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**Working Paper**

## Firm Training, Automation, and Wages: International Worker-Level Evidence

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# Firm Training, Automation, and Wages: International Worker-Level Evidence

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# Firm Training, Automation, and Wages: International Worker-Level Evidence\*

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## Abstract

Firm training is widely regarded as crucial for protecting workers from automation, yet there is a lack of empirical evidence to support this belief. Using internationally harmonized data from over 90,000 workers across 37 industrialized countries, we construct an individual-level measure of automation risk based on tasks performed at work. Our analysis reveals substantial within-occupation variation in automation risk, overlooked by existing occupation-level measures. To assess whether firm training mitigates automation risk, we exploit within-occupation and within-industry variation. Additionally, we employ entropy balancing to re-weight workers without firm training based on a rich set of background characteristics, including tested numeracy skills as a proxy for unobserved ability. We find that training reduces workers' automation risk by 3.8 percentage points, equivalent to 8% of the average automation risk. The training-induced reduction in automation risk accounts for 15% of the wage returns to firm training. Firm training is effective in reducing automation risk and increasing wages across nearly all countries, underscoring the external validity of our findings. Training is similarly effective across gender, age, and education groups, suggesting widely shared benefits rather than gains concentrated in specific demographic segments.

*Keywords: automation, entropy balancing, firm training, human capital, on-the-job training, technological change*

*JEL classification: J24, J31, J61, O33*

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## 1. Introduction

The fear of technology displacing workers has been an ongoing worry since the dawn of the Industrial Revolution. Historical accounts document the protests and destruction of machinery in England and Germany as workers resisted the mechanization of manufacturing (Goldin and Katz, 1998; Mokyr et al., 2015). In recent decades, this anxiety has resurfaced due to significant advances in information and communication technology (ICT) and industrial robotics becoming a prominent issue in public discourse and labor disputes. Unlike in the past, economists today have the data and analytical tools to rigorously assess the extent to which technology substitutes human labor. While several studies have documented the negative impacts of technological advancements on employment and wages (e.g., Acemoglu and Autor, 2011; Goos et al., 2014; Arntz et al., 2016; Cortes, 2016), it is rare for entire occupations to disappear (e.g., Atalay et al., 2020; Bachmann et al., 2022). One reason for this is that technology typically automates only specific tasks within an occupation, leaving room for workers to adapt their roles. This raises a critical question: Can firm training enable workers to update their task portfolio, thereby reducing their susceptibility to being replaced by technology? Unlike training measures in active labor market programs, which primarily focus on enhancing the employability of the unemployed, firm and on-the-job training are aimed at equipping employed workers with firm- and job-specific skills (e.g., Adhvaryu et al., 2023; Black et al., 2023; Diaz et al., 2025).

To address this question, we use detailed individual-level data on job tasks and firm training from the Programme for the International Assessment of Adult Competencies (PIAAC). We construct a measure of automation risk that varies at the individual worker level and we leverage rich data on firm training and workers' background characteristics to address selection into training. Applying entropy balancing and using variation within occupations, within industries, and within countries, we find that firm training substantially reduces the average risk of automation.

PIAAC provides comprehensive and internationally comparable information on job tasks across 37 countries for more than 90,000 workers. It surveys a broad spectrum of tasks performed at work, including manual, cognitive, digital, and social domains. Crucially, PIAAC allows us to identify tasks that are particularly difficult to automate, such as those requiring (a) social intelligence for navigating complex social interactions, (b) cognitive intelligence for complex reasoning, and (c) perception and manipulation for executing physical tasks in unstructured environments. Following Nedelkoska and Quintini (2018), we leverage this rich task data to construct an individual-level measure of automation risk. Intuitively, this measure is a weighted share of job tasks, each with different degrees of susceptibility to automation based on Frey and Osborne (2017). We document substantial variation in automation risk within occupations.<sup>1</sup> In fact, our analysis reveals a notable overlap in individual-level automation risk even between occupations at opposite ends of the occupation-level automation risk spectrum.<sup>2</sup> Moreover, we

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<sup>1</sup>We also observe significant variation in automation risk between occupations, consistent with the existing literature (e.g., Autor et al., 2003; Goos et al., 2014).

<sup>2</sup>Arntz et al. (2017) use U.S.-specific data from PIAAC to illustrate that estimates of automation risk are strongly inflated when task variation within occupations is ignored.

demonstrate that *every* occupation exhibits substantial within-occupation variation in automation risk, emphasizing the importance of going beyond existing occupation-level automation measures (e.g., [Pajarinén and Rouvinen, 2014](#); [Brzeski and Burk, 2015](#); [Frey and Osborne, 2017](#)).<sup>3</sup>

Since workers in PIAAC report their training activities in the year preceding the survey, we can exploit within-occupation variation to investigate the role of firm training in reducing workers' vulnerability to automation. Despite the data being cross-sectional, the rich background information provided by PIAAC—including tested numeracy skills as a proxy for general ability—helps mitigate biases due to selection into training. We also control for other factors typically associated with participation in training, such as educational attainment, socio-demographic characteristics, full-time employment status, industry, and employer size. In our preferred specification, which incorporates country, industry, and occupation fixed effects and applies entropy balancing, we find that participation in firm training reduces a worker's automation risk by 3.8 percentage points (pp). This is equivalent to one-fifth of a standard deviation in automation risk or 8% of the mean risk.<sup>4</sup> The magnitude of the training effect is comparable to the difference in automation risk between ICT professionals (0.40) and ICT technicians (0.44) or between business administration professionals (0.39) and business administration associates (0.43), with professional occupations in the same field typically involving more complex tasks and therefore lower automation risk.

Notably, the estimate on training effectiveness changes only little across specifications once we include occupation fixed effects. This robustness suggests that, once we solely rely on within-occupation variation, selection biases into training are unlikely to be a major identification issue. This conclusion is further supported by a bounding analysis ([Oster, 2019](#)). Overall, our results indicate that training enables workers to expand their task portfolios and increasingly engage in tasks with a lower risk of automation.

We also investigate whether the reduction in automation risk through firm training translates into actual wage gains. Our analysis shows that participation in firm training leads to a marked increase in hourly wages, with our preferred specification indicating a 7.1% wage increase. Notably, similar to our findings for automation risk, the greatest reduction in estimated training effectiveness occurs when accounting for occupation fixed effects. The wage increase from firm training is comparable to the wage gradient associated with an additional year of schooling in industrialized countries ([Hanushek et al., 2015](#)). Further analysis reveals that approximately 15% of the wage gains from training can be attributed to its role in reducing workers' automation risk, underscoring how the shift toward less automatable, technology-complementary tasks is rewarded in the labor market.

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<sup>3</sup>Our main analysis uses two-digit ISCO occupations, which are consistently available across PIAAC countries (with the exception of Austria, Canada, Estonia, and Finland, for which we use the available one-digit codes). However, in the more restricted sample of countries that report occupations at the four-digit level, we observe similar patterns of within-occupation variation in automation risk. In particular, the average standard deviation in automation risk across countries and occupations at the two-digit occupation level is 0.167, while it is 0.169 at the four-digit occupation level.

<sup>4</sup>We show that results are similar when using an alternative measure of routine task intensity. However, we rely on the automation risk index as our main measure, as it is based on a broader range of tasks.

Crucially, our individual-level automation risk measures allow us to advance the existing literature by exploiting variation in automation risk within occupations. Ideally, however, we would like to use individual-level panel data to track changes in automation risk over time and control for selection into training by accounting for pre-training automation risk in our estimations. While this approach is not feasible with the cross-sectional international PIAAC data, we can approximate or even observe workers' pre-training automation risk in country-specific extensions. These analyses yield results that are consistent with our baseline estimations.

First, we leverage a repeated cross-section of the PIAAC survey available for the United States in 2012 and 2017. Using the no-training group from 2012, we impute individual-level past automation risk for workers in the 2017 sample. While past automation risk strongly predicts contemporaneous automation risk, including it as a control has minimal impact on the training estimate. Moreover, drawing on the approach outlined by [Kleven et al. \(2019\)](#) and [Kleven et al. \(2024\)](#), we construct a pseudo panel by matching observations in the 2017 survey to observations in the 2012 survey based on an extensive set of observable characteristics. This matched pseudo panel allows us to add fixed effects to our preferred specification to account for unobserved characteristics of matched pairs.

Second, we take this analysis one step further for Germany, which, as a unique feature among participating countries, created an individual-level panel by re-surveying participants from the original PIAAC sample 3.5 years later.<sup>5</sup> Exploiting this panel dimension, we directly control for past automation risk in a value-added specification. Corroborating the U.S. pseudo-panel evidence, past automation risk is a strong predictor of contemporaneous automation risk, whereas adding it as a control yields only a slight reduction in the estimated training coefficient. The same applies when we also control for past training participation. This suggests that our baseline model already effectively accounts for selection into training based on past automation risk and general training propensity. Observing changes in automation risk and training over the 3.5-year period also enables us to estimate individual fixed effects regressions, which account for time-invariant unobserved heterogeneity. Although the variation available for the panel analysis is limited due to the relatively short time span, we still find a consistent negative relationship between firm training and automation risk.

To understand how firm training reduces automation risk, we investigate the specific tasks performed by workers after receiving training and the types of skills they develop. Our results indicate that firm training increases the use of tasks that are difficult to automate (i.e., negotiation, complex problem-solving, teaching, advising others, influence others, and plan others' work) to a very similar extent. Interestingly, training also increases the use of tasks more susceptible to automation (e.g., manual dexterity or solving simple problems), but to a much smaller degree. Additionally, we find that firm training increases workers' digital skills, which are also assessed in PIAAC. This result indicates that firm training

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<sup>5</sup>The United States and Germany are the only PIAAC countries with multiple waves of data collection.



often involves learning to work with new technology.<sup>6</sup> These shifts in task composition and improvements in digital skills suggest that training helps workers adapt to technological change by equipping them with skills that complement, as opposed to compete with, automation technologies.

Finally, our individual-level design lets us ask who benefits most from firm training. Heterogeneity is policy-relevant: it indicates whether training disproportionately helps some groups—potentially widening gaps between more and less advantaged workers—or delivers broad gains. We find that training is similarly effective across gender, age, and education groups, suggesting that benefits accrue widely rather than being concentrated in specific segments. This underscores the potential of firm training to upskill disadvantaged groups. Our large country sample also enables us to investigate cross-country heterogeneity. We find that training is significantly related to outcomes in almost all countries, whereas the magnitude of the training estimate varies considerably across countries. Notably, these heterogeneity patterns line up closely across both outcomes: automation risk and wages.

We also investigate the effectiveness of firm training by training characteristics. We find that longer training activities are more effective in reducing automation risk and increasing wages, suggesting that the benefits of training arise from genuine skill acquisition of job-specific skills rather than mere signaling effects. Additionally, training is most effective when employers fully or partially finance it, which is the case for nearly 70% of workers in our sample.<sup>7</sup> This suggests that training-induced productivity gains are more likely to materialize when employers have a vested interest in the success of the training.

Our paper contributes to three key strands of literature. First, we contribute to the literature on the labor market effects of firm training and on-the-job training, which has primarily focused on wages, productivity, and employment prospects (LaLonde, 1986; Blundell et al., 1999; Lechner, 1999; Goux and Maurin, 2000; Pischke, 2001; Dearden et al., 2006; Goerlitz, 2011; Hidalgo et al., 2014; Goerlitz and Tamm, 2016; Adhvaryu et al., 2023; Humlum et al., 2025).<sup>8</sup> These studies typically use observational data with varying identification strategies, with a few studies even exploiting random assignment of workers into training (notable exceptions are, e.g., Schwerdt et al., 2012; De Grip and Sauermann, 2012; Adhvaryu et al., 2023; Diaz et al., 2025). This literature consistently finds that firm training improves worker productivity and labor market outcomes.<sup>9</sup> Our study is the first to show that a considerable portion of the wage gains from on-the-job firm training can be attributed to its role in the reshuffling of tasks and reducing workers' risk of automation.

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<sup>6</sup>The content of firm training is not explicitly reported in PIAAC. However, prior work confirms that learning to work with new technology is often a main focus of firm training measures, particularly in environments undergoing rapid technological change (Ma et al., 2024).

<sup>7</sup>Firm-level studies demonstrate that productivity gains from training often exceed the associated wage increases, which could be a strong incentive for firms to pay for training (Konings and Vanormelingen, 2015; Ma et al., 2024).

<sup>8</sup>For comprehensive overviews of the literature, see Leuven (2005), Bassanini et al. (2007), and De Grip and Sauermann (2013).

<sup>9</sup>A substantial body of literature also explores whether adult education enhances non-pecuniary outcomes, including well-being, health, and civic, political, and cultural engagement. For an overview of this literature and supporting empirical evidence, see Ruhose et al. (2019).



Additionally, our work is related to studies on the effectiveness of active labor market policies and training programs for unemployed workers (Hujer et al., 2006; Card et al., 2010; Kluve, 2010; McCall et al., 2016).<sup>10</sup> Schmidpeter and Winter-Ebmer (2021) find that training programs for unemployed workers are especially effective in improving the chances of finding a job for individuals previously employed in routine-intensive occupations. However, Nedelkoska and Quintini (2018) and Heß et al. (2023) demonstrate that workers most exposed to automation are, in fact, the least likely to participate in training.

Second, our study contributes by investigating the role of firm training in reducing workers' automation risk within an international context. We reveal a crucial mechanism through which training can influence wage and employment outcomes—especially in light of rapid technological advancement. By utilizing harmonized data from 37 countries, we demonstrate that the effectiveness of training in mitigating automation risk is present across diverse technological landscapes and levels of automation risk across countries.<sup>11</sup> This international perspective strongly enhances the external validity of our findings, addressing a gap in the literature that has typically relied on data from single countries or specific experiments.<sup>12</sup> At the same time, we uphold the rigorous standards for addressing bias from selection into firm training. We do so by exploiting within-occupation variation and ensuring that workers with and without training are similar in tested numeracy skills (capturing unobserved ability) and in a large set of additional control variables, using entropy balancing techniques.

Third, our study complements the growing body of research on the labor market effects of technology, which has evolved from a skill-based to a task-based approach (Katz and Murphy, 1992; Krusell et al., 2000; Autor et al., 2003; Acemoglu and Autor, 2011; Akerman et al., 2015). This research documents that technological advances have favored higher-skilled workers and those in so-called non-routine jobs; largely due to their higher adaptability to new technologies and their lower susceptibility to technological change (e.g., Deming and Noray, 2020). Studies highlight impacts like job loss (Braxton and Taska, 2023), or the role of specific technologies such as mechanized telephone operations (Feigenbaum and Gross, 2024) or the introduction of robots across industries (Hirvonen et al., 2022; Adachi et al., 2024). Our study extends this research by examining how firm training helps workers update their task portfolios, reducing their vulnerability to automation and enhancing their ability to adapt to technologi-

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<sup>10</sup>See Card et al. (2018) for an overview.

<sup>11</sup>The average automation risk in our data ranges from 38% in Norway to 58% in the Slovak Republic, with a cross-country mean of 46 percent.

<sup>12</sup>Salas-Velasco (2009) and Fialho et al. (2019) utilize international datasets—Careers after Higher Education: a European Research Survey (CHEERS) and PIAAC, respectively—to explore the relationship between firm training and labor market outcomes. While the former focuses on the impact of on-the-job training for European university graduates on wages, the latter takes a broader, policy-oriented perspective, providing descriptive analyses of training frequency, trends across countries over time, and associated wage effects. Unlike our study, neither addresses the role of automation in the context of training. Brunello et al. (2023), using data from the European Investment Bank Investment Survey (EIBIS), examine the impact of automation on employer-provided training at the firm level, finding that firms appear less inclined to offer training to employees after adopting advanced digital technologies. A detailed overview of country-specific studies on the effects of training is available in Ma et al. (2024).

cal change. Additionally, we show that firm training improves digital skills, enabling workers to engage with complex technologies more effectively.

The rest of the paper is organized as follows: Section 2 presents our international worker data, while Section 3 describes how we use these data to construct individual-level measures of automation risk. Section 4 sets out our empirical strategy. Section 5 presents our results on the effects of training on workers' automation risk and wages, discusses the robustness of our estimates, and provides evidence for mechanisms. Section 6 provides evidence for heterogeneities in training effectiveness across countries, socio-demographic groups, and the type of training. Section 7 concludes.

## 2. PIAAC Data

### 2.1. International Data

Our empirical analysis is based on data from the Programme for International Assessment of Adult Competencies (PIAAC), a large-scale survey administered by the Organisation for Economic Co-operation and Development (OECD, 2013, 2016). The survey includes representative samples of working-age individuals (16 to 65 years) from 39 countries, collected in three rounds between 2011 and 2017.<sup>13</sup> The survey items are harmonized across diverse economic and cultural contexts, enabling both international analyses, when pooling all countries, and reliable cross-country comparisons. The international harmonization is especially valuable for measuring firm training, workplace tasks, and adult skills, as no other dataset provides such measures in a comparable fashion across a wide range of countries.

We utilize detailed information on participation in firm training, our treatment variable, from PIAAC's background questionnaire, where workers report on their training activities in the twelve months prior to the survey.<sup>14</sup> We define a training measure as *firm training* if it involved either organized on-the-job training sessions (e.g., training by supervisors or co-workers) or, if not classified as on-the-job training, was reported to be job-related.<sup>15</sup> We conceptualize training as one instrument in a strategic adjustment process initiated by firms in response to technological change. When automation alters the task structure, firms must decide how to adapt the workforce. This decision typically involves a combination of retraining existing workers, hiring new workers with relevant skills, and reassigning or displacing workers whose skills no longer match the firm's evolving needs. This view aligns with prior work in

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<sup>13</sup>We exclude data for the Russian Federation in our analysis. According to the OECD (2013), data for the Russian Federation are preliminary and are not representative of the entire Russian population because they do not include the population of the Moscow municipal area. Moreover, we treat England and Northern Ireland as one country: the United Kingdom. Thus, our sample has a total of 37 industrialized countries.

<sup>14</sup>Respondents in PIAAC dedicated significant time to the assessment. They spent approximately 25 to 40 minutes completing a detailed background questionnaire, followed by an additional 50 to 60 minutes on the cognitive skill assessment. Data collection was conducted in respondents' homes under the supervision of trained interviewers, utilizing a computer-based application (with a paper version available as an alternative). Each individual assessment began with the background questionnaire and then proceeded to the skill test (OECD, 2019).

<sup>15</sup>The exact questions asked were: (i) "During the last twelve months, have you attended any organized sessions for on-the-job training or training by supervisors or co-workers?" (ii) "Was this activity [respondents could choose between different types of training] mainly job-related?"

the management and economics literature, which emphasizes the complementarity between information technology, workplace organization, and human capital investments (Brynjolfsson and Hitt, 2000; Bresnahan et al., 2002). This view differs from models that treat training primarily as a market-driven re-employment tool for displaced workers.

Table 1 reports summary statistics on firm training. On average, 54.6% of workers participated in training in the year prior to the survey. Training participation varies considerably across countries—from 20.7% in Kazakhstan and 27.3% in Greece, to 71.8% in Finland. Training participation is very similar for women and men (55.2 vs. 54.0%). Moreover, it decreases with age, from 56.2% among those aged 25–34 to 48.8% among those aged 55–65, but increases notably with firm size and workers’ education. In small firms (with ten or fewer employees), only 38.6% of workers participate in training, compared to 71.5% in large firms (with more than 1,000 employees). The education gradient in training participation is similarly pronounced: among workers with lower secondary education or less, only 31.7% participate, whereas participation rises to 69.3% among those with tertiary education.

Table 1: Firm Training Characteristics by Worker Demographics

Subgroup	Participating in Training (%)	Duration of Training		Share by Financing of Training		
	Mean	Median	25th–75th percentile	Fully or partly paid by employer	Not paid by employer	No costs
<b>All workers</b>	54.63	4 days	1–12 days	68.86	16.65	14.49
<b>Age group</b>						
25–34	56.16	5 days	1–14 days	64.62	19.44	15.94
35–44	56.78	4 days	1–12 days	69.70	16.23	14.07
45–54	54.55	4 days	1–10 days	71.19	15.27	13.53
55–65	48.81	3 days	1–8 days	70.96	14.73	14.31
<b>Gender</b>						
Male	54.02	4 days	1–12 days	72.39	14.26	13.35
Female	55.21	4 days	1–11 days	65.64	18.82	15.54
<b>Education level</b>						
Lower secondary or less	31.70	2 days	4 hrs–7 days	67.13	15.54	17.33
Upper secondary and post-secondary, non-tertiary	46.83	3 days	1–8 days	70.39	15.13	14.48
Tertiary	69.31	5 days	2–15 days	68.16	17.76	14.07
<b>Firm size</b>						
1–10 employees	38.60	3 days	1–10 days	62.21	22.41	15.38
11–50 employees	53.92	3 days	1–10 days	68.43	17.20	14.36
51–250 employees	61.22	4 days	1–12 days	70.49	14.99	14.52
251–1000 employees	65.69	5 days	1–14 days	72.44	13.26	14.30
> 1000 employees	71.48	5 days	2–14 days	73.00	13.69	13.30

Notes: This table presents descriptive statistics on training participation, duration, and financing across worker demographics, with data pooled from all countries.

Data source: PIAAC.

Workers in PIAAC are also asked about the duration and financing of training (Table 1).<sup>16</sup> The median duration of firm training is four days, with a wide range from one hour to one entire year (25th percentile: one day; 75th percentile: 14 days).<sup>17</sup> Patterns in training duration mirror those in training incidence: duration is similar across genders, decreases with age, and increases with education and firm size. Overall, 68.9% of training activities are fully or partly employer-financed. Men are more likely than women to participate in employer-financed training (72.4 vs. 65.6%) and less likely to participate in unpaid or no-cost training. Employer-financed training is also more common among older workers and those in larger firms, while financing varies little by education level.

Firm training is distinct from broader categories of adult learning and education, which may include more general education programs, lifelong learning initiatives, or informal skill acquisition. We specifically refer to interventions directly linked to improving performance in one's current job or firm. However, PIAAC also provides information on participation in broader educational measures that are not directly applicable to the workplace or are mainly not financed by the employer (i.e., open/distance education, seminars/workshops, or private lessons). We include non-job-related training as a control variable to capture participants' general motivation to engage in training activities.

Our primary outcome of interest is task use at the workplace, which is captured in PIAAC's background questionnaire. Respondents are asked about the extent to which they engage in various job tasks, such as using accuracy with their hands or fingers (perception manipulation), solving complex problems (creative intelligence), or negotiating with others (social intelligence).<sup>18</sup> Based on these detailed items on task use, we construct a measure of automation risk at the individual level (see Section 3.1 for details).

