Institut des Politiques Publiques

RAPPORT IPP N° – 26

EDUCATION, SKILLS AND SKILL MISMATCH: A REVIEW AND SOME NEW EVIDENCE BASED ON THE PIAAC SURVEY

Kentaro Asai
Thomas Breda
Audrey Rain
Lucile Romanello
Marc Sangnier

Institut des Politiques Publiques

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Groupe des écoles nationales d'économie et statistique

Authors of the report

Kentaro Asai is a second year master student at Paris School of Economics. He is doing a master thesis under the supervision of Thomas Breda on skill inequality and contributed to the literature review presented in this report during a summer internship at the Institut des politiques publiques.

Thomas Breda is a junior researcher at the Centre National de la Recherche Scientifique and associate professor at Paris school of Economics. He is also director of the Programme "Employment and Labor" at the Institut des politiques publiques. His research focuses on the labor market, with a particular focus on employment relations, discriminations, and gender inequalities.
Webpage: www.parisschoolofeconomics.com/breda-thomas/

Audrey Rain is a junior researcher at the Institut des Politiques Publiques. She holds a PhD in economics from Paris II Panthéon-Assas. She joined the IPP in 2018 and her research focuses on educational, vocational training and labor market policies.

Lucile Romanello is a junior researcher at Inserm UMS011 and was before economist at the Institut des politiques publiques (IPP). Her research focuses mainly on heath inequalities. She now works on the ConstanCES based epidemiological population cohort.

Marc Sangnier is professor of economics at Aix-Marseille University, Aix-Marseille School of Economics. His research focus on public economics and political economy.

Acknowledgements

This work was done in the context of a research partnership between the *Institut des politiques publiques* and the *Direction de l'Animation de la Recherche, des Etudes et des Statistiques* (Dares) at the French Ministry of Labor. We thank the Dares for its financial support that made it possible to realise this study.

Our partners at the Dares have also provided several suggestions that dramatically improved the quality of the report. We would like to thank in particular for their useful feedback Cécile Ballini, Mathilde Gaini and Philippe Zamora. We also thank them warmly for their patience and flexibility throughout the project.

We then thank warmly the IPP team for constant support with the project. We are particularly grateful to Julien Grenet as he suggested the central idea that is investigated in Chapter 4 of this report, and provided insightful comments throughout the project. Marion Monnet also deserves to be thanked for participating in the management of the project at its initial stage, and helping throughout.

This report was written after the organization by the Dares and the IPP of a working group on skills and skill mismatch. We would like to thank all participants and their institutions for their feedback when we presented preliminary versions of our work to the group. We also thank the OECD which allowed us to access a slightly enriched version of the PIAAC data, which was very useful to undertake the analyses presented in the report.

NON-TECHNICAL SUMMARY

Objectives and contributions

- The mismatch between workers' skills and firms' skill demands is usually called "skill mismatch". It is regularly put forward as an important source of inefficiency in the labor market. It arises first when the available workforce does not have the skills demanded by firms, or vice versa, when firms are not able to use optimally the skills possessed by workers. Second, it can also arise from a non-optimal allocation of workers' skills across firms. It can hinder productive capacities and generate under-employment.

- However, skill mismatch is difficult to define and measure. What is the empirical evidence that can be used to assess that a country has a particularly accute problem of skill mismatch? Are the existing measures of skill mismatch useful to understand the causes of the mismatch and help designing policies to lower it?

- The first objective of the report is to provide an updated conceptual framework to study skill mismatch. The framework details the possible causes of mismatch and groups them into broad categories. It then reviews the strategies available to measure skill mismatch, discusses the type of mismatch they are supposed to capture, their ability to capture it, and their limits.

- Some measures of skill mismatch rely on a pre-measurement of individuals' skills. The OECD Programme of International Assessment of Adult Competencies (PIAAC) provides measures of general skills in numeracy, literacy and problem solving. Acquiring these measures is costly as it requires survey participants to take lengthy tests which are then graded. The second objective of the report is to understand the links of these measured skills with labor market outcomes to see if they can provide market relevant information that can justify their cost. We analyse whether employment and wage outcomes are explained to a larger extent by measures of skills or by information on education, which is much easier to acquire.

- We also take a policy perspective on the skills measured in PIAAC and study whether they can be affected by educational policies. To this aim, we exploit in nine OECD countries reforms that increased the age up to which schooling is compulsory. These reforms provide an exogenous variation in initial education for affected cohorts. They have been used extensively to study the returns to schooling. We contribute to this strand of research by considering skills instead of education. We aim at understanding if going to school can improve general skills, or if instead, individuals' general skills largely determine their decision to pursue longer studies. This can shed light on the policy relevance of measures of skills available in PIAAC.

Main results

- Our critical review of the literature highlights that skill mismatch may arise for several reasons, some of them inherently linked to the functioning of the labor market, others being more likely to derive from an inadequate or insufficient training at school and on the job. The relative weight of those factors in explaining skill mismatch is hard to assess.

- The measures of skill mismatch that we have reviewed are all subject to several limits which makes it difficult to assess the level of skill mismatch in a country or to compare skill mismatch across countries. As evidence of these issues, we can observe that the extent of skill mismatch in a country varies strongly depending on the indicators used. Another limit is that available measures of skill mismatch capture several of the factors that can lead to the mismatch, and are therefore of limited relevance for policy makers.

- We have analysed compulsory schooling reforms in nine countries. However, due to the limited number of observations in the PIAAC data, we have been able to detect an impact of such reforms on the number of years spent at school only in Belgium, the country where the reform of compulsory schooling was both the most binding by far, shifting the mandatory schooling length from 8 to 12 years, and where it applied the most recently (for all people born after 1969). Absent of this direct effect on the time spent at school, it was difficult to exploit similar reforms in other countries.

- In Belgium, we find a positive effect of schooling on literacy and numeracy skills. The effect on numeracy skills is less robust than that on literacy skills in the sense that it gets smaller and statistically not significant in some specifications. The causal effect of schooling on literacy skills is estimated to be comparable to the correlation between these two variables, suggesting that the latter correlation reflects primarily a causal impact of schooling on skills, rather than a selection of more skilled individuals into longer studies. Our results should be treated with caution due to data limitations that do not allow to obtain very precise estimates. They nevertheless suggest that initial education affects the general skills measured in PIAAC long after schooling, i.e. among adults around 45 years old. This implies that these measures of skills are policy relevant, in the sense that educative policy can affect them.

- Skills in numeracy, and to a smaller extent skills in literacy, have limited predictive power for labor market outcomes. For example, these two measures explain less than 4% of the variance in wages. The fact that compulsory schooling laws have more robust effects on literacy skills whereas numeracy skills are more strongly associated with labor market outcomes can lead us to question the relative importance dedicated to the different fields during primary education. From a purely market perspective, i.e. considering that the objective of initial education is essentially to improve pupils' labor market prospects (which is of course debated), policy makers may wish to shift teaching time from reading and literature to mathematics and sciences, as acquired skills in these fields are more strongly linked to better careers.

- We show that skills are less able to explain labor market outcomes than education. In particular, skills in numeracy and literacy are only able to explain a small share of the residual inter-individuals variations in wages or employment that cannot be explained by education. This means that skills have limited predictive power on labor market outcomes on top of education, raising questions regarding the interest of collecting these costly measures of skills.

- Altogether our results are compatible with the idea that initial education enables people to acquire the general skills measured in PIAAC, but also many others. As a consequence, diplomas provide more information on adult competencies than do a few selected measures of skills. They are therefore more able to predict labor market outcomes. This remains true for older workers, whose careers may have been affected by several other factors than their initial diplomas.

Contents

Introduction

The mismatch between workers' skills and firms' skill needs is usually referred to as "skill mismatch". Skill mismatch may arise when the available workforce do not have the skills demanded by firms, or vice versa, when firms are not able to use optimally the skills possessed by workers. It can also arise from a non-optimal allocation of workers across firms. Some skills for example may be under-used because the workers who have those skills do not work in the firms that need them the most.

Skill mismatch is regularly put forward as an important source of inefficiency in the labor market that can hinder productive capacities and generate under-employment (e.g., McGowan and Andrews 2015b or CEDEFOP 2018) . Policy makers in developed countries are typically concerned with the fact that workers' skills may not adapt quickly enough to the rapid evolution of the globalized economy. Such concerns contributed for example to the French initiative to invest €15 Billions between 2018 and 2022 to develop working-age adult competencies in order "to respond to the emerging issues such as digitalization and robotization, as well as to the ecological challenge".

To motivate this €15 Billions investment plan, the French government argues that about 80,000 jobs requiring digital skills will be unfilled in 2020 (see the numbers given by the *Conseil d'Orientation pour l'Emploi*). This estimate actually comes from the so-called *Empirica report* by Hüsing et al. (2015). To reach it, the authors had to make several assumptions in order to estimate the total supply and demand of ICT skills and their evolution at the macro-level in the future (see Chapter 1). Employed workers may also need to acquire new skills to adapt to the evolution of their jobs. For example, the European centre for the development of vocational training (Cedefop) highlights that "43% of adult employees have recently experienced changes in the technology they use at work and 47% saw changes in their working methods or practices"[1].

To quantify the share of workers that do not have the skills required for their jobs, several alternative approaches have been developed. In particular, the Organization for Economic Cooperation and Development (OECD) has conducted a series of international surveys to measure adult skills: the International Adult Literacy Survey (IALS), administered between 1994 and 1998; the Adult Literacy and Life Skills Survey (ALL), administered between 2003 and 2007; and the Survey of Adult Skills or Programme for the International Assessment of Adult Competencies (PIAAC), administered in 2012. These surveys conduct cognitive tests in literacy, numeracy or problem solving, and provide standardized scores on a 500 points scale, that are comparable across countries and time. In contrast to the well-known Programme for International Student Assessment (PISA) which focuses on a given age (15 years old), IALS, ALL and PIAAC measure general cognitive skills among the adult working-age population (aged 18 to 65). Combining these measures of cognitive skills with workers' self-assessment of the skills required for their job, these surveys are also used to quantify skill mismatch in each participant country. As such, they are one of the main source of information regarding skill mismatch.

Measuring skills and skill mismatch from surveys like PIAAC is however costly. To provide statistics at the country-level that are not too noisy, it is indeed necessary to rely on a representative sample of a few thousands individuals who have to take lengthy cognitive tests. Monitoring and grading these tests is financially costly for the administration in charge, while taking them also represents an opportunity cost for survey participants.

The main objective of the present report is to assess the labor market relevance of the measures of skills in numeracy and literacy available in PIAAC, and of the indicators of skill mismatch developed from these measures.[2] It makes three main

[1] In 2014, Cedefop realized the European skills and jobs survey (ESJS), which represents 49 000 adult employees in 28 EU (seeCEDEFOP 2018).

[2] The report is funded by the DARES–the Statistical Institute of the French Ministry of Labor–who participated in the elaboration of the PIAAC survey and financed the cost related to the French part of the survey.

contributions.

Chapter 1 first provides a review of the literature on skill mismatch, with the objective to clarify (i) its possible causes, (ii) the possible solutions to limit it, and (iii) the different approaches to measure it. The chapter is motivated by the observation that the concept of "skill mismatch" can have several meanings and be used in very different contexts. For example, skill mismatch can describe situations where already employed workers are not optimally matched, or at a more macroeconomic level when some types of job positions remain vacant for a long period. The mismatch between workers' available skills and employers' needed skills can first arise for several reasons, from labor market imperfections to an inadequate initial training. Second, some of these sources of mismatch can be limited by well-designed policies, while others are almost impossible to avoid. Finally, the several approaches to measure skill mismatch capture different types of mismatch, that are caused by different mechanisms. Following a recent work by the European Commission (Kiss and Vandeplas 2015), we review these approaches and try to clarify what they are capturing. We also highlight the limits and weaknesses of these measures. A particular attention is paid to the measures of mismatch developed from PIAAC.

The report then switches the focus from skill mismatch to skills, in particular the general cognitive skills measured in PIAAC. Two distinct empirical exercises are performed in order to better understand and quantify (i) if workers' scores in numeracy and literacy in PIAAC are related to their education level, (ii) if these scores are related to labor market outcomes such as the employment status or wage of a worker. Chapter 2 motivates in greater detail these empirical analyses. It also provides a description of the PIAAC data and of the empirical methods that we use.

Chapter 3 then offers an estimate of the causal effect of schooling on the general skills measured at adult age in the PIAAC survey. The challenge to identify such an effect lies in the fact that the skills measured in PIAAC might not only be related to school–if they do ever–but are also likely to be related to underlying abilities that allow to achieve higher education. In order to estimate the causal effect of schooling on measured skills at adult age, we exploit exogenous changes in schooling induced by mandatory schooling reforms in different countries. In each country, individuals born after a defined date are legally obliged to attend school longer than older cohorts. By comparing the skills in numeracy and literacy of individuals born just before and just after this date, we are able to identify the causal effect of schooling for the first individuals impacted by the mandatory increase in the minimum school leaving age.

Chapter 4 finally offers a quantitative investigation of the link between individuals' skills as measured in the PIAAC survey and their labor market outcomes (employment status and wages). Its first contribution is to provide a comprehensive quantification of the ability of skills and schooling variables to predict labor market outcomes. We study systematically for each country in PIAAC the extent to which skills can explain variations in wages and employment status that cannot be explained by education (and vice versa). The second contribution of the chapter is to study how the relative wage returns to education and skills evolve along the career path. The objective behind this study is to test the hypothesis that education better explains labor market trajectories early in a career, when it is almost the only observable information available on future employees, while skills start to be priced and predict wages only latter, as they become revealed by past labor market experience.

A general conclusion recaps the main results and take-away messages from the report and offers perspectives on the way to use PIAAC data to better understand the role of general skills in developed countries labor markets.

Chapter 1

Literature Review

1.1 Definitions

We first define the different types of skill ; in the rest of this study we focus on cognitive rather than on non-cognitive skills. We then consider two possible ways to define a skill mismatch, whether only comparing the worker's skills to the requirements of his job, or more broadly considering the allocation of human capital on the labor market.

1.1.1 Definition of skills

A skill refers to the "ability or capacity of an agent to act appropriately in a given situation." (OECD 2016c). Following OECD (2016c), in this summary skills and competencies are not distinguished and are considered to be both assessed in PIAAC.[1]

A first distinction between different types of skills is made between **cognitive and non-cognitive** ones. Cognitive capacities correspond to knowledge which can be acquired through education, while non-cognitive skills refer to personality traits, persistence or motivation (Heckman et al. 2006). Psychologists have elaborated a

[1] Alternatively, the European Qualifications Framework (EQF), which links national qualifications systems in Europe, provides a distinction between knowledge, skills and competencies, where the latter is a more inclusive term. It is described as "a demonstrated ability to apply knowledge, skills and attitudes for achieving observable results".

taxonomy of non-cognitive skills known as the "Big Five", which refer to openness to experience, conscientiousness, extraversion, agreeablenss and emotional stability.

Recently, specific attention has been paid to non-cognitive skills in order to explain trajectories on the labor market. Borghans et al. (2008) show for example that personality traits influence workers' productivity, and could partly explain wage gaps such as the ones observed between men and women.[2] Relying on UK data, Carneiro et al. (2007) also consider the long-run life consequences of cognitive and non-cognitive skills measured at age 11. They highlight the important effect of non-cognitive skills on outcomes such as employment status and wages, but also on health or involvement with crime. Regarding the determinants of non-cognitive skills, the authors suggest that the family context is highly predictive of non-cognitive skills level.[3]

A second distinction is linked to the connection of the skill with a professional context.[4] **Soft skills** are required in any professional context and mainly concern behavioural knowledge. Then, **transferable skills** are specific to a sector but one can find a use for it in another professional environment. Finally, **specific skills** are directly linked to a job and cannot be transferred to another one. In the training literature, the difference between general and firm-specific skills is usually made (Acemoglu and Pischke 1998). General skills are not attached to a particular firm and are valued in several employment opportunities. Their acquisition will translate into higher earnings in a competitive labor market, while firm-specific skills are only valued in a single firm. Firms thus do not have any incentive to fund for training delivering general skills as workers might leave for another firm. The latter will benefit from the new worker's skills without paying for its acquisition (poaching externality). As a consequence, firms should only be interested in funding training for acquiring specific skills, which are directly linked to their professional situation.

[2]In details, conscientiousness and emotional stability are more predictive than other Big Five personality traits.

[3]As non-cognitive are "more malleable than cognitive skills", the authors suggest to focus education policies on developing this type of skills.

[4]The following typology has been detailed in particular by the working group "Réseau Emplois Compétences 2017".

Finally, **educational attainment** may capture and approximate skills of individuals, however the two concepts should be distinguished. Skills refer to a worker's real ability, while diploma is an imperfect signal of actual skills. Both terms do not recover the same reality: some skills cannot be signalled with a diploma, especially non-cognitive ones (Reynaud 2001). As pointed out by Hanushek and Woessmann (2008), diploma is only informative of skills acquired at school, while they can be acquired through other channels such as family, friends, culture and so forth. Most importantly, skills are also acquired on the job through working experience. As a consequence, the set of actual skills and their level may considerably differ from one individual to another with the same level of educational attainment.

1.1.2 Definition of skill mismatch

Two types of skill mismatch are considered : in the most restrictive definition, it corresponds to an inadequate matching between a worker and his employer. In a broader sense, skill mismatch reflects a gap between the aggregate labor demand and aggregate labor supply.[5] In the rest of the report, we consider both types of skill mismatch : an inadequate matching between a worker and his employer is considered as the "individual" or "micro" skill mismatch, while a gap between the labor demand and supply is rather called skill mismatch at the "aggregate level".

Quintini (2011a) defines a **skill mismatch** as the inadequacy of a worker's skills relative to the requirements of his/her job. Skill under-utilisation (overskilling) refers to the phenomenon whereby a worker's skills exceed those required by his/her job. Inversely, under-skilling corresponds to the situation where an individual's level of skill is not sufficient for the level of skills required. This is an important concern in OECD countries, as on average 14% of workers are assessed to be mismatched in literacy and/or in numeracy according to PIAAC data (Pellizzari and Fichen 2013).

Skill gap can more broadly reflect an inefficient allocation of human capital on the labor market, in other words a situation in which the skills sought by employers

[5] McGuinness et al. (2017) for example make the distinction between skill mismatches " measured at the level of the individual's circumstances and those that are measured in terms of firm level aggregates".

are different from the skills offered by workers or job-seekers (Kiss and Vandeplas 2015). Skill shortage arises when employers are unable to recruit staff with the required skills in the accessible labour market and at the ongoing rate of pay.

Skill mismatch described above concerns the *level* of skills, in the sense that one's skills level is above or below the level of skills required. It thus refers to a **vertical mismatch**. On the other hand, **horizontal mismatch** is a field-of-study mismatch (Montt 2017) which characterizes the phenomenon whereby a worker's field of qualification does not coincide with the field of his/her work. Finally, **qualification mismatch** is sometimes used as a proxy for skill mismatch : it comes from the inadequacy between initial education and the position held. Over-qualification (under-qualification) occurs when a worker has more (less) qualifications than required by his/her job.

1.2 Skill mismatch sources

Skill mismatch might result from two separate channels. It can first be due to an imperfect matching between the employer and the worker because of imperfections on the labor market. Second, skill mismatch may also result from a gap between the aggregate supply and demand. For example, technological change is likely to shift quickly the type of skills desired by employers, while workers' competencies may not adapt and develop at the same pace.

Implications in terms of public policy will differ according to the source of skill mismatch: in the first case, reducing labor market imperfections might improve the quality of matching. For example, the optimal unemployment insurance should both give job seekers an incentive to find a job, while providing a sufficient amount of time to look for a quality job. However, if the gap between the aggregate supply and demand mainly explains skill mismatch, anticipating skills needs with relevant educational and training policies is a relevant solution. Acquiring high-level skills is for example likely to help individuals to adapt to technological change. It requires an adequate provision of adult learning opportunities, as well as employers' involvement in the design of educational curricula at the upper secondary and tertiary level. The latter also play a role in workers' skills acquisition by designing the adequate pattern of work organization,

1.2.1 Labor market imperfections and regulations impairing the matching process

Skill mismatch, either at the individual or aggregate level, may first arise from imperfections during the matching process on the labor market. The search and matching models developed by Diamond, Mortensen and Pissarides allow to model the individual job search process accounting for the behavior of firms and its interactions with other workers. The search process of job seekers and the need of employers to fill vacancies is costly and might be slowed down by institutional settings.

Imperfect information

The matching can first be imperfect due to imperfect information: the employer does not perfectly observe candidates' skills, which can lead him/her to hire individuals whose competencies are inadequate for the job considered. The fact that the share of over-skilled workers is higher among workers entering the labor market strengthens this hypothesis.[6] This is because the available information to assess the skills of inexperienced workers is basically limited to the diploma. By signalling skills, the diploma provides useful information (Spence 1973). However, as we have seen, it does not provide a perfect signal on all the skills relevant on the labor market.

Matching frictions

The labor market is intrinsically frictional in the sense that the job search process is time consuming and costly. Workers cannot observe and apply to all possible possible job offers instantly. Similarly, it takes time for firms to interview job seekers

[6] Young people overskilling has slightly increased since the 1990s in OECD countries (OECD 2016c).

to fill a vacancy. As a consequence, when they have to decide to match, firms and worker trade-off between the expected quality of their match and their future opportunities if they do not match, accounting for the fact that it may take time for them to find a better match latter on. This trade-off implies that workers and firms may decide to match even if better matches would probably have been possible, just because the cost of waiting to find (or searching for) a better match is too high. This creates (skill) mismatch. The underlying mechanism generating the mismatch is common to all two-sided markets with heterogeneous agents where there is no instantaneous market clearing.[7] It is formalized in standard search and matching models with heterogeneous workers and firms (as in e.g. Postel-Vinay and Robin 2002).

Labor market segmentation

Next, labor market segmentation partly explains imperfect matching on the labor market, as workers are locked in specific markets and thus cannot or are not willing to access all potential vacancies.

