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# Robots and Wages: A Meta-Analysis<sup>\*</sup>

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#### Abstract

The empirical evidence on how industrial robots affect employment and wages is very mixed. Our meta-study helps to uncover the potentially true effect of industrial robots on labor market outcomes and to identify drivers of the heterogeneous empirical results. By means of a systematic literature research, we collected 53 papers containing 2143 estimations for the impact of robot adoption on wages. We observe only limited evidence for a publication bias in favor of negative results. The genuine overall effect of industrial robots on wages is close to zero and both statistically and economically insignificant. With regard to the drivers of heterogeneity, we find that more positive results are obtained if primary estimations a) include more countries in their sample, b) control for ICT capital, demographic developments, or tenure, c) focus on employees that remain employed in the same sector, d) consider only non-manufacturing industries, e) are specified in long differences, and f) come from a peer-reviewed journal article. More negative effects, in turn, are reported for primary estimations that are i) weighted, ii) aggregated at country level, iii) control for trade exposure, iv) and consider only manufacturing industries. We also find some evidence for skill-biased technological change. The magnitude of that effect is albeit small and less robust than one might expect in view of skill-biased technological change. We find little evidence for data dependence.

*Keywords*: robots, IFR, meta-analysis, publication bias, labor markets, wages, skill bias, gender bias, job polarization ; *JEL codes*: E24, J23, J24, J31, O33

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# 1 Introduction

In recent years, industrial robots have received much attention in economic research. The main characteristic of a robot is that it can perform tasks autonomously. This sets the technology apart from other capital goods like ICT capital or traditional machines which still need a human to operate. If robots can render human workers obsolete, how will this affect labor market outcomes such as wages and employment? From a theoretical perspective, both positive and negative effects on wages and employment are possible. Three main transmission channels for the effect of industrial robots on employment and wages can be distinguished in the literature. As pointed out by e.g. Acemoglu and Restrepo (2020) or Hötte et al. (2022b) and Hötte et al. (2022a), the overall effect depends on whether the productivity and reinstatement effect from automation outweighs the displacement effect of workers. If industrial robots substitute for human labor, wages and employment can decrease as tasks previously performed by labor are then performed by robots. However, if robotization raises overall labor productivity, thereby inducing a higher demand for labor, wages will increase. This is especially true if tasks performed by workers are complements rather than substitutes of robots. Again et al. (2017) call this the weak link of labor within the production function. As long as labor tasks are essential, the wage share cannot decline. Productivity improvements may also induce real income gains if lower production costs are reflected in reduced output prices, thereby enabling an expansion of aggregate output that leads to higher labor demand (Hötte et al., 2022a). Moreover, new economic activities and products can emerge, causing labor to be reallocated to (newly created) occupations and tasks which are more productive and therefore better paid. This reinstatement effect would then lead to higher wages and more employment (Acemoglu and Restrepo, 2018), Hötte et al., 2022a). Increasing usage of automation technologies can also generate additional jobs in the machine-producing sector as well as related services for installing and maintaining robotic production lines (Mondolo, 2021).

To scrutinize the total outcome of these co-existing effects, quantitative research focusing on labor market effects of robotization has increased tremendously. Klump et al. (2021) provide a first comprehensive overview of the existing literature on robots, wages, and employment. The empirical evidence on how industrial robotization affects employment and wages is very mixed. A meta study can help to uncover the potentially true effect of industrial robots on labor market outcomes and to identify the drivers of the heterogeneous empirical results. Owing to the large number of research (a Google Scholar search on industrial robots and wages yielded almost 9,000 hits in August 2022), we focus on wage effects only. On the one hand, wages are a valid indicator for shifts in labor demand given a certain labor supply, while they are simultaneously related to labor productivity. On the other hand, it is unlikely that a majority of workers replaced by robots in the past decades remained long-term unemployed or dropped out of the active labor force. Instead, they were likely reallocated to other industries, firms, or occupations. Rather than observing employment levels, it is thus more interesting to analyze whether workers exposed to robotization really suffer from significant income losses. Moreover, investigating differential trends in wage development across different skill or demographic groups in the population is at the core of explaining economic inequality.

By conducting a meta-analysis on the wage effects of industrial robots, we contribute to the literature in two important ways. First, employing several meta-regression models, we try to correct the original econometric research for publication, misspecification, and omitted-variable biases to detect a potentially overall effect of robots on wages. Second, we shed light on the heterogeneity of primary empirical findings by scrutinizing the influence of study-invariant factors as well as the inclusion of essential control variables on the estimated wage effects. We are especially interested of whether the various, often contradicting results of the primary literature are driven by the level of analysis, using alternative measures for robots and wages, studying different geographic regions or time periods, distinct educational and occupational skill groups, demographic groups (gender, age, income quartile), labor mobility as well as sector of employment. The vast majority of studies relies on data of the International Federation of Robotics (IFR). These IFR data exhibit several issues and limitations that are treated differently by different authors (see Jurkat et al. (2022) for an analysis of the IFR data set). Our meta-analysis allows to investigate if the varying data preparation approaches contribute to the variation in empirical results. We consider papers using the IFR dataset as well as other data sources on industrial robots (e.g. trade data, survey data) to rule out that the IFR dataset and the necessary data preparation has a substantial influence on the estimated labor market outcomes.

By means of a systematic literature research, we collected 53 papers containing 2143 estimates of the impact of robot adoption on wages. We conducted a systematic literature search in August and September 2022 with the keywords "industrial + robot + wages" and included all eligible papers from 2018 to the third quarter of 2022. Applying simple vote counting based on the conventional 5%-significance level, reveals that 17.9% of the estimates show a positive and statistically significant effect, 38.6% report a significantly negative effect, while 43.5% are statistically insignificant.

A main challenge for meta-analysis in economics is that often the dependent and/or independent variables of the estimations vary across studies or even within studies. In our context this means that "wages" and "robots" may be differently defined. "Robots" can be expressed as density (e.g. robots per million hours worked or per 1000 employees), a simple stock of robots, a monetary value, or as a binary indicator for robot adoption (1 if at least one robot is installed or zero otherwise). Moreover, a shift share measure can be applied to distribute robot data at country-industry level to a smaller geographic unit (see e.g. Acemoglu and Restrepo, 2020). "Wages" can be expressed in wage rates (hourly vs. daily/weekly/monthly/annual), wage bills, income, or earnings cumulated over a certain period. In order to ensure comparability of estimates across and within studies we need to build a unitless index. We calculate partial correlation coefficients and perform weighted least square (WLS) regressions using alternative weighting schemes for the primary estimates. We closely follow the methodology laid out by Stanley and Doucouliagos (2012) and applied by e.g. Cazachevici et al. (2020) or Duan et al. (2020). In order

to assess the heterogeneous effects beyond potential publication bias, we construct a moderator matrix consisting of a set of study/estimation characteristics. We then employ multivariate WLS regressions with our moderator matrix to identify the drivers of heterogeneity in the estimated effects of robot adoption on wages.

We only find limited evidence for a negative publication bias. The genuine effect of industrial robots on wages beyond publication bias is close to zero and both statistically and economically not significant. This suggests that industrial robotization has so far had no visible overall effect on wages for the total population. With regard to the drivers of heterogeneous findings in the primary literature, we find that more positive results are obtained if primary estimations a) include more countries in their sample, b) control for ICT capital, demographic developments, or tenure, c) focus on employees that remain employed in the same sector, d) consider only non-manufacturing industries, e) are specified in long-differences, and f) come from a peerreviewed journal article. More negative effects, in turn, are reported for primary estimations that are i) weighted, ii) aggregated at country level, iii) control for trade exposure, iv) and consider only manufacturing industries. We also find some evidence for skill-biased technological change (SBTC) since wages are more positively affected in high-skilled occupations and more negatively affected in medium- to low-skilled occupations. The magnitude of that effect is albeit small and less robust than one might expect in light of the widespread assumption of SBTC in theoretical models incorporating automation (e.g. Fierro et al., 2022; Prettner and Strulik, 2020; Lankisch et al., 2019). Although, we find evidence for more positive wage outcomes in the non-manufacturing sector, our meta-regression results for skill groups do not support job/wage polarization due to a rise of service occupations as hypothesised by Autor and Dorn (2013).

The paper is organized as follows: Section 2 provides a broad overview of the existing empirical literature on the wage effects of robotization and some related meta-studies in economics. Section 3 the data and the selection process and section 4 describes the methodology. Section 5 studies the overall effect of robot adoption on wages and tests for the publication bias. Section 6 analyses then the heterogeneity of empirical results by means of a multivariate meta-regression framework including a moderator matrix. Section 7 repeats the meta-analysis for the subset of primary estimates that treated endogeneity by means of an instrumental variable (IV) approach. Further robustness checks are presented in section 8. Section 9 concludes and provides some recommendations for future research on the labor market impact of robots.

# 2 Related Literature

The economic and social impact of industrial robots is a rapidly evolving field of scientific research. Klump et al. (2021) provide a first comprehensive overview of the empirical literature on robot adoption. Topics covered in empirical studies comprise mainly productivity and employment effects but also the impact on international trade (e.g. Krenz et al., 2021), as well as links with demographic change (e.g. Acemoglu and Restrepo, 2022a), health (e.g. Caselli et al., 2021b) and voting behavior (e.g. Anelli et al., 2021a). By far the largest strand of literature is focused on employment and wage effects of robotization. While writing a synopsis of all the 53 studies which entered our meta-analysis is beyond the scope of this paper, we would nevertheless, like to refer to some results in order to emphasize the very mixed wage effects found in the empirical literature. The paper by Acemoglu and Restrepo (2020) as well the paper by Graetz and Michaels (2018) have become the central references for research on this topic. Both papers use the data on industrial robot adoption provided by the IFR. Acemoglu and Restrepo (2020) study US local labor market effects of increased robot exposure between 1993 and 2007. They find that one robot per thousand workers reduces wages by 0.42%, the employment-topopulation ratio by 0.2 percentage points, and substitutes for 3.3 workers. Contrary to that, Graetz and Michaels (2018) find no aggregate employment effect of increased robot use between 1993-2007 in a sample of 14 industries in 17 countries and even report a positive effect on mean hourly wages (albeit with a magnitude of only 10% of the estimated labor productivity gains). However, they find that increased robot usage lowers employment for low-skilled workers.

In line with Acemoglu and Restrepo (2020), Aghion et al. (2019) confirm that robotization reduces aggregate employment at the regional employment zone level in France and show that this effect is concentrated on less-educated workers. Similarly, Borjas and Freeman (2019) find a negative impact of industrial robots on employment and earnings in the US, concentrated on lower educated workers and those in automatable occupations. Giuntella and T. Wang (2019) find large negative effects of robot exposure on employment and wages for state-owned enterprises in China. Chiacchio et al. (2018) also apply the local labor market approach for a study of the employment and wage effects of robots in Europe. Their results support a negative impact of robots on employment but only mixed results on wages. Compagnucci et al. (2019) provide evidence for the displacement of workers in OECD countries, measured as reduced growth rates of hours worked and real wages. A number of papers report rather small negative or statistically insignificant results. Dauth et al. (2021) find a small negative but statistically insignificant impact of robot exposure on wage growth in Germany. Within manufacturing the wage effects are negative, however, they are slightly offset by positive wage effects in services. Bekhtiar et al. (2021) show that the positive wage effects documented by Graetz and Michaels (2018) are reversed into negative effects when replicating their analysis while excluding non-manufacturing industries. This reduced sample further produces no robust support for SBTC in terms of a decline in the share of hours worked by low-skilled workers. Instead, Bekhtiar et al. (2021) report evidence for wage polarization as wages of middle-skilled workers are negatively affected. Moreover, for the more recent time horizon 2010-2015, they cannot detect a clear effect of robot adoption on average wages.

Koch et al. (2021) use a panel dataset of Spanish manufacturing firms from Encuesta Sobre Estrategias Empresariales (ESEE) for the period 1990-2016 and thus belongs to the group of papers which do not employ the IFR data on industrial robots. Using a dummy variable for robot adoption, they identify positive effects of robots on employment and output for robot-

adopting firms, while average wages are not significantly affected. Non-adopters in turn suffer from output losses and reduce employment, suggesting an intra-industry reallocation of market shares. Barth et al. (2020) find in a firm-level study for Norway that robot adoption increases the skill premium by affecting the wages of highly educated workers positively and those of loweducated workers negatively. They use micro-level import data to construct a dummy for robot adoption at the firm level. Other papers relying on firm level data are: Acemoglu et al. (2020) for France, T. Wang et al. (2022) for China, and Dixon et al. (2021) for Canada. Generally, studies at the firm level tend to report only weak wage effects of robots, with some tendency to more positive findings. In contrast to Barth et al. (2020), robot measures of studies relying on trade/ customs data are usually monetary values. Examples of studies are: Bonfiglioli et al. (2021) for the US, Rodrigo (2021) for Brazil, and Lai et al. (2022) for China. The latter finds that disposable income for the high income group increases in relation to low income groups but not in relation to the mean wage. This suggests that the overall productivity effect of robot adoption increases mean wages. Low income groups, however, do not benefit from this leading to increased income inequality. Bonfiglioli et al. (2021) do not find a significant wage effect of robot adoption in the manufacturing sector, instead, wages of the non-manufacturing sector are affected positively leading to also an overall positive effect of wages averaged across sectors – a result in line with the findings of Graetz and Michaels (2018).

Opposing results of robot adoption can also be found with regard to the gender wage gap. While Aksoy et al. (2021) find that a 10% increase in robotization has led to an increase of 1.8% in the gender wage gap for a panel of 20 European countries, Ge and Zhou (2020) find that increased robot adoption has contributed to a decrease in the gender wage gap between 1990 and 2015 in US local labor markets. Albinowski and Lewandowski (2022) shed further light on the labor market outcome of robot adoption conditioned on gender, age, and occupational task content. The wage effects for younger men aged 20-49 occupied in routine manual tasks were negative whereas for women they were much smaller or lacked statistical significance altogether. Robot adoption benefited wages of young women aged 20-29 in routine, cognitive occupations more than the wages of their male counterparts. Adachi (2022) is an example for an occupation-level analysis. It finds negative wage effects from cost reductions in robot adoption in the US between 1990 and 2007 which are concentrated on "routine production" and "routine transportation" occupations. No significant effects are reported for abstract and manual service occupations. Using a large sample of Spanish workers from 2001 to 2017, Cuccu and Royuela (2022) examine transitions to different employers following involuntary dismissals associated with exposure to robots. They find a more pronounced loss of earnings among medium- and low-skilled workers, while women are more negatively affected than men. However, also high-skilled workers face a lower pay if they had to the switch the sector of employment.

In the light of these mixed results, conducting a meta-analysis of the robot-wage nexus is the right tool to assess the heterogeneity apparent in the empirical literature. A meta-analysis helps to uncover an overall effect –if such a true effect indeed exists- and to identify potential

drivers of the mixed effects by applying a rigorous econometric methodology. Meta-regression analyses are very common in medical studies but have become increasingly implemented in an economic context as well. Recent examples include Duan et al. (2020) who conduct a metaanalysis of the spill-over effects on exports, Cazachevici et al. (2020) who assess the effect of remittances on growth, Terzidis et al. (2019) who study the effect of technological progress and trade on labour market outcomes, Cardoso et al. (2021) who analyze the effects of offshoring on wages, and Knoblach et al. (2020) with a meta-regression analysis on the aggregate elasticity of substitution. Our meta regression analysis is similar to the first four studies in the sense that we need to rely on a partial correlation coefficient in order to measure effect sizes of robot adoption on wages. Knoblach et al. (2020) can retrieve their value of interest directly out of the different studies as it is already an elasticity which is by definition dimensionless. We, however, have to deal with various measures for both the independent (here robots) and the dependent variable (here wage). We will discuss this in the following section in more detail. To our knowledge, we are the first to conduct a meta-regression analysis on the wage- robot adoption relationship. Mondolo (2021) and Hötte et al. (2022a) provide literature reviews on the employment effects of technological progress covering a broad scope of technologies defined in different categories: "ICT, Robots, Innovation, TFP-style, Other" in Hötte et al. (2022a); R&D investments as a proxy for disembodies technological change as well as "Computer and ICT", "Robots", "Automation and new digital technologies", and "Artificial Intelligence" as categories of technological change embodied in capital inputs in Mondolo (2021). These reviews, however, are only of descriptive nature, and do not employ a quantitative meta-regression framework like our study does.

# **3** Data collection and constructing the dataset

We used Google Scholar as our primary search engine and conducted the search with the keywords "industrial + robot + wages" in August 2022. We inspected the first 600 hits of 8,470, sorted by relevance. We also used IDEAS/RePEc as supplementary search engine in September 2022 and obtained only 13 hits, which were all duplicates of already identified papers. Furthermore, we looked at the references of all eligible studies to detect additional relevant studies. This method is known as snowballing. We selected all papers which meet the following criteria. The paper must deal with industrial robots according to the ISO standard 8373:2012 (§ 2.9) and their effect on wages applying econometric methods. We omitted papers with simulations or event studies and papers with a broader measure of automation than industrial robots. The IFR 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" (IFR, 2020). We follow this definition and disregard papers whose measure of automation comprised service robots, other dedicated robots, numerically controlled machinery, AI or digital

technologies (e.g. Eggleston et al., 2021, Bessen et al., 2020). Purely theoretical measures of susceptibility to or risk of automation are excluded as well (e.g. Brambilla et al., 2022). We also drop papers where robots are only used as an instrument (e.g. Acemoglu and Restrepo, 2022b or Autor and Salomons, 2018). Moreover, estimations must involve a continuous measure of wages or income as dependent variable. Estimations with a measure of inequality (e.g.Brall and Schmid, 2020), skill premium (e.g. (Gravina and Foster-McGregor, 2020)), labor (income) share (e.g. Fu et al., 2021), income mobility (Berger and Engzell, 2022) or a categorical variable of wage changes (Jongwanich et al., 2022) as dependent variable were thus excluded. We do not restrict our search to peer-reviewed papers as this field of research is so young that only a fraction of papers is already published. But we take the publication status into consideration when we code our moderator matrix. Papers must be published in English and be publicly accessible (through paid journal or open access). As laid out in section 4, we need coefficients and their respective standard errors in order to calculate the effect size. Sometimes, standard errors or sample sizes were not reported. We contacted the authors whenever we lacked information. Primary estimates presented as figures were included if the authors made the exact numbers available upon request. In rare cases, when we did not receive feedback, we had to disregard the primary study for our meta-analysis. In total, we were able to code 53 primary studies with 2143 estimates. A comprehensive list of all primary studies included in the meta-analysis can be found in the Appendix (s. Table F1). The number of estimates per study ranges from 1 to 394, with an average of 40 and a median of 16 estimates per study. The process of searching an selecting eligible studies is illustrated in a PRISMA flow diagram in the Appendix (s. Figure E1).