In addition to examining the effect of training on automation risk, we also consider its impact on hourly wages as a direct measure of labor market success.<sup>19</sup>

PIAAC also assesses respondents' key workplace skills in literacy, numeracy, and problem-solving in technology-rich environments (referred to as digital skills).<sup>20</sup> PIAAC defines these skill measures as follows (OECD, 2013):

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<sup>16</sup>The PIAAC data do not provide information on the content of training programs. However, a follow-up survey to the German PIAAC study (see Section 2.2) asked respondents about training topics. Notably, "Computer or software use, information technology (IT)" was the most frequently reported field (12%), followed by "Security" (10%), "Health care" (8%), and "Communication skills," "Business knowledge," and "Project management" (6% each), suggesting a diverse training landscape with a strong emphasis on IT skills.

<sup>17</sup>Most training takes place during work hours: 75.6% of participants report that their training occurred entirely or mostly during working time.

<sup>18</sup>Responses are provided on a five-point Likert scale: 1 - Never, 2 - Less than once a month, 3 - Less than once a week but at least once a month, 4 - At least once a week but not every day, 5 - Every day.

<sup>19</sup>PIAAC's Public Use File reports hourly wages for Austria, Canada, Germany, Hungary, Sweden, Turkey, and the United States only as a worker's decile rank in the country-specific wage distribution. For Germany, we obtained the Scientific Use File, which provides continuous wage information. For the remaining countries, we follow Hanushek et al. (2015) by assigning the decile median of hourly wages to each survey participant belonging to the respective decile of the country-specific wage distribution. Using wages in coarse categories in some countries is unlikely to affect our results, as Hanushek et al. (2015) demonstrate that using decile medians instead of continuous wages has no substantive impact on their returns-to-skills estimates. Additionally, we trim the bottom and top 1% of the wage distribution in each country to limit the influence of outliers.

<sup>20</sup>These skill measures have been shown to yield considerable earnings returns in all participating countries (e.g., Hanushek et al., 2015, 2017; Falck et al., 2021).

*Numeracy*: ability to access, use, interpret, and communicate mathematical information and ideas in order to engage in and manage the mathematical demands arising from a range of situations in adult life;

*Literacy*: ability to understand, evaluate, use, and engage with written texts to participate in society, to achieve one's goals, and to develop one's knowledge and potential;

*Digital*: ability to use digital technology, communication tools and networks to acquire and evaluate information, communicate with others and perform practical tasks.

As a potential mechanism, we investigate the effect of training on digital skills, given that firm training often focuses on equipping workers with the necessary skills to operate new hardware or software (e.g., [OECD, 2017](#)).<sup>21</sup> In contrast, we treat numeracy skills as a control variable, which accounts for selection into training based on ability.<sup>22</sup> In addition to numeracy skills, we utilize the extensive set of observable worker characteristics provided by PIAAC for entropy balancing, which helps mitigate estimation bias arising from selection into firm training (see Section 4).

To ensure that our results are comparable across outcomes, we limit the sample to workers who provide information on both workplace tasks and wages, allowing our automation and wage regressions to be based on the same sample of workers. The final sample encompasses 91,470 workers.<sup>23</sup>

## 2.2. Country-Specific Extensions

In addition to the international PIAAC dataset, we leverage two country-specific extensions—those from the United States and Germany—to control even more rigorously for selection into firm training than in the broader cross-country analysis.<sup>24</sup> Both countries provide data over time, which allows us to investigate changes in the automation risk and exploit (quasi) individual-level variation within individuals.

*United States.* The United States is the only country that participated in two waves of the PIAAC survey, conducted in 2011/2012 and 2017.<sup>25</sup> Both survey rounds were administered under identical conditions, ensuring consistent data collection. Moreover, the background questionnaire included the same questions in both years, allowing for direct comparisons over time. The U.S. PIAAC data are repeated cross sections: Samples were drawn to be representative of the U.S. population aged 16 to 65 in the respective survey year, with different individuals being sampled in each wave.

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<sup>21</sup>The assessment of digital skills was an optional component of the survey, and Cyprus, France, Italy, and Spain did not participate in this assessment. Additionally, digital skills were only tested for participants who successfully completed an initial computer core test, which measured basic digital competencies such as using a keyboard and mouse or scrolling through text on the screen. For further details, see ([Falck et al., 2021](#)).

<sup>22</sup>PIAAC measures each of these skill domains on a 500-point scale. For exposition, we standardize scores in the regression analyses to have a mean of zero and a standard deviation of one in the estimation sample. Additionally, following [Hanushek et al. \(2015\)](#), we use the first plausible value of the PIAAC scores in each domain throughout.

<sup>23</sup>We also provide results for an unrestricted sample that includes observations without wages, yielding very similar findings.

<sup>24</sup>For consistency with the other PIAAC countries, our international results use only the 2012 wave for the United States and Germany.

<sup>25</sup>For exposition, we consistently refer to the earlier PIAAC wave as the 2012 wave.

*Germany.* In Germany, PIAAC offers a unique longitudinal dimension by re-surveying the sample of German adults who participated in the initial PIAAC assessment in 2011/2012 (referred to as PIAAC-L). This follow-up survey took place approximately 3.5 years later, in 2015, enabling us to track changes in training participation and job tasks over time at the individual level.<sup>26</sup>

From the original 5,379 participants in the 2012 PIAAC wave, a re-taker sample of 3,263 individuals (60.7%) was re-tested in 2015 (Rammstedt et al., 2017; Zabal et al., 2017). Survey re-takers are slightly positively selected in terms of achievement; a pattern also observed in other longitudinal assessment surveys (Martin et al., 2021). However, PIAAC provides sampling weights to adjust the re-taker sample to the general population, ensuring that results are representative of the entire German population.

### 3. Automation Risk

In this section, we describe our approach to constructing an individual-level measure of automation risk. Here, we follow the methodology developed by Nedelkoska and Quintini (2018), who use individual-level PIAAC data to estimate automation risk.<sup>27</sup> We use this measure to study how firm training impacts a worker’s specific risk of automation, rather than relying on occupational measures as most previous studies did. We show that measuring automation risk at the individual level provides new insights regarding workers’ susceptibility to automation and their need to adapt to evolving technological change.

#### 3.1. Measuring Automation Risk at the Individual Level

The construction of our individual-level automation risk measure involves two key steps. First, Nedelkoska and Quintini (2018) utilized expert assessments from Frey and Osborne (2017) to identify occupations at risk of automation, considering factors such as the difficulty of automating tasks that require social intelligence, complex problem-solving, and manual dexterity.<sup>28</sup> They then used a logistic regression model to predict automation risk, with the probability of full automation as the dependent variable and task usage data from PIAAC as the independent variables.<sup>29</sup> This relationship between tasks and automation risk is estimated using Canadian PIAAC data. The Canadian data provide more detailed

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<sup>26</sup>As part of PIAAC-L, Germany also conducted additional follow-up surveys of the original PIAAC participants in 2014 and 2016. However, these surveys did not include detailed questions on job tasks, making it impossible to construct individual-level measures of automation risk from these data.

<sup>27</sup>Both Arntz et al. (2016) and Nedelkoska and Quintini (2018) construct individual-level measures of automation risk using PIAAC task data and highlight substantial within-occupation heterogeneity. While Arntz et al. (2016) focus exclusively on the United States and do not analyze training, Nedelkoska and Quintini (2018) provide correlational evidence whether training incidence varies with workers’ automation risk in the pooled international sample.

<sup>28</sup>The occupational automation risk in Frey and Osborne (2017) is based on the identification of primary engineering bottlenecks encountered by mobile robotics and machine learning developers. To achieve this, Frey and Osborne (2017) conducted interviews with engineering scientists during a 2013 workshop at Oxford University’s Engineering Sciences Department. The scientists were asked to assess whether tasks associated with 70 different occupations could be automated using advanced computer-controlled equipment. Occupations where all tasks were deemed automatable received a value of one, whereas occupations where only some or no tasks could be automated received a value of zero.

<sup>29</sup>See Table B.1 for the exact wording of the task questions.

occupational information and a greater sample size than other PIAAC countries; this enables direct mapping to the occupational classifications used by Frey and Osborne (2017). The coefficients obtained from this regression are then applied to calculate the automation risk of jobs at a more aggregated occupational classification level, both within Canada and for other countries. The coefficients from this regression, shown in Table B.2, reflect each task’s contribution to the overall automation risk. For instance, tasks involving influencing others, solving complex problems, or negotiating, tend to reduce an individual’s automation risk. Conversely, tasks such as solving simple problems or using manual dexterity increase the risk.

To construct an individual-level automation risk measure, we apply the coefficients from Table B.2 as weights to the task usage values reported by workers in PIAAC. We then sum these weighted values across all relevant job tasks and input the sum into a logistic function to predict each worker’s individual automation risk. This results in a score ranging from zero (low probability of full automation) to one (high probability of full automation).<sup>30</sup>

*Automation Risk in 2015 for Germany.* We exploit the longitudinal dimension of the German PIAAC (see Section 2.2) to study changes in automation risk at the individual level over the roughly 3.5 years between the two survey waves. A practical complication is that *none* of the task-use items used to construct our 2012 automation index is repeated verbatim in the 2015 questionnaire. This rules out constructing an index based on a common subset of items across waves. However, the 2015 survey contains several job-task measures closely related to routine or automatable work (e.g., solving difficult problems, carrying out short, repetitive tasks, organizing one’s own work; see Table B.3). Based on these items, we proceed as follows.

First, using the 2012 data, we aggregate our automation risk measure to the 4-digit ISCO level and define a binary indicator that equals one if the occupation’s average risk is at least 0.5 and zero otherwise. We then estimate a logit model of this occupation-level indicator on the full set of 15 individual task-use items observed in the 2015 survey, linking respondents by their 2015 occupations. The estimated coefficients map the 2015 task content into the 2012 automation concept: tasks such as solving difficult problems, dealing with unexpected situations, and checking others’ work load negatively on automation risk, while obtaining detailed task specifications, carrying out short repetitive tasks, and having very similar workdays load positively (see Table B.4).

Second, we use these estimated coefficients to generate an individual-level automation score for each 2015 respondent. Concretely, for each person we take the constant from the linking model, add each estimated coefficient multiplied by that person’s reported task-use value, and sum across all tasks to obtain a single index value. We then pass this index through the same logistic transformation used for the 2012 measure, which maps the index onto the 0-1 scale. The resulting number is the respondent’s predicted probability of full automation risk in 2015, with higher values indicating higher risk.

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<sup>30</sup>The logistic function ensures that our predicted individual-level automation risk lies between zero and one.



The resulting 2015 scores correlate with the 2012 measure at  $\rho = 0.48$  at the individual level. Note that a correlation below one between the two automation risk measures is expected for at least three reasons. (i) Self-reported task-use measures contain nontrivial measurement error; even repeated items answered by the same respondents within short intervals exhibit correlations well below one (e.g., Choi et al., 2014). (ii) Regression to the mean attenuates cross-wave correlations: respondents reporting extreme values in 2012 tend to report more moderate values in 2015 even absent true change. (iii) Genuine occupational upgrading between 2012 and 2015—via job redesign, digital tool adoption, or task reallocation—can alter within-occupation task profiles. Consistent with idiosyncratic noise driving much of the attenuation, when we collapse both indices to the 2-digit ISCO level the correlation rises to  $\rho = 0.74$ .<sup>31</sup> We view this as reassuring evidence that the 2012 and 2015 measures capture the same underlying construct despite differences in survey instruments. We also show that our results are robust to a leaner 2015 automation index based on seven core task items, consistent with task-based models of technological change (see Section 5.2).

### 3.2. Descriptive Patterns in Automation Risk

Figure 1 illustrates automation risk across and within occupations. This automation risk reflects an individual worker’s probability of automation based on his/her job tasks. A value of zero suggests that the worker’s tasks make the job entirely secure from automation, while a value of one means that the worker is fully at risk of being automated. We observe a familiar pattern across occupations: Cognitive non-routine occupations exhibit the lowest average automation risk; for example, less than one-third of the tasks performed by production and service managers or teaching professionals are fully automatable. In contrast, manual routine occupations face the highest risk of automation, with food preparation assistants at the extreme end—where the likelihood of their tasks being fully automated is as high as 61 percent. Across all occupations and countries in our sample, the average automation risk lies at 46 percent.<sup>32</sup>

More notably, however, there is considerable variation in automation risk *within* occupations, indicating that workers in the same occupation perform tasks with varying degrees of automatability. For instance, food preparation assistants at the fifth percentile of automation risk are as automatable as the average production and service manager. In other words, 5% of workers in the most automatable occupation face an automation risk as low as the average risk in the least automatable occupation. Similarly, when we consider production and service managers with a higher-than-average automation risk—say, those at the 75th percentile—11% of food preparation assistants have a comparable automation risk. These examples illustrate a substantial overlap in the automation risk faced by individual workers, even when comparing occupations with the lowest and highest average risks.

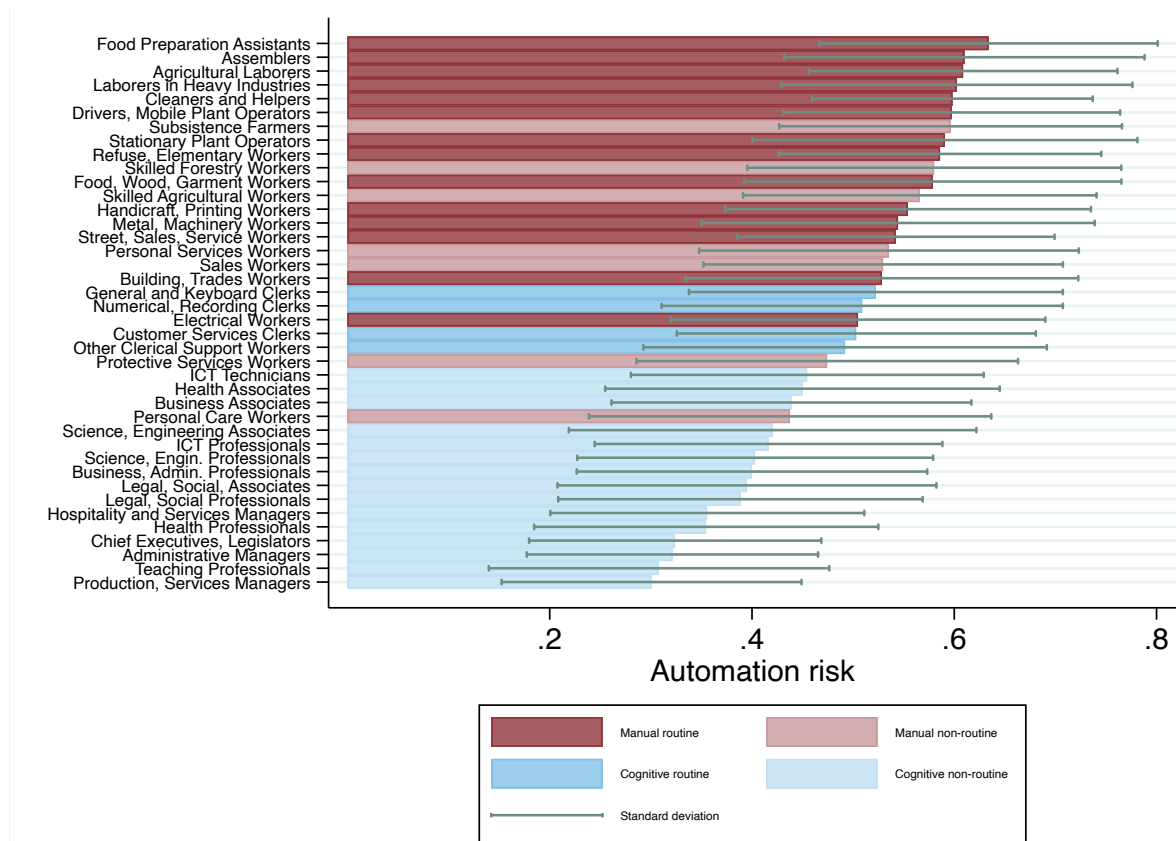
Another way to show the amount of variation in within-occupation automation risk in Figure 1 is by examining the standard deviations. We find that the standard deviation of automation risk in the

<sup>31</sup> Autor and Handel (2013) also note that measurement error is larger at the person level than at the occupation level.

<sup>32</sup> Consistent with this interpretation, our occupation-level automation risk correlates strongly with the robot-exposure index by Webb (2020) ( $\rho \approx 0.78$  pooled;  $\rho \approx 0.72$  for the U.S.), validating that the measure captures exposure to routine, programmable automation. See Figure A.1 for details.

occupation with the highest average automation risk, food preparation assistants, is 17 pp, while it is 14 pp in the occupation with the lowest average automation risk, production and service managers. Generally, substantial within-occupation variation in automation risk is evident across *all* occupations, indicating that this variation is not dependent on an occupation's average automation risk. In Figure A.2, we plot the densities of our individual-level automation risk across occupations and reveal that there is a substantial mass of workers in each occupation with an automation risk lying outside the mode of their respective occupations. These comparisons underscore that focusing solely on occupational averages would obscure significant heterogeneity and potentially lead to misleading conclusions about workers' actual automation risk based on the tasks they perform.<sup>33</sup>

Figure 1: Automation Risk Across and Within Occupations



*Notes:* The figure shows the average automation risk by two-digit ISCO occupation across all countries in our sample—except Austria, Canada, Estonia, and Finland, which report occupational information only at the one-digit level in the PIAAC Public Use File. Whiskers indicate one standard deviation from the mean (see Section 3.1 for details on the construction of the automation risk measure). Classification of occupations by routine intensity according to Cortes (2016).  
*Data source:* PIAAC.

<sup>33</sup> Our individual-level automation risk measure is a strong predictor of workers' wages, underscoring its economic relevance. Figure A.4 shows that workers in jobs less susceptible to automation tend to earn higher wages, even after controlling for country, industry, and occupation fixed effects. Thus, even within occupations, there is a negative relationship between automation risk and wages.

We also document notable shifts in the composition of tasks over time. For this analysis, we take advantage of the fact that both the United States and Germany were surveyed twice in PIAAC (see Section 2.2). Across both countries, Figure A.3 shows that occupations with a higher average automation risk in 2012 were subject to a more substantial decrease in automation risk over time.<sup>34</sup> We interpret this as evidence for deliberate employer action. It should be noted, however, that the pattern might partly arise mechanically, that is, occupations with initially high automation risk have more scope for subsequent reductions.

Moving to the individual level allows us to observe changes across and within occupations. Table B.5 indicates that individuals' automation risk in 2012 is negatively correlated with the subsequent change in automation risk (column 1), supporting the occupation-level evidence from Figure A.3. Interestingly, this negative relationship becomes even stronger within occupations (column 2), suggesting that workers in the same occupation converge in their automation risk. Including a comprehensive set of individual-level controls for 2012 (see Section 4) hardly has any effect on the results (column 3).

The observation that automation risk is converging both across and within occupations can help to explain recent findings in the literature. For example, Bachmann et al. (2022) and Boehm et al. (2024) demonstrate that, despite the decline of routine-intensive jobs in Germany, the wages of workers who remain in these occupations have not decreased. One interpretation is that these remaining workers who continue to be employed in more automatable, declining occupations may be positively selected based on their productivity. Moreover, those who remain in these occupations may be performing tasks that are less susceptible to automation, and thus cannot be replaced by current technologies. An alternative, complementary explanation, highlighted by Autor and Dorn (2009), is that automating occupations “age” by reducing the hiring of new, typically lower-paid entrants who perform routine-intensive tasks. In this view, wage stability in declining occupations reflects a compositional shift within the workforce, with fewer young or newly hired workers assigned routine tasks and more experienced workers retained to perform less automatable tasks. This mechanism suggests that technological adjustment entails within-occupation restructuring of tasks and workforce composition, rather than a uniform decline in the demand for routine-intensive jobs.

Further supporting the relevance of within-occupational transitions, Atalay et al. (2020) document a substantial shift from routine to non-routine interactive and analytical tasks in the United States between 1950 and 2000—much of which occurred within narrowly defined job titles. Similarly, Battisti et al. (2023) show that technological and organizational changes within German firms reduce the share of routine jobs, but do not necessarily result in higher unemployment or reduced wage growth for affected workers. Instead, many affected workers transition to more abstract roles within the same firm, particularly in environments with strong training programs and union representation.

Overall, our descriptive findings indicate that task demands—and consequently, automation risk—are evolving within occupations, highlighting the importance for workers to adapt to these changing

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<sup>34</sup>However, Panel (b) indicates that this negative relation is weaker in the German sample, where the same individuals were re-surveyed. The United States sampled a different population for its 2017 survey.

requirements. This paper investigates whether participation in firm training enables workers to perform tasks that are complementary to automation technologies, thereby reducing their risk of automation and increasing their wages.

## 4. Empirical Strategy

### 4.1. Basic Empirical Model

The relationship between firm training and labor market outcomes can be modeled using the following OLS regression:

$$Y_i = \alpha + \beta_1 \text{firm\_training}_i + \varepsilon_i, \quad (1)$$

where  $Y_i$  represents the outcome of interest for individual  $i$ . Our two main outcome variables are the automation risk and log hourly wages. The key explanatory variable is the indicator  $\text{firm\_training}_i$ , which equals one if a respondent participated in job-related training—such as organized sessions or training by supervisors or co-workers—within the twelve months prior to the survey, and zero otherwise (Section 2).

In this specification,  $\beta_1$  is the coefficient of interest, capturing the association between firm training and our outcomes. However, estimating  $\beta_1$  using the naive approach outlined in Equation 1 only yields correlational evidence due to potential bias from omitted variables.<sup>35</sup> Specifically, we are concerned about selection into training, that is, the possibility of unobserved factors influencing both the likelihood of receiving training and the outcomes. For example, if workers with higher innate ability are more likely to receive firm training, then the estimate of  $\beta_1$  for wages would be upward biased since more able workers also tend to earn higher wages. Similarly, if more able workers worked in less automatable jobs,  $\beta_1$  would be downward biased in the analysis of automation risk. This means that a simple comparison between workers with and without training is likely to overestimate the causal effect of training if the selection problem is not adequately addressed; this is particularly challenging in cross-sectional data.

We employ several strategies to address these concerns.<sup>36</sup> First, we leverage our rich survey data to include an extensive set of control variables, including a direct measure of ability. Second, we exploit within-occupation variation and apply entropy balancing (Hainmueller, 2012), a reweighting technique that ensures exact covariate balance between the treatment group (those who received training) and the

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<sup>35</sup>Reverse causality is mitigated in our setting as individuals are asked about training measures that they completed in the twelve months preceding the outcome assessment.

