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Jani-Petteri Ollikainen

Essays on Vocational Education, Human Capital, and the Life Cycle

JYVÄSKYLÄ UNIVERSITY
SCHOOL OF BUSINESS AND ECONOMICS

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ABSTRACT

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This dissertation comprises four essays concerning the field of economics of education. These essays focus on the relationship between vocational education, human capital, and labor market outcomes over the life cycle. The first essay details the findings of my study of how the Finnish comprehensive school reform in the 1970s affected labor market outcomes over the life cycle. This reform increased the age at which students are separated into vocational and general tracks from 11 years to 16 years and updated the curriculum to include content that was more general than vocational. The results of the study suggest a negative effect of the said reform on employment early in the individuals' career but a positive effect later in their life cycle. The results also provide some evidence that this reform led to higher earnings later in one's career.

The second essay investigates the consequences of a Finnish vocational school reform implemented between 1999 and 2001. This reform made all of the graduates eligible for university by extending two-year programs to three years. To improve the relevance of vocational education in the labor market, this reform introduced workplace learning. The results show no effect on enrollment in further education or any labor market outcomes either. However, our results indicate that this reform increased the likelihood of students dropping out.

The third essay investigates whether continuous learning in mid-career could be supported by a psychological, peer group-based intervention to enhance employees' career management skills. This study was a randomized controlled trial in which 1119 graduates from business and administration and engineering streams (average age 49) participated. The results show that this intervention increased participation in higher education one year after the intervention.

The fourth essay assesses the effects of general and vocational secondary education on cognitive skills and non-cognitive skills as measured by the Finnish Defence Forces Basic Skills Test. Our results show that there are large differences in both the cognitive and non-cognitive skills among the men whose education levels are different at the time of entering military service. However, our results also suggest that these differences are mainly due to their selection into different tracks and not a causal effect of schooling.

Keywords: human capital, life cycle, vocational education

TIIVISTELMÄ

Ollikainen, Jani-Petteri
Tutkimuksia ammatillisesta koulutuksesta ja inhimillisestä pääomasta yli elinkaaren
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Väitöskirja sisältää neljä koulutuksen taloustieteen tutkimusartikkelia. Artikkelit käsittelevät ammatillisen koulutuksen, inhimillisen pääoman ja työmarkkinatulemien yhteyttä yli elinkaaren. Ensimmäisessä artikkelissa tarkastellaan Suomen 70-luvun peruskoulu-uudistuksen vaikutusta työmarkkinatulemiin yli elinkaaren. Uudistus nosti ikää, jossa oppilaat jaettiin ammatilliseen ja yleissivistävään koulutukseen, 11:stä 16:een, ja lisäsi akateemisten oppiaineiden määrää opintosuunnitelmassa. Tutkimuksen tulokset osoittavat, että uudistuksella oli negatiivinen vaikutus työllisyyteen työuran alkuvaiheessa, mutta positiivinen vaikutus myöhemmällä uralla. Tulosten perusteella uudistus näyttäisi myös lisänneen ansioita myöhemmin työuralla.

Toinen artikkeli tutkii Suomessa vuosina 1999-–2001 toteutetun ammatillisen koulutuksen seurauksia. Uudistus pidensi aiemmat kaksivuotiset tutkinnot kolmevuotisiksi, minkä myötä kaikki ammatillisen tutkinnon suorittaneet saivat yleisen jatko-opintokelpoisuuden. Ammatillisen koulutuksen työelämävastaavuutta lisättiin sisällyttämällä opintoihin pakollinen työharjoittelujakso. Tulosten mukaan uudistus ei lisännyt oppilaiden jatko-opintoihin osallistumista tai parantanut heidän työmarkkinatulemiaan. Tulokset kuitenkin osoittavat uudistuksen johtaneen suurempaan todennäköisyyteen keskeyttää opinnot.

Kolmannessa artikkelissa tarkastellaan, voidaanko jatkuvaa oppimista tukea keskellä työuraa edistämällä työntekijöiden osaamisen kehittämistaitoja ryhmävalmennuksessa. Tutkimus toteutettiin satunnaistettuna kenttäkokeena, johon osallistui 1119 keski-iältään 49-vuotiasta tradenomia ja insinööriä. Tulokset osoittavat, että ryhmävalmennus lisäsi korkeakouluopintoihin osallistumisen todennäköisyyttä.

Neljännessä artikkelissa arvioidaan ammatillisen koulutuksen ja yleissivistävän koulutuksen vaikutusta Puolustusvoimien peruskokeissa mitattuihin kognitiivisiin kykyihin ja persoonallisuuspiirteisiin. Kuvailevan analyysin perusteella molemmissa testituloksissa on eroavaisuuksia testintekohetkeen mennessä suoritetun koulutuksen mukaan. Tulosten perusteella kuitenkin vaikuttaa, että eroavaisuudet johtuvat valikoitumisesta eivätkä siten ole koulutuksen kausaalivaikutus.

Asiasanat: inhimillinen pääoma, elinkaari, ammatillinen koulutus

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Helsinki, August 2021
Jani-Petteri Ollikainen

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CONTENTS

1 INTRODUCTION

This thesis consists of four essays in the field of economics of education. These essays focus on the economic consequences of vocational and general education and the development of human capital over an individual's life cycle. The first essay studies how the Finnish comprehensive school reform in the 1970s affected labor market outcomes over the life cycle. The second essay reports the effects of a Finnish vocational school reform that sought to ease transition from vocational to higher education. The third essay investigates the efficacy of a psychological peer-group intervention in encouraging continuous learning among workers in their mid-career. The fourth essay estimates the effect of general and vocational secondary education on cognitive skills and personality traits as measured by the Finnish Defence Forces Basic Skills Test. In this introductory chapter, I review the previous research related to this dissertation, evaluate my contribution to relevant literature, and summarize the main findings of each of these four essays.

1.1 Motivation, research questions, and related literature

Schooling systems across countries differ widely in terms of their focus on general and vocational education. While many European countries, such as Finland, offer separate vocational tracks during secondary education, other countries, like the United States, have largely eliminated vocational education in favor of general education programs.