It is common in meta-regression analysis to code a so-called moderator matrix. This matrix contains all relevant study-dependent or estimation-specific characteristics that might help to explain the mixed effects of industrial robots on wages which extend beyond potential publication bias. A properly coded moderator matrix addresses potential biases from omitted variables and misspecification in the primary literature. We divide our analysis of the determinants of the heterogeneous wage effects in the primary studies into five broad groups: data characteristics, inclusion of control variables, subsamples, estimation model, and publication status (s. Tables A1, A2, A3, and A4). Data characteristics are split further into 5 subgroups, namely level of analysis, geography, time period, and the construction of the independent, and dependent variables. Most of the explanatory variables in our multivariate meta-regression analysis are constructed as binary indicators taking on a value of 1 if the measure fits the category and 0 otherwise.

Conducting a meta-analysis in economics is usually challenging due to the fact that the construction of the independent and dependent variables often differs across studies or even within studies. Our meta-study on the wage effects of industrial robots is no exclusion to this. We identify five main wage concepts in the primary studies under inspection: (annual/ monthly/ weekly/ daily) wages, hourly wages, income, cumulative earnings, and the wage bill of a whole firm or industry. We include hourly wages separately because we deem it to be the most precise measure of wages as it accounts for potential adjustments in working time. Additionally, the moderator variable "rel\_wage" takes into account if a wage measure is expressed relative to an average or total wage measure (e.g. sector average). 47.6% of the primary estimation use wages as dependent variable, 35.2% hourly wages, 8.4% use a wage bill, 5.3% income, and 3.5% cumulated earnings. 6.9% express the wage measure in relative terms.

With regard to the independent variable, our main interest is on the wage effect of industrial robots. We do so for two reasons: Firstly, we believe that industrial robots differ greatly from other types of automated capital in the sense that they truly substitute for human labor. The clear-cut definition of industrial robots in ISO standard 8373:2012 (§ 2.9) enables us to compare wage effects of robotization across studies. Other notions of automation would differ much more across studies as the scope of technologies considered significantly varies without such a clearcut definition. Secondly, by limiting our research to industrial robots, we can check whether specific characteristics of the IFR dataset have an influence on regression results. The IFR is the main provider of worldwide data on industrial robots. However, the dataset does have some caveats (see Jurkat et al., 2022 for an extensive analysis of the IFR data). For instance, IFR robot stocks are calculated from data on annual installations without applying a continuous annual depreciation rate but instead by assuming that a robot becomes obsolete after 12 years (one-hoss shay depreciation). This is in contrast to the perpetual inventory method (PIM) which is usually applied by economists when constructing a capital stock measure. Graetz and Michaels (2018) among others calculate the stock of industrial robots via PIM with depreciation rates ranging from 5-15%. Other studies, in turn, rely on the robot stock as provided by the IFR. Furthermore, data on annual installations can be subject to compliance issues. If in a given year less than 4 robots are installed, the number is not reported by the IFR in order to avoid drawing conclusions about robot producing companies. Instead, the robots are then classified in a special category named "unspecified" at the same level of hierarchy for the industry classification or moved upwards in the hierarchy of the industry classification until compliance is achieved. This compliance mechanism has been implemented from 2014 onwards and may create discrepancies when applying the PIM method to calculate the capital stock from annual installations as compared to the robot stock provided by the IFR. The category "unspecified" however, may not only contain those robots which could not be reported due to compliance reasons but also those which could not be classified according to the industry classifications or where the destination industry is unknown. Especially in earlier years of the database (which starts in 1993) this category can be quite large. Some authors (e.g. Graetz and Michaels, 2018) drop it entirely, others allocate it across the industry classification via the respective industry shares of specified robots (e.g. Acemoglu and Restrepo, 2020). Consequently, how researchers deal with this "unspecified" category might affect the regression results.

Even though most studies we found use the IFR dataset (85.4% of the estimates are based on IFR data), there are also some studies relying on other data sources for the usage of industrial

robots. Alternative data sources are mainly trade and customs data, but also firm-level surveys, data from the Japan Robot Association (JARA) or patent data (e.g. Acemoglu et al. (2020), use French firm level data compiled from several data sources). These alternative data sources partially provide information on robot usage at a finer level of aggregation than country-industry level, e.g. at firm-level or regional level. Related to the data source and the treatment of the original data is the construction of the measure for robot usage. Analogously to the dependent variable, we identified 5 different approaches: a robot density expressed as the quantity of robots in relation to either the number of workers or hours worked (either unadjusted or adjusted for output growth), a shift-share design, a simple count of robot stocks, a monetary value of the robots used, or a dummy variable for robot adoption. If the robot measure does not fit in any of these categories, we assign it to category "other robot measures" (e.g. patent data, robot prices, or robot installations instead of stocks).

Most estimations (83.7%) use a robot density as the independent variable and many of these are so called shift-share measures (in total, 38.8% of the estimates are based on a shift-share design). A shift-share measure is typically used to distribute national robot stocks to finer levels of geographic aggregation. It combines a shift element – usually the change in the national robot stock per industry divided by the number of employees (often at a base year well before the industry has begun to install robots to avoid endogeneity on the labor market) with a share element - typically a regions' or local labor markets' (e.g. commuting zones, states, cities) industry employment shares. The term "exposure to robots" is used in the literature whenever a shift-share measure is combined with a robot density. Such a shift-share design tries to measure the risk of a region regarding potential displacement effects by robots based on its industry employment composition. Accordingly and Restrepo (2020) were the first to apply a shift-share measure to an analysis of local labor markets. Because of its ensuing widespread use, we include the shift-share design as an additional category for measures of robot usage. If it is based on a robot density, it enters both categories. Sometimes, studies call their measure "exposure to robots" but only apply a shift component. In such cases, where the share element is missing, we categorize it as a mere robot density.

There is a very high correlation of some robot measures with the usage of non-IFR data. In fact, all studies employing a monetary robot variable (i.e. trade data) and most studies using another robot measure which is not a robot stock or density, draw on non-IFR data. Therefore, we do not utilize a binary indicator for non-IFR data explicitly in our moderator matrix as it is already captured by the robot measure categories. Another moderator variables is termed "alter\_data" to capture robustness checks within primary studies. It refers to alternative imputation methods or the use of alternative data sources for the wage or robot measure within studies without changing the type of measure according to our categories.

The discussion of the shift-share design brings us directly to another important aspect of this meta-study: the level of analysis/aggregation. The impact of robot adoption on wages may differ greatly amongst industries and regions depending on the respective exposure to robots.

For instance, Bekhtiar et al. (2021) doubt the appropriateness of industry level analyses because they neglect spillover effects between industries and thus prevent drawing conclusions on the economy wide effect of robot adoption. Firm-level studies in turn allow comparing robotadopting firms to non-adopters and thereby analysing business-stealing effects or adjustments in the skill-composition of labor employed by robot adopters that may explain changes in firms' averages wages (Koch et al., 2021). Similarly, Acemoglu and Restrepo (2020)'s LLM approach is also motivated by taking into account spillover effects within and across commuting zones. Different levels of analysis tackle the empirical question from different angles, moving from a macroeconomic perspective to microeconometrics. The finer, respectively the lower the level of analysis, the more concisely the effects of robot adoption on wages can be estimated. While higher levels of aggregation measure overall net effects resulting from all the forces exerted by robots on wages, lower level of aggregation allow disentangling the heterogeneous mechanisms behind aggregate effects (Calvino and Virgillito, 2018). Consequently, we distinguish between 7 levels of analysis: country level, industry level (reference category), regional (including local labor markets) level, firm level, occupational level, demographic group level, and worker level. The occupational level refers to analyses where wages are aggregated at clearly defined occupations or occupational groups according to occupational classification schemes. The demographic cell level captures analyses that aggregate wages for certain demographic groups defined by criteria such as age, gender, education, or race. Only 1% of the primary estimates is at country level, 14.7% are at industry level, 16.8% at regional level, 7.3% at firm level, 3% at occupational level, and 23% at demographic group level. Individual-level estimates form the largest group with a share of 34.3%.

Since the estimated wage effects are very heterogeneous regarding both sign and size, it is essential to investigate potential disparities between different geographic entities. We, therefore, include 4 dummy variables indicating whether the estimation sample covers the US, Europe, Japan or developing countries. 77.3% of the estimates are based on data for a single country, while the remaining 22.7% come from multi-country samples. Of the single-country estimates, 36.5% originate from the US, 50.6% from European countries, 12.2% from developing or emerging countries, 0.6% from Japan, and 0.1% from Canada. It is often conjectured that negative effects of robot adoption may be more prevailing in countries with more flexible labor markets where firms can more easily lay off employees, e.g. leading to a dichotomy between the US and Europe (Leigh et al., 2020, Nardis and Parente, 2022). This meta-study contributes to this discussion by applying rigorous econometric analysis instead of relying on vote counting. A first inspection of the studies and their estimates suggests that strong negative effects are mainly prevalent in the US. The mean PCC for all estimates in the US is -0.452 compared to 0.061 in Europe (see Table ??). However, as we will show for our full sample of primary estimates, these effects are not driven by geography. If we introduce more moderator variables, the negative US effect is not robust across specifications. In addition, developing countries may respond differently to the adoption of robots than industrial countries. We, therefore, construct a binary variable

indicating that developing or emerging countries are analyzed by the primary study. 15 studies focus solely on developing countries<sup>1</sup>. We are indicating Japan separately since this country is the frontrunner in industrial automation (Adachi et al., 2022).

Data coverage on industrial robots by the IFR begins in 1993. However, in the early years robot adoption rates were rather low and estimated effects might be low accordingly. In order to account for this, we include the sample period of each estimation. That is, we calculate a midyear for the sample period of each primary study and define a cutoff year. We use 2007 as cutoff to separate estimates with an average sample year after 2007 from those with an average sample year before or equal to 2007 as this marks the year before the financial crisis. For robustness check, we also use 2010 as cutoff year. The number of sample years is included as well to account for longer term effects of robot adoption on wages. If the number of sample years is euqal to 1, the respective estimation is of cross-sectional nature.

Most of the primary studies examined in our meta-study control for gender, demographic shifts in working age population, the manufacturing employment share, the skill level of employees, as well as effects from trade and use of information and communication technologies (ICT). We code binary indicators for the inclusion of these control variables in primary estimations. In addition, most primary studies also provide estimates for subsamples such as age groups, educational and occupational skill types, gender, and wage quartiles. With regard to subsamples related to skill levels, we code binary indicators for subsample estimations in primary studies focusing solely on workers with a low, medium or high level of education as well as workers in high vs. medium- to low-skilled occupations to detect heterogeneous wage effects among different types of workers. This division is useful as it allows us to investigate the idea of job polarization and SBTC. In line with SBTC, there is some evidence that high-skilled workers are less prone to substitution through robots (Graetz and Michaels, 2018; Vries et al., 2020; Damelang and Otto, 2023). So robotization may impact their wages differently than other skill groups. The job polarization hypothesis is supported by some evidence that industrial robots and automation in general erodes mostly the middle-skilled workers and occupations because these are the ones which can be most easily automated due to their high routine character (Acemoglu and Autor, 2011; Autor and Dorn, 2013; Vries et al., 2020; Adachi, 2022; Bordot, 2022). Not controlling for skill levels could bias the overall wage effect.

Unfortunately, subsamples are rarely constructed by the same classification scheme across different studies. For instance, the age limits within the different age groups vary across studies. We have chosen to code four age groups (<30, 30-49, 50-59, and >=60) and regrouped studies with different age limits to fit our grid. The age of the working force might constitute another important determinant in explaining different wage effects. Accemoglu and Restrepo (2022a) conjecture that middle-aged workers are relatively more engaged in automatable production

<sup>&</sup>lt;sup>1</sup>One further study contains 42 countries in its sample, also comprising developing/ emerging countries (Jung and Lim, 2020).

tasks whereas older workers specialize in services. Some studies provide estimates for different wage percentiles. There, we proceeded in the same manner as with the age limits and regrouped these estimates into wage quartiles if necessary. When it comes to occupational skill-levels, some studies conduct subsample estimations for a wide range of occupations or occupational groups such as manual, routine, abstract, blue-collar, white-collar, professionals, non-professionals or elementary occupations. In order to ensure comparability across studies and without inflating our moderator matrix, we categorized each of these classifications by means of a conversion table according to the International Standard Classification of Occupations (ISCO-08) into high, and medium- to low skilled occupations (s. Table B1 for conversion details). Unfortunately, we had to merge middle-skilled and low-skilled occupations because of overlapping definitions. Mostly authors classify skill levels of occupations according to ISCO-08. However, sometimes they reclassify middle-skilled occupations to low-skilled occupations even if they are defined as middle-skilled occupations according to ISCO-08. This no longer allows us to analyse middleand low-skilled occupations separately in our MRA.<sup>2</sup> Educational skill levels of workers are classified according to ISCED-2011 into high, medium, and low (s. Table B2 for conversion details). Further subsamples are related to gender, labor mobility (i.e. switching employer/sector/ region/ occupation and employees with a temporary contract), and robot exposure (manufacturing and non-manufacturing subsamples, samples excluding the automotive industry (which traditionally has a high robot density), and samples excluding outliers in terms of regions most exposed to robotization).

The next pillar of our moderator matrix captures diverging estimation techniques across or within studies. Various econometric methods are applied in the primary literature, foremost OLS, quantile regression, or IV estimation techniques. Econometric specifications differ according to log in log formulations, weighted estimations, long or stacked differences. Sometimes, robots enter with time lags or multiple times in the same regression through an interaction term, a quadratic term or as foreign or domestic robots. We account for all of these estimation models and its functional forms. Specifications estimated in long differences (i.e. changes in wages and robot adoption between an initial year and a final year, with several years in between) are typically interpreted as estimating the long-term structural impact of robot adoption on wages. It is important to note, that such specifications require panel data for computing differences between the two points in time but end up as cross-sectional estimations because the time dimension is removed. 46.8% of our estimates collected from primary studies stem from panel regressions, while 53.2% are cross-sectional estimates.

To assess the quality of the publications we apply a dummy variable for publication status indicating whether estimates come from a study published in a peer-reviewed journal. Other types of publication are mainly working papers and some dissertations. 44.6% of the estimates were published in academic journals. The large share of estimates coming from working papers

 $<sup>^2 {\</sup>rm The}$  correlation between the categories "medium-skilled occupations" and "low-skilled occupations" was 0.88

(54.5%) underlines that the labor market effects of robotization are a young and rapidly emerging field of economic research.

### 4 Methodology

Frequently, the empirical literature reports many different estimates with varying signs leaving the reader puzzled regarding the true effect of a certain relationship. Readers might be induced to rely on simple vote counting techniques in order to assess an overall effect. Vote counting entails counting the negative and positive estimates and whether they are statistically significant at the 5% level or not. This is, however, not a precise methodology as it does not account for sampling error, publication bias, the strength of the effects, and the source of the variation (Stanley and Doucouliagos, 2012). Mixed results in economic research can be due to data dependence, econometric specifications, sample selection and publication bias. The latter is of special importance as one criterion for publication is often statistical significance so that estimates with low or even no statistical significance may not be reported at all. Data accessibility and/or reliance on special datasets could also bias results. The aim of meta-regression analysis (MRA) is to apply rigorous econometric analysis to summarize and explain reported empirical estimates. Applied to our context, this means systematically examining the heterogeneous effects of robot adoption on wages. Data dependence is of special interest to us since most studies we find on robots and wages use the IFR dataset. We want to rule out that results are driven by certain peculiarities of the IFR data and the particular treatment of these by the researchers (s. section 3).

MRA uses effect sizes to assess the strength and direction of empirical estimates. The aim is to uncover a potential true overall effect as well as drivers of heterogeneous findings in the primary literature. Effect sizes should be comparable across and within studies (Stanley and Doucouliagos, 2012). The most common effect size measures in economics are partial correlation coefficients (PCCs), elasticities, and t-statistics. While elasticities have the advantage of measuring the economic effect of a variable (here *robotization*) on a certain outcome (here *waqes*), they cannot be calculated as easily from the primary studies if the functional form is not in loglog. PCCs and t-statistics are both statistical measures rather than economic measures but are more easily calculated from the primary studies. Furthermore, both are especially convenient when the dependent and/or independent variables of the regression models are not identical across studies that nevertheless measure the same relation. In our case, we can distinguish between 5 main wage concepts and 5 robot measures (for details see section 3). In most cases, the t-statistics can be easily calculated from the coefficients and its standard errors reported in the primary studies. However, in order to assess the strength of the effect, PCCs are better suited as they are a unitless index ranging from -1 to +1. We, therefore, follow the recent literature on meta- analysis (Stanley and Doucouliagos, 2012; Babecky and Havranek, 2014; Duan et al., 2020; Cazachevici et al., 2020; Cardoso et al., 2021) and employ the PCC concept as our effect

Measure	Obs.	Mean	Median	SD	Min	Max
t-value	2143	-1.32963	-0.91176	4.32091	-21.58333	50
df	2143	72746.066	89	423692.6	5	2839811
PCC	2143	-0.09637	-0.03347	0.34527	-0.95492	0.99741
$SE_{PCC}$	2143	0.09293	0.08748	0.07951	0.00059	0.44695

Table 1: Descriptive statistics for effect size variables

size. The  $PCC_{is}$  is defined as follows:

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

where t denotes the t-statistic and df the degrees of freedom from estimate i in study s. Table 1 shows summary statistics for the effect size variables. Applied to our context, PCC < 0 indicates a negative effect of robotization on wages while PCC > 0 captures a positive effect of robotization on wages. PCCs close to  $\pm 1$  are very strong effects that are rarely encountered in MRA in economics. Doucouliagos (2011) provides guidelines for the relative strength of PCCs based on 22,000 partial correlation coefficients drawn from various meta-studies. A partial correlation coefficient above  $\pm 0.33$  can be considered strong while a PCC of  $\pm 0.07$  is low and  $\pm 0.17$  constitutes a moderate effect.

