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Essays in Labor Economics

by

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DEPARTMENT OF ECONOMICS AND BUSINESS

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Disclosure of coauthor contribution

Gender Differences in Skill Content of Jobs

Co-author: Balázs Reizer

The paper was developed in close cooperation with Balázs Reizer throughout all stages. We contributed equally to the idea of the paper and the data collection, data management, programming and analysis of the regression. All authors contributed equally.

The Salary Premium of Adopting a Hungarian Surname in a Multi-ethnic Austria-Hungary

Co-author: Attila Gáspár

The paper was developed in cooperation with Attila Gáspár from our research project on the economics of name changing. We developed both the main question and the identification strategy of the paper together. We contributed equally to the data collection, cleaning effort and analysis.

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Abstract

Two of the three essays are investigating the question about the role of skills in the labor market, while the third chapter looks at the labor market consequences of identity changes. The first chapter shows how a foreign takeover affects the return to specific skills, I found that the return on independent problem solving skills increases, while the returns on other skills are unchanged. The second chapter (joint with Balázs Reizer) study the gender differences in skill content of jobs, it shows that having family significantly increases the gap of skill use between men and women, we argue that time allocation by the family members is the potential mechanism driving the results. The third chapter (joint with Attila Gáspár) uses historical data to study the labor market impact of changing a foreign sounding surname to a Hungarian sounding one.

Chapter 1: Foreign Acquisition and the Return to Skills

I study the effect of foreign takeovers on the return to specific skills. Using administrative data on Hungarian workers and firms augmented with occupation level skill requirement measures, I find a positive and significant increase in the return to independent problem solving skills after a foreign acquisition, while the effect is smaller and less robust in the case of interpersonal skills, and I find no effect on the return to routine task intensity. These results are not driven by a subgroup of workers (such as managers) or by any special firm types (such as manufacturing firms), the pattern is general. I also show that the change in the valuation of independent problem solving skills can explain the increase in the white-collar wage premium. I argue that these findings are in line with the hypothesis that foreign investors decentralize the firm structure after the takeover.

Chapter 2: Gender Differences in the Skill Content of Jobs

co-author: Balázs Reizer

More than half of the gender wage gap can be attributed to differences in wages within occupations. Using the PIAAC survey, we show that women perform less skill-intensive tasks than men even within the same occupation. The gap in skill intensity cannot be explained by differential firm characteristics or differences in cognitive skills. Instead, we show that having a child significantly decreases the skill use of women and slightly increases the skill use of men. We argue that having a child affects skill use through time allocation by the parents as the child penalty disappears once we control for working hours and hours spent on housework. Finally, we do not find evidence for workplace discrimination against women.

Chapter 3: The salary premium of adopting a Hungarian surname in multi-ethnic Austria-Hungary

co-author: Attila Gáspár

By using three unique historical data sets, we study the labor market impact of changing a foreign sounding surname to a Hungarian sounding one in the early 20th century Hungary. We use pooled OLS and a name frequency based instrumental variable to estimate the impact of family name change on worker salary. We find that name changers earned 5 percent to 10 percent more than other workers in our two samples of contemporary workers. The results are mostly driven by the public sector. We interpret this as a signaling mechanism through which workers could send a costly signal (the name change) to show political loyalty and the willingness to assimilate.

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Chapter I

Foreign Acquisition and the Return to Skills

1 Introduction

There is vast empirical evidence in the literature documenting that foreign firms outperform domestic companies in several dimensions: foreign firms are larger, more productive and pay higher wages[1]. Foreign investors may bring benefits to the host country by having access to superior technologies and managerial practices that can spread to the domestic firms. Domestic workers can also benefit from the presence of multinational companies by acquiring valuable knowledge about the new technologies or receiving more training[2].

Although there is a consensus in the literature about the positive foreign-domestic wage gap[3], there are only a few studies that analyze the heterogeneity of the wage effect of foreign ownership and the conclusions are unclear. A study of Sjöholm and Lipsey [2006] on Indonesian manufacturing workers provides evidence that the wage effect is larger for white-collar workers, and Earle et al. [2018] show that those having higher skills - as measured by education for Hungarian workers - experience a larger gain at foreign acquisition. In the case of the UK, Girma and Görg [2007] found the opposite: the wage of unskilled workers increases twice as much as that of skilled workers. I show evidence for a skill-biased effect of a foreign takeover, the wage of white-collar workers increases by 8% after the takeover, while the wage of blue-collar workers is unchanged. Although defining skilled workers by white-collar or based on education is widely used in the literature, it obscures some important mechanisms because skilled workers measured by these definitions have very diverse skills [Ingram and Neumann, 2006].

[1] see Lipsey [2004] for an overview

[2] see Huttunen [2007] for a summary about the reasons for the existing foreign-domestic wage gap.

[3] with a substantial variation in the magnitude. The effect is considered to be larger in developing countries, while being small in developed countries (see Arnal and Hijzen, 2008 for an overview).

In this paper, I detect specific skills that are appreciated and that are devaluated after a foreign acquisition. To do so, I lean on detailed job descriptions and create three skill requirement indexes: (i) independent problem solving skills (IPS); (ii) interpersonal skills; (iii) routine task-intensity (RTI). While the first two measures are based on Hungarian data provided by the former Labour Office, RTI is based on US data and it is a widely used index to measure how easily the job can be substituted by computers. My focus on the skill set is motivated by a large literature that uses skill requirement measures and job descriptions to understand recent changes in the labor market[4].

Using a linked employer-employee dataset from Hungary, I find evidence that independent problem solving skill is rewarded more after a foreign takeover. The finding is robust to the inclusion of worker and firm fixed effects (along with sector-year fixed effects) and the inclusion of a wide range of control variables on worker and firm level as well. The increased return to this skill seems to be a general effect, as it is there for females and males, and also for low educated workers[5]. The measured increase in the return does not vary much by the size of the firm, although it is somewhat larger for smaller firms. Although the majority of the literature investigating the effect of foreign takeovers focuses only on manufacturing firms, Balazsi et al. [2018] show that the wage advantages and spillover effect are relevant also in the service sector, at least in the case of Hungary. I also show evidence that the increase in the return is relevant in the service sector and it is comparable in size with the effect in the manufacturing sector. The estimated results are not driven by newcomers (workers arriving to the firm after the takeover) either. I also control for mathematics skill requirement of the occupation to rule out the possibility of measuring solely a pattern that better skills are appreciated more after a takeover. I also estimate the effect separately for those who do not reach a managerial position, but the increase in the return is relevant for this subgroup as well.

I demonstrate that the effect of acquisition is skill-biased, favoring white-collar workers: the wage of white-collar workers increases by 8% after a foreign takeover, while the wage

[4]see David and Dorn [2013] for an analysis about the US labor market and Goos et al. [2014] for a study about European countries

[5]education is proxied by the education requirement of the highest occupational status achieved in 2003-2011

of blue-collar workers is unaffected. This gap decreased to 2,3% and became insignificant after controlling for independent problem solving skill index.

The results on the other two skill measures are less robust. Although there is an increase in the return to interpersonal skills after a foreign takeover, this effect disappears as soon as independent problem solving skills is included in the regression. By using event study approach, I did not find any change in the return to routine task-intensity after a foreign takeover compared to the return at always domestic firms.

While the effect of foreign ownership on wages is widely studied in the literature, less attention is paid on the effect on the composition. The results about the skill mix are less clear. Huttunen [2007] shows either no effect or a small decrease in the share of highly educated workers after a foreign acquisition in the case of Finnland. Almeida [2007] did not find evidence for the change in the composition of workforce after a foreign takeover for Portugal firms, while, Earle et al. [2018] found an increase in the share of university graduated workers in the case of Hungary. I found a small positive effect on the share of high IPS skill workers. The share of high interpersonal skill workers and those whose jobs are considered to be more routine are basically unchanged after the foreign takeover.

In the final part of the paper, I investigate the potential mechanism which can lead to my empirical results. Although I do not have direct evidence, I argue that independent problem solving skill summarizes the skills that are appreciated in a decentralized firm, thus the finding that there is an increasing return to these skills is in line with the hypothesis that foreign investors change the organization of the firm in a way that it becomes more decentralized. The finding that the worker/manager ratio increases after a foreign take-over further supports that hypothesis.

Finally I rule out some alternative hypotheses. As recent technological changes complement non-routine, information demanding tasks my results could simply just mimic the spread of computers instead of reflecting an organizational change. On the one hand, new technologies complement non-routine tasks, on the other hand, they are able to substitute routine tasks that follow logical rules as they are programmable (Autor et al., 2003; David and Dorn, 2013). This scenario is ruled out by using RTI that is a widely used measure to examine how easily the job can be substituted by computers (Goos et al., 2014). While the return to IPS skill increases after a foreign takeover even after controlling for RTI; there is no significant effect of foreign acquisition on the return to RTI. This suggests that technological change (related to computerization, automatization) has only a secondary role if any. I also show that my results are not driven by a pattern that, better skills in general are appreciated more after a takeover, and they are not driven by managers either just because managerial positions demand a higher level of IPS skills. I also rule out the possibility that my results are driven by the mechanism that firms tend to grow after a foreign take-over and the evaluation of skills changes due to this pattern.

The paper also contributes to the literature about measuring the return to specific workplace tasks and skills. It is a widely studied phenomenon that there is a decreasing return to routine tasks (Acemoglu and Autor, 2011; Autor et al., 2003; David and Dorn, 2013; Goos et al., 2014). There is less evidence on social skills and the results are rather unclear. While Deming [2017] reports an increasing return on social skills by using survey data, Abraham and Spletzer [2009] show that jobs that require a higher level of interpersonal skills pay lower wages. I contribute to this literature in two ways. First, I also focus on social skills by measuring interpersonal skills. Second, I provide evidence on how the valuation of skills can be shifted by changes in the life of the firms.

My study also relates to the literature on the effect of foreign ownership on firm organization. Bloom et al. [2012] argue that multinational companies implement their management style abroad. Bastos et al. [2018] found evidence of changes in the internal organization of firms after a foreign acquisition. Their results are in line with the hypothesis of knowledge-based hierarchies in which management practices have been improved and the evidence from an auxiliary survey suggests that as a background internal communication costs are reduced. Guadalupe et al. [2012] show that firms after a foreign takeover purchase new machines and adopt new methods of organization simultaneously. I add to this literature by showing that skills that are demanded at a decentralized firm are appreciated more after a foreign takeover and by arguing that this is in line with decentralization after a foreign takeover.

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2 Data and descriptive statistic

2.1 Data sources

2.1.1 Panel of administrative data

I use a large, longitudinal dataset linking administrative data from the National Health Insurance Fund Administration, the Central Administration of National Pension Insurance, the National Labour Office and the Educational Authority, provided the Databank of the Centre for Economic and Regional Studies of the Hungarian Academy of Sciences. The data set covers a randomly chosen 50% sample of the Hungarian population aged 5-74 in 2003 and the individuals are followed from January 2003 to December 2011 on a monthly basis. I have information on the highest paying job of a given person in a given month, days of work and amounts earned. I know the occupation and the type of employment relationship of the individual along with her or his gender, age and proxies of health status. I also know whether the individual receives transfers, but not educational attainment, only the the highest occupational status achieved in 2003-2011. A categorical variables based on this can be used as a proxy for education.[6] The data set provides information on the occupations of the workers by 4 digit occupation codes (FEOR 93). These codes are used in the dataset between 2003 and 2010, but there is a change in the use of the occupation coding system for the last year (2011). To keep the coding system consistent, I drop the last year of the sample.

The individual level data is augmented with firm level information provided by the National Tax and Customs Administration. The most important information at the firm level is the ownership status of the firm, but financial data and information on size and industry are also available. Firm information is given at the yearly level.

Although the worker level information is available on a monthly basis, due to the size of the data set (and because firm information is only available on a yearly basis) I keep

[6] the same measure as used by Balazsi et al. [2018]. Three categories are defined: low, medium and high level of education.

only information from March.[7].

The sample is restricted to workers employed with labor contract at least once in a foreign or domestic private firm, where the firm has more than 10 employees at least once during the observed period and the worker has at least three observations with non-missing data on wage, the main independent variables (skill requirement indexes, ownership) and the main control variables (age, disability, care, tenure and industry).[8][9] Observations with missing variables were excluded from the sample. The restricted sample contains 5,356,887 person-year observations corresponding to 940,872 workers working at 156,906 firms.

A firm is considered to be foreign if foreign ownership share is above 50%. 34.4% of the worker-year observations in my sample are majority foreign-owned. I split foreign firms into two groups. I define firms to be always foreign if they entered my data as foreign firms, and acquired firms are those which were domestically owned at the beginning and became foreign firms later on.[10] Since I only have information on the year of the acquisition (no exact date is reported), I use lagged ownership status to define post-acquisition years. 3.5% of the worker-year observations correspond to acquired firms, and 1.4 % of the worker-year observations correspond to post-acquisition years. Table 1 shows the number of acquisitions per year. The number of cases varies between 91 and 212 per year.

I use daily wages (monthly wage divided by days in work) normalized for the national average daily wage in the given month.[11]

[7]Although the choice of the month seems to be arbitrary, I chose March as the first representative month in any given year. On the other hand, firm level information is on a yearly level, therefore I do not see the exact date of the acquisition. It is very likely that the acquisition of any given year took place later during the year, so I take the year of the acquisition as a pre-acquisition year (I use lagged ownership statuses to define post-acquisition years).

[8]I include all of the employers of such a worker (thus firms never exceeding the 10 employees threshold might be included as well.)

[9]See more about the restriction rules and their necessity in Balazsi et al. [2018]

[10]I use the entire data set to define a firm to be acquired, thus if the firm was acquired in 2011 it is considered to be an acquired firm.

[11]I use the same definition as Balazsi et al. [2018]

Table 1: Number of acquisitions per year

Year	Number of Acquisitions
2004	165
2005	107
2006	142
2007	205
2008	212
2009	164
2010*	91
2011*	100

*As my sample ends in 2010, but the ownership is known until 2011, for firms acquired in 2010 or in 2011 I do not have pos-acquisition years.

2.1.2 Occupation description

I augment my linked employer-employee data with detailed job descriptions from external data sources. I use the official descriptions of the Hungarian Occupation Coding System (FEOR) augmented with detailed and standardized descriptions by the former National Labor Office of Hungary.[12] The aim of these descriptions was partly to give up-to-date information about the occupation (such as descriptions of the tasks, health risk, average salary etc.). More importantly for my analysis, it also aimed to help job seekers to find the best match. To achieve their aim they developed measures that help job seekers to compare skill requirements and working environments for each job. I use these indices to measure the skill requirement of the job. The job descriptions and the indices are available at 4 digit occupation code level. The former National Labor Office report the skill requirement measures by FEOR08 occupation codes, and my data set contains FEOR93 occupation codes. To overcome this issue, I use the official correspondence table of the two coding systems.

2.2 Skill requirement indices

The Labor Office provides fifteen skill requirement measures (see the list of the measures in Table A-1), all are categorical variables measured on a scale 0 (not important) to 3 (highly important) for a particular occupation.

[12]the descriptions are available at http://eletpalya.munka.hu/

In some cases, the Labor Office does not report job requirement information at the 4 digits level. I impute these missing observations in two ways. The detailed job descriptions and skill requirement measures are always missing if the FEOR code ends with a “9”. These codes correspond to the “other” or “n.e.c.” categories (such as 1339 Department managers in production and services n.e.c.). In these cases, I use the average of the skill requirement measures being in the same 3 digit level occupation group.

For occupations the code of which do not end in 9 I imputed the skill requirements based on the skill requirements of occupations that were reported by the office to require similar skill sets.[13] In the cases when no occupation with the same skill requirement (at the same 1 digit level) was reported, I drop the occupation category from my sample.

At the end of this procedure, I have the skill requirement measures for almost 350 distinct occupation groups.

Due to multicollinearity issues, all variables cannot be used in the analysis. I created two summary indices. I chose the skill requirement indices in my analysis to have economically meaningful summary indices and also based on the correlation between the indices (see the list of the measures in Table A-1). My first skill measure is the independent problem solving (IPS), which summarizes: problem solving, taking responsibility, ability to adapt to new environments and tasks, the ability to focus on work, the ability to work independently. Co-working skill, communication skill, and empathy are summarized in my interpersonal skill measure.

The summary indexes are the weighted average of the relevant skill requirement measures where the weights were calculated by Thomson’ method, which defines the weights as the product of the factor loading matrix and the inverse covariance matrix [Estabrook and Neale, 2013].[14]To gain economically meaningful variables I calculate the factor loading matrix by keeping only the relevant variables in the analysis and allowed to have a single factor, e.g. I calculate factor loading matrix only by using co-working

[13]For example in the case of the occupation titled “Electrical engineers” (FEOR08 2122) the office does not report the skill measures, but it states that the occupation titled “Telecommunications engineers” (FEOR08 2123) requires the same skill sets.

[14]all of the weights are positive

skills, communication skills, and empathy. [15]

I also use routine task-intensity (RTI) recommended by Goos et al. [2014] to measure how easily the job can be substituted by computer to capture the effect of technology and computerization on the workforce. This measure is based on US data and it is widely used in the literature[16].

In addition to these, I further use mathematics skill requirement to capture whether the job is considered to be more skill-intensive. This shows whether the job involves working with numbers and operations.

The indices are standardized to have zero mean and standard deviation of one in the sample.

The correlation coefficients between the skill measures can be found in Table 2. All of them are statistically significant, suggesting that there is a link between the set of skills that are required to fulfill a given occupation. According to the estimated correlations jobs that require a high level of IPS skill from a worker also require good interpersonal skills, and at the same time, they are considered to be less routine. Although the indices are linked, they are conceptually different. For example "Civil engineers" (FEOR 2124) requires a high level of IPS skills, but a low level of interpersonal skills. On contrary "Occupations in making up consignment of goods" (FEOR 5114) require a low level of IPS skills but a high level of interpersonal skills. "Film, stage and related assistant directors" (FEOR 3722) are a good example to show the difference between routines and independent problem solving skill set as well: it requires a low level of IPS skills but it is considered to be a non-routine job as well, according to the RTI measure. "Accounting clerks" (FEOR 3606), "Planning clerks" (FEOR 3611) and "Investment clerks" (FEOR 3611) require a high level of IPS skill set but they consist of routine tasks. There are also non-routine jobs that are at the bottom of IPS and interpersonal skill distrubtion, for example "Food and beverage industry technicians" (FEOR 3113). Also, there are jobs that are considered to involve routine tasks and do not require neither independent problem solving skills nor

[15]The main results are robust to calculating the weights by the inverse covariance matrix suggested by Anderson [2008], or by using the unweighted average of the variables.

[16]this measure was used by David and Dorn, 2013 in case of the US and by Goos et al., 2014 in case of European countries.

Table 2: Correlations between skill measures

	IPS	Interpersonal	RTI
Interpersonal	0.64***		
RTI	-0.30***	-0.22***	

interpersonal skills, for example, "Library and archive stock clerks, other filing clerks" (FEOR 4123).

The top 10 and bottom 10 IPS and interpersonal skill occupations are listed in Table A-2. As among the top 10 IPS skill occupations there are only managers (first panel of Table A-2), later I show evidences that my results are robust to the exclusion of managers. There is a larger variation in the bottom 10 IPS skill occupations, not surprisingly elementary occupations are over-represented here. Interpersonal skills are important for teachers, doctors, psychologist, while they are less important for agriculture related occupations.

2.3 Descriptive Statistics

Table 3 and Table 4 provides descriptives on firm and worker characteristics by ownership. Foreign firms are larger and they also export more often than domestic firms. Foreign firms are more prevalent in the manufacturing industry. There are also differences in workforce composition (see Table 4). Foreign firms have a younger workforce and a larger share of female workers, but the share of workers with disability allowance is lower. The educational composition is similar in the two types of firms, although the share of medium educated workers is somewhat larger at foreign firms. The average IPS skill is higher, while the average interpersonal skill is lower at foreign firms.

3 Methodology

I estimate the following model:

Table 3: Firm characteristics by ownership

Variables	Domestic firm	Foreign Firm
size	23	90
	(236)	(343)
exporting firms (%)	16.3	58.2
Industry (%)		
Agriculture	4.2	3.1
Manufakturing	18.2	29.1
Construction	11.5	2.9
Trade and repair	28.4	33
Finance, insurance	1.3	2
Utilities	0.7	0.9
Business services	35.7	28.6

Domestic firm: firms that are under domestic ownership in the given year (e.g. always domestic firm, pre-acquisition period, and years after foreign investment withdrawal). Foreign firm: firms that are under foreign ownership in the given year.

Table 4: Individual characteristics by ownership

Variable	Domestic firm	Foreign Firm
Age	39.4	37.3
	(10.6)	(10.2)
Male (%)	61.1	52.7
Workers receives care of child allowance (%)	0.8	0.8
Workers receiving disability allowance (%)	1.4	0.5
Tenure in current job (month)	25.2	28.3
	(22.4)	(23.5)
Education (%)		
Low educated worker	16.1	16.1
Medium educated worker	67.9	69.2
High educated worker	16	14.7
Average IPS skill	-0.03	0.06
	(1.02)	(0.97)
Average interpersonal skill	0.03	-0.06
	(1.00)	(0.98)
Average RTI	-0.05	0.10
	(1.04)	(0.92)
Observations	3 513 824	1 843 063

Domestic firm: firms that are under domestic ownership in the given year (e.g. always domestic firm, pre-acquisition period, and years after foreign investment withdrawal). Foreign firm: firms that are under foreign ownership in the given year. The skill measures are standardized to have zero mean and standard deviation of one in the sample.

$$lnw_{ijot} = \delta_1 AlwaysFor_j + \delta_2 Acquired_j + \delta_3 PostAcq_{jt} + \alpha SkillMeasure_o +$$
$$\gamma_1 AlwaysFor_j * SkillMeasure_o + \gamma_2 Acquierd_j * SkillMeasure_o + \gamma_3 PostAcq_{jt-1} * SkillMeasure_o$$
$$+ \mu Math_o + [\rho P_i] + \beta X_{it} + \gamma V_{jt} + [v_i + f_j] + s_{jt} + \varepsilon_{ijt} \quad (1)$$

The dependent variable is the logarithm of the daily wage of person i having an occupation o at firm j in year t. I include a set of dummies controlling for the ownership status of the firm: $AlwaysFor_j$ is a dummy that equals 1 if the firm was foreign owned at the start of the study period. $Acquired_j$ is a dummy that equals 1 for firms that were domestic owned at the start of the study period but became foreign owned later on in the sampling period; $PostAcq_{jt}$ is a dummy showing the years after the acquisition took place. $SkillMeasure_o$ is the skill requirement index of a given occupation where the worker is currently employed (namely IPS, Interpersonal skills and RTI). I include a dummy showing whether the job requires working with numbers and operations to account for the general skill requirement level of the job ($Math_o$). P_i are time invariant worker characteristics (such as gender, education).[17] X_{it} are time varying worker characteristics, such as age and its square, tenure, whether the worker receives care allowance, whether the worker receives disability payment. V_{jt} are the time varying firm level controls; s_{jt} are sector-year interactions, v_i and f_j are person and firm fixed effects.

As the aim of the analysis is to measure the change in the return to different skills, I interact the skill measures with the ownership dummies. The focus of the study is the γ_3 parameter that captures the average effect of foreign acquisition on the return to skills in all post-acquisition years. As $PostAcq$ changes from 0 to 1 within the firm, this interaction term varies within the firm and within the worker spell: it turns from zero to the value of skill requirement index of the occupation the worker is employed. In the case when worker and firm fixed effects are included, this parameter is identified from (i) workers who stayed with the firm after the acquisition and did not change occupation;

[17] education is proxied by a categorical variable based on the highest occupational status achieved in 2003-2011 (same measure used by Balazsi et al., 2018), as the educational attainment is not observed

(ii) workers who stayed with the firm after the acquisition and changed occupation; (iii) workers who arrived to the firm after the acquisition. 80% of the workers who stayed with the firm around the acquisition did not change occupation within the worker-firm spell, while a substantial part of workers arrived after the takeover (see Table A-3 for the number of sector, firm and occupation switches).

First, I estimate the model by simple OLS (v_i and f_j are excluded). As a second step, I introduce f_j firm fixed effects and exclude all time-invariant firm level controls (such as the dummy variable for always foreigners and for acquired firms). Then I include person and firm fixed effects to the model (v_i and f_j) and exclude time invariant worker and firm level controls. Worker fixed effects eliminate the potential endogeneity concern that workers with higher unobserved ability select to more skill-intensive occupations and they also receive higher wages regardless of the skill requirement. The standard errors are clustered on the firm level.

As a second approach, I perform an event study style analysis. I include leads and lags of the acquisition interacted with the skill indices. The reason for this is to examine how the effect of foreign acquisition evolves over time.

$$
\begin{gathered}
lnw_{ijot} = \delta_3 PostAcq_{jt-1} + \sum (\beta_t SkillMeasure_o * Year_t) + \\
\gamma_2 BalAcquierd_j * SkillMeasure_o + \left[\sum \gamma_{3,-s} Acquisition_{-s} + \sum \gamma_{3,+s} Acquisition_{+s}\right] * SkillMeasure_o + \\
\gamma_2 UnBalAcquired_j * SkillMeasure_o + \delta_2 UnBalPostAcq_{jt-1} * SkillMeasure_o + \\
\mu Math_o + \beta X_{it} + v_i + f_j + s_{jt} + \varepsilon_{ijt} \quad (2)
\end{gathered}
$$

The dependent variable is the logarithm of the daily wage of person i having an occupation o at firm j in year t.

I define two types of acquired firms: balanced and unbalanced. An acquired firm is considered to be balanced if the firm was observed in my data set from two years prior to two years after the acquisition (altogether for five years). By definition, a firm can only be balanced if it was acquired between 2005 and 2008 and have five consecutive years.[18] Firms acquired in 2004, or between 2009 and 2011 are considered to be unbalanced.[19] Balanced acquired firms correspond to 50 percent of my acquired worker-year observation (see Table A-3 last two rows).

I interact my skill measure with the dummies indicating whether the acquired firm is considered to be balanced or unbalanced. For balanced firms, I include leads and lags of the acquisition interacted with the skill measures. I leave out the interaction term with the year of the acquisition $\gamma_{3,0}$. Now I have more than one γ_3 parameters. The $\gamma_{3,-s}$ parameters show whether there are observable pre-trends. If there are not, these coefficients should be zero. The $\gamma_{3,+s}$ parameters show the effect of an acquisition on the return to skills in post-acquisition years. I expect these parameters to be different from zero, if foreign acquisition has an impact on the returns to skill.

I do not exclude the unbalanced firms from the sample due to the worker fixed effects estimates. However, I interact the dummy indicating that the acquired firm is unbalanced with the skill measure.

All else remain the same as previously. I include worker and firm fixed effect in the model together with sector-year interactions. I further control for time varying worker characteristics: age and its square, tenure, whether the worker receives care allowance, whether the worker receives disability payment.

4 Results

Table 5 shows the estimated results of equation 1. The first column is the OLS regression, I add firm fixed effects in the second column and worker and firm fixed effects in the third column. In general, IPS skills are rewarded at all types of firms. One standard deviation increase in IPS skills increases the wage by 2.66 percent at a domestic firm (see Table 5 first column), the effect is even larger at always foreign firms. The return

[18] firms that were acquired within this time window but do not have five consecutive years are mainly small firms whose firm id disappear from the data set.

[19] even though my sample ends in 2010, the entire data set contains information up to 2011, thus I know if a firm was acquired in 2011 although I do not have post-acquisition observation for it.

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Table 5: Return to Independent problem solving (IPS) skill

	OLS	Firm FE	2WayFE
IPS	0.113***	0.0727***	0.0266***
	(0.00477)	(0.00294)	(0.00157)
IPS * Always for.	0.176***	0.126***	0.0457***
	(0.00904)	(0.00541)	(0.00382)
IPS * Acquired	0.0354**	0.0390***	0.00240
	(0.0170)	(0.0117)	(0.00531)
IPS * PostAcq.	0.0469***	0.0367***	0.0403***
	(0.0139)	(0.00873)	(0.00736)
Rsquare	0.381	0.248	0.866
Worker FE			yes
Firm FE		yes	yes
Sector*year	yes	yes	yes
N	5,356,887	5,356,887	5,356,887

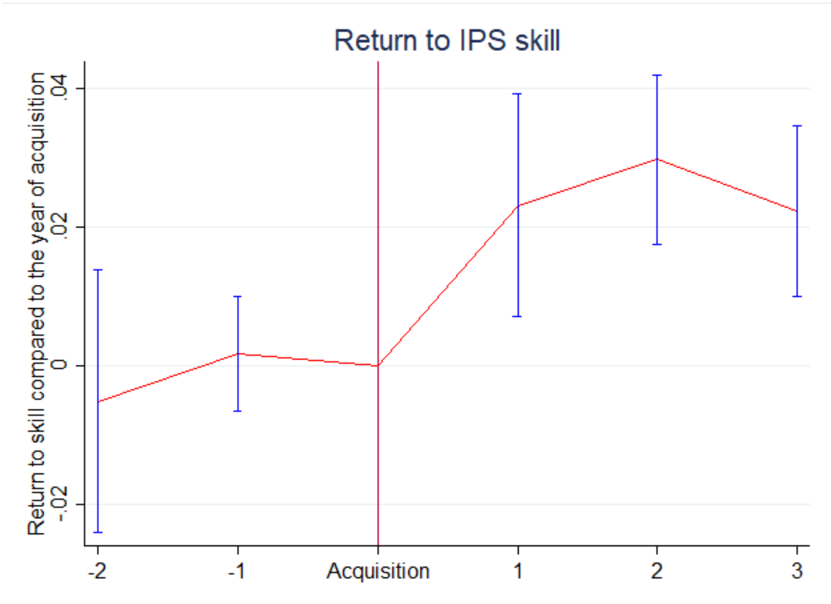
Standard errors are clustered at firm level, *** p<0.01, ** p<0.05, * p<0.1. No. Observ.: 5,356,887, Number of indiv.: 940,872, Number of firms: 156,906. Controls: age and its square,gender, education, whether the worker receive care allowance, whether the worker recieve disability payment, tenure and whether the observation is censored, mathematics skill requirement, ownership dummies (always foreigner, acquired and post acquiaition period). Time-invariant firm level controls (dummy variable for always foreigner and the dummy variable for acquired firms) are excluded as firm FE is added to the regression. Time-invariant worker level controls (gender and education) are excluded from two-way fixed model.

to IPS skill is comparable in size with the return to analytical skills in the literature, Abraham and Spletzer [2009] shows that 1 standard deviation increase in analytical skills increases the wage by almost 3 percent for all firms (they do not differentiate by firm characteristics).

As soon as worker fixed effects are added to the regression, acquired and domestic firms do not differ before the acquisition (see Table 5 column 3, the interaction term of IPS skill and the Acquired dummy is insignificant), but there is a significant increase in the return after the acquisition took place, the return almost reaches the level at always foreign firms. Worker fixed effects control for the mechanism that better ability workers select to skill-intensive occupations and receive higher wages at the same time.

Figure 1 shows the estimated return to IPS skills (by using the two-way fixed effect model) at an acquired firm around the foreign takeover compared to the return to skill at the year of the acquisition. The horizontal axis shows the event time relative to the event of the acquisition, the red vertical line represents the takeover. The graph shows that there is no pre-trend in the return to IPS skill before the acquisition takes place,

Figure 1: Return to IPS skill around the acquisition



Standard errors are clustered at firm level. 90% confidence intervals are presented. Number of observations: 5,356,887, Number of individuals: 940,872, Number of firms: 156,906. Controls: age and its square, whether the worker receive care allowance, whether the worker recieve disability payment, tenure and whether the observation is censored, mathematics skill requirement, IPS skill index interacted with year dummies, IPS skill index interacted with always foreigner dummy and with acquired dummy. Firm and worker fixed effects are included.

while there is a significant increase after the acquisition. The point estimate on the $\gamma_2 BalAcquierd_j * SkillMeasure_o$ is very small and insignificant (see Table A-4), this means that there is no difference between acquired and domestic firm in the year of the acquisition.

As a next step, I keep in my sample only the balanced acquired firms and re-estimate equation 2 on this restricted sample. In this analysis I exclude the interaction term of the year dummies with skill measure from the regression, thus I do not control for a general trend in the return to IPS skill. Figure A-1 shows the results. The results are robust to these changes. There is no pre-trend in the return to IPS skill before the acquisition, while there is a significant jump after the take-over. The estimated coefficients are larger and the confidence intervals are smaller than previously, but this can be due to the fact that in the previous analysis I control for a general trend in the return to IPS skill.