Vocational education offers students an easier transition from school to work compared to general education (Ryan, 2001; Wolter and Ryan, 2011; Zimmermann et al., 2013). It provides students with occupation-specific skills, which make them more productive in a specific occupation. Provided that there is a demand for these skills in the labor market, possessing them would lead to better employment prospects and increased wages. General education, on the other hand, is designed to develop general knowledge and skills such as literacy and numeracy skills to prepare students for higher education and lay a foundation

for lifelong learning (UNESCO, 2012).

On the other hand, in a rapidly changing economy, the job-specific skills provided by vocational education may become obsolete faster than those provided by general education. For example, computerization, digitalization, and automation have led to skill-biased technological changes where routine manual and cognitive tasks that were previously performed by specific occupations have been taken over by computers or robots, while the demand for non-routine cognitive tasks has increased (Katz et al., 1999; Autor et al., 2003). Additionally, general education may provide a stronger foundation for adopting new technologies and lifelong learning (Krueger and Kumar, 2004). On a macroeconomic level, Krueger and Kumar (2004) have claimed that the slower adoption of technology caused by skill-specific education may even help explain the difference in the growth rates of Europe and the US.

These two different views on vocational education imply that while vocational education may lead to better school-to-work transition and early labor market outcomes, it may come at the cost of greater risk of unemployment in later career stages compared to general education. Previous empirical literature supports the hypothesized trade-off between early and late labor market advantages when comparing vocational and general education. Most notably, Hanushek et al. (2017) found that while vocational education leads to better school-to-work transition, it becomes a disadvantage in terms of employment around the age of 50 years. Further, they linked the magnitude of the effect to the degree of vocationalization of a country's education system. They found that, for example, in German speaking countries, where vocational education is mostly based on apprenticeship programs, the trade-off between early and late labor market advantages is more noticeable than in countries that have school-based vocational programs, such as Finland.

Similar results have been found by studies using different international data sets (Hampf and Woessmann, 2017; Forster et al., 2016; Cörvers et al., 2011) and in country-specific studies (for example Brunello and Rocco (2017) for Great Britain, Weber (2014) for Switzerland, and Golsteyn and Stenberg (2017), and Stenberg and Westerlund (2015) for Sweden). A common limitation of these studies is that they lack an empirical setting where causal effects can be reliably estimated, such as in the case of randomization or a natural experiment. The main difficulty in estimating the causal effects of vocational and general education is that different types of students are likely to self-select into these different types of educational tracks. For example, if choosing general education is positively correlated with the student's unobserved abilities or skills valued in the labor market, the resulting estimates for returns to general education are positively biased. Thus, these studies can either only provide correlational estimates or the causal estimates are based on strong assumptions.

Causal evidence on the labor market effects of vocational and general education is scarce. Additionally, the few papers that can estimate these effects, generally suffer from a shorter follow-up period compared to the previously mentioned studies. Silliman and Virtanen (in press) exploited a regression disconti-

nuity design (RDD) created by a centralized admission system in Finland to show that upper secondary vocational education increases annual income by 7 percent. Moreover, they found no evidence of the effects diminishing with time. However, they observed earnings only up to age of 35 years. Hall (2016) found no significant effect on unemployment (up to the late 30's) resulting from a Swedish reform that added a year of general education to vocational upper secondary programs. Malamud and Pop-Eleches (2010) examined the effects of a reform step in Romania that required students to complete two years of general education before entering vocational education. Using RDD, they found that the reform step had no effect on university completion, employment, or earnings.

The first essay is solo-authored, and it contributes to this literature by examining the causal effects of the Finnish comprehensive school reform on labor market outcomes over the life cycle. The reform increased the age at which students are separated into general and vocational tracks from 11 years to 16 years. The reform also increased the amount of general content in the curriculum and simultaneously reduced the share of vocational content.

In addition to contributing to the literature on general and vocational education, the first essay also contributes to the literature on comprehensive school reforms. During the second half of the 20th century, many European countries implemented comprehensive school reforms that increased the minimum years of compulsory education, introduced national curricula, or reduced tracking. Previous studies on comprehensive school reforms in the Nordic countries have found that such reforms increased educational attainment, earnings, and intergenerational mobility (Meghir and Palme, 2005; Pekkarinen et al., 2009; Aakvik et al., 2010). However, little is known about how these comprehensive reforms affected life cycle employment and earnings patterns.

The studies focusing on the Finnish reform have shown effects on several outcomes. Pekkarinen (2008) found that the reform increased women's probability of enrolling in academic secondary and tertiary education and decreased the gender wage gap. Pekkarinen et al. (2009) showed that the reform decreased intergenerational income elasticity by 23%. Finally, Pekkala Kerr et al. (2013) associated the reform with a slight positive increase in verbal test scores measured by the Finnish Defence Forces Basic Skills Test. The students whose parents had no high school education also saw an increase in their arithmetic and logical reasoning test scores along with the verbal test scores.

The second essay is a joint work with Hannu Karhunen, and it studies the effects of a Finnish vocational school reform that made all vocational school graduates eligible for higher education by extending the length of vocational education. Until 1999, the Finnish vocational education consisted of two-year programs that only recognized eligibility for vocational higher education (known as *polytechnic universities*) in the same field, and three-year programs that recognized eligibility for vocational higher education in any field. Neither of these two programs gave eligibility for university studies. From 1999 to 2001, a vocational school reform was implemented in Finland that extended all of the programs to three years programs to give all of the vocational school graduates eligibility for higher edu-

cation, including university studies. Our main contribution is to study the effects of this reform on study completion, enrollment in higher education, and labor market outcomes.

Opening pathways from vocational to higher education could alleviate the possible trade-off between early and late career labor market advantages by increasing career flexibility and making it easier to switch occupations. The Finnish reform can also be seen as a way of increasing equality between graduates from vocational and general education. By eliminating educational "dead-ends," students' educational choices at a relatively young age would no longer restrict their schooling possibilities in the future. Reforms similar to the Finnish one were implemented in other Nordic countries in the 1990's. In addition to easing access to higher education, they also increased the length of the vocational programs and increased the proportion of general education in these programs (Bertrand et al., 2019; Hall, 2016).