Our meta-study also includes primary estimations that employ interaction or quadratic terms for the robot variable and thus report more than one coefficient for the relation of interest. To fit interaction terms into our meta-analysis, we follow Cazachevici et al. (2020) by calculating the average marginal effect of robots on wages and applying the delta method to approximate the associated standard error:

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

where  $\beta_1$  is the estimate of the robot coefficient for the linear term,  $\beta_2$  is the estimate of the coefficient for the interaction term,  $\bar{x}$  is the sample mean of the variable interacted<sup>3</sup> 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. Since the original dataset used in the respective primary studies were not available, we omit the covariation between the estimated coefficients from the formula for  $SE(\beta)$  by assuming the covariances to be zero. In case of a quadratic term, the total effect of robots on wages must be linearized using the following formula for the average marginal effect (Zigraiova and Havranek, 2016):

$$\beta = \hat{\beta}_1 + 2\hat{\beta}_2 \bar{x}; \qquad SE(\beta) = \sqrt{SE(\hat{\beta}_1)^2 + 4SE(\hat{\beta}_2)^2 \bar{x}^2 + 4\bar{x}Cov(\hat{\beta}_1, \hat{\beta}_2)}$$
(3)

<sup>&</sup>lt;sup>3</sup>If the interacted variable is a dummy,  $\bar{x}$  is set equal to 1 to compute the marginal effect for the respective subsample indicated through the dummy.

where  $\beta_1$  is the estimate of the robot coefficient for the linear term,  $\beta_2$  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. In contrast to Zigraiova and Havranek (2016), we also consider the covariance between the estimated coefficients in the formula for  $SE(\beta)$  as the original data for the respective study (Sequeira et al., 2021) were available. The resulting coefficient of robotization after computing the average marginal effect by equation (2) or (3) is subsequently transformed into the *PCC* in line with equation (1).

The *PCCs* are relatively robust to imprecise measures of the degrees of freedom (Stanley and Doucouliagos, 2012, p.156). Often, the degrees of freedom must be approximated if not all relevant information is provided within the primary studies. While calculating the degrees of freedom for default or heteroskedasticity-robust standard errors is straightforward<sup>4</sup>, it is more difficult in the context of clustered standard errors<sup>5</sup>. It is sometimes not clear whether the authors of a primary study cluster at the intersection of two or more dimensions (one-way clustering) or in multiple dimensions (multiway clustering). The latter would require that the degrees of freedom are equal to the cluster-dimension with the smallest number of clusters. This yields much less degrees of freedom than in the case of one-way clustering. We follow here the guidelines provided by Cameron and Miller (2015, p. 336): If authors write that they cluster standard errors in dimension A and dimension B (and dimension C), two(three-)-way clustering at the intersection of the two dimensions is assumed (i.e. *hyphen* instead of *and* in the cluster description).

Given the PCCs derived from the primary studies, we could now estimate the overall mean effect of robots on wages by applying a simple OLS regression with a constant only:

$$PCC_{is} = \mu + \epsilon_{is} \tag{4}$$

where, in the absence of any bias,  $\mu$  represents the true effect and the error term  $\epsilon_{is} \sim N(0, \sigma_{\epsilon_{is}})$ describes primary sampling error. However,  $\mu$  will not be efficient as the error term in equation (4) is a function of the sampling error and therefore heteroskedastic, i.e. not independently and identically distributed (Stanley and Doucouliagos, 2012, p. 61). More precise estimates with smaller standard errors are better estimates of the true effect. This is not reflected in equation (4) where all *PCCs* enter with the same weight. MRA therefore, involves weighted least squares (WLS) to receive unbiased, consistent and efficient estimates:

$$\frac{PCC_{is}}{\omega_{is}} = \frac{\mu}{\omega_{is}} + \frac{\epsilon_{is}}{\omega_{is}} \tag{5}$$

where  $\omega_{is}$  is the weight of estimate *i* of study *s*. The weight should reflect the quality of the estimate. We use precision as quality measure of the reported coefficients. For each  $PCC_{is}$  we

 $<sup>{}^{4}</sup>df = N - k - 1$ , where N is the number of observations and k is the number of regressors and/or fixed effects included in the estimation.

<sup>&</sup>lt;sup>5</sup>One-way clustering: df = G - 1; multiway clustering:  $df = G_{min}$ , following Cameron and Miller (2015).

calculate a standard error of our effect size according to:

$$SE_{PCC_{is}} = \sqrt{\frac{1 - PCC_{is}^2}{df_{is}}} = \frac{PCC_{is}}{t_{is}} \tag{6}$$

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

$$FEE1 = \frac{1}{SE_{PCC_{is}}}\tag{7}$$

which is known as the fixed effect estimator (FEE) in MRA (Stanley and Doucouliagos, 2012, p. 46).<sup>6</sup> The idea is that the more precise an estimator is as measured by a small standard error, the smaller can be the estimated effect size and vice versa in order to retain a statistically significant coefficient. Therefore, we attribute more weight to the *PCCs* with a higher precision and a lower standard error. Other approaches to measure quality could be the journal's ranking or the number of citations of the primary study. Both quality measures are not feasible for our meta-analysis since robot adoption is such a new research field that many papers have not been published in a peer-reviewed journal and may not have reached a wider audience as reflected in the number of citations yet. If we relied on papers published in journals only, we would lose 31 primary studies and more than half of our 2143 observations. Using the number of citations as a quality measure would give unusual weight to older studies regardless of their actual quality. Our "youngest" primary studies are from September 2022 and can, naturally, not have been cited by many other studies.

In addition to mere precision weighting, we also use weights adjusting for the number of estimates per study:

$$FEE2 = \frac{1}{SE_{PCC_{is}} \cdot \sqrt{n_{i \in s}}}$$
(8)

where n is the number of estimates of study s. We thereby give equal weight to studies rather than estimates (Duan et al., 2020). This helps us to uncover undue effects a study with many estimates has in comparison to a study with few estimates. We have opted to include all estimates provided by a primary study if they fit our selection criteria (industrial robots and wages) and where we could extract all the relevant information needed to construct our *PCC*. In metaanalysis it is often argued that only one estimate per study should enter the meta regression to avoid study dependence. That could be the best estimate per study as preferred by the authors of the primary study or by the authors of the meta-analysis. In both cases, results could be severely biased towards e.g. the most precise estimate or the one with the most favorable sign. Alternatively, using an average effect size per study would preclude potential within-study variation.<sup>7</sup> Selecting only one estimate per study is also misleading if studies provide estimates for several subsamples. We thus include all estimates provided within the primary studies to avoid selection bias. This has the advantage that we can include more observations and have

<sup>&</sup>lt;sup>6</sup>This fixed effect estimator should not be mistaken for a fixed effect estimation in an econometric sense. <sup>7</sup>See Stanley and Doucouliagos (2012, p. 32-33) for a comprehensive discussion of this.

more variation. It, however, raises the question of study and author dependency. Estimates from one primary study usually draw from the same data and employ similar econometric strategies. This might undermine the basic assumption of all regression analysis that the error terms are independently and identically distributed. However, we are very confident that our moderator matrix containing data characteristics and differences in estimation models helps us to overcome the problems of study dependence. Additionally, we always use robust standard errors clustered at the study-level to address within-study dependence (Stanley and Doucouliagos, 2012, p. 71).

The assumption underlying the fixed effect estimators using FEE weights as specified in equation (7) and (8) is that all estimates are drawn from one population and thus, measure one, true effect of robots on wages. The primary estimates  $(PCC_{is})$  sampled from studies i = 1, ..., N are assumed to deviate from the true effect solely due to sampling error. However, in reality, primary studies use different populations and different econometric methods. Therefore, the assumption of a uniform effect can be incorrect. Random effects estimators (REE), in contrast, explicitly allow for heterogeneity of primary estimates beyond pure sampling error. Accordingly, equation 4 must be extended by a random component  $\theta_{is} \sim N(0, \tau^2)$  allowing the true effects to vary between studies (Harbord and J. P. Higgins, 2008):

$$PCC_{is} = \mu + \theta_{is} + \epsilon_{is} \tag{9}$$

REE assume additive error variances and the weights in equation (7) and (8) are adjusted as to account for between-study heterogeneity:

$$REE1 = \frac{1}{\sqrt{SE_{PCC_{is}}^2 + \tau^2}} \tag{10}$$

$$REE2 = \frac{1}{\sqrt{SE_{PCC_is}^2 + \tau^2} \cdot \sqrt{n_{i\in s}}}$$
(11)

where  $\tau^2$  is a constant parameter capturing the differences in the mean true effect (Duan et al., 2020). While information on  $SE_{PCC}$  can be retrieved from primary studies,  $\tau^2$  must be estimated in a first step of the WLS meta-regression (Feld and Heckemeyer, 2011).<sup>8</sup> A larger  $\tau^2$  indicates a greater between-study heterogeneity. So, including  $\tau^2$  in the weighting scheme, reduces the relative importance of the precision weight and creates a more uniform weighting scheme. It reflects the circumstance that a low standard error alone rather captures the quality of the estimate within a primary study than across studies.

Implementing four different weighting schemes requires some judgement of what we deem preferable. In light of the highly right-skewed distribution of the number of estimates per study (range from 1 to 394, with a mean of 40 and a median of 16), it is necessary to adjust the weighting scheme for the number of estimates per study to avoid an undue influence of a few studies with a high number of estimates. This was done in FEE2 and REE2. To choose whether FEE

<sup>&</sup>lt;sup>8</sup>This is done using the restricted maximum-likelihood (REML) estimator (Raudenbush, 2009).

or REE is the suitable estimator, one can consult Cochran's Q-test for the null-hypothesis of between-study homogeneity, i.e.  $\tau^2 = 0$  (Feld and Heckemeyer, 2011, Stanley and Doucouliagos, 2012, p. 48-49). In all regressions we will present in the following sections, the Q-test always rejects the null-hypothesis with a statistical significance well below the 1%-level.<sup>9</sup> Moreover, the heterogeneity statistic  $I^2$  measuring the share of observed variability which reflects true differences in the effect size rather than sampling error is always above 99%, indicating a high degree of heterogeneity according to J. P. T. Higgins et al. (2003). Also from a practical perspective it appears reasonable to assume that studies (or some estimations within studies) at different levels of analysis and focusing on different (sub-)populations measure different true effects.<sup>10</sup> We therefore choose REE2 as our preferred weighting scheme.

# 5 Overall mean effect and publication bias

Applying simple vote counting based on the conventional 5%-significance level, we find that 17.9% of the estimates show a positive and statistically significant effect, 38.6% report a significantly negative effect, while 43.5% are statistically insignificant. However, as documented by Stanley and Doucouliagos (2012, pp. 43), vote counting can be very misleading. We, therefore, present further summary statistics for our measure of effect size and formally test for publication selection bias in the literature on the wage effects of industrial robots.

Table 2 reports summary measures for the PCCs computed for all estimates collected from primary studies, while Figure 1 illustrates the frequency distribution of PCCs. None of the estimators for the overall mean effect of industrial robots on wages from equations (4) and (5) shown in table 2 is statistically significant. With regard to economic significance, only the arithmetic mean and the REE1 estimator indicate a small negative effect. The unweighted average of all PCCs is equal to -0.096. A simple mean of partial correlation coefficients, however, does not consider the estimates' precision. It is more appropriate to apply the fixed effects and random effects estimators (weights FEE1, FEE2, and REE1, REE2, respectively) as given by equations (7), (8), (10), and (11). Both FEE1 and FEE2 suggest an effect of industrial robots on wages that is close to zero. This is in line with the highest frequencies of PCCs being centered around zero as shown in Figure 1. A decline in the mean effect caused by assigning larger

<sup>&</sup>lt;sup>9</sup>In the multivariate MRA where we try to systematically explain the driving forces of heterogeneity,  $\tau^2$  measures the remaining unexplained heterogeneity after including our moderator matrix, i.e. betweenstudy variability not explained by the moderator variables (Feld and Heckemeyer, 2011). Although, the value of  $\tau^2$  considerably decreases by more than 90% once the moderator matrix is introduced, the Q-test still rejects the null of homogeneity.

<sup>&</sup>lt;sup>10</sup>However, the meta-analysis literature has not yet achieved consensus on what estimator works best in practice (Reed, 2015; Doucouliagos and Paldam, 2013; Stanley and Doucouliagos, 2015; Doi et al., 2017). There are particular concerns that REE are biased in the presence of publication selection as the random effects might be correlated with the standard error (Stanley and Doucouliagos, 2012, p. 82-84). As we obtain little evidence for publication bias, we deem this concern as negligible.

Measure	Obs.	Estimate	SE	95% CI
Mean	2143	-0.09637	0.09590	[-0.28880, 0.09607]
FEE1	2143	0.00008	0.00214	[-0.00421, 0.00437]
FEE2	2143	-0.00467	0.00491	[-0.01452,  0.00518]
REE1	2143	-0.10052	0.08871	[-0.27854,  0.07750]
REE2	2143	-0.01425	0.03567	[-0.08582, 0.05732]

Note: Standard errors are clustered at study level. Q-test: Q = 43779.38, p-value = 0.0000;  $\tau^2 = 0.0869$ ;  $I^2 = 99.98\%$ 

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

weights to more precise studies, may indicate a publication selection bias. From a technical point of view, REE1 and REE2 are the appropriate estimators if there is between-study heterogeneity (which is confirmed by the Q-test). The REE1 estimator is very close to the unweighted average, delivering an overall mean effect of -0.101. As can be seen from equation (10), if the betweenstudy heterogeneity, i.e.  $\tau^2$ , is large compared to  $SE_{PCC}^2$ , the weighting scheme across primary estimates is approximately uniform and converges to an OLS estimation. Although REE1 is the appropriate weighted average under between-study heterogeneity, publication selection might reverse this convention in favor of FEE1 (Stanley and Doucouliagos, 2012, p. 47). Anyway, once the weighting scheme is adjusted for the number of estimates per study, the REE2 estimator is again close to zero. Moreover, no reliable inference can be drawn from these summary measures. In case of publication selection, all averages, weighted or not, can be biased. Furthermore, under systematic heterogeneity, any measure of average effect size may obscure the true characteristics of the economic phenomenon under research. To capture systematic heterogeneity, we need to include our moderator matrix.



Figure 1: Histogram of partial correlation coefficients.

Publication selection refers to the process of choosing research papers or reporting only a subset of estimates within a study, according to statistical significance or compliance with conventional theories. Many existing meta-analyses in the field of economics provide evidence for publication bias (e.g. Havranek and Irsova, 2011; Gunby et al., 2017). Therefore, it is essential to formally test and correct for publication selection also in the literature on the wage effects of industrial robots. For instance, there is a wide-spread fear of job replacement by automation technologies in the society (c.f. Abeliansky and Beulmann, 2021; Cheng et al., 2019) and also well-published theoretical and empirical evidence in that direction in the academic literature (e.g. Acemoglu and Restrepo, 2020; DeCanio, 2016). In consequence, researchers might selectively report negative effects of industrial robots on wages because they expect such findings to be more likely to be published.

A visual inspection of a potential publication bias is provided by means of a so-called funnel plot in Figure 2: a scatterplot of the study-specific effect sizes (PCCs, x-axis) against study precision (inverse of the standard error of PCCs, y-axis). In the absence of publication bias (and between-study heterogeneity), the primary estimates should be distributed symmetrically about the overall effect size because the sampling error is random. The typical funnel shape arises from the measure of precision on the y-axis. Estimates at the bottom of the graph exhibit larger standard errors and, thus, are widely dispersed. More precise estimates at the top of the graph, in contrast, are more compactly distributed. As our preferred estimators for the overall mean effect, FEE2 and REE2, are close to zero and statistically insignificant, one should expect that the primary estimates are symmetrically distributed around zero. The funnel plot shown in Figure 2 is quite symmetric. If any asymmetry is visible, then there are some more imprecisely estimated PCCs with a negative sign. The most precise estimates show an effect size close to zero, in line with FEE2 and REE2 in Table 2.



Figure 2: Funnel plot of partial correlation coefficients (N = 2143, from 53 studies).