<sup>36</sup>The existing literature addresses selection bias through the following approaches: The first involves instrumental variable approaches, yet this method struggles with the validity of the instruments (Bartel, 1995). The second, more common, approach uses fixed-effects regressions to control for unobserved individual-level heterogeneity (see e.g., Greenhalgh and Stewart, 1987; Lynch, 1992; Parent, 1999; Frazis and Loewenstein, 2005). Third, a few studies (e.g., Leuven and Oosterbeek, 2008; Goerlitz, 2011) adopt a quasi-experimental approach by using random reasons for non-participation in training among those participants who planned to participate to create treatment and control groups. They find that conventional methods are likely to overestimate the returns to training.

control group (those who did not).<sup>37</sup> Entropy balancing adjusts the weights so that the first and second moments of covariates potentially correlated with treatment status are identical across groups. This substantially reduces concerns about residual imbalance that could bias the estimated training effects. We also conduct a large series of robustness checks using alternative estimation approaches, including individual fixed effects models, which account for time-invariant unobserved heterogeneity (see Section 5.2 for details).

#### 4.2. Selection into Firm Training

Using PIAAC’s detailed background questionnaire and the availability of our key variables at the individual level, we augment the basic OLS model by estimating the following specification:

$$Y_{icoj} = \alpha + \beta_1 \text{firm\_training}_i + \beta_2 \text{numeracy}_i + \mathbf{X}_i' \gamma + \delta_{c(i)} + \zeta_{o(i)} + \eta_{j(i)} + \varepsilon_i, \quad (2)$$

where  $Y_{icoj}$  represents the outcome of interest for individual  $i$  who lives in country  $c$  and works in occupation  $o$  and industry  $j$ . We include country fixed effects ( $\delta_c$ ) to account for differences in firm training provision and the general quality of firm training programs across countries. Additionally, two-digit industry fixed effects ( $\eta_j$ ) are incorporated to control for variation in training frequency and effectiveness across industries.<sup>38</sup> As outlined in Section 2.1, the frequency of firm training varies by country and occupation. To account for within-occupation and within-country correlation, we estimate Equation 2 using two-way clustered standard errors at the occupation and country levels. This represents the most conservative approach, as alternative assumptions about the variance-covariance matrix yield smaller standard errors (see Table B.6).

A key innovation of our study is the use of an individual-level automation risk measure, based on detailed task data rather than aggregate occupational classifications (for details, see Section 3.1). This is crucial, as occupations can vary significantly in their task composition, and hence in their average automation risk (e.g., Acemoglu and Autor, 2011; Goos et al., 2014).<sup>39</sup> At the same time, both the demand for training (Lergetporer et al., 2023) and the take-up of training (Nedelkoska and Quintini, 2018) varies considerably across occupations. By exploiting within-occupation variation due to the inclusion of occupation fixed effects ( $\zeta_o$ ), we account for unobserved occupation-specific factors that simultaneously influence both automation risk and the likelihood of receiving firm training.

<sup>37</sup> Heckman et al. (1997, 1998) and Dehejia and Wahba (2002) propose matching estimators to construct counterfactual comparison groups. Smith and Todd (2005) evaluate a range of non-experimental estimators of training effectiveness and find that a matching difference-in-differences estimator performs best.

<sup>38</sup> For four countries (Austria, Canada, Estonia, and Finland), occupation and industry codes are available only at the one-digit level, so we include one-digit fixed effects there and two-digit fixed effects elsewhere. In countries that generally report two-digit codes, a small share of workers (about 2%) lack two-digit information; for these cases, we fall back to one-digit codes when available. Only 3% of workers provide no occupation or industry information and are therefore assigned to a “missing” category. Results are very similar when restricting the sample to observations with complete two-digit codes, and likewise when further restricting to countries with four-digit detail.

<sup>39</sup> This variation is illustrated in Figure 1 and Figure A.2.

Another innovation of our analysis is the introduction of numeracy skills as a control variable in the firm training literature. Numeracy skills provide a more accurate measure of human capital compared to previously used proxies such as years of schooling (Lynch, 1992; Arulampalam and Booth, 1997; Leuven and Oosterbeek, 1999; Bassanini et al., 2007), which have several notable limitations (for a discussion, see Hanushek and Woessmann, 2008). For instance, educational quality varies across time and countries, which is not captured by simply counting the number of school years acquired. Moreover, educational attainment measures are coarse, as individuals within the same attainment category often vary greatly when it comes to their actual human capital (Langer and Wiederhold, 2023). Furthermore, they reflect human capital only at the end of formal education, failing to account for changes in human capital throughout an individual’s working life. These limitations are particularly problematic in an international setting and in the context of rapidly changing labor markets where the relevance of specific skills may evolve over time.<sup>40</sup>

The model also includes a broad set of control variables included in the vector  $\mathbf{X}_i'$ , which have frequently been applied in the training literature (Oosterbeek, 1996, 1998; Lynch and Black, 1998; Grund and Martin, 2012). These covariates encompass standard socio-economic factors potentially related to training participation, such as educational attainment, age, gender, migration status, parental education, and whether the respondent has children. Since workers in larger firms and those who are full-time employed generally receive more training, we further include controls for firm size and full-time employment status (Fouarge and Schils, 2009).<sup>41</sup> We also account for self-organized training—activities initiated by the worker rather than the employer—to control for differences in motivation and effort between workers who generally participate in training and those who do not. This variable contains all training measures that are not job-related (e.g., self-organized training or seminar participation).

*Imputed past automation risk (United States).* We make use of the repeated cross-sections in PIAAC for the United States in the years 2012 and 2017 and impute an individual-level past automation risk for workers in the 2017 United States sample. We then include this imputed past automation risk as an individual-level control in a regression of Equation 2 for the 2017 United States sample.

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<sup>40</sup>Note that numeracy skills are assessed concurrently with our outcome measures in PIAAC. If numeracy skills also improve as a result of firm training, our estimates of training effectiveness should be interpreted as lower bounds. To test this, we estimate the effect of training on numeracy skills residualized for literacy skills—that is, the portion of numeracy skills not explained by a worker’s general ability (Table B.7, column 1). The estimate is close to zero and not statistically different from zero, suggesting that firm training does not affect genuine numeracy skills. Contrarily, we find positive and significant estimates for residualized digital skills (Table B.7, column 2), suggesting that firm training improves digital skills beyond what can be attributed to fixed general ability.

<sup>41</sup>More precisely,  $\mathbf{X}_i'$  includes the following variables: years of schooling, age in four categories (25–34, 35–44, 45–54, and 55–65 years), gender, migration status in three categories (first-generation and second-generation migrant and native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary or post-secondary/non-tertiary, and at least one parent has attained tertiary), an indicator whether the respondent has children, the age group of the oldest child in four categories (0–2, 3–5, 6–12, and 13+ years), an indicator of full-time employment, and firm size as measured by the number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+ employees). In rare cases, variables have missing values. For example, approximately 1% of respondents lack data on years of schooling. For continuous control variables, we impute missing values with the variable mean and add a variable indicating missing values. For discrete control variables, we handle missing values by creating an additional “missing” category.

Specifically, we impute the past individual-level automation risk by running the following regression for the no-training group of the 2012 sample in the United States:

$$Y_{i,2012} = \alpha + \theta \text{numeracy}_{i,2012} + \mathbf{X}'_{i,2012}\gamma + \delta_{o(i),2012} + \eta_{j(i),2012} + \varepsilon_i, \quad (3)$$

where  $Y_{i,2012} \in [0, 1]$  is the individual-level automation risk.  $\text{numeracy}_{i,2012}$  denotes the standardized numeracy score and the vector  $\mathbf{X}_{i,2012}$  contains the same controls as in the baseline international model in Equation 2 (excluding country FE). Additionally,  $\delta_{o(i),2012}$  and  $\eta_{j(i),2012}$  denote two-digit ISCO occupation and ISIC industry fixed effects, respectively. Standard errors are clustered at the two-digit occupation level.<sup>42</sup>

*Pseudo-panel (United States).* We further make use of the repeated cross-sections in PIAAC for the United States in the years 2012 and 2017 to construct a pseudo-panel by matching individuals in 2017 to individuals in 2012 based on observable characteristics (for a similar approach, see Kleven et al., 2024). We expand the 2012 matching pool to include both the United States and Canada to ensure adequate cell sizes. Matches are formed on a rich but tractable set of characteristics, all dated to 2012: gender; age (25–34, 35–44, 45–54, 55–65); education (<9, 9–12, 13–16, >16 years); migration status (first-generation, second-generation, native); parental education (neither parent upper secondary; at least one parent secondary/post-secondary non-tertiary; at least one parent tertiary); numeracy quintile. Using these matching variables strikes a balance between methodological rigor (i.e., number and level of detail of matching variables) and a sufficient number of matches.

Let  $\phi_{m(i)}$  denote the *match (pair) fixed effects* linking each 2017 respondent to a matched 2012 surrogate. We estimate:

$$Y_{it} = \phi_{m(i)} + \lambda_t + \beta \text{firm\_training}_{it} + \theta \text{numeracy}_{it} + \mathbf{Z}'_{it}\gamma + \varepsilon_{it}, \quad (4)$$

for  $t \in \{2012, 2017\}$ , where  $Y_{it} \in [0, 1]$  is individual automation risk.  $\lambda_t$  are wave fixed effects,  $\text{numeracy}_{it}$  denotes the standardized numeracy score included as a time-varying control, and the vector  $\mathbf{Z}_{it}$  contains additional time-varying controls (age of the oldest child, firm-size dummies, industry, occupation, and an indicator whether the observation is matched from the Canadian or U.S. PIAAC sample). The match fixed effects absorb all time-invariant differences within each matched pair. Standard errors are clustered at the match level.

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<sup>42</sup>We impute time-varying controls, such as age, in the 2017 sample to their 2012 values to predict the 2012 automation risk



*Value-added specification (Germany).* Using the 2012 and 2015 wave of the German PIAAC data, we estimate the following value-added model regressing automation risk in 2015 on 2014 firm training and 2012 covariates:

$$Y_{i,2015} = \alpha + \beta \text{firm\_training}_{i,2014} + \rho Y_{i,2012} + \theta \text{numeracy}_{i,2012} + \mathbf{X}'_{i,2012}\gamma + \delta_{o(i),2012} + \eta_{j(i),2012} + \varepsilon_i, \quad (5)$$

where  $Y_{i,t} \in [0, 1]$  denotes individual automation risk. We use the same control variables as in the international analysis, all taken from the 2012 PIAAC data (i.e., numeracy skills, years of schooling; age group; gender; migration; parental education; children and oldest child's age group; non-job-related training; full-time; firm-size dummies). We further include two-digit occupation fixed effects  $\delta_{o(i)}$  and two-digit industry fixed effects  $\eta_{j(i)}$ , also for 2012. Standard errors are clustered at the two-digit occupation level.

*Panel fixed-effects specification (Germany).* Making use of the panel structure in PIAAC Germany—where the same respondents are surveyed in 2012 and 2015—we also estimate a two-wave model with individual and time fixed effects:

$$Y_{it} = \alpha_i + \lambda_t + \beta \text{firm\_training}_{it} + \mathbf{Z}'_{it}\gamma + \delta_{o(i,t)} + \eta_{j(i,t)} + \varepsilon_{it}, \quad (6)$$

for  $t \in \{2012, 2015\}$ , where  $\alpha_i$  are individual fixed effects,  $\lambda_t$  wave effects, and  $\text{firm\_training}$  indicates job-related training in the past 12 months. The time-varying controls  $\mathbf{Z}_{it}$  include age, full-time status, and firm-size dummies; we additionally include contemporaneous two-digit occupation fixed effects  $\delta_{o(i,t)}$  and two-digit industry fixed effects  $\eta_{j(i,t)}$ . Standard errors are clustered at the individual level.

*Sample composition.* As our analysis focuses on firm training, it captures how training affects workers who remain integrated in the labor market. Workers who left employment, potentially due to automation, are not observed, so our results reflect the impact of training within ongoing employment relationships rather than across the entire pool of affected workers. This focus allows us to understand how training functions as an adaptation mechanism within firms rather than as a re-employment tool.<sup>43</sup> Accordingly, our estimates capture the effects of training among workers who are at risk of automation and remain employed, a population directly relevant for understanding firms' adjustment strategies.

If firms selectively train some workers while displacing others, the omitted (displaced) group likely received no training and performed more automatable tasks. Such workers are not an appropriate control group for workers who did receive training, as they likely differ fundamentally in their propensity to receive firm training (e.g., due to worker characteristics that predict training returns, such as expected performance, tenure, health, etc.). However, including them would likely inflate the automation risk of the non-trained group, implying that our estimates are likely conservative. Consequently, the estimated

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<sup>43</sup> Active labor market training programs target unemployed or displaced workers and are designed to facilitate re-employment rather than to upgrade tasks within existing jobs (Card et al., 2010, 2018); they are therefore conceptually distinct from the firm training analyzed here.

reduction in automation risk should be interpreted as a lower bound of the impact of training, given our focus on workers who remain employed.

### 4.3. Entropy Balancing

To address potential biases in estimating the effect of firm training on labor market outcomes, we apply entropy balancing ([Hainmueller, 2012](#)). Entropy balancing is a covariate reweighting technique designed to achieve exact balance on specified moments (typically the mean and variance) of covariates between treated and control groups in observational studies. It adjusts the weights of control group observations so that their covariate distribution matches that of the treatment group, thereby minimizing bias from observable differences.<sup>44</sup>

A key identification concern is that firm training is not randomly assigned. For example, better-educated or higher-skilled workers are more likely to receive training. Simply including such covariates in a linear regression does not guarantee that treatments and control groups are comparable, particularly if the relationship between covariates (e.g., education, skills) and outcomes (e.g., automation risk, wages) is non-linear. While matching techniques like propensity score matching avoid strong functional form assumptions, they achieve covariate balance only indirectly—by equating the distribution of estimated treatment probabilities.<sup>45</sup> In contrast, entropy balancing directly imposes exact balance on the covariates themselves. This approach thus reduces reliance on model specification and helps mitigate bias in non-experimental settings like ours.

In our analysis, we balance the first and second moments of all covariates included in [Equation 2](#) between treatment and control workers. The core identifying assumption is that selection into firm training is based on observables—that is, all relevant determinants of training participation are included in the model.<sup>46</sup> While this assumption cannot be tested directly, the inclusion of numeracy skills—a strong proxy for individual ability—combined with a rich set of other covariates and the use of within-occupation variation gives us confidence that we have accounted for the main drivers of selection into training.

[Table B.8](#) provides the full balancing table for our covariates. Before weighting, workers without training tend to have lower numeracy skills, are, on average, younger, less educated, and more likely to have parents with lower levels of education. They are also less likely to be employed full-time and work more often in smaller firms. After applying entropy balancing, however, the covariate distributions between workers with and without training are perfectly aligned across all these dimensions, demonstrating that the balancing exercise successfully eliminated the substantial initial differences between the two groups.

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<sup>44</sup>We implement entropy balancing by using the *ebalance* command in Stata ([Hainmueller and Xu, 2013](#)). In our empirical analysis, entropy balancing weights replace weights that give each country the same weight.

<sup>45</sup>Even if propensity scores are well-matched, residual imbalances in individual covariates can remain (especially with limited overlap or poor model fit).

<sup>46</sup>See [Cunningham \(2021\)](#) for a recent discussion.

## 5. Results

### 5.1. Training and Automation

This section investigates the relationship between firm training and individual-level automation risk, our main outcome of interest. [Table 2](#) presents the main findings. The columns progressively introduce more rigorous specifications to assess the association between firm training and automation risk. Column (1) shows the raw correlation within countries and industries, revealing that firm training is associated with an 8.2 pp reduction in automation risk. However, the coefficient on training decreases to  $-4.8$  pp when occupation fixed effects are included to control for unobserved occupation-specific characteristics that might influence both training and automation risk (column 2). The inclusion of occupation fixed effects also significantly enhances the model’s explanatory power, as indicated by the increase in the  $R^2$  from 0.12 to 0.24—essentially doubling the share of explained variation in automation risk. This suggests that a considerable amount of the variation in both occupational training and automation risk is at the between-occupation level, which we can account for with our measure of automation risk at the individual level by including occupation fixed effects. Importantly, even when analyzing within-occupation variation, the training coefficient remains sizable and precisely estimated.<sup>47</sup>

Including numeracy skills to control for unobserved ability in column (3) slightly reduces the size of the firm training estimate, though not by much. Notably, when the full set of socio-demographic and work-related control variables is added in column (4) and entropy balancing is applied in column (5), the estimated coefficient on firm training remains nearly identical to that in column (3).<sup>48</sup> This consistency suggests that occupational selection and numeracy skills effectively act as a “sufficient statistic” for other socio-demographic and work-related differences between workers with and without training.<sup>49</sup>

In the most rigorous specification presented in column (5) of [Table 2](#), we find that firm training reduces the automation risk by 3.8 pp. In terms of magnitude, this coefficient corresponds to approximately one-fifth of a standard deviation in individual automation risk within our sample or 8% of the mean automation risk. To further contextualize the magnitude of the training effect, the estimated training effect is equivalent to the difference in automation risk between ICT professionals (0.40) and ICT technicians (0.44) or between business administration professionals (0.39) and business administration associates (0.43), where professional occupations in the same field are typically characterized by a higher degree of complex tasks and consequently lower automation risk. To provide an example for the magnitude of the training effect within the same occupation, training would reduce the automation risk of metal and

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<sup>47</sup>In our main specification, we control for occupations at the two-digit level, as this classification is consistently available across PIAAC countries (except Austria, Canada, Estonia, and Finland, for which we use the available one-digit occupations). [Table B.9](#) presents estimates that include occupation fixed effects at the more detailed four-digit level for the 28 countries where such information is available. Reassuringly, the training estimates remain very similar to those in the baseline specification, supporting our strategy of using two-digit occupation controls to maximize sample size (see columns 5 and 6 of [Table B.9](#)).

<sup>48</sup>The coefficients in columns (4) and (5) are not statistically different from the coefficient in column (3) at conventional levels. Columns (1) through (4) use weights that give each country the same weight.

<sup>49</sup>Our main analysis restricts the sample to respondents that do not have missing information on both automation risk and wages (see [Section 2](#)). However, our results are very similar in a more complete sample that also has respondents with missing wage information ([Table B.10](#)).

Table 2: Training and Automation Risk

	Automation risk				
	(1)	(2)	(3)	(4)	(5)
Firm training	-0.0816*** (0.0017)	-0.0481*** (0.0034)	-0.0447*** (0.0032)	-0.0380*** (0.0028)	-0.0377*** (0.0023)
Numeracy Skills			-0.0188*** (0.0018)	-0.0098*** (0.0015)	-0.0091*** (0.0022)
Occupation FE		X	X	X	X
Further controls				X	X
Entropy balancing					X
$R^2$	0.12	0.24	0.25	0.26	0.24
Observations	91,470	91,470	91,470	91,470	91,470

Notes: Ordinary least squares estimation in columns (1)–(4) with weights such that each country has the same weight, least squares estimation with weights from entropy balancing in column (5). Dependent variable: individual-level automation risk, ranging from 0 (indicating a low probability that a worker is fully automated) to 1 (indicating a high probability that a worker is fully automated); automation risk is predicted using items on task use at work from PIAAC. Sample: employees aged 25–65 years with information on automation risk and log hourly wages. Firm Training: binary variable indicating whether the respondent participated in on-the-job training or job-related training in the 12 months prior to the survey. Numeracy skills are standardized to unit standard deviation across countries. Further controls include: years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (i.e., open/distance education, seminars/workshops, or private lessons), an indicator for full-time employment, and firm size measured by the number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+). Occupation fixed effects at the two-digit ISCO level. All regressions also control for industry (two-digit ISIC) and country fixed effects. Occupation and industry fixed effects are included at the one-digit level for Austria, Canada, Estonia, and Finland, where the PIAAC Public Use File does not report two-digit ISCO/ISIC codes. All control variables were used for the entropy balancing in column (5).  $R^2$  refers to within-country  $R^2$ . Standard errors shown in parentheses are two-way clustered at the country and occupation level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Data source: PIAAC.

machinery workers (who are at the median of the automation risk distribution) from 58.4% to 54.6 %, a decrease of seven percentiles.

Since PIAAC is an individual-level survey, our ability to control for firm characteristics is limited. However, we can approximate firm fixed effects in the data by adding region×industry×firm size fixed effects. Table B.11 presents estimates using this approach in the 30 countries where detailed regional data are available. Reassuringly, the estimates remain robust even in this more stringent specification. Thus, we are confident that our identifying assumption—namely, that our entropy balancing approach adequately accounts for all variables influencing selection into training—is valid.<sup>50</sup> Below, we provide additional robustness checks on this assumption, using coefficient bounds for unobservable selection as well as country-specific repeated cross-sectional and panel data.

<sup>50</sup>Moreover, Diaz et al. (2025) exploit within-firm variation in the assignment of managers to worker teams and workers' training take-up, highlighting the role of managers in workers' participation in firm training. Crucially in their setting, managers are rotated across teams independently of performance and other team and worker characteristics, suggesting exogenous variation in the propensity of training take-up *within firms* driven by idiosyncrasies in managers' preferences.

## 5.2. Robustness

While we utilize rich individual-level data to account for selection into training, PIAAC’s cross-sectional data do not provide information on pre-training automation risk. This limitation leaves room for potential selection bias if workers with either higher (or lower) pre-training automation risk are more likely to receive training. For example, firms might prefer to invest in training for workers already engaged in less automatable tasks, as the cost of additional training for these workers might be lower. Conversely, firms might choose to train those with a higher pre-training automation risk in order to ensure their skills remain aligned with evolving production technologies.

In this section, we conduct several robustness checks to demonstrate that unobserved selection into training is unlikely to bias our results. Below, we first apply Oster bounds to assess the influence of unobserved variables in the international analysis. Furthermore, we exploit the repeated U.S. PIAAC data to impute workers’ past automation risk at the individual level and construct a matched pseudo-panel. Finally, we leverage the German PIAAC panel data to estimate value-added and panel models. Each of these approaches consistently shows that training significantly reduces the risk of automation.

