.96 \\ 1 & if \quad T \geq 1.96 \end{cases} \tag{8}$$

Θ^* denotes the true effect of latent studies and is modeled using a t-distribution with degrees of freedom $\tilde{\nu}$ and location and scale parameters $\bar{\theta}$ and $\tilde{\tau}$, respectively (Andrews and Kasy, 2019). The probability of publication is allowed to depend on the sign of the t-statistic T. This is important, as it seems plausible that the publication prospects of a study might differ depending on whether it found a positive or negative effect of robots on productivity; and this pattern is also supported by the histogram in Figure 3. Specifically, the probability of publication is modeled as a step function, with jumps at conventional critical values, as well as at zero (see Equation 8). Since the probability is only identified up to scale, the normalization $p(t) = 1$ for $t \geq 1.96$ is imposed.[17] Thus, $\beta_{p,i}$ measure the publication probability relative to a result that is statistically significant at the 5% level. Selective publication implies that the distribution of T is reweighted by $p(t)$. Intuitively, to correct for this form of selectivity, the observed estimates must be reweighted by the inverse publication probability (Irsova et al., 2023).

The baseline AK selection model specifies parametric models for both the distribution of true effects as well as the publication probability, and is fit by maximum likelihood. Moreover, AK develop a GMM estimator that only assumes a functional form for the publication probability but is nonparametric in the distribution of true effects.[18] Applying both approaches to my meta-dataset strongly rejects the hypothesis of no selectivity (i.e., $H_0 : \beta_{p,i} = 1$). The results are shown in Tables 4 and 5. The baseline AK model suggests that negative estimates that are statistically significant at the 5% level are almost 20 times less likely to be published than significantly positive estimates. Likewise, insignificant estimates are less likely to be published: insignificant estimates with negative sign are 15 times less likely to be published and insignificant estimates with positive sign are almost six times less likely to be published, relative to significantly positive estimates. $\bar{\theta}$ suggests that the average latent study finds a small positive effect but is not significantly different from zero. These findings are largely corroborated when the GMM version is

[17] This is without loss of generality, since p(t) is allowed to be larger than 1 for the other value areas (Andrews and Kasy, 2019).

[18] I implement the baseline AK model using the provided replication code in MATLAB and the GMM version using the replication package provided for R.

used, although the average true effect across latent studies becomes ten times larger and the publication probability of significantly negative estimates is three times larger when the distribution of true effects is not parameterized.

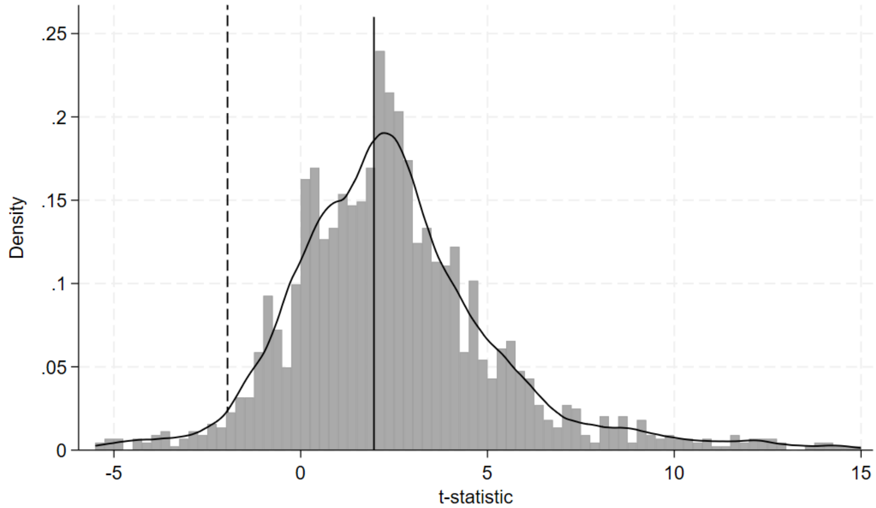


Figure 3: Distribution of t-values

Notes: N = 1837, from 85 studies. The dashed line indicates a t-value of -1.96, the solid line a t-value of 1.96. For the sake of visibility the range of t-statistics shown is limited to the interval (-6, 15).

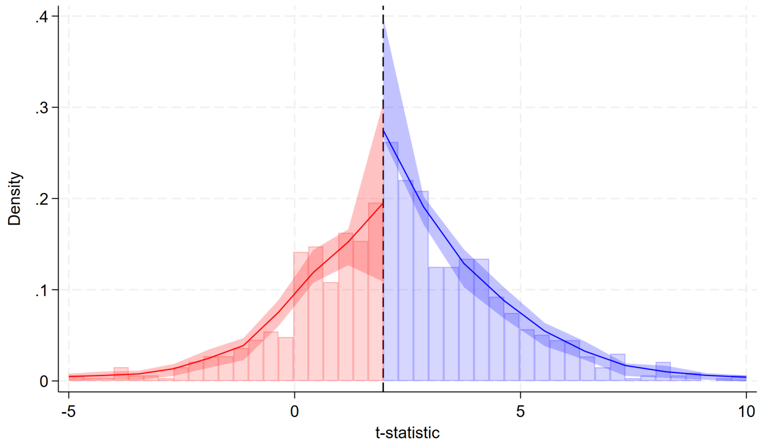
Table 4: AK model results

$\bar{\theta}$	$\tilde{\tau}$	$\tilde{\nu}$	$\beta_{p,1}$	$\beta_{p,2}$	$\beta_{p,3}$
0.001	0.014	0.922	0.051	0.065	0.169
(0.003)	(0.006)	(0.112)	(0.010)	(0.019)	(0.037)

Notes: N = 1837. Standard errors clustered at the study level are in parentheses. $\bar{\theta}$ and $\tilde{\tau}$ are location and scale parameters of a t-distribution with degrees of freedom $\tilde{\nu}$. $\beta_{p,i}$ measures the publication probability relative to a result that is statistically significant at the 5% level.

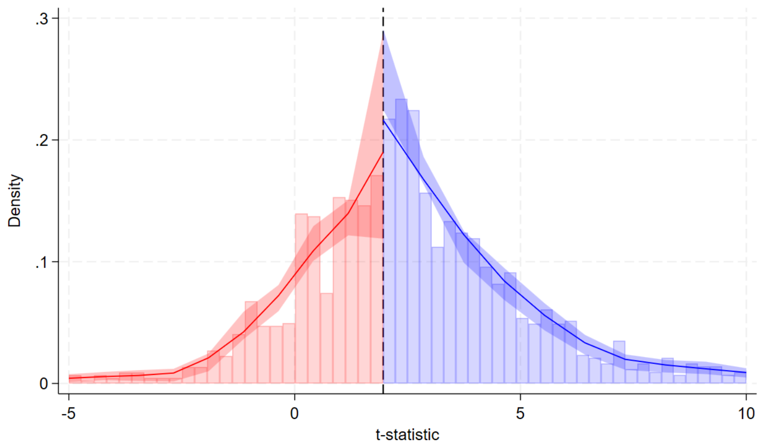
5.2.2 Funnel-based techniques

The presence of publication bias can be visually inspected by a so-called funnel plot in Figure 5: a scatterplot of the effect sizes against their precision. Without publication bias (and between-study heterogeneity), the primary estimates should be distributed symmetrically around the overall effect size (FE2 in Figure 5) 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. The funnel plot shown in



(a) Rounding issue tackled according to Kranz and Pütz (2022)

Notes: N = 1003, from 68 studies. The two-sided test for the significance of the discontinuity at 1.96 returns a p-value of 0.0404.