The positive effect of a foreign acquisition on the return to IPS skill means that the wages of those having more IPS skill intensive occupations increase more than of those having less IPS skill intensive occupations. To demonstrate this, I keep in my sample only

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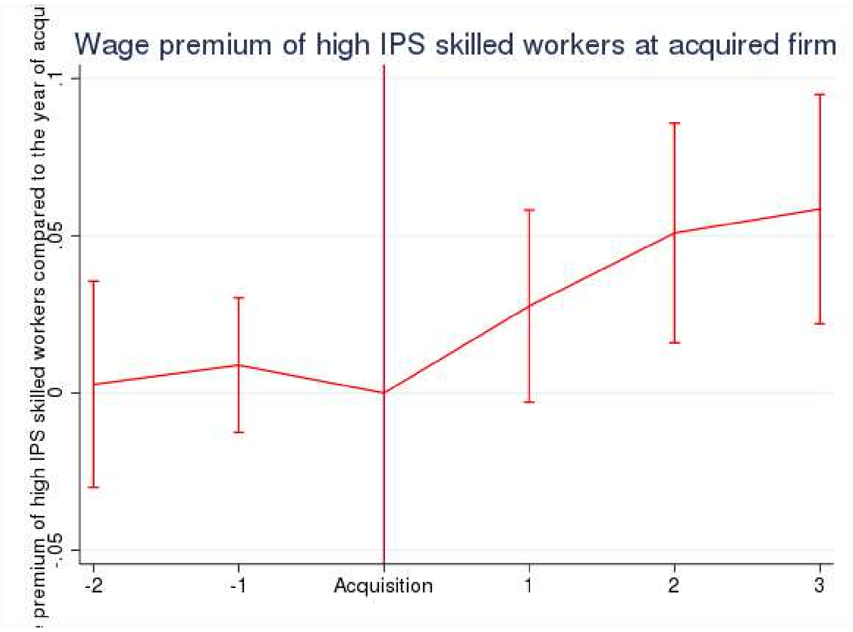
Figure 2: Wage premium of high IPS skilled workers around the acquisition



Standard errors are clustered at firm level. 95% confidence intervals are presented. Number of observations 96651 Controls: age and its square, whether the worker receive care allowance, whether the worker recieve disability payment, tenure and whether the observation is censored, mathematics skill requirement, dummy variable indicating that the occupation require above the median level of IPS skill and sector-year interactions. Firm and worker fixed effects are included.

balanced acquired firms[20] and define an occupation to be IPS skill intensive occupation if the IPS skill requirement of the occupation is above the median. I run a regression where the dependent variable is the logarithm of the daily wage (same as previously) and I estimate the evolution of the wage premia of having an IPS skill-intensive job. The control variables are the same as previously, I include firm and worker fixed effects in the regression and the standard errors are clustered on the firm level. Figure 2 shows the wage difference between those having IPS skill-intensive occupations and those having low IPS skill requirements around the acquisition (see the results in Table A-5). In line with the previous findings, we see that the difference between low IPS skilled workers and high IPS skilled workers increases after the foreign takeover.

Heterogenity of the change in return to IPS skill: I also investigate whether the estimated increase in the return to the IPS skill is relevant only for some special groups or it is a general effect. I focus on gender, education, firm size and industry.

[20]same definition is used as previously: firmst that are in my sample from two years before to two years after the acquisition

Table A-6 Panel A shows the gender gap in the increase in the return to IPS skill. In this regression, I interact the ownership status and the interaction terms of ownership status and IPS skill index with a dummy indicating if the worker is male in equation 1. Both female and male workers with a high level of IPS skills gain from a foreign takeover.

Table A-6 Panel B demonstrates the increase in the return to IPS skill by educational categories. As education is not directly observed in the data set, I use a categorical variable that is based on the highest occupational status achieved in 2003-2011 as a proxy. I introduce a dummy indicating whether the worker has a low level of education and interact this dummy with the ownership status of the firm and with the interaction term of ownership status and IPS skill index in equation 1. The estimated results are not driven by subgroup of workers based on education either (see Table A-6 Panel B).

Heterogeneity can appear at the firm level as well, therefore as a second step, I estimate the effect of foreign acquisition on the return to skill by firm types.

I do not find high heterogeneity across firm size categories either. Table A-6 Panel C shows that the increase in the return to IPS skill after a foreign takeover is very similar at small and large firms.[21]

Most of the papers in the literature focusing on foreign firms restrict their sample to manufacturing firms solely (Sjöholm and Lipsey, 2006Conyon et al., 2002, Guadalupe et al., 2012), but as Balazsi et al. [2018] argued in case of Hungary almost three-fourth of the cumulative FDI inflows have arrived to sectors outside the manufacturing and they find comparable wage effect of acquisition in the manufacturing and the service sectors as well. Because of this finding, I did not restrict my sample based on industry affiliation in the main specification, but in this section, I investigate whether there are any sectoral differences. According to Table A-6 Panel D, the estimated increase in return in manufacturing is somewhat larger, but it is relevant and significant in the case of the service sector as well.

Newcomer versus Stayers: I investigate the question whether my results are driven by those workers who newly arrived at the firm or the increased return can be observed

[21] small firms are firms under 20 employees, while large firms are firms above 20 employees.

at those who stayed with the firm around the acquisition as well. I define newcomers as those who arrived at the firm after the foreign takeover. I augmente equation 1 by the interaction term of post-acquisition dummy and the skill measure with the newcomer dummy: $\gamma_{32} PostAcq_{jt} * SkillMeasure_o * Newcomer_{it}$, and by controlling for the pattern that in general newcomers can have a different salary impact than other workers (interaction term of $PostAcq_{jt} * Newcomer_{it}$), all else remain the same. Now the γ_3 parameters in equation 1 captures the average effect of foreign acquisition on the return to skills in all post-acquisition years for stayers. Table A-3 shows the number of those who never changed occupation and those who changed their occupation separately by stayers and newcomers.

Table A-7 Panel A shows the results. First column corresponds to OLS estimate, while firm fixed effect is added in the second column and worker and firm fixed effects are included in the third column. The results suggest that the estimated increase in the return to IPS skill is not driven by workers who arrive to the firm after the takeover (see Table A-7 Panel A first row).

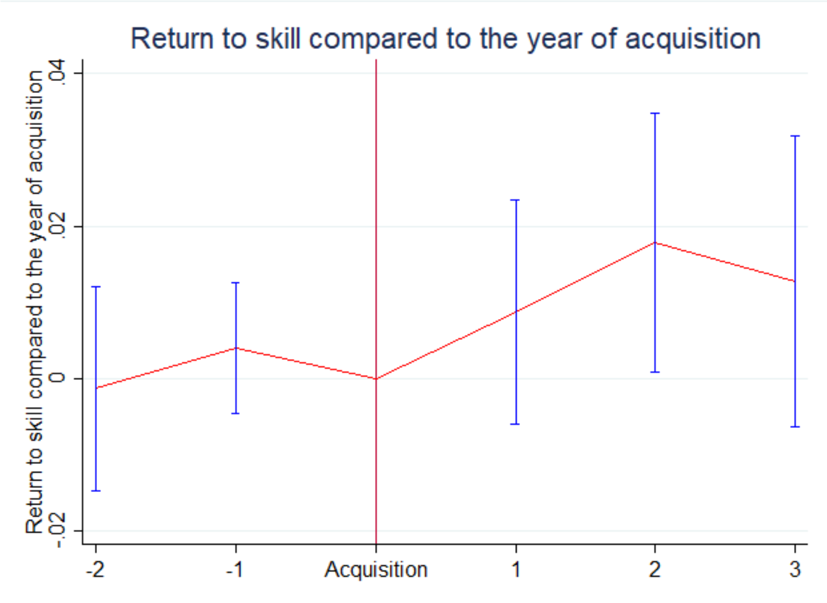
4.1 The role of other skills after the takeover

Now I investigate the role of other skill sets play after a foreign takeover. First I re-estimate equation 1 and 2 separately by skill requirement indices, namely interpersonal skill, and RTI, second, I include the three skill measure together in the same regression.

As a first step, I have estimated equation 1 separately for interpersonal skill and RTI (see Table A-8 for interpersonal skill and Table A-9 for RTI). Both have a parameter with the expected sign: jobs that require better interpersonal skills and a higher level of non-routines pay higher wages at all types of firms. Foreign firms reward interpersonal skill more than domestic, but they also punish routine tasks more. The change in the return to both of the skills is significant: positive for interpersonal skills and negative for RTI.[22]

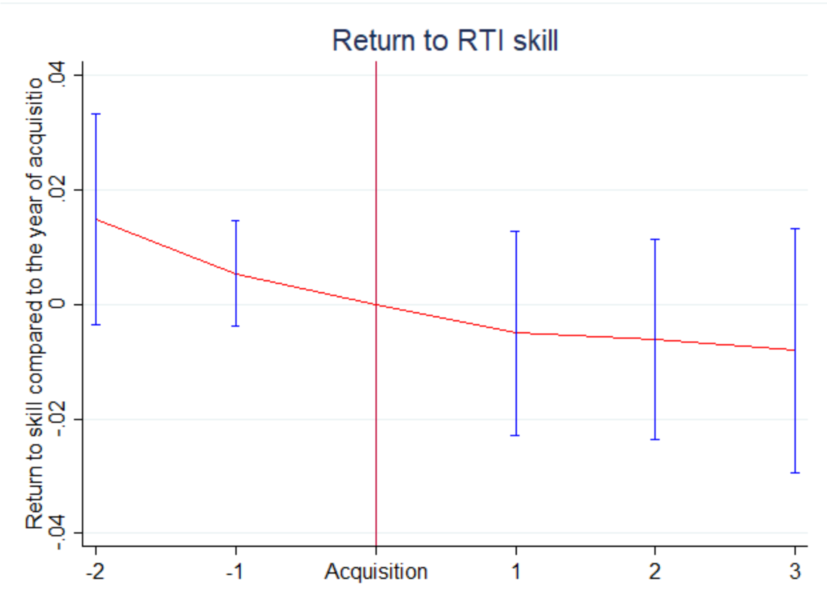
[22] I also estimated the parameters separately for newcomers and stayers. Table A-7 Panel B and Panel C show the results respectively.The results are not driven by workers who newly arrived to the firm (see Table A-7 first row of Panel B and Panel C respectively)

Figure 3: Return to Interpersonal skill around the acquistion



Standard errors are clustered at firm level. 90% confidence intervals are presented. Number of observations: 5,356,887, Number of individuals: 940,872, Number of firms: 156,906. Controls: age and its square, whether the worker receive care allowance, whether the worker recieve disability payment, tenure and whether the observation is censored, mathematics skill requirement, interpersonal skill index interacted with year dummies, interpersonal skill index interacted with always foreigner dummy and with acquired dummy. Firm and worker fixed effects are included.

Figure 4: Return to RTI around the acquisition



Standard errors are clustered at firm level, 90% confidence intervals are presented. Number of observations: 5,356,887, Number of individuals: 940,872, Number of firms: 156,906. Controls: age and its square, whether the worker receive care allowance, whether the worker recieve disability payment, tenure and whether the observation is censored, mathematics skill requirement. RTI skill index interacted with year dummies, RTI skill index interacted with always foreigner dummy and with acquired dummy. Firm and worker fixed effects are included.

Figure 3 and Figure 4 show the estimated return to interpersonal skill and RTI around the acquisition (by using two-way fixed effect model) compared to the return to skill at the year of the acquisition (see Table A-4 column 2 and 3 for the estimates). The horizontal axis is the event time relative to the acquisition on both of the graphs, the red vertical line corresponds to the event of the acquisition. According to Figure 3, there is no pre-trend in the return to interpersonal skill before the acquisition takes place, while there is an increase after the acquisition. The point estimate for the post-acquisition period is significant only in (t+2) and the magnitude is smaller than in the case of IPS skill. In the case of the RTI index, the decrease in the return starts already before the acquisition take place but the difference is insignificant for all period (see Figure 4). The Figure 4 suggests that in the case of RTI there is no effect of change in the ownership status of the firm on the return to RTI.

The results are robust to restricting the sample to the balanced acquired firm, see Figure A-2 for interpersonal skills, and Figure A-3 for RTI. There is no pre-trend in the return to interpersonal skills before the acquisition takes place, while there is a significant increase thereafter. The estimated coefficients are larger in magnitude compared to Figure 3 and all post-acquisition coefficients are significant, but in the first scenario I control for a general trend in the return to interpersonal skill, thus those coefficients can be interpreted as an effect on the top of the trend. In the case of the RTI, Figure A-3 suggests a small decreasing (marginally significant) effect, but both the results on Figure A-3 and also the fact that after controlling for the general trend in the return to RTI this small effect disappears, suggesting that this effect is not a result of a change in the ownership status of the firm.

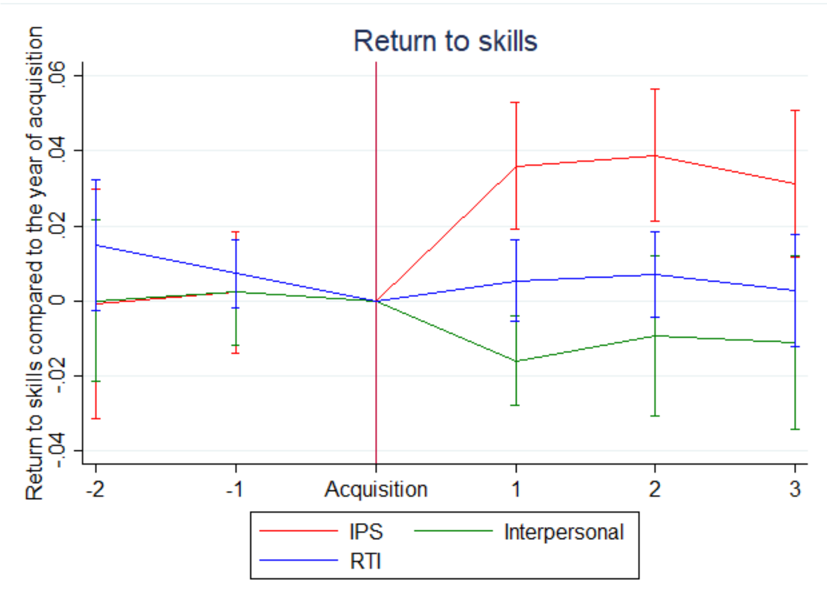
As a second step, I include the three skill measures together: IPS skill, interpersonal skills, and RTI. The results can be found in Table 6. I interacted all skill measures with the same ownership dummies as previously (see equation 1), but only the interaction term with the $PostAcq$ dummy is reported in Table 6. The estimated increase in the return to IPS skill after a takeover is basically unaffected by the inclusion of the other two skill measures. The coefficient on IPS skill remains significant, if anything than the magnitude

Table 6: Change in the return to skills after a foreign takeover - all three indices in one regression

	OLS	firm FE	2WayFE
IPS *PostAcq.	0.0639***	0.0381***	0.0470***
	(0.0187)	(0.0111)	(0.00956)
Interpersonal * PostAcq.	-0.0376**	-0.00400	-0.00980
	(0.0187)	(0.00786)	(0.00685)
RTI*PostAcq.	-0.0272*	-0.00259	-0.000343
	(0.0160)	(0.00679)	(0.00453)
Rsquare	0.383	0.253	0.866
N	5,356,887	5,356,887	5,356,887
Worker FE			yes
Firm FE		yes	yes
Sector * Year	yes	yes	yes

Standard errors are clustered at firm level, *** p<0.01, ** p<0.05, * p<0.1. Number ofobservations: 5,356,887, Number of individuals: 940,872, Number of firms: 156,906. Controls: age and its square,gender, education, whether the person recieved disability allowance and whether the person recieved care allowance, tenure and whether the observation is censored, mathematics skill requirement, ownership dummies (always foreigner, acquired and post acquiaition period), skill measures and their interaction with the ownership dummies. Time-invariant firm level controls (dummy variable for always foreigner and the dummy variable for acquired firms) are excluded as firm FE is added to the regression. Time-invariant worker level controls (gender and education) are excluded from two-way fixed model.

Figure 5: Return to skills around the acquistion - all three skill indices in one regression



Standard errors are clustered at firm level. 90% confidence intervals are presented. Number of observations: 5,356,887, Number of individuals: 940,872, Number of firms: 156,906. Controls: age and its square, whether the worker receive care allowance, whether the worker recieve disability payment, tenure and whether the observation is censored, mathematics skill requirement, skill measures and their interaction with year dummies and with the ownership dummies. Firm and worker fixed effects are included.

increased slightly compared to Table 5. Contrarily, the coefficient on interpersonal skill and RTI became insignificant and for both of the skills the magnitude became almost zero.

Figure 5 supports these results (see Table A-10 for the estimates), it shows the estimated parameters of equation 2 by using two-way fixed effect model. The horizontal axis shows the event time relative to the acquisition, the vertical brown line indicates the event of the acquisition. The results for the return to IPS skills are robust to the inclusion of the other two skill measures: the return does not differ significantly for the pre-acquisition period from the return at the time of the event, while there is a large and significant jump after the event of the takeover. The return to interpersonal skill is insignificant before the acquisition, after the acquisition the point estimates turn to negative, but the change in the return is significant only in (t+1). The change in the sign can be due to the correlation between interpersonal skill and IPS. The return to RTI is insignificant in all periods. To check the robustness of the results, I re-run the event study regression on the balanced acquired firm subsample, see Figure A-4. The results are very similar to the total-sample results.

These results suggest that a foreign takeover primarily changes the return to IPS skills and the effect on the other skills are only secondary.[23]

4.2 Divestment: Foreign-domestic takeover

Although the question about the effect of foreign-domestic takeovers is relevant, the literature is basically silent about it. While Earle et al. [2018] found a negative wage effect of divestment, Sjöholm and Lipsey [2006] did not find a clear pattern: the sign of the point estimate changed between the specifications, but in each case, they were very small. My data set does not allow me to dig very deeply into this question due to the shortness of the time window, but my results suggest that the return does not change significantly after a withdrawal.

I distinguish two types of foreign firms in the data: firms that entered the data set

[23] The results are robust to calculating the weights by the inverse covariance matrix suggested by Anderson [2008], or by using the unweighted average of the variables, see Table A-11.

Table 7: Foreign investment withdrawal and the return to IPS skills

	OLS	Firm FE	2Way FE
IPS	0.113***	0.0727***	0.0266***
	(0.00475)	(0.00293)	(0.00155)
IPS * always for.	0.180***	0.127***	0.0460***
	(0.00918)	(0.00541)	(0.00376)
IPS * acquired	0.0355**	0.0390***	0.00248
	(0.0170)	(0.0117)	(0.00531)
IPS * PostAcq.	0.0536***	0.0381***	0.0407***
	(0.0139)	(0.00884)	(0.00738)
IPS * divest.	-0.0860***	-0.0178	-0.00546
	(0.0177)	(0.0206)	(0.00778)
Rsquare	0.381	0.248	0.866
N	5,356,887	5,356,887	5,356,887
Worker FE			yes
Firm FE		yes	yes
Sector * Year	yes	yes	yes

Standard errors are clustered at firm level, *** p<0.01, ** p<0.05, * p<0.1. Number of observations: 5,356,887, Number of individuals: 940,872, Number of firms: 156,906. Controls: age and its square,gender, education, whether the person recieved disability allowance, whether the person recieved care allowance, tenure and whether the observation is censored, mathematics skill requirement, ownership dummies (always foreigner, acquired and post acquiaition period, divestment period). Time-invariant firm level controls (dummy variable for always foreigner and the dummy variable for acquired firms) are excluded as firm FE is added to the regression. Time-invariant worker level controls (gender and education) are excluded from two-way fixed model.

under foreign ownership[24] and acquired firms (entered my data as a domestic firm but were acquired during my sample). Domestic takeover can happen at any of these firms, thus I pool them together when defining divestment. The previous equation 1 is now augmented with an ownership dummy that equals 1 if the firm was previously under foreign ownership but now it is under domestic ownership. I also interact it with skill measures.

Table 7 shows the estimated results, the control variables are the same as previously: the first column corresponds to the OLS estimates, I add firm fixed effects in the second column and firm and worker fixed effects in the third column. The magnitude and significance of the rise of return to IPS skills are basically unchanged. The withdrawal of foreign investment does not decrease the gain that happened after the foreign takeover.

[24] Firms entering as foreign firm two the dataset can be of two types: greenfields or acquired firms. If the acquisition took place before 2003, I can not differentiate between these two types

4.3 Change in the Wage Premium of White-collar Workers

In this part, I investigate whether this change in the return to skills can explain the skill-biased wage effect of a foreign takeover.

First, I estimate the effect of an acquisition on the blue-collar white-collar wage gap, I re-estimate equation (1) by excluding the skill measures, and by including a dummy showing whether the current occupation of the worker is considered to be a white-collar job and by interacting this dummy with the ownership dummies. As a second step, I include the IPS skill index (and the interaction terms of the skills measures and the ownership dummies) in the regression.

Table 8 shows the estimated results. In column 1-3 skill indices are excluded, while in column 4-6 I control for the change in the return to IPS skill. A foreign acquisition increases the wage of white-collar workers by 8%, while the wage of blue-collar workers is unaffected. The wage gain of white-collar workers after a foreign acquisition decreases to 2-3% (and became insignificant) after controlling for IPS skill.

4.4 Composition

In this section, I turn my focus on the composition of the workforce and I move from worker level to firm level data. I exclude from my analysis firms that never exceed the 20 employees threshold.[25] I calculate different measures of skill composition: (1) I calculate the yearly average IPS skill level at firm; (2) share of workers having jobs that require more than average level of IPS skills; (3) whose job has skill requirement measure that is in the top 50%; (4) whose job has a skill requirement measure that is in the top 25%.

$$Y_{jt} = \beta PostAcq_{jt} + \gamma Divestment_{jt} + s_{jt} + f_j + \varepsilon_{jt} \quad (3)$$

where Y_{jt} is the measure of skill composition for firm j in year t; $PostAcq_{jt}$ is a dummy

[25] at very small firms a single change in the workforce (hiring, firing or occupation change for incumbent worker) can lead to an extreme jump in the average.

Table 8: Wage premium of white-collar workers

	OLS	Firm FE	2way FE	OLS	Firm FE	2way FE
PostAcq	0.167***	-0.00136	0.00310	0.182***	0.00530	0.0106
	(0.0504)	(0.0143)	(0.0155)	(0.0473)	(0.0114)	(0.0139)
PostAcq * white-collar	0.0557	0.0745***	0.0796***	-0.0375	0.0293	0.0232
	(0.0354)	(0.0257)	(0.0159)	(0.0521)	(0.0323)	(0.0202)
IPS				0.118***	0.0667***	0.0198***
				(0.00404)	(0.00178)	(0.00117)
IPS * AlwaysFor				0.0799***	0.0442***	0.0259***
				(0.0123)	(0.00721)	(0.00345)
IPS * Acquired				-0.0522	-0.0158	-0.00595
				(0.0335)	(0.0116)	(0.00618)
IPS * PostAcq				0.0523**	0.0259**	0.0341***
				(0.0230)	(0.0125)	(0.0113)
Rsquare	0.372	0.260	0.866	0.396	0.272	0.866
N	5,356,887	5,356,887	5,356,887	5,356,887	5,356,887	5,356,887
Worker FE			yes			yes
Firm FE		yes	yes		yes	yes
Sector * Year	yes	yes	yes	yes	yes	yes

Standard errors are clustered at firm level, *** p<0.01, ** p<0.05, * p<0.1. Number of observations: 5,356,887, Number of individuals: 940,872, Number of firms: 156,906. Controls: age and its square,gender, education, whether the person recieved disability allowance, whether the person recieved care allowance, tenure and whether the observation is censored, ownership dummies (always foreigner, acquired and post acquiaition period). Time-invariant firm level controls (dummy variable for always foreigner and the dummy variable for acquired firms) are excluded as firm FE is added to the regression. Time-invariant worker level controls (gender and education) are excluded from two-way fixed model.

Table 9: Composition effect - change in the share of high IPS skilled workers

	Average	Above the Mean	Top 50%	Top 25%
		Panel A - IPS		
PostAcq.	0.0159	0.0135*	0.0126*	0.00240
	(0.0159)	(0.00788)	(0.00756)	(0.00852)
Rsquare	0.002	0.002	0.002	0.001
N	156,898	156,898	156,898	156,898
		Panel A - Interpersonal skill		
PostAcq.	0.000655	-0.000305	-0.00132	0.00710
	(0.0152)	(0.00746)	(0.00699)	(0.00764)
Rsquare	0.002	0.002	0.002	0.001
N	156,898	156,898	156,898	156,898
		Panel B - RTI		
PostAcq.	-0.00521	-0.00104	0.00221	-0.00211
	(0.0189)	(0.00795)	(0.00778)	(0.00628)
Rsquare	0.002	0.002	0.003	0.002
N	156,898	156,898	156,898	156,898
firm FE	yes	yes	yes	yes

Standard errors are clustered at firm level, *** p<0.01, ** p<0.05, * p<0.1. Number of observations: 156,898, Number of firms: 27,447. Controls: sector-year interaction and firm fixed effects.

showing that the takeover took place at least one year ago; $Divestment_{jt}$ equals to 1 in the after-withdrawal period (e.g. the firm was once under foreign control but now it is a domestic firm); s_{jt} are the sector-year interactions; f_j are the firm fixed effects. Standard errors are clustered at the firm level.

Table 9 shows the effect of a foreign takeover on the worker composition: Panel A corresponds to IPS skill, Panel B shows the results for interpersonal skill and Panel C represents the results for RTI. Each column shows the result for a specific measure: (1) average level of the given skill; (2) share of workers above the average; (3) share of workers in the top 50%; (4) share of workers in the top 25%.

According to all measures the share of workers having higher IPS skills increases after the foreign takeover, but the effects are very small and the results are significant only for the (2) and (3) measures. The share of workers with high interpersonal skills or with jobs that involve more routine tasks does not change after a foreign takeover. In Table A-12, I include firm size as time-varying firm level control because foreign firms tend to grow and as a consequence hire more lower-skilled workers, this mechanism would decrease my skill composition measures for reasons that I am not interested in. The estimated parameters are larger, and the estimates for the average level of IPS skill became significant, while the effects remain insignificant for the other two skill measures.

Figure A-5 shows the estimated effect of a foreign takeover on the workforce composition around the acquisition: the figures on the top show the share of workers with IPS skill above the mean, while the figures on the bottom show the share of workers with IPS skill in the top 50%. The figures on the right show the estimates in the case of the full sample, while on the right figures, I restricted my sample to the acquired firms (firms that switch from domestic to foreign firm in my sample). Instead of including a simple post-acquisition dummy, I include leads and lags around the acquisition to explore the dynamic of the change in the workforce composition. All of the figures suggest a small, insignificant and positive effect.

5 Discussion of the results

In this section, I investigate the possible mechanisms which can explain my empirical results. I argue that my results are in line with the hypothesis that decentralization takes place at the firm after the acquisition. I also rule out some alternative hypothesis.

5.1 Success of foreign firms

To see how foreign take-over affects the success of the firm, I use the same firm level data as in section 4.4. I re-run the regression 3 on the logarithm of sale, size and sales revenue per worker, while I also use the net profit before taxation.[26][27] I include a dummy showing that the take-over took place at least one year ago, I also control for the after-withdrawal period, sector-year interactions, and firm fixed effects. Standard errors are clustered at the firm level.

Table 10 shows the results. Firms increase after a foreign take-over in terms of sales and size as well, while I do not find evidence for productivity improvement (measured by

[26] The information on size was estimated from the worker level data, thus they reflect the firm size in March for each year.

[27] as net profit can take negative value, I do not take the logarithm

Table 10: Foreign acquisition and the performace of the firm

	log(sales)	log(size)	log(sales per worker)	net profit before taxation (1000HUF)	worker/manager ratio
PostAcq	0.118***	0.133***	-0.00689	53,055	1.298**
	(0.0372)	(0.0334)	(0.0275)	(56,420)	(0.600)
N	152,165	156,898	151,892	156,618	76,293
Rsqaure	0.043	0.031	0.046	0.002	0.004
Firm FE	yes	yes	yes	yes	yes

Standard errors are clustered at firm level, *** p<0.01, ** p<0.05, * p<0.1 Controls: sector-year interaction. Number of firms: 1st column: 26,400; 2nd column: 27,447, 3rd column: 26,359; 4th column: 27,436, 5th column: 16,177

sales revenue per worker), and for an increase in the net profit. This could be the result of the short time window that I have in the sample, the transformation of a company can be very costly at the beginning, while the benefits can only be realized later. Figure A-6 shows the results in the case of the event-study approach, where I include leads and lags around the acquisition instead of a single post-acquisition dummy. The figures on the left show the results in the case of the full sample, while the figures on the right show the results on the sample of the acquired firms only. The results confirm the finding that firms increase after the acquisition, although there is a slight upward trend in the firm size even before the take-over.

Several reasons can be behind the success of foreign firms. Foreign investors may have the know-how of new managerial practices, they might be aware of new and more efficient organizational structure, they can also have access to superior technologies [Girma and Görg, 2007]. It also could be that neither of these is happening, foreign firms simply cherry pick the best firms on the market that would be successful even without the presence of foreign investors. Earle et al. [2018] show in the case of Hungary that the wage effect of acquisition is larger for more developed sending countries. This finding supports the hypothesis that investors from more developed countries bring more advanced technologies and organizational capital to Hungary.

5.2 Possible explanations

Decentralization of the firm and foreign ownership In the last several decades a change in the organization structure was documented in the literature. Recent analysis shows that the organizational change and technological change have their own channels in effecting the labor demand of skilled workers at the firm (Bresnahan et al., 2002, Greenan, 2003, Piva et al., 2005, Campaner et al., 2018, Lindner et al., 2018). Although the new organizational practices are very diverse, there are some common features: (i) decentralization of authority, (ii) decision-making being transferred downstream, (iii) greater responsibility at the lower level.[28] Such delegation requires information processing and problem solving skills [Campaner et al., 2018] together with increased responsibility and larger adaptability to new tasks and environments (Piva et al., 2005). Skilled workers are also considered being more autonomous which is a valuable skill in a decentralized firm (Caroli and Van Reenen, 2001). These are the skill sets that are summarized in my independent problem solving skill index.

Although I do not have direct evidence, I argue that my finding that the return to IPS skills increases after a foreign take-over can be interpreted as a sign of decentralization after a takeover, as the IPS skill index in the analysis summarizes the skill sets that are necessary for a decentralized firm. There is no significant difference between acquired and always domestic firm in the return to IPS skill before the acquisition but there is a large significant and permanent jump after the acquisition (see Figure 1).

My argument is supported by the finding that worker/manager ratio increases after a foreign take-over. I re-run the regression 3 with worker/manager ratio as dependent variable[29], I only control for sector-year interactions and firm fixed effects. The sample is the same as in section 4.4.[30] Standard errors are clustered at the firm level. The last

[28] see more about the characteristics of the new organization structure in Caroli and Van Reenen, 2001; Piva et al., 2005; Campaner et al., 2018

[29] Managers are defined as "General managers of business organisations and budgetary institutions" (FEOR 131); Department managers of business organisations and budgetary institutions (FEOR 132-133); Managers of functional units in business organisations and budgetary institutions (FEOR 134); General managers of small business undertakings (directors, chairpersons, managing directors, managers) (FEOR 141-142). All others are defined as non-manager.

[30] the lower number of observation is a result of the construction of data set, as 50% of the Hungarian population is observed, there is high chance that I do not see the manager of the firm, thus the ratio is

column of Table 10 shows the results. The ratio of worker/managers increases after a foreign takeover which is in line with the decentralization hypothesis. This finding is supported by the event study approach (see Figure A-7).

Decentralization can happen at a foreign firm for several reasons. Bloom and Van Reenen [2010] argues that even though implementing new management practices can lead to productivity improvements, firms might not implement them due to lack of knowledge or awareness about the new practices. Foreign firms might have the advantage by having the experience with better managerial practices in their home countries (Caroli and Van Reenen, 2001, Campaner et al., 2018), this could decrease the cost of implementing the new practices. In an extreme scenario, domestic firms might not even know about the new organizational form. But it is not only the novelty that might matter: having greater market access arising from the takeover could increase the benefit of the investment.

It also could be that decentralization does not improve the performance of the firm, and foreign firms appreciate independent problem solving skills only due to the fact that it is more difficult for them to supervise its Hungarian subsidiary due to the distance. If this would be the case then under the assumption that there is no friction in downward wage adjustment, we would expect a decrease in the return to IPS skill after a foreign investment withdrawal. In Table 7, I show that reverse ownership change does not decrease the return.

Diffusion of computers: I argue that my results do not only mimic the diffusion of computers that evolve together with organizational change. The recent diffusion of technologies are biased toward skilled workers.[31] The spread of new technologies, especially computer technology leads to a change in labor demand. Computers are able to substitute humans for tasks that follow logical rules that are programmable (David and Dorn, 2013; Autor et al., 2003). Routine tasks-intensity, used in my analysis, is widely used in the literature to measure how easily the job is substituted by computers (Goos et al., 2014). If my results would raise simply from the fact that computers complement skill-intensive

missing

[31]see David and Dorn [2013]; Autor et al. [2003]; Koren and Csillag [2011]; Goos et al. [2014]; Peng et al. [2018]

jobs and substitute routine tasks, a decrease in the reward for routine tasks would be expected. To test this hypothesis I include in my regression the RTI index. I did not find any evidence for a change in the return to RTI after a takeover (see Figure 4 and Figure 5).

Although the diffusion of recent technologies decreases the cost of communication, such cost reduction can also be speeded up by takeover as shown by Bastos et al. [2018]. However, I did not find robust evidence for this. Although the return on interpersonal skill increases after a foreign takeover (see Figure 3), the raise disappears after controlling for IPS skill (Table 5).