*Unobservable Selection and Coefficient Bounds.* In order to address concerns about selection on unobservables, we apply the coefficient bounds methodology proposed by [Oster \(2019\)](#). Intuitively, this approach allows us to assess how the estimated training coefficient would change if we could account for unobserved variables. The method is based on two key parameters: the relative degree of selection on unobservables (denoted as  $\delta$ )<sup>51</sup> and the maximum possible R-squared ( $R_{max}$ ) that could be achieved if all relevant variables, including unobserved ones, were included in the model.

The intuition behind the Oster bounds lies in the idea that coefficient stability, when introducing additional controls in a hypothetical “long” regression that includes unobservables, can provide insights into the potential bias from unobserved variables. If the coefficient remains stable when additional controls are included, it suggests that the unobserved variables do not meaningfully bias the estimate. By scaling the observed movements in the coefficient with changes in R-squared, the Oster bounds provide an estimate of how much the coefficient would change when accounting for unobservables, given a specified hypothetical maximum R-squared from adding observables and unobservables,  $R_{max}$ , as a factor of  $\tilde{R}$ , the R-squared of our baseline regression in column (5) of [Table 2](#).

In our analysis, the coefficients on firm training remain sizable and highly statistically significant for a range of plausible assumptions on  $\delta$  and  $R_{max}$  ([Table B.13](#)). For instance, when following the recommendation in [Oster \(2019\)](#) and the application in [Chen \(2021\)](#) to set  $\delta = 1$  and  $R_{max} = 1.4$ , the training coefficient is still at 0.0311, with a standard error of 0.0019. Even when  $\delta$  is set to 1.2—assuming unobservable factors are 20% more influential than observable ones—the training coefficient only slightly decreases and remains highly significant. This coefficient stability indicates that our training estimates are robust to potential omitted variable bias from unobserved factors.

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<sup>51</sup>For instance,  $\delta = 1$  assumes that unobservable factors were as influential as observable ones in determining the outcome.

*U.S.-Specific Analysis.* Additionally, we make use of the repeated cross-sectional PIAAC data for the United States in the years 2012 and 2017. In this U.S. analysis, we use the no-training group from 2012 to impute an individual-level past automation risk for both training and no-training workers in the 2017 sample. To achieve this, we impute time-varying variables, such as the respondent’s age or the age of their oldest child, back to their 2012 values.<sup>52</sup> Specifically, we regress automation risk on all covariates included in our baseline model (see column 5 of Table 2), but only for the no-training group in 2012. We then apply the coefficients from this regression to predict the past automation risk of individuals observed in the 2017 sample. Results are shown in Panel A of Table 3. When we add the imputed past automation risk as an individual-level control in column (2), the training coefficient remains virtually unchanged compared to the baseline estimate in column (1).

Additionally, the availability of two waves of PIAAC data in the U.S. allows us to construct a matched pseudo-panel. We draw inspiration from Kleven et al. (2024), who estimated earning losses for women following childbirth using cross-sectional data in the absence of individual-level panel data. In their approach, Kleven et al. (2024) leveraged repeated cross-sections to construct pseudo-panel data by matching individuals across survey waves based on observable characteristics. Matched individuals in earlier cross-sections serve as surrogate past observations for those in later cross-sections. Applying this method to our context, we match respondents from the 2017 PIAAC wave to respondents in the 2012 wave. The matching is performed within cells defined by fixed characteristics of gender, age group, parental and migration background, level of education, and quintiles of numeracy skills.<sup>53</sup> Ultimately, we successfully matched 1,396 observations from the 2017 U.S. sample to corresponding observations from 2012, which serve as surrogate past observations. Using this matched pseudo-panel, we can add individual fixed effects to the analysis (i.e., fixed effects for the matched group). Panel B of Table 3 presents the results. The training estimate using our matched pseudo-panel of  $-3.9$  pp (column 2) is very similar to the cross-sectional estimate in the 2017 U.S. sample (column 1).<sup>54</sup> Notably, the estimate is also consistent with the cross-sectional estimate that includes imputed past automation risk as an individual-level control, shown in column (2) of Panel A.

*Germany-Specific Analysis.* We can even go beyond the pseudo-panel approach using panel data for Germany, which allows researchers to track changes in training participation and job tasks over time at the individual level. By leveraging these data, we can directly observe how training influences automation

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<sup>52</sup>Note that this analysis assumes that workers did not change occupations, industries, or employers between 2012 and 2017.

<sup>53</sup>To ensure a sufficient number of matches within these exact cells, we use the 2012 Canadian PIAAC sample due to its larger number of observations, as Canada is the only PIAAC country that surveyed enough respondents to achieve representativeness at the sub-national level. We conduct two checks to confirm that both countries are comparable in the no-training sample. First, conditional on all characteristics that we include in our entropy balancing, this risk of automation for workers without firm training does not differ between the Canadian and U.S. samples in 2012. Second, in a regression of individual-level automation risk on covariates and country fixed effects, the country fixed effect for Canada is not statistically significant. That is, differences in automation risk between the Canadian and U.S. samples are due to compositional differences with respect to, say, occupations, and not due to fundamental differences in automation risk between both countries.

<sup>54</sup>This refers to the estimate in the 2017 sample that could be matched to corresponding observations from 2012.

Table 3: U.S. Evidence Linking Two PIAAC Waves

Panel A: Controlling for Imputed Past Automation Risk		
	Automation risk (2017)	
	(1)	(2)
Firm training	-0.0427*** (0.0102)	-0.0406*** (0.0088)
Numeracy skills	0.0067 (0.0056)	0.0115* (0.0061)
Imputed automation risk (2012)		1.1081* (0.6487)
Controls	X	X
$R^2$	0.33	0.35
Observations	1,554	1,554

Panel B: Pseudo Panel		
	Automation risk	
	(1)	(2)
Firm training	-0.0333*** (0.0122)	-0.0389*** (0.0085)
Controls	X	Time-varying
Match/Worker FE		X
$R^2$	0.35	0.50
Observations	1,396	2,792

Notes: Dependent variable: individual-level automation risk, ranging from 0 (indicating a low probability that a worker is fully automated) to 1 (indicating a high probability that a worker is fully automated); automation risk is predicted using items on task use at work from PIAAC. Sample: employees aged 25–65 years in the United States, wave 2017 (Panel A) and waves 2012 and 2017 (Panel B). Firm Training: binary variable indicating whether the respondent participated in on-the-job training or job-related training in the 12 months prior to the survey. Panel A: Imputed automation risk 2012: calculated using the coefficients obtained from a regression that includes all the covariates used in entropy balancing (see main text) within the group that did not receive any training in the 2012 sample. Age in 2012 for the 2017 sample is imputed from the 2017 values. Panel B: Sample in pseudo-panel consists of individuals surveyed in the 2017 U.S. survey wave and matched individuals in U.S. and Canada in 2012 survey wave. Matching based on the set of fixed characteristics in our control variables: gender, age group in four categories (25–34, 35–44, 45–54, 55–65), level of education in four categories (less than 9, 9–12, 13–16, more than 16 years of education), migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary, and quintile of numeracy skills. Column (2) includes survey year fixed effects, all other potentially time-varying controls (age of the oldest child, firm-size dummies, industry, and occupation), as well as fixed effects for the matched worker pairs. Industry and occupation controls are included at the one-digit level for Canada, where the PIAAC Public Use File does not report two-digit ISCO/ISIC codes. Standard errors shown in parentheses are clustered at the match level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Data source: PIAAC United States (2012 and 2017 waves).

risk when controlling for past values of automation risk, training, and even earnings; this mitigates concerns about selection bias that may arise in the cross-sectional data.

In Table 4, we estimate value-added models using both the 2012 and 2015 automation risk measures (see Section 3).<sup>55</sup> Specifically, we regress the 2015 automation risk on firm training in the prior year, controlling for the automation risk in 2012, along with other covariates measured in 2012. All models apply

<sup>55</sup>See Section 3.1 for details on how the automation measure is constructed in each wave.



entropy balancing, ensuring exact balancing between the training and no-training groups on the control variables included in the respective specification.<sup>56</sup> The results indicate that firm training is strongly negatively correlated with automation risk in 2015, consistent with our main findings from the international analysis. Notably, the training estimate is only subject to a small reduction when moving from the model with standard pre-training controls (column 1) to the model that also includes pre-training automation risk (column 2). The training estimate remains virtually unchanged after additionally controlling for prior training participation (column 3) and even when accounting for pre-training hourly wages (column 4). This consistency in the magnitude of the training coefficient, even after controlling for variables that could reasonably be associated with selection into training (e.g., based on automation risk or wage level), suggests that our approach of leveraging within-occupation variation and utilizing rich individual controls, including tested numeracy skills, effectively addresses selection concerns. This further bolsters confidence in the findings from the international analysis.

In the full-control model presented in column (4) of [Table 4](#), firm training reduces automation risk by 6 pp. The panel dimension of the German PIAAC data allows us to benchmark this estimate in several ways. First, the reduction in automation risk through training is nearly twice the average change in automation risk observed between 2012 and 2015 (3.3 pp). Second, training proves to be more effective than switching occupations in mitigating automation risk. When replacing the training variable with an indicator for having changed two-digit occupations between 2012 and 2015 in the model from column (4), the coefficient on switching occupation is  $-0.0260$  ( $p = 0.004$ ). This indicates that training is more than twice as effective as changing occupations when it comes to reducing automation risk (results not shown).

Furthermore, observing workers' occupations in both 2012 and 2015 in the German PIAAC data allows us to test a key assumption underlying our international analysis—namely, that training is unrelated to switching occupations. Our individual-level measure of automation risk allows us to include occupation fixed effects to estimate the effect of firm training within the same occupation, thereby controlling for differences in training participation and automation risk between occupations. However, if workers were more likely to receive training after switching occupations, the within-occupation comparison would be compromised. In such a scenario, the no-training workers in the observed occupation would not serve as an appropriate (no-training) counterfactual for workers who switched occupations and thus received training. For a valid within-occupational comparison of training and no-training groups, it would either be necessary to know the training participants' previous (pre-training) occupation or be sure that training and occupational switching are unrelated. While we cannot track the occupations of workers prior to training in the cross-sectional PIAAC data, the German PIAAC panel provides information on participants' occupations in both 2012 and 2015. This allows us to exploit information on workers' occupation and training participation within individuals over time. We observe that switching occupation between 2012 and 2015 is completely unrelated to training participation in 2015 ( $\rho = 0.0084$ ). This finding sup-

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<sup>56</sup>Standard errors are clustered at the two-digit occupation level (42 clusters) for consistency with the international analysis. Clustering at the four-digit occupation level (304 clusters) yields very similar results.

Table 4: Training and Automation Risk: Value-Added Approach for Germany

	Automation risk (2015)			
	(1)	(2)	(3)	(4)
Firm training (2014)	-0.0762*** (0.0112)	-0.0627*** (0.0107)	-0.0590*** (0.0102)	-0.0599*** (0.0103)
Automation risk (2012)		0.3210*** (0.0399)	0.2904*** (0.0403)	0.2924*** (0.0424)
Firm training (2012)			-0.0425*** (0.0120)	-0.0426*** (0.0121)
Log hourly wage (2012)				-0.0251 (0.0175)
Numeracy skills (2012)	-0.0228* (0.0116)	-0.0168 (0.0108)	-0.0131 (0.0121)	-0.0099 (0.0121)
Occupation FE (2012)	X	X	X	X
Further controls (2012)	X	X	X	X
Entropy balancing	X	X	X	X
$R^2$	0.38	0.42	0.42	0.43
Observations	1,585	1,585	1,585	1,585

*Notes:* Ordinary least squares estimation with weights from entropy balancing. Dependent variable: individual-level automation risk, elicited in the 2015 PIAAC survey. Automation risk ranges from 0 (indicating a low probability that a worker is fully automated) to 1 (indicating a high probability that a worker is fully automated); automation risk is predicted using items on task use at work from PIAAC. Sample: employees aged 25–65 years in Germany. Firm training, elicited in the 2015 PIAAC survey, refers to the year 2014. All control variables were elicited in the 2012 PIAAC survey. Occupation fixed effects are measured at the two-digit ISCO level. Numeracy skills are standardized to unit standard deviation within Germany. Further controls include: years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (e.g., self-organized training or seminar participation), an indicator for full-time employment, firm size measured by number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+), and industry fixed effects (two-digit ISIC). Standard errors shown in parentheses are clustered at the occupation level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

*Data source:* PIAAC Germany (2012 and 2015 waves).

ports the validity of our international analysis, confirming that training and occupational switching are indeed unrelated, corroborating the validity of our within-occupation comparisons.

Finally, we estimate a panel model with individual and survey year fixed effects in the German PIAAC data, allowing for a more rigorous analysis that controls for unobserved time-invariant characteristics of individuals as well as time-specific effects. In this demanding model, presented in Table 5, we continue to find that firm training reduces automation risk, reinforcing the robustness of our main findings. However, it is important to note that the coefficient on training in this fixed effects model is considerably smaller and less precisely estimated compared to the value-added model. This reduction in magnitude and precision likely reflects the limited amount of over-time variation in both automation risk and training within individuals (original survey and follow-up are just 3.5 years apart), making it more challenging to detect significant training effects. Nonetheless, the consistency of our findings

across different models and sources of variation is reassuring and supports the overall robustness of our findings.<sup>57</sup>

Table 5: Training and Automation Risk: Panel Analysis for Germany

	Automation risk			
	(1)	(2)	(3)	(4)
Firm training	-0.0145*	-0.0151*	-0.0149*	-0.0129
	(0.0088)	(0.0090)	(0.0090)	(0.0089)
Individual FE	X	X	X	X
Survey year FE	X	X	X	X
Industry FE		X	X	X
Occupation FE			X	X
Further controls				X
$R^2$	0.72	0.73	0.75	0.75
Individuals	1,869	1,869	1,869	1,869
Observations	3,738	3,738	3,738	3,738

*Notes:* Fixed effects panel estimation, using the 2012 and 2015 waves of PIAAC Germany. Dependent variable: individual-level automation risk, ranging from 0 (indicating a low probability that a worker is fully automated) to 1 (indicating a high probability that a worker is fully automated); automation risk is predicted using items on task use at work from PIAAC. Sample: employees aged 25–65 years in Germany (restrictions refer to the values in the 2012 survey). Industry fixed effects are measured at the two-digit ISIC level, occupation fixed effects are measured at the two-digit ISCO level. Further controls are time-varying and include: age, an indicator for full-time employment, and firm size measured by number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+). Observations are not weighted. Standard errors shown in parentheses are clustered at the individual level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

*Data source:* PIAAC Germany (2012 and 2015 waves).

*Training Effectiveness Over Time.* Since the United States and Germany participated in PIAAC twice, we have the opportunity to examine how the effectiveness of firm training in reducing automation risk has changed over time. Table B.16 leverages this time dimension by estimating our baseline model (from column 5 of Table 2) by wave, separately for the United States (columns 1–3) and Germany (columns 4–6). The results indicate that training has become more effective in reducing automation risk over time. Consistently in both countries, the effectiveness of training roughly doubles between the two waves.<sup>58</sup> While the observed increase in training effectiveness could be due to sampling variation between the two survey waves (e.g., Meager, 2019), it may also reflect true temporal heterogeneity driven by factors such as shifts in labor market dynamics or changes in training content over time in response to an evolving

<sup>57</sup>As a robustness check, we construct a leaner 2015 automation index based on seven core task items—short, repetitive tasks; very similar workdays; task diversity; dealing with unexpected situations; organizing one’s own work; solving difficult problems; and learning something new (see Table B.3 for wording). The leaner index correlates with the 2012 measure at  $\rho = 0.41$  (individual level) and  $\rho = 0.78$  (2-digit occupation level), and strongly with our default 2015 index at  $\rho = 0.91$  and  $\rho = 0.92$ , respectively. Re-estimating our value-added and panel analyses with the leaner index (Tables B.14 and B.15) yields qualitatively similar results, albeit modestly attenuated, consistent with greater measurement error when using fewer items. We therefore retain the default index as our main measure.

<sup>58</sup>Note that in the 2015 PIAAC wave in Germany, information on children and non-work-related training was not collected. To ensure comparability across specifications, these control variables are excluded in all regressions for Germany.

technological landscape. For example, demographic changes that lead to a shrinking labor force (e.g., [Acemoglu and Restrepo, 2022](#)), declining labor force participation (e.g., [Dotsey et al., 2017](#)), or a tighter labor market due to the United States' extended recovery from the Great Recession (e.g., [Cunningham, 2018](#)), could incentivize employers to invest in more effective training programs that help incumbent employees adapt to changing task demands. Additionally, the content of training may have evolved, with newer technologies requiring different task inputs (e.g., [Atalay et al., 2020](#)) that are less susceptible to automation.<sup>59</sup>

*Alternative Task Measure: Routine Intensity.* Finally, we assess the robustness of our results to an alternative measure of exposure to technological change. [Table B.12](#) replicates our main results from [Table 2](#), replacing automation risk with an individual-level measure of *routine intensity*. This index captures the extent to which a worker's tasks are rule-based versus flexible, following the notion of routine work in the task-based approach literature (e.g., [Autor et al., 2003](#); [Autor, 2013](#)). We adopt the methodology of [Marcolin et al. \(2016\)](#), constructing a routine intensity index based on PIAAC task items. The index ranges from 0 (low routine intensity) to 1 (high routine intensity) and is a weighted average of four items capturing flexibility in task sequencing, discretion in how to perform tasks, and autonomy in planning activities and time.<sup>60</sup>

Consistent with our results on automation, we find that firm training significantly reduces individual-level routine intensity. In our preferred specification using entropy balancing and the full set of controls (column (5) of [Table B.12](#)), the estimated effect is a reduction of 0.027. In terms of magnitude, the training effect on routine intensity is somewhat smaller than the effect on automation risk. Training reduces routine intensity by roughly 12% of a standard deviation (compared to roughly 20% for automation risk) or 5% of the sample mean (compared to 8% for automation risk).

### 5.3. Mechanisms

Understanding the mechanisms through which firm training reduces automation risk is crucial for developing effective policies. In this section, we first identify which specific job tasks are enhanced by training, and thus reduce the likelihood of automation. We also investigate the role of digital skills as a potential channel through which training impacts the risk of automation.

*Job Tasks.* Our composite measure of automation risk in [Table 2](#) is derived from various tasks that workers perform in their jobs. To better understand the role of training in influencing these tasks, we estimate our baseline specification using individual job tasks as outcome. Results are presented in [Figure 2](#),

<sup>59</sup>We do not observe notable differences in the duration or financing of training in the United States between 2012 and 2017.

<sup>60</sup>For the construction of the routine index, we follow [Marcolin et al. \(2016\)](#) in using the following weights: 0.243 for flexibility in task sequence; 0.256 for flexibility in how to do the work; 0.251 for frequency of planning own activities; and 0.25 for frequency of organizing own time. As task items in PIAAC are elicited on a scale from zero to five, we rescale this routine intensity index to range between 0 and 1 for comparability with our main measure of automation risk. Despite being based on an entirely different set of task measures, the resulting index is highly correlated with our automation measure ( $\rho = 0.44$ ).

where task items are ordered by their contribution to automation risk.<sup>61</sup> The results indicate that workers who receive training engage in *all* tasks more frequently than those without training. However, the increase in task intensity is particularly pronounced for tasks associated with a lower risk of automation. For instance, training participation increases the likelihood that workers frequently engage in complex problem-solving (by 7.8 pp) and in interaction-intensive activities, such as influencing others (by 8.9 pp) and negotiating (by 9.7 pp). This finding aligns with the work of [Deming \(2017\)](#), who argues that social interaction tasks are difficult to automate, making them increasingly valuable in the labor market. These tasks are also likely to complement automation technology ([Acemoglu and Autor, 2011](#)), further reducing automation risk. For example, workers who participated in training subsequently perform manual dexterity tasks (associated with a higher automation risk) only 3.4 pp more often than workers without training, while they perform planning the work of others (associated with a lower automation risk) 9.1 pp more frequently.

*Digital Skills.* Next, we investigate the role of digital skills as a potential channel through which training impacts the risk of automation. Digital skills are particularly relevant as a mechanism for several reasons. First, as the modern economy becomes increasingly digitized, workers with strong digital skills are better equipped to adapt to new technologies and perform tasks that are complementary to automation as opposed to those that can be substituted by it (e.g., [Krueger, 1993](#); [Acemoglu and Pischke, 1998](#); [Caunedo et al., 2023](#)). This means that digital skills can help workers transition into roles that are less likely to be automated, such as those involving complex problem-solving, decision-making, and social interaction—areas where human oversight continues to be essential.<sup>62</sup> Second, digital skills enable workers to leverage automation technologies more effectively, allowing them to take on new, technology-enhanced roles. For instance, workers who are proficient in digital tools may be able to use advanced software for planning, coordination, and analysis, which not only improves their productivity, but also reduces their exposure to routine tasks at higher risk of automation.

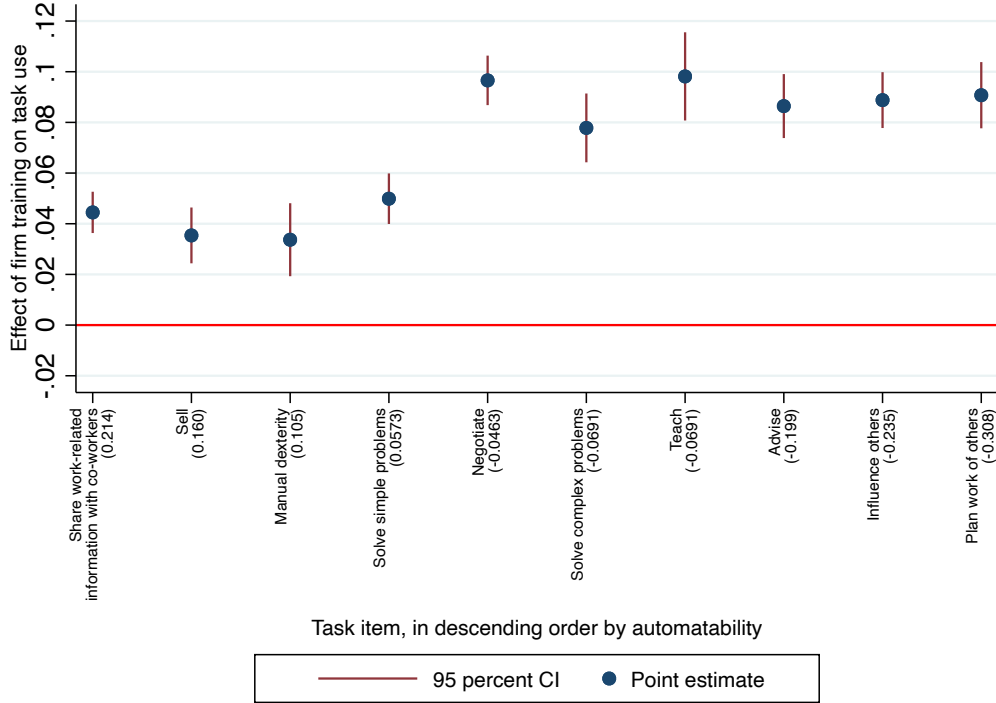
To further explore the role of digital skills as a mechanism, [Table 6](#) examines how training enhances these skills, potentially enabling workers to perform more complex, less automatable tasks. Using the same empirical strategy as in [Table 2](#), column (1) of [Table 6](#) shows that workers who receive training have digital skills that are 0.30 standard deviations higher than those of workers without training. While the coefficient decreases to 0.19 when occupation fixed effects are included in column (2), the most significant reduction occurs in column (3) when numeracy skills are added as a control. This is not surprising,

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<sup>61</sup>The task items in PIAAC are measured by the frequency of task use on a Likert scale ranging from one (never) to five (every day). To facilitate interpretation, we dichotomize this variable for the analysis in [Figure 2](#), defining a binary task variable equal to one if the respondent performs the task at least once a week (values four and five of the original task item). The binary task variable takes a value of zero if a task is performed less than once a week (values one, two, or three). This allows us to obtain OLS estimates analogous to those in [Table 2](#), which are more straightforward to interpret compared to coefficients from an ordered logit model typically applied to categorical Likert responses ([Cameron and Trivedi, 2005](#)). The results in [Figure 2](#) are robust to different specifications of the task-use cutoff.