Arguments can also be made against such reforms. First, more students may struggle in their studies and possibly drop out if providing access to higher education makes the vocational programs more demanding. This concern is even more relevant, as on average, vocational students in Finland come from less educated families and have lower GPA from compulsory school compared to their peers in the general track. Second, students might choose vocational education precisely for the job-specific skills it provides and the relatively easier transition into the labor market with little regard to higher education. From a public policy perspective, reforms providing access to higher education might lead to inefficient use of resources if vocational students are not inclined to pursue higher education anyway (Hall, 2012).

Previous literature suggests that a more general curriculum in vocational education or an increase in the length of the education are associated with an increased risk of students dropping out, and most papers find no significant effects on labor market outcomes or enrollment in higher education. Hall (2012, 2016) showed that a longer duration of vocational education, which included more general education, increased the dropout rate and later unemployment in Sweden, especially among males with low GPA. Zilic (2018) used RDD to identify the effects of a high school reform in Croatia that reduced tracking and extended general curriculum for vocational students. The reform increased the probabilities of students dropping out and not completing university for men, while no effects were found in the case of women. This reform did not affect labor market outcomes. Similar results have also been found in Spain, where replacing lower vocational year by more academic education, resulted in an increased dropout rate (Felgueroso et al., 2014). Oosterbeek and Webbink (2007) found no effects on earnings as a result of an added year of general education for vocational students in the Netherlands. Finally, Bertrand et al. (2019) studied a Norwegian reform that integrated more general education into vocational high school tracks and offered vocational students a pathway to higher education. They found that this reform increased enrollment in the vocational track, but it had no impact on college completion.However, unlike previous studies, they also showed a positive ef-

fect on adulthood earnings for men from disadvantaged backgrounds and fewer dropouts as far as women from disadvantaged backgrounds were concerned.

Another tool for improving long-term employability of vocational graduates could be a stronger focus on continuous learning. As the transition to a knowledge economy, aging of the workforce, and accelerating technological development continue, there may be a greater need for developing new skills throughout one's career. For example, the European Commission (2017) has estimated that less than half of the European labor force possess basic digital skills that are critical in today's labor markets.

Even though the need for updating professional skills throughout the life cycle is widely acknowledged, there is a shortage of effective methods for encouraging continuous learning at mid-career and beyond. An issue related to adult education is that many employees who would benefit from it the most, do not participate in it (Bassanini et al., 2005). For instance, older employees and employees with low educational attainment are significantly less likely to pursue adult education. Some methods for encouraging adult education such as the adult education allowance in Finland also suffer from misallocation. According to Kauhanen (2021), the allowance is most often used by occupational groups that are not threatened by automation or outsourcing.

The third essay is a joint work with Salla Toppinen-Tanner, Niina Jallinoja, Mervi Ruokolainen, and Jukka Vuori. This essay contributes to a small body of experimental literature on promoting continuous learning. We investigate if continuous learning can be encouraged by supporting employees' career management skills with a psychological peer group-based intervention called "Skills for Work." This intervention was specifically aimed for workers in the mid-stages of their careers and organized at the Haaga-Helia University of Applied Sciences in Finland. It was designed to support the participants' skills development and employability by enhancing their self-efficacies related to competence management and preparation for career-related setbacks. The intervention included two four-hour group meetings hosted by trained instructors. In the meetings, the participants were to identify skill- and career-related goals and setbacks and then find and practice solutions for them in groups. This intervention was based on social cognitive theories on social learning, behavioral control, and individual coping resilience (Ajzen, 1991; Bandura, 1986; Meichenbaum, 1985).

Previously, training vouchers have been a widely used tool for encouraging continuous learning. However, studies suggest that there is little evidence of their efficacy in terms of participation in training or education, or subsequent labor market outcomes. Schwerdt et al. (2012) found from a randomized field experiment in Switzerland that training vouchers did not have an effect on employment, earnings, or educational attainment. In an experiment in the Netherlands, Hidalgo et al. (2014) showed that while a training voucher increased training participation, it did not affect job mobility or earnings. Görlitz and Tamm (2017) conducted a randomized field experiment where they provided information about a training voucher to eligible employees in Germany. The experiment successfully increased knowledge about the training voucher; however, it did not

affect voucher take-up or training participation.

The final essay is co-authored with Tuomas Pekkarinen, Roope Uusitalo, and Hanna Virtanen. In this essay, we estimate the effects of secondary schooling on cognitive skills and non-cognitive skills. We measure these skills using psychological tests conducted by the Finnish Defensive Forces. We identify the causal effects of secondary schooling on the test scores by using admission cut-off to oversubscribed schools.

Research has established that cognitive skills are strong predictors of later-life outcomes, such as educational attainment and labor market success (e.g. Cawley et al. (2001)). There exist multiple measures of cognitive skills. In psychology, a distinction is made between fluid and crystallized intelligence (Cattell, 1963). Fluid intelligence is defined as the ability to reason and process different types of novel information. IQ tests such as Raven's Progressive Matrices are designed to capture fluid intelligence. Crystallized intelligence refers to acquired knowledge, and it is often measured by different achievement tests.

However, the measures of cognitive skills fail to capture the role of non-cognitive skills, which have been shown to rival cognitive skills in their ability to predict later-life outcomes (Kautz et al., 2014). Non-cognitive skills refer to a wide range of abilities or personality traits such as perseverance, self-esteem, self-efficacy, etc. The most commonly used taxonomy of non-cognitive skills is the "Big Five" which consists of the following factors: openness to experience, conscientiousness, extraversion, agreeableness, and neuroticism. Non-cognitive skills are commonly measured by self-report questionnaires.

Although the importance of cognitive and non-cognitive skills has been well established in relevant literature, there is much less research on the factors contributing to the formation of these skills. In the final essay, we contribute to the body of knowledge on this front by evaluating the role of schooling in the formation of cognitive and non-cognitive skills. Educational attainment is strongly associated with an increase in cognitive and non-cognitive skills but it is less clear if schooling has a causal effect on these skills (Schurer, 2017; Kautz et al., 2014).