Mathematics skill: One could argue that the measured increase in the return of the observed skills are solely the effect of a pattern that better skills are appreciated more after a takeover. In the previous sections, I control for whether the job requires working with numbers and operations, but in this section to rule out the scenario, that I only capture a hypothetical pattern that better skills are appreciated more after a foreign acquisition, I interact this dummy with the ownership dummies. I re-estimate the equation 1 by including the new interaction terms:

$$\omega_1 AlwaysFor_j * Math_o + \omega_2 Acquierd_j * Math_o + \omega_3 PostAcq_{jt-1} * Math_o.$$

Table A-13 shows the estimated results. Panel A is the original result (see Table 5) and Panel B shows the new results by including the new interaction terms.

My main result - a significant increase in the return to IPS skill after a foreign takeover - is robust to the inclusion of the new variables. In the case of the firm fixed effect results (second column) and two-way fixed effect results (third column) the coefficient of my interest - the coefficient of the interaction term of the Post-Acquisition dummy and IPS skill - remains significant and the magnitude is unaffected. On the other hand, there is no change in the return to working with numbers and operations after a foreign takeover controlling for the change in the return to IPS skill.

The same exercises for interpersonal skill and RTI can be found in Table A-14. Here Panel A is the repetition of the original results (A-8 and A-9) and Panel B shows the new estimates by including the new interaction terms. Columns 1-3 represent the results for interpersonal skill, while columns 4-6 are the results for RTI. Although the sign and significance of the change in the return to interpersonal skill after a takeover remain the same, the magnitude drops compared to the original results. The significant change in the return to RTI disappears. The return on mathematic skill in the post-acquisition period is positive and significant.

Occupational structure: Another possible threat to my identification strategy could be that I simply capture the mechanism that manager position demand a higher level of IPS skills and managers gain more from foreign takeover for reasons that are not related to IPS skills. However, I show in section 2.2.1. that the increased return to IPS skill is relevant among low educated workers. In this section, I further investigate this problem.

I restrict my sample to those workers who never became managers in my sample and re-estimate the equation 1 on this restricted sample. Table A-15 shows the results. I estimate the regression separately for the three skill measures, Panel A to Panel C show these results, while the columns correspond to the estimated model: OLS, firm FE and two-way fixed effect.

The point estimates for the change in return to IPS skill and to interpersonal skill after a takeover remain unchanged and significant for non-managers, while the point estimate became almost zero for RTI in the never-manager subsample (see Table A-15 column 3).

Time-varying firm level controls Firms are growing after a foreign take-over (see Table 10) and the value of different skills can change due to this rise. As firms became larger, the cost of monitoring can increase (decrease), thus the value of IPS can increase (decrease). Similarly the cost of communication can be higher (lower) at larger firms, thus the value of interpersonal skills can be different by size of the firm and also the introduction of technologies can be more beneficial in larger firms, thus the return to RTI can be lower at larger firms. To test whether my results are only driven by the fact that firms are growing after a foreign take-over, I augment my equation 1 with time-varying firm level controls: size, revenue, export, and a dummy showing whether the firm has a positive investment in the given year. Table A-16 shows the results. Panels A to C show the results for IPS, interpersonal skills and RTI respectively, while Panel D shows the results in the case when all three skill measures are included in one regression. The results are robust to the inclusion of time-varying firm level controls. All point estimates (and their confidence intervals) are comparable with the original estimates (see Table 5, Table A-8, Table A-9 and Table 6 for the original results respectively).

Based on this analysis I argue that my results are not driven by the mechanism that firms start to grow after a foreign take-over and the evaluation of skills changes due to the growth of the firm.

6 Conclusion

A large body of empirical literature exists about measuring the foreign-domestic wage gap and the wage effect of foreign acquisition. In this paper, I showed how the return to specific skills changes after a foreign takeover.

By using detailed job description together with a panel of administrative data, I document that independent problem solving skills are appreciated more after an acquisition. This result is not driven by some subgroup of workers (e.g. high skilled workers) or by a subgroup of firms (e.g. manufacturing firms), but reflects a general pattern. I also showed that the change in the reward of the skill is permanent, the effect does not vanish after a foreign investment withdrawal. The increase in the return to IPS skill explains the increase in the premium of white-collar worker that raised after a foreign takeover: the estimated 8% white-collar wage premium increase disappears as I control for the change in the return to IPS skill.

I further analyzed the question by examining how the return to interpersonal skill and routine task intensity changes. Interpersonal skill is rewarded more after a foreign takeover but the effect disappears as soon as I control for independent problem solving skill. I did not find a robust change in the return to routine task intensity after a takeover. Together these findings suggest that foreign firms appreciate independent problem solving skills in the first hand, and other skills play only a secondary role if any.

The composition of the workforce after a foreign takeover changes very little, I found

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a small, positive effect on the share of IPS skill-intensive workers, while I did not find any effect on the share of interpersonal skill-intensive workers, and on the share of workers with routine task-intensive jobs.

I argue that my findings are in line with the hypothesis that foreign investors decentralize the firm. I further argue that my results do not simply reflect the diffusion of new technologies that complement high skilled workers and substitute routine tasks, as I did not find any robust evidence for a decrease in the return to routine task intensity that is a widely used measure to examine how computerization affects the labor market.

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Chapter II

Gender Differences in the Skill Content of Jobs

with Balázs Reizer

1 Introduction

The gender gap in labor market outcomes has been decreasing fast since World War II [Olivetti and Petrongolo, 2016]. This positive trend is the result of the decreasing gender segregation across occupations and workplaces. More specifically, the relative position of women in education has increased and, as a consequence, women are now less likely to be segregated into occupations with low wages and low skill requirements [Reskin, 1993, Blau and Kahn, 2000]. Even so, the pay gap has remained considerably large between women and men having very similar labor market characteristics: Cobb-Clark and Tan [2011] show that the current gender wage differences are much larger *within* occupations than *between* occupations.

A small but growing strand of recent literature tries to uncover why women earn less than men in the same occupation. The possible explanations are differences in bargaining power [Card et al., 2016], lower overtime hours done by women [Goldin, 2014], or differences in actual skill use. Black and Spitz-Oener [2010] use German survey data to show that women tend to carry out less skill-intensive tasks and consequently earn less than men even within the same "official" occupational category. The authors also argue that half of the gender wage convergence can be attributed to the convergence in executed tasks. Similarly, the convergence in skill use within occupations has halved the part-time wage penalty of women [Elsayed et al., 2017]. The large within-occupation difference in skill use is surprising as occupations are characterized by a detailed list of tasks and duties as to what individuals should do at their workplace [ISCO, 2008].

This paper is the first to investigate directly the possible mechanisms which lead to lower cognitive skill use by women at the workplace. Our most important result is that neither job characteristics nor differences in cognitive test scores can explain the within-occupation gender gap in cognitive skill use. Likewise, a wide set of job characteristics offer no explanation. However, we find that having children increases the skill use of men and decreases the skill use of women. Besides, the gender gap in housework and working hours can explain the child penalty in skill use and a large share of the within- and between-occupation gender gaps as well. We argue that unequal division of housework is an important confounder of the results. Finally, we show that differences in preferences cannot explain the gender gap in skill use at work and we do not find evidence for workplace discrimination in task allocation either.

As a first step, we document that the tasks performed by women are significantly less skill-intensive on average than those performed by men with the same abilities and in the same occupation. We use the 1st wave of the Programme for the International Assessment of Adult Competencies (PIAAC) survey[32]. This data set is unique in the sense that it contains test scores measuring the ability to use cognitive skills as well as detailed information about the actual activities workers do at the workplace (e.g. how often they use a text editor, read directions or instructions, fill in forms etc.). The survey summarizes these activities into standardized indices measuring cognitive and non-cognitive skill use at work. The raw gender gap is around 0.3 standard deviation in numeracy, literacy skill use and in using information and communication technology skills (ICT skills). The composition effect, including schooling, 3-digit occupational categories and a wide set of job characteristics can explain only half of the unconditional gender gap in skill use at work. Furthermore, the gender gap in skill use is apparent at every educational level and in every observed country. These differences are significant in economic terms as they correspond approximately to 4 years of schooling. The novelty of our research is that we control for the cognitive test scores of individuals to show that the gender differences in

[32] The PIAAC represents 24 countries but we only use 12 countries, where we can match time use data. The results are virtually the same if we use the whole sample.

skill use cannot be explained by differences in the ability to use these skills.[33]

In the second part of the paper, we show that having children increases skill use among men and decreases skill use by women. As a consequence, the gender gap in skill use is much smaller among workers without children. We match the time use survey of the International Social Survey Program to the PIAAC data based on demographic characteristics to investigate how the hours spent on market work and housework[34] contribute to the skill use effect of children. We show that women who are responsible for a disproportionately large share of housework do less skill-intensive tasks at the workplace as well. In particular, the time spent on housework and market work can explain the majority of skill use differences among workers with and without children.

In the final part of the paper we discuss the possible mechanisms which may lead to lower skill use at the workplace. First, we investigate the negative relationship between housework and skill use at the workplace. We argue that workers have a capacity constraint in making effort and they have to divide it between housework and using cognitive skills at the workplace. Because of specialization or bargaining within the household, women are responsible for a larger share of the housework (especially in households with children) and that is why they end up using their cognitive skills less at the workplace. To support this argument, we show that coupled men spend half an hour more on housework weekly than single men (7.5 vs. 6.9 hours) while coupled women spent 6.7 hours more on housework than single women (16.9 vs 10.2 hours). The difference in housework hours is driven by women having both a partner and children. Furthermore, the gender gap in skill use is half as large among single workers than the gender gap found in the whole sample. We also show that being married increases the skill use of men and decreases the skill use of women, while the partner penalty decreases after controlling for time allocation. However, unequal division of housework cannot explain why single women with children do more housework than single men with children.

Second, we investigate the role of individual preferences. If women were to use skills

[33] Jimeno et al. [2016] show that skill use at the workplace increases cognitive test scores. That is why the cognitive test scores over-control for the gender gap in skill use at work.

[34] Note: We observe the actual working hours in the PIAAC survey and we only match housework hours.

less at the workplace only because they have different preferences toward skill use, we would expect the gender gap in skill use to disappear once we control for individual preferences. We use actual skill use in leisure time as a proxy for the unobserved preferences. The underlying assumption is that skill use in leisure time is the revealed preferences for using skills. We find that individual differences in skill use in leisure time can explain less than twenty percent of the gender gap in skill use at work. Furthermore, skill use at the workplace and the hours spent on housework are negatively related even if we control for individual preferences.

Third, we cannot find evidence that women are discriminated because employers underestimate their cognitive skills. In their corresponding analysis, Altonji and Pierret [2001] show that the initial decisions of employers are based on easily observable characteristics (e.g. gender), but as time goes on, employers learn the true skills of their workers. As a consequence, high ability workers with long experience tend to fulfill more skill-intensive tasks and are less discriminated against based on gender than employees with shorter experience. Contrary to the prediction of the model, we do not find that the gender gap in skill use decreases with tenure. The last mechanism we investigate is whether employers assume that women at a certain age are more likely to leave thefirm for maternity leave, and that is why they assign less skill-intensive tasks to these women [Yip and Wong, 2014]. However, we find that age-specific birthrates have only a minor effect on skill use at work.

Beyond the literature cited above, our paper also relates to the measurement of workplace tasks. The largest strand of literature uses official task descriptions of occupations to measure the activities performed at the workplace. These papers documented decreasing returns on routine tasks and increasing returns on non-routine cognitive tasks [Autor et al., 2003, Goos et al., 2009, Acemoglu and Autor, 2011, Autor and Dorn, 2013]. Some recent papers apply self-reported skill use measures [Spitz-Oener, 2006, Autor and Handel, 2013, Stinebrickner et al., 2017] and show large within-occupation heterogeneity in cognitive skill use. We add to these papers by showing that women *systematically* use their cognitive skills less than men of the same occupation and cognitive skills.

The paper also relates to the effect of non-cognitive skills on labor market outcomes. Weinberger [2014], Deming [2017], Deming and Kahn [2018] show that the demand for non-cognitive skills increases over time. Furthermore, Cortes et al. [2018] argue that the increasing demand for social skills has positively affected the college premium of women. We add to this literature by showing that women report lower social skill use than men of the same occupation.

2 Data and descriptive statistics

We use the Programme for the International Assessment of Adult Competencies (PIAAC) survey for our analysis. Most importantly, the survey provides a wide set of categorical questions indicating how often respondents do certain activities or use certain tools at their workplace. For each question, workers have to choose one of five categories ranging from "never" to "every day". The answers are summarized into 9 indices.[35] In this analysis, we focus on the summary indices of basic skills (numeracy skill at work, literacy skill at work and ICT skill at work) and examine whether there are any gender differences along these measures. Table 11 summarizes the short definition of the 9 indices, while Appendix Table B-1 gives more detailed information about their construction. We will refer to the indices in the first panel of Table 11 as measures of the skill intensity of a given job in our paper.

The survey is unique as it measures not only the skill intensity of the tasks that the individual carries out during his or her work but also the cognitive skills of the respondents and cognitive skill use in their leisure time. The survey assesses a broad range of abilities, from simple reading to complex problem-solving (Goodman et al., 2013). According to the OECD [2012] definition, the tests related to literacy are developed in a way so as to measure "understanding, evaluating, using and engaging with written

[35] The indices were constructed using the generalized partial credit model (GPCM). The GPCM is developed for situations where respondents have two choose from ordered categories. The outcome of the model is a continuous one dimensional "scale" which takes a higher value if the respondent is more likely to do the activities in the questionnaire. For technical details and for the reliability of indices, see Section 20.5 in OECD [2013].

Table 11: Definition of the main index variables

Name of the index	Definition	
in the main analysis		
Numeracy	Index of use of numeracy skills at work (basic or advanced)	
Writing	Index of use of writing skills at work	Literacy at work*
Reading	Index of use of reading skills at work	
ICT	Index of use of ICT** skills at work	
in the appendix		
Influence	Index of use of influencing skills at work	
Planning	Index of use of planning skills at work	
Ready to learn	Index of readiness to learn	
Task discretion	Index of use of task discretion at work	
Learning at work	Index of learning at work	

*The index of literacy at work combines the two indices, namely reading skills at work and writing skills at work, into one measurement by using the methodology developed by Anderson [2008].
**information and communication technologies

text to participate in society, to achieve one's goals and to develop one's knowledge and potential" (OECD, 2012, p. 20). Similarly, the numeracy skill tests are aimed to measure "the ability to access, use, interpret, and communicate mathematical information and ideas, to engage in and manage mathematical demands of a range of situations in adult life" (OECD, 2012, p. 33). Hereafter, we use these tests as proxies for cognitive skills. The survey also provides information on the respondents' labor market status, education, social background, occupation (3-digit ISCO codes), activities on the job etc. It also collects information on a wide set of leisure time activities (how often one reads journals in leisure time, how often the respondent uses a computer for communication in leisure time, etc). The answers on these categorical questions are summarized into four skill use indices (numeracy, reading, writing and ICT skill use). We standardized all indices to have a mean of zero and a standard deviation of one.

The study was conducted in 2011-2012 by interviewing about 5000 individuals (aged 16-65) in each of the participating countries. In our analysis, we are focusing on 12 countries only, where we can link the PIAAC data to the time use information[36]. Altogether, we observe a sample of 36,798 working individuals (see Table 12), 54% of which

[36]In Section 3, we also investigate the gender gap by country.

Table 12: Sample size by country and gender

Country	Men	Women	Total
Czech Republic	1,168	1,538	2,706
Denmark	2,016	1,960	3,976
France	1,634	1,811	3,445
Great Britain	1,638	2,585	4,223
Germany	1,357	1,612	2,969
Japan	1,569	1,522	3,091
Korea	1,718	1,665	3,383
Norway	1,282	1,461	2,743
Poland	1,603	1,809	3,412
Russian Federation	466	1,173	1,639
Slovakia	1,155	1,420	2,575
Spain	1,254	1,382	2,636
Total	16,860	19,938	36,798

are women. We use the sampling weights provided by the OECD throughout the analysis.

Table 13 provides basic descriptives for males and females. To facilitate comparison, we also provide the estimated differences across gender and the t-statistics. We use the sampling weights provided by the data set and we use the full sample.[37] Male workers are somewhat more experienced and they are more likely to have full time jobs. As a consequence, men work 7.6 hours more on average than women. Women tend to have higher levels of education and work at state-owned companies and non-profit organizations. According to the literacy and numeracy test results, males are better in mathematical problems while women have better literacy skills. These findings are similar to the patterns documented in the literature (Fryer and Levitt, 2010).

The information on housework and family care comes from the fourth wave of the International Social Survey Programme: Family and Changing Gender Roles (ISSP). The survey was conducted in 2012 and aims to measure attitudes toward marriage, child bearing and activities pursued in leisure time and at the workplace (ISSP, 2016). The database contains self-reported information on the hours spent on housework and family care separately.[38] As a first step, we calculate average housework and family care by

[37]The results are virtually the same for the sub-sample where all measures of the skill intensity of the job are available.

[38]The ISSP survey asks "On average, how many hours a week do you personally spend on household

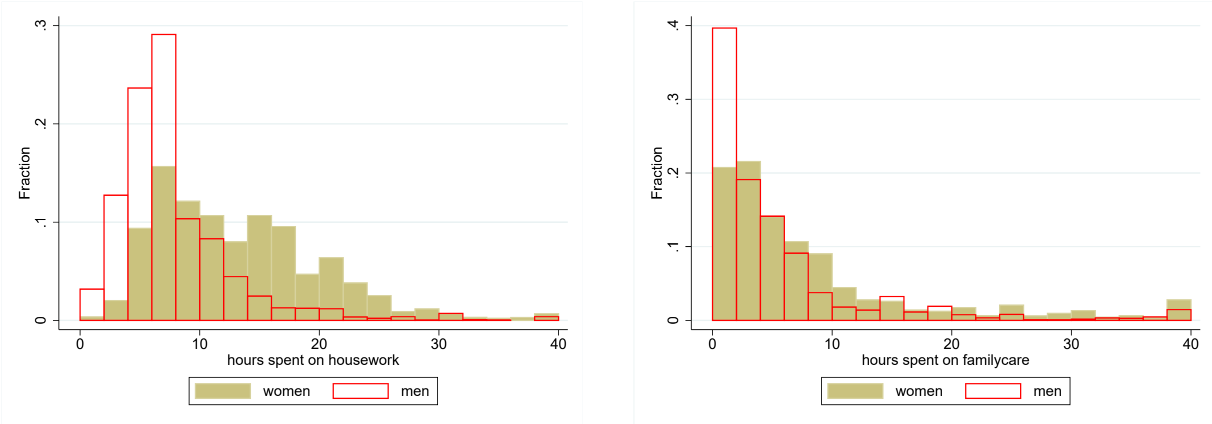
Table 13: Descriptive statistics of the main variables

Variable	Male	Female	Difference	t-stat
Experience (year)	19.94	17.73	-2.20	-7.37
	(0.21)	(0.20)		
Years of education	12.67	13.12	0.45	7.90
	(0.04)	(0.04)		
Share of full time workers	0.81	0.66	-0.14	-13.43
	(0.006)	(0.008)		
Weekly work hours	42.5	34.9	-7.6	-37.3
	(0.150)	(0.137)		
Share of those who have children	0.64	0.69	0.05	4.39
	(0.007)	(0.007)		
Native	0.81	0.82	0.01	0.66
	(0.007)	(0.007)		
Employed in private sector	0.82	0.69	-0.13	-13.06
	(0.006)	(0.007)		
Share of public & non-profit organizations	0.18	0.31	0.13	12.72
	(0.006)	(0.007)		
Average numeracy test score*	0.08	-0.09	-0.17	-7.47
	(0.015)	(0.020)		
Average literacy test score*	-0.02	0.03	0.05	2.14
	(0.017)	(0.021)		
Observations	19,313	17,319		

*Standardized test score with a mean of 0 and a variance of 1

Figure 6: Distribution of weekly housework and family care by gender (hours)

Panel A: distribution of hours spent on housework   Panel B: distribution of hours spent on family care



Note: The number of hours spent on housework and family care is winsorized at 40 hours.

country of origin, gender, marital status, 1-digit occupational category, educational level and by the number of children. These categorical variables define 1476 distinct segments which we observe both in the ISSP and the PIAAC. Next, we merge the segment-level average hours spent on housework from the ISSP with the individual observations in the PIAAC data.[39]

Figure 6 Panel A shows the distribution of weekly housework in the PIAAC database. According to the figure, the hours spent on housework vary significantly across individuals and we also find important gender differences in this regard. On average, women devote 7.2 more hours to housework than men and they are significantly less likely to report fewer than 10 hours. Compared to housework hours, we can observe a much smaller gender difference in the hours spent on family care. Although men are more likely to report very low hours spent on family care, on average, women spend only 3.2 hours more on family care than men.

We can also test the reliability of the results by comparing the self-reported and spouse-reported hours spent on housework. The ISSP survey includes only one member of the household and the respondent has to gauge the amount of her own and her spouse's

work, not including childcare and leisure time activities?" and "On average, how many hours a week do you spend looking after family members (e.g. children, elderly, ill or disabled family members)?"

[39]The segments represents 9425 individuals in the ISSP, which means that the segments contain 6.4 individuals on average. The between-group variation of housework hours covers more than 60 percent of the total variance in household hours (The total standard deviation of housework is 10.5 hours, while the between-segment variation is 6.6 hours). The information loss is less in the case of family care, where the total standard deviation is 12.6, while the between-group variation is 10 hours.

housework. If people systematically overestimate their own housework, we would assume that self-reported housework hours will be hihger than spouse-reported housework hours.[40] In contrast, Appendix Figure B-1 highlights that the distribution of housework remarkably overlaps for both men and women. That is why we conclude that the number of self-reported hours spent on housework is indeed an unbiased measure of the activities at home.

Finally, we plot the average hours spent on family care as the function of hours spent on housework. By doing so, we test whether people responsible for an especially large amount of housework can devolve family care to other adults in the family/household. Appendix Figure B-2 groups the people into 20 equally sized bins by the amount of reported housework and plots the average hours spent on family care for men and women. The figure highlights that women spend more time on family care at every level of housework and people who report larger amounts of housework also spend more time on family care. Based on these facts, we conclude that there is no trade-off between doing more housework and spending more time on family care.

Finally, we show that having a family (partner and children) affects the time allocation of workers, while differences in occupations has little effect on the amount of time spent on housework and family care. We run a regression where the dependent variable is hours spent on housework or hours spent on family care and we control for family structure (having partner and having children and also their interaction), education, occupation, and country. Appendix Table B-2 shows the results for housework and Appendix Table B-3 shows the results for family care. Controlling for education, occupation and a wide set of interaction terms does not increase the R-square compared to the case when we only control for family structure (first column in both Tables). The variables that proxy the family structure (gender, child, partner, and their interaction terms) have a significant effect on the time allocation of workers, while the occupation dummies are (mainly) insignificant. Being highly educated decreases the time spent on housework and increases the time spent on family care. Having children increases the hours spent on housework

[40] This may be especially problematic among women, who may over-report their housework because of social expectations.

by 2 hours for women and the hours spent on family care by 9.4 hours for women (see Appendix Table B-2, 3rd column and Appendix Table B-3, 3rd column respectively). Having a partner increases the hours spent on housework for both gender, but significantly more for women, while there is no gender gap in the effect of having a partner on hours spent on family care.

3 Results

This section shows that women use their cognitive skills at the workplace less often than men but the heterogeneity in individual and job characteristics cannot, in itself, explain this gender gap. To prove this claim, we run Mincerian-type regressions where the left hand side variable is one of the indices measuring the skill intensity of the job (see Table 11). We pool all countries in our sample together. Our main right hand side variable is gender, while controlling for different sets of variables:

$$y_i = \alpha + \beta * female_i + X_i\gamma + u_i, \tag{4}$$

where y_i denotes the examined skill intensity measure (standardized to have a mean of zero and a standard deviation of one) and X_i is the set of control variables. The main coefficient of interest is β showing the gender gap in skill use at the workplace. Most importantly, we can make use of the data on the numeracy and literacy test scores of the survey respondents.[41] The test scores enable us to show that women do not use their cognitive skills less because of their lack of skills. Besides, controlling for individual skills, we also mimic a Mincerian-type wage equation by controlling for years of education, experience, experience-square, occupation (3-digit ISCO codes), etc.[42] As occupations are defined by a detailed list of tasks and duties the employees have to fulfill at their workplace, the occupation categories alone should explain the individual heterogeneity in skill use at work. By including occupational categories and cognitive test scores in the

[41] The survey does not measure ICT skills.

[42] The remaining control variables are country fixed effects, parents' highest level of education and parents' immigration status, dummy for full time work, self-employment dummy, dummy for those having a permanent contract, dummies for 1-digit industry, 5 firm size categories, private sector control.

control variables, we do not only control for the tasks what employees should carry out at work but also for the individual's ability of using cognitive skills.

The point estimates for equation 4 are shown in Table 14. The three skill use indices are shown in separate panels while the columns differ in control variables. According to Column (1), women use their cognitive skills with an approximately 0.3 standard deviation less than males. The raw differences are somewhat larger in numeracy skill and literacy skill use (coef. 0.29, s.e. 0.02) than in ICT skill use (coef. 0.27, s.e. 0.02).

To better understand the magnitude of these point estimates we add years of education and cognitive test scores to Column (2). Not surprisingly, the years of education is positively correlated with skill use at work. On the one hand, workers with one more year of education use their cognitive skills with 0.05-0.1 standard deviation (s.e. 0.005) more. This means that the gender gap in cognitive skill use is large: it is of the same magnitude as approximately 3-4 extra years of schooling.

In line with our intuition, cognitive test scores are positively correlated with skill use at work. Individuals having one standard deviation higher numeracy and literacy test scores use also their literacy and ICT skills with approximately 0.05 standard deviation more. However, numeracy skill use is related only to the numeracy test scores. Workers with one standard deviation larger numeracy test scores use their numeracy skills with 0.198 standard deviation more (s.e. 0.03), but better literacy skill scores do not affect their numeracy skill use significantly. Finally, Column (2) also reveals that individual differences in cognitive test scores cannot explain the gender differences in skill use at work.

Column (3) incorporates the full set of individual and job characteristics. The control variables are actual working hours, experience, square of experience, dummies for 1-digit industry codes, 5 firm size categories and a wide set of information on family background. Most importantly, Column (3) includes 3-digit ISCO codes to control for tasks the workers should execute at their workplace. According to the results, these variables cannot explain the gender gap in skill use: two-thirds of the raw gender gap in literacy skill use and half of the raw gap in numeracy and ICT skill use remain unexplained.

Table 14: Gender gap in skill use at work

	(1)		(2)		(3)	
Panel A: Numeracy skill use at work						
	coeff.	(s.e.)	coeff.	(s.e.)	coeff.	(s.e.)
Gender gap	-0.293***	(0.017)	-0.229***	(0.017)	-0.144***	(0.016)
Years of education			0.054***	(0.003)	0.027***	(0.004)
Literacy test scores			-0.040	(0.030)	-0.006	(0.023)
Numeracy test scores			0.198***	(0.030)	0.143***	(0.022)
Observations	30,263		30,263		30,263	
R-squared	0.021		0.087		0.261	
Controls for job characteristics	No		No		Yes	
Panel A: Literacy skill use at work						
Gender gap	-0.298***	(0.016)	-0.254***	(0.017)	-0.172***	(0.017)
Years of education			0.103***	(0.004)	0.049***	(0.004)
Literacy test scores			0.045**	(0.022)	0.007	(0.019)
Numeracy test scores			0.065***	(0.024)	0.014	(0.019)
Observations	31,278		31,277		31,277	
R-squared	0.022		0.140		0.319	
Controls for job characteristics	No		No		Yes	
Panel C: ICT skill use at work						
Gender gap	-0.275***	(0.017)	-0.245***	(0.018)	-0.134***	(0.018)
Years of education			0.072***	(0.004)	0.037***	(0.004)
Literacy test scores			0.053**	(0.022)	0.038*	(0.023)
Numeracy test scores			0.050**	(0.025)	0.004	(0.024)
Observations	25,931		25,931		25,931	
R-squared	0.019		0.073		0.290	
Controls for job characteristics	No		No		Yes	

Notes: Standard errors are in parentheses *** p<0.01, ** p<0.05, * p<0.1. Control variables differ by column. Column (2) controls for years of education and standardized literacy and numeracy skills. Column (3) also controls for partner dummy, child dummy, years of education, experience, experience^2, occupation categories (ISCO 3-digit), country fixed effects, parents' highest level of education and parents' immigration status, dummy for full time work, self-employment dummy, dummy for having a permanent contract, dummies for 1-digit industry, 5 firm size categories, private sector. Standard errors are calculated with the jackknife method (suggested by OECD, 2013) using 80 replication weights. All of the results are calculated by using sampling weights provided by the survey.

Heterogeneity of the gender gap by groups. We also investigate whether the gender gap in skill use differs by groups. First, we estimate the skill use by country. Appendix Table B-4 shows that there is significant heterogeneity across countries. We observe the largest gender gap in skill use in Korea and Japan, where gender inequality is traditionally large. Surprisingly, the gender gap in skill use is also very large in Scandinavian countries (Denmark and Norway), which are considered some of the most gender-equal societies. In contrast, we find the smallest gender gap in skill use at work in the Post-Communist countries (Poland, Russia, Slovakia). These countries have the lowest gender gap in numeracy and literacy skill use but an above-average gender gap in ICT skill use.

Appendix Figure B-3 plots the gender gap in skill use by educational categories. We find a significant gender gap in every educational category. Women with secondary education find the largest penalty in numeracy and literacy skill use compared to men of the same educational level. This difference remains significant even if we control for occupation, cognitive test scores, working hours and other control variables. Furthermore, women with professional degrees suffer the largest penalty in ICT skill use, but the gap decreases once we control for worker composition.

We do not find large heterogeneity across broad occupational categories either. Appendix Figure B-4 shows that the gender gap is of a similar magnitude in every broad occupational categories.[43] The only notable exemptions are service jobs where the gender gap in literacy and ICT skill use is above-average, although we do not find such a difference in numeracy skill use.

Finally, we investigate the gender gap in skill use by firm size. Appendix Figure B-5 shows that the unconditional gender gap in skill use is apparent at every firm size but somewhat smaller at the largest firms. This negative relationship is robust to introducing controls for individual characteristics (e.g. occupation and cognitive skills, working hours) and it is the most apparent in ICT skills.

Gender differences in cognitive skills and the skill requirement of jobs. The cognitive test scores of men and women do not differ much on average (Table 13) and they only

[43] The categories are based on 1-digit ISCO codes.

have a small effect on actual skill use at work (Table 14). Still, we can construct a simple theoretical scenario where the gender gap in skill use at work is driven by differences in cognitive skills. Assume that women have better cognitive test scores than men in occupations with very low skill requirements (thus with a small gender gap in actual skill use), while women have relatively lower cognitive test scores in occupations with high cognitive skill requirements (thus a large gender gap in actual skill use). In this case, the cognitive test scores and the gender gap in skill use would be uncorrelated in the whole sample but negatively correlated across occupations. To test this scenario, Appendix Figure B-6 plots the average skill use at work by the gender gap in skill use. Every dot displays a specific 3-digit ISCO code. The horizontal axis shows the average gender gap in cognitive test scores in a given occupation (a positive number means that women in that occupation have better skills than men on average). The vertical axis represents the average skill use in the given occupation[44]. The figure highlights that women have higher cognitive test scores than men in occupations with high literacy skill use, but the gender gap in cognitive test scores is uncorrelated with numeracy and ICT skill use. Based on these facts, we conclude that the gender gap in skill use cannot be explained by the lack of cognitive skills in highly skill-intensive occupations.

Non-cognitve skill use at work. Women on average have better non-cognitive skills than men [Jacob, 2002]; that is why women may specialize in tasks which need higher non-cognitive skill use and lower cognitive skill use than the tasks fulfilled by men. If this was the main reason for the gender gap in cognitive skill use, we would expect that women report higher non-cognitive skill use than men.

To test this hypothesis, we estimate the gender difference in non-cognitive skill use. The PIAAC survey has four indices measuring non-cognitive skill use, most importantly, the planning and influencing skill use at the workplace. We re-estimate Equation 4 using these variables in Appendix Table B-5. Column (1) shows that women use planning and influencing skills with 0.15-0.22 standard deviation less than men. Furthermore, Column (2) and (3) reveal that the gap does not disappear once we control for occupation, cognitive

[44] For the sake of simplicity, we pool the skill use of men and women together.

skills and a wide set of other control variables. Finally, Panel C and Panel D show that women have also lower task discretion and use their learning skills less often. As women use non-cognitve skill less often than men, we conclude that specialization in non-cognitive skill use cannot explain the lower gender gap in cognitive skill use.

Similarly, if women are more focused on non-cognitive skill use, we expect a larger gender gap in cognitive skill use in occupations with the highest non-cognitive skill requirements. That is why we estimate the relationship between the non-cognitive skill requirements of occupations and the within-occupation gender gap in cognitive skill use. We use the importance of cooperation in the given occupation as a proxy for the non-cognitive skill requirements of that occupation[45]. Appendix Figure B-7 orders the 3-digit occupations by the importance of cooperation and plots the gender gap in cognitive skill use in every occupations. The figure highlights that there is no significant relationship between the cooperation skill requirements of the occupation and the gender gap in cognitive skill use. This result also suggests that women do not report lower cognitive skill use than men because they overweight the importance of non-cognitive skill use.