<sup>62</sup>For instance, digital technologies might enable workers to effectively manage and plan the work of others using computer-aided tools.

Figure 2: Training and Task Use



*Notes:* The figure shows how firm training affects job tasks related to automation. Estimates are based on our baseline specification in column (5) of Table 2, using individual job tasks instead of the composite automation measure as outcome. The values of the responses to the task items elicited in PIAAC represent the frequency of task use given on a Likert scale ranging from 1 (never) to 5 (every day). For ease of interpretation, we binarize the task items (1: task is performed at least once a week; 0: task is performed less than once a week). Training coefficients are shown in descending order by automatability; automatability weights (i.e., factor loadings from Table B.2) are shown on the horizontal axis in parentheses. 95% confidence intervals are based on standard errors two-way clustered at the country and occupation level.

*Data source:* PIAAC.

given the high correlation between numeracy and digital skills ( $\rho = 0.74$ ). In the most demanding specification, which incorporates additional controls and applies entropy balancing (column 5 of Table 6), firm training is associated with an increase in digital skills of 0.048 standard deviations. This effect size is roughly 15% of the average difference in digital skills between an ICT professional (standardized digital skill score of 0.847) and a business and administration professional (0.472). Alternatively, it represents about 30% of the difference in digital skills between workers aged 25 to 34 (standardized digital skill score of 0.241) and those aged 35 to 44 (0.085). These findings suggest that enhancing digital skills through firm training may enable workers to perform more complex tasks less susceptible to automation.

The PIAAC assessment of digital skills was optional, allowing countries to opt out entirely. In participating countries, only those participants who demonstrated basic computer skills and confidence in computer usage were assessed (Section 2.1). This implies sample selection since participation in the digital skill assessment is potentially correlated with unobserved factors such as ability, motivation, and effort. In Table B.17, we present results after imputing missing digital skills in various ways. Column (1)

Table 6: Training and Digital Skills

	Digital skills				
	(1)	(2)	(3)	(4)	(5)
Firm training	0.2992*** (0.0213)	0.1932*** (0.0150)	0.0845*** (0.0128)	0.0728*** (0.0112)	0.0481*** (0.0089)
Numeracy skills			0.8171*** (0.0120)	0.7731*** (0.0115)	0.7678*** (0.0102)
Occupation FE		X	X	X	X
Further controls				X	X
Entropy balancing					X
$R^2$	0.08	0.15	0.54	0.58	0.59
Observations	72,180	72,180	72,180	72,180	72,180

*Notes:* Ordinary least squares estimation in columns (1)–(4), least squares estimation with weights from entropy balancing in column (5). Dependent variable: digital skills standardized to standard deviation 1 across countries. Sample: employees aged 25–65 years with information on digital skills, automation risk, and log hourly wages. Firm Training: binary variable indicating whether the respondent participated in on-the-job training or job-related training in the 12 months prior to the survey. Controls: numeracy skills (standardized to unit standard deviation across countries), years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (i.e., open/distance education, seminars/workshops, or private lessons), an indicator for full-time employment, and firm size measured by the number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+). Occupation fixed effects at the two-digit ISCO level. All regressions also control for industry (two-digit ISIC) and country fixed effects. Occupation and industry fixed effects are included at the one-digit level for Austria, Canada, Estonia, and Finland, where the PIAAC Public Use File does not report two-digit ISCO/ISIC codes. All control variables were used for the entropy balancing in column (5).  $R^2$  refers to within-country  $R^2$ . Standard errors shown in parentheses are two-way clustered at the country and occupation level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

*Data source:* PIAAC.

replicates the preferred specification from column (5) of Table 6. In columns (2)–(4), we impute missing digital skills with zero, the global minimum, and the country-specific minimum, respectively. As anticipated, the training estimate increases in these more inclusive samples, as individuals with imputed digital skills are less likely to engage in firm training. Consistently across all imputation methods, the training estimate more than doubles. Therefore, we interpret the training estimate in the baseline sample without imputed digital skills as a lower bound of the true effect of firm training on digital skills.

Additionally, we consider the mere fact of whether an individual participated in the digital skill assessment as indicative of their basic digital skills; individuals without any computer experience or those who failed a simple initial computer test are likely to possess very limited to no digital skills. Intriguingly, we find that firm training significantly increases the probability of individuals having at least basic digital skills by 3.6 pp in column (5) of Table B.18, corresponding to 5% of the average probability of having basic digital skills (78%).

#### 5.4. Training and Wages

In this section, we analyze the impact of firm training on wages. Higher wages typically reflect increased productivity, which can result from enhanced skills, better job performance, and greater adaptability to technological changes. Understanding the relationship between training and wages provides insights into how training contributes towards individual labor market success.



[Table 7](#) presents the relationship between firm training and log hourly wages. As with our analysis of automation risk, column (1) demonstrates the association between training and wages, controlling only for country and industry fixed effects. In this specification, workers who have received training earn 20.8% more than workers without training. However, similar to the findings in [Table 2](#), the coefficient drops substantially after we include occupation fixed effects in column (2), and it further decreases when numeracy skills are added as control in column (3). This suggests that a considerable portion of the initial wage premium associated with training is explained by occupational sorting and selection into training based on ability. When additional worker and firm characteristics are included in column (4), the training coefficient only changes slightly, indicating that these factors have a limited impact on the estimated effect of training. After applying entropy balancing in column (5), we find that firm training increases wages by 7.1 percent. While previous country-specific analyses of training effects on wages reveal highly heterogeneous effect sizes—varying by country, period of analysis, and estimation approach (see Online Appendix A in [Ma et al., 2024](#))—our estimate falls roughly in the middle of this range. The estimated 7% wage increase is sizable, comparable to the wage gains associated with an additional year of schooling in industrialized countries (see Table A.2 in [Hanushek et al., 2015](#)).<sup>63</sup>

These results corroborate the notion that firm training enhances workers’ productivity by shifting their task composition toward less automatable, more technology-complementary activities. This shift increases workers’ marginal product of labor, leading to higher wages. But how important is a reduced risk of automation for the wage gains from training? One simple way to address this question is to add automation risk as an additional control when examining the relationship between training and wages. In column (6), we observe a reduction in the estimated wage effect of training by about 15%. This suggests that a sizable portion of the wage gains from training can be attributed to its role in lowering workers’ risk of automation.<sup>64</sup>

## 6. Heterogeneity in Training Effectiveness

This section investigates the effects of firm training on automation risk and wages, with a focus on differences across countries, socio-demographic groups, and types of training. We begin by exploring how the effectiveness of training varies by country, considering diverse institutional frameworks and

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<sup>63</sup>However, direct comparisons between the returns to schooling and those to firm-sponsored training should be made with caution. Schooling estimates typically reflect average effects across the population, often capturing broad human capital accumulation rather than job-specific skill acquisition. In contrast, firm training is highly targeted, relatively short in duration, and usually tailored to the immediate technological or organizational needs of the firm. Such alignment between training content and current production tasks can generate high short-run productivity gains and thus larger estimated wage effects for participants. Moreover, while returns to schooling accumulate gradually and persist over the life cycle, returns to job training are likely to be more immediate but potentially less durable. Hence, our estimated wage effects of training should be interpreted as capturing the short-run, marginal productivity gains from targeted, firm-integrated training—rather than as a direct analogue to the long-run returns from additional schooling years.

<sup>64</sup>The results in columns (1)–(5) of [Table 7](#) remain virtually unchanged when we include the full sample without restricting it to workers with information on automation, as there are very few missing values related to job tasks and the corresponding risk of automation.

Table 7: Training and Wages

	Log hourly wages					
	(1)	(2)	(3)	(4)	(5)	(6)
Firm training	0.2078*** (0.0182)	0.1192*** (0.0089)	0.1032*** (0.0084)	0.0843*** (0.0071)	0.0713*** (0.0058)	0.0616*** (0.0055)
Numeracy skills			0.0870*** (0.0078)	0.0576*** (0.0063)	0.0597*** (0.0056)	0.0574*** (0.0056)
Automation risk						-0.2575*** (0.0217)
Occupation FE		X	X	X	X	X
Further controls				X	X	X
Entropy balancing					X	X
$R^2$	0.16	0.29	0.30	0.37	0.38	0.39
Observations	91,470	91,470	91,470	91,470	91,470	91,470

*Notes:* Ordinary least squares estimation in columns (1)–(4), least squares estimation with weights from entropy balancing in column (5). Dependent variable: log hourly wages, excluding bonuses for wage and salary workers. Sample: employees aged 25–65 years with information on automation risk and log hourly wages. Firm training: binary variable indicating whether the respondent participated in on-the-job training or job-related training in the 12 months prior to the survey. Numeracy skills are standardized to unit standard deviation across countries. Further controls: years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (i.e., open/distance education, seminars/workshops, or private lessons), an indicator for full-time employment, and firm size measured by the number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+). Occupation fixed effects at the two-digit ISCO level. Occupation and industry fixed effects are included at the one-digit level for Austria, Canada, Estonia, and Finland, where the PIAAC Public Use File does not report two-digit ISCO/ISIC codes. All regressions also control for industry (two-digit ISIC) and country fixed effects. All control variables were used for the entropy balancing in columns (5) and (6).  $R^2$  refers to within-country  $R^2$ . Standard errors shown in parentheses are two-way clustered at the country and occupation level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

*Data source:* PIAAC.

cultural contexts. We then examine heterogeneity in training effects by age and gender, which may influence how much workers are exposed to technological change. Finally, we analyze the impact of different types of training to understand which forms of training are most effective in reducing automation risk and increasing wages.

### 6.1. Heterogeneity by Country

Examining the returns to training by country is crucial, as it allows us to assess the consistency of our findings across diverse institutional frameworks and cultural contexts. Our international dataset is uniquely suited for this purpose, enabling us to explore how training impacts workers in different countries and to verify whether the effects we observe hold true across a wide range of settings.

Figure A.5 and Figure A.6 show how firm training affects automation risk and log hourly wages for each country individually, using our main estimation model outlined in Equation 2. We observe substantial cross-country heterogeneity in the effectiveness of training. Specifically, the reduction in automation risk due to training ranges from just 1 pp in Turkey to as much as 6 pp in Cyprus. Similarly, the effect of training on wages varies widely, from a modest 1% increase in France to a substantial 21% increase in

Mexico.<sup>65</sup> These variations highlight the critical role of institutional and cultural contexts in shaping the effectiveness of training programs.<sup>66</sup> Despite this cross-country variation in training returns, the effect of training on both automation risk and wages is statistically significant in almost all countries in our sample.<sup>67</sup> This consistency underscores the external validity of our findings across diverse economic and institutional environments.

## 6.2. *Heterogeneity by Gender and Age*

Next, we examine heterogeneity in the effectiveness of firm training by age and gender. This analysis provides insight into whether certain demographic groups benefit differentially from training and whether training can help mitigate structural inequalities in the labor market. Understanding these dimensions is particularly relevant as labor forces in many industrialized countries age and as policymakers seek to ensure equal access to skill upgrading across groups.

Figure A.7 displays the estimated effects of training on automation risk (left panel) and log wages (right panel) by gender and age group. Across both outcomes, training appears similarly effective for men and women. We find no systematic gender differences in training effects at any point in the life cycle. For automation risk, the estimated coefficients for men and women closely overlap across age groups, suggesting that both benefit comparably from training in reducing exposure to automation. Likewise, the wage effects of training are positive and of similar magnitude for men and women in all age groups. These findings imply that firm training benefits workers broadly, without favoring specific demographic groups. This is encouraging for measures aimed at fostering inclusive skill upgrading, as it suggests that training can effectively enhance productivity and earnings potential across workers of both genders and all stages of the career.<sup>68</sup>

Figure A.8 shows the effect of firm training on automation risk and wages by education level, categorizing workers as low (lower secondary degree or less), medium (upper secondary and post-secondary degree), or high (tertiary degree) educated. Notably, we find no significant differences in training effectiveness across these education levels. While the literature on skill- and task-biased technological change suggests that technology typically complements higher-educated workers and substitutes lower-educated ones (e.g., Acemoglu and Autor, 2011; Goos et al., 2014), our results highlight the potential of firm training to bridge outcome gaps across education groups.

<sup>65</sup> Bassanini et al. (2007) find wage returns to training across European countries ranging from 3.7 to 21.6 percent.

<sup>66</sup> In unreported analyses, we also examined whether differences in training effects on automation risk are systematically related to features of country economies, such as labor market regulations and pension generosity. However, we found little evidence to support this.

<sup>67</sup> The training coefficient is not statistically significant at the 5% level in Czech Republic, Kazakhstan, Slovakia, Spain, and Turkey for automation risk and in Chile, France, New Zealand, and Turkey for wages.

<sup>68</sup> Consistent with the absence of gender-age differences in training effectiveness, we find little evidence for differential take-up of training. Within each age group, men and women exhibit quite similar training rates.

### 6.3. Heterogeneity by Type of Training

Finally, we examine heterogeneity in training effectiveness by its duration and financing. Dividing the sample into quintiles based on training duration, ranging from less than one day in the first quintile to over 14 days in the fifth, [Figure A.9](#) demonstrates that the effectiveness of training on automation risk and wages generally increases with duration. However, training lasting more than 14 days, while still significant and substantial, is less effective than training in the 6- to 14-day range.<sup>69</sup> The overall positive relationship between training duration and returns suggests that the benefits of training are driven by skill acquisition and content rather than signaling effects alone.

Regarding financing, [Figure A.10](#) indicates that firm training is most effective for reducing automation risk and increasing wages when fully or partly financed by the employer (approximately 70% of workers in our sample), compared to training not financed by the employer or offered free of charge. Consistent with the findings on training duration, this suggests that productivity gains from training are more likely when employers anticipate a return on their investment in training outcomes.

## 7. Conclusions

Using data from 37 industrialized countries, and more than 90,000 workers, we examine how firm training on-the-job can help workers adapt to evolving task requirements and reduce their susceptibility to automation. To tackle the potential endogeneity of training participation, we apply entropy balancing on a comprehensive set of worker characteristics, including tested numeracy skills to control for unobserved ability. Drawing on within-country, within-industry, and within-occupation variation to identify training effects, our analysis overcomes the identification challenges that typically plague cross-country studies.

Our results show that firm training significantly reduces automation risk, with training participants experiencing a reduction in automation risk of 3.8 pp—equivalent to a 8% decrease in the average automation risk. Additionally, workers who undergo training receive approximately 7% higher wages compared to those without training. While the positive wage effects of training are well-documented in the literature and the magnitude of our wage return estimate falls into the average of those documented in previous studies across different countries (see [Ma et al., 2024](#)), our study is the first to demonstrate that approximately 15% of these wage gains can be directly attributed to the reduction in automation risk. This underscores the importance that reduced susceptibility to automation through training plays in improving workers' labor market prospects. Evidence from meta-analyses of active labor market programs further suggests that experimental and non-experimental evaluations tend to yield comparable effect sizes ([Card et al., 2018](#)), which is reassuring for our non-experimental approach.

Training effectively mitigates automation risk and enhances wages in nearly all countries, underscoring the external validity of our findings. At the individual level, effects are broadly similar across gender,

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<sup>69</sup>This may reflect selection effects. Workers with lower levels of education generally receive shorter training ([Table 1](#)). However, workers in the highest quintile of training duration tend to have lower education levels and numeracy skills compared to those in the third and fourth quintile.

age, and education. These patterns suggest that firm training delivers broadly shared gains and can support inclusive skill upgrading rather than widening pre-existing gaps. Additionally, training effectiveness tends to increase with training duration, and is highest when training is fully or partly financed by the employer. These patterns highlight the importance of both training content and employer investment in maximizing training gains.

While our study provides insights into the role of firm training in reducing automation risk, it also raises important questions that warrant further investigation. A key limitation of our analysis is the lack of detailed information on the specific content of training programs. Understanding which type of training is more effective is crucial for tailoring more effective policy interventions. Future research should aim to collect data on the types of training provided, particularly focusing on how these programs align with the evolving demands of the labor market. Accordingly, efforts to expand training are likely most effective when they support firms in implementing strategies that combine technology adoption, organizational restructuring, and skill upgrading. Without these complementarities, simply increasing training volumes may fail to replicate the gains we observe in our setting. Finally, while our study emphasizes the importance of reducing the risk of automation, future work should consider how training interacts with other forms of human capital investments, such as formal education and lifelong learning, to provide a more comprehensive understanding of how workers can adapt to and thrive in an increasingly automated world.

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# Online Appendix

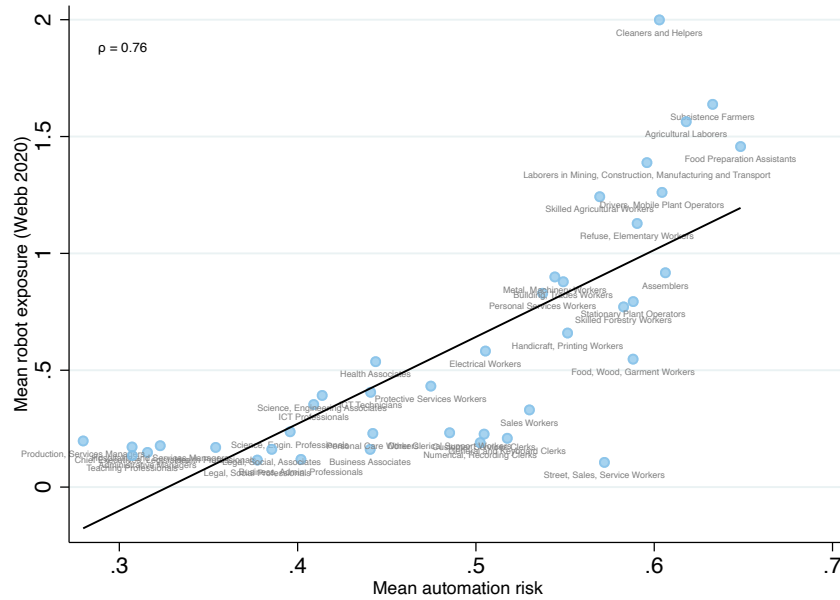
for

“Firm Training, Automation, and Wages: International Worker-Level Evidence”

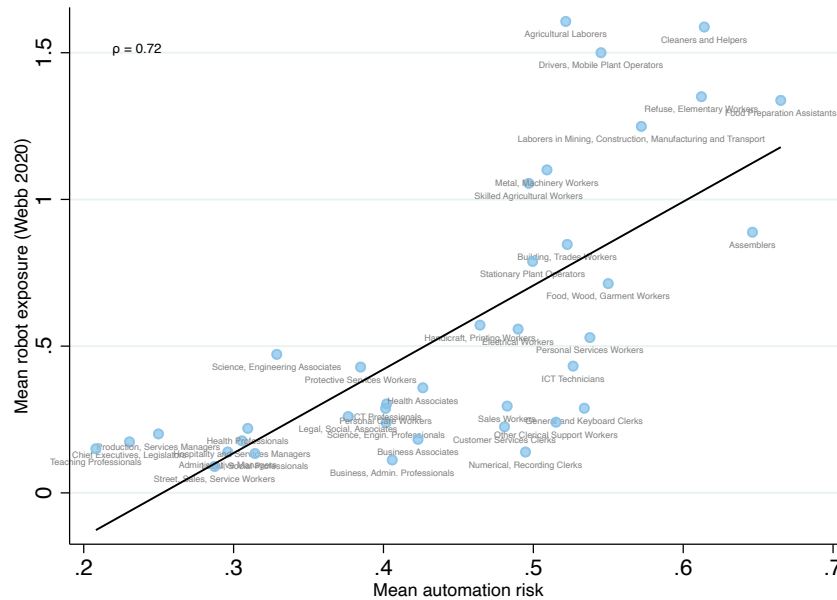
## Appendix A. Additional Figures

Figure A.1: Automation Risk and Robot Exposure (Webb, 2020)

(a) All Countries



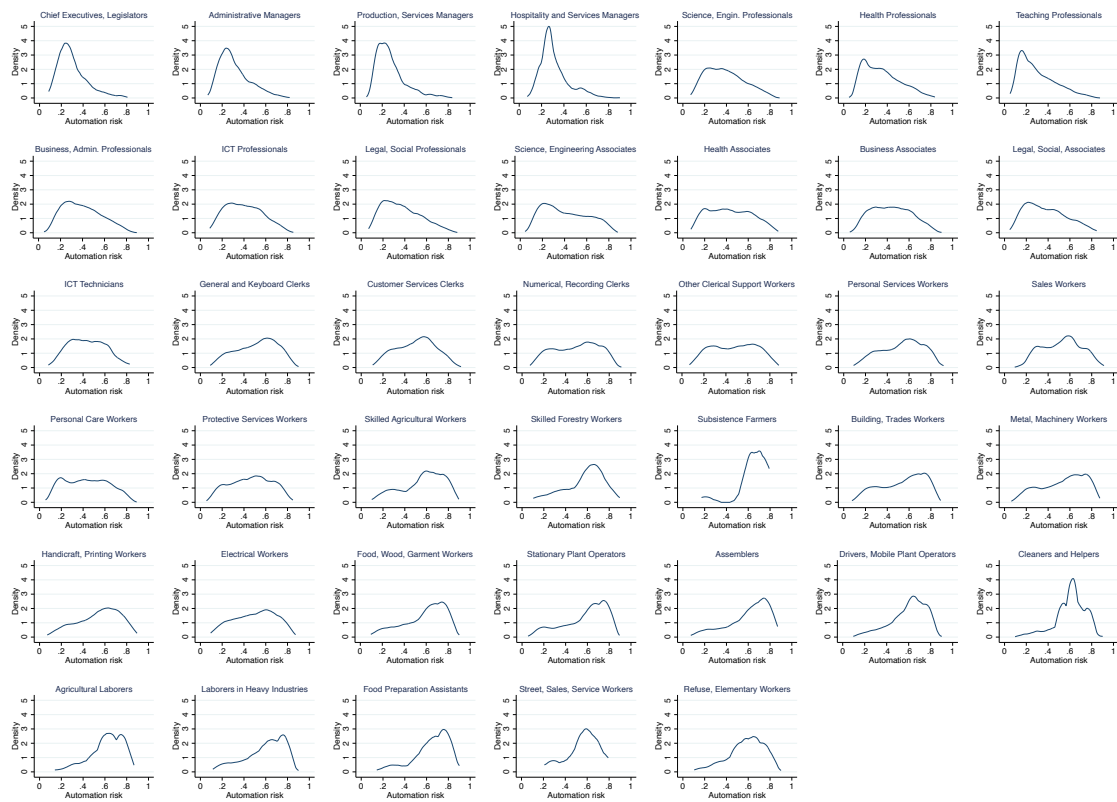
(b) United States



*Notes:* The figure plots mean automation risk against Webb (2020)'s robot-exposure index at the two-digit ISCO level. Webb's index is reported for U.S. SOC occupations; we map SOC to ISCO using a SOC–ISCO crosswalk and aggregate to two-digit ISCO with PIAAC employment weights. Our automation risk index ranges from 0 (indicating a low probability that a worker is fully automated) to 1 (indicating a high probability that a worker is fully automated), predicted from PIAAC task-use items. Webb's robot-exposure index ranges from 0 (low exposure) to 1 (high exposure). Panel (a) includes all countries except Austria, Canada, Estonia, and Finland, which report occupational information only at the one-digit level in the PIAAC Public Use File.