An important type of segmentation is geographical: the existence of many local markets prevents workers from matching to a job if it is located too far from their household. Wasmer and Zenou (2006) detail several channels for interpreting this spatial effect on the labor market. On the labor supply side, workers living far from employment areas own less information on employment possibilities, which raises the cost of this information. They may also be reluctant to have long commuting times, or to move their housing as they may loose all sort of local amenities, such as those related to leisure activities or friends networks. Lee and Wolpin (2006) show for example the existence of important mobility costs in the US, such that output in the manufacturing and tertiary sectors would have been double their current levels if these mobility costs had been zero. Direct mobility costs are linked to removal costs, while indirect costs such as risk aversion (Bowles 1970), or the loss of local amenities, are likely to be the most important . As an illustration, Van Leuvensteijn and Koning (2004) show that homeowners are more likely to be unemployed as

[7]For example, a similar logic is sometimes applied to the marriage market.

they are less mobile. Finally, employers might favor less remote candidates as well.

Another source of segmentation is the dualism of the labor market, which consists in the distinction between a primary and a secondary labor market.[8] The primary market is characterized by a stable level of earnings and more employment security, while turnover is more important in the secondary market. Conditions of work are also less advantageous, and occupations are mainly held by young individuals, women, or workers from ethnic minorities workers. Dualism is likely to prevent workers locked in the secondary market to access job opportunities in the primary market, and it can therefore increase mismatch by constraining the matching process and the set of possible matches. [9]

Labor market regulations

The most obvious source of regulation likely to create segmentation and mismatch might be employment protection legislation (EPL). EPL may create rigidities on the labor market which could reduce the speed at which employers are able to adjust to structural changes (Quintini 2011a) and to break a bad match. It also increases dualism on the labor market, which in turn is a possible source of mismatch (see above). Berton et al. (2017) provide evidence of this by relying on a quasi-experimental setting : it consists in a 2012 Italian reform which decreased the EPL level for open-ended contracts differently for companies of different sizes. Looking at qualification mismatch (rather than skill mismatch), they show that such a reform improved the quality of matches on the labor market and was followed by a relatively small increase in productivity.

However, the effects of labor regulations in general and of EPL in particular are theoretically ambiguous. By increasing job security and making job contracts more enforceable, labor regulations may also increase the time period workers expect to spend with their current employer. As a consequence, workers may be more willing

[8]This distinction has been mainly described in Doeringer and Piore (1975).

[9]Reich et al. (1973) identify three additional types of segmentation. Within the primary labor market, "subordinate" primary jobs are routinized and require discipline and responsiveness to rules and authority, while "independent" jobs encourage self-initiative and creativity. The two last types of segmentation identified by the authors rely on discrimination of specific social groups: minority workers and women only access to less well-paid jobs, and some jobs are restricted to these groups.

to take long-term and more risky investments, and in particular to invest in firm-specific skills or innovative activity. Griffith and Macartney (2014) find empirical evidence that these effects are at work.

On the other hand, as geographical segmentation is strongly linked to imperfect workers' mobility, which largely comes from high workers' moving costs (Kennan and Walker 2011), it is largely independent from labor market regulations.

Note that the latter sources of imperfect matching might create the two types of skill mismatch mentioned above. Skill mismatch as an inadequate matching between employers and workers can indeed come from imperfect information or matching frictions. However workers' lack of mobility can also induce a more general gap between the labor supply and demand.

1.2.2 Gap between the aggregate supply and demand

If workers were optimally matched on the labor market, skill mismatch might still arise because of a gap between the labor supply and demand. In this situation, inadequate initial or continuous training might lead workers' skills not to fit employers' requirements.

Skill biased technological change and job polarization

The skill biased technological change is a first explanation of an insufficiently skilled labor supply. It consists in a shift in the production technology that raises the relative productivity of skilled workers compared to unskilled ones: those using computer technology see their productivity rising while unskilled workers see their tasks replaced by computerisation (Bekman et al. 1998; Card and Lemieux 2001; Autor et al. 2008). The relative demand for skilled workers thus mechanically increases. Those recent shifts in technology require specific skills which are not immediately available in the labor supply, giving rise to a potential skill mismatch.

This estimated skill premium increase did not manage to fully explain the parallel rise in some non-qualified job wages. A more recent analysis suggests that job polarization occurred in Europe and the US since the 1990s and led to a disproportionate increase in high-paid and low-paid employment. Such a modification in the wage distribution is due to the rising use of non-routine tasks in those jobs, to the detriment of manufacturing and clerical work (Goos et al. 2014). On the other hand, the demand for unskilled labour is less affected, which is notably due to the demand for services that are hard to be replaced by IT technology. Acemoglu and Autor (2011) provide evidence of an increasing share in employment of high-skill and low-skill occupations, in comparison with medium skilled occupation. David and Dorn (2013) confirm that those observed changes rely on the service sector evolution : tasks such as home health aides, food preparation and serving or jobs in security services are intensive in non-routine manual tasks. On the other tail of the wage distribution, abstract tasks that require problem-solving capabilities cannot be automatized either. Testing their model on US data, they identify commuting zones that were initially relatively intensive in routine job activities.[10] In those areas, where tasks became easier to computerize, employment and wages increased at both ends of the occupational skill distribution at a higher pace than in other commuting zones.

To sum up, technological change has affected labor demand and the nature of job tasks. The demand for routine tasks has decreased relative to the demand of low-skill non-routine tasks (in part due to the raise of home-care services) and high-skill non-routine tasks. These phenomena have changed the skills required at work, possibly generating a gap between the skills possessed by the workforce and those actually needed. As a consequence, unemployment might emerge mainly among low-skilled and high-skilled individuals specialized in non-routine tasks.

As previously, the mentioned channels might contribute to both types of skill mismatch. A lack of adequate skills results in a gap between the labor supply and demand but might also lead an employer to hire a worker who does not totally correspond to the required profile. In this case the resulting skill mismatch refers to an inadequate fit between the employer and the worker.

[10] The Dictionary of Occupational Titles allows to link mapping task data to occupation data from the Census.

1.2.3 Actions to take

Smoothing the labor market imperfections

One potential solution to reduce mismatch probability is adapting the unemployment insurance system to allow workers searching longer before taking a job : higher payments increase the resources available for a job search. Relying on NLS79 data, Centeno (2004) finds that a more generous unemployment insurance system induces a longer job tenure for workers, which is interpreted as a better match quality. Tatsiramos (2004) finds the same conclusion for European countries. On the other hand, Van Leuvensteijn and Koning (2004) show that the reduction of the potential duration of benefits in Slovenia did not accelerate job search intensity of unemployed individuals without lowering the quality of the post-unemployment job match.[11]

Another relevant action to implement to tackle labor market imperfections would be to compensate for costs associated with workers' costs linked to mobility.

Adapting educational policies

In this context, medium-skilled individuals might thus suffer from skill mismatch if they do not train for acquiring adequate skills regarding the new labor demand requirements, and educational public policies need to account for those changes. As an illustration, Hanushek et al. (2017b) highlight that though vocational education allow young individuals to enter the labor market more rapidly than general education, this initial gain might be offset by less adaptability to technological change and thus diminish employment later in life. Indeed, they compare 18 countries using the International Adult Literacy Survey (IALS) and find evidence of such a trade-off : the age-employment pattern differs between individuals with general and vocational education, mainly in apprenticeship countries such as Denmark, Germany, and Switzerland.

[11] The job match quality is measured through the duration of the newly found jobs and the distribution between fixed-term and permanent jobs.

The role of firms in the development of workers' skills

It is worthwhile noting that firms also play a role in their workers' skills improvement. Indeed, work organization has been shown to influence the workforce' skills and to keep individuals employed. Greenan et al. (2017) show that work organizations characterized by "a relatively high level of learning, problem solving and discretion" decrease workers' probability to loose their job[12] compared to the four other types of organization considered by the authors. The authors rely on PIAAC to capture some dimensions of work organization such as autonomy in the job or collaboration in the workplace. The authors estimate a multilevel logistic regression, thus results could be biased by unobserved individuals' or firms' characteristics. However it provides at least descriptive evidence that employers play a role in workers' acquisition of transversal skills.

Employers also directly play a role through training provided in the firm. Cabrales et al. (2014) rely on PIAAC data to study the link between on-the-job training and achievement at test scores. They show that the availability of training at the workplace is associated with a significant improvement of workers' cognitive skills : it accounts for 15% and 28% of the raw score gaps in literacy and numeracy, respectively. [13]

It thus appears that firms play an important role in maintaining and developing workers' transversal and cognitive skills, which could contribute to the overall development of the skills supply, and in the end reduce the gap between the labor supply and demand. Firms' training may also contribute to convert a "bad" matching into a better by allowing the worker to develop the required skills for a job.

[12]Vulnerability to non-employment is defined as being currently non-employed while having been employed at some point during the last twelve months previous the PIAAC survey.

[13]The authors estimate regressions where unobserved variables cannot be accounted for, which should lead to consider those results as descriptive evidence.

1.3 Skill mismatch consequences

The impacts of skill mismatch have been largely documented, mainly on wages. However the lack of panel data and the estimation of simple regressions in the majority of studies require to consider those results cautiously. Two types of consequences arise : on the one hand, the gap between supply and demand of skills results in unemployment. On the other hand, the inadequate matching between a firm and a worker might lead to cut a part of her wage, reduce job satisfaction and productivity at work.

Effect on unemployment

Skill mismatch, as an imbalance between the supply and demand for skills, first creates unemployment. This effect has been theoretically documented : Thisse and Zenou (2000) develop a model where labor market is imperfectly competitive because both firms and workers are heterogeneous, and where the imbalance induced between the demand and supply of skills leads to unemployment. On the empirical side, Şahin et al. (2014) state that mismatch explains one third of the total observed increase in the unemployment rate in the US.[14] A second strand of the literature rather focuses on the state dependence between a current skill mismatch and the further probability to get unemployed. For example, Mavromaras et al. (2015) show that skill mismatch is an additional worker's characteristic which increases a high-educated workers' probability of future unemployment. Similarly, Baert et al. (2013) investigate whether overeducation acts as a "stepping stone" for young graduates for speeding up their transition toward better positions.[15] On the contrary, they find that overeducation is a "trap" and locks workers into bad positions. One possible explanation is that those individuals access less often to training and thus acquire less additional skills than well-matched individuals with

[14] The authors consider skill mismatch in a broad sense as the difference between sectors, occupations or locations in which workers are looking for job and those where available jobs are. They construct a mismatch index to quantify the fraction of hires lost because of misallocation. In details, they compute the planner's hires and compare it to the observed aggregate hires in each sector.

[15] As mentioned by the authors, following the career mobility theory, overeducation could be an "investment in work experience which enhances promotion opportunities to higher level positions inside or outside the firm".

a similar educational level.

Effect on wages

Regarding wages, evidence tends to show that among individuals that have the same measured skills, those who are over-skilled regarding their job earn less than those who are not. However, the estimated wage gap is lower than the corresponding gap between over-qualified and non-over-qualified individuals having the same diplomas. Similarly, under-qualified workers earn less than other employees doing the same job but with a higher level of qualification (Quintini 2011a). Relying on a meta-analysis of the effect of overducation on wages, McGuinness et al. (2017)[16] show that the wage penalty due to overskilling is estimated to be smaller than the overeducation wage penalty, 7.5% against 13.5% on average (Di Pietro and Urwin 2006 ; McGuinness and Sloane 2011; Sánchez-Sánchez and McGuinness 2015). Typically, McGuinness and Sloane (2011) estimate the effect of overskilling and overeducation in a wage equation, considering UK graduates in the REFLEX database.[17] They account for unobserved heterogeneity relying on a propensity score matching model. The authors measure large wage penalties for being overskilled, though being half less important than those linked to overeducation ; the effect is only significant for men. In France, relying on *Enquête Génération 98*, Béduwé and Giret (2011) confirm that a vertical skill mismatch induces an important wage penalty, contrary to an horizontal mismatch which is neutral in terms of earnings. Indeed, in their regression analysis the coefficient associated to having an appropriate level but a different field of education is not significant.[18]

Sloane (2014) accounts for individual unobserved heterogeneity on a more con-

[16]"Of the 86 papers on overeducation, four are review articles and the remaining 82 carry out some type of empirical analysis. The subjective method for measuring overeducation is used in 42 papers, the empirical approach in 32 papers and the job-evaluation method in 24 papers."

[17]Individuals were defined as overeducated if they answered that a below tertiary level of education was most appropriate for the job. Overskilling relied on the response to a question asking individuals to rate on a 1 to 5 scale the extent to which their skills and knowledge were used in their work with a response of 1 or 2 deemed consistent with overskilling.

[18]The authors rely on diploma and jobs nomenclatures as well as on correspondence tables to link both. An individual is considered as vertically mismatched if the job level corresponds to her level of qualification. Horizontal mismatch corresponds to whether her field of training corresponds to her job.

vincing way, relying on the Household, Income, and Labour Dynamics in Australia (HILDA) which are panel data.[19] They do not find evidence of any wage penalty either linked to overskilling or to overeducation once accounting for unobserved heterogeneity through fixed and random effects models. However workers combining both overskilling and overeducation experience a wage penalty of 6% compared to other workers.

Why is there a wage penalty linked to over-education?

In the most standard theory of human capital, workers decide to acquire qualifications based on their expected returns that do not depend on the matching process on the labor market. This implies that a given level of qualifications should lead a given wage level, independently of workers being over- or under-skilled. This prediction from the basic human capital theory is in contradiction with the empirical results described above and puts into questions such results.

The wage penalty linked to over-skilling may however be easily explained in slightly more sophisticated models that take into account the two sides of the labor market and the fact that workers with similar qualifications may be matched with different firms and doing jobs that are not equally productive. For example, in the assignment model proposed by Sattinger (1993), the worker faces a distribution of potential wages and job characteristics and chooses a job relying on utility maximization. Thus, wages are not strictly proportional to an individual's human capital but also depend on the assigned job.[20] In search and matching models with heterogeneous workers and firms (e.g. Postel-Vinay and Robin, 2002), a match productivity also depends on the productive characteristics of the firm, leading similar workers to be paid differently when they work in different firms.

Another potential explanation for the wage penalty associated with over-education is that qualification does no fully reflect workers' skills. To limit this problem, several studies control for workers' skills in order to measure the wage penalty as-

[19]The authors only consider male college graduates.

[20]McGuinness (2006) presents a review of the literature on overeducation, where he concludes that predictions of the human capital theory are put into question by the existence of different returns to same level of education. On the other hand, he suggests that the assignment theory better explains the findings of considered studies.

sociated with over-education among workers with similar measured skills (Bauer 2002; Chevalier 2003; Frenette 2004). The latter papers conclude that once skill differences across workers with similar levels of education are accounted for, the wage penalty associated with overeducation disappears. As an example, Chevalier (2003) relaxes the assumption that graduates are homogeneous in their skills endowment. He divides over-educated workers by their skill level and makes the distinction between "apparently over-educated" workers, who own similar unobserved skills as matched graduates, while the "genuinely over-educated" workers have a lower skill endowment. In the first case, over-education is associated with a wage penalty of 5%-11%, while the second type of overeducated workers suffers from a pay penalty of 22%-26%. Then, wage penalties seem more related to a lower ability-endowment than to a real skill mismatch. However, McGuinness (2006) highlight that those studies assume that all unobserved individual differences are only linked to skills, while they might also relate to other personal or job characteristics.

To wrap-up, overeducation seems to have a significant negative effect on individual wages. However when overskilling can be measured and when other unobserved characteristics can be accounted for on a clean way, the concluding message is less clear.

Effect on job satisfaction

An extensive literature has highlighted the negative effect of overeducation on job satisfaction (Tsang and Levin 1985 ; Verhaest and Omey 2006, Verhofstadt et al. 2003), while Allen and Van der Velden (2001) show that overskilling is a better predictor of job satisaction than overeducation. The authors rely on data collected for the project "Higher education and graduate employment in Europe", a comparative study in Europe to analyze the labor market situation of graduates from tertiary education.[21] They rely on workers' self-rating of the educational level required for their current job, as well as on their perceived degree of skill mismatch in their job. The authors find that skill under-utilisation has a strong negative effect on job satis-

[21]The authors restrict their analysis to the Netherlands.

faction, while the coefficient for educational mismatch is not statistically significant when tested separately. Sloane (2014) also provides evidence that, once accounting for individual unoberved heterogeneity, being overskilled still greatly reduces job satisfaction, whether alone or combined with overeducation. However it might be that overskilled individuals own unobserved characteristics leading them to be more demanding regarding their job ; studies previously mentioned do not clearly control for those unobserved characteristics, which could bias their results.

Effect on productivity

Finally, McGowan and Andrews (2015b) provides descriptive evidence with PIAAC data that overskilling induces a lower labor productivity, while the latter does not seem to be affected by underskilling.[22] The authors thus argue that increasing the skill level does not always induce a higher level of productivity, which can first appear as counter-intuitive. McGowan and Andrews (2015b) highlight that mismatch could induce spillover effects by preventing an efficient allocation of high-skilled workers, and thus reduce the aggregate level of productivity. Indeed, more productive firms need to employ a larger share of high-skilled workers but they might encounter some difficulties to do so if the pool of such workers is fixed and to the extent that they are under-utilizing their skills in low productive firms. This approach connects with the larger existing literature on resource misallocation and on its impact on countries' productivity (Bartelsman et al. 2013). As an example, Acemoglu et al. (2013) show that policy intervention providing support for R&D are effective only when they encourage the exit of the less productive firms ("low-type") as it releases some resources for innovation in the most productive ones ("high-type" firms).

[22]The authors rely on OECD approach to measure mismatch, the so-called "self-assessment method" (see section 1.4.2 for more details). The share of workers that are well-matched or overskilled are then aggregated at the 1-digit industry level. They estimate a regression controlling for both country and industry fixed effects.

1.4 How to measure skill mismatch?

As previously mentioned, a skill mismatch might refer to the inadequacy between a matched workers' skills and her job requirements, or to the gap between the skills of the job seekers and the skill requirements of vacant jobs. The latter approach either relies on employers' assessments of their recruitment difficulties or on both demand and supply information. It was however previously highlighted that the two main sources of skill mismatch do not systematically overlap the two definitions of skill mismatch. In the same way, indicators presented here to measure skill mismatch are not systematically linked to one source of skill mismatch.

Table 1.1 first shows the different ways to measure skills supply and skill demand, as presented in Gregorini et al. (2016). Both can first be identified through an indirect measure, mainly through educational attainment. Indeed, qualification has first been used as a proxy for human capital and thus for assessing the adequacy with the occupied job. Data directly skills have then been considered in order to better assess the match between a job and a worker. Then skills can directly be measured through surveys such as PISA for skills supply, and job vacancies surveys for skills demand. Finally, skills can be assessed through subjective / task-based self-reporting, which suffers from common limits of subjective measures, as they are relatively less precise and comparable across each others. The measurement of skill mismatch can rely on the three types of indicators in Table 1.1 or on more direct measures (e.g. direct self-assessment). The mismatch between job seekers' skills and hiring needs (section 1.4.1) typically relies on the indicators in Table 1.1 while the micro-level mismatch between a worker's skills and her job requirements (section 1.4.2) is captured through more direct measures.

Table 1.1: **Measuring different types of skills**

	Skills supply	Skills demand
Proxy / indirect measure	Educational attainment	Employment by educational attainment
Direct measure	Assessment, standardized testing	Data on job vacancies / newly employed
Self-reported measure	Self-reported ability to perform tasks	Subjective assessment by employers

Source : Gregorini et al. (2016).

1.4.1 Mismatch between job seekers' skills and hiring needs

Indirect measure : employment by educational attainment

A possible indicator of skills mismatch at the aggregated level is the variation of employment and unemployment rates across skill groups (Kiss and Vandeplas 2015). Comparing the discrepancy between the employment and unemployment rates of the high, medium and low-skilled individuals allows to assess whether workers' skills met employers' needs or not. The EU Labor Force Survey frequently collects data, which allows to implement international comparisons and to track skill mismatch evolution over time. However the distinction into three main skill levels is relatively simplistic and rather relies on the educational level : low-skilled individuals have a primary or lower secondary education, medium-skilled have an upper secondary or post-secondary non-tertiary education and high-skilled have a tertiary education. Those data provide relevant information regarding the level of the satisfied part of skills demand between broad educational levels, however it does not account for skills heterogeneity within the latter.

Direct measure : vacancy analysis and skill anticipation tools

A vacancy analysis provides information on how the labor demand is satisfied, and usually relies on public employment service data. It aims at identifying prolonged unfilled vacancies or high job vacancy rates. One limitation is that jobs advertised through national employment agencies or through the internet are not representative of the whole labour market. Data on the newly employed ("in current job for 12 months or less") is also available from the EU-LFS.

Picturing the simultaneous situation of the labor supply and demand allows to anticipate potential mismatch between both. National and European initiatives have been implemented in this perspective. Indeed, many countries developed their own national skill anticipation and assessment (SAA) tool in order to assess potential skill shortages, as well as current and future skill needs on the labor market. They rely on quantitative data related to labour market and educational information.[23] A common limitation to those different approaches is that occupations are considered as proxies for skills, while the latter are transversal to several occupations and there does not exist a robust mapping to link both notions.

An interesting mapping between occupation and skills is proposed in O*NET data,[24] which is used in the *OECD Skills for Jobs Database*. The latter provides an overview of skill mismatch in Europe (OECD 2017). The first stage consists in elaborating an occupational shortage index, which provides information regarding the extent of shortage or surplus in an occupation. It relies on wage, employment and talent data. Then, the index is combined with O*NET data, which associates each occupation to a set of specific skills.

In Europe, the European Skills, Competences, Qualifications and Occupations (ESCO) classification is similar and was elaborated by the European Commission. The job analysis methodology, which consists elaborating such a mapping between occupation and skills, has the advantage of providing an independent referential, however it fully depends on the *ex ante* study of each job requirements in skills, which is cumbersome and subject to experts' appreciation. Moreover the referential is likely to be rapidly outdated as occupational requirements change over time.