	(1)	(2)	(3)	(4)
	WLS FEE1	WLS FEE2	WLS REE1	WLS REE2
Publication bias $(\beta_1)$	-1.4715*	.459	2871	.1937
	(.8672)	(.696)	(1.0375)	(.4784)
True effect $(\beta_0)$	.0017	0053	0773	0297
	(.0014)	(.005)	(.0555)	(.0381)
Observations	2143	2143	2143	2143
R-squared	.0962	.0045	.0038	.0016

Table 3: Funnel-asymmetry test for publication bias (FAT-PET)

Notes: Standard errors clustered at study level are in parentheses. The weights used in the WLS estimation are indicated in the column header. Q-test: Q = 39565.80, p-value = 0.0000;  $\tau^2 = .08554$ ;  $I^2 = 99.98\%$ ; \*\*\*p < .01,\*\*p < .05, \*p < .1

When publication selection bias is present, the reported effect size is correlated with its standard error. To formally test for publication bias, we perform the so-called funnel-asymmetry test (FAT), which means estimating the following regression:

$$PCC_{is} = \beta_0 + \beta_1 SE_{PCC_{is}} + \epsilon_{is} \tag{12}$$

where PCC and  $SE_{PCC}$  are the partial correlation coefficient and its associated standard error of estimate i in study s as previously defined, respectively, and  $\epsilon_{is}$  is the regression error term. Coefficient  $\beta_0$  represents the true empirical effect corrected for potential publication selection (precision-effect test, PET). Coefficient  $\beta_1$  indicates the direction and magnitude of publication bias (FAT). Equation (12) is heteroskedastic by construction, since the explanatory variable is estimated as the standard deviation of the dependent variable. Therefore, it is estimated by WLS instead of OLS using the weights given in equations (7), (8), (10), and (11). To account for dependence among estimates stemming from the same primary study, we cluster standard errors at study level. Table 3 presents the results from FAT and PET. The FAT provides only modest evidence for publication selection. Solely weighting scheme FEE1 indicates a negative publication bias with a substantial magnitude of selectivity according to the practical guidance provided by Doucouliagos and Stanley (2013). In contrast, all other specifications do not point to any significant publication bias. The underlying, "true", effect corrected for publication bias shows a lack of statistical and economic significance in all specifications (PET). This suggests that industrial robotization has so far had no visible overall effect on wages for the total population.

# 6 Assessing heterogeneity in a multivariate MRA

The univariate regression presented above (Eq. 12) may give biased estimates if important variables that explain heterogeneity between estimates are omitted. For instance, a specific

method choice made by the authors of primary studies may affect both the standard error and the reported point estimate in the same direction. Then the standard error variable will be correlated with the error term, resulting in a biased estimate of  $\beta_1$  (Havranek, 2015). We therefore add a matrix of moderator variables to Equation (12) and estimate the following model to identify the drivers of heterogeneity in the estimated effects of industrial robots on wages:

$$PCC_{is} = \beta_0 + \beta_1 SE_{PCC_{is}} + \sum \beta_k Z_{kis} + \epsilon_{is}$$
, for estimate *i* in study *s* (13)

where k represents the number of moderator variables,  $\beta_k$  is the coefficient of the corresponding moderator variable, and  $\epsilon_{is}$  denotes the error term. As we face the challenge of meta-analyzing a set of primary estimates that vary along many characteristics of research design, our moderator matrix is composed of 85 variables to account for as many of these characteristics as possible and alleviate omitted variable bias concerns. Table C1 in the appendix shows that the majority of variables displays a statistically significant effect at least in one of our WLS specifications. We attribute this to the great heterogeneity of the primary studies. It is common in MRA to reduce the number of moderator variables by means of a variable selection procedure to mitigate multicollinearity (Stanley and Doucouliagos, 2012, p. 91). We therefore employ a backwards stepwise regression procedure that sequentially chooses the "best" set of moderator variables by minimizing the Bayesian information criterion (BIC) (Duan et al., 2020; Lindsey, 2014). Reassuringly, within a certain weighting scheme, coefficients estimated for a given moderator in the specifications with the full set of moderators and with selected moderators always exhibit the same sign and a similar magnitude. Hence, we conclude that the selected model specifications do not suffer from omitted variable bias while preventing multicollinearity from obscuring the results. As recommended by Stanley and Doucouliagos (2012, p. 103-104), we focus on research characteristics with consistent findings across alternative MRA model specifications. Tables 4 and 5 show the results for those moderator variables that are selected in every weighting scheme and always have a statistically significant effect that is consistent across all weighting schemes. Table 4 presents the coefficients of these moderators in the regression model with the selected set of moderator variables, while Table 5 shows their coefficients in the regression model with the full set of moderator variables. The full table including all moderator variables can be found in the appendix (Table C1).

Also after running our multivariate meta-regression, the evidence for a negative publication bias remains very limited. Only weighting scheme FEE1 robustly indicates a negative publication bias with a substantial degree of selectivity according to the classification provided by Doucouliagos and Stanley (2013). In weighting scheme REE1, the standard error is not even selected by the backwards stepwise regression procedure. Our preferred weighting scheme REE2 does also not point to any significant publication selection. In the following thematically organised subsections, we will discuss the findings for our moderator variables.

	(1)	(2)	(3)	(4)
	WLS FEE1	WLS FEE2	WLS REE1	WLS REE2
se_pcc	-1.927***	-1.2066**		4808
	(.5188)	(.4875)		(.3433)
$\operatorname{country\_lev}$	737***	7957***	8768***	-1.1071***
	(.1946)	(.1819)	(.119)	(.1067)
c_num	.0361***	.0286***	.0269***	.0296***
	(.006)	(.0057)	(.0047)	(.0033)
tenure	.302**	.644***	.1351*	.2722***
	(.1401)	(.1181)	(.0752)	(.081)
$trade\_control$	1068***	1525***	0606***	1061***
	(.0342)	(.0236)	(.02)	(.0278)
$ict\_control$	$.0617^{**}$	.0885***	.0681***	.084***
	(.0269)	(.0319)	(.0178)	(.0291)
$demograph\_control$	.2531***	.2169***	.0912***	.143***
	(.0604)	(.0585)	(.0316)	(.042)
stay_sec	.0413***	.0521***	.0585***	$.1671^{***}$
	(.0011)	(.0082)	(.0176)	(.0545)
quartile_4	1581**	1194**	$1784^{***}$	2103***
	(.0618)	(.0563)	(.047)	(.0505)
manuf	0303**	0129***	$1456^{***}$	154***
	(.0144)	(.0045)	(.0369)	(.0309)
non_manuf	.241**	.1371**	.2055**	$.1176^{*}$
	(.0974)	(.0593)	(.0864)	(.0646)
weighted	1256***	1088***	0621***	1263***
	(.029)	(.0282)	(.021)	(.0246)
long_dif	.227***	.2032***	$.1695^{***}$	$.1919^{***}$
	(.0452)	(.0424)	(.0515)	(.0536)
journal	.0643*	.1016***	.0754**	$.1622^{***}$
	(.0321)	(.0224)	(.0313)	(.0305)
_cons	0263	3212***	2818***	5393***
	(.0927)	(.062)	(.0882)	(.0638)
Observations	2143	2143	2143	2143
R-squared	.6675	.8758	.7764	.7641

Table 4: Meta-regression analysis - selected specification

Note: Standard errors clustered at study level are in parentheses. The weights used in the WLS estimation are indicated in the column header. The moderator variables were selected by means of a backwards stepwise regression algorithm which sequentially selects the explanatory variables that minimize the BIC information criterion. Only those moderator variables are shown that exhibit consistent and statistically significant results across all weighting schemes. \*\*\*p < .01, \*\*p < .05, \*p < .1

	(1)	(2)	(3)	(4)
	WLS FEE1	WLS FEE2	WLS REE1	WLS REE2
se_pcc	-1.7819***	-1.0414	.3503	3916
	(.5426)	(.6237)	(.4485)	(.5678)
$\operatorname{country\_lev}$	6046***	7338***	9165***	-1.1702***
	(.1893)	(.2262)	(.164)	(.1332)
c_num	.034***	.0263***	.0209***	.0315***
	(.0064)	(.0077)	(.0075)	(.0045)
tenure	.3315*	.6407***	.1156	.3996***
	(.1727)	(.1447)	(.1044)	(.1415)
$trade\_control$	0939**	1511***	0866**	1081**
	(.044)	(.0458)	(.0349)	(.0416)
$ict\_control$	.0628*	.0901*	.0518*	.0823**
	(.0346)	(.0457)	(.0289)	(.037)
$demograph\_control$	.2847***	.2298***	.0566	.1019**
	(.1004)	(.0731)	(.0631)	(.0486)
stay_sec	.0449***	.0573***	.0672**	.1891**
	(.0022)	(.0163)	(.0284)	(.0763)
quartile_4	1362	1784**	1938**	2207**
	(.0918)	(.0865)	(.0741)	(.0965)
manuf	0289**	0126***	1315***	1731***
	(.0139)	(.0043)	(.0409)	(.042)
non_manuf	.2327**	.1615**	.2169**	.1051
	(.098)	(.0663)	(.0884)	(.0711)
weighted	1271***	1128***	0731***	1212***
	(.0369)	(.0401)	(.0249)	(.0265)
long_dif	.2287***	.2233***	.1302**	.1561**
	(.0536)	(.0577)	(.0615)	(.0641)
journal	.0619*	.1293***	.0571	.1703***
	(.0322)	(.0339)	(.0504)	(.0492)
_cons	0337	3296**	1987	6093***
	(.1946)	(.1555)	(.162)	(.141)
Observations	2143	2143	2143	2143
R-squared	.677	.8801	.7853	.7815

Table 5: Meta-regression analysis - full specification

Note: Standard errors clustered at study level are in parentheses. The weights used in the WLS estimation are indicated in the column header. Only the coefficients of the selected moderators from Table 4 are shown but estimated in the model specification including all 85 moderator variables. \*\*\*p < .01, \*\*p < .05, \*p < .1

#### 6.1 Data characteristics

Two of the moderator variables related to geography, time, and level of analysis exhibit robust and consistent results (Tables 4 and 5). First, estimations at country level ( $country\_lev = 1$ ) find clearly more negative wage effects of industrial robots compared to estimations at industry level. This suggests aggregate macroeconomic net effects resulting from all the direct and indirect effects at lower levels of aggregation could be negative. Second, adding countries to the estimation sample ( $c\_num$ ) leads to more positive findings in the primary literature. Multi-country samples move closer to a global economic perspective, possibly capturing trade effects of robotization and benefits from further specialization. Moreover, although less robust, our findings suggest that estimations at firm level ( $firm\_lev = 1$ ) tend to produce more positive effects compared to analyses at industry level, whereas estimations at the level of occupations seem to result in more negative primary estimates (Table C1).

With regard to geographic disparities in the labor market effect of industrial robots, we find some evidence that the estimated effects in Japan and in developing/ emerging countries are more positive, both statistically and economically significant. These are interesting findings. Since Japan is a frontrunner in industrial robotization (Adachi et al., 2022), labor demand reductions due to robotization are likely to be outweighed by larger productivity effects. We believe that the higher robot penetration in Japan and the greater market maturity might have contributed to higher productivities. From the perspective of development economics, it is reassuring that the wage effects of robots are more beneficial in developing and emerging countries compared to the rest of the world. This might be explained by a larger scope for productivity increases through robotization in less developed regions compared to industrialized countries. Moreover, we do not find a clear evidence for disparities between US and European labor markets. These results hold only for the full sample. Results are different when considering subsamples see 7.2 and robustness checks 8.1. In the full sample, the bivariate correlations between PCCs and our moderator variables "us" (corr = -0.65) and "europe" (corr = +0.47) suggest strong geographic differences. These correlations are, however, partially absorbed by other characteristics of research design in a multivariate meta-regression analysis. Our preferred weighting scheme REE2 suggests more positive effects in European countries. For the US, only FEE1 and REE1 suggest a negative impact, which however vanishes once the weights are adjusted for the number of estimations per study. This observation can be attributed to the high number of negative primary estimates for US labor markets stemming from few studies.

With respect to the time period under research, we find some evidence that studies considering more recent time periods (with a mid-point after year 2007) tend to find a more positive effect of robots on wages (c.f. significantly negative coefficient of period\_2007 in Table C1). This is interesting as industrial automation proceeded or even accelerated in the last decade, such that any effect of labor substitution should have become more visible in recent years. Using 2010 instead of 2007 as cutoff year leaves our results qualitatively unchanged.<sup>11</sup>

 $<sup>^{11}\</sup>mathrm{MRA}$  results with moderator variable  $period\_2010$  instead of  $period\_2007$  are available upon request.

None of our moderator variables capturing the construction of the robot measure shows up in Tables 4 and 5 as we do not find robust evidence for a systematic influence of different robot measures on the estimated effect. However, our preferred weighting scheme REE2 suggests that primary estimations using absolute robot stocks, a robot dummy, a monetary value of robot capital, or other robot measures (e.g. patent data) as explanatory variable tend to report more positive effect sizes than primary estimations using a robot density (Table C1). While robot stock is usually taken from IFR data, the other measures rely usually non-IFR data. As all coefficients are positive in our preferred specification, we conclude that data dependence is not an issue here. Furthermore, applying the PIM for the construction of the robot measure seems to positively influence the estimated effect.

Just as for the construction of the robot measure, we also do not find robust evidence for varying definitions of the wage measure as driver of heterogeneity. While weighting scheme FEE2 suggests that using income as dependent variable is associated with more negative findings, our preferred weighting scheme REE2 indicates that different wage measures do not explain the heterogeneous findings in the primary literature at all (Table C1).

As might be expected, alternative data constructions within studies, typically used as robustness checks in the primary literature, are also not driving heterogeneous findings: *alter\_data* is never selected by our backwards stepwise regression procedure and the point estimates in the full specification are always close to zero. Excluding estimates where *alter\_data* = 1 from our analysis, leaves the findings presented in sections 5 and 6 qualitatively unchanged.<sup>12</sup>

#### 6.2 Control variables

Four control variables show a robust and systematic influence on the estimated effects (Tables 4 and 5). While control variables for ICT capital, demographic developments, and tenure positively affect the estimated outcomes, controlling for trade flows reduces the estimated effect. ICT capital and trade exposure are widespread control variables in the primary literature and seem to be important for isolating the effect industrial robots have on wages. For all other control variables, the evidence for a systematic influence on the estimated effect size remains rather small. Our preferred weighting scheme REE2 suggests that also control variables related to education, firm size, unionization, and other capital positively affect the estimated outcome, whereas controlling for productivity/ value added and migration has a negative impact on the estimated effect. It is worth noting that the inclusion of some control variables depends on the level of analysis as those control variables are not available at higher levels of aggregation. For instance, only estimations at individual level did control for tenure. Controls for firm size were only used at the level of firms, demographic groups, and individuals. This dependence complicates the detection of consistent MRA findings for the level of analysis as some of the variation in this research dimension can also be attributed to the inclusion of different control variables.

<sup>&</sup>lt;sup>12</sup>This reduces the number of primary estimates to 1,829. Results are available upon request.

#### 6.3 Subsamples related to skill-levels

Many primary studies report estimations for subgroups according to skill level, either regarding education levels or the requirements of occupational tasks. Interestingly, despite the widespread view of skill-biased technological change in the economic literature on automation (e.g. Fierro et al., 2022; Prettner and Strulik, 2020; Lankisch et al., 2019), none of our moderator variables capturing these research dimensions have robust results. We only find few indications that industrial automation the wage of workers with low educational attainment (Table C1). Workers in high-skilled occupations benefit from robot adoption while workers in medium- to low skilled occupations do not. There is only one specification with REE1 weights where we observe a more negative wage impact for medium- to low-skilled occupations. So our meta-regression results for educational and occupational skill groups do not support the job/wage polarization hypothesis of Acemoglu and Autor (2011) and Autor and Dorn (2013) as we never see more advantageous effects for high- and low-skilled individuals compared to the total population. Generally, occupational skill levels are more relevant than educational skills on the employee level for explaining heterogeneous wage effects.

#### 6.4 Subsamples related to labor mobility

Some primary studies explicitly run subsample regressions considering only workers which either experienced a change in employment along the dimensions of employer/firm, sector, region, or occupation or not. Only one of the moderator variables capturing this research characteristic shows a clear pattern for explaining the heterogeneity of primary estimations: employees who remain in an automating sector benefit from more positive wage developments compared to the total population (s.  $stay\_sec$  in Tables 4 and 5). This might be attributed to productivity increases in the respective sector after adopting robots. In contrast to that, we find some evidence that workers staying in the same occupation are more negatively affected ( $stay\_occ$  in Table C1). This suggests occupational mobility might be rewarded and serves as a mechanism to alleviate any displacement effects of robots. Surprisingly, estimations considering only workers with temporary contracts seem to result in more positive effect sizes (s.  $temp\_contract$  in Table C1). It should be treated with caution, as it is based on a low number of primary estimates originating from only two studies.

#### 6.5 Subsamples related to demographic and income groups

The evidence for demographic groups as drivers of heterogeneity remains very limited. None of the moderator variables related to gender and age groups exhibit robust findings. There is only some evidence that male employees are more negatively affected (Table C1). This could be explained by men being more likely to work in occupations with a high degree of physical work which are more prone to automation. Differential effects across certain age groups seem to be no explanation for the heterogeneous empirical findings in the primary literature.

Surprisingly, primary estimations focusing only on the top quartile of the income distribution robustly show more negative outcomes compared to the total population (s. quartile\_4 in Tables 4 and 5). This finding is in conflict with the skill-biased technological change hypothesis. If high-skilled employees are favored by industrial robots, one would expect more positive wage effects in the top quartile of the income distribution as high-skilled workers are typically located in the upper part of the income distribution. Although this finding might alleviate concerns that automation drives inequality, some caution is required. As can be seen in Table C1, we also find evidence that primary estimations for the two bottom quartiles exhibit more negative effect sizes in a similar order of magnitude.

#### 6.6 Subsamples related to robot exposure

It is evident from Tables 4 and 5 that the wage effects of industrial robots are more negative in the manufacturing sector, whereas more positive effects are found in the non-manufacturing sector. This can be explained by the fact that robotization so far has mainly taken place in manufacturing industries such that any labor-substituting effects of robots should have been focused on this sector. Simultaneously, industrial robotization might have led to new labor demand in the service sector, especially in services related to automation (e.g. developing software used in robotic systems or consultancy for robot adoption). This result is in line with a reallocation of labor from manufacturing activities to services, as also documented by Mann and Püttmann (2023) for a broader scope of automation technologies. Consequently, advancing robotization may boost structural change as regards accelerating the ongoing servitization of economies (Breemersch et al., 2019). Frequently implemented robustness checks in the primary literature are to exclude outliers in terms of industries (in particular, the automotive industry) or regions with the highest exposure to robots from the estimation sample. This appears to have no systematic effect on the reported effect sizes (s. *excl\_automotive* and *outlier* in Table C1).