Previous research suggests that cognitive skills stabilize early in the life cycle, leaving little room for schooling to affect them (Borghans et al., 2008). However, some more recent studies have shown non-cognitive skills to be more malleable than cognitive skills in the adolescent years. The studies show that non-cognitive skills can be improved with different adolescent interventions, but the effect of the schooling system on these skills is still an open question (Kautz et al., 2014; Schurer, 2017).

1.2 Research methodology and data

This dissertation consists of four empirical studies. The first essay uses Finnish administrative register data to study the effects of the Finnish comprehensive school reform of 1972–1977. In this essay, I estimate the labor market effects of

the reform over the life cycle. The sample was restricted to individuals born between 1960 and 1966 to include the last cohort entirely where none were affected by the reform (1960) and the first cohort to be entirely affected (1966). Using information on each person's birth year and their municipality of residence from 1972 to 1977, I could determine whether they were affected by the reform or not. I could then link their reform status to information on their employment status, annual earnings, and completed degrees from 1987 to 2016, which were used as the outcome variables in the estimated models. With the available data and the cohorts chosen for the study, I could estimate the reform's effects from age 21 years to age 56 years.

To identify the causal effect of the reform, I exploit a natural experiment created by the gradual implementation of the reform across municipalities. The first municipalities to adopt the reform in 1972 were mainly from Lapland in the north of the country. From there, the reform moved toward the south with the capital region finally having adopted it in 1977. Students who were entering first to fifth grades (ages 7 years to 11 years) during the year in which the reform was adopted in their municipality were moved to the new comprehensive school system. The reform's implementation scheme creates possibly exogenous variation in the individuals' reform status within cohorts and municipalities, which allows me to compare the treated to individuals from the same cohort in different municipalities and to individuals in the same municipality from different cohorts. In practice, I use a differences-in-differences approach that controls for municipality- and cohort-fixed effects. The reform's effects are allowed to vary with age in the model to examine how the effects evolve over the life cycle.

The second essay studies the consequences of a reform of vocational education in Finland that sought to ease transition from vocational to higher education. Consequently, all two-year programs that had previously existed in upper secondary vocational education were extended to three years. The new programs also included six months of on-the-job learning to increase the labor market relevance of the vocational programs. For our empirical analysis, we used administrative register data from The Finnish National Board of Education (the Joint Application Registry) and Statistics Finland (Register of Completed Education and Degrees, Finnish Longitudinal Census Files, and the Longitudinal Employment Statistics Files). We focus on applicants who were 15 years to 17 years old when they were first admitted to vocational school and who had passed through the basic education syllabus in the comprehensive school.

The reform was implemented in three stages across different vocational school study lines during the years 1999-2001. For example, the programs in construction adopted the reform in 1999, the programs in electrical engineering in 2000, and the programs in agriculture in 2001. We used this staggered implementation in a differences-in-differences model to estimate the reform's causal effects on the students' subsequent labor market and educational outcomes up to 13 years after enrollment. We explored possible heterogeneous effects by gender.

Our identification strategy relied on the assumption that the timing of the reform in different programs is unrelated to other program-specific changes in

the outcome variables. To relax this assumption, we also showed estimates with program-specific linear time trends, which have little effect on our estimates and do not affect our conclusions. Our estimates could also be biased if the reform also impacted the students' track choices or their decision to attend vocational education at all. To ensure that our selection on observable characteristics did not reflect in our estimates, we added a vector of control variables to our regression. To further support our identification strategy, we showed that the student characteristics in the reformed programs were not affected as a result of the reform.

The first two essays employ difference-in-differences designs with multiple time periods and groups (sometimes also referred to as a two-way fixed effects model). Recently, several studies have shown that when there is variation in the timing of treatment and heterogeneous treatment effects, the two-fixed effects estimator may produce biased estimates (e.g. Borusyak and Jaravel (2017); Callaway and Sant'Anna (2020); De Chaisemartin and d'Haultfoeuille (2020); Goodman-Bacon (2021)).

The two-way fixed effects estimator is equal to the weighted sum of the treatment effect across treated units in different times. When units (e.g. regions) adopt the treatment at different periods, the early adopters were used as a control group for the later adopters. As shown by De Chaisemartin and d'Haultfoeuille (2020), this aspect of the estimator can cause negative weights for some of the treatment effects. The negative weights are an issue when the treatment effects are heterogeneous across periods or groups. For example, even if all of the treatment effects are positive, their weighted average could be negative.

Several of the papers comprising this literature propose alternative estimators that are robust to heterogeneous treatment effects. To check the robustness of the first two essays' results to heterogeneous treatment effects, I re-estimated key results from these essays using the estimator proposed by De Chaisemartin and d'Haultfoeuille (2020). Based on these estimations, the results were found to be robust to heterogeneous treatment effects and the conclusions drawn from them were unaffected.

The third essay evaluates the effects of the mid-career peer group intervention called "Skills for Work" on participants' subsequent education and employment. The study was implemented as a randomized controlled trial at the Haaga-Helia University of Applied Sciences. A total of 1119 graduates from the business and administration and engineering streams with an average age of 49 years participated in the study. The participants were randomized into treatment (n = 561) and control (n = 558) groups. The members of the treatment group were invited to participate in the peer-group intervention.

The treatment and control groups received a baseline questionnaire before the intervention and a follow-up questionnaire one year after the intervention. The data for the study was collected from these surveys. First, we showed that the random assignment was successful, by comparing the means of different background characteristics between the treatment and the control groups. We found no statistically significant differences and concluded that the randomization was successful. We then estimate the effects of the intervention on later education and

employment by using ordinary least squares (OLS) and instrumental variables estimation (IV) with the random assignment as an instrument for attendance in the intervention.

The final essay estimates the effects of general upper secondary schooling on cognitive- and non-cognitive skills. We obtained the measures for these skills from the psychological Basic Skills test administered by the Finnish Defense Forces for the years 1982 to 2000. As all of the Finnish men are required to participate in either military service or civil service unless they are exempt from service (e.g., for medical reasons), the test data covered nearly entire cohorts of men. We used the birth cohorts from 1962 to 1979 in our analysis. In these cohorts, around 70% of men have attended military service and taken the test.