The idea of mapping occupations and skills has been used in the *Empirica Report* by Hüsing et al. (2015) to estimate the number of jobs requiring digital skills which will be unfilled in 2020.[25] However the complexity of their methodology might also demonstrate the limits of such a method. Indeed, they first identify as ICT practitioners individuals that work or have worked in an occupation considered (by experts) to require ICT skills. Second, they then quantify the supply of ICT professionals at a given point in time as the number of employed and unemployed ICT practitioners (measured with the labor force survey). The demand for ICT professionals is then the sum of the number of employed ICT practitioners and of the number of open job vacancies for ICT professionals. This latter number is

[23] In France, France Stratégie and the Ministry of Labor have developed "Prospective des métiers et qualifications" (PMQ).

[24] The Occupational Information Network (O*NET) has been adapted from the former the US Dictionary of Occupational Titles (DOT) to better suit the current labor market.

[25] The *Conseil d'Orientation pour l'Emploi* relies on these estimates for anticipating a future need in France of 80,000 jobs requiring digital skills.

estimated from an analysis of online vacancy data (www.jobfeed.com). The gap between these estimates of demand and supply finally provides an estimate of the excess demand for ICT professionals. Things get more tricky when it comes to predict the future. To forecast the evolution supply of ICT professionals in the future, the authors estimate the inflows and outflows of e-skills to/from the labor market using for example forecasts on future computer science graduates and retirees. To forecast the evolution of the demand for ICT professionals, they use data on the trends in ICT workforce or firms' IT spending which they combine with macro forecasts for GDP growth and IT spending in the coming years. The difference between the predicted demand and supply in the future is then used to estimate the future needs in digital skills.

Self-reported measure : subjective assessment by employers

Employers' surveys provide employers' assessment about skills shortages and requirements,[26] however they are subjective and their comparability is low within a country or at the international level (OECD 2017). Moreover, questions relate to recruitment difficulties, which might not only be due to a lack of skills in the available workforce but also to unattractive working conditions or to inadequate human resources policies. It is also worth noting that those survey data are not fully consistent: in France for example, the European Company Survey (ECS) stated that around 50% of surveyed employers had difficulties to hire workers with required skills in 2013, while at the same time the Manpower international survey assessed that 29% of employers had difficulties to fill vacancies (OECD 2016a). Such a large gap invites to consider the results from employers' surveys cautiously.

1.4.2 Mismatch between workers' skills and their job requirements

Once a worker has been hired by a firm, skill mismatch can arise if her level of skills does not fit the one of the job. Three main sources of information allow to

[26] In France, the "Besoin de main d'œuvre" survey provides information about employers' future needs in terms of occupations.

measure skill mismatch : the worker's self-assessment regarding whether he is well-matched or not, the measure of his skills and the extent to which he uses these skills. Adequately, three indicators of skill mismatch arise : the self-reporting approach, the realized approach, which is the official methodology adopted by OECD in its report on PIAAC data (OECD 2013), and the comparison of workers' skills use to his skills level. It is important to notice that the latter indicator is sometimes considered as a variant of the realized approach methodology, though it does not rely on the same assumptions.[27]

Those indicators allow to measure a vertical mismatch, i.e to assess to what extent workers' level of skills is adapted to the one required by the employer.

The self-reporting approach

First, the auto-evaluation approach directly relies on workers' view of the adequation between their skills and their job requirements. In the case of *direct* self-reporting individuals are asked whether they consider themselves to be over- or under-qualified, while in the case of *indirect* self-assessment the question deals with which qualification is needed to get or to perform their jobs. For example, Allen and Van der Velden (2001) use the data of Higher Education and Graduate Employment in Europe. The paper classifies skill mismatch relying on the response to the following questions: "Do you think you have the skills to cope with more demanding duties than those they are required to perform in their current job?" and "Do you think you would need further training in order to cope well with their present duties?". Individuals answer on a five-point scale. The authors regard these self-reports as indicators of the degree of skill mismatch and of the skill deficit, respectively. Simple regressions using these indicators reveal the negative effect of skill underutilization (i.e. being over-skilled) on wage and job satisfaction, and positive effect on on-the-job search behaviours. Following studies relying on the same methodology show similar results : Di Pietro and Urwin (2006) find a negative impact of skill mismatch on earnings, and McGuinness and Sloane (2011)

[27] In details, the former method compares the distribution of skills level of self-reported well-matched individuals to each individual level of skills, while the latter does not rely to the self-reported answers regarding mismatch.

report a negative impact of skill under-utilization on wage only for men, and a negative impact on job satisfaction for both sexes.

Table 1.2 summarizes studies relying on the self-reporting approach.

Table 1.2: **Self-report approach**

Name	Data	Sample Characteristics	Methodolgy (to identify skill mismatch)	Result	Advantages/Disadvantage
Allen et al. (2001)	Higher Education and Graduate Employment in Europe	Graduates of tertiary education (university and higher vocational training) in 11 European countries and Japan	Skill underutilization: those who gave 4 or 5 to the question "My current job offers me sufficient scope to use my knowledge and skills". Skill deficit: those who answered 4 or 5 to "I would perform better in my current job if I possessed additional knowledge and skills". Matched: those who answered 1-3 to both questions. (higher number indicate stronger agreenment to the statement)	15% skill overskilling and 53% of skill deficit are self-reported. Negative effect of overskilling on wage and job satisfaction, and positive effect on-the-job search behaviors	Selected samples (Only highly educated people and relatively young). Cross-country data. Bias regarding the self-report. Skill domain not well-defined.
MacGuiness et al. (2009)	UK data in Flexible Professional in the Knowledge Society (REFLEX)	UK university graduates	Those who answered 1 or 2 to "To what extent are your knowledge and skills utilized in your current work?" are classified as over-skilled and those who answered 4 or 5 as under-skilled.	Negative impact of skill underutilization on wage only for men, and negative impact on job satisfaction for both sexes.	Selected samples (Only highly educated people and relatively young). Only UK. Bias regarding the self-report. Interpretation of 4 and 5 as under-skilled is doubtful. Skill domain not well-defined.
Pietro et al. (2006)	ISTAT (National Statistical Italian Centre) data	Italian university graduates	Answers of "none" and "a little" to "the extent to which they have used the knowledge and the skills acquired at university in their current job" are classified as mismatch. No mismatch, otherwise.	Negative effect of skill underutilization on wage, but not a strong evidence for job-search behavior.	Selected samples (Only highly educated people and relatively young). Bias regarding the self-report. Only skill underutilization no skill deficit indicator. Skill domain not well-defined.
Allen et al. (2013b)	Flexible Professional in the Knowledge Society (REFLEX)	University graduates	Using the same question above, answers scaling from 1 to 5 are used as a reverse indicator of skill surplus.	Wage penalty associated with over-education is due mostly to skill heterogeneity in private sector, whereas it is due more to wage setting process in public sector .	Selected samples (Only highly educated people and relatively young). Bias regarind the self-report. Only skill surplus, no skill deficit indicator. Skill domain not well-defined.
Green et al. (2017)	Skills Survey in UK	Aged 20 to 60	Over-skilling based on two questions and under-skilling based on one question.	Incident of over-qualification occurs due largely to skill heterogeneity	Bias regarind the self report. Skill domain not well-defined.
Béduwé et al. (2011)	French data "Generation 98 survery"	Vocational program graduates	Based on the question asking whther one's skill is fully, over-, or under- utilized	Skill match rate ranging from 58% to 73% depending on matching status of qualifications. 5% of wage penalty, job dissatisfaction and active job search behavior associated with over-skilling.	Limited to vocational training.
Pellizzari et al. (2013)	PIAAC	Country by country, literacy and numeracy	1) For each occupation, classify over-skilled and under skilled by self-report 2) Calculate the max and min value of skill levels among those classified as well matched for each occupation	In pooled sample, literacy well-matched is 86%. 4% under-skilled and 10% under-skilled. Overlap of literacy and numeracy mismatch is as high as 94%. Men are more likely to be over-skilled than women, Tertiary graduatews less likely to be underskilled and foreign workers more likely to be under skilled.	1 digit code. Assumption= treatment of skill use as an endogenous choice of the worker. Average plausible value to reflect measurement error.

The realized approach

Such a subjective measure of skill mismatch can be compared to the (seemingly) more objective realized approach. Relying on the same questions as previously, one can define the minimum and maximum skill[28] endowment of workers who neither

[28]Typically, literacy and numeracy skills are considered in studies relying on PIAAC data.

feel the need for further training nor feel capable of doing more demanding jobs. Then, one considers some bottom and top percentiles of the within-job distributions of workers' skills, usually the 95th and the 5th percentiles of the within-occupation distribution of skill of workers declaring to be well-matched. When considering the overall sample, a worker is declared to be mismatched if his level of skill is below or above the previously defined cutoffs. In details, he is considered as under-skilled if his individual level of skills is inferior to the 5th percentile and over-skilled if it exceeds the 95th percentile level.

This method has been elaborated by Pellizzari and Fichen (2013) for PIAAC data and is the official methodology adopted by OECD in its report on PIAAC data (OECD 2013). Boxes 1 et 2 summarize the work of Pellizzari and Fichen (2013).

Perry et al. (2016) extend the OECD method, through two channels:[29] they first increase the number of observations for defining the required level of skills per occupation, which allows them to categorize the skill level requirement at a finer level. Then, the authors consider the whole sample instead of the one of well-matched workers only. They argue that skill levels of workers who declare being well-matched in PIAAC do not importantly differ from the one of workers who report to be unmatched. Though it raises the sample size, it is unclear whether relying on the well-matched or unmatched workers is the more relevant.

Krahn et al. (1998) also rely on this methodology using the International Adult Literacy Survey (IALS), while Desjardins and Rubenson (2011) use the Adult Literacy and Lifeskills Survey (ALL). Importantly, they do not consider the skill level of individuals but their skill use. The realized approach is still implemented, in the sense that the authors compare the skill use distribution of self-declared well-matched individuals to each individual assessment of skill use.

Comparison of skills level to skills use

An alternative option has been proposed by Allen et al. (2013), who chose not to rely on any self-assessment of workers' mismatch. They subtract each measure

[29]The authors focus on numeracy skills mismatch, as numeracy skills are considered to be more comparable across countries.

of skill use from the corresponding measure of skill level, by creating a common standardized index. When the difference is null or relatively small, the individual is considered as well-matched. Said differently, an individual is considered to be badly-matched when he does not fully make use of his skills at work, or when he intensively uses some skills at work that he insufficiently masters.

Table 1.3 summarizes studies relying on the realized approach and the comparison of skills level to skills use.

Table 1.3: **Realized match approach**

Name	Data	Sample Characteristics	Methodolgy (to identify skill mismatch)	Result	Advantages/Disadvantage
Krahn et al. (1998)	Canadian data on International Adult Literacy Survey (IALS)	Canadian individuals aged 16-65 years old. Literacy only	*Skill use.* Respondents were classified into one of five levels in literacy. The approach combines the observed skills and skill use variables to arrive at four match and mismatch categories : low-skill match, high-skill match, deficit mismatch and surplus mismatch.	About 43% of Canadian workers with high level document literacy had a literacy surplus (i.e. over-skilled), whereas 15% of workers with low literacy level were classified as literacy deficit (i.e. under-skilled).	Skill use is not same as required skill level. Still arbitrary cut-off.
Desjardins et al. (2011)	Adult Literacy and Lifeskills Survey (ALLS)	16- to 65-year olds in participating countries. Both Numeracy and Literacy	*Skill use.* Same approach as above	Across countries, the proportion of literacy and numeracy mismatches were around 31-41% and 35-52% respectively, of which the skill deficit (under-skilled) constitutes 9-29% and 6-20% of mismatches respectively.	Skill use is not same as required skill level. Still arbitrary cut-off.
Allen et al. (2013a)	PIAAC	Paid employees (no students or apprentice/intern). Both Numeracy and Literacy	*Skill use.* 1) Create standardized skill use and skill level index in the scale of 5 for the skill domains of numeracy and literacy and 2) subtract each measure of skill use from the corresponding measure of skill level and 3) define those less than -1.5 as skill overutilized, and larger than 1.5 as skill underutilized	Literacy underutilization is associated with a wage penalty of around 11%, and overutilization with a wage premium of around 7%, against 4% and 5% respectively in numeracy.	Skill use is not same as required skill level. Still arbitrary cut-off.
Perry et al. (2014)	PIAAC	Full time, country by country (but focus on Germany, Austria and U.S.). Only Numeracy mismatch	*Skill level.* 1) Calculate the mean proficiency score for each occupation and 2) classify mismatch those beyond one standard deviation from the mean. Uses 10 different plausible values	Under-skilled, well matched and over-skilled ratio are: Germnay (7.4%, 87.2% and 5.37%), Austria (6.9%, 87.5%,5.6%), US (7.6%, 86.7%, 5.7%)	Arbitrary cut off. Existence of those simultaneously well-matched and mismatched (due to plausible values). 2-digit occupation and more samples.

Pellizzari and Fichen (2013) "A New Measure of Skills Mismatch: Theory and Evidence from the Survey of Adult Skills (PIAAC)"

Authors use PIAAC data. Building upon the theory and a number of assumptions, for each occupation, they construct a maximum level of skills and a minimum level of skills. A worker is said over-skilled if his/her skill level is above the maximum level, under-skilled if it is below the minimum level and well-matched if it is within the maximum and the minimum.

The following is the brief description of the theory. They assume that a worker i endogenously chooses his (exertion of) skill level s_i at work and he needs to pay a cost to deploy (exert) skills c_i which is zero below his skill endowment η_i, but increases constantly above the skill endowment. Each job j has a production function, with the only input being a single worker, which produces an output y_{ij} as a function of s_i. The output begins with $-k_j$, with k_j being the fixed cost for production, and increases constantly up o the threshold (the maximum) beyond which marginal production is null. The production function is assumed to have local linearity, fixed operational and discontinuously declining fixed cost.

A worker's utility is given as:

$$U_i = w_{ij} - 1(y_{ij} < 0).F - c_i(s_i)$$

where F is the (large enough) fixed cost he incurs when he does not deploy sufficient skills and produces negative output. Wage is determined by $w_{ij} = \gamma_i y_{ij}$ as in the bargaining model.

The output is decided according to:

$$y_{ij} = \begin{cases} \beta_j s_i - k_j if s_i \leq max_j \\ \beta_j max_j - k_j if s_i > max_j \end{cases}$$

with β_j being the constant marginal production.

Pellizzari and Fichen (2013) "A New Measure of Skills Mismatch: Theory and Evidence from the Survey of Adult Skills (PIAAC)" (cont'd)

With this model, the mismatch is defined in the following way :

- A worker i is well-matched for a given job j if $min_j \leq \eta_i \leq max_j$. He is at optimal by choosing $s_i\star = max_j$.
- A worker i is under-skilled for a given job j if $\eta_i \leq min_j$. Assuming that the F is sufficiently large, he chooses to exert $s_i\star = min_j$ (to avoid the payment of F).
- A worker i is over-skilled for a given job j if $max_j \leq \eta_i$. Since the output does not change above max_i, he chooses $s_i\star = max_j$.

In order to estimate the empirical threshold skills min_j and max_j using PIAAC, the authors make an additional assumption that jobs are homogeneous within an occupation. They use the level of skills estimated (literacy and numeracy) and the self-report by workers regarding the skill mismatch also available in PIAAC. Then, those who answer yes to the question "Do you feel that you have the skills to cope with more demanding duties than those you are required to perform in your current job?" are regarded as over-skilled workers, and those who answer yes to the question "Do you feel that you need further training in order to cope well with your present duties?" are regarded as under-skilled workers, and those who said no to both of questions are regarded as well-matched workers. Then, assuming that jobs are homogeneous in an occupation, one can say that the lowest level of skills possessed among those who answered no to both of questions is $\widehat{min_j}$ and the highest level of skills possessed among them is $\widehat{max_j}$. The strength of this method is that one does not need to create any indicator for skill use at work, yet one can estimate the extent of skill mismatch by using the estimated values of min and max. On the other hand, several strong assumptions lie in the theory and moreover the methodology does not allow identifying the cause of the mismatch. In addition, since they use only a portion of respondents in estimating the minimum and maximum level of skills for each occupation, they ended up using 1-digit code to define an occupation, which ignores a lot of heterogeneity within an occupation.

1.5 Limits of existing measures of skill mismatch

1.5.1 Limitations of the self-reporting and realized approaches

Limitations of the self-reporting approach

Worker's self-assessment of skill mismatch has the advantage of being easily implementable in a survey. However such results rely on the hypothesis that individuals truly assess the skill level required for their job and/or the extent to which their own skills match this requirement. Hartog (2000) highlights that measurement bias can arise as respondents are likely to overstate their job requirements and to exagerate the status of their position. On the contrary, they might also overestimate their own level of skills, which will lead to measure a higher rate of overskilling than the real one.

A weakness of indicators of skill mismatch based on workers' self-assessment is that they seem difficult to compare across countries (despite the common practice to do so). This is the case for two reasons. First, differences in cultural traits across countries are likely to affect the way individuals consider their jobs and assess their own abilities. The problems highlighted by Hartog (2000) mentioned above are likely to impact countries differentially, making any comparison difficult. The second issue is more practical as it relates to translation problems in the questions used to build the mismatch indicators. The exact meaning of the questions might differ from one translation to another. As an example, the PIAAC question "Do you think you have the skills to cope with more demanding duties than those they are required to perform in their current job?" is translated in French by "D'après vous, êtes-vous assez compétent(e) pour exercer des fonctions plus exigeantes que celles qui sont actuellement les vôtres ?"[30] while the question "Do you think you would need further training in order to cope well with your present duties?" is translated by "Pensez-vous avoir besoin d'une formation supplémentaire pour vous sentir à l'aise dans vos fonctions actuelles ?"[31]. Despite all efforts made to get the

[30]There is a slight difference between "having the skills" and "being competent".

[31]"Coping well with your present duties" is not fully similar to "feel comfortable with your current

best possible translations, this example illustrates that the words used in different languages keep having slightly different meanings.

Limitations of the realized approach

Indicators relying on the realized approach allow to measure quantitatively the extent of skill mismatch in a given occupation. Said differently, this method provides the advantage of measuring the distance of an individual's skills to the average level required. However the realized approach suffers from important limitations. The main one it that it still relies on workers' self-assessment as the primary source of information to identify mismatch. In a way this measure seems tautological, as it consists in comparing individuals' level of skills to an indicator they have contributed to build. It is therefore subject to the same problems as above, in particular regarding international comparisons. Second, in several studies using the realized approach thresholds are arbitrary.[32] Finally, the hypothesis that the skill use is a relevant proxy for an individual's real level of skills is often made.

It also has to be noted that this methodology can only be implemented on PIAAC data, as the latter provides information on individuals' skill use and feeling of adequacy with their job.

Contradictions between indicators

Finally, the self-testing and realized approach have their own limitations, and they do not lead to the same conclusions. When applied to overeducation assessment, McGuinness et al. (2017) highlight that the two latter approaches induce to assess different levels of qualification mismatch. As an example, Barone and Ortiz (2011) consider the incidence of overeducation in Europe among university graduates, comparing both the realized approach to the subjective one: in Austria, they find that 9.6% of those graduates are overskilled with the first method while it would amount to 1.1% with the second method. It is also interesting to note that

duties".

[32] As an example, Allen et al. (2013) define that when the difference between skill use and skill level index exceeds more than 1.5, the individual is mismatched. Perry et al. (2016) classify as mismatched individuals beyond one standard deviation from the mean.

the assessed level qualification mismatch is not the same when measured in PIAAC and *OECD Skills for Job Database* previously mentioned. Relying on the latter indicator, qualification mismatch reaches 35.1% (in 2015) of workers in France while it amounts to 44.3% relying on PIAAC data. Surprisingly, the diagnosis changes from one source to another : under-qualification is an important issue in France according to the first source, as it reaches one of the highest levels in Europe (23.4% of workers), while PIAAC data measure a more than average rate of overqualification (31.3%).

Regarding skill mismatch in particular, its measurement in PIAAC, also sometimes seems at odds with objective measures of a country performance in terms of education and productivity. In France for example, the level of skill mismatch lies in the OECD average. In literacy, overskilling represents 7% of the population and underskilling concerns 4% of individuals, against on average 11% and 4%, respectively. At the same time, the level of literacy proficiency is significantly lower in France than in the rest of developed OECD countries. The difference between France mean score and the overall average is -17.3%, against -3.2% in Germany or -2.8% in the US. If the actual level of skills is so low in France, one may have expected French workers to feel more under-skilled. A reason why they could not feel underskilled could be that the demand for skilled workers is lower in France than elsewhere, implying that workers do not have to realize tasks that require advanced skills, and therefore do not feel underskilled despite their low average skill levels. However, France is not among the least developed countries in the OECD, and it might be unlikely that the demand for highly-skilled workers is much lower than elsewhere. A better explanation might be that French workers are more likely to feel overskilled for cultural reasons or because they did not interpret the questions they had to answer in the exact same way than workers in other countries.

Similarly, French workers declare to be overqualified more than workers in other European countries, putting France among the countries where overqualification is the highest (when measured from workers' self-assessment). However, the average number of years spent at school is not well above the OECD average in France. This means that the French workers have not been educated for a particularly long time, but nevertheless feel over-qualified. If their feeling is to be taken seriously, we should expect French workers to perform tasks requiring on average a lower qualification than in many OECD countries since their education is considered too good for those tasks but still not as good as in many other countries. This idea of French workers performing low-qualification tasks is then hard to reconcile with the fact that hourly labor productivity in France is among the highest in the world.[33] Again, a better explanation might be that French workers disproportionately consider themselves overqualified, without this feeling being linked to any clear labor market reality. The importance attached to diploma in France might also contribute to feel overqualified.