(b) De-rounding approach according to Brodeur et al. (2016)

Notes: N = 1596, from 81 studies. The two-sided test for the significance of the discontinuity at 1.96 returns a p-value of 0.0039. The result is very similar when the t-statistics remain unadjusted for rounding (p-value = 0.0028; results are available upon request).

Figure 4: Discontinuity test of the density of the t-statistic at 1.96

Notes: The solid lines represent the estimated local polynomial density of the t-statistics (Cattaneo et al., 2018, 2020). The order of the local polynomial is 2 (quadratic). The kernel function used to construct the local polynomial estimator is epanechnikov. The areas around the lines indicate the bias-corrected confidence intervals at 95%. Standard errors are based on the jackknife estimator. In line with Picchio and Ubaldi (2024), I exclude primary estimations with quadratic and interaction terms from my meta-dataset to perform the discontinuity test.

Table 5: GMM AK model results

Θ	Σ	$\beta_{p,1}$	$\beta_{p,2}$	$\beta_{p,3}$
0.011	0.075	0.158	0.054	0.104
(0.018)	(0.019)	(0.209)	(0.050)	(0.075)

Notes: N = 1837. Standard errors clustered at the study level are in parentheses. Θ and Σ correspond to the estimation of the mean and standard error of the true effect across latent studies. $\beta_{p,i}$ measures the publication probability relative to a result that is statistically significant at the 5% level.

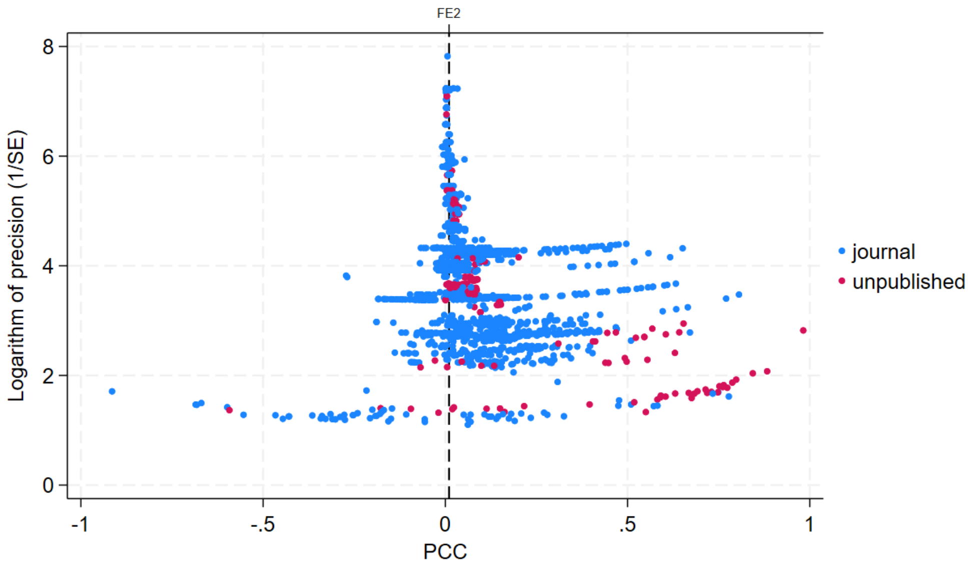


Figure 5: Funnel plot of $PCCs$

Notes: N = 1837, from 85 studies. The dashed line labeled FE2 indicates the value of the FE2 estimator from Table 3.

Figure 5 is quite asymmetric with more imprecisely estimated $PCCs$ to the right of the overall effect. The most precise estimates show a very small positive effect, in line with the estimators FE1 and FE2 in Table 3. This may point to a positive publication bias.

In case of publication 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; Stanley and Doucouliagos, 2012, pp. 60 ff.):

$$PCC_{is} = \beta_0 + \beta_1 SE_{PCC_{is}} + \epsilon_{is} \tag{9}$$

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 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 (9) exhibits heteroskedasticity by construction because the independent variable is the standard deviation of the dependent variable. Therefore, it must be estimated by UWLS using the weights given in Equations (5) and (7).

Table 6 presents the FAT-PET results. I find strong evidence of publication selection with a "severe" magnitude of selectivity ($\beta_1 > 2$) according to the practical guidance of Doucouliagos and Stanley (2013). A way to control for between-study heterogeneity is running the precision-weighted FAT-PET model with study fixed effects. Thereby, only within-study variation is considered, and again a significant and severe magnitude of selectivity is found. The evidence of publication bias is also 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 in a 2SLS estimation 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).

The precision effect corrected for publication bias shows statistical significance for the FE2 estimator. However, the effect beyond bias lacks economic significance (PET), as it is well below the threshold of 0.07. This suggests that industrial robotization has, so far, had only a marginal effect on productivity.

The FAT-PET approach assumes that publication bias is a linear function of the standard error, which was criticized by Stanley and Doucouliagos (2014). To relax this assumption, I also implement alternative techniques. My findings are confirmed by non-linear techniques correcting for publication bias in Table 7.[19] Using the squared standard error of PCC in Equation (9) instead of a linear term is called "precision effect estimate with standard error" (PEESE) (Stanley and Doucouliagos, 2014). 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

[19] 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 6.

Table 6: FAT-PET

	(1) UWLS FE1	(2) UWLS FE2	(3) Study-FE	(4) MAIVE
Publication bias (β_1)	2.9595***	2.1417***	3.104***	2.2371***
	(.6803)	(.3929)	(.3738)	(.5369)
Precision effect (β_0)	.0036	.0072***	.002	.0049
	(.0024)	(.0026)	(.0043)	(.0192)

Notes: N = 1837, from 85 studies. Standard errors clustered at the study level are in parentheses. The specification with study fixed effects and MAIVE are based on precision weighting (i.e., FE1 weights). $^{***}p < .01$,$^{**}p < .05$, $^{*}p < .1$

Table 7: Non-linear techniques correcting for publication bias

	(1) PEESE	(2) MAIVE-PEESE	(3) WAAP	(4) Top 10%
SE^2_{PCC}	15.0104*	8.7937		
	(8.6603)	(5.408)		
Precision effect (β_0)	.01***	.0442**	.0063***	.0063***
	(.0029)	(.0174)	(.0017)	(.0021)
Observations	1837	1837	164	184
No. of studies	85	85	15	19

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

social science research has been conventionally set at 80% or larger. It corresponds to a probability of a Type II error that is not larger than four times the 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}):

$$SE_{PCC_{is}} \leq |\beta_0|/2.8 \tag{10}$$

"Top 10%" is the unweighted average among the estimates in the top decile of precision. Both WAAP and "Top 10%" point to a statistically significant but very small effect of robotization on productivity.