The test consists of two main parts: one for cognitive skills and the other for non-cognitive skills. The cognitive part measures verbal, numerical, and logical reasoning skills. The non-cognitive part measures eight different personality traits that army psychologists consider important skills for military leaders (e.g., achievement striving, self-confidence, deliberation, and sociability). Importantly for the external validity of the tests, Jokela et al. (2017) show that both the cognitive and non-cognitive test scores are highly predictive of earnings between ages 30-34 years.

We linked the test score data to various register data sets using an individual ID. Our data on completed education came from Statistics Finland (Register of Completed Education and Degrees). The data on application and admission to secondary education came from the National Board of Education (Joint Application Registry). Our earnings data were based on tax records. Additionally, we used information on family relations from Statistics Finland to link individuals to their parents in the same registers.

To estimate the causal effect of admission into the general track on cognitive- and non-cognitive skills, we employed RDD created by admission cut-offs in oversubscribed general upper secondary schools. The general upper secondary schools select their students based on compulsory school GPA. For each applicant, we used the general upper secondary school with the lowest GPA requirement from their list of applications as the relevant cut-off. To obtain our reduced form estimates, we pooled the data on each school and year to estimate the effects of being above the cut-off on the test scores. We also employed an instrumental variable strategy (fuzzy RDD) to obtain the local average treatment effects (LATE) of being admitted into or graduating general upper secondary school on the cognitive and non-cognitive skills from the reduced form estimates.

1.3 Summary of the main findings and conclusions

This section summarizes the main findings and conclusions of each of the essays in this dissertation. The dissertation started by studying the effects of the Finnish comprehensive school reform on labor market outcomes over the life cycle in the

first essay. I found evidence of a trade-off between labor market outcomes early and late in the life cycle because of the reform. The results showed that the reform impacted employment negatively in the early 20s. However, this negative effect reduced with age and was positive from the mid-30s onward. The estimated effect on labor earnings was imprecise and sensitive to different modeling assumptions, but it did indicate a positive effect from late 30s onward. These results are similar to the findings of previous studies studying the life cycle effects of general and vocational education. Consequently, I found that the changes in educational outcomes are a possible mediator of the reform's effects on labor market outcomes. I analyzed the reform's effect on completed education at age 30 years and found that the reform increased educational attainment and the probability of choosing the academic track rather than the vocational track in upper secondary education. Finally, the possible heterogeneous effects by gender were considered, and they showed that the estimated effects on the labor market and educational outcomes are similar for both men and women.

Based on these results, I concluded that policy makers as well as individuals should be aware of the advantages and possible pitfalls of general and vocational education. The long-term effects should be taken into account when making decisions on one's own education or educational policy. The policies promoting vocational education should be based on careful consideration of the possible advantages and disadvantages over the entire life cycle. To alleviate the possible late-career disadvantages, policy makers should ensure that vocational education provides sufficient general skills. Additionally, it may be worth implementing policies to support continuous learning to enhance skill development throughout the life cycle. A limitation of this essay is that assessing the relative importance of the tracking age change and curriculum changes is impossible as these changes occur simultaneously. In practice, comprehensive school reforms are likely to involve changes in both the curriculum and tracking age, making the results of this study relevant for evaluating the life cycle effects of past and future reforms. Nonetheless, weighing the respective importance of these mechanisms could provide valuable insights for designing education policy in the future, suggesting an avenue for future research.

In the second essay, we studied the consequences of a reform of vocational education in Finland that sought to ease transition from vocational to higher education and increase the labor market relevance of vocational education. Our results suggest that the reform was unsuccessful in achieving these goals. First, we find no effect on enrollment in higher education for vocational students. Second, we estimate the reform's effect on earnings and employment up to 13 years after enrollment. We estimated negative effects on the probability of employment and annual earnings two to four years after enrollment, but we found no significant effects after these years. The negative effects two to four years after enrollment are likely to be a result of the increased average graduation duration caused by the elimination of the previous two-year programs. Further, we show that these negative effects on labor market outcomes are greater for men than for women.

Finally, we examined if the reform led to an increase in the probability of

dropping out, for, on average, graduating became harder due to the fact that the programs that previously took two years to complete would now take three years. We defined dropout as no completed upper secondary degrees five years after initial enrollment. Our estimates show an increase of 1.8 p.p. in the probability of dropout (corresponding to a 6.5% increase from the mean). The reform's effect on dropout does not differ for men and women, but we found that the time taken for graduation increased more for men than for women after the reform. The difference in time taken for graduation may explain why the negative effect on the labor market outcomes is larger for men than for women. Our results underline a trade-off between higher demands placed on students and increased dropout rates. Ultimately, the increase in dropout rates could explain why the reform was not successful in meeting its goals of easing transition from vocational to higher education and increasing the labor market relevance of vocational education. We concluded that possible unintended consequences of all measures should be carefully considered and acknowledged by policy makers when designing educational reforms.

In the third essay, we detail the results of our investigation of the short-term effects of the "Skills for Work" intervention on employment and subsequent tertiary education. Our intention-to-treat (ITT) estimates from the randomized controlled trial suggested that the intervention increased the probability of attending tertiary education after one year by 8 percentage points. When we take into account the fact that not all members of the treatment group attended the intervention and estimate the average treatment effect on the treated (ATET) by IV, we find a 12 percentage point increase in the probability of attending tertiary education for the treatment group[1]. To interpret the magnitude of the estimates, we compared them to the average of the outcome variable in the control group. Based on these calculations, the intervention approximately doubled the probability of attending tertiary education. We found that the intervention had no effects on employment.

Thereafter, we discuss two mechanisms which could have led to an increase in participation in higher education. First, the intervention was designed to enhance self-efficacies related to competence management and preparation for career-related setbacks. The effects of the intervention on further education might have been mediated by an increase in career management skills. Second, since the intervention was organized at an education institute, it is possible that this aspect of the intervention itself could have affected the participants. For example, the participants could have gained information on the courses and programs provided by the institute. We cannot, however, fully disentangle these two mechanisms using our survey data. Using register data could help to disentangle the effects by, for example, allowing us to study if the participants enrolled in the same institution where the intervention was organized. Register data would also provide a richer set of data on the educational outcomes, such as whether the individuals were enrolled in a higher degree in the same field as their previous

[1] We can consider the resulting estimate from the IV model as ATET instead of only the LATE, as there were no treated individuals in the control group (e.g., Bloom (1984)).

degree, or if they were switching occupations. This is left for future research.