3.1 The effect of children and time allocation on the gender gap in skill use

In the previous section we showed that the gender gap in skill use cannot be explained by education, occupation, firm characteristics, or by differences in ability. In this section, we investigate how having a child and gender differences in working hours contribute to the gender gap in skill use. This exercise is motivated by previous studies showing that children in the family [Kleven et al., forthcoming] and working hours [Goldin, 2014] are key drivers of the gender pay gap.

Table 15 shows the effect of children on the gender gap in skill use at work. Column (1) of Panel A highlights that women without children use their numeracy skills with 0.225 (s.e. 0.027) standard deviation less than men without children. This result suggests

[45]We use the standardized importance of cooperation measure of O*NET [2018] and the crosswalk of Hardy et al. [2018] to link the O*NET occupational categories to the 3-digit ISCO-08 codes.

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Table 15: The effect of children on the gender gap

	(1)		(2)		(3)	
Panel A: Numeracy skill use at work						
	coeff.	(s.e.)	coeff.	(s.e.)	coeff.	(s.e.)
Female	-0.225***	(0.027)	-0.208***	(0.026)	-0.155***	(0.025)
Children	0.094***	(0.024)	0.082***	(0.024)	0.006	(0.024)
FemaleXChildren	-0.109***	(0.030)	-0.038	(0.029)	0.014	(0.026)
Observations	30,263		30,263		30,263	
R-squared	0.023		0.088		0.261	
Panel A: Literacy skill use at work						
Female	-0.138***	(0.032)	-0.173***	(0.030)	-0.168***	(0.033)
Children	0.203***	(0.033)	0.178***	(0.031)	-0.048	(0.033)
FemaleXChildren	-0.339***	(0.047)	-0.207***	(0.043)	-0.061	(0.039)
Observations	31,278		31,277		31,277	
R-squared	0.027		0.142		0.319	
Panel C: ICT skill use at work						
Female	-0.157***	(0.033)	-0.160***	(0.032)	-0.082***	(0.027)
Children	0.077**	(0.032)	0.069**	(0.031)	-0.023	(0.027)
FemaleXChildren	-0.189***	(0.042)	-0.138***	(0.041)	-0.086***	(0.033)
Observations	25,931		25,931		25,931	
R-squared	0.021		0.074		0.291	
Controls for skills	No		Yes		Yes	
Controls for job characteristics	No		No		Yes	

Notes: Standard errors are in parentheses *** p<0.01, ** p<0.05, * p<0.1. Control variables differ by column. Column (2) controls for years of education and standardized literacy and numeracy skills. Column (3) also controls for partner dummy, child dummy, years of education, experience, experience^2, occupation categories (ISCO 3-digit), country fixed effects, parents' highest level of education and parents' immigration status, dummy for full time work, self-employment dummy, dummy for having a permanent contract, dummies for 1-digit industry, 5 firm size categories, private sector. Standard errors are calculated with the jackknife method (suggested by OECD, 2013) using 80 replication weights. All of the results are calculated by using sampling weights provided by the survey.

that the gender gap in numeracy skill use is significantly smaller among people without children than in the whole sample. The parameter of the children dummy shows that men having a child use their cognitive skills with 0.094 (s.e. 0.024) standard deviation more than men without children. Finally, women having a child use their numeracy skills with 0.094-0.109=0.015 standard deviation less than women without children. Turning to literacy and ICT skill use, we see similar patterns but the negative effect of children on women's skill use is much larger.

Column (2) highlights that the children penalty in literacy and ICT skill use somewhat decreases once we control for education and cognitive test scores, and it disappears in the case of numeracy skill use. Column (3) highlights that the effect of children on women's skill use is insignificant in the case of numeracy and literacy skill use conditional on occupation and other job characteristics. We only find a significantly negative coefficient in ICT skill use, but the child penalty halves compared to the raw differences. It is important to note that occupation and other job characteristics over-control for the effect of children on skill use at work if men and women choose jobs which are reconcilable whith their childrearing preferences. That is why Column (3) provides a lower bound for women's child penalty in skill use at work.

The time allocation of parents is a crucial channel through which children affect labor market outcomes. After having a child, parents may spend more time on housework and family care and allocate less effort to work. That is why we re-estimate Table 15 conditional on the actual hours worked at the workplace and segment-level average hours spent on housework and family care.

Using segment-level averages as a proxy for individual housework has two important consequences. First, this measure of household activities is not correlated with unobserved individual characteristics, which is correlated both with individual hours spent on housework and skill use at work. Therefore, the estimates can be interpreted as the reduced form estimate of an IV regression where the instrument of individual housework is the leave-out-mean of the group [Townsend et al., 1994].[46] Second, the group-level average

[46] The intrumental variable remain correlated with segment-level unobserved characteristics [Angrist, 2014].

measures individual housework and family care with a random measurement error. This implies that we underestimate the effect of housework on skill use at work (attenuation bias) and overestimate the conditional gender gap in absolute terms[Bollinger, 2003]. The same argument applies to using group-level averages as a proxy for individual hours spent on family care. That is why Table 16 gives a higher bound in absolute terms for the gender gap in skill use at work.

Column (1) in Table 16 shows that one additional hour worked at the workplace increases numeracy skill use with 0.012 standard deviation, while spending one additional hour on family care has exactly the opposite effect. The coefficients are somewhat smaller once we control for education or cognitive test scores (Column (2)) or include a wide set of job characteristics in Column (3). On the contrary, the hours spent on family care have only a minor effect on the skills used at the workplace. What is more, Column (3) shows that conditional on job characteristics the hours spent on family care have no effect on skill use at work. The coefficient is exactly zero and its standard error is also small (0.001). This result arises because the hours spent on family care may not be important conditional on hours spent on housework and market work or because of the attenuation bias[47]. Finally, the hours spent on market work and housework have a much larger effect on literacy skill use and ICT skill use, while the hours spent on family care have no effect on these variables either.

Housework hours have an important effect also on the gender gap in cognitive skill use[48]. Hours spent on market work and housework explain 60 percent of the raw gender gap in ICT skill use (Table 16 Column (1)) among individuals without children. Moreover, there is no significant gender gap in literacy skill use among individuals without children conditional on work hours and housework. The effect of children on the skill use of men and women decreases if we take into account the effect of the hours spent on market work and housework (compared to Table 15). Column (2) and (3) highlight that conditional on job characteristics and hours, women with children use their numeracy skills more than

[47] The segment-level averages measure individual-level hours spent on family care with measurement error.

[48] The results are virtually unchanged if we do not control for hours spent on family care

men. These results suggest that the labor market effects of having a child are closely related to the time allocation of families.

4 Discussion

In this section, we investigate the possible mechanisms which could contribute to lower skill use by women even after we have controlled for occupation and cognitive abilities. These are: (i) unequal division of housework; (ii) the possibility that women have different preferences towards skill use from otherwise similar men; (iii) discriminative assumptions of employers about the cognitive skills of women; and (iv) discrimination based on birth rates.

4.1 Unequal division of housework

Individuals may have a capacity constraint on effort and they have to divide their effort between housework and using skills at the workplace. In other words, individuals who exert high effort at the workplace have less time or are too tired to devote high effort to housework as well. Similarly, individuals doing a lot of housework can use their cognitive skills less at the workplace. Because of bargaining or specialization within the household, women are responsible for a larger share of housework. Consequently, they end up using their skills less at the workplace.[49] Under these assumptions, married men with children want to increase the effort at the workplace (and use more skills) to increase the market income of the household, while women increase the effort level spent on housework. In line with the empirical findings, this mechanism predicts that the skill use advantage of men (and disadvantage of women) with children disappears once we control for hours spent on market work and housework. To support this argument, we show that (i) the gender gap in skill use is much smaller among single households, (ii) housework is unevenly divided

[49] If bargaining causes the unequal division of housework then women would prefer to do less housework than they actually do. However, Becker [1985] frames the division of housework as a matter of specialization. He argues that specialization in specific tasks (housework or market work) increases marginal productivity and broadens the Pareto frontier of the household. In this framework, women do more housework to improve the total utility of the household and not because of bargaining constraints. We cannot empirically differentiate between specialization and bargaining.

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Table 16: The effect of children and time allocation of the parents on the gender gap

	(1)		(2)		(3)	
Panel A: Numeracy skill use at work						
	coeff.	(s.e.)	coeff.	(s.e.)	coeff.	(s.e.)
Female	-0.139***	(0.027)	-0.131***	(0.026)	-0.113***	(0.029)
Children	0.060**	(0.025)	0.048*	(0.025)	-0.000	(0.024)
FemaleXChildren	-0.004	(0.028)	0.045*	(0.027)	0.073***	(0.025)
Hours worked	0.012***	(0.001)	0.011***	(0.001)	0.009***	(0.001)
Hours spent on housework	-0.012***	(0.002)	-0.009***	(0.002)	-0.005**	(0.002)
Hours spent on family care	0.004***	(0.001)	0.004***	(0.001)	0.000	(0.001)
Observations	29,938		29,938		29,938	
R-squared	0.053		0.114		0.276	
Panel A: Literacy skill use at work						
Female	0.051	(0.032)	0.003	(0.030)	-0.111***	(0.034)
Children	0.170***	(0.033)	0.145***	(0.032)	-0.063*	(0.035)
FemaleXChildren	-0.132***	(0.048)	-0.023	(0.045)	0.032	(0.042)
Hours worked	0.017***	(0.001)	0.015***	(0.001)	0.013***	(0.001)
Hours spent on housework	-0.030***	(0.003)	-0.027***	(0.003)	-0.008***	(0.002)
Hours spent on family care	0.004***	(0.001)	0.003**	(0.001)	0.002*	(0.001)
Observations	30,955		30,954		30,954	(0.001)
R-squared	0.079		0.189		0.341	
Panel C: ICT skill use at work						
Female	-0.067**	(0.031)	-0.068**	(0.030)	-0.034	(0.027)
Children	0.056*	(0.032)	0.048	(0.031)	-0.029	(0.027)
FemaleXChildren	-0.080*	(0.042)	-0.032	(0.040)	-0.012	(0.031)
Hours worked	0.010***	(0.001)	0.010***	(0.001)	0.009***	(0.001)
Hours spent on housework	-0.014***	(0.002)	-0.015***	(0.002)	-0.009***	(0.002)
Hours spent on family care	0.003*	(0.002)	0.003	(0.002)	-0.001	(0.001)
Observations	25,701		25,701		25,701	
R-squared	0.044		0.097		0.304	
Controls for skills	No		Yes		Yes	
Controls for job characteristics	No		No		Yes	

Notes: Standard errors are in parentheses *** p<0.01, ** p<0.05, * p<0.1. Control variables differ by column. Column (2) controls for years of education and standardized literacy and numeracy skills. Column (3) also controls for partner dummy, child dummy, years of education, experience, experience^2, occupation categories (ISCO 3-digit), country fixed effects, parents' highest level of education and parents' immigration status, dummy for full time work, self-employment dummy, dummy for having a permanent contract, dummies for 1-digit industry, 5 firm size categories, private sector. Standard errors are calculated with the jackknife method (suggested by OECD, 2013) using 80 replication weights. All of the results are calculated by using sampling weights provided by the survey.

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Table 17: Gender gap in skill use at work - Single households

	(1)		(2)		(3)	
	Panel A: Numeracy skill use at work					
	coeff.	(s.e.)	coeff.	(s.e.)	coeff.	(s.e.)
Gender gap	-0.163***	(0.030)	-0.117***	(0.026)	-0.094***	(0.028)
Hours worked					0.007***	(0.001)
Hours spent on housework					-0.003	(0.003)
Hours spent on family care					0.001	(0.004)
Observations	10,374		10,374		10,167	
R-squared	0.007		0.236		0.248	
	Panel A: Literacy skill use at work					
Gender gap	-0.099***	(0.032)	-0.114***	(0.028)	-0.067**	(0.027)
Hours worked					0.010***	(0.001)
Hours spent on housework					-0.008**	(0.004)
Hours spent on family care					0.003	(0.002)
Observations	10,611		10,611		10,410	
R-squared	0.002		0.324		0.346	
	Panel C: ICT skill use at work					
Gender gap	-0.135***	(0.034)	-0.040	(0.032)	-0.021	(0.032)
Hours worked					0.007***	(0.001)
Hours spent on housework					-0.007*	(0.004)
Hours spent on family care					-0.002	(0.003)
R-squared	8,587		8,587		8,427	
	0.004		0.325		0.339	
Controls	No		Yes		Yes	

Notes: Standard errors are in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables differ by column. Column (2) controls for years of education and standardized literacy and numeracy skills. Column (3) also controls for partner dummy, child dummy, years of education, experience, experience^2, occupation categories (ISCO 3-digit), country fixed effects, parents' highest level of education and parents' immigration status, dummy for full time work, self-employment dummy, dummy for having a permanent contract, dummies for 1-digit industry, 5 firm size categories, private sector. Standard errors are calculated with the jackknife method (suggested by OECD, 2013) using 80 replication weights. All of the results are calculated by using sampling weights provided by the survey.

within partnerships, but there is no gender difference in housework hours among singles and (iii) having a partner increases the skill use of men, while decreases the skill use of women, controlling for time allocation shrinks the marriage penalty of women.

Table 17 shows the gender gap in skill use among workers without a spouse. The most striking result is that the raw gender gap is less than half of the raw gender gap found in the full sample and it is even smaller than the gender gap among individuals without children (see the first column in Table 14 and Table 15, respectively). We find the lowest gender skill gap in literacy skill use, where the raw gap among workers without a spouse is one third of the gap found in the total sample (0.099 vs. 0.298 standard deviation).

Furthermore, Column (2) shows that the conditional gender gap in skill use is also smaller than the conditional gender gap among workers with a spouse. Finally, if women living with a partner trade off market effort for housework, we expect no relationship between housework and skill use at the workplace among single workers. Still, Column (3) shows mixed results. While hours spent on housework is not correlated with numeracy skill use at work it is negatively correlated with literacy and ICTskill use at the workplace. In the case of the full sample, an individual who spends an average amount of hours on housework (10.6 hours per week), uses his numeracy skill 0.053 standard deviation less (-0.005*10.6, see Table 16, third column), his literacy skill 0.085 standard deviation less (-0.008*10.6) and his ICT skill 0.095 standard deviation less (-0.009*10.6) than someone who does not do any housework at all. In the case of workers without a partner, the effect is smaller. There is no significant effect of housework on skill use in the case of numeracy skill, while a worker who spends an average amount of hours on housework (9.4 hours per week) uses his literacy skills 0.075 standard deviation less and his ICT skill 0.066 standard deviations less (see Table 17 last column) than an unmarried worker who does not spend any time on housework.

Column (3) shows mixed results about the gender gap in skill use as well. While the gender gap in numeracy skill is comparable in size with the gap found in the full sample, the gender gap in literacy skill use halved. In the case of ICT the gender gap was already insignificant in the full sample.

Table 18 summarizes the hours spent on housework by gender and partnership status. The most apparent difference is that women spend more time on housework than men, independent of their partnership status. Not surprisingly, single men without children spend the least amount of time on housework (6.8 hours a week), 2 hours less than single women without children. Furthermore, the table shows ample evidence of unequal division of housework between the partners. Women without children living in a partnership spend 3.2 hours more on housework weekly than single women without children, while men living in a partnership spend only 0.6 hours more on housework than their single counterparts. As a consequence, women living with a partner spend more than almost twice as much

Table 18: Hours spent on housework by gender

	Singles		With partner	
	No Children	Children	No Children	Children
Panel (A) Men				
Hours spent on housework	6.774	7.297	7.332	7.549
	(4.659)	(4.426)	(4.761)	(5.935)
Hours spent on family care	3.085	4.799	4.105	6.592
	(4.496)	(8.173)	(6.2)	(8.524)
Observations	4,653	1,797	1,023	9,349
Panel (B) Women				
Hours spent on housework	8.908	15.212	12.16	17.217
	(5.621)	(6.708)	(6.889)	(7.108)
Hours spent on family care	3.836	6.417	7.455	10.943
	(4.061)	(5.6)	(9.314)	(10.062)
Observations	4,407	2,120	2,922	10,482

hours on housework than men.

On the contrary, we do not find such a large gender difference in hours spent on family care.[50] Living with a partner increases the hours spent on family care for men and women alike. Similarly, people having children spend 2.5 hours more on family care than people without children, regardless of gender. This implies that men and women share family care duties equally.

Furthermore, if housework hours depend only on the division of housework within households, single men and women with children would allocate a similar number of hours on housework. On the contrary, we find that single women with children spend 15.2 hours per week on housework, while single men with children spend only 7.3 hours on housework. This difference cannot be explained by unequal division of housework and other mechanisms may also play a role.

Finally, we show, that having a family affects women and men differently. Appendix Table B-6 shows the results. In line with our previous arguments having a partner increases the productivity (skill use at work) for men and decreases the skill use at work for

[50]Note: The ISSP survey does not specify whether family care is related to children, parents or other family members.

women (see column (1), (3) and (5) for numeracy skill use, literacy skill use and ICT skill use respectively). The marriage penalty for women decreases (in the case of numeracy skill it even disappears) as we control for time allocation (see column (2), (4) and (6) for numeracy skill use, literacy skill use and ICT skill use respectively), while the advantage of married men does not decrease. Similarly to the previous results (see Table 16) conditional on job characteristics, time allocation and having a partner, women with children use their numeracy skills more than men. These results suggest that the labor market effects of having a family is closely related to the time allocation of families.

4.2 Gender differences in skill use preferences

It is possible that for any unobserved reason, women prefer to use their cognitive skills less often than men. As a consequence, women use their skills less at the workplace and are willing to do more housework instead of other leisure time activities with skill use requirements (e.g. reading books). Under these assumptions, both the gender dummy and the housework hours are negatively correlated with the unobserved preferences, and their estimated effect on skill use at work would be overestimated. On the contrary, women would not use their cognitive skills less than men if we could control for unobserved preferences. Similarly, hours spent on housework should not affect the gender gap in skill use conditional on skill use preferences either.

As we cannot observe preferences directly, we use skill use in leisure time as a proxy for actual preferences. We make use of the PIAAC survey questions about activities which are arguably done for leisure and also need the use of cognitive skills (e.g. reading books and magazines, browsing on the Internet). These questions are organized into four standardized skill use indices (numeracy, reading, writing and ICT) which we use as additional control variables serving as proxies for individual preferences. These indices are valid proxies only if two underlying assumptions hold. First, skill use in leisure time shows the revealed preferences toward skill use. In other words, we assume that individuals actually use cognitive skills more in their leisure time if they prefer to use skills more. Second, individuals who prefer to use cognitive skills more also do more skill-intensive leisure time activities instead of housework.

The drawback of this exercise is that skill use in leisure time over-controls for gender differences at the workplace and household hours for two reasons [Angrist and Pischke, 2008]. First, women may have less free time to do skill-intensive leisure time activities because of housework duties. Second, skill use at the workplace and in leisure time can be both complements and substitutes. If skill use at the workplace and at home are complements (substitutes) we expect a positive (negative) correlation between these two variables. Accordingly, women doing less skill-intensive tasks at the workplace would also use their cognitive skills less (more) in their leisure time.

Table 19 shows the gender gap in skill use at the workplace conditional on skill use in leisure time. Individuals using a specific skill more in their leisure time use their corresponding skill more at the workplace as well. For example, Column (1) reveals that individuals using numeracy skills more in their leisure time use their numeracy skills at the workplace as well, while ICT skill use at the workplace is positively related to ICT skill use in leisure time. These suggest that skill use at work and in leisure time are complements. On the contrary, we do not find strong cross-correlations among different types of skill use. E.g. numeracy skill use in leisure time is associated only with numeracy skill use at work, while it has a much weaker effect on literacy and ICT skill use at the workplace. Furthermore, these relationships remain qualitatively the same even if we control for a wide set of control variables. As in Column (3) and (4) we also control for numeracy and literacy test scores, skill use in leisure time does not only measure the ability to use skills.

Turning to the main variables of interest, Column (1) reveals that individual differences in leisure time activities can only explain a small share of the gender gap in skill use at work. Once we include skill use in leisure time as an additional control, the raw gender gap decreases from 0.3 standard deviation (Table 14, Column (1)) down to 0.25 in the case of numeracy and literacy skill use. The drop is larger in the case of ICT skill use, where skill use in leisure time can explain one third of the gender gap in skill use at work. Finally, Column (2) and (4) show that the hours spent on housework are negatively related to skill use at the workplace even if we control for skill use in leisure time. In the case of numeracy skill use at work, the estimated effect of one additional hour of housework drops from 0.01 (Table 16 Column (1)) down to 0.007 standard deviation, but the difference is not significant in statistical terms.

Table 19: Gender gap in skill use at work and leisure time activities

	(1)		(2)		(3)		(4)	
Panel A: Numeracy skill use at work								
	coeff.	(s.e.)	coeff.	(s.e.)	coeff.	(s.e.)	coeff.	(s.e.)
Gender gap at the workplace	-0.246***	(0.019)	-0.201***	(0.022)	-0.105***	(0.017)	-0.067***	(0.022)
Housework hours			-0.007***	(0.002)			-0.006***	(0.002)
Numeracy skill use for leisure	0.297***	(0.014)	0.305***	(0.014)	0.274***	(0.013)	0.276***	(0.013)
Reading skill use for leisure	0.052**	(0.020)	0.045**	(0.021)	0.090***	(0.018)	0.090***	(0.018)
Writing skill use for leisure	-0.015	(0.017)	-0.016	(0.017)	0.008	(0.014)	0.008	(0.014)
ICT skill use for leisure	0.105***	(0.012)	0.103***	(0.012)	0.061***	(0.012)	0.059***	(0.011)
Observations	24,748		24,709		24,748		24,709	
R-squared	0.136		0.138		0.327		0.328	
Panel A: Literacy skill use at work								
Gender gap at the workplace	-0.258***	(0.020)	-0.169***	(0.022)	-0.117***	(0.021)	-0.061***	(0.023)
Housework hours			-0.014***	(0.002)			-0.009***	(0.002)
Numeracy skill use for leisure	-0.038**	(0.015)	-0.024	(0.015)	0.003	(0.014)	0.005	(0.014)
Reading skill use for leisure	0.467***	(0.019)	0.451***	(0.019)	0.370***	(0.017)	0.368***	(0.017)
Writing skill use for leisure	0.133***	(0.014)	0.130***	(0.014)	0.129***	(0.011)	0.130***	(0.012)
ICT skill use for leisure	0.094***	(0.014)	0.091***	(0.014)	0.097***	(0.013)	0.096***	(0.013)
Observations	25,713		25,676		25,713		25,676	
R-squared	0.196		0.201		0.403		0.405	
Panel C: ICT skill use at work								
Gender gap at the workplace	-0.194***	(0.016)	-0.122***	(0.018)	-0.041***	(0.015)	0.023	(0.016)
Housework hours			-0.012***	(0.001)			-0.011***	(0.002)
Numeracy skill use for leisure	-0.005	(0.016)	0.007	(0.016)	-0.019	(0.013)	-0.016	(0.013)
Reading skill use for leisure	0.052***	(0.015)	0.040**	(0.016)	0.069***	(0.016)	0.069***	(0.017)
Writing skill use for leisure	0.018	(0.012)	0.015	(0.013)	0.040***	(0.011)	0.040***	(0.011)
ICT skill use for leisure	0.416***	(0.014)	0.415***	(0.014)	0.343***	(0.014)	0.341***	(0.014)
R-squared	22,738		22,704		22,738		22,704	(0.226)
	0.191		0.196		0.392		0.396	
Additional controls	No		No		Yes		Yes	

Notes: Standard errors are in parentheses *** p<0.01, ** p<0.05, * p<0.1. Control variables differ by column. Column (3) and (4) also control for years of education and standardized literacy and numeracy skills, partner dummy, child dummy, years of education, experience, experience^2, occupation categories (ISCO 3-digit), country fixed effects, parents' highest level of education and parents' immigration status, dummy for full time work, self-employment dummy, dummy for having a permanent contract, dummies for 1-digit industry, 5 firm size categories, private sector. Standard errors are calculated with the jackknife method (suggested by OECD, 2013) using 80 replication weights. All of the results are calculated by using sampling weights provided by the survey.

4.3 Discriminative assumptions about cognitive skills

One reason why employers may assign less skill-intensive tasks to women is because they assume that women have inferior cognitive skills compared to men. Altonji and Pierret [2001] studied this issue and found that employers cannot observe individual skills at the beginning of workers' carrier, but firms can learn over time and get information about individual skills. As a consequence, firms discriminate less and less over time based on easily observable characteristics. It follows from their argument that cognitive skills have an increasing effect on skill use at work as time goes on, while easily observable characteristics (e.g. gender) have a decreasing effect. We can also formalize the argument and estimate the following regression:

$$y_i = \beta_0 + \beta_1 * female_i + \beta_2 * female_i * exp_i + \beta_3 * skill_i + \beta_4 * skill_i * exp_i + \gamma * X_i + u_i \quad (5)$$

As in Equation 4, the dependent variable is cognitive skill use at work. Exp_i denotes the labor market experience of workers while $skill_i$ denotes the cognitive test scores. If women are discriminated because they are assumed to have lower skills then β_4 is positive and β_2 increases once we add β_4 to the regression [Altonji and Pierret, 2001].

The estimation results are shown in Table 20. Contrary to the predictions of this alternative mechanism, the effect of skills does not increase with experience and the gender gap in skill use does not decrease faster once we control for the dynamic effects of cognitive skills. We conclude that women are not assigned tasks requiring lower skills because they are assumed to have inferior skills[51].

[51] Another possibility is that firms do not learn about the skills of individuals. However, this conclusion would be in a strong contrast with previous literature on employer learning [Lange, 2007, Schönberg, 2007, Arcidiacono et al., 2010, Rockoff et al., 2012]

Table 20: Discriminative assumptions about cognitive skills

	(1)	(2)	(3)	(4)	(5)	(6)
	Numeracy skill use		Literacy skill use		ICT skill use	
Years of education	0.029***	0.029***	0.074***	0.074***	0.067***	0.067***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Female	-0.348***	-0.352***	-0.288***	-0.291***	-0.303***	-0.310***
	(0.040)	(0.040)	(0.042)	(0.044)	(0.038)	(0.039)
Experience	0.004**	0.004**	-0.000	-0.000	0.003	0.003*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Female*experience	-0.001	-0.001	0.008***	0.008***	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Numeracy test score	0.137***	0.129***	-0.012	-0.025	0.007	-0.011
	(0.033)	(0.045)	(0.025)	(0.048)	(0.034)	(0.046)
Num. test score*experience		0.000		0.001		0.001
		(0.002)		(0.002)		(0.002)
Literacy test score	-0.053	-0.071	0.037	0.046	0.043	0.024
	(0.033)	(0.052)	(0.023)	(0.044)	(0.029)	(0.048)
Lit. test score*experience		0.001		-0.001		0.001
		(0.003)		(0.002)		(0.002)
Observations	21,133	21,133	21,133	21,133	21,133	21,133
R-squared	0.045	0.045	0.069	0.069	0.055	0.056

The table shows the point estimates for Equation 5. The dependent variables are shown at the top of the column. The control variables are the same as in Table 14: partner dummy, child dummy, years of education, experience, experience^2, cognitive test results, occupation categories (ISCO 3-digit), country fixed effects, parents' highest level of education and parents' immigration status, dummy for full time work, self-employment dummy, dummy for having a permanent contract, dummies for 1-digit industry, 5 firm size categories, private sector and a set of social skills (categorical variable for cultural engagement, political efficacy, social trust, social trust 2 and health status). Standard errors are calculated with the jackknife method (suggested by OECD, 2013) using 80 replication weights. All of the results are calculated by using sampling weights provided by the survey.

4.4 Discrimination based on expectations of childbirth

Some employers may offer less skill-intensive tasks to workers who are expected to stay with the firm for a shorter period of time. As a consequence, employers may discriminate against women because they are more likely to exit the firm for maternity leave [Yip and Wong, 2014]. To test this hypothesis, we organize workers in labor market segments by country, education and age, and merge the segment-specific birth rates from the Human Fertility Database[52]. Using the merged database, we run the following regression:

$$y_i = \beta_0 + \beta_1 * female_i + \beta_2 * fertillty_c + \beta_3 * female_i * fertillty_c + \gamma * X_i + u_i \quad (6)$$

Again, the left hand-side variables are the skill use indices at work. $Fertility_c$ denotes country-education-age specific birth rates, while X_i are the same control variables as in Equation 4. The parameter of $fertility_c$ measures the effect of women's fertility rate on men in the same demographic segment[53]. This parameter can even be positive if firms allocate the skill-intensive tasks from women to men more in higher fertility rate segments[54]. Our main variable of interest is β_3, which is negative if women of a larger fertility rate cohort are assigned less skill-intensive tasks. We consider this parameter as the measure of statistical discrimination, as it shows the effect of the average behavior of the labor market segment on individual outcomes.

The point estimates for Equation 6 show mixed results (Table 21). The estimated effect of women's fertility rate on men (β_2) varies a lot between the skill use indices and they are highly sensitive to the inclusion of control variables, but are mostly positive. As the average fertility rate in our sample is 0.03, the estimated parameters seem to have a very low effect on the skill use of men.

[52] The data are available at the homepage of the Human Fertility Database: http://www.humanfertility.org/cgi-bin/main.php

[53] As the fertility rate is defined for women only, we merge women's fertility by country-education-age to the data. E.g. in the case of a 27-year-old Italian men with a university degree, this parameter shows the effect of the fertility rate of a similar Italian woman (27-year-old, with a university degree).

[54] This may be the case if workers with different skill levels and young and old workers are not perfect substitutes [Card and Lemieux, 2001], but women and men of the same age and skills are close substitutes.

Table 21: The effect of birth rate on the gender gap in skill use

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Numeracy skill use		Literacy skill use		ICT skill use	
Gender gap	-0.311***	-0.140***	-0.353***	-0.193***	-0.335***	-0.163***
	(0.028)	(0.027)	(0.025)	(0.027)	(0.024)	(0.027)
Fertility rate	0.521	1.133***	-1.663***	1.409***	0.407	1.055***
	(0.366)	(0.371)	(0.367)	(0.428)	(0.393)	(0.380)
Fertility rate*women	-0.194	-0.823**	0.980*	-0.679	1.785***	0.852*
	(0.522)	(0.404)	(0.498)	(0.485)	(0.493)	(0.465)
Controls		YES		YES		YES
Observations	21,130	21,130	21,130	21,130	21,130	21,130
R-squared	0.025	0.223	0.028	0.207	0.022	0.273

Notes: The table shows the point estimates for Equation 6. The dependent variables are shown at the top of the column. The control variables are the same as in Table 14: partner dummy, child dummy, years of education, experience, experience^2, cognitive test results, occupation categories (ISCO 3-digit), country fixed effects, parents' highest level of education and parents' immigration status, dummy for full time work, self-employment dummy, dummy for having a permanent contract, dummies for 1-digit industry, 5 firm size categories, private sector. Standard errors are calculated with the jackknife method (suggested by OECD, 2013) using 80 replication weights. All of the results are calculated by using sampling weights provided by the survey.

Turning to the main variable of interest, Column (2) shows that the fertility rate decreases the numeracy skill use of women compared to men of the same age and educational level. Again, the point estimates are low, as the gender gap in skill use would decrease only by 0.823*0.03=0.024 if the birth rate decreased to zero. Moreover, Column (4) reveals that the birth rate does not decrease the literacy skill use of women significantly. The point estimate is negative but statistically not different from zero (coef. -0.68 s.e 0.49). Finally, we do not find a significant negative relationship between the fertility rate and ICT skill use of women (coeff 0.85 s.e. 0.46) even if we control for individual characteristics in Column (6). Based on these results, we conclude that discrimination based on cohort-specific fertility rates cannot explain the gender gap in skill use.

5 Conclusion

Although a large body of empirical literature documents the gender differences prevailing on the labor market, we know much less about what people actually do at their workplace and what causes the within-occupation gender differences. To the best of our knowledge, we are the first to document within-occupation differences in skill use and to examine the underlying mechanisms at the same time.

By using an international survey (PIAAC - Programme for the International Assessment of Adult Competencies) that provides detailed information on tasks performed during work, we found that women report significantly lower levels of numeracy and computer skill usage, and they also read and write significantly less at the workplace than men. This finding is robust against taking into account composition effects (demographic and firm characteristics, different levels of education and experience) and controlling for cognitive skill differences. We argue that the most important predictor of the gap in the skill intensity of jobs across genders is that women do more housework than otherwise identical men. This relationship is the strongest among women with children. Since our finding is robust even after controlling for cognitive test scores, we argue that this difference cannot be attributed to the lack of capability.

We argue that unequal division of housework is an important confounder of the results. More precisely, workers have to divide their effort between housework and skill use at work. Because of household bargaining or specialization within the household, women end up doing more housework and using their skills less at the workplace. We corroborate this explanation by showing that (i) cohabitation with a spouse increases the hours spent on housework much more among women than among men; (ii) the gender gap in skill use at work is much smaller among single individuals. We also showed that individual preferences toward skill use cannot explain the empirical findings and we do not find evidence of statistical discrimination in task allocation either.

Finally, our results imply that division of housework has an effect on labor market outcomes, therefore, policies which aim to decrease gender segregation between occupations cannot fully eliminate gender differences on the labor market. However, further research is needed to explain why women in a partnership and single women with children increase their housework hours compared to men and single women without children.