5.2.3 Subset analysis

A disadvantage of all approaches to test and correct for publication presented above is that they do not explain the heterogeneity between primary estimates. A first step toward analyzing heterogeneity is to apply the FAT-PET approach to subsets of my meta-dataset (Irsova et al., 2023). This allows to summarize primary research for different empirical contexts with respect to different measures of productivity, the level of analysis, the empirical methodology, and the quality of articles. Brodeur et al. (2020), for example,

Table 8: FAT-PET for subsets

	(1) MSMS	(2) IV	(3) LP	(4) TFP	(5) GTFP	(6) Top SJR	(7) Firm level	(8) Aggr. level	(9) Elasticity
Publication bias	2.8499***	2.6173***	2.0651***	2.378***	3.2825**	2.7809***	1.8629***	2.2111***	3.2065***
	(.4193)	(.5425)	(.6267)	(.4913)	(1.2979)	(.391)	(.4811)	(.687)	(1.1459)
Precision effect	.0038	.0068**	.0028**	.0083***	-.0167	.0021	.0075***	.0115	.0031***
	(.0023)	(.0027)	(.0012)	(.0027)	(.0328)	(.0023)	(.0027)	(.0189)	(.0005)
Observations	673	256	1196	506	135	350	423	1414	977
No. of studies	60	35	42	44	12	18	43	42	31

Notes: Standard errors clustered at the study level are in parentheses. MSMS represents the subset of estimates that achieve a score of 3 or 4 on the MSMS. IV refers to the subset of IV estimates (MSMS score of 4). LP represents only estimates for labor productivity, TFP for total factor productivity, and GTFP for green TFP. Top SJR uses the subset of estimates from journals in the top quartile of the SJR index in my meta-dataset. Firm level refers to firm-level estimates and "Aggr. level" to estimates at the level of industries or geographical units. Elasticity represents the subset of double-log estimations. $^{***}p < .01$,$^{**}p < .05$, $^{*}p < .1$

showed that tests based on DID or IV approaches are more likely to suffer from publication bias. Moreover, one could argue that IV estimates are generally less precise than OLS estimates, such that IV estimates are penalized by precision weighting in the full meta-dataset. Similarly, firm-level estimates are typically based on higher sample sizes than more aggregated estimates and, thus, obtain more weight by precision weighting.

The evidence of publication bias and a very small effect beyond publication bias is robust across several subsets of primary estimates (Table 8). It holds among estimates that treat endogeneity, i.e., estimates that achieve a score of 3 or 4 on the MSMS, as well as only among IV estimates (MSMS score of 4).[20] The pattern of publication bias is also evident for all productivity measures employed in the primary literature. 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 $\geq$ 2.063) as well as for estimates at the firm level or more aggregated levels.

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 FAT-PET model (Stanley and Doucouliagos, 2012, pp. 26). Using elasticities as an alternative effect size measure still provides evidence of a positive publication bias and only a marginally positive effect of robots on productivity beyond publication bias.

[20] 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).

6 Drivers of heterogeneity in the primary literature

6.1 Multivariate MRA-model

Equation 9 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. (9) and estimate the following multivariate MRA model to identify the drivers of heterogeneity in the primary literature:

$$PCC_{is} = \beta_0 + \beta_1 SE_{PCC_{is}} + \sum \beta_k Z_{kis} + \epsilon_{is} \tag{11}$$

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 34 variables to account for as many research characteristics as possible (see Table 1). In MRA, the main drivers of heterogeneity are usually identified by means of a variable selection or model averaging procedure to treat potential multicollinearity and model uncertainty (Stanley and Doucouliagos, 2012, p. 91). The next subsection will present the drivers of heterogeneity identified by these procedures, focusing on the most robust findings.

6.2 Results

6.2.1 General-to-specific procedure

First, I follow the approaches of Stanley et al. (2018) and Guarascio et al. (2025) 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 0.1.[21] Table 9 shows the results of my multivariate MRA. Employing the GTS approach for the WLS regression with FE2 weights results in a selection of 13 moderator variables that are drivers of heterogeneity.[22]

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 re-

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

[22] The variance inflation factors of all moderator variables in the full model used as starting point in the GTS procedure are below 10 and mostly below 5.

cent 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 (*secondary_sec)* 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, estimations that exclude observational units with the highest exposure to robots are somewhat higher (*excl_high_exposure*). 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 of adjustment costs at lower productivity levels. The coefficients of the moderator variables *p_quartile_1* and *p_quartile_2* point to smaller productivity effects from robot adoption for users with an initial productivity below the median, while this effect is more pronounced in the bottom quartile of productivity. This suggests that automating less efficient processes is no panacea to boost productivity.

Among the different levels of analysis, the GTS procedure points to lower effects at the industry level relative to firm-level estimations. This could be attributed to an intra-industry reallocation of market shares from non-adopting firms to firms with robots, netting out the productivity effects at the industry level (Koch et al., 2021). It could also be explained by a concentration of productivity effects among few firms such that the aggregate effect at the industry level is more blurry. Stiebale et al. (2024) refer to this phenomenon as the rise of "superstar" firms.

Regarding econometric specifications, the GTS procedure shows that primary estimates based on a quantile regression (*quant_reg*) are associated with more positive findings on average. In contrast, estimations with interaction or quadratic terms for the robot variable result in smaller effects. These specifications require the calculation of marginal effects according to Equations 2 and 3. To rule out a distorting influence of specifications with interaction/quadratic terms, I exclude them from my estimation in column (4) of Table 9. This leaves my findings qualitatively 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 (Fan et al., 2021; Jung and Lim, 2020). If labor costs and productivity are also positively correlated, this results in a positive omitted variable bias. Likewise, estimations that do not control for the economic structure tend to result in more positive effects. A positive omitted variable bias from *structure_omit* can be explained through a positive correlation between the manufacturing share and productivity as well as robot adoption, since industrial robots are primarily used in the manufacturing sector (Fernández-Macías et al., 2021). Moreover, estimations that fail to control for general capital input are associated with smaller effects. Assuming that capital input is positively correlated with productivity, this would suggest a substitutive relationship between robot usage and other forms of capital input.