#### 6.7 Estimation technique and publication status

With respect to different estimation techniques as drivers of heterogeneity, we find strong evidence that weighted estimations produce more negative effects, while regression model specifications in long differences are associated with more positive findings (s. *weighted* and *long\_dif* in Tables 4 and 5). We also find quite robust evidence for more positive results in specifications with stacked differences (s. *stacked\_dif* in Table C1), which are often implemented as a robustness check for the long difference specifications. Especially regression models in long differences are to be understood as measuring the long-term structural effect of robots. Therefore, more positive outcomes for those specifications are a reassuring message for the long-run effect of progressing automation.

Moreover, our preferred weighting scheme REE2 suggests that specifications where both wage and robot measure are logarithmically transformed come a long with more positive effects. With respect to the inclusion of fixed effects, we obtain some evidence that entity fixed effects exert a positive influence on the estimated effect, while industry and entity-year fixed effects are negatively associated with the estimated effect (Table C1). Since the clustering of standard errors matters for computing the degrees of freedom used in the formula for the *PCCs*, it is reassuring that primary estimates with non-clustered standard errors are not associated with systematically different effect sizes (s. *secl\_no* in Table C1). Also, different regression approaches than OLS (i.e. IV, GMM, quantile regression) do not explain the heterogeneity of the findings in the primary literature.

Effects reported in peer-reviewed journals are systematically more positive than effects reported in working papers. This creates further doubts as to a potentially negative publication bias. Instead, already published articles report more positive effect sizes than working papers and dissertations. However, one needs to take into account that the labor market impact of industrial robots is a young field of research with a high share of articles that have yet to be published. Thus, the evidence for more positive findings in journals can quickly change.

#### 6.8 Overall mean effect in the multivariate MRA framework

To scrutinize the overall effect of robots on wages in our multivariate MRA, one needs to look at the intercept (*\_cons*). The constant 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. As an estimation with zero countries and zero sample years does not exist, we must adjust the constant for the average number of countries (=3.43) and years (=6.83) used among the primary estimates included in our meta-analysis. The reference group is made up of estimates from OLS, at industry level, in a global multi-country sample (or Canada), using a robot density as robot measure, non-hourly wages as wage measure, and employing no fixed effects. Consulting our preferred weighting scheme REE2 in the selected specification of Table 4 gives a large negative mean effect of -0.378 for the reference group. However, this effect must be interpreted with caution and most econometricians would likely deem it not representative because it is based on estimates from estimations without any control variables. If we consider the coefficients of the selected dummy variables indicating the inclusion of control variables for tenure, trade, ICT, and demography, the mean effect changes to 0.015, again close to zero. If we additionally account for heterogeneity between economic sectors, the effect becomes -0.139 for the manufacturing sector but +0.132 for the non-manufacturing sector. Anyway, defining a representative effect is not possible without some judgement by the meta-analyst of what is viewed as "best practice" research (Stanley and Doucouliagos, 2012, p. 93). For instance, if we expected researchers to apply weighted estimations specified in long differences to capture long-term effects, the overall mean effect would become 0.08, i.e. a small positive effect according to the classification of Doucouliagos (2011).

# 7 Treatment of endogeneity

Endogeneity, reverse causation, and simultaneity are important issues in empirical research. In our context, high wages could drive investment into industrial robots. Several articles, therefore, investigate the effect of wages on robot adoption (e.g. Fernández-Macías et al., 2021; Fan et al., 2021; Samwer and Chen, 2020). Further endogeneity concerns in the primary literature are typically that unobserved shocks affect both robot adoption and labor demand, e.g. a local recession or industry-specific institutional changes such as wage pressure from unionization (Acemoglu and Restrepo, 2020); or that certain industries, regions or firms select into robot adoption and fundamentally differ from non-adopting industries/ regions/ firms, thereby following different trends in wage evolution even absent automation (Koch et al., 2021). Although our moderator matrix used in section 6 already addresses biases from omitted variables (dummies for inclusion of control variables in primary estimations) and measurement deviations (dummies for different wage and robot measures) as well as the treatment of endogeneity (moderator iv). this section presents MRA results for the subset of primary estimates based on an IV/2SLSapproach. This allows to focus on the drivers of heterogeneity among estimates that were (to some extent) cleaned from endogeneity. The most frequently applied IV approach in the primary literature is to instrument robot adoption in the country under research by robot adoption in other, comparable countries (e.g. Acemoglu and Restrepo, 2020; Dottori, 2021; Dauth et al., 2021; Ge and Zhou, 2020). The IV-subsample comprises 36 studies with 1092 estimates.

#### 7.1 Overall mean effect and publication bias in the IV-subsample

As shown in table 6, the *PCC* summary measures in the IV-subsample are very close to zero. The FEE1 and FEE2 estimators now indicate a statistically significant mean effect, which however lacks economic significance. The unweighted mean and the REE1 estimator are shifted towards zero compared to the full sample, while the REE2 estimator is practically unchanged. Eyeballing the funnelplot for the IV-subsample shown in Figure 3 suggests a very symmetric distribution of primary estimates around zero. This is mirrored in the absence of any evidence for publication selection in the FAT for the IV-subsample (s. Table 7). The evidence for a slightly negative overall true effect (PET) of robots on wages becomes somewhat stronger as a statistically significant effect is found in all weighting schemes except for REE2. The magnitude of this effect, however, is always well below the threshold for a small effect as defined by Doucouliagos (2011).

Measure	Obs.	Estimate	SE	95% CI
Mean	1092	0.00584	0.03927	[-0.07387, 0.08556]
FEE1	1092	-0.02078	0.00645	[-0.03387, -0.00770]
FEE2	1092	-0.01133	0.00107	[-0.01350, -0.00916]
REE1	1092	-0.00574	0.03054	[-0.06774,  0.05627]
REE2	1092	-0.01471	0.04102	[-0.09798,  0.06857]

Table 6: Summary measures of the PCCs in the IV-subsample

Note: Standard errors are clustered at study level. Q-test:  $Q=13058.71,\,p\text{-value}=0.0000;\,\tau^2=0.0167;\,I^2=99.41\%$ 



Figure 3: Funnel plot of partial correlation coefficients (N = 1092, from 36 studies).

	(1)	(2)	(3)	(4)
	WLS FEE1	WLS FEE2	WLS REE1	WLS REE2
Publication bias $(\beta_1)$	2291	.0626	.607	.2219
	(.4432)	(.5514)	(.6178)	(.7157)
True effect $(\beta_0)$	0196***	0114***	0473**	0347
	(.0067)	(.0011)	(.0216)	(.0337)
Observations	1092	1092	1092	1092
R-squared	.0031	.0003	.0315	.0025

Table 7: Funnel-asymmetry test for publication bias (FAT-PET) in the IV-subsample

Notes: Standard errors clustered at study level are in parentheses. The weights used in the WLS estimation are indicated in the column header. Q-test: Q = 13017.63, p-value = 0.0000;  $\tau^2 = .01695$ ;  $I^2 = 99.41\%$ ; \*\*\*p < .01,\*\*p < .05, \*p < .1

#### 7.2 Assessing heterogeneity in the IV-subsample

In the IV-subsample, 16 moderator variables are robustly and consistently identified as drivers of heterogeneity (s. Table 8).<sup>13</sup> With regard to the level of analysis, estimates at the highest level of aggregation are clearly more negative compared to industry-level estimates, while finer levels of aggregations (*region\_lev*, *occ\_lev*, *individual\_lev*) are positively associated with the effect size. This at odds with the results of the full sample and the other robustness checks where occupational level is associated with more negative findings. Adding more countries to the sample also increases the effect size. As regards geographic disparities, there is strong evidence among IV-estimates that the wage effects of robots are more negative in the US compared to other parts of the world. Controlling for ICT usage and unionization is positively linked to the effect size.

Interestingly, in the IV-subsample we obtain evidence for skill-biased technical change favoring high-skilled occupations, although the magnitude of the skill-bias still appears limited. In addition to individuals who stay in an automating sector, also individuals who stay at an employer that adopted robots, seem to benefit from more positive wage outcomes.

Further, among IV-estimates we obtain strong evidence that the construction of the robot measure can influence the effect size. Relative to a robot density, absolute robot stocks, monetary measures as well as adoption dummies are associated with more positive findings. Besides, applying a shift-share measure or using the PIM is also positively associated with the reported effect size. These findings related to the construction of the robot measure have already been suggested by our preferred weighting scheme REE2 in the total meta-sample.

The coefficients for *manuf* and *non\_manuf* point to more adverse effects in manufacturing industries and more advantageous effects in the non-manufacturing sector also in the IV-subsample but due to the reduced number of observations are more imprecisely estimated, especially for *non\_manuf*. Moreover, more recent IV estimates with a mid-year after 2007 tend to be more favorable: *period\_2007* is always selected with a negative sign and also statistically significant except for weighting scheme REE2.

 $<sup>^{13}</sup>$ Results for the full moderator matrix are available upon request. The moderator variables gmm and  $other\_robot\_measure$  are no longer relevant in the IV-subsample.

	(1)	(2)	(3)	(4)
	WLS FEE1	WLS FEE2	WLS REE1	WLS REE2
se_pcc	.7114**	3568	.7684***	
	(.3365)	(.7572)	(.2428)	
country_lev	-1.3901***	-1.6408***	-1.1788***	-1.1937***
	(.1448)	(.125)	(.1349)	(.1188)
region_lev	.6021***	.5788***	.3131***	.1953***
	(.1034)	(.1347)	(.0651)	(.0449)
occ_lev	.3842***	.5923***	.2328***	.3743***
	(.1178)	(.1638)	(.0669)	(.0567)
individual_lev	.4702***	.4895***	.2883***	.2941***
	(.1041)	(.139)	(.0574)	(.048)
c_num	.0427***	.0454***	.0359***	.034***
	(.0059)	(.0077)	(.0049)	(.0048)
us	1995***	2921***	2196***	2148***
	(.0212)	(.0767)	(.0251)	(.0301)
ict_control	.2073***	.4537***	.1103***	.1752***
	(.0646)	(.1636)	(.0376)	(.0248)
union	.5469***	.8447***	.2976***	.6463***
	(.0688)	(.1667)	(.0435)	(.1624)
high_skill_occ	.0434***	.0406***	.0405***	.0323***
	(.0005)	(.0043)	(.0018)	(.0091)
stay_empl	.3221***	.4527***	.3688***	.3879***
	(.0361)	(.1109)	(.0441)	(.0382)
stay_sec	.0429***	.043***	.0381***	.0438***
	(0)	(.0002)	(.0003)	(.0019)
shift_share	.1424***	.3246***	.1036*	$.1617^{***}$
	(.0517)	(.1146)	(.0522)	(.0516)
rob_stock	.4339***	.6618***	.449***	.8887***
	(.0936)	(.1138)	(.085)	(.0942)
rob_dum	.4398***	.6674***	.456***	.9135***
	(.0908)	(.1153)	(.0841)	(.0925)
$monetary\_rob$	.1943***	.3458***	.2342***	.601***
	(.0591)	(.0722)	(.0699)	(.0694)
pim	.2432***	.3237***	.2224***	.3043***
	(.0465)	(.0647)	(.044)	(.044)
_cons	7484***	8134***	5737***	798***
	(.1334)	(.1746)	(.0826)	(.1065)
Observations	1092	1092	1092	1092
R-squared	.7528	.7672	.7145	.8218

 Table 8: Meta-regression analysis for the IV-subsample

Note: Standard errors clustered at study level are in parentheses. The weights used in the WLS estimation are indicated in the column header. The moderator variables were selected by means of a backwards stepwise regression algorithm which sequentially selects the explanatory variables that minimize the BIC information criterion. Only those moderator variables are shown that exhibit consistent and statistically significant results across all weighting schemes. \*\*\*p < .01, \*\*p < .05, \*p < .1

# 8 Robustness checks

#### 8.1 Exclude estimates with non-clustered standard errors

The t-statistics of primary estimates with non-clustered standard errors might be less reliable because clustered standard errors are typically higher and thus come along with a more conservative statistical inference.<sup>14</sup> Therefore, as robustness check, we exclude all estimates with non-clustered standard errors from our meta-analysis. This reduces our sample to 42 studies with 1852 estimates. Reassuringly, our main results presented above are robust to the exclusion

<sup>&</sup>lt;sup>14</sup>Additionally, the computation of the degrees of freedom for primary estimates with non-clustered standard errors deviates from those with clustered standard errors. Primary estimates with nonclustered standard errors often have much higher degrees of freedom.

of estimates with non-clustered standard errors.<sup>15</sup> The evidence for a negative publication bias becomes even weaker (FAT) and the overall mean effect (PET) is still close to zero (Table 9). Weighting scheme FEE1 suggests a slightly negative true effect (0.019), which is significant at the 10%-level. Weighting scheme FEE2 in turn indicates a slightly positive true effect with a similar magnitude (0.013) that is significant at the 5%-level. The true effect in weighting schemes REE1 and REE2 becomes somewhat larger in absolute terms compared to Table 3 but still lacks statistical significance.

In our multivariate MRA framework (Table D1), 8 of the 13 moderator variables identified as drivers of heterogeneity in Tables 4 and 5 remain qualitatively unchanged: *country\_lev*, *c\_num*, *trade\_control*, *stay\_sec*, *manuf*, *non\_manuf*, *weighted*, and *long\_dif*. However, *tenure*, *ict\_control*, *demograph\_control*, *quartile\_4*, and *journal* are no longer robustly selected in the backwards stepwise regression procedure. Instead, in the subsample of estimates with clustered standard errors, controlling for productivity and firm size are identified as drivers of heterogeneity. While controlling for productivity or value added (*prod\_va*) slightly decreases the reported effect sizes, controlling for the size of the firm in which workers are employed seems to exert a positive influence on the effect size.

Moreover, the evidence for skill-biased technological change becomes stronger as subsamples with only highly educated workers are robustly found to exhibit more positive wage effects of industrial robots. With respect to geographic disparities, estimates for the US as well as developing countries show clearly more negative effects compared to global samples. The latter effect could be due to reshoring activities (Krenz et al., 2021) Regarding the construction of the robot measure, we find evidence that robot stocks and monetary measures are associated with more positive effect sizes. This has already been indicated by the REE weighting schemes in the full sample of primary estimates (s. Table C1).

Furthermore, with regard to the estimation technique, time fixed effects are positively and industry fixed effects negatively associated with reported effects sizes. Lastly, primary estimates for which we had to compute an average marginal effect are positively associated with the effect size. This result leads to a further robustness check where we exclude all self-computed average marginal effects from our meta-analysis.

#### 8.2 Exclude average marginal effects

Some primary estimations employ an interaction or quadratic term for the robot variable.<sup>16</sup> To include such estimations in our meta-analyses we compute average marginal effects and associated standard errors as described in equations (2) or (3). Excluding these self-computed marginal effects from our meta-analysis reduces our sample to 51 studies with 1812 estimates

 $<sup>^{15}\</sup>mathrm{Results}$  for the full MRA table are available upon request.

<sup>&</sup>lt;sup>16</sup>Interacted variables are frequently binary indicators for gender, education levels, or occupational groups but also continuous variables for ICT intensity (Hötte et al., 2022b) or labor market concentration (Liu, 2022)
	(1)	(2)	(3)	(4)
	WLS FEE1	WLS FEE2	WLS REE1	WLS REE2
Publication bias $(\beta_1)$	8661	0238	.1407	.3591
	(1.1013)	(.744)	(.9627)	(.5124)
True effect $(\beta_0)$	0191*	.0134**	1086	0494
	(.0107)	(.0064)	(.0813)	(.056)
Observations	1852	1852	1852	1852
R-squared	.0288	0	.0009	.0049

Table 9: FAT-PET for estimates with clustered SE

Notes: Standard errors clustered at study level are in parentheses. The weights used in the WLS estimation are indicated in the column header. Q-test: Q = 30896.32, p-value = 0.0000;  $\tau^2 = .09366$ ;  $I^2 = 99.70\%$ ; \*\*\*p < .01,\*\*p < .05, \*p < .1

but leaves our findings qualitatively unchanged.<sup>17</sup> The overall mean effect of robots on wages (PET) remains close to zero and is also statistically insignificant except for FEE2-weights (Table 10). The evidence for publication selection remains weak: the coefficient on the standard error is only significant in weighting scheme FEE1, like before (Table 10, column 1). In our multivariate MRA, the robustly and consistently selected moderator variables are largely unchanged (Table D2).

	(1)	(2)	(3)	(4)
	WLS FEEI	WLS FEE2	WLS REEI	WLS REE2
Publication bias $(\beta_1)$	-1.669*	.6787	2797	.4146
	(.8974)	(.7827)	(1.1219)	(.4918)
True effect $(\beta_0)$	0018	0088**	0905	0363
	(.0042)	(.0037)	(.0644)	(.0405)
Observations	1812	1812	1812	1812
R-squared	.1169	.0089	.0035	.0072

Table 10: FAT-PET excluding average marginal effects

Notes: Standard errors clustered at study level are in parentheses. The weights used in the WLS estimation are indicated in the column header. Q-test: Q = 33984.44, p-value = 0.0000;  $\tau^2 = .0873$ ;  $I^2 = 99.95\%$ ; \*\*\*\*p < .01, \*\*p < .05, \*p < .1

## 8.3 Subsample of single-country estimates

The majority of primary estimates in our meta-analysis come from single-country studies (s. section 3). These estimates as well as estimates for a sample of several European countries or

<sup>&</sup>lt;sup>17</sup>Results for the full MRA table are available upon request.

developing/emerging countries can be clearly assigned to one of the geographic categories us, europe, dev\_country, or japan. However, there are also studies that analyze a global sample of countries covering more than one of the geographic categories (e.g. OECD countries). Such estimates cannot be unambiguously assigned to one of the geographic countries and thus form the reference category (together with two estimates for Canada) in our multivariate MRA framework in section 6. One might argue that this reference group is inappropriate as global samples of countries also comprise our geographic categories used to identify potential geographic differences in the wage effect of robots. As robustness check, we therefore focus only on estimates obtained from a single country, i.e. if  $c_{-num} = 1$ . This reduces our MRA sample to 42 studies with 1656 estimates. Since estimates for European countries form the largest group among single-country estimates, europe is now used as reference category to identify geographic disparities.<sup>18</sup> Our main results presented in sections 5 and 6 are largely preserved in the subset of single-country estimates.<sup>19</sup> The evidence for a negative publication bias (FAT) becomes somewhat stronger but is still not robust in weighting schemes adjusting for the number of equations per study (Table 11). The true effect (PET) remains very close to zero and also lacks statistical significance except for weighting scheme FEE1.