Our findings are promising as they show that employees can be encouraged to develop their professional competencies mid-career with short peer-group training. From a public policy perspective, the intervention could be useful for supporting continuous learning later in the working career, and it could be especially useful for the employees who are not active in educating themselves. When generalizing the results, we noted that the participants were bachelors of business and administration and engineers and were already relatively highly educated. Further research in this area could focus on the effectiveness of the intervention for people with lower levels of education and workers in occupational groups most likely to be impacted by, say, automation and offshoring.

The final essay estimates the causal effects of general upper secondary education on cognitive and non-cognitive skills measured by a psychological test battery conducted by the Finnish Defence Forces on new conscripts. We began by anchoring the test scores to average annual earnings at ages 35-39 years. We did so to provide an economic interpretation for our estimates and to convert the test scores from the different sub-tests to a one-dimensional interval scale.

We found large differences in both the cognitive and non-cognitive test scores between graduates from the general and vocational tracks in upper secondary education. Anchoring the test scores to earnings at ages 35-39 years revealed that the raw gap in the test scores between those with a completed general education and those without, is worth around 28% of annual earnings at ages 35-39 years. Looking at the cognitive and non-cognitive skills separately, the differences in the test scores are 21% and 7%, respectively.

However, our RDD estimates of the causal effects of general education suggest that there is no direct effect of general education on cognitive and non-cognitive skills. According to our results, completing general upper secondary school has no significant causal effect on either cognitive or non-cognitive skills. The estimates are not statistically significantly different from zero and small in magnitude compared to the raw gap we observed in the test scores between those who graduated general education and those who did not. Further, we showed that being above the cut-off does have a significant impact on peer characteristics. For example, those above the admission cut-off to general education have, on average, a larger share of female peers, have peers with higher GPA from compulsory school, and have peers who score higher in the cognitive and non-cognitive tests. Thus, the zero effects on the test scores are unlikely to be explained by the general and vocational tracks being similar.

In accordance with the lack of effects on the aggregate skill measures, we found no effects on the individual sub-tests either. Finding no effects on measures of fluid intelligence is consistent with the findings of previous literature, according to which fluid intelligence is independent of education (e.g., Carlsson et al. (2015)). Surprisingly, however, we find no effects on measures of verbal or arithmetic skills, although there is much greater focus on languages and math in the general track. The Basic Skills Test measures rather low level arithmetic and verbal skills compared to what is taught in general upper secondary education,

which could explain these findings. Interestingly, we find a significant negative effect on masculinity.

Given our finding that the track choice in upper secondary education does not affect the development of cognitive or non-cognitive skills, the large differences we observed in the test scores between those who graduated general education and those that did not, arose from selection rather than from a direct effect of education. Our study is limited to applicants at the margin of admission to general education. Further research could investigate if the effects of secondary education are different at other parts of the student distribution by, for example, looking at the effects of admission for a more selective school on the test scores. Our study focuses on school track choice in secondary education, but investigating how the education system affects cognitive and non-cognitive skills in earlier or later years, would be an interesting avenue for future research.

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2 COMPREHENSIVE SCHOOL REFORM AND LABOR MARKET OUTCOMES OVER THE LIFECYCLE: EVIDENCE FROM FINLAND

Abstract[*, **]

This study focused on the labor market effects of the Finnish comprehensive school reform in 1972–1977 over the lifecycle. The reform increased the age at which students are separated into vocational and general tracks from 11 to 16 as well as updated the curriculum to include more general content instead of vocational. Using longitudinal administrative register data and exploiting the gradual implementation of the reform, I found a negative effect on employment in early career and a positive effect later in the lifecycle. Results for labor earnings are more nuanced and sensitive to different model specifications, but a positive effect was found in the late career. After the reform, the treated were more likely to choose academic secondary education which could mediate the effects on employment. The results were similar for men and women.

Keywords: comprehensive school, vocational education, employment, earnings, lifecycle
JEL Classifications: I21, I28, J24

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2.1 Introduction

Post World War II, many European countries implemented comprehensive school reforms to increase the number of years of compulsory education, introduce national curricula, and/or abolish or delay tracking. Previous studies have found that comprehensive school reforms increase educational attainment and earnings, on average, as well as intergenerational mobility (Meghir and Palme, 2005; Pekkarinen et al., 2009; Aakvik et al., 2010). Little is known about how these reforms affect labor market outcomes over the lifecycle.

The main contribution of this paper is to provide one of the first estimates—to the best of my knowledge—of the effects of a comprehensive school reform on lifecycle employment and earnings patterns by studying the Finnish comprehensive school reform of 1972–1977 [1]. The reform increased the age at which students are separated into general and vocational tracks from 11 to 16. The reform also updated the curriculum to include more general content instead of vocational, keeping the number of years of compulsory schooling constant. An attractive feature of the reform is that it was gradually implemented across municipalities. The implementation scheme gave rise to a natural experiment utilized in this paper to compare outcomes across birth cohorts and municipalities using a difference-in-differences approach. The implementation scheme, together with extensive administrative register data on the labor market and educational outcomes, means that the causal effects of the reform may be studied through a nearly 30-year period.

Although studies on the reform have been conducted earlier, its effects on labor market outcomes over the lifecycle remain unknown. Previous studies show that the reform did have an impact on several outcomes. First, the reform increased women's probability of enrolling in academic secondary and tertiary education and decreased the gender wage gap (Pekkarinen, 2008). Second, the reform decreased the intergenerational income elasticity by 23% (Pekkarinen et al., 2009). It also had a slight positive impact on verbal test scores measured by the Finnish Army Basic Skills Test. Among students whose parents had less than a high school education, arithmetic and logical reasoning test scores along with verbal test scores saw an increase as a result of the reform (Pekkala Kerr et al., 2013).