*Data source:* PIAAC and Webb (2020).

Figure A.2: Densities of Automation Risk by Occupation

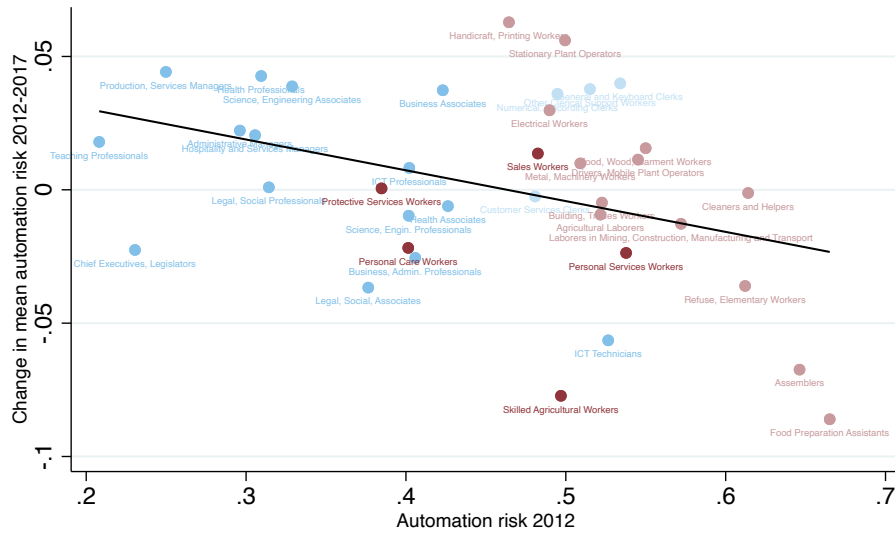


*Notes:* The figure shows the density of the individual-level automation risk by occupation at the two-digit ISCO occupation level across all countries in our sample—except Austria, Canada, Estonia, and Finland, which report occupational information only at the one-digit level in the PIAAC Public Use File. The automation risk ranges from 0 (indicating a low probability that a worker is fully automated) to 1 (indicating a high probability that a worker is fully automated); automation risk is predicted using items on task use at work from PIAAC.

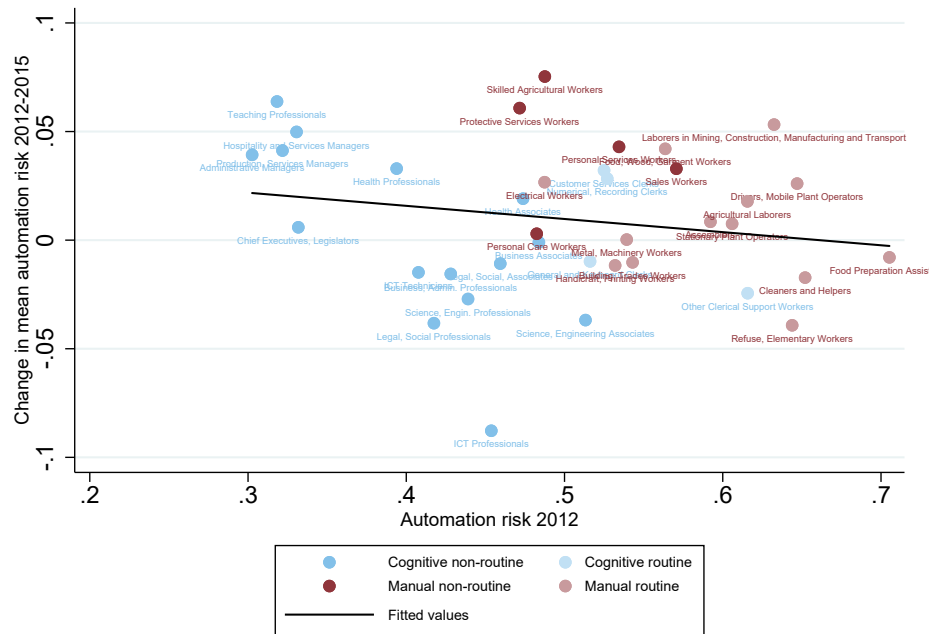
*Data source:* PIAAC.

Figure A.3: Change in Automation Risk over Time

(a) United States



(b) Germany

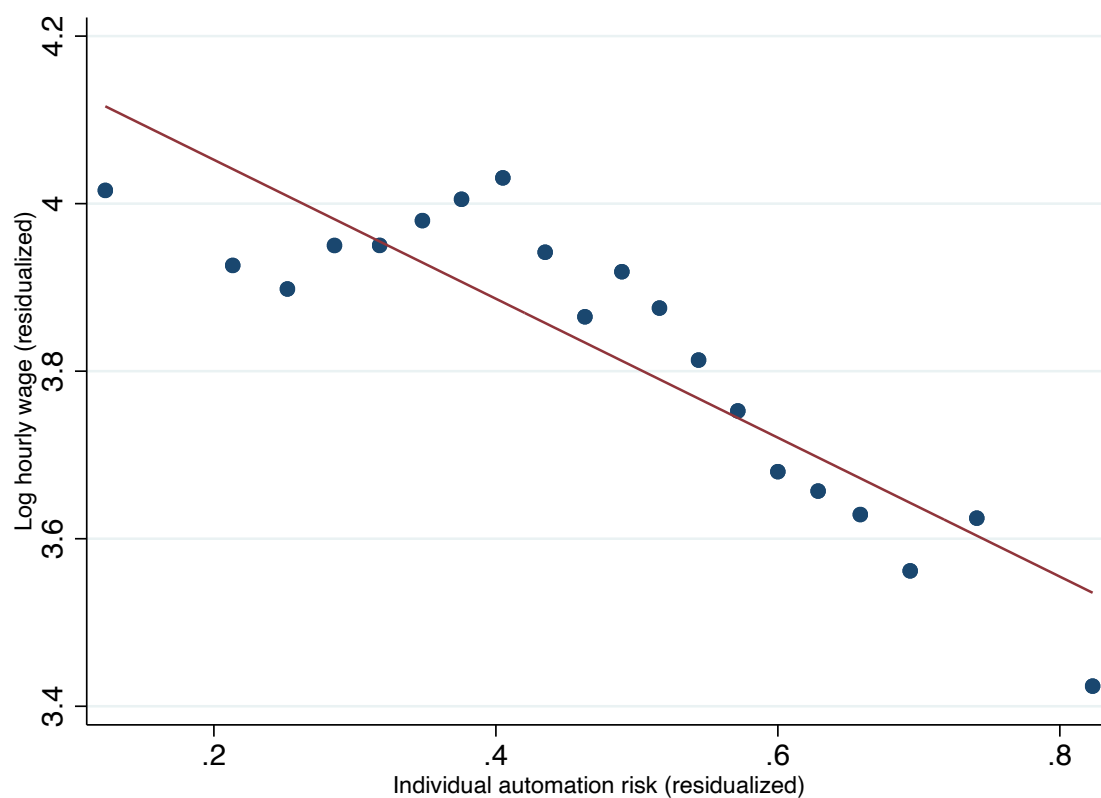


*Notes:* The figure plots the average automation risk in 2012 against the change in average automation risk between 2012 and 2017 for the United States (Panel A) and between 2012 and 2015 for Germany (Panel B), by two-digit ISCO occupation. For details on the construction of the automation risk measure, see Section 3.1.

*Data source:* PIAAC United States (2012 and 2017 waves), PIAAC Germany (2012 and 2015 waves).



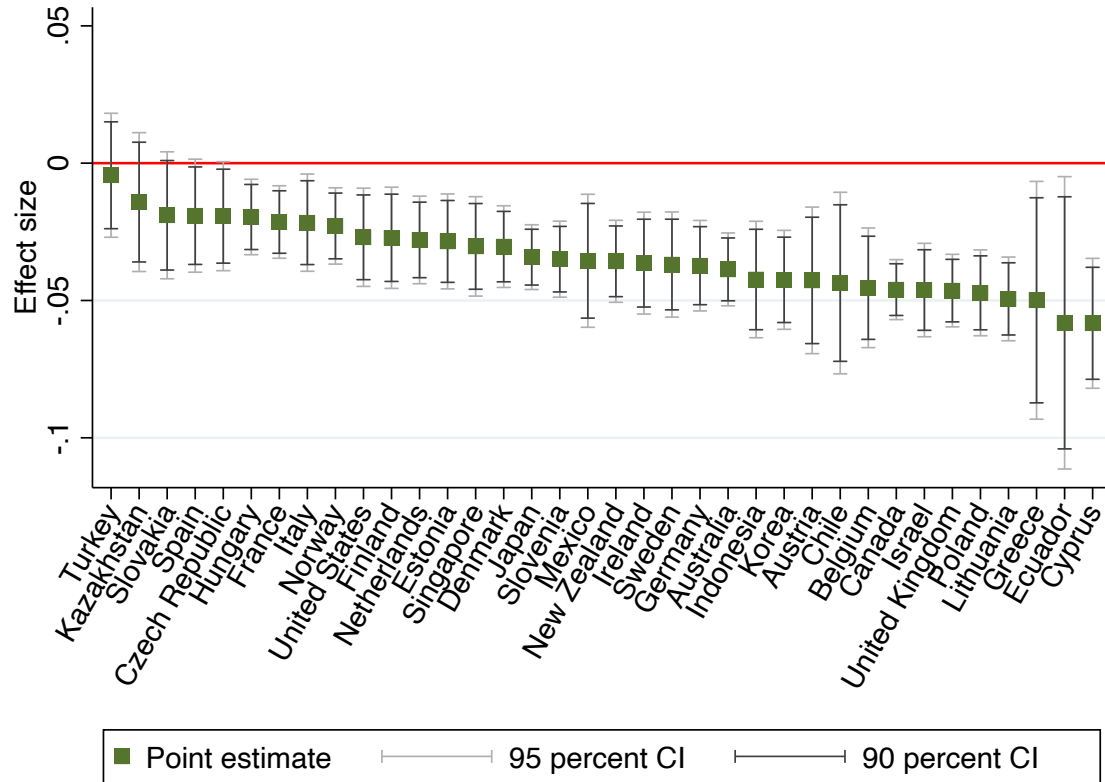
Figure A.4: Automation Risk and Wages



*Notes:* The figure displays a binned scatter plot showing the relationship between automation risk and log hourly wages. The automation risk is measured at the individual level, ranging from 0 (indicating a low probability that a worker is fully automated) to 1 (indicating a high probability that a worker is fully automated); automation risk is predicted using items on task use at work from PIAAC. To construct the figure, we divided the average automation risk into 20 ranked equal-sized groups and plotted the mean of the log hourly wages against the mean of average automation risk in each bin. The figure shows the residualized relationship after accounting for country, industry (two-digit ISIC), and occupation (two-digit ISCO) fixed effects; occupation and industry fixed effects are included at the one-digit level for Austria, Canada, Estonia, and Finland, where the PIAAC Public Use File does not report two-digit ISCO/ISIC codes. Best-fit line shown in red.

*Data source:* PIAAC.

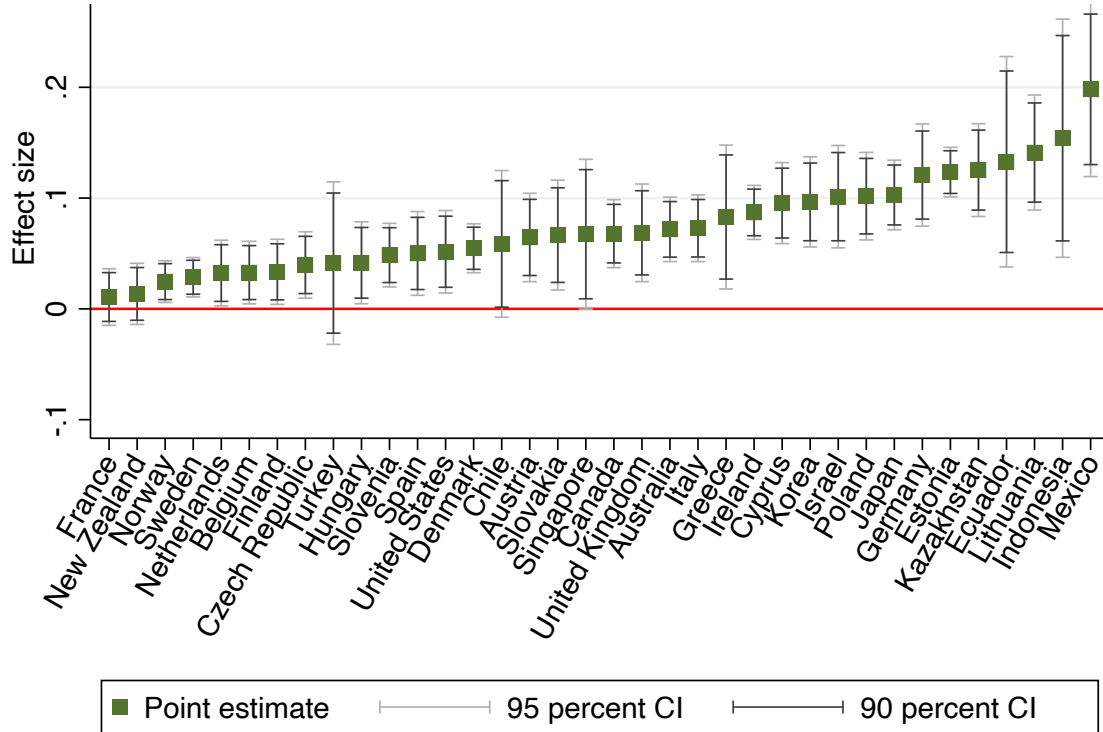
Figure A.5: Training and Automation Risk by Country



*Notes:* The figure shows the effect of training on automation risk separately for each PIAAC country. Least squares estimation with weights from entropy balancing. Dependent variable: individual-level automation risk, ranging from 0 (indicating a low probability that a worker is fully automated) to 1 (indicating a high probability that a worker is fully automated); automation risk is predicted using items on task use at work from PIAAC. Sample: employees aged 25–65 years with information on automation risk and log hourly wages. Firm Training: binary variable indicating whether the respondent participated in on-the-job training or job-related training in the 12 months prior to the survey. Included controls: numeracy skills, years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (i.e., open/distance education, seminars/workshops, or private lessons), an indicator for full-time employment, and firm size measured by the number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+). Industry fixed effects are at the two-digit ISIC level, and occupation fixed effects are at the two-digit ISCO level. Fixed effects are included at the one-digit level for Austria, Canada, Estonia, and Finland, where the PIAAC Public Use File does not report two-digit ISCO/ISIC codes. All control variables were used for the entropy balancing. 95% confidence intervals are based on standard errors clustered at the occupation level.

*Data source:* PIAAC.

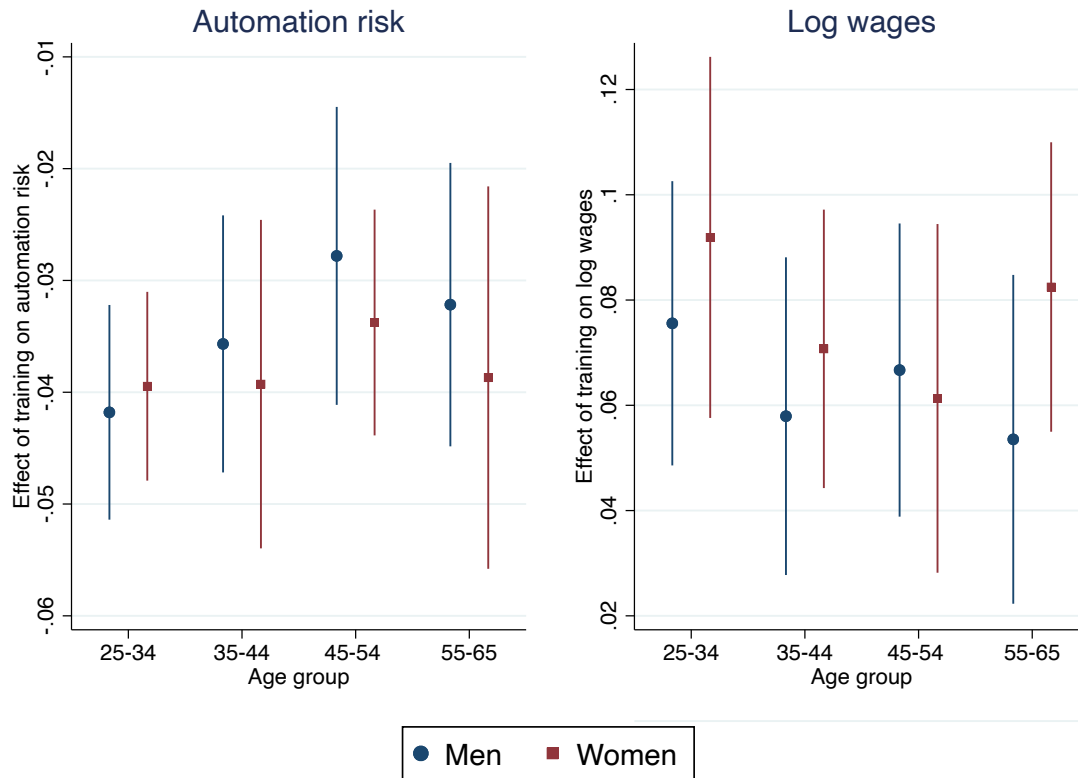
Figure A.6: Training and Wages by Country



*Notes:* The figure shows the effect of training on wages separately for each PIAAC country. Least squares estimation with weights from entropy balancing. Dependent variable: log hourly wages. Sample: employees aged 25–65 years with information on automation risk and log hourly wages. Firm Training: binary variable indicating whether the respondent participated in on-the-job training or job-related training in the 12 months prior to the survey. Included controls: numeracy skills, years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (i.e., open/distance education, seminars/workshops, or private lessons), an indicator for full-time employment, and firm size measured by the number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+). Industry fixed effects are at the two-digit ISIC level, and occupation fixed effects are at the two-digit ISCO level. Fixed effects are included at the one-digit level for Austria, Canada, Estonia, and Finland, where the PIAAC Public Use File does not report two-digit ISCO/ISIC codes. All control variables were used for the entropy balancing. 95% confidence intervals are based on standard errors clustered at the occupation level.

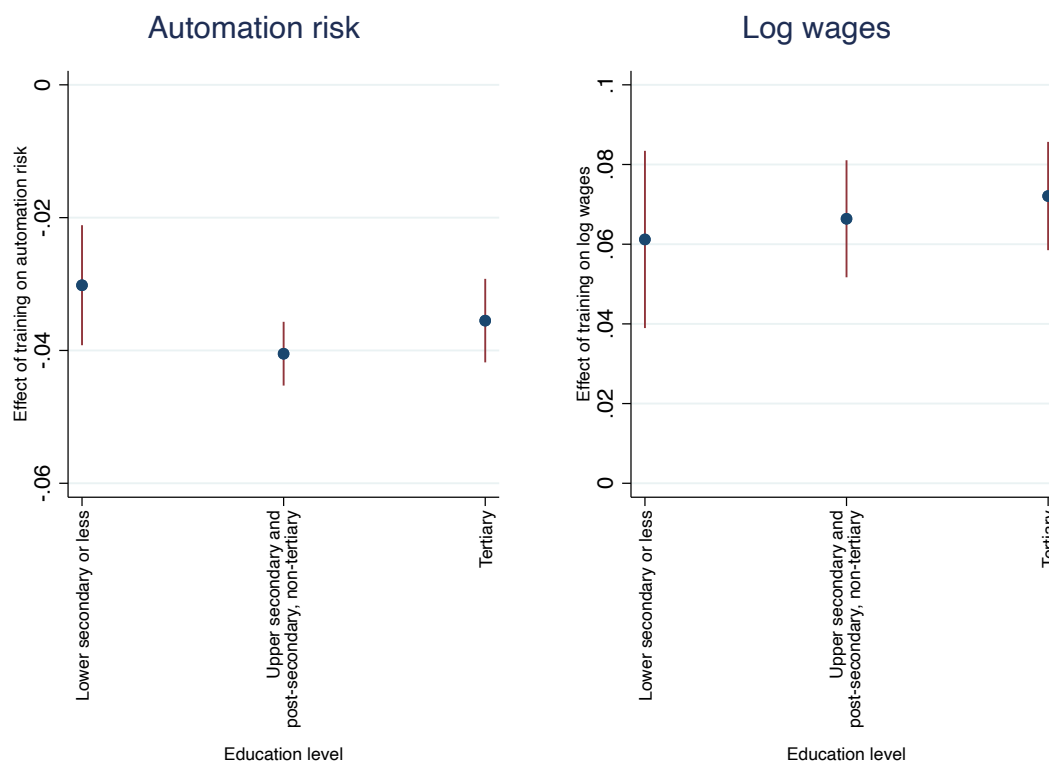
*Data source:* PIAAC.