Furthermore, I find some evidence of data dependence: estimates that are not based on IFR data 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 (Alguacil et al., 2022; Cathles et al., 2020; Jäger et al., 2016, 2015; Koch et al., 2021).

As a robustness check, I drop all primary estimates with GTFP as dependent variable in column (3) of Table 9. GTFP may be viewed as conceptually different compared to labor productivity and TFP as traditional productivity measures. Nevertheless, this leaves the sign and significance of the moderator variables unchanged.

Apart from the drivers of heterogeneity, it is important to mention, that the evidence of a "severe" 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. Using the estimates from the selected and preferred specification in column (2) of Table 9, results in an estimated mean effect of 0.0164 that is statistically significant. Other moderator variables that may be deemed important for quantifying a "best practice estimate", i.e., a treatment of endogeneity (*msms*) or using clustered standard errors (*secl*), are neither statistically significant in the full model of column (1) nor selected by the GTS procedure in column (2) of Table 9. Thus, they are assumed to have no relevant influence on the estimated mean effect. Primary estimations that address endogeneity do not seem to provide 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 6 shows the estimated mean effects and the associated 95% confidence intervals for several subgroups of primary estimates. These effects are computed from the

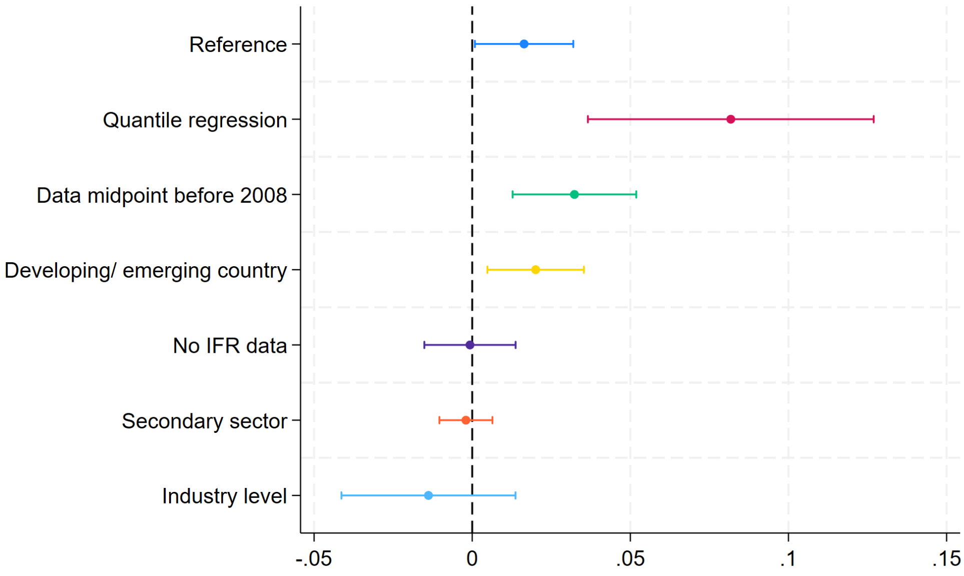


Figure 6: Heterogeneous meta effects

Notes: Effects are computed from the estimates in column (2) of Table 9. Dots indicate point estimates and capped bars represent 95% confidence intervals.

following formula:

$$\text{Meta effect} = \beta_0 + \beta_{SE_{PCC}} \cdot 0 + \beta_k \tag{12}$$

where β_k is the coefficient of the relevant moderator variable in column (2) of Table 9 for the respective subgroup. Figure 6 shows that the meta-effects for developing/emerging countries and for primary estimates with a data midpoint before year 2008 show a statistically significant effect with a positive sign. The magnitude is, however, well below the threshold of economic significance. Only the meta-effect for quantile regressions passes the threshold of a small effect, with a point estimate of 0.082. The meta-effects for the secondary sector, industry-level estimates, and for estimates based on non-IFR data are statistically insignificant.[23]

6.2.2 Bayesian Model Averaging

The sequential t-testing involved in the stepwise GTS procedure implemented in subsection 6.2.1 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).

[23] 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.

Table 9: Multivariate MRA

	(1) Full model	(2) GTS	(3) No GTFP	(4) No marginal effects
SE_{PCC}	2.5297***	2.3789***	2.4619***	2.53***
	(.4254)	(.4368)	(.4891)	(.467)
ind_lev	-.0411**	-.0302***	-.0268**	-.033***
	(.0182)	(.0111)	(.0127)	(.0114)
period_2007	.0166***	.0159***	.0158***	.0161***
	(.0035)	(.004)	(.0041)	(.0039)
non_ifr	-.0177***	-.0171***	-.017***	-.0172***
	(.0051)	(.003)	(.003)	(.003)
quant_reg	.0875**	.0654***	.042**	.0858***
	(.0336)	(.022)	(.0193)	(.0271)
iq_spec	-.0205**	-.0157**	-.0133**	
	(.0094)	(.0068)	(.0065)	
dev_country	.005	.0037***	.004***	.0037***
	(.0037)	(.0013)	(.0013)	(.0013)
p_quartile_1	-.0235*	-.0147**	-.0107***	-.0143**
	(.0131)	(.0066)	(.0033)	(.006)
p_quartile_2	-.0128	-.0069**	-.006*	-.0069**
	(.0082)	(.0034)	(.0033)	(.0034)
secondary_sec	-.031	-.0184***	-.0198***	-.018***
	(.0264)	(.0065)	(.0071)	(.0065)
excl_high_exposure	.0124	.0122**	.0103**	.0038
	(.0093)	(.0052)	(.0051)	(.0047)
capital_omit	-.0087*	-.0067**	-.0065*	-.0067**
	(.0052)	(.0033)	(.0033)	(.0034)
lc_omit	.0097	.0075**	.0077**	.0076**
	(.0064)	(.0031)	(.003)	(.0031)
structure_omit	.0047	.0042**	.0044**	.0043**
	(.0041)	(.0017)	(.0017)	(.0018)
_cons	.0447	.0164**	.0169**	.0155*
	(.0335)	(.0078)	(.0082)	(.0079)
Observations	1849	1849	1714	1608
No. of studies	85	85	74	81
R-squared	.4523	.4134	.4237	.4408

Notes: The UWLS FE2 estimator is employed. Standard errors clustered at the study level are in parentheses. GTS refers to the moderator variables selected in the general-to-specific procedure. Column (3) excludes primary estimates with GTFP as dependent variable. Column (4) excludes primary estimations where marginal effects had to be computed, i.e., specifications with interaction/quadratic terms. Only those moderator variables are shown that were selected in the GTS procedure. Results of the full model in column (1) with all variables are available upon request. $^{***}p < .01$, $^{**}p < .05$, $^{*}p < .1$

To address model uncertainty and multicollinearity among the 34 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) as alternative approach (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). BMA does not select a specific 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 34 moderator variables in addition to SE_{PCC}, the model space is represented by 2^{35} 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).[24] 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 the dilution model prior proposed by George (2010) and recommended by Irsova et al. (2023). The dilution model prior penalizes models with highly collinear regressors (Zeugner and Feldkircher, 2015). The unit information prior (UIP) on Zellner's g is used 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. For a detailed review of model averaging and its applications in economics, the reader is referred to Steel (2020).