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Part III

The Salary premium of adopting a Hungarian surname in a multi-ethnic Austria-Hungary

with Attila Gáspár

1 Introduction

What people think about themselves has an enormous effect on their behavior.[55] As a consequence, politicians are constantly trying to shape the identification of the general populace. In modern democracies this effort is made in a quite nuanced way (like promoting ideas such as the "American dream" or "European values"), while less democratic countries (such as the People's Republic of China, see Cantoni et al., 2014) engage more actively in directly trying to transform the mindset of the people.

There is a growing body of literature which shows that identity is endogenous; we add to this work by empirically documenting a case in which the state economically incentivized the change of identity by paying a salary premium to workers who decided to change their family names. We study the early 20th century Kingdom of Hungary, where the aim of the elite was to establish a Hungarian majority in a multi-ethnic country by assimilating minorities. Changing a foreign sounding family name to a Hungarian sounding one was a costly signal a worker of foreign origin could send in order to show compliance to this goal.

Using unique historical data sets we show that the employees who decided to change their foreign sounding family names to Hungarian sounding family names earned more by

[55] McCright and Dunlap [2011] shows that political identity determines attitudes towards factual information (e.g. more Republicans believe climate change to be a hoax. Akerlof and Kranton [2002] examined children behavior at school and found that students exert different levels of effort in studying to conform the norm established by theor peer groups. Alesina and Giuliano [2013] provide a review on the vast literature on the interplay between culture and economics.

five to ten percent. We identify this earnings premium by pooled OLS and a surname-level instrument. The instrument uses the overrepresentation of family names among name changers compared to their share in the population surname distribution, and thus mitigates identification concerns caused by individual level omitted variables.

This paper extends our previous paper (Gáspár and Pető, 2019, Chapter 3 in Gáspár, 2019) where we studied family name changes at the town level and showed that country-wide name changing patterns are consistent with economic incentives for assimilation. In the current paper we use three unique historical data sets to study the impact of name changing on the individual level. The first is the database of all name changing events that were sanctioned by the Ministry of the Interior that happened between 1867 and 1932.[56] The second is the database of municipal workers of the city of Budapest between 1904 and 1912 which we digitalized and processed. The third is the database of the reserve officers of the Royal Hungarian Army before World War I.[57] We match workers based in these two data sets on names and birth dates with the name changing database to identify name changers.

We regress earnings of workers on a dummy indicating name changer status in an OLS regression and using instrumental variables. The instrumental variable is defined on the level of surnames. We define it as the natural logarithm of the share of people among all name changers who had that previous name divided by the population share of the name, which we identify from marriage records. By defining the IV this way we are able to restrict our attention to potential name changers only (i.e. only those people who had such family names that got changed at one point). The IV provides a natural ranking of names from the very common Hungarian names (which were only changed because adoption, divorce etc.) to the most foreign sounding names. The identification assumption behind the instrument is that conditional on the decision to change name the overrepresentation of a foreign name does not affect the salary of the worker (e.g. a person named "Roth" will not earn more/less as a person named "Rosenberg" just because of his

[56] The database was compiled by the Hungarian Association for Family History Research; the Association generously agreed that we use it for our research (Farkas and Kovesdi, 2015).

[57] Viktor Karády and Péter Tibor Nagy started the digitalization of this data set, and shared it with us; we went on with the work with the help a research grant from The History Project of INET.

name, but because of his decision to Hungarianize). We provide auxiliary evidence which supports this exclusion restriction.

In the final part of the paper we investigate the potential mechanisms that could give rise to the salary premium of name changers. We make use of a historical policy experiment: a one-year public sector campaign in 1898 that promoted name changing which was ended by public outcry and an unrelated political crisis in the following year. A one-time shock to the overall number of name changers decreased the estimated name changing premia significantly, which suggests that the employer used name changing to screen loyal workers from the labor force, and the value of this signal diluted after 1898. An alternative mechanism would be that employers want to discriminate foreign workers irrespective of their family name, but they use the names as a screening device. We show evidence that new names are not choosen in a way to blend into society - adopted names are still informative of foreign background. In contrast, the most common Hungarian names are underrepresented among name changers, suggesting that hiding their foreign background by adopting a very common surname was not the main concern of name changers.

The advantage of using data from the late 19th and early 20th century Hungary is twofold. Data on identity is very scarce, and looking at contemporary context the researcher usually has to rely on survey-based evidence (e.g Fryer and Torelli, 2010, Langevin et al., 2013), which has questionable reliability in such sensitive issues. In our case we rely exclusively on administrative data, both in the case of earnings and in the case of identity (i.e. religion and name changing status). While the use of historical data greatly alleviates privacy concerns, the case of Hungary in the early 20th century provides a close enough comparison for present day. Hungary at that point had an operating market economy, constitutional governance under the rule of law, and considerable respect for individual liberties.

The paper contributes to the literature on the economics of identity (Akerlof and Kranton 2000, 2002, 2005, 2008); in particular, to the literature on endogenous identity. Examples in the literature show that ethnicity (Cassan, 2015, Jia and Persson, 2017, Nix and Qian,

2015) and religion (Atkin et al., 2019, Botticini and Eckstein, 2007) can be endogenous, and there is even example to be found on the strategic choice of first names (Algan et al. 2013, Arai and Thoursie 2009, Biavaschi et al. 2013, Carneiro et al. 2016). We show evidence on the impact of government-sponsored assimilation on earnings.

The paper also contributes to the literature on wage determination and discrimination at the workplace (e.g. Willis, 1986, Guryan and Charles, 2013). The first interesting feature of our case from the viewpoint of this literature is that name-based discrimination is different from gender and racial discrimination, as name changers become identical to other workers with Hungarian names. The second interesting feature is that in the salary differential between changers and non-changers seems not to be statistical discrimination, as differences remain in place many years after the name change, while statistical discrimination is expected to diminish over time as employers learn about their employees (Altonji and Pierret, 2001). Then, as a consequence, the result contradicts the Beckerian argument that taste-based discrimination cannot be sustained in equilibrium (Becker, 2010).

2 Background and data

2.1 Historical background

At the turn of the 19th and the 20th century, Hungary was a multiethnic country.[58] Native Hungarian speakers constituted 45% of the general population in 1881 (source: Census of 1881; see Table 22 for detailed buildup of the population by language). This fact was the result of the tumultous two hundred years between the early 16th century and the early 18th. Occupation, military conflicts, popular uprisings, famine and disease left large swath of territory depopulated, only to be repopulated by both spontaneous and planned immigration.[59] After 1867, Hungary regained sovereignty as a coequal constituent

[58]We give a more thorough description of how the cultural diversity of 19th century Hungary came to be in a previous paper Gáspár and Pető [2019] (Chapter 3 in Gáspár [2019])

[59]See Kontler [1999] for a historical overview.

of imperial Austria-Hungary.[60] This arrangement (the "Compromise of 1867") created a constitutional monarchy which guaranteed basic personal, economic and political rights to its citizens.[61]

Table 22: Mother tongue of the population of Hungary in 1881

German	13.10%
Slovak	13.04%
Romanian	16.93%
Ruthenian	2.49%
Croat/Serb	4.41%
Hungarian	44.92%
Total population	13.728.622

Source: Census of 1881.

Although less than half of the country spoke Hungarian as their mother tongue, the elite was almost exclusively Hungarian in their language and their identity, so they perceived the cultural composition of the country as a political threat. They seeked to maintain the status quo by transforming the cultural landscape, and assimilating minorities (Fenyvesi, 1998; Karady, 2012). They used many tools to achieve this goal, such as making Hungarian education compulsory, and requiring the knowledge of Hungarian in some state jobs (Karády, 2001, Héjj and Olszewski, 2015). Encouraging family name changes of names that have a particular ethnic sounding was another such a tool. This meant that the person who changed their name demonstratively chose Hungarian identity and subscribed to the assimilatory "social contract" of the elite (Karády, 2001).

2.2 Name changing

By the end of the 19th century, family name Hungarianization was broadly thought of as a pre-requisite for achieving high social status (Karady, 2012). Cases of family name Hungarianization have been documented since the early 19th century.[62] We know this

[60] See Taylor [1976] for a detailed description of the empire.

[61] By "basic political rights" we mean that regular elections were held, though the suffrage was far from universal.

[62] In our previous paper Gáspár and Pető [2019] (Chapter 3 in Gáspár, 2019) we give a detailed summary of the historical context of name changing. Here we only reiterate the most important facts.

because the state asserted a monopoly over family name changes and regulated against spontaneous name changes (Hornyánszky, 1895). During the Austria-Hungary period the Minister of the Interior of the Hungarian government had the final authority over permitting the change of the family name (Karády and Kozma, 2002b). The administrative cost was substantial before 1880, when it was reduced to a nominal fee as part of the state's increasing encouragement of family name Hungarianization.

Ethnic background was not recorded upon name changing, we can infer this from the former name and religion of name changers. The most overrepresented group among name changers were Hungarian Jews (Karády and Kozma, 2002b, Karády, 2001). In fact, during the whole time period the only year when Jews did not represent the majority of name changers was the year of 1898. Catholic name changers were the second most frequent (suggesting German and Slovak backgrounds) (Gáspár and Pető, 2019, Chapter 3 in Gáspár, 2019).

It is important to note that name changing remained largely voluntary for the whole time period; with one notable exception of the campaign of 1898. In that year the administration of Prime Minister Bánffy Dezső initiated a campaign within the public sector to speed up name changing (Karády and Kozma, 2002b), which involved putting pressure on public sector workers with a foreign name to "lead with an example", and speeding up the administrative process for applicants outside of the public sector (Karady, 2012, Karády and Kozma, 2002b, Nagy, 1992). The fact that name changing remained, at the end of the day, an individual choice, is highlighted by the public outcry caused by even this covert administrative pressure. German minority representatives raised their voices the highest in the national assembly (Karády and Kozma, 2002a).

Figure 7 shows the number of people who changed family names each year from the year of the Compromise in 1867 to 1932. The figure clearly shows the surge in name changing after the lowering of the administrative costs in 1881, and the spike in 1898, which is the consequence of the Bánffy-era policies.

Figure 7: Name changes over time

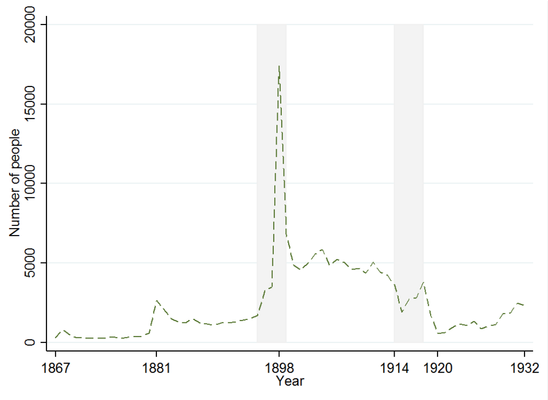


Figure reproduced from Gáspár and Pető [2019] (Chapter 3 in Gáspár [2019]). Source of data: Farkas and Kovesdi [2015] data set. The first shaded area corresponds to the Bánffy cabinet; the second shaded area corresponds to World War I.

2.3 Data and samples

We want to compare salaries of name changers to salaries of workers who did not change their names. Unfortunately, systematic labor force surveys were not conducted in the period that we study.[63] However, there are special communities of workers for whom quite substantial amount of relevant data are available. We use two data sources that represent two different such communities. One is the municipal employees of Budapest, the other is the reserve officers of the Royal Hungarian Army. We hand-match these data on the individual level (based on names, birth dates, and in some cases, birth places) with the universe of name changing cases, population surname distributions and data on education outcomes.

Employee data 1: Municipal employees of Budapest

The local government of the capital city of Budapest compiled and published substantial amount of information on their workforce in their yearbooks. We digitalized two waves

[63] In fact, it was not until 1975 that such surveys started. See https://www.ksh.hu/mult_torteneti_kronologia (in Hungarian).

of the Municipal Almanac (1904, 1907) and two waves of its subsequent publication, the Municipal Yearbook (1909, 1912). These books are the contemporary equivalent of the "Yellow Pages", as they contain comprehensive list of municipal employees and their identification and contact information (their age, where do they work, what is their occupation, the length of their tenure at the local government). However, they also bear an accountability purpose, as they list the salary of the worker, and the year of entering service.

We digitalized these ledgers to get a pooled cross-section of municipal workers. These contain 4136 individual workers altogether, and 8355 person-salary observations. We are not able to use these data to build a proper panel, as there is little variation in salaries between two subsequent years, and few (779) people whom we can identify for all four years. The coverage of the data varies as well, as the earlier version of the ledger (the Municipal Yearbook) is a more concise publication, and it does not cover less of the low skilled workforce. As a consequence, we only use the most recent observations of all workers and treat this data set as a pooled cross-section of workers.

Since this data comes from published, well-kept administrative ledgers, few data cleaning steps were needed. We dropped all females (there were few to begin with, and the other sample is exclusively male, so this ensured comparison), and people in occupations that were shared only by a few other individuals (the number of changer or non-changer is less than 3). As the data covers public sector workers, salaries are not entirely flexible and variation in them mostly comes mostly from promotion. We still use the salary to capture this variation, as promotion information is much more noisy, and salaries are comparable among workers who work in different hierarchies (promotion means different things for city administrators, accountants, workers at public hospitals etc.).

Employee data 2: Reserve officers of the Royal Hungarian Army

The other employee data set we use is that of the reserve officers of the Army. These are the people who received a military training, but retained their civilian jobs as well. The

Army kept track of a lot of information on their schooling, family background, skills.[64] We observe the year of birth, schooling, out-of-school training they received, religion, places of birth and residence. Our sample consists of the 4060 reserve officers for whom we observe salaries later then entering the labor market. We use these second, mid-career salary observations in our cross-sectional regressions.

The data points range from 1869 to 1915, a considerably longer time period than the municipal employees data. From the raw data we calculate years of schooling, a dummy for any outside training, a dummy indicating if the officer was Jewish, and a dummy for broad occupation categories. We also create a wide occupation category - a dummy indicating if the occupation of the officer was clerical (he worked in an administrative job in a hierarchical organization of any kind).

Because the raw data is hand-written, and the degree to which the forms are filled out varies a lot, we restricted the data to those observations which have relatively few missing information. Our final estimation sample consists of all reserve officers for whom we observe salaries later then entering the labor market with non-missing year of the observation (this rule ensure that they remained reserve officers for a protracted period as well), and whose salary observations are closer than 15 years from one another. We only use officers for whom all salary observations are coming between 1876 and 1910. We drop officers whose birth year is unknown; those who have an occupation where we do not have enough observation (at least 15 individuals and at least 3 name changer). We drop officers whose salary less than 500 Ft and higher than 10000 Ft as these, based on their occupations, were misrecorded.

Data on name changing

We use the data set collected by the Hungarian Society for Family History Research (Farkas and Kovesdi, 2015). This data set contains all name changing events that were

[64]Original files are available for inspection in a printed form at the Museum of Military History in Budapest. Viktor Karády and Péter Tibor Nagy started digitalizing the data set. They shared their existing data with us, and then we processed the rest of the records with the help of an INET - The History Project Grant.

sanctioned by the Minister of the Interior between 1867 and 1932. The data set includes the first name, both family names (dropped and adopted) for every individual, the date of name change, age at name change, places of birth and residence at the time of name change, religion and official document number for every individual who changed names. We describe the data in more detail in Gáspár and Pető [2019] (Chapter 3 in Gáspár [2019]).

To identify name changers, we matched this data with the two employee data sets based on name (including former and adopted family name), birth date, and occupation. We used string matching algorithms to match names, then hand-picked the best match considering occupation as well. We created a dummy indicating if the worker was a name changer based on the presence of a plausible match in the name changing data set. We consider every worker a name changer whom we found in the name changing data base and changed name no later than the year of observation in the respective worker data set. We coded every worker as "non-changer" whom we either did not found in the name changing data set, or whom we found but changed name more than 2 years after the observation in the worker data set.

We have two variables that indicate the year of the name change: one indicates the year of applying for permit and the other indicates the year when the permit was issued. The year of the permit is always observed, the application dates are often missing; when both are observed, they not necessarily the same (in these cases the median difference is 1 year). We drop workers for whom the year of the permit is 1 to 3 years after the wage observation, as we cannot be sure whether they already applied for name change or not, and whether the wage impact manifests upon application or permission.

Auxiliary data sets

We hand-match the municipal employee data set with the list of high school graduates compiled by Viktor Karády and Péter Tibor Nagy Karády [2012]. This list contains all graduates of all high schools who finished school between 1850 and 1919 in Hungary.[65]

[65] To be more precise, the graduates of "reáliskola" and "gimnázium" types of high schools, which qualified their graduates for tertiary education. We give a more detailed description of the data and the

We obtain three variables from this data set. One is a dummy indicating if the worker was a high school graduate, the other is the average of their grades subjects which were categorized as "intellectual". The third variable is the religion of the high school graduate, which we used to identify Jewish high school graduates, as they are overrepresented among name changers and were more likely to receive high school education (Karády, 1995, Karady, 2008). We identified 39,5% of people as high school graduates in our sample, of which 66% had observable grade point average. 11% percent of the graduates in our sample were Jews. We did not match the reserve officer data set with the high school data, because the reserve officer data includes information on education and religion.

Another data source we use is the population share of surnames in order to identify which surnames are overrepresented among name changers. For this purpose we use the digitalized civil marriage records which we also obtained from the Hungarian Association for Family History Research. We describe the data in more detail in Gáspár and Pető [2019], Bukowski et al. [2019] (Chapter 2 in Gáspár [2019]); we were unable to identify systematic biases in the surname composition within marriage records, so we treat it as a random sample of the adult surname distribution.

3 Empirical design and results

3.1 Descriptive statistics

Throughout the analysis, we treat municipal employees and reserve officers separately. The reason for this is that the two data sets cover slightly different time periods, and the control variables we observe are not the same.

Table 23 shows descriptive statistics for the workers in the municipal employees dataset by sample restriction and name changer status, Table C-1 and Table C-2 shows the t-test statistics for the full sample and for the restricted sample respectively. We see that workers are not significantly different in terms of their experience, while name changers are slightly younger and are more likely to be high school graduates, and have a Jewish background. It is worth noting that we only observe religion for those who have a high

high school education in Gáspár and Pető [2019], Bukowski et al. [2019] (Chapter 2 in Gáspár [2019]).

Table 23: Descriptive statistics of Municipal Employees

	Full sample		Restricted sample	
Variables	Non-Changer	Changer	Non-Changer	Changer
Age	41.93	41.081	41.76	40.7061
	(11.76)	(11.41)	(11.55)	(11.532)
Experience	13.05	12.996	12.47	12.741
	(9.94)	(9.54)	(9.96)	(9.54)
High school (%)	36%	55%	42%	58%
Jewish (%)	3%	10%	5%	123%
GPA (%)				
GPA=1	4%	8%	4%	9%
GPA=2	8%	11%	9%	122%
GPA=3	9%	14%	11%	14%
GPA=4	2%	5%	3%	4%
GPA=5	0%	0%	0%	0%
Observations	3048	654	1532	529

note: restricted sample is the sample of those workers for whom the instrument is defined

school diploma, so the actual share of Jews might be higher than we see in the data. The variable “GPA” is the rounded average grade of “intellectual subjects”. Grading was on a scale of 1 to 5, which is familiar to the Hungarian reader from the current grading system, but - in contrast with present customs - a grade of 1 represented the highest achievable mark, and a grade of 5 was equivalent to failing. Name changers are more likely to have high school education and also perform better at the school.

Table 24 shows descriptive statistics for the reserve officer data set (Table C-3 and Table C-4 shows the results of the t-test by sample restriction). The average reserve officer in our data is much younger than the average municipal worker was (by about 10 years), but changers and non-changers are not different in this regard. There is no clear pattern in observed skills - changers and non-changers do not differ in terms of years of schooling, while name changers are more likely to have extra training outside of formal schooling. The fact that here we see no difference in educational attainment (which we did see with municipal workers) can be an artifact of the differences in measurement. While with municipal workers we observed whether the individuals graduated from a “gimnázium” or a “reáliskola” type of elite high school institution, we did not know whether they had enrolled in alternative forms of schooling (such as “kereskedelmi iskola” or “tanítóképző”).[66]

[66]“Trade school” or “teacher training institution”. These forms of secondary education did not give their

Table 24: Descriptive statistics of Reserve Officers

	Full sample		Restricted sample	
Variables	Non-Changer	Changer	Non-Changer	Changer
Age	31.76	31.97	31.73	31.99
	(4.18)	(2.43)	(3.71)	(2.42)
Years of school	14.41	14.38	14.33	14.38
	(2.64)	(2.67)	(2.65)	(2.67)
Training (%)	8%	11%	9%	12%
Clerical occ. (%)	47%	61%	47%	62%
Jewish	15%	55%	25%	58%
Observations	2233	244	1148	224

note: restricted sample is the sample of those workers for whom the instrument is defined

The fact that we observe only the years of schooling with reserve officers might mask such compositional differences.

Jews are overrepresented in name changers in this data set as well. More interesting is the much higher share of people with a clerical occupation among name changers. This means that name changing was much more likely to happen at workplaces with a strict hierarchy, where loyalty to one's supervisor might be more important than in other jobs (such as, for example, with somewhat independent professions such as doctors, journalists etc.). This is also highlighted by the fact that in the reserve officer data set the raw share of name changers is just 10%, while in the municipal employee data set (where, by definition, everyone has a clerical occupation) the same share is 18%; the same shares in the restricted samples (where we look at potential name changers only) are 16% (reserve officers) against 26% (municipal employees). It seems that having a clerical job is one of the most important factors in the decision to change name, or would-be name changers prefer clerical jobs.

3.2 OLS estimation of the name changer salary differential

3.2.1 Empirical design

In order to identify the causal impact of name changing on the salary of a worker, ideally we would want to estimate the following equation using ordinary least squares:

$$salary_{it} = \beta * changer_{it} + \delta * controls_{it} + \alpha_i + \lambda_t + \epsilon_{it} \tag{7}$$

Where $changer_{it}$ is a dummy indicating if person i has changed his family name to a Hungarian sounding one until the year t, $controls_{it}$ is a vector of individual control variables, while α and λ are individual and time fixed effects, respectively. In this simple difference-in-differences setting we would identify the impact of name changing from comparing name changers to people who do not change names before and after family name change.

Unfortunately for us, this empirical strategy is not feasible with the data we have at hand. The first reason for that is data quality; we only observe most people once, at most twice; and we only observe a handful of people before and after the name change. The second reason is that name changing is a strategic decision and it usually happened at the start of a person's career. The mean age in the sample of all name changing events is 27; the age at which we observe name changers is 41 years in the Municipal Employees data, and 32 in the reserve officers data.

This second point would also pose a threat to identification in the hypothetical case if we were observing a full panel with an appropriate number of observation before and after the name change. We do not observe counterfactual salaries of name changers and non-name changing workers with a foreign name, so sample selection is an issue.

We first run the feasible version of the regression Equation 7, where we regress the

graduates the opportunity to enter universities, but provided valuable training and were very popular among those who did not have the opportunity to go to more elite institutions. A more thorough description of the Hungarian secondary education system of the time can be found in Bukowski et al. [2019], which is Chapter 2 in Gáspár [2019].

salary of the worker on a dummy indicating if the person changed name, control variables, and a time dummy. Because of the issues discussed, we cannot introduce individual fixed effects.

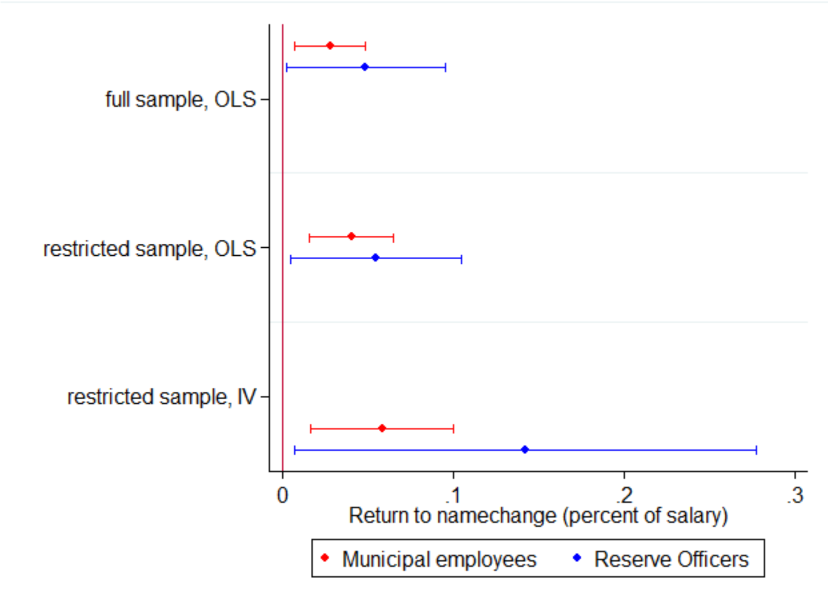
3.2.2 Results

Columns 1 and 4 of Table 25 show the OLS estimation of the name changer dummy from Equation 7 in the municipal employees and the reserve officer data sets, respectively. Table C-5 and Table C-6 shows the estimates for the control variables as well. Figure 8 plots the coefficients of interest from all regressions along with 10 percent confidence intervals. Our results show that name changers earn more than non-changers (by about 2.8% among municipal employees and about 5% among reserve officers); the coefficients from the two data sets are significantly different from zero but not significantly different from one another.

3.2.3 Threats to identification

There are three main threats to identification. First, name changing is only relevant for a subset of workers (those with a foreign background). If, for example, non-changing minority workers are discriminated against, while name changers are not, OLS estimation will confound the estimation of the wage differential between changing and non changing foreigners (a positive term) and between changing foreigners and locals (zero), creating attenuation bias. Second, if selection into name changing is correlated with unobserved factors that affect salaries, the OLS estimates are also plagued by selection bias. The third pitfall is reverse causality. If people who earn more are more likely to change name, what we observe is the effect of salary increase on name changing propensity, instead of the causal effect of name changing on salaries.

Figure 8: The effect of changing name



90 % confidence intervals are shown by using robust standard errors. Control variables in the case of Municipal Employees (red): age and its square, experience at the municipality and its square, whether the worker was found in the education data, average GPA score, occupation dummies, workplace dummies, jewish dummy, year of the observation. All controls are included in the first stage as well. Control variables in the case of Reserve Officers (blue): age and its square, years of school and its square, occupation dummies, dummy indicating whether the worker recieved any training outside the school, jewish dummy, year of observation. All controls are included in the first stage as well. Restricted sample is the sample of those workers for whom the instrument is defined.

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Table 25: The effect of changing name

	Municipal Employees			Reserve Officers		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	IV	OLS	OLS	IV
	Full sample	Restricted	Restricted	Full sample	Restricted	Restricted
	log(salary)	log(salary)	log(salary)	log(salary)	log(salary)	logsalary)
Changer	0.0276**	0.0400***	0.0580**	0.0485*	0.0544*	0.142*
	(0.0125)	(0.0150)	(0.0255)	(0.0283)	(0.0306)	(0.0823)
Experience	0.0198***	0.0210***	0.0207***			
	(0.00221)	(0.00299)	(0.00295)			
Square of Exp./100	-0.00683	-0.0113	-0.0107			
	(0.00689)	(0.0102)	(0.0101)			
Years of School				0.00932	0.0340	0.0336
				(0.0338)	(0.0441)	(0.0430)
Square of YRS				0.00720	-0.100	-0.0997
				(0.123)	(0.162)	(0.158)
Obs.	3,702	2,061	2,061	2,477	1,372	1,372
Rsquare	0.768	0.770	0.770	0.179	0.180	0.175
First stage						
			Changer			Changer
log(overrep)			0.107***			0.0666***
			(0.00372)			(0.00450)
Partial R square			0.26			0.14
F statistics			31.38			6.60

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Control variables in the case of Municipal Employees (column 1-3): age and its square, experience at the municipality and its square, whethet the worker was found in the education data, average GPA score, occupation dummies, workplace dummies, jewish dummy, year of the observation. All controls are included in the first stage as well. Control variables in the case of Reserve Officers (column 4-6): age and its square, years of school and its square, occupation dummies, dummy indicating whether the worker recieved any training outside the school, jewish dummy, year of observation. All controls are included in the first stage as well. Restricted sample is the sample of those workers for whom the instrument is defined

3.3 Instrumental variables estimation of the name changing premium

3.3.1 Empirical design

To overcome the identification concerns we raised with the OLS, we turn to an instrumental variables (IV) regression, where the IV is the natural logarithm of the overrepresentation of a given family name among name changers. The first stage of the IV regression takes the following form:

$$changer_i = \beta' \cdot controls_i + \lambda'_t + log(overrep_n) + \theta_i.$$

Where $overrep_n$ is the surname-specific overrepresentation figure:

$$overrep_n = log\left[\frac{\#surname_n \; in \; changers}{\#changers} / \frac{\#surname_n}{population}\right]. \tag{8}$$

We calculate $overrep_n$ from the *previous* name for people who change name, and the current name for everyone else (so if someone changes name from Schmidt to Kovács, we use Schmidt to calculate the instrument). This instrument needs the following exclusion restriction to be satisfied:

$$E[log(overrep_n) \cdot \epsilon_i \mid controls_i] = 0 \tag{9}$$

In plain words this means that, conditional on observed information, the surname should not in itself have any effect on the salaries directly, only through the decision to change one's family name. The idea behind this instrument is that some names are more likely to be changed than others, and by using only the variation in name changing that is driven by the surname itself, we can reduce the omitted variable and selection issues inherent in the OLS estimation of the impact of name changing on salaries.

The instrument is only defined for a subset of all family names. Notably, the instrument is undefined if either $\#surname_n \; in \; changers = 0$, or if $\frac{\#surname_n}{population} = 0$. The expression means that the IV is only defined for foreign names, as these are the ones which get Hungarianized.[67] This is not of a particularly great concern, as this is equivalent to restricting the sample of all workers to potential name changers only (people with a foreign name). Thus the IV mitigates the first potential concern with the OLS, which was the confusion of potential name changers and non-changers in the control group.

The second restriction (the instrument being defined only for names with a positive population share) in theory, should trivially not be binding if we observed the actual population shares of names instead of proxies. But it does happen in practice, we use the name share in the sample of the marriage records as a feasible alternative. This will mean that for some rare names the instrument will not be defined due to sampling error. This chance increases in the rarity of the given surname. We do not think that this raises particularly severe concerns for our analysis, as we look at ordinary workers instead of, for example, the high aristocracy, whose names would be particularly rare. Because the instrument is not defined for the whole population, we run all regressions on the whole sample and the restricted sample as well - the sample of those workers for whom the instrument is defined.

Most importantly, the instrumental variable mitigates the potential selection bias and reverse causality inherent in the OLS estimates, as it only uses information from family names at birth, which are exogenous to unobserved characteristics and labor market outcomes later in life.

We can interpret the instrument in more than one way. We can argue that the instrument measures the "foreignness" of a surname. We could think that having a Hungarian surname gives the same payoff for everyone, but people with more foreign surnames get reminded more often that having a Hungarian surname would be beneficial. This would have effectively generated variation in the psychological cost of name changing and people with more foreign names would have become more likely to change their family names and receive the monetary payoff.

We can capture this variation by looking at the conditional probability that any person

[67]Strictly speaking this would be true only if all name changes were name Hungarianizations. However, some name changes happen when people divorce, get adopted, or have any other idiosyncratic reason for name change. So in practice for some Hungarian names the IV will be defined, just it will have a very low value.

Table 26: Name distinctiveness

decile	Municipal Employee		Reserve Officers	
1st	-4.0	Lukács	-3.9	Fábian
2nd	-3.6	Bernáth	-3.2	Rácz
3rd	-2.4	Palasti	-1.5	Lehoczky
4th	-1.0	Zimmermann	-0.6	Novák
5th	-0.4	Mihalovics	-0.05	Winkler
6th	-0.0	Schmidt	0.4	Czigler
7th	0.5	Hofbauer	0.8	Pfeifer
8th	1	Wippler	1.2	Glück
9th	1.5	Baintner	1.7	Salzberger
10th	4.4	Losteiner	4.4	Rosenthal

in the population who has this family name would change it. By Bayes' rule the probability of name change conditioning on only the family name can be written as:

$$P(changer_i \mid name = n) = \frac{P(name = n \mid changer_i)}{P(name = n)} P(changer_i) \tag{10}$$

The ratio $\frac{P(name=n|changer_i)}{P(name=n)}$ is the theoretical equivalent of the observed name overrepresentation, which is $\frac{\#surname_n \; in \; changers}{\#changers} / \frac{\#surname_n}{population}$. Taking logs the conditional probability becomes additively separable from all other individual level factors that drive the probability of name change ($P(changer_i)$). We use this as an instrument in the First Stage of the IV regression.

Another way to interpret the instrument is to think of a surname as proxy for community ties. This amounts to saying that all people who share a family name are a relative of one another with some probability. This probability is higher if the shared name is "Habsburg", and lower, if the shared name is "Schmidt".

This interpretation of the instrument posits that there is some basic probability of name changing is determined on the family level (captured by the instrument), which is otherwise not correlated with this particular worker's labor market outcomes. The eventual probability of name change for the individual depends on this basic, "family level" probability, and an idiosyncratic deviation from that, which might be contaminated by selection bias or reverse causality. This is why we instrument the personal decision with the "family level" probability.