1.5.2 Limitations of the comparison between skills level and skill use

Compared to previous methods, the comparison between skills level and skills use allows not to rely on the self-reported measure of skill mismatch. However it is important to keep in mind that this indicator can still be biased. Indeed, when the worker's recruitment did not answer to the employer's need, the latter might still try to adapt the tasks realized by the worker to the individuals' skills rather than to the job requirements. In that case, the worker will use each skill at work exactly depending on his own level of skills, which will correspond to a "well-matched" situation according to the indicator we consider.

This argument can be used for previously mentioned indicators as well : for example an individual will report to be well-matched exactly because the employer adapted the job to the workers' skills. Again, it does not mean that the hiring need was filled.

1.5.3 What does each indicator measure?

Beyond the issue of the imperfections of each measurement method, one could ask the question of what is exactly measured through each approach. It appears that

[33] It ranged second in 2015 regarding work productivity, after the US.

each of them provides information on different channels leading to skill mismatch.

For example, indicators providing information on the mismatch between job seekers' skills and hiring needs first capture information mismatch due to some labor market regulations or segmentation: a skill shortage in a specific field might be due to workers' geographical constraints, or to the fact that employers lack information regarding individuals' abilities. Such indicators might also reflect a gap between the educational level of the labor supply and skills required by the labor demand. The inadequacy might either be vertical or horizontal.

Second, the measured mismatch between workers' skills and jobs requirements may reflect the extent to which labor market regulations prevented employers and employees to perfectly match. For example, the cost of information regarding the workers' skills might lead the employer to hire an under-skilled worker. On the supply side, the cost of job search might provide an incentive to accept a job for which a worker is overskilled. In the same way as above, such skill mismatch indicators might also reflect a gap between the labor supply and demand if employers have to fill a vacancy with an underskilled worker because the global level of training is not sufficient for the required tasks.

As a result, one cannot link an indicator to a single source of skill mismatch, however combining both types of items should allow to better reflect the allocation of skills on the labor market. Moreover, focusing on one single indicator might lead to give too much credit to one picture of skill mismatch on the labor market, while each indicator has some limitations.

1.6 Conclusion

This chapter has reviewed the literature on skill mismatch and of the existing indicators of mismatch. It has shown the limitation of using workers' surveys such as PIAAC to measure skill mismatch. The realized approach methodology adopted so far to exploit PIAAC data has the advantage of providing information on the gap between the average and individual skills level for a given occupation. However it still suffers from important drawbacks, among which the strong assumption that declared well-matched workers did really match on the labor market. In practical terms, measured skill mismatch in PIAAC also does a very poor job in predicting wages, as shown in figure 1.1. This absence of a relationship between the skill mismatch indicator and wages also raises the question of the practical relevance of such an indicator.[34]

Surprisingly, there is no indicator of skill mismatch based on workers' surveys that combines the three sources of information available in such surveys: (i) workers' measured skills (in numeracy, literacy and problem solving in PIAAC), (ii) their declared use of these skills at work, and (iii) their self-assessed mismatch. Combining these three dimensions may help to limit some of the issues discussed earlier and provide more reliable measures of mismatch. In statistical terms at least, using more information should not hurt : relying on the job analysis approach previously mentioned, by linking occupations to skills, could also provide more insights about potential skill mismatch.

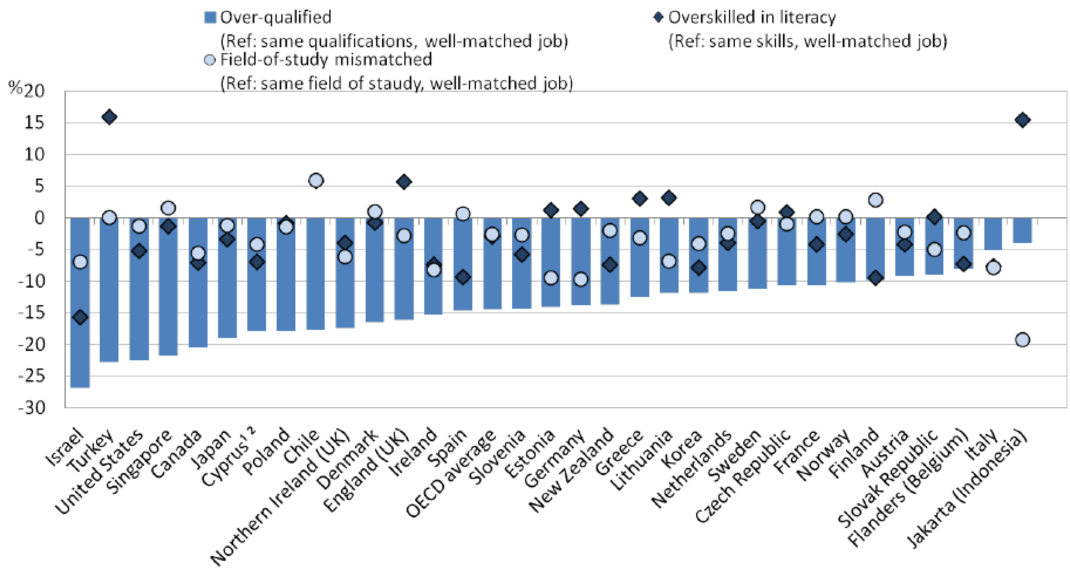
Finally, regarding potential consequences of skill mismatch, adequate policies should work on reducing it, whether it is driven by an imperfect matching or by a gap between the aggregate supply and demand. In the first case, smoothing labor market imperfections may allow workers' skills to match with those required in their job. Moreover, through human resources policies, firms have an important role for skill mismatch reduction. Indeed, OECD comparisons highlight the positive correlation between the adoption of better managerial practices and a lower level of skill mismatch (McGowan and Andrews 2015a). This calls for spending enough resources on recruitment in order to limit bad matches. Small firms might experience more frequently skill mismatch because they have limited funds for recruitment practices. They might also be less able to initially define clearly required skills, especially in innovative sectors.

If skill mismatch is mainly due to a gap between the aggregate supply and demand (which is hard to assess only based on PIAAC indicators), educational policies

[34]Figure 1.1 shows however that the association between wages and over-qualification is much stronger, suggesting that measures of qualification mismatch may be more related to actual labor market outcomes.

should be implemented in order to provide individuals with the skills demanded on the labor market. Beyond initial education, lifelong training should also improve the match quality on the labor market, whether it is implemented within a firm, externally, or while an individual is unemployed.

Figure 1.1: **Mismatch and earnings**



Source : OECD, 2016b

Chapter 2

Objectives of the empirical analyses, Data and Methods

We provide here context and motivation for the empirical analyses undertaken in the two following chapters. In a nutshell, the main objective of the empirical work is to provide a quantitative assessment of the relationships between the measures of general skills provided in PIAAC and labor market outcomes such as earnings and employment. After presenting the motivation, the chapter presents the PIAAC survey. It concludes with a non-technical overview of the methods and approaches used in the two following empirical chapters, highlighting their benefits and limits. The details of these methods are provided in the corresponding chapters.

2.1 Motivation for the empirical analyses

The previous chapter reviewed the challenges related to measuring skill mismatch and relating measured mismatch to a specific cause (gap between the aggregate supply and demand of skills, matching frictions, market segmentation, labor market regulation, etc.) that may be tackled with adequate policies. Some of the measures of skill mismatch are based on workers' surveys such as PIAAC. They combine information on workers' general skills in numeracy, literacy and problem solving, their feeling of being mismatch, and/or the extent to which they use different skills at work. These measures have several weaknesses that shed doubt on their usefulness, in particular for cross-country comparisons of skill mismatch.

Though those data might not be perfectly suited to measure skill mismatch, the information on the skills available in the working age population in a given country might have some value in itself. One may want to compare the level of skills across countries, or understand how an individual's skills may explain her labor market outcomes. Such an understanding could in turn be useful to design policy, for example for the design of training programs likely to upgrade the skills that are the most relevant for career outcomes.

Leaving aside skill mismatch, the availability of measures seems to offer a great opportunity to understand the determinants of labor market success, both at the individual level (which skills have the most successful workers?) and country level (which skills are the most widespread in countries where labor productivity is the highest?). This however comes at a direct cost as it requires to ask adults to take lengthy tests.[1] It is financially costly to administer these tests, and the time spent being tested also represents an opportunity cost for the surveyed workers as they could use this time for other tasks. In practical terms, acquiring information about an individual's education takes a few seconds, while getting measures of her skills may take hours. This simple observation leads us to a first research question: what is the predictive power of measured skills on labor market outcomes once one has controlled for workers' education? In other words, is there any value added to have measures of skills once one has already collected information on diplomas? If the answer is negative, we may conclude that it is not worth paying the cost to measure adults' skills.

Even if general skills matter for labor market outcomes on top of diplomas, they may be very difficult to acquire at adult age. In that case, the role of skills on the labor market could be interesting to researchers but of limited relevance for policy makers. One reason to suspect that the skills measured in PIAAC may be difficult to modify is that they are very general, and typically acquired very young, implying that training workers at adult age might not be very efficient to improve such skills.

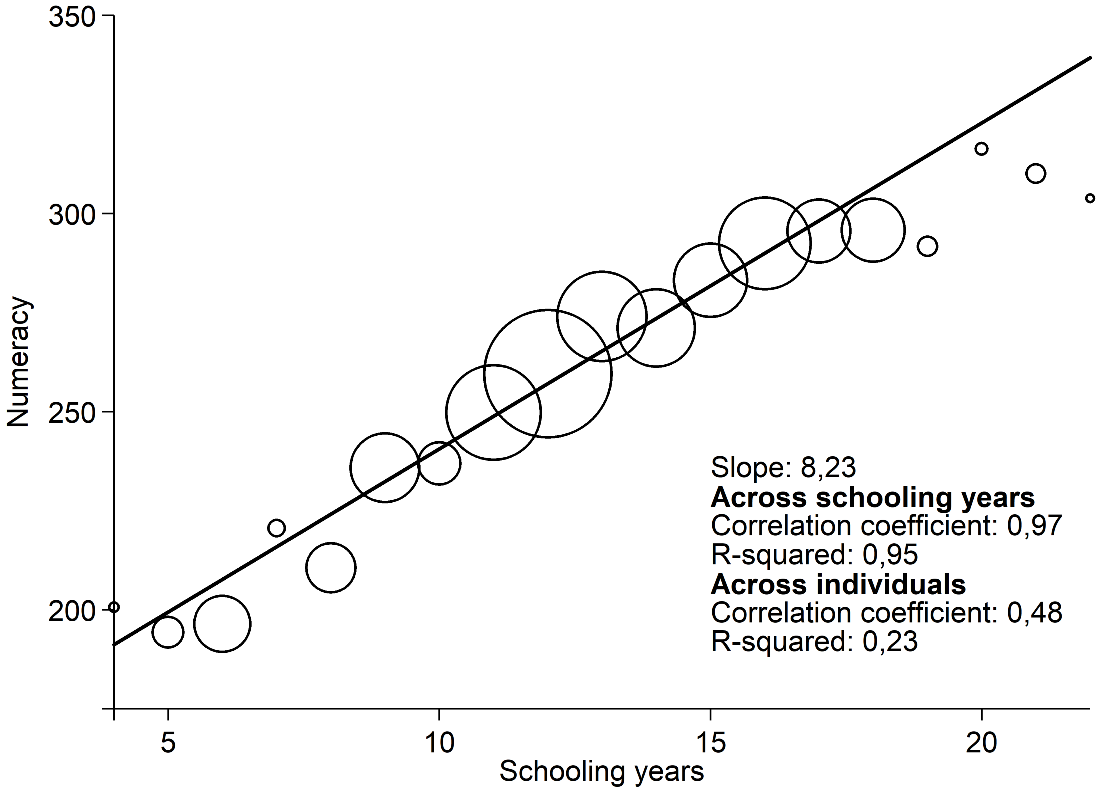
[1]It is also subject to several challenges as adults may be reluctant to be tested, implying that they only put limited effort during tests, or under-perform in formal test conditions as they are no longer used to take tests. Such behaviors may vary across countries, making cross-country comparisons of skill levels difficult as well.

It is however more likely that initial education affect such skills. This is the second question we ask: what are the returns to education in terms of general skills? If we do not find any significant return, we may conclude that these measures of skills are virtually impossible to modify and not policy relevant. In the opposite case, we would conclude that adequate school policies, and possibly on-the-job training policies as well, can improve skills that may be relevant for the labor market.

In chapter 3, we start by looking at the second question: to what extent are general skills in numeracy and literacy learned at school? We then turn to the first question in chapter 4: are those skills that are possibly acquired at school really relevant in the labor market? All together, the following chapters should shed light on the role of general skills in explaining labor market trajectories and on the policy relevance of such measures of skills.

Chapter 3 will also contribute to the literature on the returns to education. As is illustrated in figures 2.1 and 2.2, there is a strong positive linear relationship between the number of years of education and skills in numeracy or literacy. This correlation may arise because education has a causal effect on skills, or because more able people self-select into longer studies. The former explanation is consistent with the Beckerian view of education as a way to accumulate human capital, while the latter would give some leeway to theories that consider education and diplomas as signals of pre-existing abilities (Spence 1973). Chapter 3 offers a strategy to isolate the share of the correlation between individuals' education and skills that reflects a causal effect of education on skills. Doing so, it may allow us to interpret the non-causal part of this correlation as the signal-component of education. Of course, the whole exercise relies on a few specific measures of skills, and a limit is that skills acquired at school, or skills that individuals signal by going to school may be in part different from the skills measured in PIAAC. The study of the relationship between skills measured in PIAAC and labor market outcomes in chapter 4 will be useful to discuss this possible limit as it may suggest that these skills are relevant for labor market outcomes, and therefore something workers are willing to learn or signal.

Figure 2.1: **Correlation between the numbers of years at school and numeracy scores.**

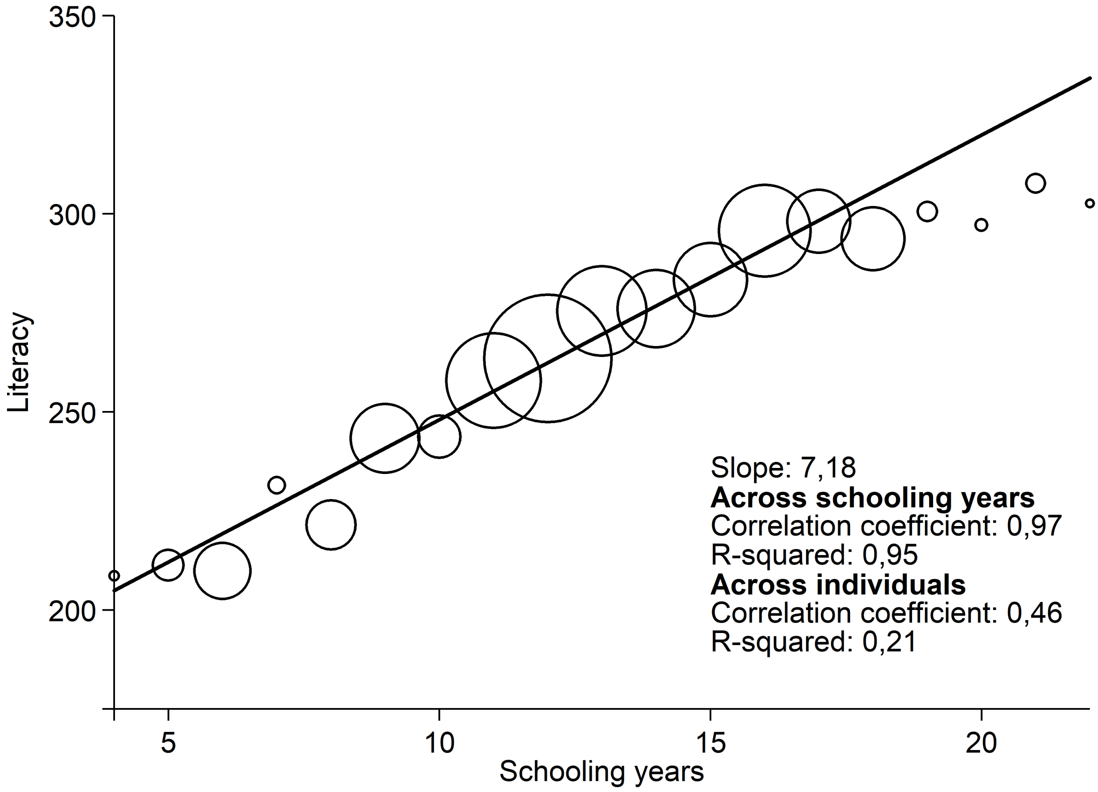


2.2 The PIAAC survey

Our study relies on the Programme for the International Assessment of Adult Competencies (PIAAC). This survey was developed by the OECD in order to enhance the comparability across countries of data on adults' skills. This survey primarily aims at assessing key cognitive skills, in three domains : numeracy, literacy, and problem solving in technology-rich environments. We focus on the two first measures in what follows as problem solving scores are available for way less countries. To get results that are easier to interpret quantitatively, we have sometimes standardized skills to have mean zero and standard deviation one. When we do so, one standard deviation in numeracy skills corresponds to about one out of five proficiency levels in PIAAC and it roughly amounts to twice the learning difference between school-attending PIAAC respondents in lower secondary and upper secondary education.

Two successive rounds were administered, Between August 2011 and March

Figure 2.2: **Correlation between the numbers of years at school and literacy scores.**



2012 the first wave of PIAAC data was collected, which produced data on 23 countries, mostly from the OECD (see OECD 2013). The second wave of data was collected between April 2014 and March 2015 ; it included nine additional countries, among which non-OECD countries and new members to the OECD, which extended the sample to 32 countries (OECD 2016b). PIAAC data provide larger samples than previous surveys such as IALS, as around 5,000 individuals were surveyed in each country for the PIAAC survey.

Adults between 16 and 65 are interviewed at home in their native language. Individuals were supposed to answer questions on a computer, though pencil-and-paper survey was possible for those with insufficient computer knowledge. Additional information is also available regarding individuals' education, income, labor-market status, experience or demographic characteristics. It is worth noting that the age is usually provided for each individual, though the OECD provided us an improved version of the survey with each individual's birth year and quarter. It allows us to adopt a more refined identification strategy in chapter 4.

The PIAAC survey measures literacy, numeracy and "problem solving in technology rich environments" skills, however our analysis focuses on the two first components. Literacy is defined in terms of reading of written texts and does not include the ability to write, as it is harder to assess this skill and to compare in international comparative perspective. Typically, individuals read either texts stored as digital information or print-based texts. Questions asked assess to which extent the individual is able to access to an information, to interpret it and to relate it to another information (for example by assessing the credibility of a text). To provide more detailed information about adults with poor literacy skills, a test of "reading component" skills is included, which aims at assessing knowledge of vocabulary or the fluency in reading passages of text. Numeracy tests measure the individuals' ability to "access, use, interpret and communicate mathematical information and ideas, in order to engage in and manage the mathematical demands of a range of situations in adult life" (OECD 2016c). Concretely, the survey assesses the individuals' ability to identify mathematical information, to use it in calculations and to interpret mathematical information, in a graph for example.

2.3 Methods

To assess the causal effect of education on general skills, we use an identification strategy that is standard in the literature on the returns to education. The strategy exploits reforms that increased the compulsory schooling age. These reforms are of course not retroactive and only concern individual born after a specified cut-off date. The identification strategy consists in comparing individuals born just before and just after this cut-off date. The former are subject to the old compulsory schooling age, while the latter face the more recent one and are obliged to stay at school longer. Around the cut-off, these treated and control individuals are likely to be otherwise similar, as they are born virtually at the same time and parents do not target very precisely their children birth date, and are unlikely to do it in reaction to compulsory schooling regulations.

In practice, the comparison of people around the cut-off date is implemented using two standard Regression Discontinuity Design (RDD) techniques. These techniques exploit the fact that individuals' average time spent at school should be a discontinuous function of birth dates due to the reform, as the reform forces individuals born after the cut-off date to stay longer at school. This exogenous change in the time spent at school serves as an instrument for the measured individuals' skills. Intuitively, the idea is to consider the function that relates individuals' average skill level to their birth date, and to look for a discontinuous variation in this function at the date where the reform of compulsory schooling kicks in. Such a discontinuity may be attributed to the reform. By comparing the effect of the reform on the number of years spent at school and on the average skill level (the size of the two estimated discontinuities), one can get a local estimator of the causal impact of a one additional year of education on general skills.

The method has however limits that need to be mentioned. First, the reforms do not affect all individuals but only those who would have left school before the new compulsory age. These individuals would have typically left school between 14 and 16 years old. What is identified with the RDD is the effect of education on skills for those individuals, and for a year of lower secondary education. This is not necessarily a big issue as mathematics and reading are in most countries the central topics studied at school at that age, implying that we perhaps have a well suited design to identify a causal effect of education on skills in numeracy and literacy. However, one should keep in mind that a year of tertiary education may have a different effect on skills. Similarly, the causal effect of education on skills may be different for individuals who would have pursued schooling anyway (who have not been affected by the reform).

The second limit of RDD is that it captures a local effect, in the sense that the effect of schooling on skills is only estimated for people born around the birth date at which the reform starts to apply. It cannot directly be extrapolated for people born long before or after this date. In particular, one should keep in mind that skills are measured in PIAAC between 2011 and 2015 (see above) whereas the cutoff dates of the reforms of compulsory schooling we exploit in nine OECD countries vary between 1949 for Italy and France and 1969 for Belgium. This implies that the individuals used to identify the causal effect of education on skills are relatively old when their skills are measured (around 65 for Italian, and around 45 for Belgium). They may have acquired general skills at school and partly lost them afterward, for example because they invested in other skills more specific to their job.