A drawback of BMA is that it does not allow the clustering of standard errors. BMA, thus, entails a less conservative statistical inference, since it does not address the potential dependence of primary estimates from the same study. Figure 7 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 on the moderator variables with a PIP of above 0.5, following the classification described

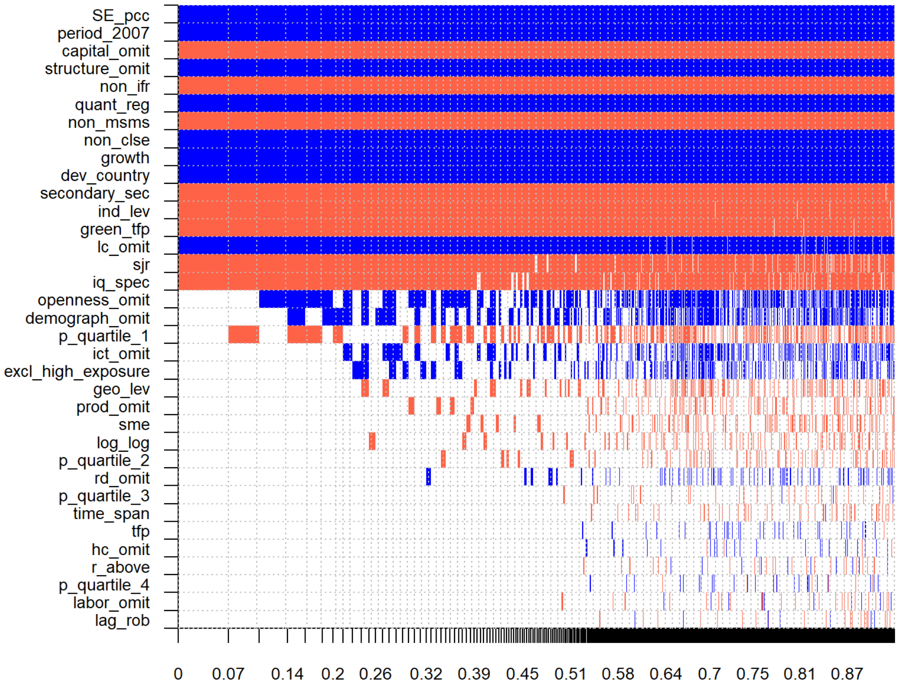
[24] I use 2 million iterations with 1 million burn-ins to achieve convergence.

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. 16 moderator variables are found to pass the threshold of a PIP of at least 0.5. Ten of these variables align with the drivers of heterogeneity identified by the GTS approach in subsection 6.2.1. The variables *p_quartile_1*, *p_quartile_2*, and *excl_high_exposure*, however, are not found to be important variables in the BMA approach. Instead, the BMA indicates that *non_msms*, *non_clse*, *growth*, *green_tfp*, *sjr*, and *openness_omit* are relevant variables, which were not included in the GTS selection in Section 6.2.

As a frequentist check, I run a UWLS-FE2 regression with clustered standard errors that includes all moderator variables with a PIP above 0.5 in BMA. This reveals that, except for *green_tfp* and *growth*, only those moderator variables attain statistical significance, which have already been identified as drivers of heterogeneity by the GTS approach in subsection 6.2.1. 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 estimated 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). The coefficient of *growth* indicates that specifications in terms of growth rates or changes in productivity can result in slightly higher estimates. Although the BMA approach suggests that treating endogeneity improves the identification of productivity effects from robot usage, this variables does not retain statistical significance in the frequentist check. Likewise, the BMA results indicate a potential overestimation of productivity effects when primary inference is not based on clustered standard errors. However, this variable loses its significance in the frequentist check as well.

Table 10 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

Figure 7: Inclusion of moderator variables in BMA



Notes: This figure presents the BMA results using UIP and Dilution as priors (George, 2010). 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.

of 1 and always exhibits a positive coefficient; *secondary_sec* and has a PIP above 0.99 and always shows a negative coefficient. dev_country is also assigned a PIP above 0.99 and always has a positive sign. Likewise, the importance of capital input, economic structure, and labor costs as control variables is confirmed. The relevance and sign of the variables *non_ifr*, *ind_lev*, *quant_reg*, *iq_spec,* are also reinforced. As before, the magnitude of the moderator variables' estimated coefficients is quite small, with posterior means that are mostly close to zero. Last but not least, the BMA approach reinforces the evidence of a severe positive publication bias.

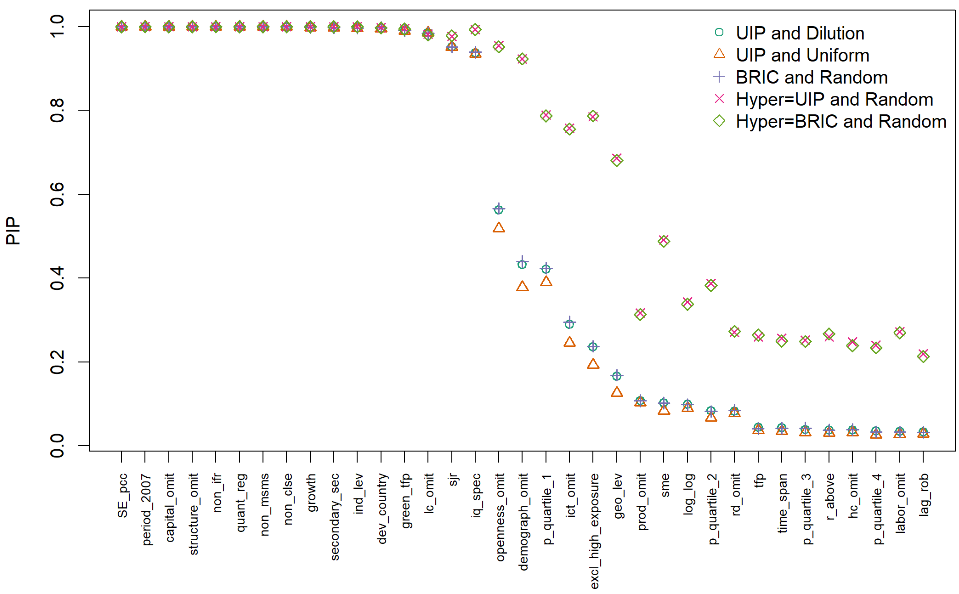
Another drawback of BMA is the required selection of prior distributions. To rule out a sensitivity of the BMA results to the choice of priors, Figure 8 shows the moderators' PIPs, based on alternative model and g-priors. The PIP of the variables with the highest PIPs in the baseline setting is very robust to alternative priors. The hyper g-prior generally results in larger PIPs for variables with lower PIPs in the baseline setting and assigns some additional variables a PIP above 0.5, including *p_quartile_1* and *excl_high_exposure* which were also selected in the GTS procedure.