Figure A.7: Heterogeneity by Age and Gender



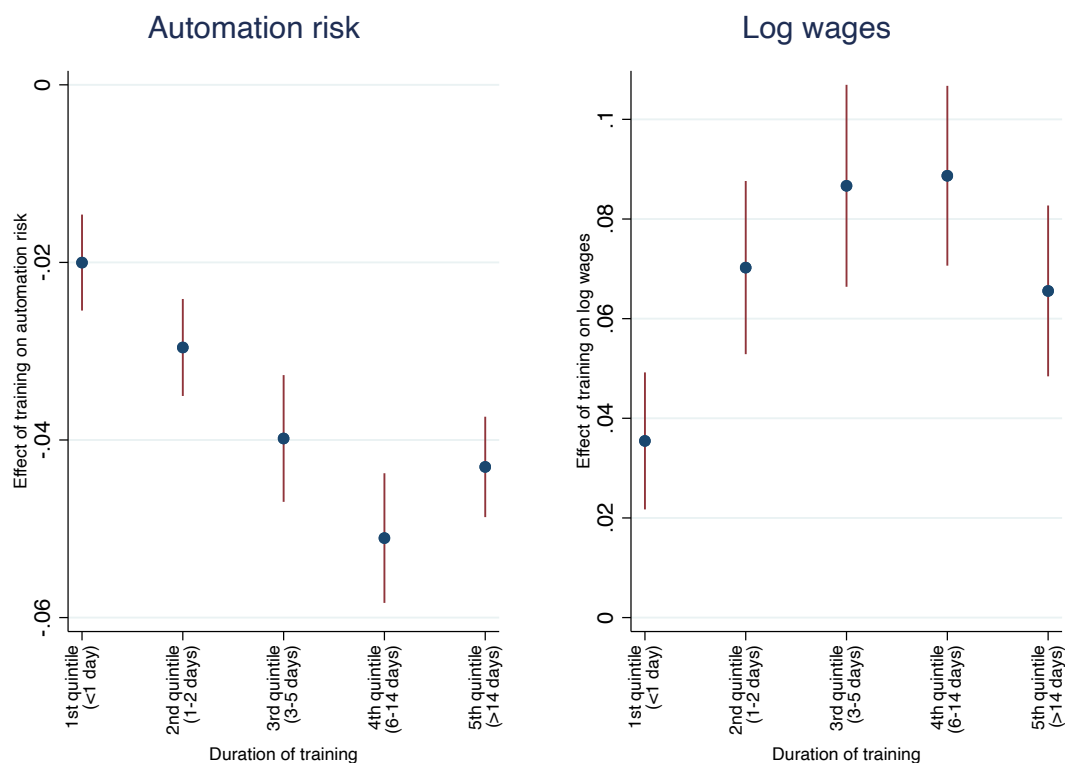
*Notes:* Least squares estimation with weights from entropy balancing in both figures. Separate estimations by gender. Dependent variable in left figure: individual-level automation risk, ranging from 0 (indicating a low probability that a worker is fully automated) to 1 (indicating a high probability that a worker is fully automated); automation risk is predicted using items on task use at work from PIAAC. Dependent variable in right figure: log hourly wages. Sample: employees aged 25–65 years with information on automation risk and log hourly wages. Firm Training: binary variable indicating whether the respondent participated in on-the-job training or job-related training in the 12 months prior to the survey. Included controls are numeracy skills, years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (i.e., open/distance education, seminars/workshops, or private lessons), an indicator for full-time employment, and firm size measured by the number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+). Industry fixed effects are at the two-digit ISIC level, and occupation fixed effects are at the two-digit ISCO level. Fixed effects are included at the one-digit level for Austria, Canada, Estonia, and Finland, where the PIAAC Public Use File does not report two-digit ISCO/ISIC codes. All regressions also control for country fixed effects. All control variables were used for the entropy balancing.  $R^2$  refers to within-country  $R^2$ . 95% confidence intervals are based on standard errors two-way clustered at the country and occupation level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ . Data source: PIAAC.

Figure A.8: Heterogeneity by Education Level



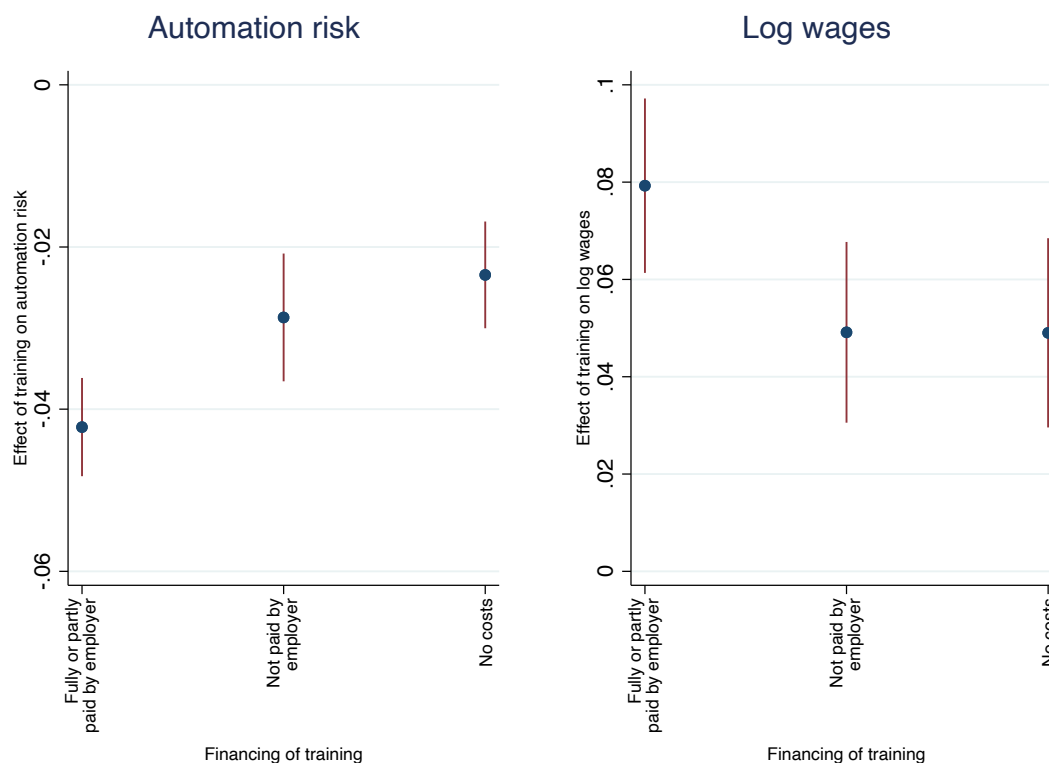
*Notes:* Least squares estimation with weights from entropy balancing in both figures. Separate estimations by highest educational degree obtained. Dependent variable in left figure: individual-level automation risk, ranging from 0 (indicating a low probability that a worker is fully automated) to 1 (indicating a high probability that a worker is fully automated); automation risk is predicted using items on task use at work from PIAAC. Dependent variable in right figure: log hourly wages. Sample: employees aged 25–65 years with information on automation risk and log hourly wages. Firm Training: binary variable indicating whether the respondent participated in on-the-job training or job-related training in the 12 months prior to the survey. Included controls are numeracy skills, years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (i.e., open/distance education, seminars/workshops, or private lessons), an indicator for full-time employment, and firm size measured by the number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+). Industry fixed effects are at the two-digit ISIC level, and occupation fixed effects are at the two-digit ISCO level. Fixed effects are included at the one-digit level for Austria, Canada, Estonia, and Finland, where the PIAAC Public Use File does not report two-digit ISCO/ISIC codes. All regressions also control for country fixed effects. All control variables were used for the entropy balancing. 95% confidence intervals are based on standard errors two-way clustered at the country and occupation level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .  
*Data source:* PIAAC.

Figure A.9: Heterogeneity by Duration of Training



*Notes:* Least squares estimation with weights from entropy balancing in both figures. Separate estimations by duration of training. Dependent variable in left figure: individual-level automation risk, ranging from 0 (indicating a low probability that a worker is fully automated) to 1 (indicating a high probability that a worker is fully automated); automation risk is predicted using items on task use at work from PIAAC. Dependent variable in right figure: log hourly wages. Sample: employees aged 25–65 years with information on automation risk and log hourly wages. Firm Training: binary variable indicating whether the respondent participated in on-the-job training or job-related training in the 12 months prior to the survey. Included controls are numeracy skills, years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (i.e., open/distance education, seminars/workshops, or private lessons), an indicator for full-time employment, and firm size measured by the number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+). Industry fixed effects are at the two-digit ISIC level, and occupation fixed effects are at the two-digit ISCO level. Fixed effects are included at the one-digit level for Austria, Canada, Estonia, and Finland, where the PIAAC Public Use File does not report two-digit ISCO/ISIC codes. All regressions also control for country fixed effects. All control variables were used for the entropy balancing. 95% confidence intervals are based on standard errors two-way clustered at the country and occupation level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .  
*Data source:* PIAAC.

Figure A.10: Heterogeneity by Financing of Training



*Notes:* Least squares estimation with weights from entropy balancing in both figures. Separate estimations by how the training was financed. Dependent variable in left figure: individual-level automation risk, ranging from 0 (indicating a low probability that a worker is fully automated) to 1 (indicating a high probability that a worker is fully automated); automation risk is predicted using items on task use at work from PIAAC. Dependent variable in right figure: log hourly wages. Sample: employees aged 25–65 years with information on automation risk and log hourly wages. Firm Training: binary variable indicating whether the respondent participated in on-the-job training or job-related training in the 12 months prior to the survey. Included controls are numeracy skills, years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (i.e., open/distance education, seminars/workshops, or private lessons), an indicator for full-time employment, and firm size measured by the number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+). Industry fixed effects are at the two-digit ISIC level, and occupation fixed effects are at the two-digit ISCO level. Fixed effects are included at the one-digit level for Austria, Canada, Estonia, and Finland, where the PIAAC Public Use File does not report two-digit ISCO/ISIC codes. All regressions also control for country fixed effects. All control variables were used for the entropy balancing. 95% confidence intervals are based on standard errors two-way clustered at the country and occupation level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .  
Data source: PIAAC.



## Appendix B. Additional Tables

Table B.1: Automation Risk: Description of Included Tasks

PIAAC Variable	Variable Description
<i>Simple problems</i>	The next question is about “problem solving” tasks you do in your current job. Think of “problem solving” as what happens when you are faced with a new or difficult situation which requires you to think for a while about what to do next. How often are you usually faced by relatively simple problems that take no more than 5 minutes to find a good solution?
<i>Complex problems</i>	And how often are you usually confronted with more complex problems that take at least 30 minutes to find a good solution? The 30 minutes only refers to the time needed to THINK of a solution, not the time needed to carry it out.
<i>Communicate</i>	How often does your current job usually involve ... sharing work-related information with co-workers?
<i>Teach</i>	instructing, training or teaching people, individually or in groups?
<i>Sell</i>	selling a product or selling a service?
<i>Advise</i>	providing advice?
<i>Plan work for others</i>	planning the activities of others?
<i>Influence others</i>	working to persuade or influence people?
<i>Negotiate</i>	negotiating with people either inside or outside your firm or organization?
<i>Manual dexterity</i>	using skill or accuracy with your hands or fingers?

*Notes:* The table provides an overview of the job tasks in PIAAC used to construct the automation risk measure.

Table B.2: Factor Loadings Automation Risk: International Analysis

Task	Logit Coefficient
Plan work of others	-0.308*** (0.0234)
Influence others	-0.235*** (0.0267)
Advise	-0.199*** (0.0270)
Teach	-0.0691*** (0.0255)
Complex problems	-0.0691** (0.0297)
Negotiate	-0.0463* (0.0255)
Simple problems	0.0573* (0.0309)
Manual dexterity	0.105*** (0.0220)
Sell	0.160*** (0.0206)
Sharing information	0.214*** (0.0260)
Pseudo $R^2$	0.137
Observations	4,656

*Notes:* The table shows factor loadings from [Nedelkoska and Quintini \(2018\)](#), Table 4.3, derived from a logistic regression at the individual level. Dependent variable: Occupational automation risk from [Frey and Osborne \(2017\)](#), based on expert surveys assessing engineering bottlenecks (70 four-digit occupations). Occupations in which all tasks can be automated receive a value of 1, while those with only partially automatable tasks receive a value of 0. Independent variables: PIAAC task items corresponding to engineering bottlenecks identified by [Frey and Osborne \(2017\)](#). PIAAC assesses the frequency of task use, with responses recorded on a Likert scale ranging from 1 (never) to 5 (every day). Task items are ordered by degree of automatability. Coefficients are estimated on the Canadian PIAAC sample, which offers a detailed categorization of 4-digit ISCO occupations and provides the largest sample size among all participating PIAAC countries.

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

*Data source:* PIAAC Canada.

Table B.3: Job Tasks in German PIAAC Data

Label	Question
Problem Solving - Solve Difficult Problems	How often do you have to solve difficult problems as part of your job?
Routine - Deal with Unexpected Situations	How often do you have to react to situations at work that you could not foresee?
Interaction - Check Work of Others	How often do you have to check the quality of other people's work as part of your job?
Learning - Learn Something New	How often do you have to learn new things at work?
Autonomy - Personally Involved in Strategic Decisions	How often are you personally involved in important strategic decisions in your company, such as those regarding products, services, staffing, or finances?
Autonomy - Look for New Tasks for Yourself	How often do you have the opportunity to find new tasks for yourself at work?
Routine - Do New Things	How often do you have to do things at work that you have not done before?
Autonomy - Organize Your Own Work	How often can you organize your work yourself?
Routine - Need to Familiarize Oneself with Tasks	How often do you get tasks at work that require you to familiarize yourself with them first?
Autonomy - Quality of Your Work is Monitored	How often is the quality of your work monitored?
Routine - Task Diversity	How often do the tasks you have to complete at work change?
Autonomy - Can Determine Own Working Pace	How often can you set your own work pace?
Routine - Work Days Very Similar	How often is one workday very similar to another one for you?
Routine - Carry Out Short, Repetitive Tasks	How often do you have to carry out short, repetitive tasks in your daily work?
Routine - Get Detailed Specifications for Tasks	How often are you told how to do your job down to the last detail?

*Notes:* The table presents job tasks from the German PIAAC analysis. Task use items from the 2015 PIAAC survey, which capture the frequency of task use. Responses are recorded on a Likert scale ranging from 1 (very rarely or never) to 5 (always or very often).

*Data source:* PIAAC Germany (2015 wave).

Table B.4: Automation Risk in German PIAAC Data

Task	Logit Coefficient
Solve difficult problems	-0.3760*** (0.0516)
Deal with unexpected situations	-0.2100*** (0.0486)
Check work of others	-0.1786*** (0.0333)
Learn something new	-0.1586*** (0.0531)
Personally involved in strategic decisions	-0.1478*** (0.0330)
Look for new tasks for yourself	-0.1212*** (0.0400)
Do new things	-0.0813 (0.0590)
Organize your own work	-0.0760* (0.0445)
Need to familiarize oneself with tasks	0.0104 (0.0571)
Quality of your work is monitored	0.0466 (0.0351)
Task diversity	0.0641 (0.0488)
Can determine own working pace	0.0839** (0.0414)
Work days very similar	0.1022*** (0.0390)
Carry out short, repetitive tasks	0.1371*** (0.0499)
Get detailed specifications for tasks	0.1999*** (0.0373)
Pseudo $R^2$	0.162
Observations	3,068

*Notes:* The table presents factor loadings from the German PIAAC analysis, derived from a logistic regression at the individual level. Dependent variable: Indicator of high automation risk in 2012, defined at the occupational level. To construct this variable, we first collapse our individual-level automation risk measure from the 2012 survey (see Section 3.1) to the 4-digit ISCO occupation level. We then generate a binary indicator, assigning a value of 1 if the aggregated automation risk is 0.5 or higher, and 0 if it is below 0.5. Independent variables: Task use items from the 2015 PIAAC survey, which capture the frequency of task use. Responses are recorded on a Likert scale ranging from 1 (very rarely or never) to 5 (always or very often). Task items are ordered by degree of automatability. Standard errors shown in parentheses. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

*Data source:* PIAAC Germany (2012 and 2015 waves).

Table B.5: Change in Automation Risk over Time in Germany

	Change in automation risk 2012–2015		
	(1)	(2)	(3)
Automation risk (2012)	-0.3830*** (0.0498)	-0.6226*** (0.0300)	-0.6704*** (0.0309)
Occupation FE (2012)		X	X
Further controls (2012)			X
$R^2$	0.11	0.30	0.43
Observations	1,713	1,713	1,713

*Notes:* Ordinary least squares estimation. Dependent variable: change in individual-level automation risk between 2012 and 2015. Automation risk ranges from 0 (indicating a low probability that a worker is fully automated) to 1 (indicating a high probability that a worker is fully automated); automation risk is predicted using items on task use at work from PIAAC. Sample: employees aged 25–65 years in Germany. Occupation fixed effects are measured at the two-digit ISCO level. Further controls refer to 2012 and include numeracy skills, years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (e.g., self-organized training or seminar participation), an indicator for full-time employment, firm size measured by number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+), and industry fixed effects (two-digit ISIC). Standard errors shown in parentheses are clustered at the occupation level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

*Data source:* PIAAC Germany (2012 and 2015 waves).

Table B.6: Training and Automation Risk: Different Levels of Clustering Standard Errors

	Automation risk				
	(1)	(2)	(3)	(4)	(5)
Firm training	-0.0377*** (0.0018)	-0.0377*** (0.0022)	-0.0377*** (0.0022)	-0.0377*** (0.0020)	-0.0377*** (0.0023)
Numeracy Skills	-0.0091*** (0.0012)	-0.0091*** (0.0022)	-0.0091*** (0.0014)	-0.0091*** (0.0015)	-0.0091*** (0.0022)
Occupation FE	X	X	X	X	X
Further controls	X	X	X	X	X
Entropy balancing	X	X	X	X	X
$R^2$	0.24	0.24	0.24	0.24	0.24
Observations	91,470	91,470	91,470	91,470	91,470
Clustering	None (robust s.e.)	Country level	Occupation level	Country × occupation level	Two-way country and occupation level (baseline)

*Notes:* Least squares estimation with weights from entropy balancing. Dependent variable: individual-level automation risk, ranging from 0 (indicating a low probability that a worker is fully automated) to 1 (indicating a high probability that a worker is fully automated); automation risk is predicted using items on task use at work from PIAAC. Sample: employees aged 25–65 years with information on automation risk and log hourly wages. Firm Training: binary variable indicating whether the respondent participated in on-the-job training or job-related training in the 12 months prior to the survey. Numeracy skills are standardized to unit standard deviation across countries. Further controls include: years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (i.e., open/distance education, seminars/workshops, or private lessons), an indicator for full-time employment, and firm size measured by the number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+). Occupation fixed effects at the two-digit ISCO level. All regressions also control for industry (two-digit ISIC) and country fixed effects. Occupation and industry fixed effects are included at the one-digit level for Austria, Canada, Estonia, and Finland, where the PIAAC Public Use File does not report two-digit ISCO/ISIC codes. All control variables were used for the entropy balancing.  $R^2$  refers to within-country  $R^2$ . \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

*Data source:* PIAAC.

Table B.7: Training and Residualized Skills

	Residualized numeracy skills	Residualized digital skills
	(1)	(2)
Firm training	0.0061 (0.0059)	0.0292*** (0.0088)
Occupation FE	X	X
Further controls	X	X
Entropy balancing	X	X
$R^2$	0.08	0.08
Observations	72,180	72,180

*Notes:* Ordinary least squares estimation with weights from entropy balancing. Dependent variable: Numeracy and digital skills, standardized to unit standard deviation across countries, each residualized for literacy skills. Sample: employees aged 25–65 years with information on automation risk and log hourly wages. Firm Training: binary variable indicating whether the respondent participated in on-the-job training or job-related training in the 12 months prior to the survey. Further controls include: years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (i.e., open/distance education, seminars/workshops, or private lessons), an indicator for full-time employment, and firm size measured by the number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+). Occupation fixed effects at the two-digit ISCO level. All regressions also control for industry (two-digit ISIC) and country fixed effects. Occupation and industry fixed effects are included at the one-digit level for Austria, Canada, Estonia, and Finland, where the PIAAC Public Use File does not report two-digit ISCO/ISIC codes. All control variables were used for the entropy balancing.  $R^2$  refers to within-country  $R^2$ . Standard errors shown in parentheses are two-way clustered at the country and occupation level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

*Data source:* PIAAC.