To measure the ability of skills to explain labor market outcomes such as employment status and wages, we use simple variance decomposition techniques in chapter 4. More specifically, we assess the share of the inter-individuals variance in wages or employment status that can be explained by measures of skills conditional and unconditional on education. These techniques are descriptive, and do not identify a causal effect of skills on labor market outcomes. Skills and wages may for example be both explained by an omitted variable such as the actual tasks executed at work. It may be that high-paying jobs rely more on some general skills which are enhanced on the job by the fact that they are often used. Unfortunately, we do not have any exogenous source of variation in skills. Indeed, compulsory schooling reforms may influence the acquisition of skills, but they primarily impact initial education, so that a causal effect of skills on labor market outcomes cannot be identified independently of education.

Chapter 3

The effect of schooling on skills: A causal analysis using mandatory schooling reforms

3.1 Methodology

3.1.1 Objective

The objective of this chapter is to estimate the effect of schooling on the general skills measured in the PIAAC survey as well as on labor market outcomes.

The challenge in identifying such an effect lies in the fact that the skills measured in PIAAC might not only be related to school—if they do ever—but are also likely to be related to underlying abilities that allow to achieve higher education. This two-way relationship between skills and schooling calls for a research design that allows to identify a causal effect.

3.1.2 Identification strategy

In order to estimate the causal effect of schooling on measured skills at adult age, we take advantage of exogenous changes in schooling induced by mandatory schooling reforms in different countries. These reforms only apply to some cohorts. In each country, individuals born after a defined date are legally obliged to attend school longer than older cohorts. By comparing outcomes between cohorts on each side of the reform's cutoff, a regression discontinuity design makes it possible to identify locally the causal effect of schooling as the increase in completed schooling from one group to another was exogeneously imposed rather than chosen by children or their family.

Several caveats appear in the application of such a research design to mandatory schooling reforms. First, such reforms might not apply fully to targeted cohorts, or they may be poorly enforced. This implies that not all children born after the cutoff date may actually attend school until the new compulsory schooling age. Second, most of the children born around the cutoff date would in reality have attended school beyond the new compulsory schooling age. As a consequence, only children who would not have attended school longer than the previously requested length actually experience a significant increase in schooling. For other children, this increase is mitigated by the fact that they pursue longer studies.

Second, these reforms usually took place in the second half of the twentieth century in a general context of increasing educational levels. In other words, average schooling was steadily increasing from one cohort to the next. Finally, reforms of mandatory schooling primarily affect intermediary educational levels.

The literature retains two main approaches to estimate a treatment effect in regression discontinuity designs: the local polynomial approach and the local randomization approach (see Imbens and Lemieux 2008, Lee and Lemieux 2010 and Cattaneo et al. 2018a,b among others).

3.1.2.1 Local polynomial approach

The local polynomial approach consists in using only observations that are located within some bandwidth h around the cutoff value that determines which cohorts are supposed to experience longer schooling than others. As a first step, we calculate the difference in completed schooling years between cohorts that are affected by a reform and those who are not. This is achieved in three steps. First, we construct the assignment variable, q, as the difference between a cohort's and the first affected cohort quarterly birth date in each country. Second, we estimate the local relationship between q and completed schooling years via local estimations of order-p polynomials on both sides of the cutoff. The treatment effect is then recovered by calculating the difference between the value of these polynomials at the cutoff. This is the difference between the intercepts of the left- and right-side polynomials as we have normalized the value of q to be 0 at the cutoff. In other words, we estimate the two polynomial relationships:

$$\text{Schooling}_i = \sum_{j=0}^{p} \beta_j^- (q_i)^j + \varepsilon_i, \text{ if } 0 < q_i < h_{\text{max}}, \tag{3.1}$$

and

$$\text{Schooling}_i = \sum_{j=0}^{p} \beta_j^+ (q_i)^j + \varepsilon_i, \text{ if } h_{\text{min}} < q_i < 0, \tag{3.2}$$

and retrieve the increase in schooling at the discontinuity thanks to $\beta_0^+ - \beta_0^-$.

This step provides what is usually referred to as a **first-stage estimate**. It measures the extent to which reforms of compulsory schooling length have indeed increased schooling duration. Absent of a significant first stage, there is little hope to detect an effect of a reform on outcomes that it did not directly target. In the opposite case, one can directly try to estimate the effect of compulsory schooling reforms on the outcomes of interest such as skills. This is simply done by replacing schooling by the variables of interest in equations (3.1) and (3.2). Such estimates are called **reduced-form estimates** and provide the direct effect of the reforms on the outcomes of interest. An alternative approach is to use the reforms as an instrument to study the causal effect of schooling on skills or labor market outcomes. This can simply be done by dividing the estimated effect of these reforms on the outcome of interest by their estimated effect on schooling duration. Doing so, one obtains a local estimator of the causal effect of one additional year of schooling on the outcome of interest. Instead of rescaling regression coefficients, one can also obtain these causal effects directly from a standard **two-stages instrumental variable approach**. This approach consists in estimating directly by OLS the relationship between the outcome of interest and schooling duration instrumented by the reforms (in a pooled version of equations (3.1) and (3.2)).

The local polynomial approach imposes to select the order of the polynomial that will be used, as well as the bandwidth within which the former will be fitted. Both choices hinge on trade-offs. As for the choice of p, low-order polynomials provide low-quality fits but high-order ones give too much weight to outlaying observations. We follow the literature by using first and second order polynomial forms (Cattaneo et al. 2018a). The choice of the window around the discontinuity results from a trade-off between two opposing forces (Imbens and Lemieux 2008, Lee and Lemieux 2010). On the one side, the possibility to identify separate trends before and after the reform diminishes together with the number of observations as the window size shrinks. On the other side, the comparability across cohorts drops as the window size increases along with the inclusion of younger and older cohorts. We address this bias-variance trade-off by using Cattaneo et al. (2018a) methodology to optimally set two different bandwidths on each side of the first affected cohort in each country. This approach consists in selecting bandwidths that minimize the mean squared error of local polynomials on each side of the cut-off.

3.1.2.2 Local randomization approach

The above described local polynomial approach is based on assumptions of continuity of the assignment process around the treatment cutoff. Such assumptions might not hold in cases where the assignment variable essentially takes discrete values and where many observations share the same assignment value. This is actually the case with schooling reforms we will investigate given that we do not observe the exact birth date, but only the year and quarter of birth. This implies that several individuals are assigned to the same "birth date group" on each side of the cutoff date, making the continuity assumption not fully satisfied. The local randomization approach allows to relax continuity assumptions and takes advantage of the a priori random allocation of observations on each side of the cutoff.

Following the local randomization approach, the treatment effect can be retrieved by comparing the mean outcome of observation just before and just after the allocation threshold. By selecting a window around the threshold within which observations are otherwise similar, the difference in means is an unbiased estimate of the treatment effect. Using completed schooling years as outcome, this approach again allows to estimate the increase in schooling due to the reform in each country. As previously described, this first-stage effect can then be used as an instrument to uncover the effects of schooling on skills or labor market outcomes in a two-stage procedure. Alternatively, reduced form estimates of these effects can be obtained by swapping schooling for the variables of interest in the first-stage.

The local randomization approach mostly requires to select the window around the cutoff within which observations will be compared. This choice again triggers a trade-off between comparability and statistical power as the larger the window, the higher the statistical power but the less distant cohorts can be considered as similar as they might be affected by different shocks. Cattaneo et al. (2018b) recommends to use an iterative procedure to select the optimal comparison window. This procedure consists in choosing the window w so that it is the largest window around the cutoff in which covariates are balanced in this window and in all the smaller ones.

This window-selection procedure essentially necessitates to select covariates that are a priori not affected by the reform or for which we want to make sure there is not any difference across the threshold. Accordingly, we select PIAAC respondents' gender and parental education as covariates.

3.1.3 Sample selection and key variables definition

The objective of this study and the above described identification strategy impose a number of constraints on the choice of PIAAC countries to be included in the sample. First, included countries must have implemented a reform that changed the length of mandatory schooling; second, labor-market situation information—among which employment status and wage—must be available from the PIAAC survey; and finally, cohorts affected by the reform must be sufficiently old for education to be terminated by the time of interview. Starting with reforms surveyed by Brunello et al. (2009), the second constraint leads to the exclusion of Austria and Sweden. Similarly, the third constraint leads to exclude Poland whose 1999 reform is too recent. All in all, 9 European countries where included in the sample. Table 3.1 displays the list of countries included in the sample, together with brief descriptions of key features of the investigated reforms.

Table 3.1: **List of countries included in the sample.**

	Reform year	Birth quarter of first affected cohort	Change in mandatory schooling length
Belgium	1983	1969, q1	from 8 to 12 years
Denmark	1971	1957, q1	from 7 to 9 years
France	1959	1953, q1	from 8 to 10 years
Greece	1975	1963, q1	from 6 to 9 years
Ireland	1972	1958, q1	from 8 to 9 years
Italy	1963	1949, q1	from 5 to 9 years
Netherlands	1975	1959, q4	from 9 to 10 years
Spain	1970	1957, q1	from 6 to 8 years
United Kingdom	1972	1957, q4	from 10 to 11 years

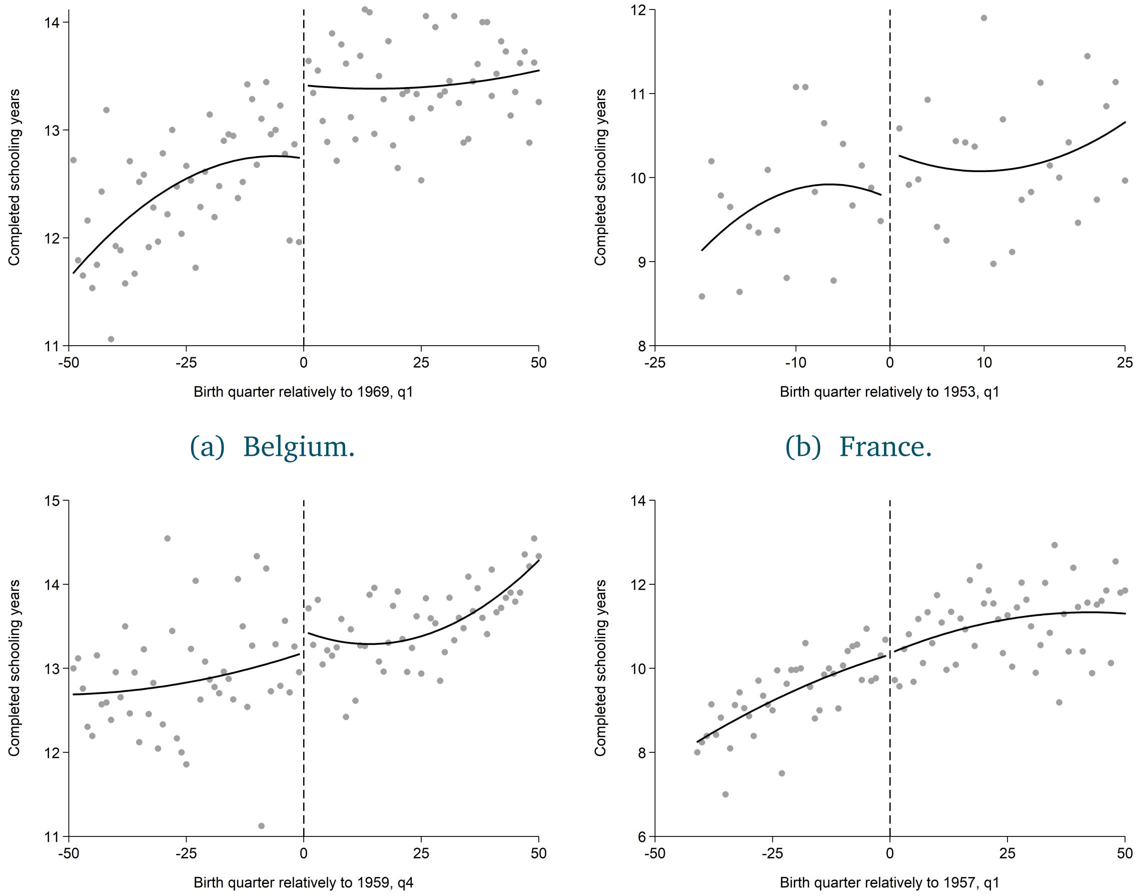
Source: Brunello et al. (2009). $q1$ and $q4$ stand for first and fourth quarters, respectively.

In this study, we measure skills using the first plausible values of numeracy and literacy skills as measured in the PIAAC survey. We construct individuals employment status using information about the activity they report over the last week. Namely, we consider as employed any individual who report that she was working or that she was away from job but will return. Finally, we construct the log of wage as the log of reported hourly earnings excluding bonuses corrected for purchase power parity.

3.2 Results

Figure 3.1 provides a general visual representation of the identification strategies by plotting the average completed schooling years for each quarterly birth cohort in Belgium, France, the Netherlands and Spain. Sub-figures are supplemented by adjusted fits using second order polynomials on each side of the cutoff. Visual inspection of the data suggests that completed schooling time did increase in Belgium, France and in the Netherlands following reforms of mandatory schooling length. This contrasts with Spain where there seems to be no difference in completed schooling between affected and non-affected cohorts. The subsequent analysis will consist into optimally selecting the comparison window around the cutoff and precisely testing for differences in completed schooling.

Figure 3.1: **Difference in completed schooling years between affected and non-affected cohorts.**



(a) Belgium.

(b) France.

(c) Netherlands.

(d) Spain.

Source: PIACC survey. Each point represents a birth cohort as identified by the within-country distance to the first affected cohort (see Table 3.1). Lines are second order polynomial fits estimated on each side of the cutoff.

3.2.1 Local polynomial approach

3.2.1.1 Effect of mandatory schooling reforms on completed schooling years

Table 3.2 presents discontinuity estimates for each of the 9 countries included in the sample. For each country, we select the optimal bandwidths around the first affected cohort by minimizing the common mean squared error over the full pool of candidate observations, allowing for different numbers of selected birth quarters on each side of the cutoff. Estimated discontinuities in schooling years are bias-corrected using Calonico et al. (2017) methodology.

As shown by estimates tabulated in the top panel of Table 3.2, the method only reveals one positive and statistically significant increase in completed schooling years (Belgium) when using first order polynomial adjustments. Reforms conducted in three other countries (France, Italy and the Netherlands) also seem to be associated with increasing completed schooling but uncovered estimates are not statistically significant at conventional confidence levels. As for the five other countries (Denmark, Greece, Ireland, Spain and the United Kingdom), data do not allow us to identify, nor to suspect, a positive association between reforms and completed schooling years. The bottom panel of Table 3.2 shows that the aforementioned findings persist when using second order polynomial adjustments on both sides of the threshold.

Estimated discontinuities must be interpreted as differences in completed schooling years due to the reforms. Their magnitude thus directly relates to the increase in schooling mandated by the reforms in the different countries (see Table 3.1). As a consequence, it is not surprising that the largest and most statistically significant discontinuity is estimated for Belgium as this country has implemented a four year increase in mandatory schooling for cohorts born after 1969. In contrast, uncovering a sizable discontinuity in completed schooling is more challenging in countries that implemented more modest changes in mandatory schooling. This *a priori* small jump is made hardly detectable in a context where the standard deviation of schooling amounts about 3 years in a typical investigated country.[1]

As a consequence, the estimated discontinuities in completed years of schooling presented in Table 3.2 lead us to immediately exclude five countries from the analysis: Denmark, Greece, Ireland, Spain and the United Kingdom. These are countries for which the approach proved unable to identify positive increases in completed schooling around reforms. The second part of the analysis will be performed on observations from the four countries for which we were able to identify a positive—although not always statistically significant—association between re-

[1]Restricting the sample to cohorts 10 quarters apart from the first affect cohort in each country, the standard deviation of completed schooling years ranges from 2.5 in Belgium, Denmark and the United Kingdom to 4.3 in Italy.

Table 3.2: **Estimated increases in completed schooling associated with mandatory schooling reforms: Local polynomial approach.**

Panel A: First order polynomial adjustments

	Optimal window				
	Left of cutoff		Right of cutoff		
	# of quarters	# of obs.	# of quarters	# of obs.	Discontinuity
Belgium	15	380	26	552	1.38 (0.004)
Denmark	10	483	37	1,095	-0.37 (0.346)
France	5	143	24	834	0.92 (0.416)
Greece	16	317	16	372	-0.87 (0.229)
Ireland	18	355	29	650	-0.27 (0.676)
Italy	10	239	36	752	0.28 (0.799)
Netherlands	20	470	23	614	0.42 (0.362)
Spain	16	398	17	447	-0.65 (0.345)
United Kingdom	9	219	29	951	-0.23 (0.650)

Panel B: Second order polynomial adjustments

	Optimal window				
	Left of cutoff		Right of cutoff		
	# of quarters	# of obs.	# of quarters	# of obs.	Discontinuity
Belgium	24	628	32	675	1.48 (0.009)
Denmark	18	841	42	1,238	-0.37 (0.405)
France	7	174	33	1,123	1.84 (0.246)
Greece	23	491	21	548	-1.28 (0.148)
Ireland	23	459	46	1,025	-0.22 (0.784)
Italy	11	239	51	1,035	0.34 (0.850)
Netherlands	26	623	41	1,053	0.49 (0.385)
Spain	19	465	29	787	-0.72 (0.399)
United Kingdom	13	365	42	1,513	-0.16 (0.801)

Bias-corrected robust p-values between parentheses. Each line displays the outcomes from a separate estimation using Calonico et al. (2017) methodology and allowing for different bandwidths on each side of the cutoff.

forms and completed schooling years: Belgium, France, Italy and the Netherlands.

3.2.1.2 Effect of additional schooling on skills and labor market outcomes

Table 3.3 displays reduced form and two stages estimates of the effect of one additional schooling year on literacy and numeracy skills for the four selected countries. The top panel uses first order polynomial adjustments on both sides of the reforms. Reduced form estimates allow to uncover positive effects of schooling on literacy skills in Belgium and Italy. These estimates are statistically significant at the 10% confidence level. The relation estimated for Belgium seems to persist using the two stages procedure, while the one of Italy does not as the first stage was not strong for this country. Numeracy skills do not seem to be positively associated with increases in schooling in any of the selected countries. The bottom panel of Table

Table 3.3: **Estimates of the effect of additional schooling on literacy and numeracy skills: Local polynomial approach.**

Panel A: First order polynomial adjustments

	Literacy		Numeracy	
	Reduced form	Two stages	Reduced form	Two stages
Belgium	18.09 (0.063)	10.57 (0.169)	8.24 (0.464)	4.25 (0.609)
France	1.93 (0.933)	-16.41 (0.769)	1.13 (0.967)	-11.91 (0.804)
Italy	24.34 (0.061)	120.09 (0.694)	14.45 (0.322)	72.41 (0.602)
Netherlands	2.32 (0.814)	6.20 (0.834)	5.64 (0.571)	12.35 (0.655)

Panel B: Second order polynomial adjustments

	Literacy		Numeracy	
	Reduced form	Two stages	Reduced form	Two stages
Belgium	15.86 (0.150)	8.28 (0.288)	7.38 (0.565)	3.64 (0.666)
France	-5.42 (0.868)	-2.17 (0.935)	-2.59 (0.948)	0.70 (0.981)
Italy	23.51 (0.254)	4.12 (0.996)	19.16 (0.410)	23.38 (0.962)
Netherlands	3.54 (0.747)	4.58 (0.846)	5.98 (0.588)	10.74 (0.651)

Bias-corrected robust p-values between parentheses. Each cell displays the outcome from a separate estimation using Calonico et al. (2017) methodology and bandwidths as selected from Table 3.2. First stages of two stages estimations are estimates displayed in Table 3.2.

3.3 uses second order polynomial adjustments and confirms results of the top panel as the only barely statistically significant increase in skills is the one estimated for literacy skills in Belgium. This estimate suggests that one additional schooling year is associated with an increase in literacy score that amounts to about 25% of the latter.[2]

Table 3.4 tabulates estimates of the effect of additional schooling on labor market outcomes. As for preceding estimations, both reduced form and two stage estimates using first and second order polynomial adjustments are presented. As shown by coefficients and associated p-values, the approach does not allow us to identify any positive effect of additional schooling on labor market outcomes measured as the probability to be employed and the (log of) wage conditionally on being in employment.

[2]Restricting the sample to cohorts that belong to the optimal bandwidth, the standard deviation of literacy skills is 41. Two stages estimates for Belgium amount 10.57 and 8.28 for one additional year of schooling depending on the polynomial adjustment order: $\frac{10.57}{41} = 0.26$ and $\frac{8.28}{41} = 0.20$. Similarly, reduced form estimates for Belgium amount 18.09 and 15.86 for 1.38 additional schooling year (see Table 3.2): $\frac{18.09}{1.38\times 41} = 0.32$ and $\frac{15.86}{1.38\times 41} = 0.28$.

Table 3.4: **Estimates of the effect of additional schooling on labor market outcomes: Local polynomial approach.**

Panel A: First order polynomial adjustments

	Employment		Wage (log)	
	Reduced form	Two stages	Reduced form	Two stages
Belgium	0.05 (0.530)	0.03 (0.565)	0.01 (0.926)	0.00 (0.959)
France	0.07 (0.756)	-0.23 (0.769)	-0.72 (0.526)	0.17 (0.825)
Italy	-0.11 (0.347)	-0.48 (0.874)	n/a	n/a
Netherlands	0.03 (0.639)	0.15 (0.512)	0.16 (0.161)	0.44 (0.891)

Panel B: Second order polynomial adjustments

	Employment		Wage (log)	
	Reduced form	Two stages	Reduced form	Two stages
Belgium	0.02 (0.825)	0.01 (0.849)	0.00 (0.970)	0.00 (0.968)
France	0.05 (0.864)	0.07 (0.804)	-0.88 (0.554)	-0.08 (0.806)
Italy	-0.10 (0.575)	0.08 (0.986)	n/a	n/a
Netherlands	0.05 (0.584)	0.13 (0.550)	0.12 (0.366)	0.64 (0.712)

Bias-corrected robust p-values between parentheses. Each cell displays the outcome from a separate estimation using Calonico et al. (2017) methodology and bandwidths as selected from Table 3.2. First stages of two stages estimations are estimates displayed in Table 3.2. The number of observation with wage data is insufficient to perform the estimation on the left of the cutoff in Italy.