Table 10: BMA results

	BMA				Frequentist check	
	PIP	Post Mean	Post SD	Cond.Pos.Sign	Coef	SE
SE_{PCC}	1	2.8883	0.1735	1	2.4973***	0.4832
period_2007	1	0.0167	0.0008	1	0.0167***	0.0035
capital_omit	1	-0.0065	0.0014	0	-0.0067**	0.0029
structure_omit	1	0.0086	0.0016	1	0.0067**	0.0033
non_ifr	1	-0.0143	0.0012	0	-0.0151***	0.0034
quant_reg	1	0.0881	0.0131	1	0.0643**	0.0265
non_msms	1	-0.0042	0.0009	0	-0.0045	0.0037
non_clse	0.9999	0.0058	0.0012	1	0.0051	0.0034
growth	0.9996	0.0112	0.0021	1	0.0089*	0.005
dev_country	0.9993	0.0064	0.0010	1	0.0060***	0.0013
secondary_sec	0.9992	-0.0158	0.0042	0	-0.0207**	0.0082
ind_lev	0.9981	-0.0309	0.0073	0	-0.0326***	0.0117
green_tfp	0.9953	-0.0275	0.0066	0	-0.0285*	0.016
lc_omit	0.9882	0.0079	0.0023	1	0.0080*	0.0045
sjr	0.9619	-0.0035	0.0012	0	-0.0036	0.0022
iq_spec	0.9457	-0.0136	0.0052	0	-0.0141**	0.0065
openness_omit	0.5605	0.0017	0.0017	1	0.0022	0.0033
demograph_omit	0.4302	0.0026	0.0034	1		
p_quartile_1	0.4207	-0.0058	0.0077	0		
ict_omit	0.2812	0.0019	0.0034	1		
excl_high_exposure	0.2302	0.0033	0.0068	1		
geo_lev	0.1541	-0.0019	0.0052	0		
prod_omit	0.0984	-0.0003	0.0009	0.0035		
sme	0.0928	-0.0014	0.0052	0		
log_log	0.0911	-0.0001	0.0005	0		
p_quartile_2	0.0754	-0.0005	0.0022	0		
rd_omit	0.0750	0.0003	0.0013	0.9997		
p_quartile_3	0.0321	-0.0001	0.0011	0.0907		
time_span	0.0316	0.0000	0.0002	0.0437		
tfp	0.0305	0.0000	0.0003	0.9979		
hc_omit	0.0294	0.0000	0.0006	0.8544		
r_above	0.0290	-0.0001	0.0016	0.2777		
p_quartile_4	0.0263	0.0001	0.0010	0.8531		
labor_omit	0.0261	0.0000	0.0003	0.2801		
lag_rob	0.0247	0.0000	0.0003	0.2684		
(Intercept)	1	-0.0929	NA	NA	0.0121	0.0096
Observations	1849				1849	

Notes: 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 8: Sensitivity of PIPs to alternative priors



Notes: UIP and Dilution is the baseline setting, proposed by George (2010). The dilution model prior penalizes models with highly collinear regressors (Zeugner and Feldkircher, 2015). UIP and Uniform are the priors recommended by Eicher et al. (2011). The uniform model prior implies a prior expected model size of $k/2$. 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).

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.:

$$PCC_{adj_{is}} = \frac{t_{is}}{\sqrt{t_{is}^2 + df_{is} + 3}} \tag{13}$$

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). They adjust the formula for the standard error of PCC by using the meta-analytic sample mean of $PCCs$ instead of the specific PCC of estimate i in study s, thereby also affecting the weighting scheme:

$$SE_{PCC_{is}} = \sqrt{\frac{1 - \overline{PCC_{is}}^2}{df_{is}}} \tag{14}$$

Third, I follow van Aert (2023) and Xue et al. (2024) and apply a Fisher's z transformation to $PCCs$. A criticism of meta-analyzing $PCCs$ is that the assumptions of known sampling variances and normality are violated by definition (van Aert, 2023): The distribution of $PCCs$ is not normal when its value is close to -1 and $+1$ (Stanley and Doucouliagos, 2012, p. 25) and the sampling variance (and standard error) of the PCC is a function of the PCC itself (van Aert, 2023). These issues can be overcome by implementing the following Fisher's z transformation:

$$z_{is} = \frac{1}{2} \times \log\left(\frac{1 + PCC_{is}}{1 - PCC_{is}}\right) \tag{15}$$

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

$$se(z_{is}) = \frac{1}{\sqrt{(df_{is} - 3)}} \tag{16}$$

Fourth, I follow many recent meta-analyses and apply a winsorization of the effect size variables (PCC and SE_{PCC}) at the 1st and 99th percentiles of their distributions; i.e., values below the 1st percentile and above the 99th percentile are replaced with the values of the 1st and 99th percentiles, respectively. This robustness check controls for an undue influence of extreme values in the effect size variables and precision weights (Guarascio et al., 2025; Knaisch and Pöschel, 2024; Opatrny et al., 2025).

7.2 Publication bias

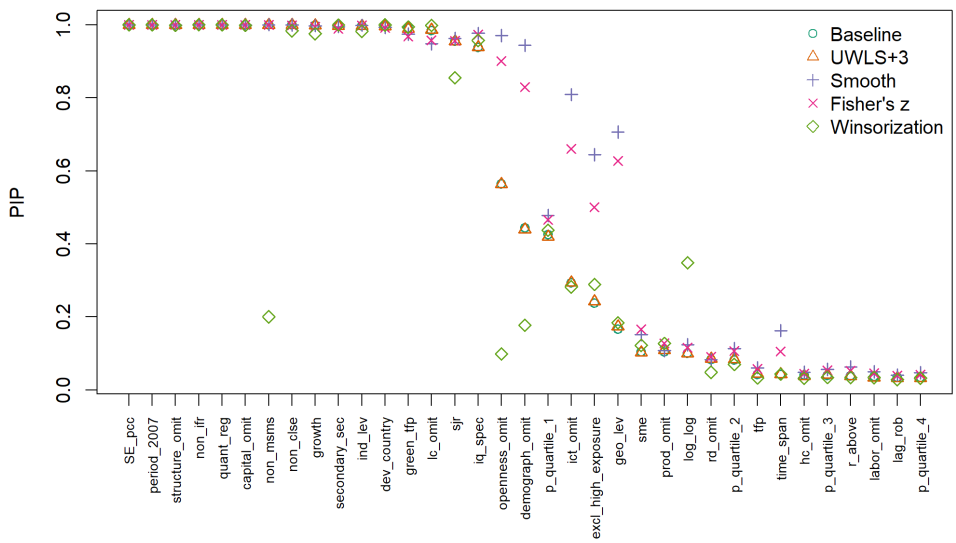
Table 11 shows that my FAT-PET findings reported in Section 5 are robust to the alternative meta-analytic estimators presented in Section 7.1. There is still clear evidence of a severe publication selection bias in favor of positive results. Beyond publication bias, only a very small effect of robots on productivity remains.