Table B.8: Balancing

	Training		No Training		No Training (Entropy Weighted)	Difference (2)–(4)	Difference (2)–(5)
	N (1)	Mean/(SE) (2)	N (3)	Mean/(SE) (4)	Mean/(SE) (5)	p-value (6)	p-value (7)
Numeracy skills	49,968	0.283 (0.004)	41,502	-0.178 (0.005)	0.283 (0.004)	0.000	1.000
Years of schooling	49,968	14.162 (0.012)	41,502	12.486 (0.014)	14.162 (0.012)	0.000	1.000
Age							
25–34 years	49,968	0.282 (0.002)	41,502	0.265 (0.002)	0.282 (0.002)	0.000	1.000
35–44 years	49,968	0.296 (0.002)	41,502	0.271 (0.002)	0.296 (0.002)	0.000	1.000
45–54 years	49,968	0.267 (0.002)	41,502	0.268 (0.002)	0.267 (0.002)	0.778	1.000
55–65 years	49,968	0.156 (0.002)	41,502	0.197 (0.002)	0.156 (0.002)	0.000	1.000
Female	49,968	0.517 (0.002)	41,502	0.505 (0.002)	0.517 (0.002)	0.000	1.000
Migration status							
Native	49,968	0.854 (0.002)	41,502	0.849 (0.002)	0.854 (0.002)	0.018	1.000
First-generation migrant	49,968	0.111 (0.001)	41,502	0.118 (0.002)	0.111 (0.001)	0.001	1.000
Second-generation migrant	49,968	0.035 (0.001)	41,502	0.033 (0.001)	0.035 (0.001)	0.332	1.000
Parental education							
Neither parent with upper secondary education	49,968	0.295 (0.002)	41,502	0.413 (0.002)	0.295 (0.002)	0.000	1.000
At least one parent with upper secondary education	49,968	0.373 (0.002)	41,502	0.348 (0.002)	0.373 (0.002)	0.000	1.000
At least one parent with tertiary education	49,968	0.293 (0.002)	41,502	0.184 (0.002)	0.293 (0.002)	0.000	1.000
Age of oldest child							
No children	49,968	0.828 (0.002)	41,502	0.807 (0.002)	0.828 (0.002)	0.000	1.000
Below 3 years	49,968	0.038 (0.001)	41,502	0.037 (0.001)	0.038 (0.001)	0.283	1.000
3–5 years	49,968	0.027 (0.001)	41,502	0.027 (0.001)	0.027 (0.001)	0.991	1.000
6–12 years	49,968	0.037 (0.001)	41,502	0.039 (0.001)	0.037 (0.001)	0.111	1.000
13 years or more	49,968	0.070 (0.001)	41,502	0.090 (0.001)	0.070 (0.001)	0.000	1.000
Training (other)	49,968	0.043 (0.001)	41,502	0.077 (0.001)	0.043 (0.001)	0.000	1.000
Full-time employment	49,968	0.885 (0.001)	41,502	0.827 (0.002)	0.885 (0.001)	0.000	1.000
Firm size							
1–10 employees	49,968	0.175 (0.002)	41,502	0.335 (0.002)	0.175 (0.002)	0.000	1.000
11–50 employees	49,968	0.301 (0.002)	41,502	0.310 (0.002)	0.301 (0.002)	0.005	1.000
51–250 employees	49,968	0.268 (0.002)	41,502	0.205 (0.002)	0.268 (0.002)	0.000	1.000
251–1000 employees	49,968	0.145 (0.002)	41,502	0.091 (0.001)	0.145 (0.002)	0.000	1.000
> 1000 employees	49,968	0.107 (0.001)	41,502	0.051 (0.001)	0.107 (0.001)	0.000	1.000

*Notes:* Balancing table showing covariate means and standard deviations (in parentheses) in the training group (columns 1 and 2), the no-training group (columns 3 and 4), and the no-training group after entropy weighting (column 5). Entropy weighting follows [Hainmueller \(2012\)](#). Entropy weighting also includes fixed effects for two-digit ISCO occupations and two-digit ISIC industries (except Austria, Canada, Estonia, and Finland, for which we use the available one-digit information), as well as countries (not shown in the balancing table for expositional reasons).

*Data source:* PIAAC.



Table B.9: Training and Automation Risk: Occupation Fixed Effects at Four-Digit Level

	Automation risk					
	(1)	(2)	(3)	(4)	(5)	(6)
Job training	-0.0807*** (0.0051)	-0.0427*** (0.0031)	-0.0404*** (0.0029)	-0.0356*** (0.0025)	-0.0348*** (0.0022)	-0.0388*** (0.0025)
Numeracy skills			-0.0155*** (0.0023)	-0.0082*** (0.0021)	-0.0071** (0.0034)	-0.0062* (0.0037)
Occupation FE		4-digit	4-digit	4-digit	4-digit	2-digit
Further controls				X	X	X
Entropy balancing					X	X
$R^2$	0.11	0.27	0.28	0.29	0.27	0.23
Observations	48,764	48,764	48,764	48,764	48,764	48,764

*Notes:* Ordinary least squares estimation in columns (1)–(4) with weights such that each country has the same weight, least squares estimation with weights from entropy balancing in columns (5) and (6). Columns (2)–(5) include occupation fixed effects at the four-digit level. Column (6) repeats the specification in column (5) using occupation fixed effects at the two-digit level for comparison. Dependent variable: individual-level automation risk, ranging from 0 (indicating a low probability that a worker is fully automated) to 1 (indicating a high probability that a worker is fully automated); automation risk is predicted using items on task use at work from PIAAC. Sample: employees aged 25–65 years with information on automation risk and log hourly wages. Sample is restricted to the 28 countries that report occupations at the four-digit level. Firm Training: binary variable indicating whether the respondent participated in on-the-job training or job-related training in the 12 months prior to the survey. Numeracy skills are standardized to unit standard deviation across countries. Further controls include: years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (i.e., open/distance education, seminars/workshops, or private lessons), an indicator for full-time employment, and firm size measured by the number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+). Occupation fixed effects at the four-digit ISCO level. All regressions also control for industry (two-digit ISIC) and country fixed effects. Occupation and industry fixed effects are included at the one-digit level for Austria, Canada, Estonia, and Finland, where the PIAAC Public Use File does not report two-digit ISCO/ISIC codes. All control variables were used for the entropy balancing in column (5).  $R^2$  refers to within-country  $R^2$ . Standard errors shown in parentheses are two-way clustered at the country and occupation level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

*Data source:* PIAAC.

Table B.10: Training and Automation Risk: Unrestricted Sample

	Automation risk				
	(1)	(2)	(3)	(4)	(5)
Firm training	-0.0807*** (0.0017)	-0.0483*** (0.0016)	-0.0447*** (0.0016)	-0.0396*** (0.0016)	-0.0375*** (0.0017)
Numeracy skills			-0.0183*** (0.0009)	-0.0133*** (0.0009)	-0.0088*** (0.0011)
Occupation FE		X	X	X	X
Further controls				X	X
Entropy balancing					X
$R^2$	0.11	0.24	0.24	0.25	0.24
Observations	101,949	101,949	101,949	101,949	101,949

*Notes:* The table replicates Table 2 when not restricting the sample to individuals with non-missing wage information. Ordinary least squares estimation in columns (1)–(4) with weights such that each country has the same weight, least squares estimation with weights from entropy balancing in column (5). Dependent variable: individual-level automation risk, ranging from 0 (indicating a low probability that a worker is fully automated) to 1 (indicating a high probability that a worker is fully automated); automation risk is predicted using items on task use at work from PIAAC. Sample: employees aged 25–65 years. Firm Training: binary variable indicating whether the respondent participated in on-the-job training or job-related training in the 12 months prior to the survey. Controls: numeracy skills (standardized to unit standard deviation across countries), years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (i.e., open/distance education, seminars/workshops, or private lessons), an indicator for full-time employment, and firm size measured by the number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+). Occupation fixed effects at the two-digit ISCO level. All regressions also control for industry (two-digit ISIC) and country fixed effects. Occupation and industry fixed effects are included at the one-digit level for Austria, Canada, Estonia, and Finland, where the PIAAC Public Use File does not report two-digit ISCO/ISIC codes. All control variables were used for the entropy balancing in column (5).  $R^2$  refers to within-country  $R^2$ . Standard errors shown in parentheses are two-way clustered at the country and occupation level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

*Data source:* PIAAC.

Table B.11: Training and Automation Risk: Region  $\times$  Industry  $\times$  Firm Size Fixed Effects

	Automation risk	
	(1)	(2)
Job training	-0.0384*** (0.0022)	-0.0333*** (0.0040)
Numeracy skills	-0.0087*** (0.0015)	-0.0090*** (0.0016)
Occupation FE	X	X
Further controls	X	X
Entropy balancing	X	X
Region $\times$ industry $\times$ firm size FE		X
$R^2$	0.24	0.55
Observations	57,874	57,874

*Notes:* Ordinary least squares estimation with weights from entropy balancing. Dependent variable: individual-level automation risk, ranging from 0 (indicating a low probability that a worker is fully automated) to 1 (indicating a high probability that a worker is fully automated); automation risk is predicted using items on task use at work from PIAAC. Sample: employees aged 25–65 years with information on automation risk and log hourly wages. Firm Training: binary variable indicating whether the respondent participated in on-the-job training or job-related training in the 12 months prior to the survey. Numeracy skills are standardized to unit standard deviation across countries. Column (1) replicates column (5) in Table 2 for countries that provide regional information in PIAAC. Column (2) includes region  $\times$  industry  $\times$  firm size fixed effects as a proxy for firm fixed effects. Sample: Observations with regional and industry information at the two-digit level. Regional information in PIAAC is available at the two-digit territorial level, i.e., the first administrative tier of sub-national government (e.g., federal states in Germany). Regional or industry information at the two-digit level is not available for 8 out of 37 countries in our sample (Australia, Austria, Canada, Estonia, Finland, Norway, Turkey, and the United States). Industry fixed effects are at the two-digit ISIC level, whereas firm size is measured by the number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+). Further controls include: years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (i.e., open/distance education, seminars/workshops, or private lessons), and an indicator for full-time employment. Occupation fixed effects at the two-digit ISCO level. All regressions also control for industry (two-digit ISIC) and country fixed effects. All control variables included in the respective specification were used for the entropy balancing.  $R^2$  refers to within-country  $R^2$ . Standard errors shown in parentheses are two-way clustered at the country and occupation level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

*Data source:* PIAAC.

Table B.12: Alternative Measure: Training and Routine Intensity

	Routine intensity index				
	(1)	(2)	(3)	(4)	(5)
Firm training	-0.0731*** (0.0061)	-0.0433*** (0.0043)	-0.0378*** (0.0041)	-0.0374*** (0.0041)	-0.0273*** (0.0032)
Numeracy skills			-0.0297*** (0.0025)	-0.0224*** (0.0022)	-0.0198*** (0.0017)
Occupation FE		X	X	X	X
Further controls				X	X
Entropy balancing					X
$R^2$	0.09	0.20	0.21	0.22	0.20
Observations	91,242	91,242	91,242	91,242	91,242

Notes: Ordinary least squares estimation in columns (1)–(4) with weights such that each country has the same weight, least squares estimation with weights from entropy balancing in column (5). Dependent variable: individual-level routine intensity index, constructed following Marcolin et al. (2016) based on workplace tasks in PIAAC; index ranging from 0 (indicating a low routine intensity) to 1 (indicating a high routine intensity); Sample: employees aged 25–65 years with information on workplace tasks, automation risk, and log hourly wages. Firm Training: binary variable indicating whether the respondent participated in on-the-job training or job-related training in the 12 months prior to the survey. Numeracy skills are standardized to unit standard deviation across countries. Further controls include: years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (i.e., open/distance education, seminars/workshops, or private lessons), an indicator for full-time employment, and firm size measured by the number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+). Occupation fixed effects at the two-digit ISCO level. All regressions also control for industry (two-digit ISIC) and country fixed effects. Occupation and industry fixed effects are included at the one-digit level for Austria, Canada, Estonia, and Finland, where the PIAAC Public Use File does not report two-digit ISCO/ISIC codes. All control variables were used for the entropy balancing in column (5).  $R^2$  refers to within-country  $R^2$ . Standard errors shown in parentheses are two-way clustered at the country and occupation level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Data source: PIAAC.

Table B.13: Training and Automation Risk: Coefficient Bounds

	$\delta = 0.8$	$\delta = 1$	$\delta = 1.2$
$R_{max} = 1.3\tilde{R}$	-0.0360*** (0.0017)	-0.0351*** (0.0018)	-0.0341*** (0.0016)
$R_{max} = 1.4\tilde{R}$	-0.0329*** (0.0015)	-0.0311*** (0.0019)	-0.0293*** (0.0016)
$R_{max} = 1.5\tilde{R}$	-0.0234*** (0.0010)	-0.0192*** (0.0018)	-0.0147*** (0.0014)
Observations	91,470	91,470	91,470

Notes: This table shows coefficient bounds following Oster (2019) of our baseline firm training estimate (see Table 2, column 5) for different assumptions regarding  $R_{max}$  and  $\delta$ .  $R_{max}$  is the hypothetical maximum R-squared that can be explained by observables and unobservables, as a factor of  $\tilde{R}$ , the R-squared of our baseline regression with observable covariates in column (5) of Table 2 ( $\tilde{R} = 0.24$ ). We follow Chen (2021) in relying on the within- $\tilde{R}$  (i.e., after partialling out country fixed effects) to estimate all values. Following the recommendation in Oster (2019), we provide coefficient bounds for  $R_{max} = 1.4\tilde{R}$  and additional checks for the cases  $R_{max} = 1.3\tilde{R}$  and  $R_{max} = 1.5\tilde{R}$ .  $\delta$  indicates the degree of selection on unobservables relative to observables; if  $\delta = 1$ , unobservable factors are as influential as observable ones in determining the outcome. Again following Chen (2021), we include the case of  $\delta = 1$ , and additionally provide coefficient bounds for  $\delta = 0.8$  and  $\delta = 1.2$ . Standard errors shown in parentheses are two-way clustered at the country and occupation level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Data source: PIAAC.

Table B.14: Training and Automation Risk (Leaner 2015 Index): Value-Added Approach for Germany

	Automation risk (2015)			
	(1)	(2)	(3)	(4)
Firm training (2014)	-0.0695*** (0.0117)	-0.0581*** (0.0109)	-0.0536*** (0.0106)	-0.0540*** (0.0106)
Automation risk (2012)		0.2366*** (0.0383)	0.2094*** (0.0377)	0.2087*** (0.0407)
Firm training (2012)			-0.0450*** (0.0099)	-0.0422*** (0.0096)
Log hourly wage (2012)				-0.0275* (0.0162)
Numeracy skills (2012)	-0.0182* (0.0097)	-0.0146 (0.0087)	-0.0125 (0.0093)	-0.0090 (0.0092)
Occupation FE (2012)	X	X	X	X
Further controls (2012)	X	X	X	X
Entropy balancing	X	X	X	X
$R^2$	0.31	0.34	0.34	0.34
Observations	1,601	1,601	1,601	1,601

Notes: Replicates Table 4 using a leaner automation index in 2015 based on the seven workplace task questions that most closely align with the theoretical foundations of task-based models of automation (see text for details). Ordinary least squares estimation with weights from entropy balancing. Dependent variable: individual-level automation risk, elicited in the 2015 PIAAC survey. Automation risk ranges from 0 (indicating a low probability that a worker is fully automated) to 1 (indicating a high probability that a worker is fully automated); automation risk is predicted using items on task use at work from PIAAC. Sample: employees aged 25–65 years in Germany. Firm training, elicited in the 2015 PIAAC survey, refers to the year 2014. All control variables were elicited in the 2012 PIAAC survey. Occupation fixed effects are measured at the two-digit ISCO level. Numeracy skills are standardized to unit standard deviation within Germany. Further controls include: years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (e.g., self-organized training or seminar participation), an indicator for full-time employment, firm size measured by number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+), and industry fixed effects (two-digit ISIC). Standard errors shown in parentheses are clustered at the occupation level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Data source: PIAAC Germany (2012 and 2015 waves).

Table B.15: Training and Automation Risk (Leaner 2015 Index): Panel Analysis for Germany

	<b>Automation risk</b>			
	(1)	(2)	(3)	(4)
Firm training	-0.0130 (0.0086)	-0.0150* (0.0087)	-0.0135 (0.0088)	-0.0119 (0.0087)
Individual FE	X	X	X	X
Survey year FE	X	X	X	X
Industry FE		X	X	X
Occupation FE			X	X
Further controls				X
$R^2$	0.69	0.70	0.72	0.72
Individuals	1,887	1,887	1,887	1,887
Observations	3,774	3,774	3,774	3,774

Notes: Replicates Table 5 using a leaner automation index in 2015 based on the seven workplace task questions that most closely align with the theoretical foundations of task-based models of automation (see text for details). Occupation and industry fixed effects panel estimation, using the 2012 and 2015 waves of PIAAC Germany. Dependent variable: individual-level automation risk, ranging from 0 (indicating a low probability that a worker is fully automated) to 1 (indicating a high probability that a worker is fully automated); automation risk is predicted using items on task use at work from PIAAC. Sample: employees aged 25–65 years in Germany (restrictions refer to the values in the 2012 survey). Industry fixed effects are measured at the two-digit ISIC level, occupation fixed effects are measured at the two-digit ISCO level. Further controls are time-varying and include: age, an indicator for full-time employment, and firm size measured by number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+). Observations are not weighted. Standard errors shown in parentheses are clustered at the individual level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Data source: PIAAC Germany (2012 and 2015 waves).

Table B.16: Change in Training Effectiveness over Time in the United States and Germany

	Automation risk					
	United States			Germany		
	Pooled (1)	2012 Only (2)	2017 Only (3)	Pooled (4)	2012 Only (5)	2015 Only (6)
Job training	-0.0319*** (0.0033)	-0.0271*** (0.0096)	-0.0397*** (0.0119)	-0.0469*** (0.0073)	-0.0357*** (0.0092)	-0.0726*** (0.0098)
Numeracy skills	0.0029 (0.0035)	0.0007 (0.0071)	0.0097 (0.0071)	-0.0190*** (0.0044)	-0.0192*** (0.0043)	-0.0152* (0.0087)
$R^2$	0.32	0.37	0.34	0.30	0.31	0.41
Observations	4,367	2,430	1,937	4,976	3,069	1,907

*Notes:* Least squares estimation with weights from entropy balancing. Dependent variable: individual-level automation risk, ranging from 0 (indicating a low probability that a worker is fully automated) to 1 (indicating a high probability that a worker is fully automated); automation risk in each wave is predicted using items on task use at work from PIAAC. Sample: employees aged 25–65 years in the United States (columns 1–3) and in Germany (columns 4–6); restrictions are applied in each survey wave separately. Firm Training: binary variable indicating whether the respondent participated in on-the-job training or job-related training in the 12 months prior to the survey; the 2015 PIAAC wave in Germany elicits participation in firm training in 2014. Numeracy skills are standardized to unit standard deviation across countries. Numeracy skills are standardized to unit standard deviation within Germany. Further controls include: years of schooling, years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (i.e., open/distance education, seminars/workshops, or private lessons), an indicator for full-time employment, and firm size measured by the number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+). Analysis for Germany excludes controls for child-related variables and non-work-related training, as these data were not collected in the 2015 PIAAC wave. Occupation fixed effects at the two-digit ISCO level. All regressions also control for industry (two-digit ISIC) and country fixed effects. Entropy balancing is based on all control variables included in the respective specification. Standard errors shown in parentheses are clustered at the occupation level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

*Data source:* PIAAC United States (2012 and 2017 waves); PIAAC Germany (2012 and 2015 waves).

Table B.17: Training and Digital Skills, Imputed Digital Skills for Missing Data

	Baseline	Digital skills		
		Missings imputed with		
		zeros	global minimum	country minimum
	(1)	(2)	(3)	(4)
Firm training	0.0481*** (0.0089)	0.0939*** (0.0100)	0.0940*** (0.0098)	0.0855*** (0.0097)
Numeracy skills	0.7678*** (0.0102)	0.3392*** (0.0144)	0.3800*** (0.0142)	0.4309*** (0.0188)
Observations	72,180	91,470	91,470	91,470
$R^2$	0.59	0.33	0.37	0.41
Occupation FE	X	X	X	X
Further controls	X	X	X	X
Entropy balancing	X	X	X	X

*Notes:* Least squares estimation with weights from entropy balancing. Dependent variable: digital skills standardized to standard deviation 1 across countries with different imputations. Sample: employees aged 25–65 years with information on automation risk and log hourly wages; column (1) replicates column (5) of Table 6, that is, it is restricted to employees with non-missing digital skills. Firm Training: binary variable indicating whether the respondent participated in on-the-job training or job-related training in the 12 months prior to the survey. Controls: numeracy skills (standardized to unit standard deviation across countries), years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (i.e., open/distance education, seminars/workshops, or private lessons), an indicator for full-time employment, and firm size measured by the number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+). Occupation fixed effects at the two-digit ISCO level. All regressions also control for industry (two-digit ISIC) and country fixed effects. Occupation and industry fixed effects are included at the one-digit level for Austria, Canada, Estonia, and Finland, where the PIAAC Public Use File does not report two-digit ISCO/ISIC codes. All regressions also control for country fixed effects. All control variables were used for the entropy balancing.  $R^2$  refers to within-country  $R^2$ . Standard errors shown in parentheses are two-way clustered at the country and occupation level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

*Data source:* PIAAC.



Table B.18: Training and Basic Digital Skills

	Basic digital skills				
	(1)	(2)	(3)	(4)	(5)
Firm training	0.1351*** (0.0125)	0.0894*** (0.0085)	0.0718*** (0.0074)	0.0589*** (0.0056)	0.0356*** (0.0044)
Numeracy skills			0.0958*** (0.0096)	0.0726*** (0.0084)	0.0505*** (0.0068)
Occupation FE		X	X	X	X
Further controls				X	X
Entropy balancing					X
$R^2$	0.08	0.15	0.18	0.23	0.15
Observations	91,470	91,470	91,470	91,470	91,470

*Notes:* Ordinary least squares estimation in columns (1)–(4) with weights such that each country has the same weight, least squares estimation with weights from entropy balancing in column (5). Dependent variable: Binary indicator for basic digital skills, which takes a value of one if the respondent was able to participate in PIAAC in a computer-based mode, zero otherwise. There are three reasons for why individuals may lack basic digital skills in PIAAC (see Falck et al., 2021, 2022): (i) individuals had no prior computer experience; (ii) individuals failed a computer core test, which assessed basic digital competencies such as using a keyboard/mouse or scrolling through a web page; (iii) individuals refused to take part in the computer-based assessment. Sample: employees aged 25–65 years with information on automation risk and log hourly wages. Firm Training: binary variable indicating whether the respondent participated in on-the-job training or job-related training in the 12 months prior to the survey. Controls: numeracy skills (standardized to unit standard deviation across countries), years of schooling, age group in four categories (25–34, 35–44, 45–54, 55–65), gender, migration status in three categories (first-generation migrant, second-generation migrant, native), parental education in three categories (neither parent has attained upper secondary, at least one parent has attained secondary and post-secondary/non-tertiary, at least one parent has attained tertiary), an indicator whether the respondent has children, age group of the oldest child in four categories (0–2, 3–5, 6–12, 13+), an indicator for participation in non-job-related training (i.e., open/distance education, seminars/workshops, or private lessons), an indicator for full-time employment, and firm size measured by the number of employees in five categories (1–10, 11–50, 51–250, 251–1000, 1000+). Occupation fixed effects at the two-digit ISCO level. All regressions also control for industry (two-digit ISIC) and country fixed effects. Occupation and industry fixed effects are included at the one-digit level for Austria, Canada, Estonia, and Finland, where the PIAAC Public Use File does not report two-digit ISCO/ISIC codes. All control variables were used for the entropy balancing in column (5).  $R^2$  refers to within-country  $R^2$ . Standard errors shown in parentheses are two-way clustered at the country and occupation level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

*Data source:* PIAAC.



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