If many people change names within a worker's extended family, then the psychological cost of name change will be smaller for the worker, as he will not face condemnation from his own relatives for abandoning the family name (as in Fryer Jr and Levitt, 2004). Two factors determine how strong this effect is, which is represented in the definition of the IV in Equation 8. First is the share of name changers of one particular foreign family name within all name changers. This determines how strong the impact of my relatives on my decision are. The second determines how likely it is that the other name changers with whom I share a family name are my relatives - this is the inverse of the population share of the surname. By taking logs we exclude from the sample everyone who has a Hungarian name ($\frac{\#surname_n \; in \; changers}{\#changers} = 0$). Table 26 shows examples of how the instrument is related to the types of family names.

3.3.2 Results

First, for the sake of comparison, we re-estimate the OLS coefficients for the restricted sample on which the IV is defined. The estimated coefficients are larger (though not significantly) when we only look at the sample of individuals who are name changers or had a foreign name (Columns 2 and 5 in Table 25). This means that people with a foreign name ("potential name changers") earn less than the mean worker in their respective data sets.

Columns 3 and 6 in Table 25 show the respective instrumental variables results on the restricted samples. Column 3 in Table C-5 and in Table C-6 shows the estimates on the control variables as well and Column 4 shows the first stage estimates. The IV coefficients are substantially larger than their OLS counterparts, though less precisely estimated: name changers earn from 5.8% (Municipal Employees) to 14.2% (Reserve Officers) more than their non-changing counterparts. Based on the First Stage F statistics we can confidently reject weak identification in the case of the Municipal Employees, while we cannot rule it out in the case of Reserve Officers (Stock and Yogo, 2002).

This suggests that the OLS regression coefficients are biased downwards. This could be a result of an omitted variable that is positively correlated with salaries, but negatively with name changing (or the other way around). If people with lower unobserved ability are more likely to change name (for example, if loyalty serves as a substitute for ability), that would produce such a downward bias. However, unobserved ability is plausibly correlated with observed skills, and we see systematic relationship between observed skills and name changer status. Another potential mechanism can be a subjective disutility from name changing that is positively correlated with income (one cares more about intangible sources of utility, such as family heritage, once the basic needs are satisfied). Then low earners would self-select into name changing in higher numbers, suppressing the OLS results. Finally, measurement error in the salaries in itself could cause the OLS results to be biased downwards.

The estimated coefficients are large, but not unreasonably so. The largest IV coefficients show that name changers had a salary premium similar to three extra years of experience (municipal workers), or four extra years of schooling (reserve officers), if we compare to the linear component of these control variables only.

3.3.3 Threats to identification

The IV estimates are unbiased if the exclusion restriction holds (that is, if the instrument is not correlated with unobserved factors driving salaries). There are two main concerns which we need to address. One is that the instrument might be correlated with unobserved characteristics of workers which are relevant for their salaries (e.g. people with rare surnames have more human capital). The second, related concern is related to the community tie interpretation of the instrument. If two people from the same family shared higher innate ability or social capital, and they also shared a higher propensity of name change (social or psychological costs associated with family name change can be lower if one does it together with other family members). More directly, family members might even help each other at the workplace, which can result in higher salaries correlated with, but not causally related to, a similar propensity to change names.

Though the assumption behind the exclusion restriction is not directly testable, we show indirect evidence that increases our confidence in the validity of the assumption. In Tables C-7 we show that there is no systematic relationship between observed skills and the instrument in case of the municipal workers. We regress the instrument on different combinations of the relevant explanatory variables. Column 1 has a set of dummies for the (rounded) GPA of those whom we found in the high school database. To limit the number of estimated coefficients, in Column 2 we only estimate a single dummy indicating if the worker had bad grades as a student (GPA>=3). In Columns 3 and 4 we do the same with occupation and workplace dummies included. In Columns 5 and 6 we include an interaction of the high school dummy with a dummy indicating if the graduate was Jewish (we observe religion of high school graduates).

The most important finding is that the instrument does not depend on the GPA for those whom we found in the high school graduate data. The only exception is the significant coefficient on the worst students (3 observations altogether). The negative coefficients on the dummies indicating if the worker was found in the high school graduate data set is also insignificant, except for the cases when we include the Jewish interaction dummy. This latter is potentially an artifact of the data which would be present even if there was no observable selection on skills. Jews are overrepresented among name changers, so Jewish names have a higher IV. Those people whom we do not find in the education data are both Jews or not Jews, so the comparison group has a higher IV than those who are found in the education data, but are not Jewish.

We see a very similar (lack of) pattern with the reserve officer data set, where we can find no significant correlation between years of schooling, training participation, having a clerical occupation and the instrument (Columns 1, 2 and 3 of Table C-8), or all above combined (Columns 4 and 5).

In Figures C-1 and C-2 we show that conditional on the name changing decision there is no relationship between the instrument and the outcome of interest in any of the two data sets. These figures are binned scatter plots that show the relationship between the salary and the logarithm of name overrepresentation. We control for the same variables as in the main regression in calculating the slopes, which are not significantly different from

Table 27: Relatives at the workplace

	log(salary) (1)	log(salary) (2)
Name changer	0.0288**	0.0272**
	(0.0128)	(0.0136)
NC × Relative	-0.0115	0.00832
	(0.0522)	(0.0298)
Relative defined at	Workplace	Anywhere
Observation	3256	3256
R-square	0.774	0.774

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Control variables: age and its square, experience at the municipality and its square, whethet the worker was found in the education data, average GPA score, occupation dummies, workplace dummies, jewish dummy, year of the observation. We drop observations with no information on the exact workplace, but the results are robust to the inclusion of the inclusion of these observations as well.

zero in any of the cases. We conclude that we were unable to find threats that undermine the validity of the identifying assumption behind the instrumental variables estimates.

Finally, we check how likely it is that related name changers help each other at the workplace. For this we can use the Municipality Employee data, which covers all workers of the same (broadly defined) employee. As we use the universe of name changing records that were available, we are able to determine about every worker whether they are name changers or not. Using this information we identify in our data all name changers who share both previous and adopted family names . These workers are the most likely to berelated. In Column 1 and 2 of Table 27 we regress salaries on the name changer dummy and a dummy indicating if the name changer had a relative. The regression includes the same control variables as the baseline wage regressions. In Column 1 we consider only those name changers relatives who work at the same broadly defined workplace at given same year. In Column 2 we consider everyone who had a relative in the Municipality working anywhere at the given year. There are 18 people categorized as relatives in Column 1, and 87 in Column 2, so these are rare cases. The coefficient on having a relative is very small and never significantly different from zero, so we are confident that this mechanism is not driving our results.

3.4 Discussion

Why do changers earn more than non-changers? The most obvious explanation would be discrimination from the municipal government's part, and the government uses foreign names to screen workers of foreign origin. An alternative explanation would be that workers who are more dedicated to their jobs use name changing as a costly signal to communicate their types - that they are loyal, politically trustworthy individuals. We explore the second explanation first, as our results suggests that this is more plausible.

The signaling hypothesis

In Section 2.2 we discussed the name Hungarianization campaign of 1898. We argued in previous work (Gáspár and Pető, 2019 in Gáspár, 2019) that the campaign potentially had an impact on later cohorts of name changers through reducing the cost of further name changing. If this was the case, the impact of name changing on their salaries might also have been different. The historical sources argue that name changing was a "pledge of allegiance" to the Hungarian elite and the status quo of the Monarchy (see, for example, Karady, 2012). In the language of economics this could be interpreted as a case of signaling, where workers are sending a costly signal on their political views with the act of name changing. The costliness is attested by the fact that name changing did not become society-wide during the Austria-Hungary period,[68] and that the policy of 1898 could have such a dramatic impact. However, such a one-time shock to the number of people who have changed names might have reduced the signaling value of name changing by decreasing its psychological costs. If that is the case, we would expect that the salary premia of name changers decrease after 1898.

To study this question, we estimate the following regression by OLS:

[68] Karády [2001] estimates that about 1% of the population who could have potentially been involved was indeed involved in the name changing movement in this era.

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$$y_{it} = \alpha + \beta_1 ChangedBefore1898_i + \beta_2 ChangedIn1898_i + \beta_3 ChangedAfter1898_i + controls_{it} + \varepsilon_{it} \quad (11)$$

In other words, instead of estimating an average effect of name changes, we estimate different coefficients for name changing events that happened before, during and after the policy of 1898.

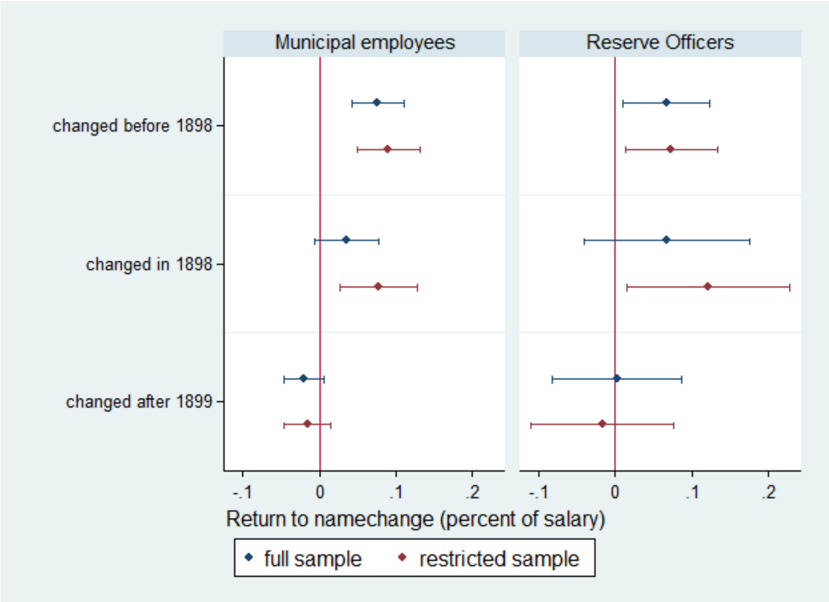
Figure 9 shows the estimated coefficients of interest for the two data sets (see Table 28 for regression results, and Table C-5 and Table C-6 reports the control variables). The left panel shows coefficients estimated from the data set of municipal employees of Budapest, while the right shows the results for reserve officers. The vertical axis corresponds to the estimated β coefficients for (i) those who changed before 1898; (ii) those who changed in 1898; (iii) those who changed after 1898. We present the point estimates with their corresponding ten percent confidence intervals for the full sample. Results blue correspond to estimates from the whole sample, while red corresponds to estimates from the restricted sample.

The results show the same pattern for both data sets. Only those name changers earn significantly more than their non-changer counterparts who changed name no later than 1898. In their case the impact is larger and more precise than in the case when we estimate a single coefficient. The estimated coefficients for later year name changers are zero. This is in line with the signaling value hypothesis. If more people change name, the signaling value of name changing is diluted, and name changers are rewarded less for the act.

Even though the OLS estimate is zero after 1898, we have reason to believe that this is a downward biased estimate of a positive name changing premium, as in the baseline results the IV estimates were larger than the OLS estimates, and because there had to be some reason for name changing after all, as it still was a costly act.

If the goal of the government had been to discriminate based on foreign background, we should see no differences in the name changing premia before and after this event.

Figure 9: The effect of changing name and the Hungarianization campaign of 1898



90 % confidence intervals are shown by using robust standard errors. Control variables in the case of Municipal Employees (left figure): age and its square, experience at the municipality and its square, whether the worker was found in the education data, average GPA score, occupation dummies, workplace dummies, jewish dummy, year of the observation. Control variables in the case of Reserve Officers (right figure): age and its square, years of school and its square, occupation dummies, dummy indicating whether the worker recieved any training outside the school, jewish dummy, year of observation. Restricted sample is the sample of those workers for whom the instrument is defined.

If anything, the premia should be larger: those who did not change name even under the 1898 policy were much more likely to have foreign background, thus discrimination against them should have increased.

Although people whose mother tongue was Hungarian constituted only a plurality in society (47% in 1900, source: census of 1900), people who knew Hungarian were in a majority (59.5% in 1900, source: census of 1900). The share of minorities who spoke Hungarian was higher in major urban centers (where our data is mostly coming from). As we are looking at jobs where knowing Hungarian was essential (municipality employees, army officers), we assume that (potential) name changers that we observe speak Hungarian, and are otherwise indistinguishable from the rest of the workforce. This is in line with historical accounts (see e.g. Karády and Kozma, 2002a).

Table 28: The effect of changing name and the Hungarianization campaign of 1898

	Municipal Employees		Reserve Officers	
	OLS	OLS	OLS	OLS
	Full sample	Restricted	Full sample	Restricted
	log(salary)	log(salary)	log(salary)	log(salary)
Changed				
- before 1898	0.0763***	0.0900***	0.0666*	0.0729**
	(0.0211)	(0.0250)	(0.0347)	(0.0367)
- in 1898	0.0355	0.0769**	0.0676	0.122*
	(0.0251)	(0.0309)	(0.0658)	(0.0647)
- after 1898	-0.0199	-0.0159	0.00193	-0.0171
	(0.0157)	(0.0182)	(0.0518)	(0.0565)
Obs.	3,702	2,061	2,477	1,372
Rsquare	0.769	0.772	0.179	0.181

Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables in the case of Municipal Employees (column 1 and 2): age and its square, experience at the municipality and its square, whether the worker was found in the education data, average GPA score, occupation dummies, workplace dummies, jewish dummy, year of the observation. Control variables in the case of Reserve Officers (column 3 and 4): age and its square, years of school and its square, occupation dummies, dummy indicating whether the worker recieved any training outside the school, jewish dummy, year of observation. Restricted sample is the sample of those workers for whom the instrument is defined

The screening hypothesis

It also could be that employers want to discriminate non-Hungarian workers irrespective of their names but they use the family name as a screening device to decide who has foreign background. In this scenario, it would be rational for workers to adopt their new names in a way that does not carry any information. The most realistic solution for this is to choose common Hungarian names. In comparison, we see that all very common names are underrepresented among name changers and name changers tend to adopt family names that are in the Hungarian language, but are indicative of a name changer background, as they are rare among people who are themselves not name changers.

We calculated for each Hungarian family name how likely it is that their bearers are name changers, that is, the overrepresentation of the name among name changers relative to their population shares:

$$\frac{\#surname_n \; in \; changers}{\#changers} / \frac{\#surname_n}{population}. \tag{12}$$

We use this measure to rank each family name. Table 29 column 2 shows the top 10

Table 29: Rank of names

Top 10 overrep name			Top 10 population		
Rank by overrep.	Name	Rank by popshare	Rank by pop share	Name	Rank by overrep
1.	Dér	23239.	1.	Nagy	5272.
2.	Várnagy	17434.	2.	Szabó	5143.
3.	Mármaros	19774.	3.	Kovács	4427.
4.	Tölgyes	17814.	4.	Tóth	5876.
5.	Abosi	19806.	5.	Horváth	5097.
6.	Vereczkei	20436.	6.	Varga	5358.
7.	Fertő	22536.	7.	Molnár	4263.
8.	Altai	21913.	8.	Német	5511.
9.	Lövész	17234.	9.	Kis	4588.
10.	Várhelyi	14319.	10.	Farkas	4779.
Number of family names in population		36 901	Number of family names in changer data set		8 474

adopted names based on this measure. We ranked each family name based on the name frequency in our population data set. Table 29 column 5 shows the top 10 family names based on our population data.

Table 29 column 3 shows the rank of the top 10 overrepresented names in the population, e.g. “Dér” is the most overrepresented name among name changers, while it was ranked 23239 out of the 36901 names in the population. In general, we can see that the overrepresented names are at the bottom of the surname frequency distribution. On the other hand all common names (Table 29 column 5 shows the top 10 family name based on our population) are underrepresented among name changers, e.g. the most popular family name “Nagy” (“Big”) is only at the 5272th place out of 8474 family name in our name changer data set.

We argue that this name adoption strategy is inconsistent with the screening hypothesis. If the employer had used the name to screen people of foreign background regardless of their intentions to assimilate, name changers would have chosen to blend in, and chose very frequent family names. In that case, the employer would not have been able to extract any information on their backgrounds.

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4 Conclusion

In this paper we showed how the Hungarian state engaged in identity manipulation using economic incentives. Extending our previous work in which we looked at aggregate name changing figures and census data, in this paper we documented a similar phenomenon using data on the individual level.

Looking at two independent historical data sets we find that municipal workers who had a foreign name enjoyed higher earnings if their changed it to a Hungarian names. The same was true in the civilian occupation of reserve officers whom we observe. We use a name frequency based instrument to mitigate identification concerns related to the endogeneity of the name changing decision.

We argue that name changing was used as a costly signaling mechanism, where public sector workers could show political loyalty by responding to the state's assimilation incentives. We argue that the observed behavior is incompatible with discrimination that uses family names to screen for ethnic background, but does not care about the latter for its own sake. The reason is that name changers tended to choose distinctive Hungarian names, and not the most common ones.

We find the results intriguing, because the traditional approach for many years was to treat identity as exogenous when one studied discrimination. Only recently have researchers starting to endogenize different aspects of identity in their formal and verbal models of the economy (e.g. Nix and Qian, 2015). Our aim with this paper was to contribute to this ongoing shift of viewpoints.

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Appendix A : for Chapter 1

Table A-1: Skill requirement measures

Original skill measures	Summary indices
Problem solving	
Taking responsibility	
Ability to adapt to new environments and tasks	IPS
Ability to focus on work	
Ability to work independently	
Co-working skills	
Communication skills	Interpersonal skill
Empathy	
Mathematics skills	Mathematics skills
Manual skills	
Spatial orientation skills	
Creativity	
Stress tolerance	
Ability to work precisely	
Organizing ability	

Table A-2: Top 10 and bottom 10 occupation

FEOR	Title
	Top 10 based on IPS skills
1321	Department managers in agriculture and forestry
1322	Department managers in manufacturing
1324	Department managers in wholesale and retail trade
1326	Department managers in transportation, forwarding and storage
1327	Department managers in communication and postal services
1331	Department managers in business services
1334	Department managers in education
1335	Department managers in cultural services
1345	Supply and distribution managers
1347	Computing services managers
	Bottom 10 based on IPS skills
9117	Garbage collector
9140	Navvies, construction labourers
7511	Animal hair and feather processing workers
7323	Hatters, milliners, cap makers
7334	Pelt dressers, fur dyers
9210	Agricultural labourers
4123	Library and archive stock clerks, other filing clerks
5125	Housekeeper
9115	Chambermaids
9112	Vehicle, window and related clearners
	Top 10 based on Interpersonal skill
2547	Psychologists
2211	General practitioners
2222	Optometrists
2614	Cultural organizers
2618	Qualified coaches
2225	Institution based nurses
3722	Film, stage and related assistant directors
3721	Supporting actors
2431	Primary school teachers
2421	Secondary teachers
	Bottom 10 based on Interpersional skills
3125	Forestry and natural reserve technicians
4192	Stenographers, typists
5316	Dry cleaners
5344	Cinema projectionists
5353	Pests control professional
6112	Bio-gardeners
6113	Vegetable growers
6114	Fruit growers
6115	Wine growers
6116	Ornamental plant and flower gardeners

Table A-3: Identification

	N of case	corresponds to N of workers	corresponds to N of worker-years
all firm		940 872	5 356 887
never changed occupation		465 779	2 665 636
changed occupation within worker-firm spell		172 642	809 590
changed occupation at least once in the sample		475 093	2 691 251
+ changed firm at least once		387 554	2 143 136
changed firm and occ. at the same time	502 889	368 496	2 018 825
domestic ⟶foreign* + occ. at least once		114 813	652 248
domestic ⟶foreig* + occ. at the same time	95 922	94 532	526 679
		corresponds to N of werkers	corresponds to N o acquired worker-yea
acquired firm**		56 436	187 748
- changed occupation within worker-firm spell		6 040	157 634
stayed at with the firm around the acquistion		19 577	102 000
- and changed occupation within worker-firm spell		4 238	22 942
arrived to firm after the acq. (from (t+1) onward)		13 861	31 542
- and changed occ. at the same time		9 538	21 407
balanced acquired firm***		26 087	96 651
unbalanced acquired firm****		30 349	91 097

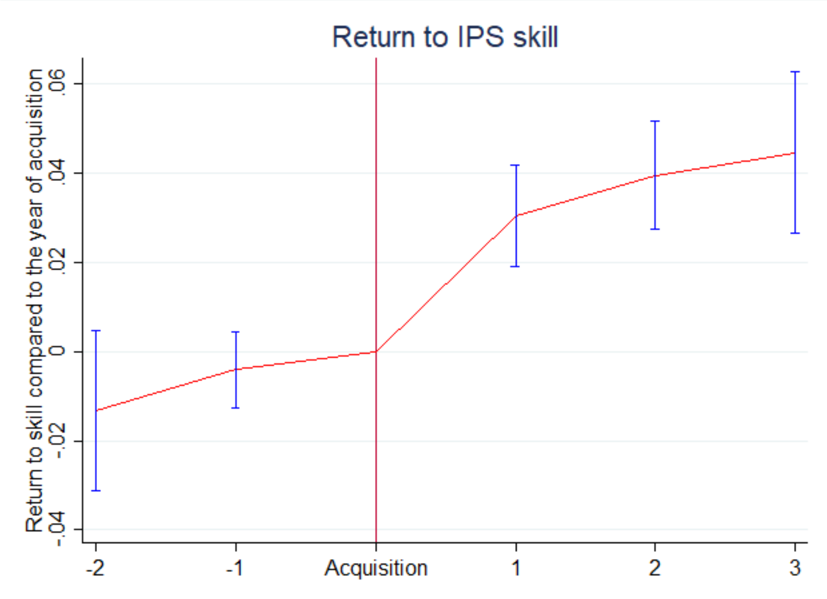
*It could happen in two way: (1) by changing firm (2) the firm changed its status: acquired firms
**Firms that were acquired in 2010 or in 2011 are considered to be acquired firms altough they dont have post-acquisition years in my sample
*** a firm is considered to be balanced firm if it was in my sample from two years before to two years after the acquisition (altogether for 5 years) - by definition is should be acquired between 2005 and 2008
**** unbalanced firms are either acquired in 2004, 2009, 2010, 2011 or do not have 5 consecutive (very small firms).

Table A-4: Return to skills - event study approach

	2way FE	2wayFE	2wayFE
Interaction with the skill index	IPS	Interpersonal	RTI
Acquired (Balanced) * Skill Index	0.00720	0.000261	-0.00379
	(0.00810)	(0.00905)	(0.00642)
<=(t-3) * Skill Index	-0.00977	-0.000256	0.00392
	(0.0104)	(0.00974)	(0.0103)
(t-2) * Skill Index	-0.00513	-0.00129	0.0148
	(0.0115)	(0.00817)	(0.00939)
(t-1) * Skill Index	0.00174	0.00404	0.00536
	(0.00506)	(0.00520)	(0.00470)
(t+1) * Skill Index	0.0231**	0.00876	-0.00500
	(0.00976)	(0.00894)	(0.00908)
(t+2) * Skill Index	0.0297***	0.0178*	-0.00615
	(0.00739)	(0.0103)	(0.00893)
(t+3) * Skill Index	0.0223***	0.0128	-0.00808
	(0.00745)	(0.0116)	(0.0109)
>=(t+4) * Skill Index	0.0253**	0.0255**	-0.0200
	(0.0103)	(0.0118)	(0.0124)
N	5,356,887	5,356,887	5,356,887
R	0.866	0.866	0.866
Worker FE	yes	yes	yes
Firm FE	yes	yes	yes
Sector * year	yes	yes	yes

Standard errors are clustered at firm level, *** p<0.01, ** p<0.05, * p<0.1. Number of observations: 5,356,887, No. indiv.: 940 ,872, Number of firms: 156,906. Controls: age and its square, whether the person recived disablity allowance, wether the person recieved care allowance, tenure and whether the observation is censored, mathematics skill requirement, post acquiaition dummy, skill-year interaction terms, interaction of the skill measure with ownership dummies.

Figure A-1: Return to IPS skill - event study approach - sample of balanced acquired firm



Standard errors are clustered at firm level. 90% confidence intervals are presented.. Number of observations 96651. Controls: age and its square, whether the worker receive care allowance, whether the worker recieve disability payment, tenure and whether the observation is censored, mathematics skill requirement. Firm and worker fixed effects are included.

Table A-5: Wage premia of IPS skill intensive occupations

	Parameters	St. errors
IPS skill intensive occ.	0.0808**	(0.0360)
<=(t-3) * skill intensive occ.	-0.0268	(0.0282)
(t-2) * skill intensive occ.	0.0201	(0.0157)
(t-1) * skill intensive occ.	0.0111	(0.0106)
(t+1) * skill intensive occ.	0.0411***	(0.0108)
(t+2) * skill intensive occ.	0.0650***	(0.0161)
(t+3) * skill intensive occ.	0.0938***	(0.0280)
>=(t+4) * skill intensive occ.	0.0821**	(0.0354)
N	96,651	
Rsquare	0.240	
Firm FE	yes	

Standard errors are clustered at firm level, *** p<0.01, ** p<0.05, * p<0.1. Number of observations 96,651 , number of firms 492. Controls: Controls: age and its square,gender, education, whether the person recieved disability allowance, whether the person recieved care allowance, tenure and whether the observation is censored, mathematics skill requirement.

Table A-6: The change in the return to IPS skills after a foreign takeover by worker and firm characteristics

	OLS	FirmFE	2WayFE
Panel A - gender			
IPS * PostAcq	0.0411**	0.0316***	0.0322***
	(0.0184)	(0.0101)	(0.00812)
IPS * PostAcq. * male	0.00789	0.00724	0.0132*
	(0.0240)	(0.0107)	(0.00755)
Rsquare	0.383	0.250	0.866
N	5,356,887	5,356,887	5,356,887
Panel B - education			
IPS * PostAcq.	0.0360***	0.0304***	0.0389***
	(0.0134)	(0.00863)	(0.00600)
IPS * PostAcq. * low	0.0264	0.0214	0.0312*
	(0.0372)	(0.0225)	(0.0178)
Rsquare	0.376	0.223	0.866
N	5,356,887	5,356,887	5,356,887
Panel C - size			
IPS * PostAcq * small	0.0614***	0.0479***	0.0414***
	(0.0154)	(0.0107)	(0.00993)
IPS * PostAcq * large	0.0413**	0.0299***	0.0366***
	(0.0161)	(0.00955)	(0.00851)
Rsquare	0.411	0.252	0.866
N	5,356,887	5,356,887	5,356,887
Panel D - industry			
IPS * PostAcq * manufacture	0.0368**	0.0216*	0.0371***
	(0.0187)	(0.0111)	(0.00514)
IPS * PostAcq * service	0.0580**	0.0510***	0.0521***
	(0.0244)	(0.0116)	(0.0135)
Rsquare	0.384	0.248	0.866
N	5,356,887	5,356,887	5,356,887
Worker FE			yes
Firm FE		yes	yes
Sector * Year	yes	yes	yes

Standard errors are clustered at firm level, *** p<0.01, ** p<0.05, * p<0.1. No. Observ.: 5,356,887, Number of individuals: 940 ,872, Number of firms: 156,906. Controls: age and its square,gender, education, whether the person recived disablity allowance, wether the person recieved care allowance, tenure and whether the observation is censored, mathematics skill requirement, ownership dummies (always foreigner, acquired and post acquiaition period). In panel I further control for the full set of interaction terms of gender, ownership status and IPS skill index. In panel B I further control for the full set of interaction terms with low education status, ownership and IPS skill measure. In panel C small firms are firms with less than 20 employees, large firms are firms with more than 20 employees. I also control for the interaction terms with ownership status and size, ownership status, size and skill measures. In panel D I further control for the interaction terms of industry dummies with ownership status and for the interaction with ownership status and IPS skill index.

A Time-invariant firm level controls are excluded as firm FE is added to the regression. Time-invariant worker level controls (gender and education) are excluded from two-way fixed model.

Table A-7: The change in the return to IPS skills after a foreign takeover for stayers and for newcomers

	OLS	firmFE	2wayFE
	Panel A - IPS		
Skill*PostAcq	0.0383***	0.0376***	0.0440***
	(0.0123)	(0.00829)	(0.00870)
Skill*PostAcq*Newcomer	0.0170	-0.00208	-0.0124
	(0.0171)	(0.0128)	(0.00829)
N	5,356,887	5,356,887	5,356,887
Rsquare	0.381	0.248	0.866
	Panel B - Interpersonal		
Skill*PostAcq	0.0226	0.0214**	0.0218***
	(0.0165)	(0.00883)	(0.00750)
Skill*PostAcq*Newcomer	-0.0135	0.00504	-0.00270
	(0.0225)	(0.0169)	(0.00807)
N	5,356,887	5,356,887	5,356,887
Rsquare	0.356	0.236	0.866
	Panel C - RTI		
Skill*PostAcq	-0.0444***	-0.0221***	-0.0189***
	(0.0134)	(0.00754)	(0.00630)
Skill*PostAcq*Newcommer	0.00570	0.0108	0.0152**
	(0.0170)	(0.0123)	(0.00639)
N	5,356,887	5,356,887	5,356,887
	0.343	0.226	0.866
Worker FE			yes
Firm FE		yes	yes
Sector * Year	yes	yes	yes

Standard errors are clustered at firm level, *** p<0.01, ** p<0.05, * p<0.1. Number of observ.: 5,356,887, Number of individuals: 940,872, Number of firms: 156,906. Controls: age and its square,gender, education, whether the person recieved disability allowance, whether the person recieved care allowance, tenure and whether the observation is censored, mathematics skill requirement, ownership dummies (always foreigner, acquired and post acquiaition period), the interaction term of post-acquisition perdiod and newcomer, the interaction terms of the skill measures and the ownership dummies. Time-invariant firm level controls are excluded as firm FE is added to the regression. Time-invariant worker level controls (gender and education) are excluded from two-way fixed model. Newcomer is a dummy showing whether the worker arrived to the firm in the post-acquiistion perdiod.

Table A-8: Return to interpersonal skills

	OLS	firm FE	2WayFE
Interpersonal	0.0474***	0.0432***	0.0134***
	(0.00416)	(0.00171)	(0.00107)
Int.p.* Always For.	0.148***	0.102***	0.0375***
	(0.00881)	(0.00589)	(0.00364)
Int.p. * Acquired	0.0230*	0.0302***	0.00572
	(0.0139)	(0.0116)	(0.00484)
Int.p. * PostAcq.	0.0150	0.0236***	0.0209***
	(0.0162)	(0.00785)	(0.00650)
Rsquare	0.356	0.236	0.866
N	5,356,887	5,356,887	5,356,887
Worker FE			yes
Firm FE		yes	yes
Sector * Year	yes	yes	yes

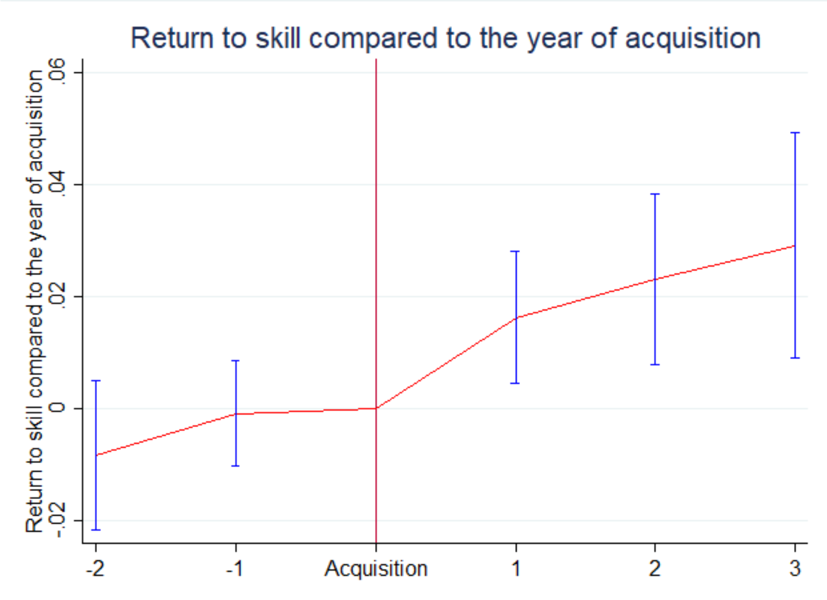
Standard errors are clustered at firm level, *** p<0.01, ** p<0.05, * p<0.1. Number of observations: 5,356,887, Number of individuals: 940,872, Number of firms: 156,906. Controls: age and its square,gender, education, whether the person recieved disability allowance, whether the person recieved care allowance, tenure and whether the observation is censored, mathematics skill requirement, ownership dummies (always foreigner, acquired and post acquiaition period). Time-invariant firm level controls are excluded as firm FE is added to the regression. Time-invariant worker level controls (gender and education) are excluded from two-way fixed model.