In our multivariate MRA, the moderator variables tenure, trade\_control, ict\_control, demograph\_control, stay\_sec, manuf, non\_manuf, and weighted are robustly identified as drivers of heterogeneity in the primary literature (Table D3). It is important to note that country\_lev and  $c_num$  are no longer relevant in the subset of single-country estimates and thus are removed from the moderator matrix. quartile\_4 and journal are significant and selected by the backwards stepwise regression procedure in only 3 out of 4 weighting schemes (non-selected for REE1 weights).

Additionally, we obtain strong evidence that occupation-level analyses and earlier samples with a mid-year before 2007 produce more negative findings among single-country estimates. Also, controlling for the share of manufacturing is negatively associated with the reported effect size. Geographic disparities in the wage effect of robot adoption are, however, still not robust and consistent. Weighting schemes FEE1, REE1, and REE2 suggest that the findings are more negative for the US compared to Europe, whereas FEE2 indicates more positive effects in the US. Moderator variable *japan* is selected and clearly positive in all weighting schemes but insignificant for our preferred REE2 weights. Moderator variable *dev\_country* is only selected when FEE weights are used and there shows a significantly positive link with the effect size.

## 9 Conclusion

To our knowledge, this is the first meta-analysis on the effects of industrial robot adoption on wages. Through systematic search and review of the existing literature we were able to code

 $<sup>^{18}\</sup>mathrm{The}$  two estimates for Canada are also again part of the reference group.

 $<sup>^{19}\</sup>mathrm{Results}$  for the full MRA table are available upon request.

	(1)	(2)	(3)	(4)
	WLS FEE1	WLS FEE2	WLS REE1	WLS REE2
Publication bias $(\beta_1)$	-2.089**	.4051	-1.9454*	4144
	(.9472)	(.8799)	(1.0378)	(.5183)
True effect $(\beta_0)$	.0024*	0052	0128	0073
	(.0012)	(.005)	(.0178)	(.0377)
Observations	1656	1656	1656	1656
R-squared	.1546	.0028	.1477	.0062

Table 11: FAT-PET for single-country estimations

Notes: Standard errors clustered at study level are in parentheses. The weights used in the WLS estimation are indicated in the column header. Q-test: Q = 35075.65, p-value = 0.0000;  $\tau^2 = .0647$ ;  $I^2 = 99.98\%$ ; \*\*\*p < .01, \*\*p < .05, \*p < .1

53 papers with overall 2143 estimates. In line with the MRA framework in economic contexts (Stanley and Doucouliagos, 2012, p. 25), we utilize partial correlation coefficients to analyze the true effect of robots on wages and potential publication, omitted variable and misspecification biases in the empirical literature. In addition to precision weighting, we adjust also by the number of estimates per study. We only find limited evidence for a negative publication bias. The coefficient on publication selection is only statistically significant with simple precision weighting whereas it lacks statistical significance in all other specifications (i.e. adjusting the weighting scheme for between-study heterogeneity and/ or the number of estimates per study). The true effect corrected for any publication bias is close to zero and both statistically and economically not significant. This suggests that robot adoption has so far had no visible effect on the average wage of the total population.

We further assess the heterogeneity of effect sizes reported in primary studies by employing a moderator matrix in our WLS estimations. A properly coded moderator matrix can help to identify the data characteristics and estimation methods which drive both sign and strength of the estimated effect sizes. We find that more positive results are obtained if primary estimations a) include more countries in their sample, b) control for ICT capital, demographic developments, or tenure, c) focus on employees that remain employed in the same sector, d) consider only nonmanufacturing industries, e) are specified in long-differences, and f) come from a peer-reviewed journal article. More negative effects, in turn, are reported for primary estimations that are i) weighted, ii) aggregated at country level, iii) control for trade exposure, iv) and consider only manufacturing industries. We do not find evidence for data dependence. However, the treatment of IFR data becomes relevant in the IV-subsample: Recalculating the IFR robot stock via PIM yields more positive wage results. Reassuringly, reallocating the "unspecified" robots is not driving the results.

We also find some evidence for skill-biased technological change since wages are more positively affected in high-skilled occupations and more negatively affected in medium- to low-skilled occupations. The magnitude of that effect is albeit small and less robust than one might expect in light of the widespread view of skill-biased technological change in theoretical models incorporating automation. Similarly, the often cited dichotomy in labor market outcomes between the US and Europe cannot be robustly estimated when one controls for more research characteristics. Instead, we find some evidence that the wage effects of industrial robots are more positive in Japan as well as in developing or emerging countries. Furthermore, studies that consider more recent time periods tend to find more positive effects.

The findings of our meta-analysis allow to give some recommendations for future research on the labor market effects of robots. So far, little research has tried to measure the macroeconomic effect of robotization on wages at the country level. As country-level analyses seem to find more negative effects, there is a need of further research on the net effect of robotization resulting from the heterogeneous effects at lower levels of aggregation. Ideally, future studies would complement data at different levels of analysis to both measure equilibrium effects and disentangling the adjustment mechanisms along the path to a new labor market equilibrium. In doing so, control variables for ICT usage, trade flows, and demographic developments are important for isolating the effect of robot adoption on wages (or related labor market outcomes like employment or the labor share of GDP). In terms of compositional adjustments, it is crucial to differentiate employment in manufacturing and non-manufacturing industries. Country-level analyses may allow to track links between automation, trends in the manufacturing value added share, and employment or wage developments in the manufacturing sector, especially in comparison to the service sector. Global multi-country samples can help detecting international spillover and relocation effects with respect to output and employment by considering adjustments in global value chains associated with robot adoption. Since weighted estimations seem to systematically reduce the reported effect size, unweighted estimations as well as alternative weightings may be useful robustness checks in future research. When it comes to employing IV/2SLS estimations, researchers should ideally use alternative measures for robotization to avoid any undue influence of certain constructions applied on robot data. While research on the labor market impact of automation will grow further and the adoption of robots multiplies, it will be interesting to update our meta-analysis at a later point in time.

Variable	Description	Incidence	Mean	Mean PCC	Min PCC	Max PCC
Level of analysis $(i)$	reference cat.: industry level)					
country_lev	= 1 if the analysis is at the country level	20	0.009	-0.559	-0.862	0.226
region_lev	= 1 if the analysis is at the region level	361	0.168	-0.068	-0.856	0.461
firm_lev	= 1 if the analysis is at the firm level	156	0.073	0.018	-0.451	0.139
occ_lev	= 1 if the analysis is at the occupation level	65	0.030	-0.224	-0.872	0.190
dem_lev	= 1 if the analysis is at the level of demographic groups	492	0.230	-0.304	-0.862	0.926
individual_lev	= 1 if the analysis is at the individual level	735	0.343	0.047	-0.818	0.997
Geography (referen	nce cat.: global/multi-country sample)					
c_num	Number of countries in the sample	2143	3.434	-0.157	-0.955	0.997
sn	= 1 if the analysis focuses only on the USA	604	0.282	-0.452	-0.955	0.461
europe	= 1 if the analysis focuses only on European countries	1115	0.520	0.061	-0.862	0.997
japan	= 1 if the analysis focuses only on Japan	10	0.005	0.243	0.105	0.468
dev_country	= 1 if the analysis focuses only on developing countries	328	0.153	-0.035	-0.683	0.611
Time period (refer	$ence\ cat.:\ sample\ midpoint\ after\ 2007)$					
period_2007	= 1 if the midpoint of the sample period is smaller or equal year 2007	972	0.454	-0.259	-0.955	0.787
sample_years	Number of years in the sample	2143	6.834	-0.197	-0.955	0.765
Construction of ro	bot measure (reference cat.: robot density)					
shift_share	= 1 if the robot measure is based on a shift-share design	832	0.388	-0.262	-0.862	0.611
rob_stock	= 1 if the absolute robot stock is used as robot measure	43	0.020	0.162	-0.818	0.992
rob_dum	= 1 if a dummy for robot adoption is used	198	0.092	0.019	-0.089	0.139
monetary_rob	= 1 if the robot measure is a monetary value	41	0.019	0.059	-0.499	0.468
other_rob_measure	= 1 if the robot measure is not captured by any category (e.g. patent data)	73	0.034	-0.108	-0.872	0.341
unspec	= 1 if the estimation includes unspecified robots from the IFR data	857	0.400	-0.304	-0.955	0.787

Table A1: Moderator variables for data characteristics

pim	= 1 the robot data are re-calculated using the perpetual inventory method	216	0.101	-0.011	-0.862	0.787
foreign_rob	= 1 if the explanatory variable measures robot adoption in foreign countries	478	0.223	-0.471	-0.955	0.611
lag_rob	= 1 if the robot variable is lagged	521	0.243	0.070	-0.747	0.926
Definition of wag	e measure (reference cat.: daily/weekly/monthly/annual wage)					
hourly_wage	= 1 if hourly wage is used as dependent variable	754	0.352	-0.219	-0.872	0.997
income	= 1 if income is used as dependent variable	113	0.053	-0.087	-0.823	0.162
cumulated_earn	= 1 if cumulated earnings are used as dependent variable (earned money is cumulated	74	0.035	0.076	-0.625	0.658
	over several years)					
wage_bill	= 1 if a wage bill is used as dependent variable	181	0.084	-0.365	-0.955	0.343
rel_wage	= 1 if the dependent variable is expressed relative to an average or total wage measure	147	0.069	0.016	-0.235	0.341
	(e.g. sector average)					
alter_data	= 1 if the estimation is based on an alternative data preparation (e.g. other data	314	0.147	-0.269	-0.938	0.301
	source or alternative computation/ imputation of wage or robot data without changing					
	the type of measure)					
Note: This table	presents the description of all moderator variables. "Incidence" reports the number of cas	es a bine	urv variab	le is equal t	to 1. "Mean	" gives the

Ð unweighted sample average of each moderator variable. "Mean PCC" is the unweighted mean of PCCs, conditional on that the respective moderator variable is b equal to 1. "Min PCC" and "Max PCC" show the minimum and maximum value of PCCs, given that the respective moderator variable is equal to 1.

Variable	Description	Incidence	Mean	Mean PCC	Min PCC	Max PCC
education	= 1 if the estimation controls for educational/ skill levels	1273	0.594	-0.115	-0.872	0.997
prod_va	= 1 if the estimation controls for value added/ productivity	171	0.080	-0.037	-0.875	0.600
gender_control	= 1 if the estimations controls for female share of (fe-)male dummy	1428	0.666	-0.101	-0.872	0.997
migration	= 1 if the estimation controls for for eigner/migrant/ethnicity share or dummy	1117	0.521	-0.175	-0.872	0.658
firm_size	= 1 if the estimation controls for firm size	348	0.162	0.191	-0.818	0.997
tenure	= 1 if the estimation controls for tenure	155	0.072	0.138	-0.818	0.997
trade_control	= 1 if the estimation controls for trade (e.g. import exposure, offshoring, FDI)	1438	0.671	-0.164	-0.955	0.658
ict_control	= 1 if the estimation controls for ICT	957	0.447	0.021	-0.862	0.997
capital_control	= 1 if the estimation controls for overall capital (total capital stock, non-ICT capital,	151	0.070	-0.066	-0.862	0.765
	capital intensity)					
factor_comp	= 1 if the estimation controls for initial wage/ income or capital and labor compen-	314	0.147	0.035	-0.850	0.765
	sation					
union	= 1 if the estimation controls for unionization rates or dummy	14	0.007	0.072	-0.018	0.226
demograph_control	= 1 if the estimation controls for demographics (e.g. working age population share,	1595	0.744	-0.093	-0.872	0.997
	avg. age of population, population growth, age of individuals)					
manuf_share	= 1 if the estimation controls for the share of manufacturing or several manufacturing	734	0.343	-0.336	-0.955	0.441
	industries					
routine	= 1 if the estimation controls for routine employment (e.g. share of routine tasks	479	0.224	-0.351	-0.862	0.461
	employment)					
Note: This table pr	resents the description of all moderator variables. "Incidence" reports the number of $c$	ases a binar	ty varial	ble is equal t	o 1. "Mean	" gives the

Table A2: Moderator variables for the inclusion of control variables (reference cat.: no controls)

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unweighted sample average of each moderator variable. "Mean PCC" is the unweighted mean of PCCs, conditional on that the respective moderator variable is equal to 1. "Min PCC" and "Max PCC" show the minimum and maximum value of PCCs, given that the respective moderator variable is equal to 1. 

Variable	Description	Incidence	Mean	Mean PCC	Min PCC	Max PCC
Subsamples rela	ated to skill levels (reference cat.: total population)					
high_educ	= 1 if the sample includes only workers with a high level of education	85	0.040	0.035	-0.667	0.735
medium_educ	= 1 if the sample includes only workers with a medium level of education	86	0.040	-0.022	-0.818	0.992
low_educ	= 1 if the sample includes only workers with a low level of education	84	0.039	0.011	-0.818	0.992
high_skill_occ	= 1 if the sample includes only workers in high-skilled occupations	334	0.156	0.020	-0.169	0.770
ml_skill_occ	= 1 if the sample includes only workers in medium- or low-skilled occupations	396	0.185	-0.040	-0.955	0.990
Subsamples rela	ited to labor mobility (reference cat.: total population)					
temp_contract	= 1 if the sample includes only workers with a temporary contract	12	0.006	0.121	-0.024	0.997
$\operatorname{stay}_{-}\operatorname{empl}$	= 1 if the sample includes only workers remaining at the same employer/firm/plant	44	0.021	0.349	-0.060	0.658
$switch_{empl}$	= 1 if the sample includes only workers who switched the employer/plant/firm or	418	0.195	-0.030	-0.625	0.265
	newly recruited workers					
stay_sec	= 1 if the sample includes only workers who remain in the same sector	120	0.056	0.130	-0.140	0.658
$switch\_sec$	= 1 if the sample includes only workers who change their sector of employment	86	0.040	-0.059	-0.435	0.004
stay_reg	= 1 if the sample includes only workers who remain in the same region	118	0.055	0.105	-0.119	0.658
$switch_reg$	= 1 if the sample includes only workers who change their region of employment	80	0.037	-0.025	-0.093	0.016
stay_occ	= 1 if the sample includes only workers who remain in the same occupation	17	0.008	-0.137	-0.625	0.225
switch_occ	= 1 if the sample includes only workers who change their occupation	17	0.008	0.040	-0.276	0.658
Subsamples rela	ited to demographic groups (reference cat.: total population)					
male	= 1 if the sample includes only men	172	0.080	0.147	-0.752	0.926
female	= 1 if the sample includes only women	164	0.077	0.161	-0.747	0.882
$age_{-}30$	= 1 if the sample includes only workers below an age of 30	211	0.098	0.275	-0.445	0.926
$age_{-}30_{-}49$	= 1 if the sample includes only workers aged between 30 and 49	233	0.109	0.231	-0.559	0.926
$age_{50}59$	= 1 if the sample includes only workers aged between 50 and 59	208	0.097	0.286	-0.571	0.926
age_60	= 1 if the sample includes only workers with an age equal to or above 60	66	0.031	0.005	-0.571	0.501

Table A3: Moderator variables for subsample estimations

quartile_1	= 1 if the sample includes only individuals in the 1st quartile of the earnings distri-	6	0.004	0.185	-0.129	0.882
	bution (0-25 percentile)					
quartile_2	= 1 if the sample includes only individuals in the 2nd quartile of the earnings distri-	17	0.008	0.246	-0.141	0.756
	bution (26-50 percentile)					
quartile_3	= 1 if the sample includes only individuals in the 3rd quartile of the earnings distri-	14	0.007	0.153	-0.193	0.711
	bution (51-75 percentile)					
quartile_4	= 1 if the sample includes only individuals in the 4th quartile of the earnings distri-	15	0.007	0.066	-0.193	0.500
	bution (76-100 percentile)					
Subsamples relat	ed to robot exposure (reference cat.: full sample)					
manuf	= 1 if the sample includes only manufacturing industries	450	0.210	-0.103	-0.955	0.992
non_manuf	= 1 if the sample includes only non-manufacturing industries	00	0.028	0.201	-0.244	0.611
excl_automotive	= 1 if the automotive industry is excluded from the sample	42	0.020	-0.098	-0.462	0.585
outlier	= 1 if the estimation excludes outliers (e.g. most exposed countries or regions)	143	0.067	-0.228	-0.760	0.655
Note: This table	presents the description of all moderator variables. "Incidence" reports the number of cas	es a bin	ary variab	le is equal	to 1. "Mean	" gives the

ē unweighted sample average of each moderator variable. "Mean PCC" is the unweighted mean of PCCs, conditional on that the respective moderator variable is equal to 1. "Min PCC" and "Max PCC" show the minimum and maximum value of PCCs, given that the respective moderator variable is equal to 1.