The reform effectively reduced the weight of vocational education in the Finnish school system by increasing the age of tracking into vocational and academic tracks and reducing the amount of vocational education in the curriculum

[1] Bhuller et al. (2017) use the Norwegian compulsory schooling law reform as an instrument for education to estimate the effect of schooling on lifecycle earnings. They find that additional schooling results in higher lifetime earnings and a steeper age-earnings pattern. Unlike the Norwegian reform which increased minimum compulsory schooling from 7 to 9 years, the Finnish reform did not affect the minimum years of compulsory schooling, making the mechanisms of the two reforms distinct. Thus, this paper does not attempt to estimate the effects of additional schooling but rather focuses on reporting the effects of the reform over the lifecycle.

of compulsory schooling. Thus, the present paper's hypothesis of the effects of the reform across the lifecycle is based on the literature on the labor market effects of vocational and general education. First, it has been argued that vocational education offers youths an easier transition from school to work and, thus, may be an effective policy in combating youth unemployment (Ryan, 2001; Wolter and Ryan, 2011; Zimmermann et al., 2013). However, due to technological advancements and structural changes in labor markets, the skills provided by vocational education may become obsolete faster than those provided by general education. Due to this skill depreciation, vocational education may lead to relatively high unemployment rates in later career stages. Krueger and Kumar (2004) claim that the slower adaptation of technology caused by skill-specific education may even help explain the difference in the growth rates of Europe and the US.

The existing empirical literature on the labor market effects of vocational and general education mostly supports a trade-off between early and late labor market advantages. The most notable study in this literature is Hanushek et al. (2017). Using microdata from the International Adult Literacy Survey (IALS), it was found that vocational education indeed leads to better school-to-work transition, but it also becomes a disadvantage in terms of employment as early as the age of 50. Further, they found that the magnitude of the effect depends on the intensity of the treatment i.e., the degree of vocationalization of the country's education system. For example, in Germany, where vocational education is strongly based on apprenticeship programs, the trade-off between early and late labor market advantages is starker than in countries such as Finland, which have school-based vocational programs.

Hampf and Woessmann (2017) and Forster et al. (2016) identified similar effects as Hanushek et al. (2017) using the more recent and richer PIAAC survey. Cörvers et al. (2011) estimated age-earnings patterns consistent with a trade-off between early and late labor market advantages for Germany, the Netherlands, and the United Kingdom. In addition, there are also recent country-specific studies with similar results, such as Brunello and Rocco (2017) for Great Britain, Weber (2014) for Switzerland, and Golsteyn and Stenberg (2017) and Stenberg and Westerlund (2015) for Sweden. A contrasting result from Sweden is by Hall (2016), who found no significant effect on risk of unemployment from a pilot scheme of a reform from 1988 to 1993, which lengthened upper secondary vocational education by an additional year of general education. Silliman and Virtanen (in press) exploited a regression discontinuity design created by a centralized admission system in Finland to show that upper secondary vocational education increases annual income by 7 percent. They found no evidence of the effects diminishing with time. However, their data allows the individuals to be followed only until the age of 31, making it impossible to estimate the effects in late career stages.

The present paper also makes a secondary contribution to this literature by examining the causal effects of reducing the vocationalization of a school system over the lifecycle. Many previous papers comparing the lifecycle effects of vocational and general education either don't establish causality or base it on rather strong assumptions. For example, as a result of using cross-sectional data, the

identification of causal effects in Hanushek et al. (2017) was based on the assumption that the selection of the type of education does not vary over time. Another possible source of bias in their model was that the test scores from IALS used as control variables could have very well been affected by the person's education before taking the test (treatment), resulting in a "bad control" situation (see Angrist and Pischke (2009)).

The results of this paper support the hypothesis of a trade-off between employment early and late in the lifecycle. The results show that the reform harmed employment in the early career but that this gap reduced with age and finally turned into a positive effect from the mid-30s onward. Results for labor earnings are more imprecise and sensitive to different model specifications but a positive effect was found from late 30s onward. Changes in educational outcomes are a likely mediator of the effects of the reform. An analysis of the reform's effect on completed degrees at age 30 showed that the reform increases educational attainment and the probability of choosing the academic track instead of the vocational track. Further, the results related to the labor market and educational outcomes are similar for men and women.

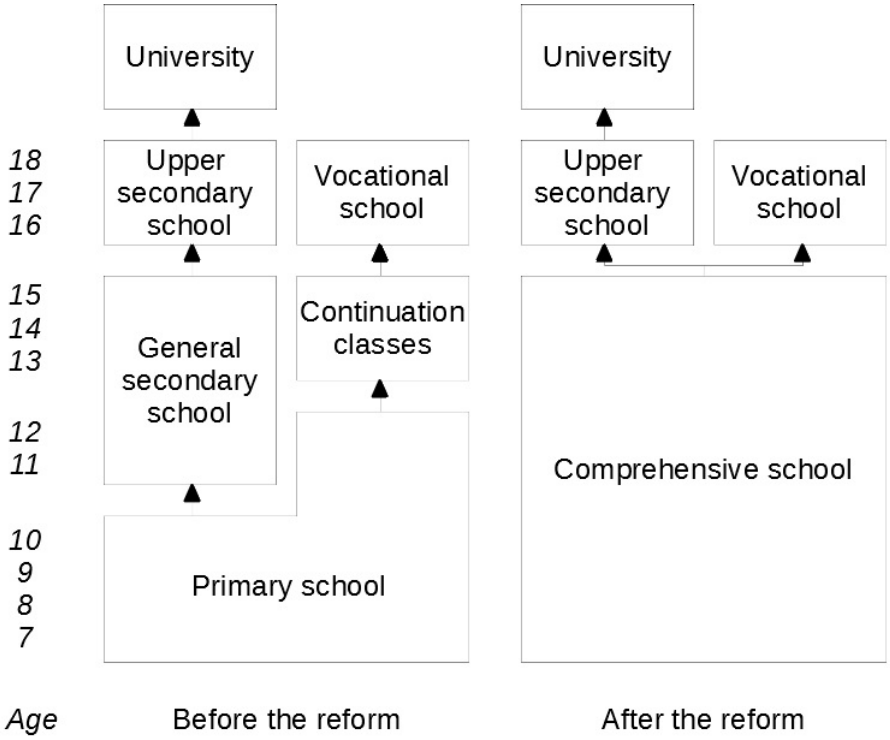
2.2 The Finnish Comprehensive School Reform of 1972–1977

Before the comprehensive school reform, there was a two-track school system in place in Finland. A comparison of the Finnish school systems before and after the reform is depicted in Figure 2.1. Under the old two-track system, students attended primary school (*kansakoulu*) for four years starting from age 7, after which they chose between applying to a more academic general secondary school (*oppikoulu*) and continuing primary school. Those who were accepted into the five-year general secondary school often continued into a three-year upper secondary school ending in a matriculation examination, the completion of which made them eligible for university.