Table A-9: Return to RTI

	OLS	firm FE	2WayFE
RTI	-0.0106***	-0.0167***	-0.00899***
	(0.00283)	(0.00151)	(0.000969)
RTI*foreign not acq	-0.136***	-0.100***	-0.0329***
	(0.0102)	(0.00563)	(0.00383)
RTI* acquired	-0.0164	-0.0243***	-0.000157
	(0.0141)	(0.00617)	(0.00372)
RTI*PostAcq.	-0.0445***	-0.0172***	-0.0140**
	(0.0149)	(0.00640)	(0.00546)
Rsquare	0.343	0.226	0.866
N	5,356,887	5,356,887	5,356,887
Worker FE			yes
Firm FE		yes	yes
Sector * Year	yes	yes	yes

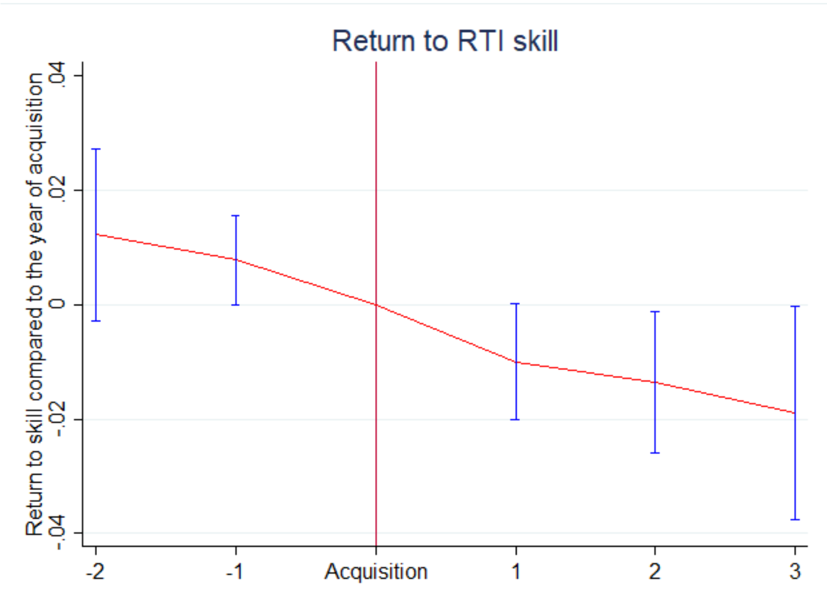
Standard errors are clustered at firm level, *** p<0.01, ** p<0.05, * p<0.1. Number of observations: 5,356,887, Number of individuals: 940,872, Number of firms: 156,906. Controls: age and its square,gender, education, whether the person recieve disability allowance, whether the person recieve care allowance, tenure and whether the observation is censored, mathematics skill requirement, ownership dummies (always foreigner, acquired and post acquiaition period). Time-invariant firm level controls are excluded as firm FE is added to the regression. Time-invariant worker level controls (gender and education) are excluded from two-way fixed model.

Figure A-2: Return to Interpersonal skills - event study approach - sample of balanced acquired firm



Standard errors are clustered at firm level. 90% confidence intervals are presented. Number of observations 96651. Controls: age and its square, whether the worker receive care allowance, whether the worker recieve disability payment, tenure and whether the observation is censored, mathematics skill requirement. Firm and worker fixed effects are included.

Figure A-3: Return to RTI skills - event study approach - sample of balanced acquired firm



Standard errors are clustered at firm level. 90% confidence intervals are presented. Number of observations 96651. Controls: age and its square, whether the worker receive care allowance, whether the worker recieve disability payment, tenure and whether the observation is censored, mathematics skill requirement. Firm and worker fixed effects are included.

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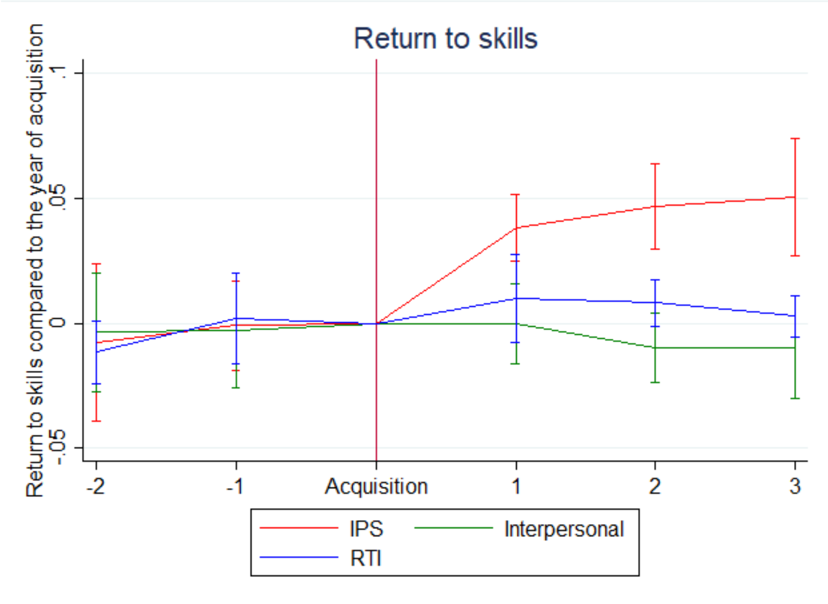
Table A-10: Return to skills - event study approach

	2way FE	
Interaction with the skill index	parametes	st. errors
Acquired (Balanced) * IPS	0.00697	(0.0101)
<=(t-3) * IPS	-0.0138	(0.0140)
(t-2) * IPS	-0.000696	(0.0186)
(t-1) * IPS	0.00239	(0.00981)
(t+1) * IPS	0.0359***	(0.0103)
(t+2) * IPS	0.0389***	(0.0108)
(t+3) * IPS	0.0312***	(0.0118)
>=(t+4) * IPS	0.0191	(0.0142)
Acquired (Balanced) * Interp.	-0.00168	(0.0103)
<=(t-3) * Interp.	0.00754	(0.0127)
(t-2) * Interp.	8.79e-05	(0.0130)
(t-1) * Interp.	0.00233	(0.00854)
(t+1) * Interp.	-0.0160**	(0.00728)
(t+2) * Interp.	-0.00928	(0.0129)
(t+3) * Interp.	-0.0111	(0.0141)
>=(t+4) * Interp.	0.00698	(0.0144)
Acquired (Balanced) * RTI	-0.00381	(0.00555)
<=(t-3) * RTI	0.00328	(0.0107)
(t-2) * RTI	0.0148	(0.0106)
(t-1) * RTI	0.00730	(0.00553)
(t+1) * RTI	0.00544	(0.00663)
(t+2) * RTI	0.00700	(0.00695)
(t+3) * RTI	0.00280	(0.00915)
>=(t+4) * RTI	-0.00476	(0.0116)
N	5,356,887	
R	0.866	
Worker FE	yes	
Firm FE	yes	
Sector * year	yes	

Standard errors are clustered at firm level, *** p<0.01, ** p<0.05, * p<0.1. Number of observ.: 5,356,887, Number of individuals: 940 ,872, Number of firms: 156,906. Controls: age and its square, whether the person recived disablity allowance, wether the person recieved care allowance, tenure and whether the observation is censored, mathematics skill requirement, post acquiaition dummy, skill-year interaction terms, interaction of the skill measure with ownership dummies.

Figure A-4: Return to skills - event study approach - sample of balanced acquired firm



Standard errors are clustered at firm level. 90% confidence intervals are presented. Number of observations 96651. Controls: age and its square, whether the worker receive care allowance, whether the worker recieve disability payment, tenure and whether the observation is censored, mathematics skill requirement. Firm and worker fixed effects are included.

Table A-11: Robustness - using different weighting methods

	Anderson, 2008			Unweighted		
	IPS	Interp.	All three	IPS	Interp.	All three
IPS*PostAcq	0.0392***		0.0392***	0.0410***		0.0467***
	(0.00781)		(0.00969)	(0.00742)		(0.00954)
Interp. PostAcq		0.0217***	-0.000413		0.0214***	-0.00784
		(0.00619)	(0.00646)		(0.00631)	(0.00670)
RTI * PostAcq			-0.000753			0.000469
			(0.00423)			(0.00435)
N	5,356,887	5,356,887	5,356,887	5,356,887	5,356,887	5,356,887
Rsquare	0.866	0.866	0.866	0.866	0.866	0.866
worker FE	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
sector*year	yes	yes	yes	yes	yes	yes

Standard errors are clustered at firm level, *** p<0.01, ** p<0.05, * p<0.1. Number ofobservations: 5,356,887, Number of individuals: 940,872, Number of firms: 156,906. Controls: age and its square,gender, education, whether the person recieved disability allowance and whether the person recieved care allowance, tenure and whether the observation is censored, mathematics skill requirement, skill measures and their interaction with the ownership dummies.

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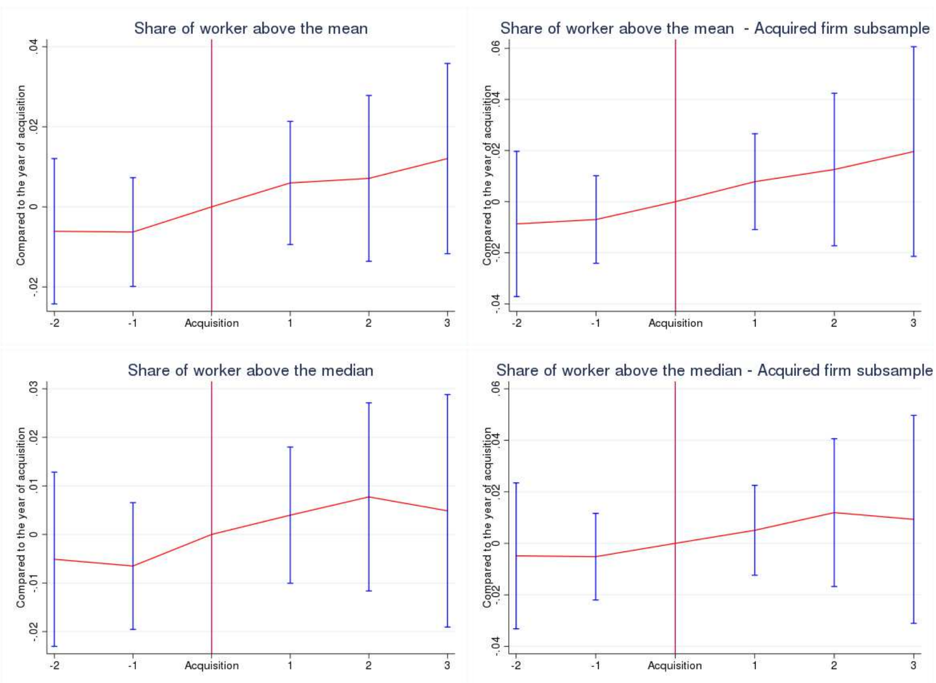
Table A-12: Composition effect- share of high IPS skilled workers

	Average	Above the Mean	Top 50%	Top 25%
		Panel A - IPS		
PostAcq.	0.0325**	0.0192**	0.0175**	0.00980
	(0.0152)	(0.00774)	(0.00735)	(0.00820)
Rsquare	0.055	0.031	0.034	0.045
N	155,867	155,867	155,867	155,867
		Panel A - Interpersonal skill		
PostAcq.	0.0128	0.00527	0.00529	0.0115
	(0.0152)	(0.00735)	(0.00694)	(0.00771)
Rsquare	0.038	0.037	0.038	0.019
N	155,867	155,867	155,867	155,867
		Panel B - RTI		
PostAcq.	-0.0112	-0.00646	-0.00281	-0.00350
	(0.0188)	(0.00786)	(0.00769)	(0.00623)
Rsquare	0.006	0.023	0.021	0.004
N	155,867	155,867	155,867	155,867
Firm Size	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes

Standard errors are clustered at firm level, *** p<0.01, ** p<0.05, * p<0.1. Number of observations: 155,867, Number of firms: 27,345. Controls: firm size, sector-year interaction and firm fixed effects.

Figure A-5: Composition effect around the acquisition



Standard errors are clustered at firm level. 95% confidence intervals are presented. Left figures: number of observations: 156,898, number of firms: 27,447, right figure: number of observations: 5,110, number of firm: 792. Controls: sector-year interaction and firm fixed effects..

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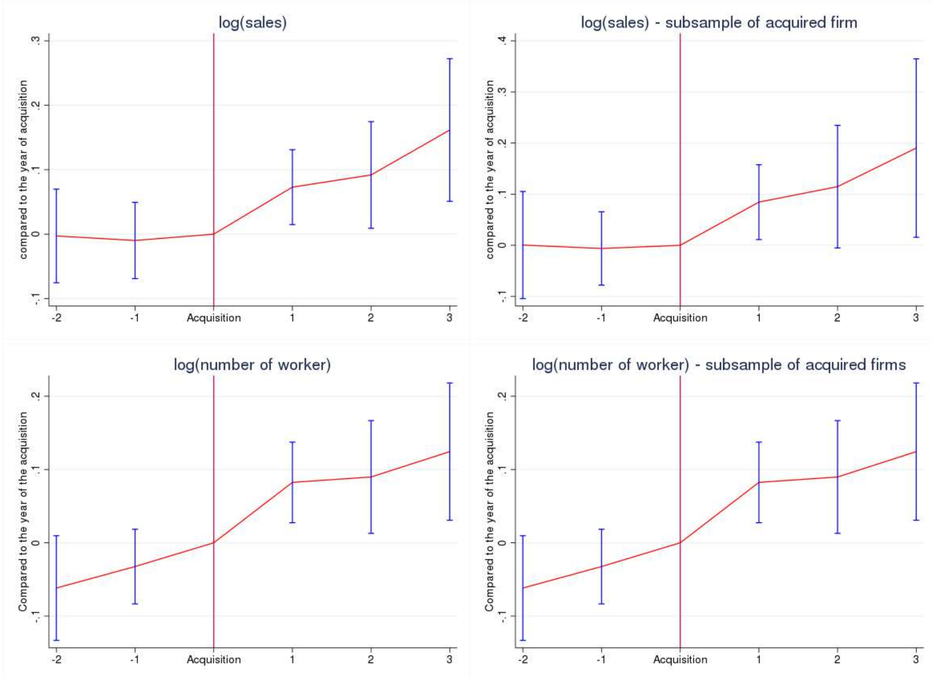
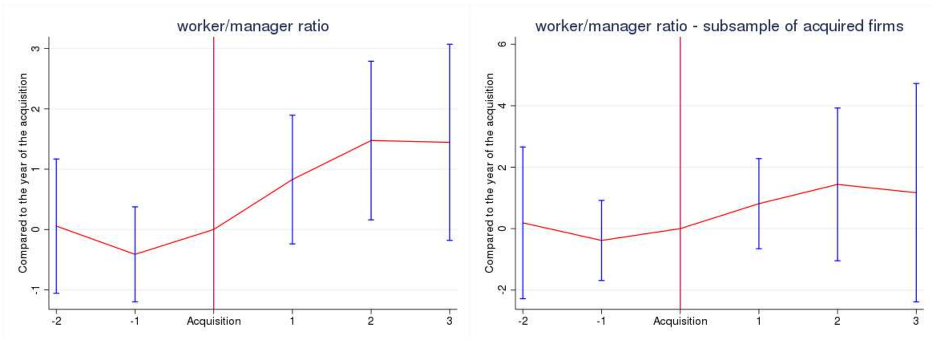
Figure A-6: Foreign take-over and the performace of the firm



Standard errors are clustered at firm level. 95% confidence intervals are presented. Number of observations: 1st figure: 152,165, 2nd figure: 4992, 3nd figure: 156,898 and 4th figure: 5110. Controls: sector-year interaction and firm fixed effects.

Figure A-7: Worker/Manager ratio around the foreign acquisition



Standard errors are clustered at firm level. 95% confidence intervals are presented. Number of observations: 76,293. Controls: sector-year interaction and firm fixed effects.

Table A-13: General increase in the return to skills

	OLS	Firm FE	2wayFE
Panel A - Original regression - Table 5			
IPS * PostAcq.	0.0469***	0.0367***	0.0403***
	(0.0139)	(0.00873)	(0.00736)
Rsquare	0.381	0.248	0.866
N	5,356,887	5,356,887	5,356,887
Panel B - Math. skills interaction			
IPS * PostAcq.	0.0352	0.0366***	0.0363***
	(0.0301)	(0.00992)	(0.00974)
Math. * PostAcq.	0.0635	0.00201	-0.0112
	(0.110)	(0.0230)	(0.00807)
Rsquare	0.381	0.248	0.866
N	5,356,887	5,356,887	5,356,887
Worker FE			yes
Firm FE		yes	yes
Sector * Year	yes	yes	yes

Standard errors are clustered at firm level, *** p<0.01, ** p<0.05, * p<0.1. Number of observations: 5,356,887, Number of individuals: 940,872,Number of firms: 156,906. Controls: age and its square,gender, education, whether the person recieved disability allowance, wether the person recieved care allowance, tenure and whether the observation is censored, mathematics skill requirement, ownership dummies (always foreigner, acquired and post acquiaition period). Time-invariant firm level controls are excluded as firm FE is added to the regression. Time-invariant worker level controls (gender and education) are excluded from two-way fixed model.

Table A-14: General increase in the return to skills

	Interpersonal skill			RTI		
	OLS	firmFE	2wayFE	OLS	firmFE	2wayFE
	Panel A - Original regression - Table A-8 and A-9					
Skill * PostAcq.	0.0150	0.0236***	0.0209***	-0.0445***	-0.0172***	-0.0140**
	(0.0162)	(0.00785)	(0.00650)	(0.0149)	(0.00640)	(0.00546)
Rsquare	0.356	0.236	0.866	0.343	0.226	0.866
N	5,356,887	5,356,887	5,356,887	5,356,887	5,356,887	5,356,887
	Panel B - Math. skills interaction					
Skill * PostAcq.	0.00445	0.0193***	0.0141**	-0.0361**	-0.0117*	-0.00712
	(0.0187)	(0.00739)	(0.00637)	(0.0154)	(0.00659)	(0.00535)
Math. * PostAcq.	0.106	0.0382*	0.0598***	0.0827	0.0503**	0.0671***
	(0.0755)	(0.0200)	(0.0131)	(0.0675)	(0.0234)	(0.0136)
Rsquare	0.357	0.237	0.866	0.345	0.227	0.866
N	5,356,887	5,356,887	5,356,887	5,356,887	5,356,887	5,356,887
Worker FE			yes			yes
Firm FE		yes	yes		yes	yes
Sector * Year	yes	yes	yes	yes	yes	yes

Standard errors are clustered at firm level, *** p<0.01, ** p<0.05, * p<0.1. Number of observations: 5,356,887, Number of individuals: 940,872, Number of firms: 156,906. Controls: age and its square,gender, education, whether the person recieve disability allowance, whether the person recieve care allowance, tenure and whether the observation is censored, mathematics skill requirement, ownership dummies (always foreigner, acquired and post acquiaition period). Time-invariant firm level controls are excluded as firm FE is added to the regression. Time-invariant worker level controls (gender and education) are excluded from two-way fixed model.

Table A-15: Sumbsample of those who were never manager

	OLS	firmFE	2wayFE
	Panel A - IPS		
IPS	0.140***	0.0894***	0.0229***
	(0.00445)	(0.00247)	(0.00155)
IPS * Always For.	0.132***	0.0898***	0.0329***
	(0.0123)	(0.00629)	(0.00335)
IPS * Acq.	-0.0178	0.00775	-0.00728
	(0.0264)	(0.0124)	(0.00624)
IPS * PostAcq	0.0566***	0.0340***	0.0384***
	(0.0186)	(0.0102)	(0.0100)
Rsuare	0.364	0.147	0.847
N	4,683,889	4,683,889	4,683,889
	Panel B - Interpersonal		
IPS	0.0678***	0.0564***	0.00930***
	(0.00408)	(0.00161)	(0.00110)
IPS * Always For.	0.115***	0.0661***	0.0235***
	(0.0111)	(0.00655)	(0.00302)
IPS * Acq.	-0.00677	0.00720	-0.000773
	(0.0183)	(0.0117)	(0.00502)
PostAcq	0.0104	0.0133	0.0147**
	(0.0192)	(0.00888)	(0.00707)
Rsquare	0.333	0.132	0.847
N	4,683,889	4,683,889	4,683,889
	Panel C - RTI		
IPS	-0.0209***	-0.0175***	-0.00507***
	(0.00280)	(0.00147)	(0.000993)
IPS * Always For.	-0.0827***	-0.0572***	-0.0179***
	(0.0118)	(0.00537)	(0.00290)
IPS * Acq.	0.0177	8.22e-05	0.00335
	(0.0172)	(0.00818)	(0.00362)
PostAcq	-0.0431***	-0.0119*	-0.00571
	(0.0160)	(0.00641)	(0.00575)
Rsquare	0.313	0.117	0.847
N	4,683,889	4,683,889	4,683,889
Worker FE			yes
Firm FE		yes	yes
Sector * Year	yes	yes	yes

Standard errors are clustered at firm level, *** p<0.01, ** p<0.05, * p<0.1. Number of observations: 4,683,889, Number of individuals: 830,083, No. firms: 134,353. Controls: age and its square,gender, education, whether the person is disable and recepient of care allowance, tenure and whether the observation is censored, mathematics skill requirement, ownership dummies (always foreigner, acquired and post acquiaition period). Time-invariant firm level controls are excluded as firm FE is added to the regression. Time-invariant worker level controls (gender and education) are excluded from two-way fixed model.

Table A-16: Time-varying firm level controls

	OLS	firmFE	2wayFE
Panel A - IPS			
IPS * PostAcq.	0.0582***	0.0373***	0.0408***
	(0.0139)	(0.00856)	(0.00774)
Rsquare	0.460	0.251	0.868
N	5,290,513	5,290,513	5,290,513
Panel B - interpersonal			
Interpers. * PostAcq.	0.0312**	0.0244***	0.0216***
	(0.0135)	(0.00776)	(0.00661)
Rsquare	0.442	0.238	0.868
N	5,290,513	5,290,513	5,290,513
Panel C - RTI			
RTI * PostAcq.	-0.0382***	-0.0172***	-0.0139**
	(0.0132)	(0.00643)	(0.00551)
Rsquare	0.433	0.228	0.868
N	5,290,513	5,290,513	5,290,513
Panel D - All indices in one regression			
IPS * PostAcq.	0.0662***	0.0384***	0.0472***
	(0.0174)	(0.0109)	(0.00971)
Interpers. * PostAcq.	-0.0198	-0.00355	-0.00920
	(0.0145)	(0.00771)	(0.00650)
RTI * PostAcq.	-0.0165	-0.00235	0.00067
	(0.0139)	(0.00679)	(0.00434)
Rsquare	0.463	0.256	0.868
N	5,290,513	5,290,513	5,290,513
Worker FE			yes
Firm FE		yes	yes
Sector * Year	yes	yes	yes
Time varying firm level controls	yes	yes	yes

Standard errors are clustered at firm level, *** p<0.01, ** p<0.05, * p<0.1. Number of observations: 5,290,513, Number of individuals: 939,441, Number of firms: 151,692. Controls: age and its square,gender, education, whether the person recieved disability allowance, whether the person recieved care allowance, tenure and whether the observation is censored, mathematics skill requirement, ownership dummies (always foreigner, acquired and post acquiaition period). I further add firm level time-varying controls to the regression: size, revenue, dummy whether the firm is an exporter and a dummy whether the firm invested in the given year. Time-invariant firm level controls are excluded as firm FE is added to the regression. Time-invariant worker level controls (gender and education) are excluded from two-way fixed model.

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Appendix B : for Chapter 2

Table B-1: The construction of skill use indices

Cognitive skill use indices

Index of use of numeracy skills at work
How often - Calculating costs or budgets
How often - Use or calculate fractions or percentages
How often - Use a calculator
How often - Prepare charts graphs or tables
How often - Use simple algebra or formulas
How often - Use advanced math or statistics

Index of use of writing skills at work
How often - Write letters memos or mails
How often - Write articles
How often - Write reports
How often - Fill in forms

Index of use of reading skills at work
How often - Read directions or instructions
How often - Read letters memos or mails
How often - Read newspapers or magazines
How often - Read professional journals or publications
How often - Read books
How often - Read manuals or reference materials
How often - Read financial statements
How often - Read diagrams maps or schematics

Index of use of ICT skills at work
How often - For mail
How often - Work related info
How often - Conduct transactions
How often - Spreadsheets
How often - Real-time discussions

Non-cognitive skill use indices

Index of use of planning skills at work
How often - Planning own activities
How often - Planning others' activities
How often - Organizing own time

Index of use of influencing skills at work
How often - Teaching people
How often – Presentations
How often - Advising people
How often - Planning others' activities
How often - Influencing people
How often - Negotiating with people

Index of learning at work
How often - Learning from co-workers/supervisors
How often - Learning - Learning-by-doing
How often - Learning - Keeping up to date

Index of use of task discretion at work
Work flexibility - Sequence of tasks
Work flexibility - How to do the work
Work flexibility - Speed of work
Work flexibility - Working hours

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Table B-2: Family structure, occupation education and time spent on housework

	(1)		(2)		(3)		(4)	
VARIABLES	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.
Female	6.122***	(0.205)	6.224***	(0.216)	2.973***	(0.391)	1.721	(3.194)
Child	2.348*	(1.199)	0.070	(2.392)	-0.506	(1.787)	2.032	(1.831)
Partner	1.227	(1.702)	3.021**	(1.378)	2.112**	(1.013)	2.444	(2.137)
Partner*Female					3.938***	(0.528)		
Female*Child					2.080**	(1.012)		
Partner*Child					-0.496	(2.336)		
Female*Child*Partner					-1.253	(1.138)		
Armed force			-0.436	(0.928)	-0.901	(1.477)	-5.184	(3.212)
Manager			0.161	(0.921)	0.420	(0.988)	2.739	(1.882)
Professionals			-0.257	(0.906)	-0.322	(0.805)	-0.759	(1.828)
Technicians and ass. prof.			-0.075	(0.937)	-0.961	(0.770)	0.497	(1.637)
Clerks			0.586	(0.927)	-1.043	(0.864)	0.905	(2.228)
Service workrers			3.962***	(1.275)	-0.504	(0.822)	1.679	(1.573)
Skilled agricult			0.413	(0.924)	3.606	(2.579)	12.694	(15.102)
Craft workers			0.010	(0.963)	0.038	(0.918)	1.126	(1.488)
Operators			2.080**	(0.963)	-0.004	(1.020)	1.066	(1.827)
Medium educ.			-0.047	(0.313)	-0.693	(0.619)	1.587	(2.494)
High educ.			-1.700***	(0.330)	-2.637***	(0.599)	2.104	(2.710)
Country FE	yes		yes		yes		yes	
Child interactions					yes			
Partner interactions					yes			
Triple int. (*partner*child)					yes			
Occupation interactions							yes	
Observations	10,011		9,940		9,940		9,940	
R-squared	0.198		0.214		0.220		0.236	

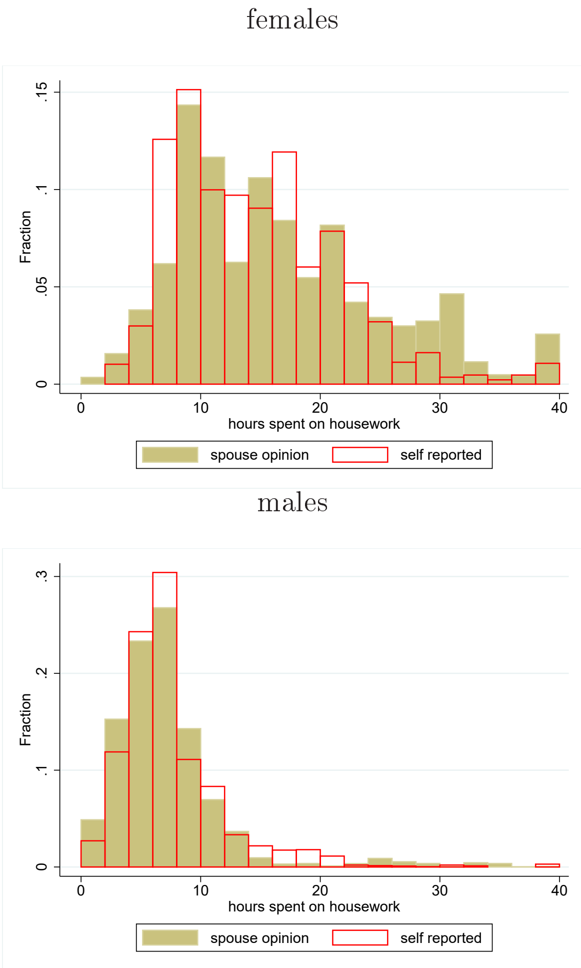
Notes: The dependent variable is hourse spend on housework reported by the worke. Every regression controls for country fixed effects. In the third column we interact all variables with the child and partner dummy and also include all the triple interaction terms (everything is interacted with child*partner). In the fourth column we interact every variable with the occupation dummies. The left out categories are: elementary occupations and low education. Standard errors are in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Table B-3: Family structure, occupation education and time spent on familycare

VARIABLES	(1) coeff.	s.e.	(2) coeff.	s.e.	(6) coeff.	s.e.	(7) coeff.	s.e.
Female	5.348***	(0.330)	5.095***	(0.348)	0.707	(0.434)	7.930***	(2.477)
Child	15.115***	(2.748)	5.220**	(2.370)	3.375	(2.592)	13.543***	(3.369)
Partner	3.340**	(1.389)	4.097***	(1.325)	1.835*	(0.985)	11.699***	(3.485)
Partner*Female					0.804	(0.584)		
Female*Child					9.408***	(1.946)		
Partner*Child					5.089*	(3.074)		
Female*Child*Partner					-0.353	(2.123)		
Armed force			-0.714	(1.851)	-3.106***	(0.678)	-16.611***	(6.115)
Manager			0.104	(1.804)	-0.302	(0.916)	-0.235	(3.157)
Professionals			0.595	(1.789)	-0.251	(0.747)	0.934	(2.927)
Technicians and ass. prof.			0.265	(1.831)	-0.199	(0.669)	3.589	(2.711)
Clerks			1.555	(1.815)	0.065	(0.771)	-0.871	(2.966)
Service workrers			0.060	(2.040)	1.661*	(0.893)	1.772	(2.537)
Skilled agricult			0.482	(1.811)	2.054	(1.823)	2.795	(3.180)
Craft workers			0.692	(1.874)	-0.322	(0.758)	2.030	(2.168)
Operators			1.395	(1.841)	1.026	(0.884)	3.365	(2.371)
Medium educ.			1.320***	(0.488)	0.079	(0.620)	7.658**	(3.803)
High educ.			1.878***	(0.566)	-0.558	(0.675)	8.788*	(5.194)
Country FE	yes		yes		yes		yes	
Child interactions					yes			
Partner interactions					yes			
Triple int. (*partner*child)					yes			
Occupation interaction							yes	
Observations	9,481		9,416		9,416		9,416	
R-squared	0.273		0.274		0.278		0.273	

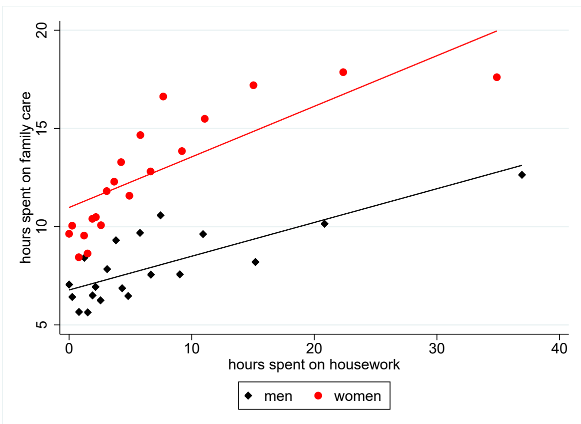
Notes: The dependent variable is hourse spend on family care reported by the worke. Every regression controls for country fixed effects. In the third column we interact all variables with the child and partner dummy and also include all the triple interaction terms (everything is interacted with child*partner). In the fourth column we interact every variable with the occupation dummies. The left out categories are: elementary occupations and low education. Standard errors are in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figure B-1: Self-reported and spouse-reported hours spent on housework (weekly hours)



Notes: The figure shows that the self-reported and spouse-reported hours spent on housework are similar. Single households are omitted and hours spent on housework are winsorized at 40 hours.

Figure B-2: Amount of family care by the hours spent on housework



Notes: The figure shows the average hours spent on family care as the function of hours spent on housework. Both the hours spent on housework and family care are winsorized at 40 hours.