Table 3.5: **Estimated increases in completed schooling associated with mandatory schooling reforms: Local randomization approach.**

	Optimal window			
	# of quarters	# of obs. (left)	# of obs. (right)	Discontinuity
Belgium	14	380	329	0.69 (0.000)
Denmark	Covariates balance test failed			
France	4	118	147	0.51 (0.285)
Greece	14	301	351	0.54 (0.063)
Ireland	Covariates balance test failed			
Italy	Covariates balance test failed			
Netherlands	Covariates balance test failed			
Spain	Covariates balance test failed			
United Kingdom	Covariates balance test failed			

P-values in parentheses. Each line displays the outcomes from a separate estimation using Cattaneo et al. (2016) methodology and respondent's gender and parents education as covariates. *Covariates balance test failed* means that the covariates balance test failed even for the smallest window around the cutoff.

3.2.2 Local randomization approach

3.2.2.1 Effect of mandatory schooling reforms on completed schooling years

Table 3.5 displays the outcomes of local randomization tests *à la* Cattaneo et al. (2016) using gender and parents education as covariates for the 9 countries included in the sample. Covariates balance tests fail for six countries (Denmark, Ireland, Italy, the Netherlands, Spain and the United Kingdom) even for the smallest window around the cutoff. In contrast, the local randomization approach allows us to uncover positive changes in completed schooling for three countries (Belgium, France and Greece) over which the next steps of the analysis can be performed.

3.2.2.2 Effect of additional schooling on skills and labor market outcomes

Table 3.6 displays reduced form and two stages estimates of the effect of one additional schooling year on literacy and numeracy skills for the three selected countries. Schooling is found to increase both literacy and numeracy skills in Belgium and Greece, although estimates are not statistically significant at conventional levels of confidence for Greece. As for Belgium, estimates' order of magnitudes suggest that one additional year of schooling increases literacy and numeracy scores

Table 3.6: **Estimates of the effect of additional schooling on literacy and numeracy skills: Local randomization approach.**

	Literacy		Numeracy	
	Reduced form	Two stages	Reduced form	Two stages
Belgium	8.92 (0.003)	13.00 (0.001)	7.95 (0.024)	11.47 (0.012)
France	0.99 (0.864)	1.66 (0.888)	-0.85 (0.900)	-1.99 (0.898)
Greece	4.83 (0.170)	8.96 (0.185)	4.73 (0.202)	8.77 (0.168)

P-values in parentheses. Each cell displays the outcome from a separate estimation using Cattaneo et al. (2016) methodology and bandwidths as displayed in Table 3.5. First stages of two stages estimations are estimates displayed in Table 3.5.

Table 3.7: **Estimates of the effect of additional schooling on labor market outcomes: Local randomization approach.**

	Employment		Wage (log)	
	Reduced form	Two stages	Reduced form	Two stages
Belgium	0.05 (0.047)	0.06 (0.065)	-0.01 (0.677)	-0.02 (0.699)
France	0.25 (0.000)	0.50 (0.273)	-0.09 (0.641)	-0.34 (0.864)
Greece	0.06 (0.152)	0.10 (0.209)	-0.05 (0.593)	-0.07 (0.672)

P-values in parentheses. Each cell displays the outcome from a separate estimation using Cattaneo et al. (2016) methodology and bandwidths as displayed in Table 3.5. First stages of two stages estimations are estimates displayed in Table 3.5.

by about 30 and 25% of their standard deviations, respectively.[3]

Local randomization estimates of the effect of additional schooling on labor market outcomes are displayed in Table 3.7. This approach reveals a positive but not very robust effect of schooling on the probability to be employed. In contrast, tabulated estimates suggest that longer schooling is not associated with higher wages conditionally on being in employment.

3.3 Conclusion

The two implemented approaches revealed partly successful in detecting significant increases in completed schooling that can be used to identify the effect of education on skills as measured in the PIAAC survey and labor market outcomes. This mitigated success in identifying first stage effects might be due to at least three

[3]Restricting the sample to cohorts that belong to the optimal bandwidth, the standard deviation of literacy (numeracy) score is 41 (46). The relevant estimates for Belgium amounts 13.00 $(11,47)$ for one additional year of schooling: $\frac{13.00}{41} = 0.32$ $(\frac{11.47}{46} = 0.25)$.

non-mutually exclusive reasons. First, actual increase in completed schooling from one cohort to the next might be small in some countries. The literature actually always reports average first stage estimates that are much smaller than the actual increases in mandatory schooling associated with reforms (see Table 3.1). This is mostly due to the fact that only a fraction of the population is actually hit by the new constraint. This results into lower statistical power which jeopardizes the identification of the treatment effect. While this issue could be partly alleviated by identifying the a priori most affected groups of children, such an approach is not implementable using data from the PIAAC survey as the latter contains too few observations per country (about 5,000 individuals).[4] Second, reforms of mandatory schooling may be accompanied by implicit or explicit changes in other schooling policies that may also affect the average time spent in education or make cohorts on each side of the cut-off not perfectly comparable.[5] Third, the PIAAC survey is not designed to match a representative sample of each country's population in terms of schooling achievement. These two latter issues are best illustrated by the fact that six out of the nine countries we surveyed failed to pass the covariates balance test of the local randomization approach. This strongly suggests that reforms also modified relative gender schooling and/or affected differently children from different social backgrounds. A conclusion that follows from these remarks is that it is difficult to identify an effect of compulsory schooling length on the skills measured in the PIAAC survey.

Given the above mentioned warnings, the two presented approaches portray an uncertain relationship between schooling and skills as measured by the PIAAC survey. There is however one country—Belgium—for which both methods allow us to identify a large first stage effect. Table 3.8 summarizes estimates of the causal effect of one additional year of schooling on literacy and numeracy skills, together with the raw estimate of the relationship that exist between schooling on skills

[4]Identifying the a priori most affected groups could be achieved using parents' education for example. However, a typical quarterly birth cohort included in the sample includes about 27 respondents, out of which only 6 have parents who completed higher education. Estimation and statistical inference would be very challenging to achieve using such small groups.

[5]Note also these other changes in schooling policies may also prevent us from identifying a pure effect of the increase in compulsory schooling length if they happen exactly at the same time.

Table 3.8: **Share of the schooling-skills relationship that can be attributed to the causal effect of schooling on skills, based on estimates from Belgium.**

	Causal effect of one additional schooling year			Within sample schooling-skills raw estimate	Share of schooling-skills correlation attributable to the causal effect		
	Mean estimate	95% confidence interval Lo. bound	Up. bound		Mean	Lo. bound	Up. bound
Local polynomial approach, first order polynomial adjustments							
Literacy	10.57	-4.46	25.61	8.59	123%	-52%	298%
Numeracy	4.25	-11.49	19.99	9.35	45%	-123%	214%
Local polynomial approach, second order polynomial adjustments							
Literacy	8.28	-6.87	23.43	8.78	94%	-78%	267%
Numeracy	3.64	-12.23	19.51	9.50	38%	-129%	205%
Local randomization approach							
Literacy	13.00	5.28	20.71	8.06	161%	66%	257%
Numeracy	11.47	2.51	20.42	8.79	130%	29%	232%

The *within sample schooling-skills raw estimate* is the coefficient of schooling year from an OLS regression of literacy or numeracy score on completed schooling years performed on interviewees that belong to the relevant method's optimal bandwidth as described in Tables 3.2 and 3.5 for Belgium. The *causal effects of one additional schooling year* are from two-stage estimations displayed in Tables 3.3 and 3.6. The last three columns of the Table divide the estimated causal effects of education on skills and their confidence intervals by the estimated correlation between schooling and skills.

within the optimally selected samples. These figures allow us to compare causal and raw estimates in order to provide some insight about the share of the schooling-skills relationship that can be attributed to the causal effect of schooling on skills. While most estimates are imprecise, converging average coefficients suggest that close to 100% of the relationship between schooling and literacy skills might be due to the causal impact of schooling on skills. As for numeracy, average estimates are more heterogeneous but do also suggest that 50 to 100% of the relationship between schooling and skills could be attributed to the causal effect of schooling. The local randomization approach provides more precise estimates and allows us to reject at the 5% significance level that less than 66% (resp. 29%) of correlation between schooling and literacy (resp. numeracy) skills at adult age reflects a causal effect of schooling on skills. Estimates based on the two other methods are however much less precise, making it hard to draw any conclusion.

As for the estimated impact of schooling on labor market outcomes, the two approaches consistently show that there is no effect on wage conditionally on being in employment. In contrast, we report uncertain and mild positive effect on the probability to be employed. Both findings are in line with the literature that generally finds either small or zero returns to schooling (see Meghir and Palme 2005, Oreopoulos 2006, Pischke and von Wachter 2008, Oreopoulos and Salvanes 2011, Grenet 2013 and Stephens and Yang 2014 among others). In addition, it is worth noting that the reported results could also be linked to the fact that our research design explicitly focuses on relatively old workers while it might be the case that schooling has more effect on labor market outcomes for younger ones.

Chapter 4

Labor market outcomes: What can we learn from skills?

4.1 Objectives

This chapter offers a quantitative look at skills as measured in the PIAAC survey in order to assess whether skills can help to understand individuals' labor market outcomes.

Table 4.1 illustrates the general relationship between wages, skills and schooling by displaying standardized coefficients from distinctly estimated wage equations. The dependent variable is the (log of) wage net of country fixed effects, the respondent's gender and parents education. The first three columns include PIAAC numeracy and literacy scores as explanatory variables. Standardized estimated coefficients portray the positive and statistically significant raw returns to skills. The fourth column uses the number of completed years of schooling as explanatory variable of interest. The associated standardized estimated coefficient depicts a positive relationship. It is larger in magnitude than that estimated using skills. Finally, we include both skills scores and completed schooling years as explanatory variables in the fifth column of Table 4.1. While the three estimated coefficients remain positive and statistically significant, they all drop in magnitude because of the correlation that exist among them, and not much overall explanatory power is gained from their simultaneous inclusion in a simple regression as shown by the successive R-squared statistics. However, estimates of the returns to skills experience a much larger relative drop than the one capturing the raw returns to schooling. This questions the informativeness of measured skills and naturally raises the question of whether there exists circumstances in which skills exhibit particularly low or high wage returns.

The work presented in this chapter complements approaches *à la* Quintini (2011b) or Branche-Seigeot (2015) who explore the relative returns to skills and education on the labor market, as done in Table 4.1. We contribute to the existing literature thanks to two complementary steps. First, instead of assuming these returns to be equal for all workers, we will compare how they vary during workers' career, as returns to skills and to schooling might differ depending on the labor market experience. The idea behind this first approach is that skills may not be directly observable, implying that employers have to rely on signals of these skills for their hiring, promotion and compensation decisions. However, skills may be revealed with working experience, and the available information available on CVs to assess a job seeker's skills increases with her previous work experience. As a consequence, if the skills measured in PIAAC are relevant on the labor market, and if the market is able to price and reward them, the returns to skills is likely to increase over the working life, whereas that of education may in contrast decrease if education is only an imperfect signal of workers' skills that becomes less and less important over the career path.

Table 4.1: **Standardized wage returns to skills and completed schooling years.**

Dependent variable: log of wage	(1)	(2)	(3)	(4)	(5)
Numeracy score	0.194		0.176		0.135
	(0.000)		(0.000)		(0.000)
Literacy score		0.168	0.021		-0.018
		(0.000)	(0.005)		(0.018)
Completed schooling years				0.268	0.231
				(0.000)	(0.000)
R-squared	0.037	0.028	0.037	0.070	0.083

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. White heteroskedastic standard errors in parentheses. OLS repressions. Each column displays estimates from a separate regression. $57,524$ observations. All regressions include a constant term The dependent variable is an individual's (log of) wage net of country fixed effects and of the following co-variates: gender and parents' education.

The second contribution of the chapter is to provide a more comprehensive quantification of the ability of skills and schooling variables to predict labor market outcomes. Indeed, we study the extent to which skills can explain variations in wages and employment status that cannot be explained by education (and vice versa). More importantly, we do it systematically for each country in PIAAC where measures of skills and education are available.

4.2 Relative returns to skills and education

4.2.1 Methodology

In order to investigate the relative role of skills and education in explaining differences in labor market outcomes, we start by estimating employment and wage equations for different age-groups. The general form of estimated expressions is as follows:

$$y_i = \alpha + \beta_{\text{Skill}}\text{Skill}_i + \beta_{\text{Education}}\text{Education}_i + \sum_{j=1}^{K}\gamma_j x_i^j + \mathbb{I}_{c(i)} + \varepsilon_i, \quad \text{if } i \in A(g), \qquad (4.1)$$

where α is a constant term, y_i is respondent i labor market outcome, Skill_i and Education_i are respectively measures of skills and education, x_i^j is some observable characteristic, $\mathbb{I}_c$ is a set of country fixed effects that account for average differences in employment and wages across countries, and ε_i is the error term. Finally, $A(g)$ denotes some age-group g created using a 7-year window around each candidate age: individual $i \in A(g)$ if $\text{a}_i \in [g-3, g+3]$. The set of observable characteristics includes respondent's gender, parents' education, labor market experience and age to further account for local age effects within age-groups.

We estimate expression (4.1) by including education and skills variables either separately or simultaneously. The comparison of standardized β_{Skill} and $\beta_{\text{Education}}$ across age-groups and depending on whether they are estimated separately or simultaneously will help us to assess the relative explanatory powers of skills and education.

The analysis is performed on all countries included in the PIAAC survey for which age and wage information are available. To ensure simplicity and tractability, skills are measured using PIAAC numeracy skills and education is simply captured by completed schooling years.

4.2.2 Results

Figure 4.1 displays estimated coefficients when using the probability to be employed as dependent variable. Plain lines plot the separately estimated standardized coefficients of education—measured as schooling years—and numeracy skills over the life-cycle. Three observations shall be made following the visual inspection of these lines. First, both variables gain in explanatory power over the first decade of an individual's life and loose in explanatory power by the end of her professional career. Second, the progressive decrease is less steep for numeracy skills than for schooling years and the former almost reach the explanatory power of the latter for senior individuals. Finally, the standardized coefficient of numeracy skills is close to zero for the youngest individuals, while the education variable still has a positive return for this group.

Dashed lines of Figure 4.1 represents the estimated standardized coefficients of skills and education when both variables are entered simultaneously in equation (4.1). While the overall structure of both dashed lines remains similar to the one of plain lines, their relative position with respect to the latter is informative. The gap between the plain and the dashed line for education is smaller than the one for numeracy. This finding suggests that, while the correlation between education and skills lower both variables' explanatory power, the one of skills drops dramatically more than the one of education.

Figure 4.2 reproduces the preceding analysis using (the log of) wage as dependent variable. Two facts are worth noting. First, the explanatory power of education does not exceed the one of skills for the youngest age-groups. Second, both variables do not experience any decline in their explanatory power for older groups. As for the relative evolutions of estimates when introducing variables separately or simultaneously in equation (4.1), Figure 4.2 conveys the same conclusions when using employment as the dependent variable.

Figure 4.1: **Relative returns to schooling years and numeracy for the probability to be employed.**

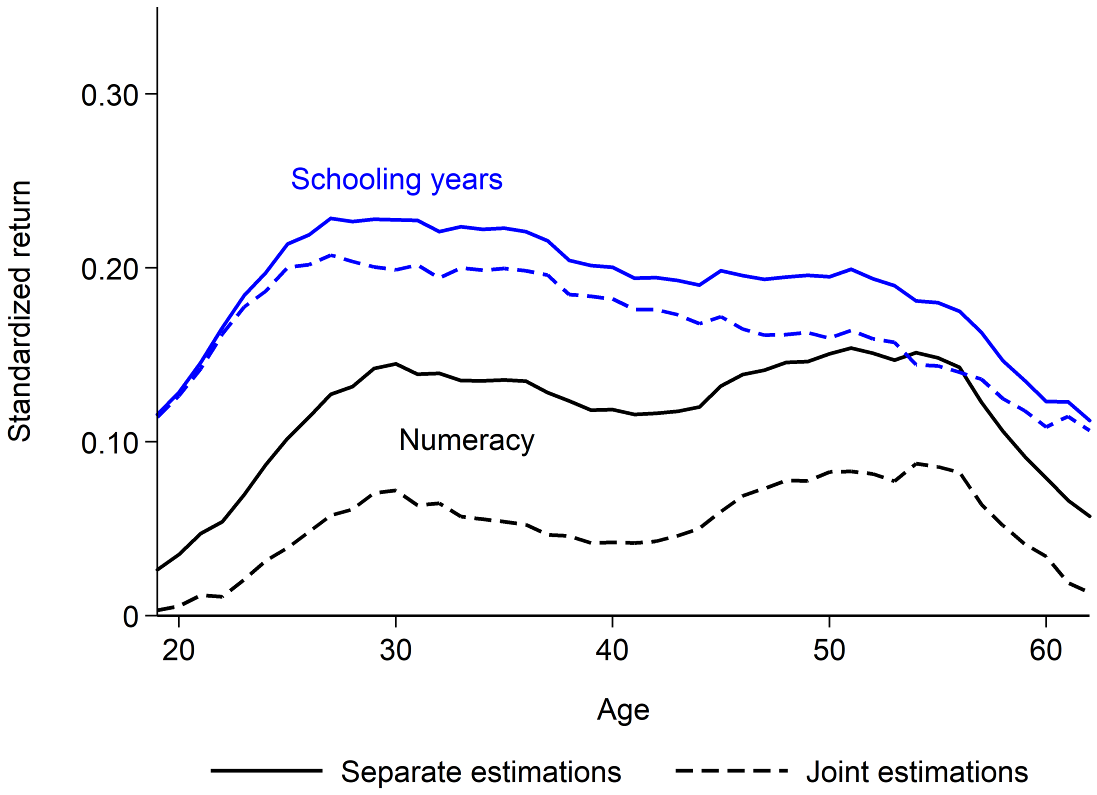


Figure 4.8, presented in the Appendix, displays confidence intervals associated with this figure's estimates.

Figure 4.3 adopts a different perspective: it plots the standardized returns of education and numeracy skills on wages of workers with less than one year of tenure in their current job, but for different groups of prior experience on the labor market. While returns to education do not vary much with experience, returns to skills do increase substantially for more experienced workers. Again, the comparison between the separately and the simultaneously estimated coefficients is informative. While the gap between the plain and the dashed lines is constant for skills, it's widening substantially for education. This increasing gap goes along with the steady increase of the explanatory power of skills for hiring wages. The latter equals the one of education for highly experience workers.

Finally, Figure 4.4 explores the relative explanatory power of education and numeracy skills depending on the time workers have spent with their current em-

Figure 4.2: **Relative returns to schooling years and numeracy for wages.**

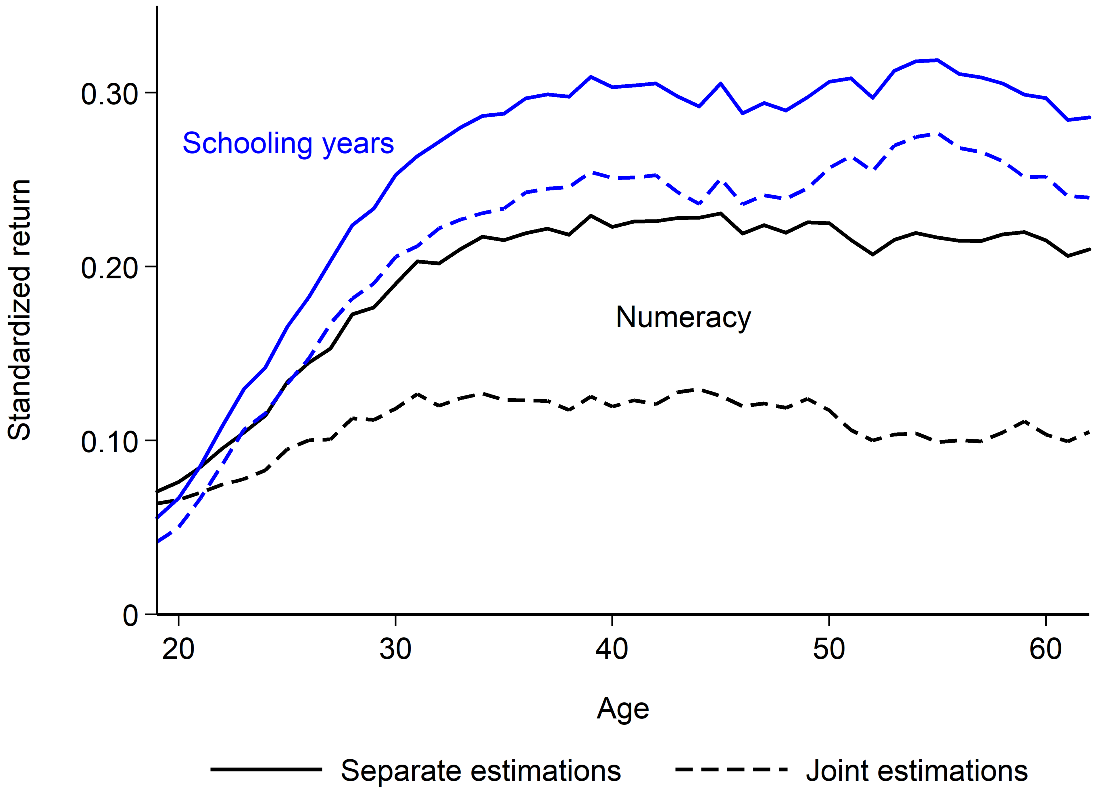


Figure 4.7, presented in the Appendix, displays confidence intervals associated with this figure's estimates.

ployer. Skills do not seem to become more strongly related to wages as the employment relationship lasts longer. Such a result does not support the idea that employers can observe and reward skills more easily as time passes.