Table 11: FAT-PET with alternative estimators

	(1) UWLS+3	(2) Smooth estimator	(3) Fisher's z	(4) Winsorization
Publication bias (β_1)	2.1412***	2.0257***	2.0812***	2.3166***
	(.393)	(.3644)	(.372)	(.4097)
Precision effect (β_0)	.0072***	.0074***	.0073***	.0058**
	(.0026)	(.0026)	(.0026)	(.0028)

Notes: N = 1837. Standard errors clustered at study level are in parentheses. FE2 weights are used. The respective estimator is indicated in the column header. $^{***}p < .01$, $^{**}p < .05$, $^{*}p < .1$

Figure 9: Sensitivity of PIPs to alternative estimators



Notes: UIP and Dilution are used as priors in the BMA procedure (George, 2010).

7.3 Drivers of heterogeneity

I also implement the GTS and BMA procedure for the alternative meta-analytic estimators. Table 12 shows the results of these robustness checks for the GTS procedure. A positive publication bias with a severe magnitude is still evident. Likewise, the main drivers of heterogeneity identified in Section 6.2 are very robust to the alternative meta-analytic estimators. Only *p_quartile_2* and *structure_omit* are not selected across all alternative implementations. As before, the magnitude of the estimated regression coefficients and the constant term is rather small in terms of *PCCs* (Doucouliagos, 2011).
Figure 9 illustrates the sensitivity of PIPs in the baseline BMA procedure when the alternative specifications are used. The main drivers of heterogeneity presented in Section 6.2 are robust to this robustness check.

Table 12: General-to-specific with alternative estimators

	(1) UWLS+3	(2) Smooth	(3) Fisher's z	(4) Winsorization
SE_{PCC}	2.3789***	2.2428***	2.2843***	2.3723***
	(.4373)	(.392)	(.4128)	(.4175)
ind_lev	-.0303***	-.0268**	-.03***	-.0268**
	(.0111)	(.0103)	(.0109)	(.0114)
period_2007	.0159***	.0158***	.0164***	.0122**
	(.004)	(.0041)	(.0037)	(.0047)
non_ifr	-.0171***	-.0169***	-.018***	-.0151***
	(.003)	(.0033)	(.0031)	(.0035)
quant_reg	.0653***	.0508***	.0615***	.0652***
	(.022)	(.0146)	(.0172)	(.0224)
iq_spec	-.0157**	-.0148**	-.0158**	-.0169**
	(.0069)	(.0064)	(.0068)	(.0066)
dev_country	.0037***	.0034**	.004***	.0043***
	(.0013)	(.0013)	(.0014)	(.0016)
p_quartile_1	-.0146**	-.0137**	-.0179***	-.0155**
	(.0066)	(.0056)	(.0062)	(.0075)
p_quartile_2	-.0069**	-.0065**	-.0082***	
	(.0034)	(.0032)	(.0027)	
secondary_sec	-.0184***	-.0166***	-.0156**	-.0159**
	(.0065)	(.0061)	(.0063)	(.0067)
excl_high_exposure	.0123**	.0122**	.0114**	.0123**
	(.0052)	(.0051)	(.0054)	(.0051)
capital_omit	-.0067**	-.0065**	-.0072**	-.0055*
	(.0033)	(.0033)	(.0032)	(.0029)
lc_omit	.0075**	.0071**	.0089***	.007**
	(.0031)	(.003)	(.0028)	(.0028)
structure_omit	.0042**			.0046**
	(.0017)			(.0019)
prod_omit			-.006*	
			(.0032)	
p_quartile_3			-.0056**	
			(.0028)	
sjr			-.0033*	
			(.002)	
_cons	.0164**	.0192***	.0242***	.0138*
	(.0078)	(.0067)	(.0071)	(.008)
Observations	1849	1849	1849	1849
R-squared	.4134	.447	.4431	.3743

Notes: All columns are estimated using unrestricted WLS with FE2 weights. Standard errors clustered at the study level are in parentheses. $^{***}p < .01$, $^{**}p < .05$, $^{*}p < .1$

8 Discussion

The very small meta-analytic effect of industrial robot deployment on productivity points to a potential continuation of the Solow paradox and may be referred to as a ”modern productivity paradox” (Brynjolfsson et al., 2019; Capello et al., 2022). There are certainly tasks at which robots are better and cheaper than human labor, such as lifting heavy objects repeatedly and quickly. This raises the question of what are the reasons for the absence of a stronger productivity boost at the meta-analytic level. In the existing literature on the productivity paradox and technology adoption, several arguments 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 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. (2025) 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 effects 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 aligns with Koch et al. (2021), who report a net job creation for robot-adopting firms. Thus, it cannot be ruled out a priori that a simultaneity between growing labor demand and output gains dampens 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 by 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 are compensated by positive employment effects in the service sector. Similarly, Dottori (2021) finds that labor is reallocated toward less robot-intensive industries. 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 sectors 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 to some extant also reflected in my meta-analysis, since the moderator variable for the bottom productivity quartile points to lower productivity gains from robot adoption among entities with lower initial productivity.

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; Schwab, 2016; Skilton and Hovsepian, 2018). It is well-documented that it took many decades for these technologies to diffuse and for their benefits to fully unfold (Crafts, 2021). 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 (Brynjolfsson et al., 2019; Capello et al., 2022; Hoebert et al., 2023). 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 lower productivity effects found for the bottom quartile of productivity (*p_quartile_1*) in my multivariate MRA may be an example of more pronounced adjustment processes and, thus, higher adjustment costs of robot adoption in less productive entities. Such adjustment costs can also take the form of consultancy services from robot integrators to redesign the production system (Hoebert et al., 2023; 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 attacks. 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. 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 network externalities and spillover effects of intelligent automation may help 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 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*: Industrial robots may simply be 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, and productivity growth is simply returning to its more modest long-term trend after previous industrial revolutions (Brynjolfsson et al., 2019, pp. 40-41; Brynjolfsson et al., 2020; Schweikl and Obermaier, 2020). In this vein, Fernández-Macías et al. (2021, p. 79) 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. 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 (Acemoglu and Restrepo, 2019; Büchel and Floreano, 2018). 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).