Variable	Description	Incidence	Mean	Mean PCC	Min PCC	Max PCC
Estimation tech	hnique (reference cat.: OLS, no fixed effects)					
quant_reg	= 1 if a quantile regression is employed	16	0.007	0.378	-0.027	0.882
gmm	= 1 if GMM is employed	26	0.012	0.046	-0.174	0.252
weighted	= 1 if the estimation is weighted	972	0.454	-0.198	-0.955	0.926
iv	= 1 if the estimation is based on an IV approach	1092	0.510	0.006	-0.862	0.840
log_log	= 1 if both dependent and independent variable are in logarithmic transformation	43	0.020	0.019	-0.555	0.723
long_dif	= 1 if the estimation is based on long differences	677	0.316	-0.240	-0.955	0.997
stacked_dif	= 1 if the estimation is based on stacked differences	415	0.194	-0.063	-0.938	0.926
multiple	= 1 if robots occur more than once in the regression (e.g. domestic and foreign robots,	584	0.273	-0.015	-0.862	0.992
	quadratic term, interaction term)					
marginal_comp	$\sim = 1$ if the effect size is based on a self-computed marginal effect (non-linear or inter-	331	0.154	-0.031	-0.862	0.879
	action term)					
time_fe	= 1 if the estimation includes time fixed effects	1163	0.543	-0.046	-0.938	0.997
entity_fe	= 1 if the estimation includes panel fixed effects or dummies for certain regions)	1838	0.858	-0.081	-0.862	0.992
ind_fe	= 1 if the estimation includes industry fixed effects	606	0.283	-0.035	-0.841	0.787
ey_fe	= 1 if the estimation includes entity-year fixed effects (e.g. country-year FE)	471	0.220	-0.151	-0.862	0.787
secl_no	= 1 if inference was based on non-clustered standard errors (default or only	291	0.136	-0.155	-0.955	0.232
	heteroskedasticity-robust)					
Publication stat	tus (reference cat.: working paper)					

Table A4: Moderator variables for estimation model and publication status

Note: This table presents the description of all moderator variables. "Incidence" reports the number of cases a binary variable is equal to 1. "Mean" gives the unweighted sample average of each moderator variable. "Mean PCC" is the unweighted mean of PCCs, conditional on that the respective moderator variable is equal to 1. "Min PCC" and "Max PCC" show the minimum and maximum value of PCCs, given that the respective moderator variable is equal 0.926-0.955-0.1780.446956= 1 if the estimate is from a study published in a peer-reviewed journal journal to 1.

		Assigned skill level	l
Subsamples in primary estimations	high-skilled	medium-skilled	low-skilled
Low Frey-Osborne Probability of Computerization	1	0	0
Managers, legal professionals	1	0	0
STEM (science, technology, engineering, math) profession-	1	0	0
als and other professionals			
Non-routine cognitive	1	0	0
Abstract	1	0	0
White collar workers	1	1	0
Robot operators	1	1	0
Non-routine	1	0	1
Sales and clerical workers	0	1	0
Craft and related trade workers	0	1	0
Operating machines, processing, asssembling, maintenance	0	1	0
Routine cognitive	0	1	0
High Frey-Osborne Probability of Computerization	0	1	1
Non-professionals	0	1	1
Blue collar workers	0	1	1
Routine	0	1	1
Manual	0	1	1
Routine manual	0	1	1
Non-routine manual	0	1	1
Production workers	0	1	1
Routine production (e.g. welders)	0	1	1
Routine others (e.g. repairer)	0	1	1
Workers exposed to robot applications	0	1	1
Administrative assistants, first and second officers, third	0	1	1
officers and specialists, unskilled			
Elementary occupations	0	0	1
Transportation, storage security, routine transportation	0	0	1
(material-moving, e.g. hand laborer)			
Service (manual)	0	0	1

Table B1: Conversion of occupational skill levels according to ISCO-08

Note: The assignment follows ISCO-08 skill levels, i.e. High-skilled = 3 + 4 (majors 1-3); medium-skilled = 2 (majors 4-8); low-skilled = 1 (major 9)

	A	Assigned skill level	
Subsamples in primary estimations	high-skilled	medium-skilled	low-skilled
University graduates (Masters and doctoral degree)	1	0	0
College or professional degree	1	0	0
Sixteen or More Years of School	1	0	0
Some college	0	1	0
Highschool degree	0	1	0
Upper secondary education	0	1	0
Vocational training degree	0	1	0
Upper secondary and post-secondary education	0	1	0
Twelve to Fifteen Years of School	0	1	0
Non-college	0	1	1
Apprentice	0	1	1
Elementary and/or high school diplomas	0	1	1
Less than high school	0	0	1
Compulsory school (incl. primary school and lower secondary)	0	0	1
Less than vocational training degree	0	0	1
Middle school education or below	0	0	1
Pre-primary, primary and lower secondary education	0	0	1
Less Than Twelve Years of School	0	0	1

## Table B2: Conversion of educational skill levels according to ISCED-2011

Note: The assignment follows ISCED 2011 1st digit levels, i.e. high-skilled = 5-8; middle-skilled = 3 + 4; low-skilled = 0-2

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Table C

	(1)	(2)	(3)	(4)	(5)	(9)	(2)	(8)
	WLS FEE1	WLS FEE1 selected	WLS FEE2	WLS FEE2 selected	WLS REE1	WLS REE1 selected	WLS REE2	WLS REE2 selected
se-pcc	-1.7819***	$-1.927^{***}$	-1.0414	$-1.2066^{**}$	.3503		3916	4808
	(.5426)	(.5188)	(.6237)	(.4875)	(.4485)		(.5678)	(.3433)
country_lev	6046***	737***	7338***	7957***	$9165^{***}$	8768***	$-1.1702^{***}$	$-1.1071^{***}$
	(.1893)	(.1946)	(.2262)	(.1819)	(.164)	(.119)	(.1332)	(.1067)
region_lev	$.2384^{**}$	$.2573^{***}$	0911		.1767	$.1617^{***}$	0895	0639*
	(.0949)	(.0603)	(.1183)		(.1125)	(.0353)	(.0851)	(.0332)
firm_lev	$.4396^{***}$	$.4418^{***}$	.1239	$.1879^{***}$	$.2262^{**}$	$.2772^{***}$	01	
	(.0983)	(.0554)	(.1136)	(.0451)	(.1026)	(.0618)	(.0922)	
occ_lev	0864		3187**	2805***	0932	084	$1911^{*}$	$1992^{***}$
	(.1265)		(.1325)	(.0853)	(.1488)	(.0792)	(.0961)	(.0646)
dem_lev	.1351	$.1739^{**}$	1779	0818*	.018		2198**	1761***
	(.1002)	(.0692)	(.1242)	(.0474)	(.1048)		(.0971)	(.0475)
individual_lev	.1282	$.1635^{**}$	1293	0455	.1488	$.1768^{***}$	0137	
	(.1351)	(.0632)	(.1234)	(.0451)	(.1037)	(.0352)	(.0844)	
c_num	$.034^{***}$	$.0361^{***}$	$.0263^{***}$	$.0286^{***}$	$.0209^{***}$	$.0269^{***}$	$.0315^{***}$	$.0296^{***}$
	(.0064)	(900)	(200.)	(.0057)	(.0075)	(.0047)	(.0045)	(.0033)
SU	1366	$1642^{***}$	.0624	.0341	2449*	$2129^{***}$	.0087	
	(.1394)	(.0561)	(.0945)	(.0353)	(.1392)	(.0543)	(.0974)	
europe	1539	$1723^{***}$	.0329		.0162		$.202^{**}$	$.1956^{***}$
	(.1277)	(.0399)	(.0762)		(.1019)		(.0833)	(.0375)
japan	0517		.2295	$.1875^{**}$	$.2624^{*}$	$.237^{***}$	$.2542^{*}$	$.2531^{***}$
	(.1923)		(.1517)	(.0842)	(.1329)	(.0515)	(.1309)	(.081)
dev_country	.0346		$.2613^{***}$	$.2205^{***}$	0429		$.2404^{***}$	$.2574^{***}$
	(.1381)		(.0864)	(.0344)	(.1022)		(.0851)	(.0345)
period_2007	0349***	$0363^{***}$	.0118		0537**	0473**	006	

	(.0086)	(.0104)	(.032)		(.025)	(.0228)	(.037)	
sample_years	0019***	0018***	.0038	$.0028^{***}$	0009		$.0091^{**}$	.0087***
	(.0005)	(.0006)	(.0036)	(.001)	(.0011)		(.0039)	(.0026)
education	035	0371	.0089		0042		.0203	
	(.0443)	(.0335)	(.0311)		(.0362)		(.0482)	
prod_va	0002		0147	0141	0692**	0588**	1952***	$1948^{***}$
	(.0025)		(.0161)	(.0139)	(.0317)	(.0266)	(.0417)	(.04)
gender_control	.0005		.0004		.0264		.029	.0449
	(.0004)		(.0003)		(.0275)		(.0337)	(.0294)
migration	0044		0295		.0247		0588*	0606*
	(.0496)		(.0456)		(.0407)		(.0328)	(.036)
firm_size	.0009		.0154	.0175	.0831	.0775*	$.1561^{***}$	$.1372^{***}$
	(.0022)		(.0163)	(.0161)	(.0516)	(.0388)	(.0437)	(.0262)
tenure	$.3315^{*}$	$.302^{**}$	$.6407^{***}$	$.644^{***}$	.1156	$.1351^{*}$	$.3996^{***}$	$.2722^{***}$
	(.1727)	(.1401)	(.1447)	(.1181)	(.1044)	(.0752)	(.1415)	(.081)
trade_control	0939**	$1068^{***}$	1511***	$1525^{***}$	0866**	0606***	1081**	$1061^{***}$
	(.044)	(.0342)	(.0458)	(.0236)	(.0349)	(.02)	(.0416)	(.0278)
ict_control	$.0628^{*}$	$.0617^{**}$	$.0901^{*}$	$.0885^{***}$	$.0518^{*}$	$.0681^{***}$	$.0823^{**}$	.084***
	(.0346)	(.0269)	(.0457)	(.0319)	(.0289)	(.0178)	(.037)	(.0291)
capital_control	0242		0224		.036		.0861	$.0931^{**}$
	(.0464)		(.049)		(.0338)		(.0636)	(.0396)
factor_comp	0047		0075		0538	0523	0606	0562
	(.0031)		(.0128)		(.0413)	(.0388)	(.0559)	(.0435)
union	0529		.0508	.0759	.1475*		$.118^{*}$	$.1128^{*}$
	(.1234)		(.0857)	(.0548)	(.0767)		(.0619)	(.0611)
demograph_control	$.2847^{***}$	$.2531^{***}$	$.2298^{***}$	$.2169^{***}$	.0566	$.0912^{***}$	$.1019^{**}$	$.143^{***}$
	(.1004)	(.0604)	(.0731)	(.0585)	(.0631)	(.0316)	(.0486)	(.042)
$manuf\_share$	1833**	$1504^{***}$	0921	$0934^{***}$	0353		.0309	
	(.0841)	(.0495)	(.0551)	(.0309)	(.0367)		(.0426)	

routine	.067	$.0823^{*}$	.0603	.0493	.0423		.0064	
	(.05)	(.0431)	(.069)	(.0445)	(.0288)		(.0584)	
high_educ	0051		0006		001		.0154	
	(.0168)		(.0225)		(.0303)		(.0454)	
medium_educ	0065		0019		0038		0133	
	(.0169)		(.0225)		(.0291)		(.0251)	
low_educ	0084	0027***	0038	0026***	0218		0638	
	(.0168)	(0)	(.0225)	(.0001)	(.0327)		(.04)	
high_skill_occ	$.0036^{***}$	$.0044^{**}$	$.0033^{***}$	$.0037^{***}$	0011		003	
	(.0011)	(.0021)	(.0005)	(2000.)	(.0172)		(.0162)	
ml_skill_occ	0014		0005		0235	$0211^{*}$	2600.	
	(.0016)		(.0005)		(.0177)	(.011)	(.0179)	
temp_contract	$.053^{***}$	$.0526^{***}$	$.1606^{*}$	$.1669^{*}$	.0646		$.3076^{***}$	$.3627^{***}$
	(.0167)	(.0187)	(.0826)	(.097)	(.0573)		(.0935)	(.0818)
stay-empl	2039	1806	482***	$5163^{***}$	.1689	$.2054^{**}$	1774	1563
	(.2489)	(.2002)	(.1651)	(.1464)	(.1045)	(.0973)	(.1405)	(.1141)
$\operatorname{switch}_{\operatorname{empl}}$	0913	0935	.1163	.0735*	0226		.0915	
	(.0993)	(.0628)	(.0789)	(.0389)	(.0852)		(.1079)	
stay_sec	$.0449^{***}$	$.0413^{***}$	$.0573^{***}$	$.0521^{***}$	$.0672^{**}$	$.0585^{***}$	$.1891^{**}$	$.1671^{***}$
	(.0022)	(.0011)	(.0163)	(.0082)	(.0284)	(.0176)	(.0763)	(.0545)
switch_sec	0154***	0189***	008		0045		.0155	
	(.0013)	(.0001)	(.0094)		(.0133)		(.051)	
stay_reg	.0022		.0149		.0279		$.1668^{*}$	.1455*
	(.0022)		(.0167)		(.0285)		(.0942)	(.0749)
$\operatorname{switch}_{\operatorname{reg}}$	.0087***		.0169		.0224		.0838*	
	(.0013)		(.0105)		(.0161)		(.0437)	
stay_occ	0195		1097	$1373^{***}$	1166	$1391^{***}$	1761	$1321^{***}$
	(.1736)		(.1416)	(.0426)	(.1172)	(.0513)	(.1362)	(.0421)
switch_occ	.0986		.0027		.011		051	

	(.1742)		(.1412)		(.1162)		(.1204)	
$\operatorname{male}$	0066		0506**	0476**	.0084		0494**	
	(900)		(.0195)	(.0188)	(.028)		(.0235)	
female	0034		0027		.0261		0072	
	(.003)		(.0122)		(.0288)		(.0229)	
age_30	.0045		.0141		.0441	.0535	.0496	
	(.0165)		(.0306)		(.0298)	(.0346)	(.0367)	
$age_{-}30_{-}49$	0118		0037		0061		0217	
	(.017)		(.0217)		(.0303)		(.033)	
$age_{50}_{59}$	***6800.		.006		.0152		.0059	
	(.0031)		(.0172)		(.0162)		(.0382)	
age_60	.0189		002		.0379	.0476	.0315	
	(.0182)		(.024)		(.0479)	(.0462)	(.0736)	
quartile_1	1246	1465**	1668*	1078*	0657		1607*	1595***
	(.0918)	(.0617)	(.0865)	(.0563)	(.0476)		(.0882)	(.051)
quartile_2	1361	$1579^{**}$	1783**	$1193^{**}$	0867**		1687*	164***
	(.0918)	(.0617)	(.0865)	(.0563)	(.0332)		(.0884)	(.0538)
quartile_3	.0621		.1197		.0139		0776	
	(.0935)		(.0917)		(.0501)		(.0734)	
quartile_4	1362	$1581^{**}$	1784**	$1194^{**}$	1938**	1784***	2207**	2103***
	(.0918)	(.0618)	(.0865)	(.0563)	(.0741)	(.047)	(.0965)	(.0505)
manuf	0289**	$0303^{**}$	$0126^{***}$	$0129^{***}$	$1315^{***}$	$1456^{***}$	$1731^{***}$	154***
	(.0139)	(.0144)	(.0043)	(.0045)	(.0409)	(.0369)	(.042)	(.0309)
non_manuf	$.2327^{**}$	$.241^{**}$	$.1615^{**}$	$.1371^{**}$	$.2169^{**}$	$.2055^{**}$	.1051	$.1176^{*}$
	(800)	(.0974)	(.0663)	(.0593)	(.0884)	(.0864)	(.0711)	(.0646)
excl_automotive	$.0151^{***}$		.0062		.0382		0299	
	(.0017)		(.0062)		(.0326)		(.0566)	
outlier	.0326	.0308	.0262		.1026	.0998	.0459	
	(.0301)	(.0307)	(.0406)		(.0731)	(.0716)	(.0396)	

shift_share	1946	186***	.001		0285		.1467	
	(.1277)	(.0613)	(.1039)		(.0764)		(.0882)	
$rob\_stock$	0232		.0186		$.2918^{***}$	$.2706^{***}$	$.275^{***}$	$.1916^{***}$
	(.1728)		(.1053)		(.0762)	(.0757)	(.0667)	(.051)
rob_dum	1396	1155*	047	038	.1606	$.1495^{*}$	$.2797^{***}$	$.1999^{***}$
	(.1454)	(.0686)	(2660.)	(.03)	(6660.)	(.0778)	(.0961)	(.0506)
monetary_rob	0164		.1269	$.0981^{***}$	$.2131^{**}$	$.2335^{***}$	$.3143^{***}$	$.2515^{***}$
	(.127)		(.0829)	(.0273)	(.0913)	(.0732)	(9620)	(.0409)
other_rob_measure	1883	1676**	0421		.1662	$.1477^{**}$	$.2991^{***}$	$.1909^{***}$
	(.1881)	(.0811)	(.1209)		(.1258)	(.0609)	(.1076)	(.058)
unspec	$1256^{**}$	1143**	0584	0689*	0597	0662	0326	
	(.0616)	(.0474)	(.0546)	(.0411)	(.0541)	(.0434)	(.0417)	
pim	0752	0615	1098*	0895*	$.1414^{***}$	$.1174^{***}$	$.1463^{**}$	$.111^{***}$
	(.0723)	(.0505)	(.0594)	(.0469)	(.0495)	(.0382)	(.0571)	(.0415)
foreign_rob	.0029		0517		082	1048**	1129	$1189^{**}$
	(.0359)		(.0387)		(.0621)	(.0511)	(.07)	(.0549)
lag_rob	0413	0443	0339		.0235		0545	069**
	(.0297)	(.0293)	(.0226)		(.0517)		(.0406)	(.0335)
hourly_wage	1289**	$1359^{**}$	.002		0446	0436	.0417	
	(.0526)	(.0512)	(.0542)		(.0465)	(.0392)	(.0446)	
income	0838	0828	$1886^{**}$	2082***	.0206		0292	
	(.085)	(.0687)	(.0788)	(.0651)	(.0297)		(.0382)	
cumulated_earn	1224		1651	1531	0239		0693	
	(.1906)		(.2281)	(.2225)	(.0677)		(.1143)	
wage_bill	0862	0834	.0053		0827	084*	0463	0844
	(.0567)	(.0518)	(.0417)		(.0517)	(.0466)	(.0638)	(.0563)
rel_wage	006		.0862	.0973*	.0312		.0673	
	(.0973)		(.0844)	(.0511)	(.0722)		(.0747)	
alter_data	$.0059^{***}$		0034		0052		0197*	