All in all, Figures 4.1, 4.2 and 4.3 show that the explanatory power of skills matches the one of education along the life cycle but never exceeds it. In addition, the comparison of separately and simultaneously estimated coefficients suggests that the explanatory power of numeracy skills is substantially embedded in the one of schooling years. Skills only seem to gain in explanatory power for wages of experienced workers who change job.

Figures 4.5 and 4.6 plot the simultaneously estimated returns of schooling years and numeracy skills for the employment probability and wage, respectively, for four separate countries: Denmark, France, Spain and the United Kingdom. The country-specific pattern of standardized coefficients is not much different from the

Figure 4.3: **Relative returns to schooling years and numeracy for hiring wages.**

Figure 4.9, presented in the Appendix, displays confidence intervals associated with this figure's estimates.

one estimated for all countries: the explanatory power of skills never outperforms the one of schooling years when both variables are entered simultaneously in a regression. Some interesting differences do however appear across countries. For example, numeracy skills turn out to be as important as schooling to explain employment differences for older individuals in Denmark. As for the United Kingdom, both measures seem to perform equally in explaining employment differences for all ages and to lose explanatory power for older cohorts.

4.3 Additional explanatory power from skills

4.3.1 Methodology

As illustrated by results presented in the preceding section, there does not seem to be many situations in which skills can outperform the explanatory power of

Figure 4.4: **Relative returns to schooling years and numeracy for wages depending on tenure in firm.**

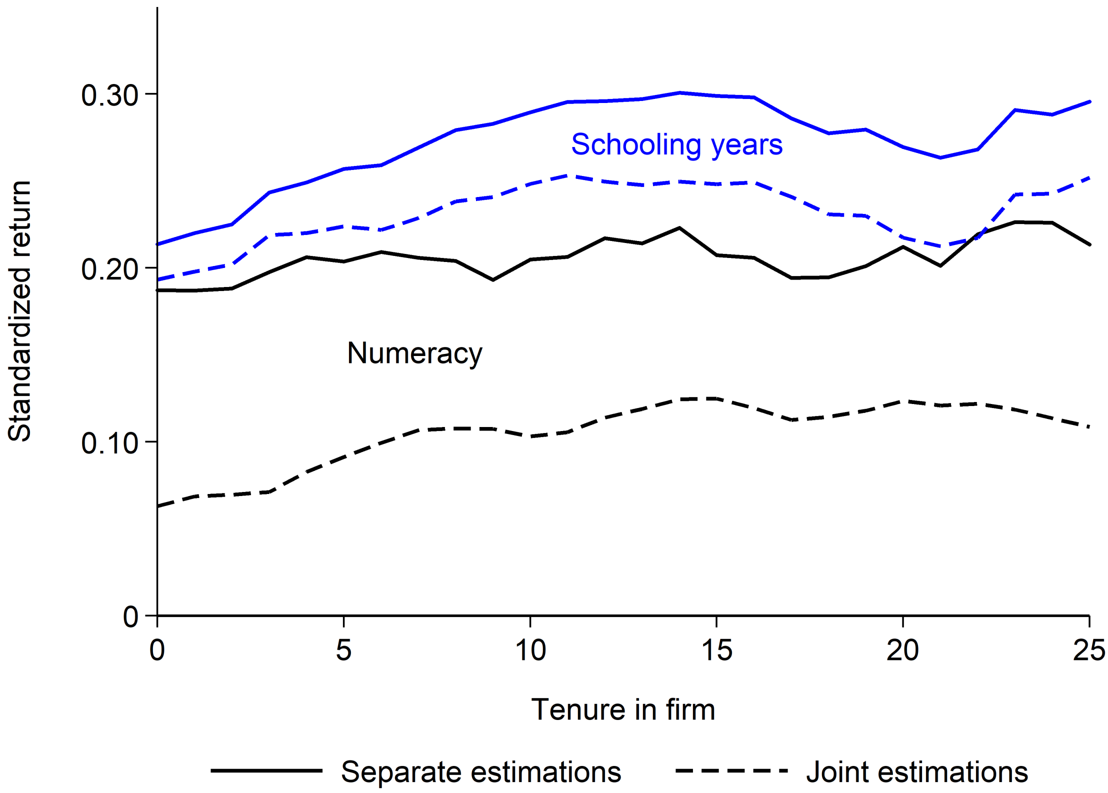


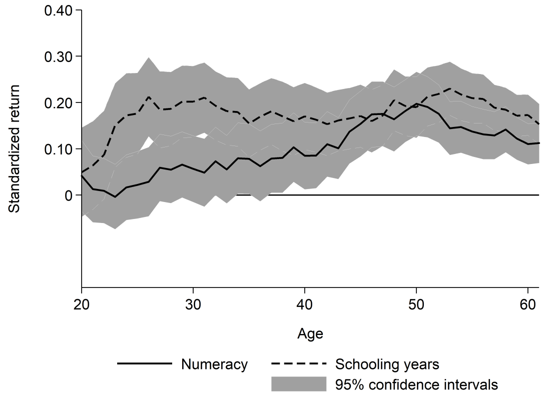
Figure 4.10, presented in the Appendix, displays confidence intervals associated with this figure's estimates.

education. In order to provide a general measure of this apparent failure, we will analyze R-squared statistics of sequences of regressions.

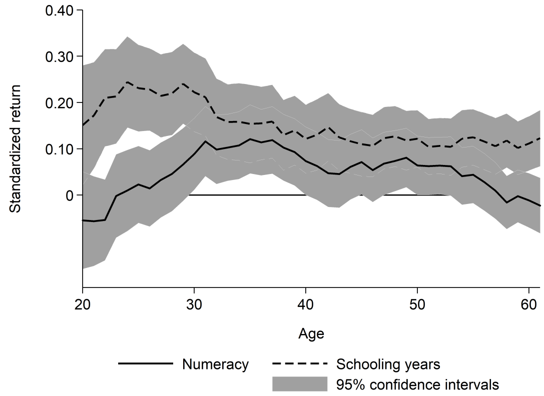
The procedure runs as follows. We first regress an individual's labor market outcome on measures of her educational level and keep track of the R-squared of this estimation, R_1^2, which captures the share of the outcome variance that can be explained by education. We then regress the residuals of the first regression on numeracy and literacy scores and store the R-squared of this estimation, R_2^2. We then compute the additional share of the outcome variance that can be explained by skills as $R_2^2 \times (1 - R_1^2)$. The ratio $\frac{R_2^2 \times (1-R_1^2)}{R_1^2}$ finally helps us to quantify the relative gain in explanatory power due to skills with respect to the one of education variables.

The analysis is performed on all countries included in the PIAAC survey for which wage information is available. Within-country samples are further restricted

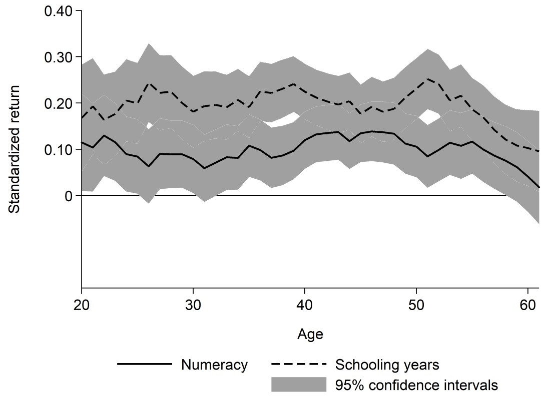
Figure 4.5: **Relative returns to schooling years and numeracy for the probability to be employed in Denmark, France, Spain and the United Kingdom: Joint estimations.**



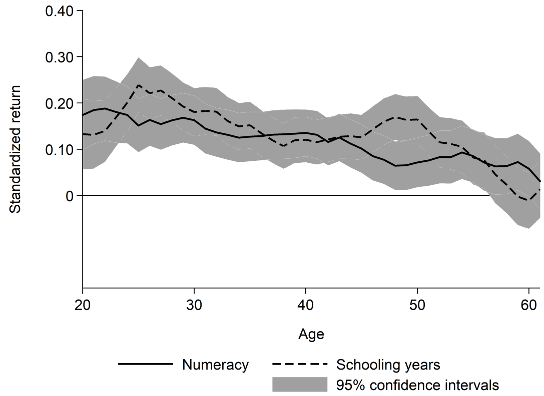
(a) Denmark.



(b) France.



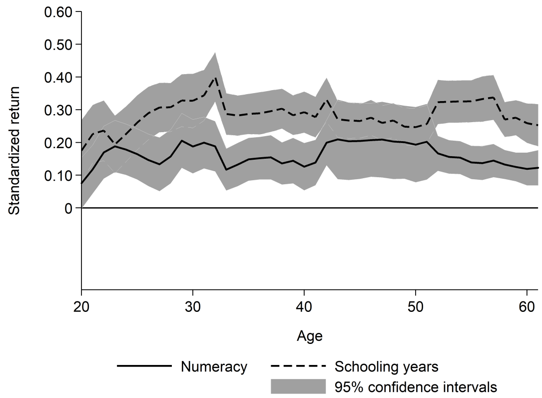
(c) Spain.



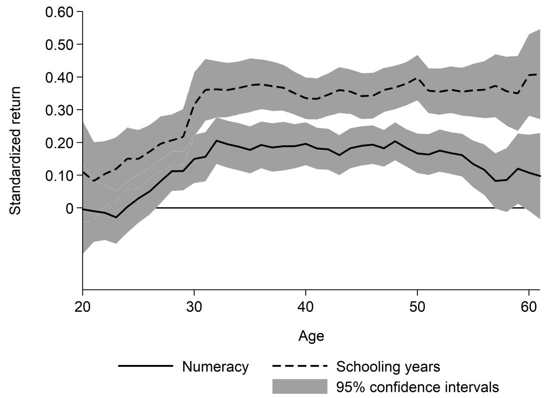
(d) United Kingdom.

to surveyed individuals for which information on both education and skills is available. We analyze the two following labor market outcomes: employment status and wage. We adjust both outcomes for differences across countries and individuals' gender and parents' education. To do so, we run preliminary regressions of these outcomes on country fixed effects and dummy variables for gender and parental education, and we keep residuals.

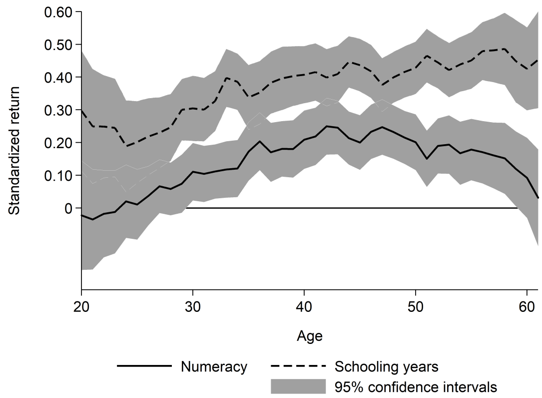
Figure 4.6: **Relative returns to schooling years and numeracy for wages in Denmark, France, Spain and the United Kingdom: Joint estimations.**



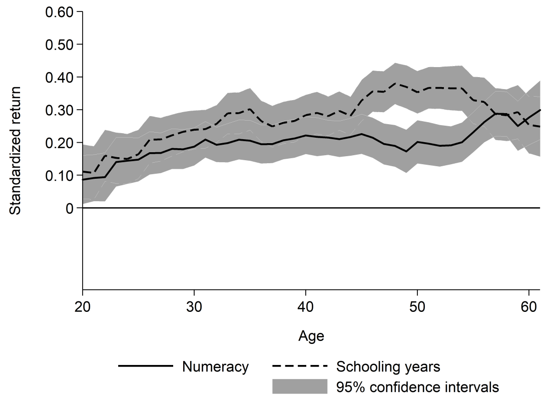
(a) Denmark.



(b) France.



(c) Spain.



(d) United Kingdom.

4.3.2 Results

We start by measuring education by a full set of 117 diploma $\times$ field of study interactions. In a linear probability model estimated over all countries, this set of variables can explain up to 2.3% of differences in employment across individuals. Once accounted for education in such a way, numeracy and literacy skills explain 0.6% of differences in employment situations across individuals. When measuring education using only schooling duration, the first R-squared reaches 1.5% while the second caps at 0.7%. This means that even using an approach that restricts by construction the explanatory power of education, only little is gained thanks to skills.

Table 4.2 reproduces this analysis for each of the countries in the data. Countries' R-squared statistics generally exceed the one obtained when all countries are considered together. This is because in the latter case, the returns to skills (or education) is constrained to be equal in all countries, making it a worse predictor of employment outcome in each country. There are large differences in the explanatory power of education across countries. For instance, the R-squared of the full set of education variables ranges from 1.9% in Korea to 6.8% in Spain. However, these differences in the explanatory power of education measures are not systematically compensated by changes in the explanatory power of skills. While skills have more additional explanatory power in some countries compared to others (for example, these statistic exceeds 2.0% in Denmark and Norway), the first-order explanatory power of education is still much larger than the additional one provided by skills in those cases. There are however countries in which the relative gain in explanatory power due to skills is more sizable and reaches 40 to 50% of the one of education : Denmark, Finland, the Netherlands, New Zealand, Norway and the United-Kingdom. The fact that skills play a greater relative role to explain employment outcomes in less regulated countries such as New Zealand or the United-Kingdom may reflect that the labor markets in such countries are more able to value skills directly. However, we also find that skills have a sizable explanatory power in Nordic countries whose labor markets are strongly covered by collective bargaining. This makes it difficult to conclude that there is a clear relationship between the extent of labor market regulation and the relative explanatory power of skills in a country. Another factor that may play a role is the total level of employment in a country: in countries where employment is high, variations in employment status are mechanically low, implying that they may be harder to predict. Results presented in Table 4.2 do not however seem to support such a conclusion either.

As shown by the right panel of Table 4.2, summarizing education as schooling years naturally increases the relative gain in explanatory power that can be obtained thanks to skills, but this improvement is more due to a drop in the explanatory power of constrained education variables than to a significant change in

Table 4.2: **Additional share of employment differences explained by skills when education is already accounted for.**

	Education: Diploma × field of study			Education: Schooling years		
	Share of employ. diff. explained by education	Additional share explained by skills	Relative gain	Share of employ. diff. explained by education	Additional share explained by skills	Relative gain
All countries	0.023	0.006	0.27	0.015	0.007	0.50
Belgium	0.038	0.012	0.31	0.011	0.012	1.09
Chile	0.037	0.003	0.09	0.017	0.004	0.22
Cyprus	0.050	0.005	0.11	0.029	0.005	0.16
Czech Republic	0.024	0.003	0.13	0.008	0.005	0.63
Denmark	0.041	0.022	0.55	0.015	0.027	1.79
Finland	0.044	0.019	0.43	0.022	0.021	0.92
France	0.031	0.008	0.24	0.011	0.009	0.85
Greece	0.046	0.011	0.23	0.020	0.013	0.63
Ireland	0.058	0.005	0.09	0.015	0.006	0.43
Israel	0.026	0.007	0.28	0.008	0.010	1.37
Italy	0.045	0.005	0.11	0.015	0.009	0.62
Japan	0.020	0.001	0.05	0.004	0.001	0.27
Korea	0.019	0.003	0.16	0.005	0.004	0.74
Lithuania	0.056	0.013	0.24	0.031	0.013	0.41
Netherlands	0.033	0.014	0.42	0.007	0.012	1.68
New Zealand	0.037	0.015	0.42	0.013	0.017	1.33
Norway	0.042	0.022	0.51	0.012	0.024	2.03
Poland	0.045	0.002	0.05	0.042	0.002	0.05
Russian Federation	0.044	0.004	0.09	0.017	0.005	0.27
Slovakia	0.051	0.018	0.36	0.023	0.024	1.02
Slovenia	0.048	0.003	0.07	0.021	0.004	0.21
Spain	0.068	0.006	0.08	0.032	0.008	0.26
United Kingdom	0.035	0.013	0.38	0.012	0.018	1.53

"*Employ.*" and "*diff.*" stand for "employment" and "differences", respectively.

the contribution of skills.

We next replicate the analysis using (the log of) wage as dependent variable. Applying the above described procedure to all countries, the explanatory power of education measures equals 9.0% when using the full set of interaction and 7.0% when using completed schooling years. The additional explanatory power of skills amounts to about 1% only in both cases. Separately repeating the procedure for each country conveys the same conclusions as previously shown by R-squared statistics displayed in Table 4.3.

A concern that may arise from this analysis is that less degrees of freedom are allowed for skills than for education when the latter is measured using diploma × field of study interactions. Tables 4.5 and 4.6, presented in the Appendix, allow for larger flexibility of measured skills by using sixth order polynomials in literacy

Table 4.3: **Additional share of wage differences explained by skills when education is already accounted for.**

	Education: Diploma × field of study			Education: Schooling years		
	Share of wage diff. explained by education	Additional share explained by skills	Relative gain	Share of wage diff. explained by education	Additional share explained by skills	Relative gain
All countries	0.090	0.008	0.09	0.070	0.012	0.17
Belgium	0.167	0.005	0.03	0.118	0.008	0.07
Chile	0.166	0.009	0.06	0.142	0.015	0.11
Cyprus	0.130	0.009	0.07	0.102	0.008	0.08
Czech Republic	0.111	0.001	0.01	0.079	0.002	0.03
Denmark	0.175	0.012	0.07	0.101	0.016	0.16
Finland	0.298	0.005	0.02	0.229	0.015	0.07
France	0.157	0.011	0.07	0.109	0.013	0.12
Greece	0.211	0.004	0.02	0.114	0.005	0.05
Ireland	0.098	0.006	0.06	0.052	0.014	0.27
Israel	0.108	0.021	0.19	0.055	0.040	0.73
Italy	0.115	0.019	0.16	0.072	0.025	0.35
Japan	0.072	0.015	0.21	0.036	0.022	0.61
Korea	0.125	0.001	0.01	0.094	0.003	0.03
Lithuania	0.165	0.004	0.02	0.126	0.004	0.04
Netherlands	0.134	0.001	0.01	0.105	0.005	0.05
New Zealand	0.167	0.025	0.15	0.130	0.027	0.21
Norway	0.147	0.017	0.11	0.094	0.026	0.27
Poland	0.123	0.009	0.08	0.097	0.008	0.09
Russian Federation	0.054	0.006	0.11	0.004	0.008	2.03
Slovakia	0.074	0.002	0.02	0.052	0.003	0.06
Slovenia	0.228	0.012	0.05	0.183	0.014	0.08
Spain	0.175	0.007	0.04	0.116	0.010	0.09
United Kingdom	0.154	0.016	0.10	0.105	0.023	0.22

"*Diff.*" stands for "differences".

and numeracy score, and 100 interactions terms constructed from the deciles of the two scores. As shown by tabulated statistics, the relative explanatory power of skills does increase when using such specifications, but it still only rarely exceeds the primary explanatory power of education.

Tables 4.7 and 4.8, presented in Appendix, mirror Tables 4.2 and 4.3 by swapping education and skills measures. In other words, these tables figures present the additional explanatory power that can be gained thanks to education when skills are already accounted for. Reported R-squared statistics clearly show that, despite the increasing first-step explanatory power of skills, education measures provide a very important additional explanatory power to differences in employment or wage that are not explained by skills.

Table 4.4 groups all countries but breaks down results according to individuals'

Table 4.4: **Additional share of employment and wage differences explained by skills when education is already accounted for: decomposition along immigration status and educational levels.**

	Employment			Wage		
	Share of diff. explained by education	Additional share explained by skills	Relative gain	Share of diff. explained by education	Additional share explained by skills	Relative gain
Immigrants	0.032	0.008	0.25	0.089	0.023	0.26
Non-migrants	0.024	0.005	0.22	0.094	0.005	0.06
Lower education	0.013	0.015	1.14	0.032	0.018	0.55
Intermediary education	0.008	0.006	0.75	0.016	0.010	0.63
Higher education	0.012	0.008	0.65	0.038	0.009	0.24

"*Diff.*" stands for "differences".

immigration status or educational level. While the additional explanatory power of skills looks larger for migrants compared to the rest of the population, it remains remarkably weak relative to the first-order explanatory power of education. When the analysis is reproduced by levels of education, education is obviously less able to predict labor market outcomes as the analysis is performed within similarly educated individuals. However, the explanatory power of skills does not rise dramatically in these sub-population either. To put in a nutshell, it is worth noting that the additional explanatory power, while remaining modest, peaks for immigrants and lower educated individuals compared to non-immigrants and intermediary or highly educated individuals, respectively.

4.4 Conclusion

Our findings about the relative importance of skills and schooling in terms of explanatory power match results obtained by Quintini (2011b) who also reports both quasi-systematically higher returns to schooling than to skills and important differences in the return to skills across countries. More generally, our results echo the analysis of wage equations by Branche-Seigeot (2015) who highlights that most of the returns to skills actually transit by education variables.

Two main conclusions arise from the above presented analysis. First, skills do as well as education measures to explain individuals' labor market situations in only very particular cases, such as when experienced workers change job and get a new wage offer. Similarly, skills exhibit different explanatory powers in different countries. This finding is consistent with Hanushek et al. (2017a) who document different returns to skills depending on a country's economic situation.

Second, skills, as measured in the PIAAC survey, rarely outperform simple education measures when aiming at explaining differences in individuals' labor market outcomes. Even in situations and/or countries where skills exhibit larger returns, their additional explanatory power in explaining labor market outcomes remains modest compared to the one of education variables. This result might be due either to the fact that skills measured in PIAAC are already well captured by diplomas, or to the fact that labor markets barely value them. In all cases, measured skills appear to have limited informative content from the statistician's point of view.