9 Conclusion

I meta-analyze for the first time the relationship between the adoption of industrial robots and productivity. Through a systematic literature review, I identify 85 relevant primary studies with more than 1800 estimates of the productivity effect of robots. I find strong evidence that this empirical literature suffers from 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 that treat endogeneity, elasticity estimates, and estimates from higher-quality journals). Beyond publication bias, there is evidence of a statistically significant but very small positive effect of robots on productivity. So far, robotization seems to have exerted only a marginal boost to productivity.

My multivariate MRA of the drivers of heterogeneity in the primary literature points to diminishing returns of robotization. This is 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) evidence that developing/emerging countries tend to benefit more from robot adoption relative to advanced economies. With regard to econometric methodology, I find evidence that quantile regression is associated with stronger productivity effects, while including an interaction or quadratic term of the robot variable lowers the estimated effects. Capital input, economic structure, and labor costs are identified as important control variables to isolate the effect of robot adoption on productivity. Estimations at the industry level can result in lower effects relative to firm-level analyses. Moreover, I find some evidence that productivity measures adjusted for the emission of pollutants are smaller compared to traditional productivity measures, possibly pointing to a rebound effect in energy and resource consumption after robot adoption. Further, I obtain evidence of 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 robust across alternative selection procedures to identify the most important drivers of heterogeneity.

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 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 of diminishing productivity returns to robot installation. More research is also required with regard to the relationship between robot adoption and environmental outcomes. The global challenge of reconciling economic growth and environmental sustainability would benefit from studies that investigate the prerequisites such that technology adoption can contribute to both goals.

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, 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).

Appendix

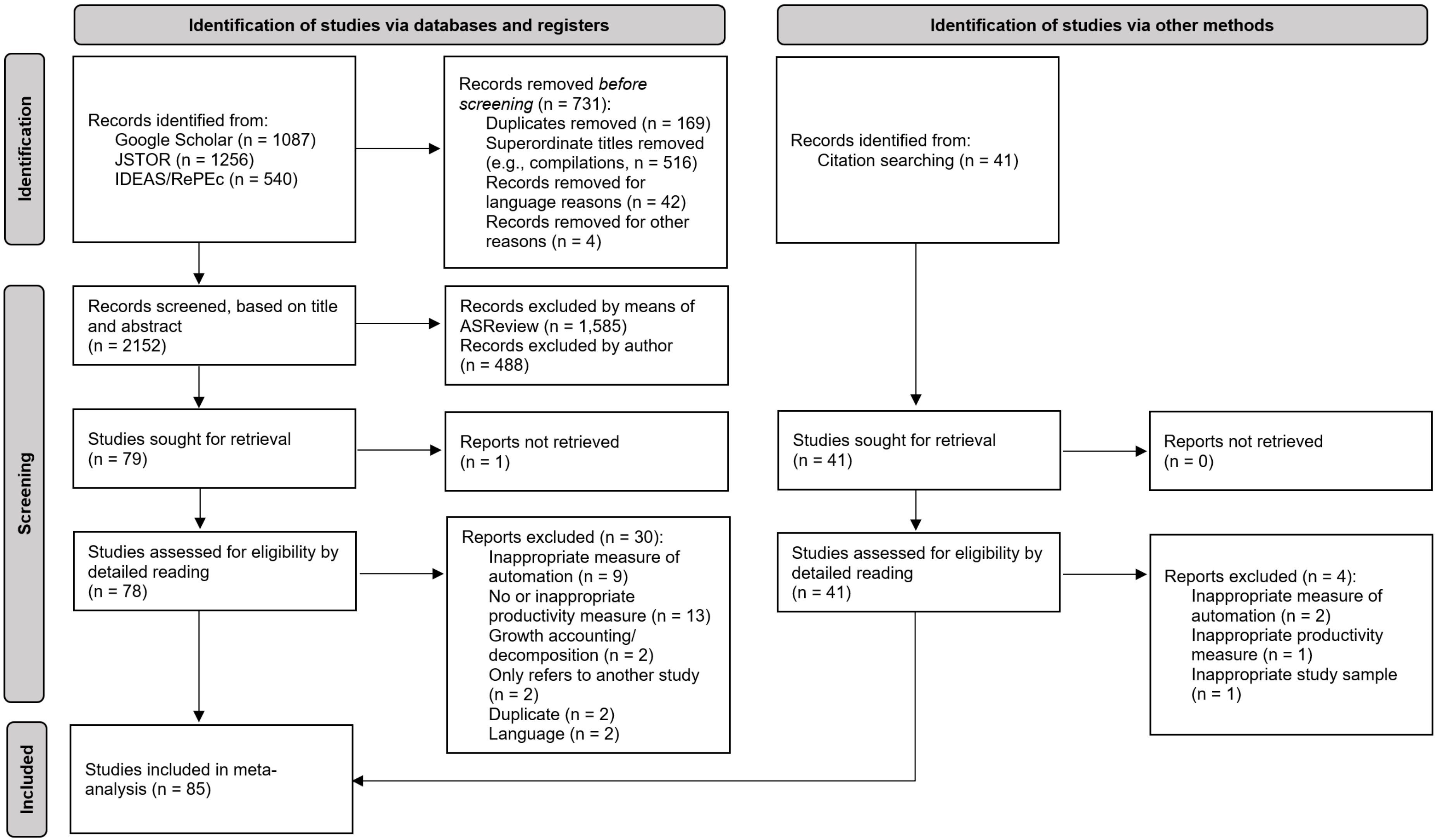


Figure A1: PRISMA 2020 flow diagram following Page et al. (2021).

Table A1: Overview of studies included in the meta-analysis

Study	Estimates	Mean PCC	SD PCC	Countries	Time span	Robot data	Level
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
Autor and Salomons (2018)	1	0.065	.	OECD	1993-2007	IFR	Industry
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)	21	0.013	0.014	France	1994-2013	French Customs Authority DOUANE	Firm
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
Cathles et al. (2020)	3	0.067	0.024	Europe + USA	2019	EIB investment Survey (EIBIS)	Firm
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	0.026	0.058	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
Freeman et al. (2024)	6	0.003	0.001	China	2001-2012	China Customs Database	Firm

Table A1: Overview of studies included in the meta-analysis

Study	**Estimates**	**Mean PCC**	**SD PCC**	**Countries**	**Time span**	**Robot data**	**Level**
Fu et al. (2021)	12	0.107	0.098	Worldwide developed	2004-2016	IFR	Country
Gong et al. (2023)	3	0.100	0.027	Worldwide	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
Huang et al. (2022)	9	0.015	0.015	China	2001-2010	China Customs Database	Firm
Huang et al. (2023)	17	0.011	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	Worldwide	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
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
Rojko et al. (2020)	1	-0.915	.	USA	2009-2018	IFR	Country
Shen and Zhang (2023)	1	0.059	.	China	2006-2020	IFR	Regional

Table A1: Overview of studies included in the meta-analysis

Study	Estimates	Mean PCC	SD PCC	Countries	Time span	Robot data	Level
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
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
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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