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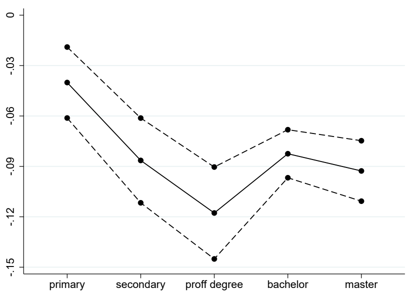
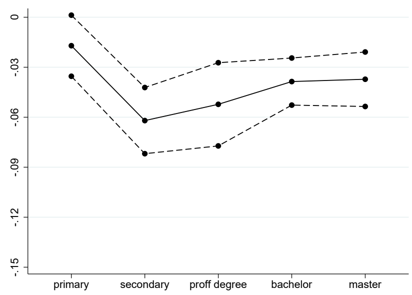
Figure B-3: The gender gap in skill use by educational level

Panel A: Gender gap in numeracy skill use at work

unconditional gap

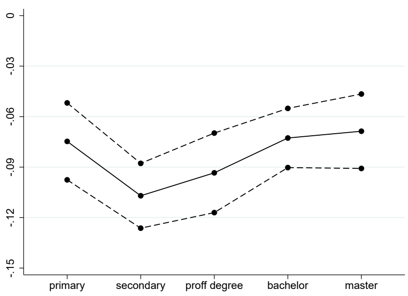


conditional gap

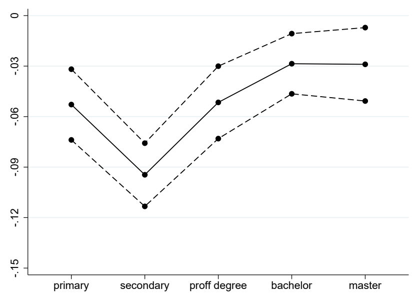


Panel B: Gender gap in literacy skill use at work

unconditional gap

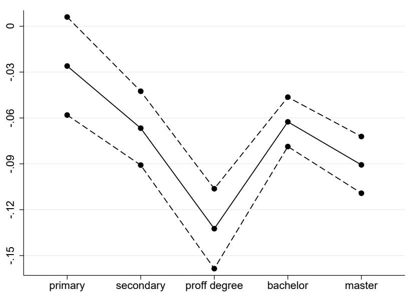


conditional gap

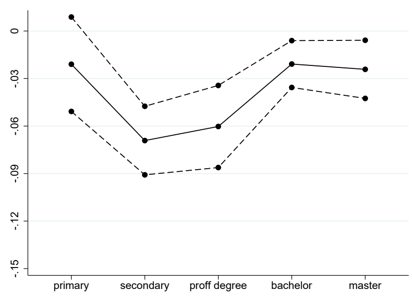


Panel C: Gender gap in ICT skill use at work

unconditional gap



conditional gap



Notes: The figure shows the gender gap in cognitive test scores by educational level. The figures on the left show the raw gap, while the figures on the right use the same control variables as in Table 4 Column (3): partner dummy, child dummy, years of education, experience, experience^2, cognitive test results, occupation categories (ISCO 3-digit), country fixed effects, parents' highest level of education and parents' immigration status, dummy for full time work, self-employment dummy, dummy for having a permanent contract, dummies for 1-digit industry, 5 firm size categories. Standard errors are calculated with the jackknife method (suggested by OECD, 2013) using 80 replication weights. All of the results are calculated by using sampling weights provided by the survey.

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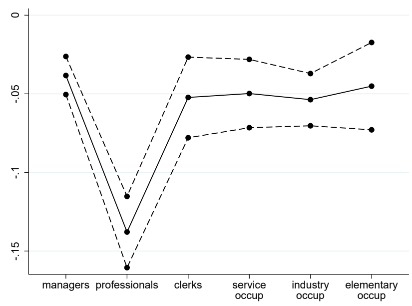
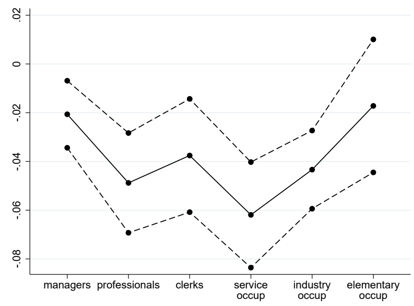
Figure B-4: The gender gap in skill use by occupation groups

Panel A: Gender gap in numeracy skill use at work

unconditional gap

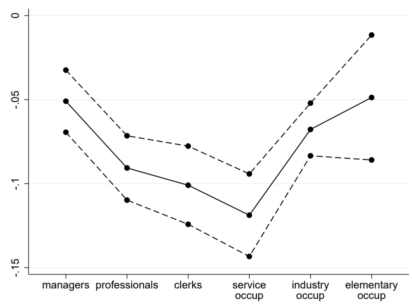


conditional gap

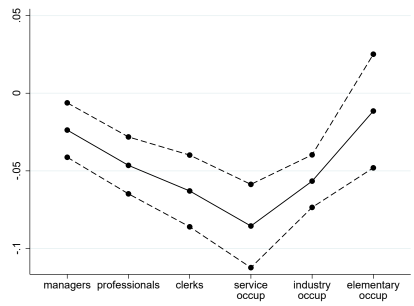


Panel B: Gender gap in literacy skill use at work

unconditional gap

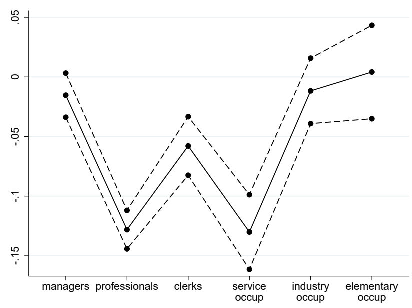


conditional gap

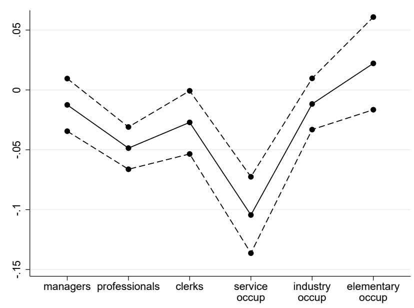


Panel C: Gender gap in ICT skill use at work

unconditional gap



conditional gap



Notes: The figure shows the gender gap in cognitive test scores by occupational categories. The figures on the left show the raw gap, while the figures on the right use the same control variables as in Table 4 Column (3): partner dummy, child dummy, years of education, experience, experience^2, cognitive test results, occupation categories (ISCO 3-digit), country fixed effects, parents' highest level of education and parents' immigration status, dummy for full time work, self-employment dummy, dummy for having a permanent contract, dummies for 1-digit industry, 5 firm size categories. Standard errors are calculated with the jackknife method (suggested by OECD, 2013) using 80 replication weights. All of the results are calculated by using sampling weights provided by the survey.

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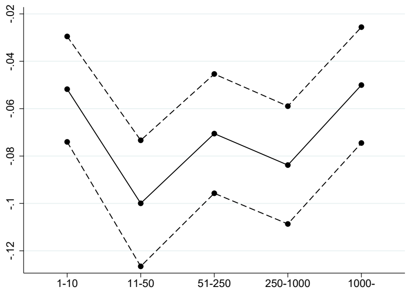
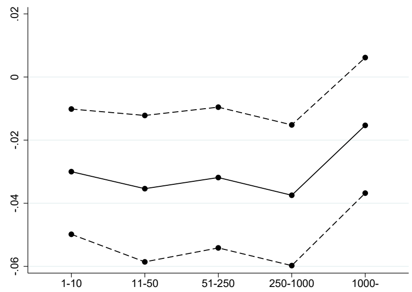
Figure B-5: Gender gap in skill use by firm size

Panel A: Gender gap in numeracy skill use at work

unconditional gap

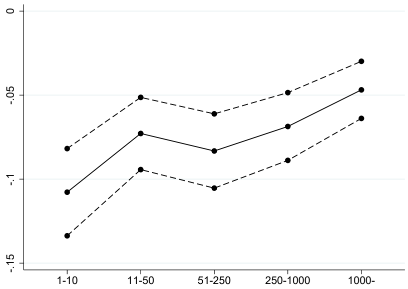


conditional gap

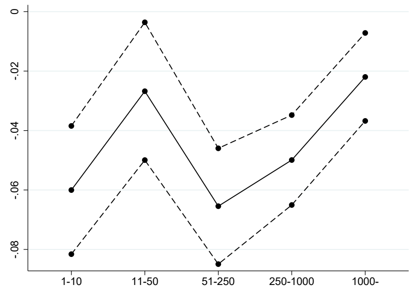


Panel B: Gender gap in literacy skill use at work

unconditional gap

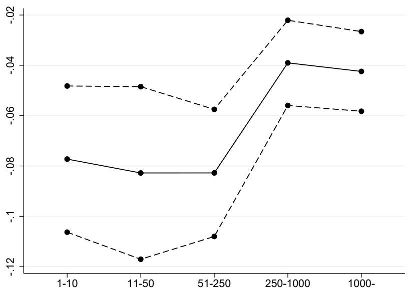


conditional gap

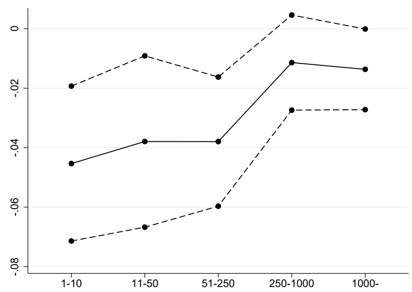


Panel C: Gender gap in ICT skill use at work

unconditional gap



conditional gap



Notes: The figure shows the gender gap in cognitive test scores by firm size categories. The figures on the left show the raw gap, while the figures on the right use the same control variables as in Table 4 Column (3): partner dummy, child dummy, years of education, experience, experience^2, cognitive test results, occupation categories (ISCO 3-digit), country fixed effects, parents' highest level of education and parents' immigration status, dummy for full time work, self-employment dummy, dummy for having a permanent contract, dummies for 1-digit industry, 5 firm size categories, private sector and a set of social skills (categorical variable for cultural engagement, political efficacy, social trust, social trust 2 and health status). Standard errors are calculated with the jackknife method (suggested by OECD, 2013) using 80 replication weights. All of the results are calculated by using sampling weights provided by the survey.

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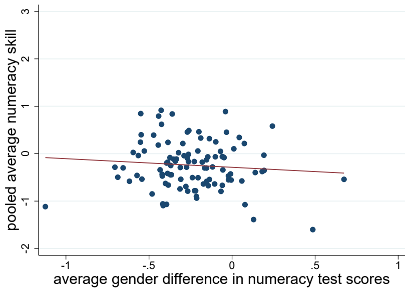
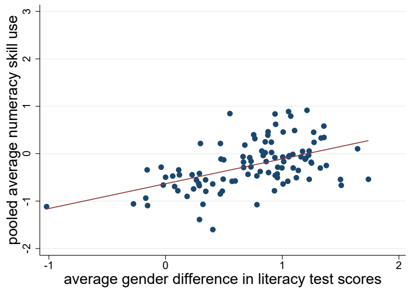
Figure B-6: Average skill use and gender gap test scores by occupations

Panel A: Numeracy skill use at work by gender gap in cognitive skills

numeracy test scores

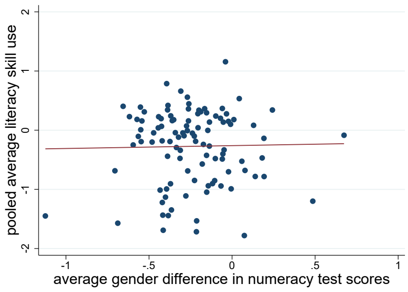


literacy test scores

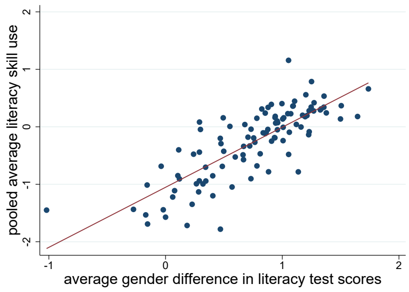


Panel B: Literacy skill use at work by gender gap in cognitive skills

numeracy test scores

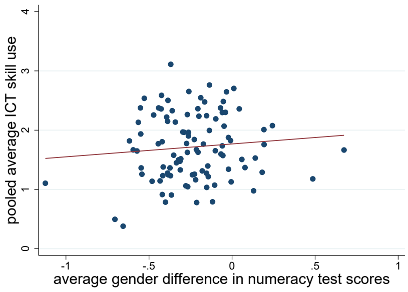


literacy test scores

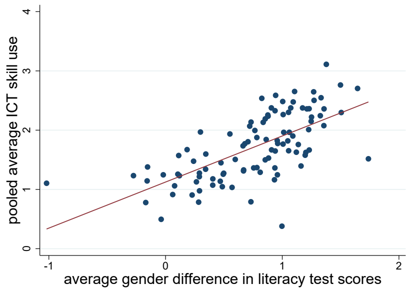


Panel C: ICT skill use at work by gender gap in cognitive skills

Numeracy test scores



Literacy test scores



Notes: The figure shows the average skill use in a given occupation (vertical axis) by the gender gap in cognitive test scores (horizontal axis) in a given occupation. Every dot represents an occupation defined by 3-digit ISCO codes.

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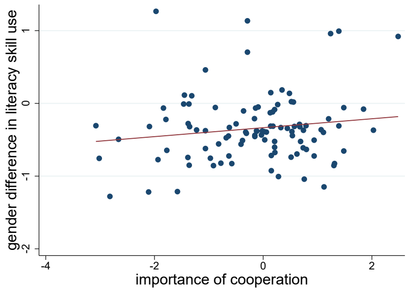
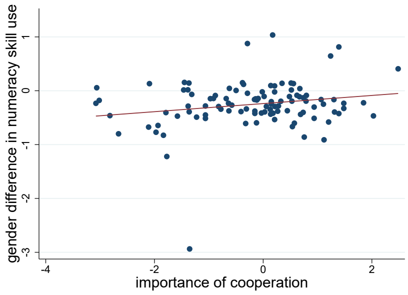
Figure B-7: Non-cognitive skill requirements of the occupation and the gender gap in cognitive skill use

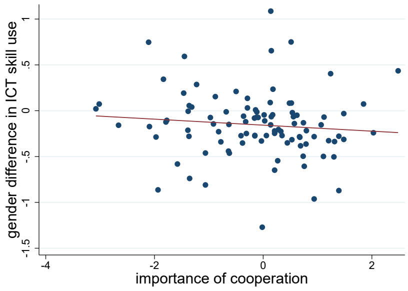
Panel A: Numeracy skill use



Panel B: Literacy skill use



Panel C: ICT skill use



Notes: The figure shows the average gender gap in a given occupation (vertical axis) by importance of cooperation (horizontal axis) in a given occupation. Every dot represents an occupation defined by 3-digit ISCO codes.

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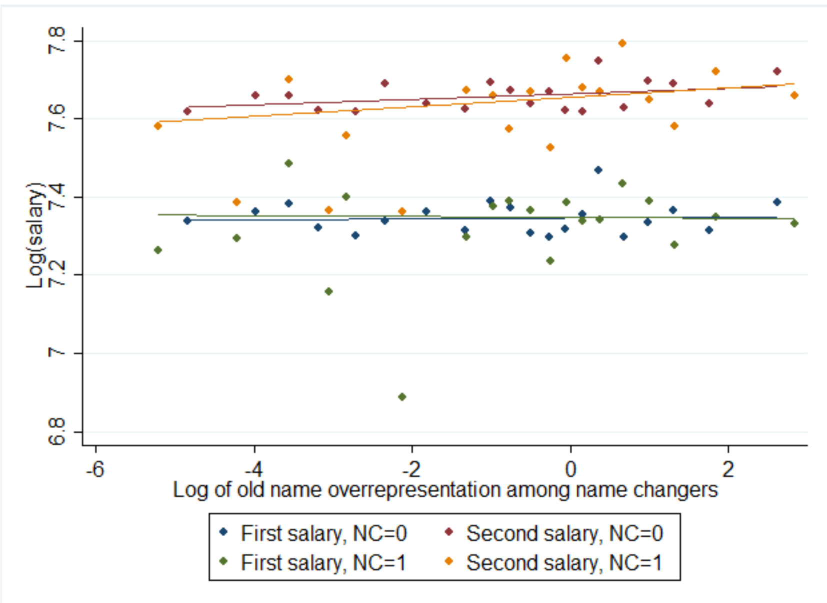
Table B-4: Gender gap in skill use by country

Country	(1) Numeracy		(2) Literacy		(3) ICT	
	coeff.	(s.e.)	coeff.	(s.e.)	coeff.	(s.e.)
Czech Republic	-0.057	(0.078)	-0.239***	(0.064)	-0.021	(0.066)
Denmark	-0.250***	(0.045)	-0.192***	(0.044)	-0.183***	(0.041)
France	-0.176***	(0.036)	-0.185***	(0.030)	-0.030	(0.037)
Great Britain	-0.182***	(0.049)	-0.157***	(0.042)	-0.089*	(0.051)
Germany	-0.154***	(0.053)	-0.219***	(0.044)	-0.066	(0.049)
Japan	-0.217***	(0.039)	-0.207***	(0.045)	-0.205***	(0.048)
Republic of Korea	-0.141***	(0.038)	-0.128***	(0.044)	-0.086*	(0.049)
Norway	-0.350***	(0.041)	-0.282***	(0.040)	-0.195***	(0.035)
Poland	-0.100**	(0.048)	-0.093*	(0.053)	-0.132**	(0.061)
Russia	0.052	(0.080)	-0.048	(0.068)	-0.138***	(0.049)
Slovakia	-0.061	(0.055)	-0.091*	(0.050)	0.022	(0.051)
Spain	-0.158***	(0.051)	-0.252***	(0.051)	-0.247***	(0.054)

Notes: The columns show the gender gap by skill use indices. Every row contains regressions for the given country. Every regression controls for years of education and standardized literacy and numeracy skills, for partner dummy, child dummy, years of education, experience, experience^2, occupation categories (ISCO 3-digit), parents' highest level of education and parents' immigration status, dummy for full time work, self-employment dummy, dummy for having a permanent contract, dummies for 1-digit industry, 5 firm size categories, private sector. Standard errors are calculated with the jackknife method (suggested by OECD, 2013) using 80 replication weights. All of the results are calculated by using sampling weights provided by the survey. Standard errors are in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Appendix C : for Chapter 3

Figure C-1: Salary, IV and the decision to change name - Municipal Employees



Controls are the same as in the main regression: age and its square, experience and its square, whether the individual was found in the education data and his grade, occupation dummies, workplace dummies, Jewish dummy and the year of observation

Table B-5: Non-cognitive skill use at work

	(1)		(2)		(3)	
	coeff.	(s.e.)	coeff.	(s.e.)	coeff.	(s.e.)
Panel A: use of planning skills at work						
Gender gap	-0.154***	(0.015)	-0.119***	(0.016)	-0.033**	(0.016)
Years of education			0.064***	(0.004)	0.019***	(0.004)
Literacy test scores			-0.015	(0.018)	0.020	(0.015)
Numeracy test scores			0.055***	(0.020)	0.042***	(0.016)
Panel B: use of influencing skills at work						
Gender gap	-0.213***	(0.021)	-0.177***	(0.021)	-0.150***	(0.018)
Years of education			0.090***	(0.005)	0.032***	(0.005)
Literacy test scores			-0.051**	(0.020)	-0.037**	(0.018)
Numeracy test scores			0.079***	(0.023)	0.063***	(0.018)
Panel C: use of task discretion at work						
Gender gap	-0.223***	(0.016)	-0.190***	(0.018)	-0.060***	(0.015)
Years of education			0.024***	(0.003)	0.010**	(0.004)
Literacy test scores			-0.005	(0.020)	0.015	(0.016)
Numeracy test scores			0.087***	(0.021)	0.015	(0.017)
Panel C: use of learning skills at work						
Gender gap	-0.079***	(0.017)	-0.059***	(0.019)	-0.077***	(0.015)
Years of education			0.066***	(0.006)	0.032***	(0.005)
Literacy test scores			-0.009	(0.023)	0.025	(0.024)
Numeracy test scores			0.006	(0.026)	-0.004	(0.023)
Additonal controls						Yes

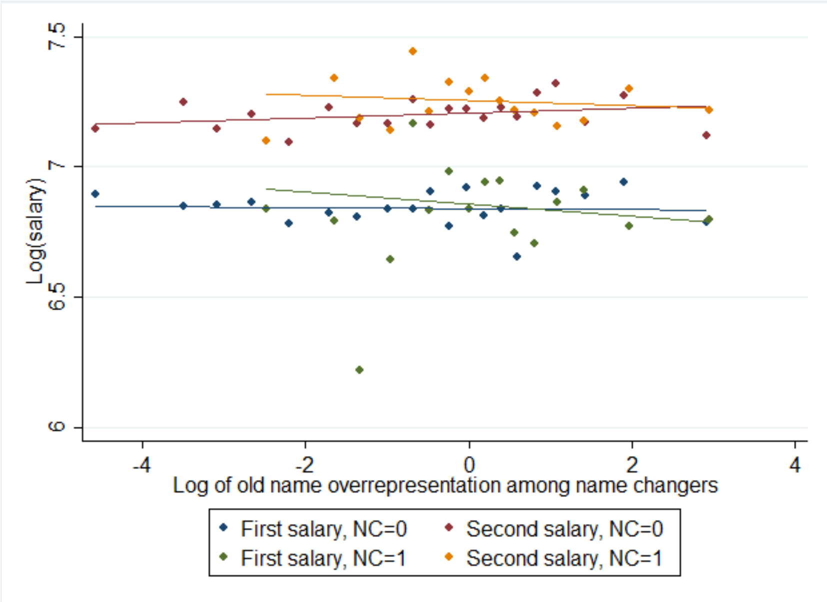
Notes: Control variables differ by column. Column (2) controls for years of education and standardized literacy and numeracy skills. The additional control variables are the same as in Table 4 Column (3): partner dummy, child dummy, years of education, experience, experience^2, cognitive test results, occupation categories (ISCO 3-digit), country fixed effects, parents' highest level of education and parents' immigration status, dummy for full time work, self-employment dummy, dummy for having a permanent contract, dummies for 1-digit industry, 5 firm size categories, private sector and a set of social skills (categorical variable for cultural engagement, political efficacy, social trust, social trust 2 and health status). Standard errors are calculated with the jackknife method (suggested by OECD, 2013) using 80 replication weights. All of the results are calculated by using sampling weights provided by the survey. Standard errors are in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Table B-6: The effect of having family on gender gap

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Numeracy skill use		Literacy skill use		ICT skill use	
Partner	0.087***	0.073***	0.142***	0.132***	0.174***	0.177***
	(0.028)	(0.028)	(0.034)	(0.032)	(0.036)	(0.033)
Female	-0.118***	-0.097***	-0.097***	-0.068**	-0.035	-0.009
	(0.028)	(0.029)	(0.029)	(0.029)	(0.031)	(0.030)
Partner*Female	-0.108***	-0.049	-0.128***	-0.076*	-0.132***	-0.073*
	(0.038)	(0.045)	(0.038)	(0.040)	(0.040)	(0.038)
Child	-0.030	-0.016	-0.083***	-0.077**	-0.065**	-0.051*
	(0.026)	(0.028)	(0.031)	(0.033)	(0.029)	(0.029)
Child*Female	0.073**	0.097***	0.018	0.064	-0.015	0.024
	(0.033)	(0.032)	(0.039)	(0.040)	(0.038)	(0.037)
Workhour		0.009***		0.011***		0.008***
		(0.001)		(0.001)		(0.001)
Housework		-0.005**		-0.006***		-0.008***
		(0.002)		(0.002)		(0.002)
Familycare		0.000		0.002*		-0.001
		(0.001)		(0.001)		(0.001)
Education	yes	yes	yes	yes	yes	yes
Cognitive skill	yes	yes	yes	yes	yes	yes
Job charact.	yes	yes	yes	yes	yes	yes
Observations	30,263	29,938	31,277	30,954	25,931	25,701
R-squared	0.261	0.276	0.320	0.341	0.291	0.305

Notes: Standard errors are in parentheses *** p<0.01, ** p<0.05, * p<0.1. Control variables are years of education, standardized literacy and numeracy skill, experience, experience^2, occupation categories (ISCO 3-digit), country fixed effects, parents' highest level of education and parents' immigration status, dummy for full time work, self-employment dummy, dummy for having a permanent contract, dummies for 1-digit industry, 5 firm size categories, private sector. Standard errors are calculated with the jackknife method (suggested by OECD, 2013) using 80 replication weights. All of the results are calculated by using sampling weights provided by the survey.

Figure C-2: Salary, IV and the decision to change name - Reserve Officers



Control variables are the same as in the main regression: age and its square, years of school and its square, occupation dummies, whether the worker participated in any training outside the school, Jewish dummy, year of observation

Table C-1: t-test - Municipal Employee

	(1)						
	Mean(Non-Changer)	st dev	Mean(Changer)	st dev	Diff.	Std. Error	Obs.
Age	41.93	11.76	41.08	11.41	0.85*	0.50	3702
Experience	13.05	9.94	12.99	9.54	0.06	0.43	3702
High School	0.36	0.48	0.55	0.50	-0.19***	0.02	3702
Jewish	0.03	0.17	0.10	0.30	-0.07***	0.01	3702
GPA=1	0.04	0.19	0.08	0.27	-0.04***	0.01	3702
GPA=2	0.08	0.27	0.11	0.32	-0.04***	0.01	3702
GPA=3	0.09	0.29	0.14	0.35	-0.05***	0.01	3702
GPA=4	0.02	0.16	0.05	0.21	-0.02***	0.01	3702
GPA=5	0.00	0.03	0.00	0.04	-0.00	0.00	3702

10.14754/CEU.2019.07

Table C-2: t-test - Municipal Employee - restricted sample

	(1)						
	Mean(Changer)	st dev	Mean(Non-Changer)	st dev	Diff.	Std. Error	Obs.
Age	41.76	11.55	40.70	11.53	1.06*	0.58	2061
Experience	12.47	9.96	12.74	9.54	-0.27	0.50	2061
High School	0.42	0.49	0.58	0.49	-0.16***	0.02	2061
Jewish	0.05	0.21	0.12	0.33	-0.07***	0.01	2061
GPA=1	0.04	0.20	0.09	0.28	-0.04***	0.01	2061
GPA=2	0.09	0.29	0.12	0.32	-0.03*	0.02	2061
GPA=3	0.11	0.31	0.14	0.35	-0.04**	0.02	2061
GPA=4	0.03	0.17	0.04	0.20	-0.01	0.01	2061
GPA=5	0.00	0.04	0.00	0.00	0.00	0.00	2061

Restricted sample is the sample of those workers for whom the instrument is defined.

Table C-3: t-test - Reserve Officers

	(1)						
	Mean(Non-Changer)	st dev	Mean(Changer)	st dev	Diff.	Std. Error	Obs
Age	31.76	4.18	31.97	2.43	-0.21	0.27	247
Years of School	14.41	2.64	14.38	2.67	0.03	0.18	247
Training	0.08	0.27	0.11	0.32	-0.04*	0.02	247
Clerical occ.	0.47	0.50	0.61	0.49	-0.14***	0.03	247
Jew	0.15	0.36	0.55	0.50	-0.40***	0.03	247

Table C-4: t-test - Reserve Officers - restricted sample

	(1)						
	Mean(Non-Changer)	st dev	Mean(Changer)	st dev	Diff.	Std. Error	Obs
Age	31.73	3.71	31.99	2.42	-0.25	0.26	137
Years of School	14.33	2.65	14.38	2.67	-0.05	0.19	137
Training	0.09	0.29	0.12	0.32	-0.03	0.02	137
Clerical occ.	0.47	0.50	0.62	0.49	-0.15***	0.04	137
Jew	0.25	0.43	0.58	0.50	-0.33***	0.03	137

Restricted sample is the sample of those workers for whom the instrument is defined.

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Table C-5: Regression results - Municipal Employees

	(1) log(salary)	(2) log(salary)	(3) log(salary)	(4) Changer	(5) log(salary)	(6) log(salary)
Changer	0.0276**	0.0400***	0.0580**			
	(0.0125)	(0.0150)	(0.0255)			
Age	0.0165***	0.0147	0.0149	-0.0102**	0.0160***	0.0139
	(0.00539)	(0.00945)	(0.00928)	(0.00507)	(0.00529)	(0.00920)
Sq. of age	-0.0180***	-0.0174	-0.0176	0.00927*	-0.0177***	-0.0169
	(0.00649)	(0.0115)	(0.0113)	(0.00548)	(0.00635)	(0.0112)
Experience	0.0198***	0.0210***	0.0207***	0.00918***	0.0197***	0.0207***
	(0.00221)	(0.00299)	(0.00295)	(0.00307)	(0.00220)	(0.00296)
Sq. of exp/100	-0.00683	-0.0113	-0.0107	-0.0175**	-0.00649	-0.0106
	(0.00689)	(0.0102)	(0.0101)	(0.00792)	(0.00684)	(0.0100)
Jewish * High School	-0.0555	-0.0592	-0.0624	0.0242	-0.0495	-0.0509
	(0.0358)	(0.0419)	(0.0420)	(0.0419)	(0.0357)	(0.0418)
iv				0.107***		
				(0.00372)		
changed before 1898					0.0763***	0.0900***
					(0.0211)	(0.0250)
changed in 1898					0.0355	0.0769**
					(0.0251)	(0.0309)
changed after 1899					-0.0199	-0.0159
					(0.0157)	(0.0182)
Constant	7.022***	7.146***	7.138***	0.415***	7.035***	7.170***
	(0.122)	(0.201)	(0.198)	(0.126)	(0.120)	(0.197)
Year	Yes	Yes	Yes	Yes	Yes	Yes
Occupation	Yes	Yes	Yes	Yes	Yes	Yes
Workplace	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3702	2061	2061	2061	3702	2061

Robust standard errors are in the parenthesis. *** p<0.01, ** p<0.05, * p<0.1.Control variables: high school dummy, GPA scores. Restricted sample is the sample of those workers for whom the instrument is defined.

Table C-6: Regression results - Reserve Officers

	(1) log(salary)	(2) log(salary)	(3) log(salary)	(4) changer	(5) log(salary)	(6) log(salary)
changer	0.0485*	0.0544*	0.142*			
	(0.0283)	(0.0306)	(0.0823)			
Age	0.00961***	0.00504	0.00350	0.0104*	0.00959***	0.00497
	(0.00211)	(0.0222)	(0.0216)	(0.00540)	(0.00211)	(0.0222)
Square of Age	0.00589	0.0158	0.0180	-0.0112	0.00586	0.0158
	(0.00465)	(0.0342)	(0.0334)	(0.00804)	(0.00465)	(0.0343)
Years of School	0.00932	0.0340	0.0336	0.00725	0.00886	0.0331
	(0.0338)	(0.0441)	(0.0430)	(0.0377)	(0.0339)	(0.0446)
Sq of yrs of school	0.00720	-0.100	-0.0997	-0.0187	0.00859	-0.0985
	(0.123)	(0.162)	(0.158)	(0.137)	(0.124)	(0.163)
Training	-0.0398	-0.0487	-0.0478	-0.00830	-0.0407	-0.0521
	(0.0259)	(0.0350)	(0.0345)	(0.0368)	(0.0259)	(0.0350)
jew	0.0615**	0.0583*	0.0393	0.110***	0.0612**	0.0590**
	(0.0254)	(0.0298)	(0.0338)	(0.0264)	(0.0254)	(0.0298)
IV				0.0666***		
				(0.00450)		
changed before 1898					0.0666*	0.0729**
					(0.0347)	(0.0367)
changed in 1898					0.0676	0.122*
					(0.0658)	(0.0647)
changed after 1899					0.00193	-0.0171
					(0.0518)	(0.0565)
Constant	6.748***	6.576***	6.888***	-0.168	6.760***	6.587***
	(0.315)	(0.498)	(0.483)	(0.286)	(0.314)	(0.501)
Year	Yes	Yes	Yes	Yes	Yes	Yes
Occupation	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2477	1372	1372	1372	2477	1372

Robust standard errors are in the parenthesis. *** p<0.01, ** p<0.05, * p<0.1.
Control variables: occupation dummies, year fixed effects. Restricted sample is the sample of those workers for whom the instrument is defined.

Table C-7: IV and observable characteristics of the worker - Municipal Employees

	(1) IV	(2) IV	(3) IV	(4) IV	(5) IV	(6) IV
High school	-0.0867	-0.115	-0.137	-0.202	-0.442**	-0.492***
	(0.224)	(0.141)	(0.223)	(0.143)	(0.221)	(0.143)
No mental score	0.0974	0.125	0.0761	0.142	0.241	0.291*
	(0.249)	(0.178)	(0.246)	(0.176)	(0.242)	(0.172)
GPA=2	-0.0432		-0.0991		-0.0726	
	(0.266)		(0.265)		(0.255)	
GPA=3	-0.198		-0.249		-0.125	
	(0.263)		(0.261)		(0.253)	
GPA=4	-0.587*		-0.479		-0.392	
	(0.346)		(0.346)		(0.340)	
GPA=5	-2.668***		-3.103***		-2.651***	
	(0.544)		(0.737)		(0.707)	
GPA>=3		-0.281		-0.264		-0.161
		(0.183)		(0.183)		(0.175)
High sch. * Jewish					1.692***	1.703***
					(0.182)	(0.183)
Constant	-0.608*	-0.618*	1.420**	1.445**	0.965	0.992
	(0.329)	(0.327)	(0.685)	(0.681)	(0.700)	(0.696)
Year	Yes	Yes	Yes	Yes	Yes	Yes
Occupation	No	No	Yes	Yes	Yes	Yes
Workplace	No	No	Yes	Yes	Yes	Yes
Observations	2061	2061	2061	2061	2061	2061

Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.001. In column 3-6 we control for age and ist square, experience and its square.

Table C-8: IV and observable characteristics of the workers - Reserve Officers

	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
Years of school	-0.249			-0.260	-0.0881
	(0.227)			(0.227)	(0.203)
Square of yrs of school	0.00914			0.00980	0.00296
	(0.00827)			(0.00828)	(0.00748)
Training		0.0498		0.0441	0.00683
		(0.204)		(0.216)	(0.195)
Clerical occupation			0.102	0.107	0.134
			(0.118)	(0.125)	(0.118)
Jewish					1.669***
					(0.110)
Constant	0.738	-0.887	-0.921	-0.747	-1.504
	(1.706)	(0.810)	(0.805)	(2.007)	(1.803)
Year	Yes	Yes	Yes	Yes	Yes
Observations	1331	1331	1331	1331	1331

Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.001. In column 4-5 we control for age and its square

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