Conclusion

Combining various approaches, the report attempts to discuss how the measures of skills provided in the PIAAC survey may be used by policy makers and researchers. It starts with a review of the literature on skill mismatch, focusing both on measures of mismatch based on the skills variables in PIAAC, and on other type of information. This review highlights several challenges related to measuring skill mismatch and its possible causes. It concludes in particular that cross-country comparisons of skill mismatch based on the available information in PIAAC should be considered very cautiously.[1]

The report then moves the focus from skill mismatch to skills. It investigates if measures of skills can be informative on their own, on top of their debated utility to measure skill mismatch. We first try to understand if the general skills in numeracy and literacy measured in PIAAC are impacted by initial education, in particular when they are measured long after schooling, i.e. among adults around 45 years old. We conclude that this is the case, at least in Belgium, the only country where our identification strategy can be convincingly implemented. In this country, the causal effect of schooling on literacy skills is estimated to be comparable in magnitude to the correlation between these two variables, suggesting that the latter correlation reflects primarily a causal impact of schooling on skills, rather than a selection of more skilled individuals into longer studies. Such a result shows that educative policies that aim at improving numeracy and literacy skills can have long-run effects. This result also indicates that the information on skills available in PIAAC is relevant to measure the effect of schooling or educational policies on

[1]However, cross-country comparisons of skills (not skill mismatch) can offer interesting insights, as explained at the end of this conclusion.

individuals' general human capital.

We also show that the skills measured in PIAAC are significantly related to labor market outcomes. For example, an increase of one standard deviation in the distribution of numeracy skills is associated with a roughly 19% wage increase, while the corresponding number for literacy skills is around 17%. A weakness of this result is that it is entirely descriptive in the sense that it is not based on exogenous variations in skills themselves, whose effects on wages or employment could be causally estimated. Assuming that the relationship between skills and labor market prospects partly reflects a causal effect of the former on the latter, we can (cautiously) conclude that policies that improve the general skills as measured in PIAAC may in turn have significant positive impacts on workers' labor market prospects.

Interestingly, the relationship between general measures of skills and labor market outcomes is slightly stronger and more robust for numeracy than for literacy skills. For example, when both measures of skills are used simultaneously as predictors of wages, the relationship between literacy skills and wages almost drops to 0. Assuming again that these descriptive results partly reflect a stronger causal effect of numeracy skills on labor market prospects, one may question the relative importance dedicated to numeracy and literacy during primary education. We indeed find that primary education mostly affects literacy skills while numeracy skills appear more connected to labor market outcomes latter on. Switching teaching time from reading to quantitative subjects may improve the ability of initial education to build-up students' numeracy skills and therefore it may also improve their labor market prospects (and productivity in general, if it is reflected by wages to some extent).

As skills measured in PIAAC are strongly correlated with (and impacted by) initial education, we may wonder which of skills and education is the most strongly related to labor market outcomes. We find that the available information on education (either the number of years spent at school or more detailed information on diplomas and fields of study) can explain about twice more of the inter-individual variations in wages or employment than can the available information on skills in numeracy or literacy. Furthermore, when both education and skills are considered as explanatory variables for wages or employment in linear regression models, the partial effect of education decreases only slightly, while that of skills drops strongly.

Altogether, results reported in this study are consistent with the idea that education enables people to acquire the general skills measured in PIAAC, but also more specific skills than the ones measured in this survey. As a consequence, diplomas provide more information on adult competencies than do these few measures of skills. They are therefore better predictors of labor market outcomes. This remains true even for older workers whose careers may have been affected by several other factors than their initial diplomas.

As an alternative explanation, one could view the diploma as a signal that has long-lasting effects due to the fact that labor markets are not able to value underlying skills. In countries where institutional factors such as pay scales bargained by social partners matter a lot, wages may not reflect primarily individuals' skills. However, diplomas and education, which determine the starting point of a career and can directly be taken into account in pay scales, are more likely to have long-lasting effects on career paths in countries that have stronger labor market institutions. This less market-oriented interpretation is also consistent with the fact that education explains labor market outcomes better than general skills. However, the cross-country comparison of the returns to education and skills presented in chapter 4 does not provide strong support for this interpretation. Indeed, it seems that there is only a small relationship between the extent of labor market regulation in a country and the returns to skills or education in that country. For example, the four countries where the returns to skills in terms of wages are the highest are Slovenia, Finland, New-Zealand, and the United-Kingdom. It includes the two Anglo-saxon countries included in our sample whose labor markets are among the least regulated. Additionally, the four countries where the gain in explanatory power due to skills on top of diplomas also include New-Zealand and the United-Kingdom. On the other hand, the returns to skills as compared to the returns to schooling is not particularly low in countries that have the reputation to have more regulated labor markets, such as France or Norway.

Leaving aside possible interpretations, one may wonder if it is worth paying the financial and social cost of acquiring measures of skills if these measures do not provide much useful information to predict labor market outcomes once education is already controlled for. A possible advantage of these measures is that they allow to compare adults' achievement across countries, which is hardly feasible relying on qualifications, as certification frameworks do not perfectly overlap each other. However, the PISA survey already provides measures of skills in math, science and reading, though for 15 years old students. Having measures of skills for students rather than for adults, who are no longer used to take tests, may actually provide more accurate comparisons. PISA may however provide misleading approximations of adults' skills if students keep investing in general skills well after 15 years old. From our own calculations, the correlation between PIAAC and PISA achievement across countries appears relatively high: 0.77 in mathematics and 0.68 in literacy. This means that PISA provides good estimates of cross-country differences in skills levels among adults.

PISA remains nevertheless not adapted to a number of interesting empirical investigations that can be done with PIAAC and the previous surveys on adult skills (such as the International Adult Literacy Survey, IALS). First, these surveys make it possible to track the evolution of available skills in the adult population of a country across time. A positive evolution is likely to arise due to the expansion of secondary and post-secondary education, and this cannot be tracked with PISA which mostly focuses on kids that have not completed their education yet. Surveys of adult skills therefore offer a general tool to study if a nation becomes more skilled over time, which is in itself a question of interest. Panel data on skills can also be used to study how the skill-content of a given number of years of education varies over time. For example, with the expansion of higher education and the policy objective introduced in France in 1985 to award a baccalaureate to 80% individuals in each cohort, many are those who think that the skills required to obtain a baccalaureate nowadays are lower than that required a few decades ago. Such intuition is confirmed by the work of Micheaux and Murat (2006) and Murat and Rocher (2016) who also show that individuals' proficiency levels in numeracy and literacy start to decrease after 45 years old, which may have negative consequences on the labor market for older workers.

Another interesting application is the understanding of the differences in the wage structure and wage inequality across countries. Wage inequalities in the U.S. are for example about twice as large as wage inequalities in Sweden or Italy. This is true whatever the way these inequalities are measured. Some scholars have tried to use surveys of adult skills (including PIAAC and IALS) to understand if these differences could be driven by larger disparities in working-age adult skills in some countries than others. Devroye and Freeman (2001) and Blau and Kahn (2005) conclude that the bulk of cross-country differences in wage inequality cannot be explained by skills while Leuven et al. (2004) and Broecke et al. (2017), using a slightly more sophisticated approach that explicitly accounts for the effect of relative supply and demand on wages, reach less clear-cut conclusions. Asai (2018) has also used IALS and PIAAC in panel to study the contribution of skills and of the wage return to skills to the increase in wage inequality observed in several countries over the recent years. These papers are interesting illustrations of how surveys of adult skills can be used to help understanding the key determinants of important recent trends and cross-country differences in the labor market. In total, the type of cross-country and cross-time comparisons mentioned above probably reflects the most useful application of data on adult skills such as those included in PIAAC.

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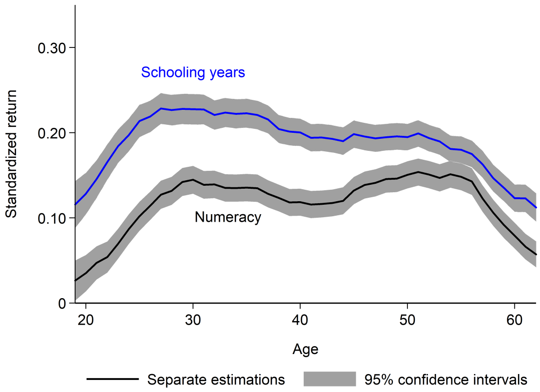
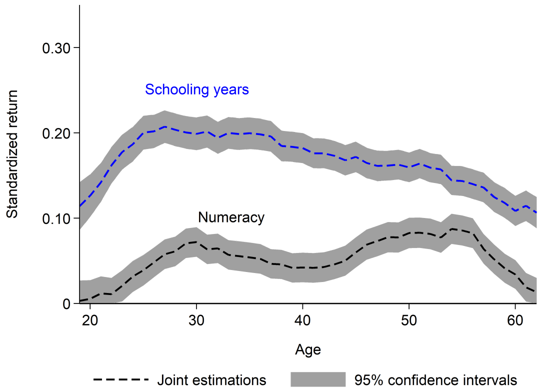
Appendix

Figure 4.7: **Relative returns to schooling years and numeracy for the probability to be employed: Confidence intervals.**



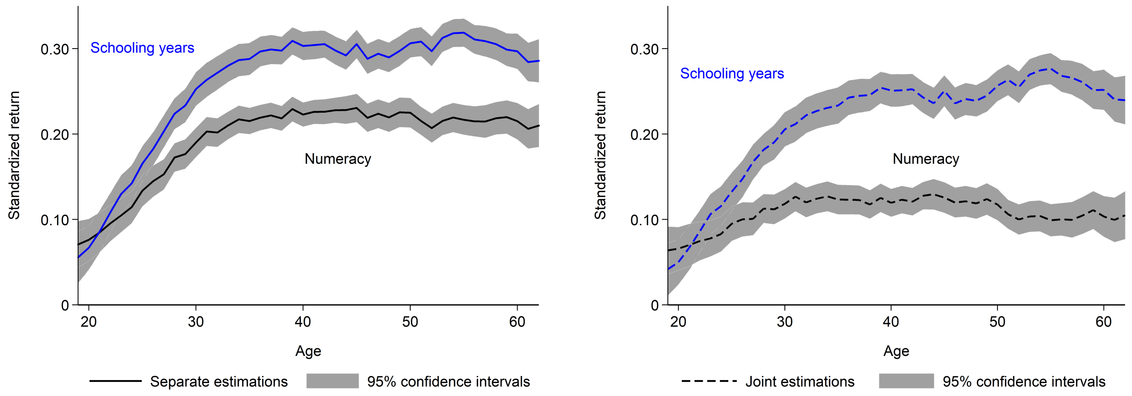
(a) Separate estimations.



(b) Joint estimations.

This figures displays confidence intervals associated with estimates presented in Figure 4.1.

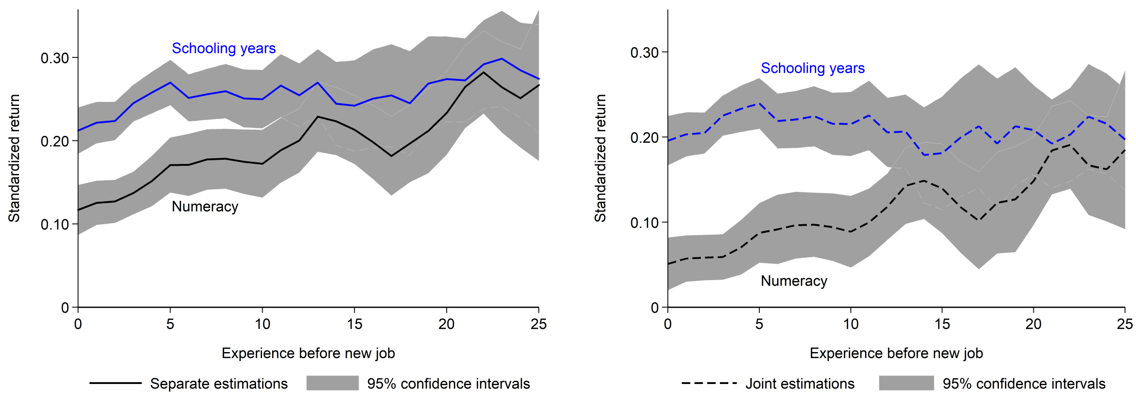
Figure 4.8: **Relative returns to schooling years and numeracy for wages: Confidence intervals.**



(a) Separate estimations. (b) Joint estimations.

This figures displays confidence intervals associated with estimates presented in Figure 4.2.

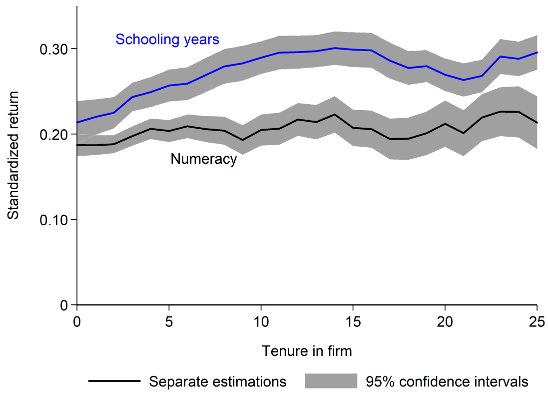
Figure 4.9: **Relative returns to schooling years and numeracy for hiring wages: Confidence intervals.**



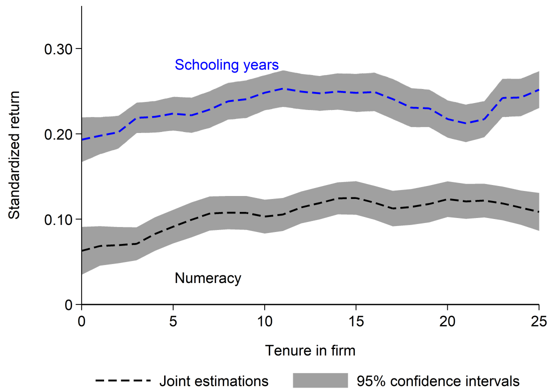
(a) Separate estimations. (b) Joint estimations.

This figures displays confidence intervals associated with estimates presented in Figure 4.3.

Figure 4.10: **Relative returns to schooling years and numeracy for wages depending on tenure in firm: Confidence intervals.**



(a) Separate estimations.



(b) Joint estimations.

This figures displays confidence intervals associated with estimates presented in Figure 4.4.

Table 4.5: **Additional share of employment differences explained by skills when education is already accounted for: Skills sixth-order polynomials and interacted fixed effects..**

		Skills: Sixth-order polynomials		Skills: Interacted fixed effects	
	Share of employ. diff. explained by education	Additional share explained by skills	Relative gain	Additional share explained by skills	Relative gain
All countries	0.023	0.007	0.30	0.008	0.34
Belgium	0.038	0.018	0.48	0.030	0.81
Chile	0.037	0.011	0.29	0.023	0.62
Cyprus	0.050	0.009	0.18	0.026	0.52
Czech Republic	0.024	0.006	0.26	0.019	0.79
Denmark	0.041	0.027	0.65	0.038	0.93
Finland	0.044	0.027	0.61	0.054	1.22
France	0.031	0.011	0.34	0.021	0.67
Greece	0.046	0.016	0.35	0.039	0.85
Ireland	0.058	0.010	0.17	0.028	0.49
Israel	0.026	0.011	0.43	0.058	2.26
Italy	0.045	0.008	0.18	0.033	0.72
Japan	0.020	0.003	0.16	0.021	1.07
Korea	0.019	0.005	0.26	0.020	1.06
Lithuania	0.056	0.016	0.28	0.026	0.47
Netherlands	0.033	0.020	0.61	0.040	1.21
New Zealand	0.037	0.024	0.65	0.034	0.94
Norway	0.042	0.026	0.62	0.039	0.92
Poland	0.045	0.005	0.11	0.018	0.40
Russian Federation	0.044	0.007	0.16	0.035	0.80
Slovakia	0.051	0.021	0.41	0.035	0.68
Slovenia	0.048	0.006	0.12	0.020	0.42
Spain	0.068	0.010	0.15	0.030	0.44
United Kingdom	0.035	0.019	0.54	0.026	0.74

Education is measured using diploma × field of study fixed effects. The skills *interacted fixed effects* is a set of 100 interactions terms constructed from the deciles of literacy and numeracy scores. "*Employ.*" and "*diff.*" stand for "employment" and "differences", respectively.

Table 4.6: **Additional share of wage differences explained by skills when education is already accounted for: Skills sixth-order polynomials and interacted fixed effects.**

		Skills: Sixth-order polynomials		Skills: Interacted fixed effects	
	Share of wage diff. explained by education	Additional share explained by skills	Relative gain	Additional share explained by skills	Relative gain
All countries	0.090	0.009	0.10	0.009	0.10
Belgium	0.167	0.011	0.07	0.034	0.20
Chile	0.166	0.014	0.09	0.050	0.30
Cyprus	0.130	0.013	0.10	0.057	0.44
Czech Republic	0.111	0.005	0.04	0.028	0.25
Denmark	0.175	0.015	0.08	0.027	0.16
Finland	0.298	0.014	0.05	0.026	0.09
France	0.157	0.013	0.08	0.032	0.20
Greece	0.211	0.008	0.04	0.057	0.27
Ireland	0.098	0.007	0.08	0.041	0.42
Israel	0.108	0.025	0.23	0.066	0.61
Italy	0.115	0.024	0.21	0.065	0.56
Japan	0.072	0.016	0.23	0.040	0.56
Korea	0.125	0.005	0.04	0.024	0.19
Lithuania	0.165	0.008	0.05	0.034	0.21
Netherlands	0.134	0.009	0.07	0.028	0.21
New Zealand	0.167	0.027	0.16	0.041	0.24
Norway	0.147	0.022	0.15	0.040	0.27
Poland	0.123	0.014	0.11	0.031	0.25
Russian Federation	0.054	0.010	0.18	0.061	1.13
Slovakia	0.074	0.007	0.09	0.036	0.48
Slovenia	0.228	0.014	0.06	0.040	0.17
Spain	0.175	0.013	0.08	0.045	0.26
United Kingdom	0.154	0.017	0.11	0.037	0.24

Education is measured using diploma × field of study fixed effects. The skills *interacted fixed effects* is a set of 100 interactions terms constructed from the deciles of literacy and numeracy scores. "*Diff.*" stands for "differences".

Table 4.7: **Additional share of employment differences explained by education when skills already accounted for.**

	Education: Diploma × field of study			Education: Schooling years		
	Share of employ. diff. explained by skills	Additional share explained by education	Relative gain	Share of employ. diff. explained by skills	Additional share explained by education	Relative gain
All countries	0.015	0.014	0.93	0.015	0.007	0.45
Belgium	0.024	0.030	1.27	0.024	0.001	0.06
Chile	0.012	0.028	2.27	0.012	0.008	0.65
Cyprus	0.014	0.042	3.10	0.014	0.019	1.38
Czech Republic	0.011	0.017	1.52	0.011	0.002	0.20
Denmark	0.042	0.026	0.62	0.042	0.003	0.07
Finland	0.041	0.028	0.66	0.041	0.004	0.11
France	0.019	0.022	1.14	0.019	0.002	0.10
Greece	0.017	0.041	2.45	0.017	0.015	0.91
Ireland	0.014	0.051	3.62	0.014	0.007	0.51
Israel	0.014	0.020	1.46	0.014	0.004	0.32
Italy	0.014	0.036	2.52	0.014	0.009	0.63
Japan	0.003	0.020	7.74	0.003	0.002	0.72
Korea	0.003	0.020	6.43	0.003	0.005	1.52
Lithuania	0.031	0.040	1.30	0.031	0.013	0.41
Netherlands	0.018	0.032	1.82	0.018	0.003	0.16
New Zealand	0.026	0.028	1.06	0.026	0.004	0.15
Norway	0.034	0.034	1.02	0.034	0.003	0.09
Poland	0.010	0.037	3.76	0.010	0.030	3.07
Russian Federation	0.006	0.043	6.78	0.006	0.015	2.46
Slovakia	0.040	0.030	0.75	0.040	0.007	0.18
Slovenia	0.017	0.034	2.02	0.017	0.008	0.48
Spain	0.021	0.052	2.53	0.021	0.018	0.86
United Kingdom	0.031	0.020	0.64	0.031	0.002	0.06

"*Employ.*" and "*diff.*" stand for "employment" and "differences", respectively.

Table 4.8: **Additional share of wage differences explained by education when skills are already accounted for.**

	Education: Diploma × field of study			Education: Schooling years		
	Share of wage diff. explained by skills	Additional share explained by education	Relative gain	Share of wage diff. explained by skills	Additional share explained by education	Relative gain
All countries	0.037	0.057	1.55	0.037	0.042	1.14
Belgium	0.049	0.103	2.10	0.049	0.062	1.27
Chile	0.070	0.096	1.38	0.070	0.079	1.13
Cyprus	0.038	0.097	2.57	0.038	0.067	1.77
Czech Republic	0.027	0.078	2.86	0.027	0.046	1.69
Denmark	0.053	0.131	2.47	0.053	0.060	1.13
Finland	0.080	0.206	2.59	0.080	0.144	1.81
France	0.051	0.108	2.11	0.051	0.054	1.06
Greece	0.026	0.185	6.97	0.026	0.089	3.34
Ireland	0.036	0.064	1.75	0.036	0.028	0.76
Israel	0.058	0.068	1.16	0.058	0.036	0.62
Italy	0.045	0.092	2.06	0.045	0.052	1.17
Japan	0.040	0.050	1.25	0.040	0.017	0.42
Korea	0.025	0.094	3.77	0.025	0.064	2.56
Lithuania	0.042	0.123	2.94	0.042	0.081	1.94
Netherlands	0.024	0.105	4.34	0.024	0.081	3.38
New Zealand	0.078	0.113	1.45	0.078	0.076	0.98
Norway	0.063	0.100	1.58	0.063	0.054	0.85
Poland	0.040	0.090	2.24	0.040	0.057	1.43
Russian Federation	0.010	0.051	5.03	0.010	0.002	0.24
Slovakia	0.018	0.055	3.05	0.018	0.035	1.92
Slovenia	0.095	0.137	1.43	0.095	0.087	0.91
Spain	0.042	0.134	3.15	0.042	0.075	1.77
United Kingdom	0.073	0.096	1.32	0.073	0.052	0.71

"*Diff.*" stands for "differences".

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