	(.0012)		(.0081)		(.0064)		(.0106)	
reg	$.0485^{*}$	$.047^{*}$	.059		.2975	$.2779^{*}$	.0561	
	(.0243)	(.0249)	(.0417)		(.1824)	(.145)	(.1618)	
	.0542		0469		0425		0596	
	(.1378)		(.1197)		(.1582)		(.1003)	
ed	$1271^{***}$	$1256^{***}$	1128***	1088***	0731***	$0621^{***}$	$1212^{***}$	1263***
	(.0369)	(.029)	(.0401)	(.0282)	(.0249)	(.021)	(.0265)	(.0246)
	0017		$0106^{***}$	0108***	.0227		.011	
	(.0102)		(.0017)	(.0021)	(.0236)		(.0188)	
50	1079	108	0127		.0328		$.1241^{*}$	$.1584^{**}$
	(.0886)	(.0677)	(.0378)		(.0659)		(.0691)	(.073)
lif	$.2287^{***}$	$.227^{***}$	$.2233^{***}$	$.2032^{***}$	$.1302^{**}$	$.1695^{***}$	$.1561^{**}$	$.1919^{***}$
	(.0536)	(.0452)	(.0577)	(.0424)	(.0615)	(.0515)	(.0641)	(.0536)
d_dif	$.2437^{***}$	$.2273^{***}$	$.1235^{*}$	$.1253^{**}$	.0218	.0764	.1283	.178**
	(.066)	(.0432)	(.063)	(.0474)	(.0576)	(.0466)	(070)	(.0773)
ole	.0405	.0426	0022		***660.	$.1025^{***}$	.0098	
	(.044)	(.0338)	(.0306)		(.0331)	(.0297)	(.0444)	
nal_comp	0005		0005		.0038		0107	
	(.0006)		(.0005)		(.0354)		(.036)	
Ð	0318		.0228		.0545	.0555	.0239	
	(.0719)		(.0475)		(.0572)	(.0535)	(.0396)	
fe	.0176		$.1261^{*}$	$.1224^{***}$	$1222^{***}$	$1025^{**}$	$.1216^{**}$	$.1481^{***}$
	(.0352)		(.0663)	(.0421)	(.0407)	(.0393)	(.0518)	(.0468)
	0725	0644	0612	0774**	0434		0802*	0782***
	(.0506)	(.0604)	(.0412)	(.0318)	(.0382)		(.045)	(.0206)
	$.0019^{***}$	$.0019^{***}$	$.0019^{***}$		.0102		0707*	0647*
	(.0002)	(.0001)	(.0002)		(.0173)		(.0404)	(.037)
0	.0891	.0554	.0068		0152		0265	
	(.1091)	(.0735)	(.0647)		(.0642)		(.0602)	

journal	$.0619^{*}$	$.0643^{*}$	$.1293^{***}$	$.1016^{***}$	.0571	$.0754^{**}$	$.1703^{***}$	$.1622^{***}$
	(.0322)	(.0321)	(.0339)	(.0224)	(.0504)	(.0313)	(.0492)	(.0305)
CONS	0337	0263	3296**	$3212^{***}$	1987	2818***	6093***	$5393^{***}$
	(.1946)	(.0927)	(.1555)	(.062)	(.162)	(.0882)	(.141)	(.0638)
Observations	2143	2143	2143	2143	2143	2143	2143	2143
R-squared	.677	.6675	.8801	.8758	.7853	.7764	.7815	.7641
Note: Standard errors	s clustered at stu	ıdy level are in paı	rentheses. The weig	ghts used in the WI	S estimation are	indicated in the col-	umn header. In col	umns 2, 4, 6, and 8,

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the moderator variables were selected by means of a backwards stepwise regression algorithm which sequentially selects the explanatory variables that minimize the BIC information criterion. Q-test: Q = 14138.63, p-value = 0.0000;  $\tau^2 = .007315$ ;  $I^2 = 99.78\%$ ; \*\*\* p < .01, \*\* p < .05, \*p < .1

	(1)	(2)	(3)	(4)
	WLS FEE1	WLS FEE2	WLS REE1	WLS REE2
se_pcc		-1.4483***		8249
		(.5025)		(.5591)
$\operatorname{country\_lev}$	-1.3319***	-1.2357***	-1.2173***	-1.2207***
	(.0952)	(.0779)	(.0909)	(.0879)
c_num	.0225***	.0348***	.0207***	.0313***
	(.0049)	(.0056)	(.0037)	(.0052)
us	3082***	1006***	3198***	3185***
	(.0427)	(.0365)	(.0839)	(.073)
$dev_country$	3176***	0751**	2517***	2218***
	(.0397)	(.035)	(.0317)	(.0609)
prod_va	0674*	0974*	1048***	1188**
	(.0346)	(.0526)	(.0267)	(.0469)
$\operatorname{firm\_size}$	.316***	.2022***	.2446***	.1606***
	(.04)	(.0454)	(.0369)	(.0369)
$trade\_control$	1467***	0997**	06***	0892***
	(.0283)	(.0396)	(.0204)	(.0252)
high_educ	.1352***	.1415***	.1099*	.1386**
	(.0494)	(.0434)	(.0591)	(.0676)
stay_sec	.0434***	.0502***	.0502***	.1402***
	(.0005)	(.0057)	(.0091)	(.0435)
manuf	0221**	0098***	128***	0961**
	(.0096)	(.0014)	(.0369)	(.0356)
non_manuf	.2033**	.1643**	.1848**	.1127*
	(.0814)	(.0678)	(.0823)	(.0623)
$rob\_stock$	.3192***	.1021*	.4875***	.3448***
	(.0439)	(.0576)	(.0452)	(.0637)
$monetary\_rob$	.1463***	.1089**	.302***	.2085***
	(.0535)	(.051)	(.0731)	(.0577)
weighted	0914***	0994***	0706**	1291***
	(.0172)	(.0225)	(.0268)	(.0292)
long_dif	.11**	.1913***	.1024**	.1577***
	(.0441)	(.047)	(.0454)	(.0536)
$marginal\_comp$	.1121***	.1473**	.0878*	.1192***
	(.033)	(.0648)	(.0436)	(.0321)
time_fe	.135***	.126***	.1424***	.1607***
	(.0379)	(.0296)	(.0438)	(.0465)
ind_fe	1025**	1333***	1087***	122***
	(.0469)	(.0404)	(.0326)	(.032)
_cons	.0585	2498***	1452	4612***
	(.0796)	(.0867)	(.0987)	(.1003)
Observations	1852	1852	1852	1852

Table D1: Meta-regression analysis for estimates with clustered SE

R-squared	.8055	.931	.8037	.8313
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Note: Standard errors clustered at study level are in parentheses. The weights used in the WLS estimation are indicated in the column header. The moderator variables were selected by means of a backwards stepwise regression algorithm which sequentially selects the explanatory variables that minimize the BIC information criterion. Only those moderator variables are shown that exhibit consistent and statistically significant results across all weighting schemes. \*\*\*p < .01,\*\*p < .05, \*p < .1

	(1)	(2)	(3)	(4)
	WLS FEE1	WLS FEE2	WLS REE1	WLS REE2
se_pcc	-1.8437***	-1.2517*		6566
	(.5022)	(.6234)		(.4333)
$\operatorname{country\_lev}$	8911***	7154**	5928***	7312***
	(.3101)	(.2786)	(.1644)	(.225)
occ_lev	2475***	3965***	2327**	3909***
	(.0549)	(.0888)	(.0876)	(.081)
c_num	.0308***	.0169**	.0196***	.0242***
	(.0073)	(.0067)	(.0049)	(.0049)
$trade\_control$	1214***	1264***	058***	0693***
	(.0393)	(.0204)	(.0196)	(.0227)
$ict\_control$	.0448*	.0607*	.0586***	.0785**
	(.0243)	(.0309)	(.0199)	(.0312)
stay_sec	.0412***	.0499***	.0547***	.1519***
	(.001)	(.0055)	(.0141)	(.0551)
manuf	0284**	012***	1778***	2054***
	(.0134)	(.0037)	(.0403)	(.038)
weighted	1213***	1086***	0609**	1231***
	(.0256)	(.0329)	(.0257)	(.03)
long_dif	.2609***	.0909**	.1169***	.1737***
	(.0461)	(.0366)	(.032)	(.0624)
_cons	0523	0598	1352	4815***
	(.0599)	(.0684)	(.0878)	(.0665)
Observations	1812	1812	1812	1812
R-squared	.6816	.8921	.7902	.7921

Table D2: Meta-regression analysis without average marginal effects

Note: Standard errors clustered at study level are in parentheses. The weights used in the WLS estimation are indicated in the column header. The moderator variables were selected by means of a backwards stepwise regression algorithm which sequentially selects the explanatory variables that minimize the BIC information criterion. Only those moderator variables are shown that exhibit consistent and statistically significant results across all weighting schemes. \*\*\*p < .01, \*\*p < .05, \*p < .1

	(1)	(2)	(3)	(4)
	WLS FEE1	WLS FEE2	WLS REE1	WLS REE2
se_pcc	-2.305***	-2.5365***		-1.7689***
	(.6162)	(.4969)		(.6131)
occ_lev	1627***	9996***	2969***	6612***
	(.058)	(.3172)	(.0919)	(.1681)
period_2007	0437***	0591**	0591**	0937***
	(.0062)	(.0255)	(.0251)	(.0315)
tenure	.4173***	.6724***	.109**	.3815***
	(.1398)	(.1084)	(.0534)	(.1031)
$trade\_control$	1508***	1053***	0784***	0752**
	(.0374)	(.0378)	(.0285)	(.0352)
$ict\_control$	.0552	.1057**	.0931***	.1469***
	(.0472)	(.0426)	(.0308)	(.0418)
$demograph\_control$	.24***	.2689***	.1514**	.1568***
	(.0654)	(.0529)	(.0634)	(.044)
manuf_share	2718***	1887***	0957**	1228**
	(.0799)	(.0591)	(.0394)	(.0571)
stay_sec	.0414***	.0528***	.0593***	.212**
	(.0012)	(.01)	(.0185)	(.0901)
manuf	0281**	0112***	1104***	1362***
	(.0133)	(.003)	(.0323)	(.0423)
non_manuf	.2498**	.161**	.2053**	.1051*
	(.1043)	(.0645)	(.0837)	(.0623)
weighted	1606***	0779**	0829***	069**
	(.0382)	(.035)	(.025)	(.032)
_cons	0904	$.5153^{*}$	0084	.1883
	(.0805)	(.3001)	(.0636)	(.1959)
Observations	1656	1656	1656	1656
R-squared	.6992	.9057	.7969	.81

Table D3: Meta-regression analysis for single country estimations

Note: Standard errors clustered at study level are in parentheses. The weights used in the WLS estimation are indicated in the column header. The moderator variables were selected by means of a backwards stepwise regression algorithm which sequentially selects the explanatory variables that minimize the BIC information criterion. Only those moderator variables are shown that exhibit consistent and statistically significant results across all weighting schemes. \*\*\*p < .01, \*\*p < .05, \*p < .1





	Level of analysis	Individual	Industry; region; demographic group	Demographic group		Firm			Occupation	Region		Demographic group;	renntainin		Industry		Region	Region	Firm; individual	Industry	Region
	Robot data	IFR	IFR	IFR	Survey by the French Ministry of	Industry, SYMOP,	customs data, and	fiscal files	JARA, IFR	JARA		IFR			IFR		IFR	IFR	Norwegian Trade Statistics Register	IFR	Customs data
į	Time span	2002 - 2014	1990-2014	1990-2007		2010-2015			1990-2007	1978-2017		2006-2014			2010 - 2018		2005 - 2016	2000-2015	1999-2016	1993-2015	1990-2015
	Countries	Germany	$\mathbf{USA}$	USA		France			USA	Japan	20	European	countries	14	European	countries	$\mathbf{USA}$	Mexico	Norway	17 OECD countries	$\mathbf{USA}$
	SD of PCC	0.042	0.170	0.206		0.120			0.206	0.021		0.189			0.076		0.048	0.142	0.011	0.355	0.207
	Mean of PCC	-0.061	-0.624	-0.275		0.028			-0.317	0.133		0.620			0.011		-0.111	0.020	0.013	-0.042	0.190
	Nr. of estimates	18	346	54		4			26	9		104			136		ъ	12	140	60	14
	Study	Abeliansky and Beulmann 2021	Acemoglu and Restrepo 2020	Acemoglu and Restrepo 2022a		Acemoglu et al. 2020			A dachi 2022	Adachi et al. 2022		Aksoy et al. 2021			Albinowski and T	Lewannowski 2022	Anelli et al. 2021b	Artuc et al. 2019	Barth et al. 2020	Bekhtiar et al. 2021	Bonfiglioli et al. 2021

tudy	Nr. of estimates	Mean of PCC	SD of PCC	Countries	Time span	Robot data	Level of analysis
	24	-0.076	0.069	USA	2004-2016	IFR	Region
				Ar-			
5	120	-0.076	0.076	gentina, Brazil,	2004-2016	IFR	Region
				Mexcio			
	20	0.415	0.578	Czech Republic	2010-2020	IFR	Individual
()	21	0.072	0.140	Indonesia	2008-2015	IFR	Firm; region
	33	0.003	0.023	Italy	2011-2018	IFR	Region
18	6	-0.021	0.032	6 EU countries	1995-2007	IFR	Demographic group
021	1	-0.726		USA	1990-2015	IFR	Region
	4	-0.001	0.191	16 OECD countries	2011-2016	IFR	Industry
æ	394	-0.026	0.036	Spain	2001-2017	IFR	Individual
0	5	0.741	0.362	Germany & Italy	2010-2014	IFR	Individual
	116	-0.025	0.278	Germany	1994-2014	IFR	Demographic group; region; individual
	18	0.027	0.116	Turkey	2004-2017	IFR	Occupation
	4	0.408	0.042	Japan	1979-2012	JARA	Industry

Level of analysis	Industry		Firm	Individual	Individual	Region	Region; individual	Industry	Region		Country; industry		Country	Region		Firm		Industry
Robot data	IFR		Canadian Border Services Agency	IFR	Google Patents Public Data	IFR	IFR	IFR	China Commodity Trade Database		IFR		IFR	IFR	Encuesta Sobre	Estrategias	$\operatorname{Empresariales}$	IFR
Time span	2008-2014		2000-2015	1994-2016	2011 - 2021	1990-2015	2000-2016	1993-2007	2006-2015		1995-2016		2001-2017	1997-2011		1990-2016		2004-2007
Countries	10 emerging	countries	Canada	Italy	USA	$\mathbf{USA}$	China	17 OECD countries	China	19	European	countries	42 countries	UK		$\operatorname{Spain}$		9 OECD countries
SD of PCC	660'0		0.103	0.223	0.071	0.071	0.081	0.240	0.159		0.491		0.135	0.092		0.013		0.023
Mean of PCC	-0.021		0.032	0.211	-0.059	-0.165	-0.142	0.593	0.160		-0.360		0.130	0.011		-0.008		0.216
Nr. of estimates	9		3	68	26	18	33	16	9		28		2	48		24		2
Study	Diaz Pavez and	Martínez-Zarzoso 2021	Dixon et al. 2020	Dottori 2021	Fossen et al. 2022	Ge and Zhou 2020	Giuntella and T. Wang 2019	Graetz and Michaels 2018	Han 2022		Hötte et al. 2022b		Jung and Lim 2020	Kariel 2021		Koch et al. 2021		Kromann et al. 2020

Study	Nr. of estimates	Mean of PCC	SD of PCC	Countries	Time span	Robot data	Level of analysis
Kugler et al. 2020	30	-0.061	0.309	Colombia	2011-2016	IFR	Individual
Lai et al. 2022	5	-0.015	0.019	China	2010 - 2016	Customs data	Region
Liu 2022	16	-0.196	0.086	USA	2006-2014	IFR	Occupation
Pedemonte et al. 2018	9	-0.345	0.336	Mexico	2000-2015	IFR	Region
						China	
Ren et al. 2018	16	0.096	0.024	China	2014	Employer- $Employee$	Individual
						Survey	
						Customs data by	
Rodrigo $2021$	3	0.128	0.049	$\operatorname{Brazil}$	2006-2013	the Secretariat of	Region
						Foreign Trade	
Sequeira et al. 2021	61	-0.366	0.097	$\mathbf{USA}$	1990-2014	IFR	Demographic group
Ctarlaton and Wahh						Encuesta Sobre	
ouaptetont and webb	1	0.022		$\operatorname{Spain}$	1990-2016	Estrategias	Firm
7070/7077						$\operatorname{Empresariales}$	
Stemmler 2020	24	-0.012	0.101	$\operatorname{Brazil}$	1994-2014	IFR	Region
J. C. Wang 2022	4	0.010	0.009	USA	1992-2016	Customs data	Firm
T. Wang et al. 2022	27	0.056	0.022	China	2000-2013	Customs data	Firm
Webb 2020	И	_0 731	0.153	11S A	1080-9010	Google Patents	Ocennation
	2	TO 1.0-	001.0	1700	0T07_000T	Public Data	Accupation
Wettstein et al. 2018	4	-0.012	0.006	USA	1994-2015	IFR	Individual
Yang et al. 2022	1	0.098		China	2014	IFR	Individual

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