Students who did not attend general secondary school spent two more years in primary school. After this, they attended practically oriented continuation classes at the primary school level for two to three more years. Upon graduating from primary school, they had the possibility of continuing their education in vocational schools. Unlike the general secondary schools, the continuation classes did not make them eligible for upper secondary school or universities. The continuation classes consisted mostly of vocational education. It was required by law for the curriculum to contain subjects and practical exercises relevant to the local industries. For example, in rural areas, the continuation classes could involve agriculture or forestry.

Under the reformed educational system, all students attended the same comprehensive school until the age of 16, after which they could apply to either a vocational or an upper secondary school. The curriculum in comprehensive schools was close to that of the old general secondary schools, which exposed the

FIGURE 2.1 Finnish education system before and after the reform. Source: Pekkarinen (2008)



students, who would have stayed in primary school without the reform, to a more academic curriculum (see Table 2.1). Compared to primary school, comprehensive school included a greater level of mathematics and science and two compulsory foreign languages. Although the structure of the comprehensive school was similar for all students, ability grouping was not abolished until the mid-1980s. For example, mathematics and foreign languages had three different levels in grades 7 to 9 (Sahlberg, 2014).

The reform did not increase the minimum school leaving age, which was already 16 before the reform, nor did it increase the length of compulsory schooling (Pekkala Kerr et al., 2013). Thus, the effects of the reform arise from the changes in the tracking age and curriculum. The change in tracking age also caused the selection criteria into the vocational and academic tracks to differ from before the reform, which is also likely to imply changes in peer groups. Unfortunately, as is the case with many real-life reforms, the relative importance of the different factors could not be determined since they tend to change simultaneously.

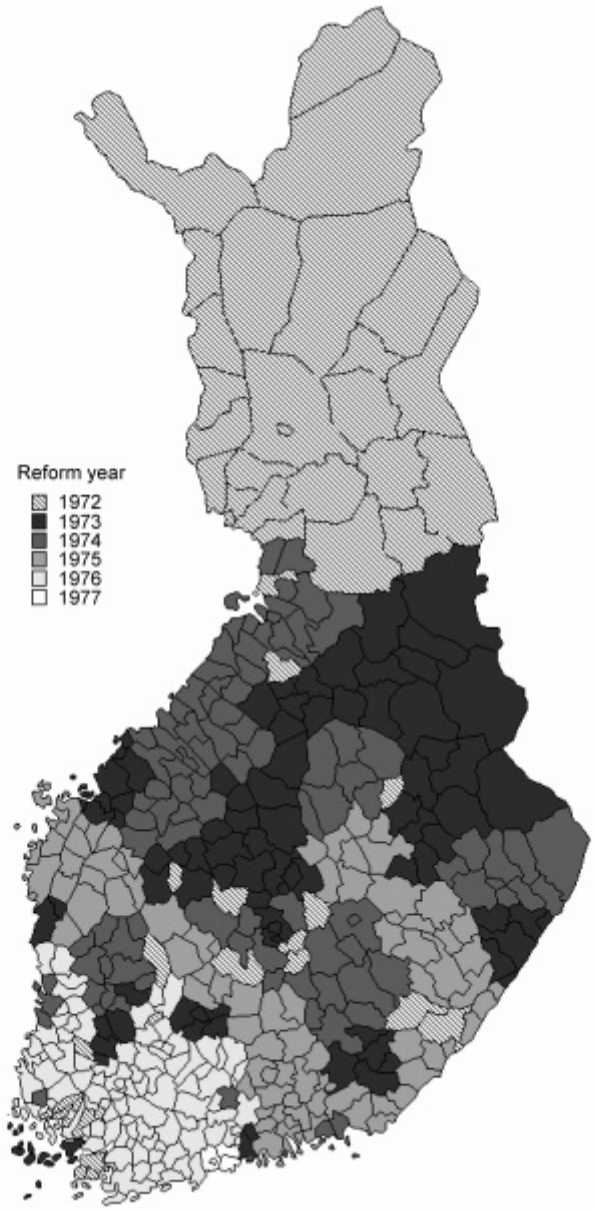
The reform was implemented gradually across the country (see Figure 2.2). Beginning in 1972, the first municipalities to adopt the reform were mainly from Lapland in the north of the country. From there, the reform moved towards the south with the capital region being last to adopt it in 1977. Students who were entering first to fifth grades (ages 7 to 11) by the end of the year in which the reform came into effect in their municipality were affected by it. This gradual implementation is what creates the natural experiment that was exploited in this study.

TABLE 2.1 Examples of 7th grade curriculum in primary school, general secondary school, and comprehensive school. Table adapted from Kettunen et al. (2012).

Subject	Continuation classes of primary school (Helsinki 1932)	General secondary school (1941 curriculum)	Comprehensive school (1970 curriculum)
Religion, history of religions, and ethics	1	2	1
Finnish	4	4	3
Civics	3		1
History, social studies, and economics		3	2
Swedish		3	
First foreign language			3
Second foreign language		5	2
Biology	2		
Natural history and geographics		4	2
Physics and chemistry			2
Health education	1		
Singing/Music	2*	2*	1**
Gymnastics and sports/Physical education	2	4	2
Drawing, modelling, and cursive/Art		2	1**
Craft and techical drawing/Craft	14	2	2
Home economics	9*		1
Accounting	2		
Vocational counseling/Student counseling	1		1
Optional subjects			4-6

* Only for girls
** Optional with Music/Art

FIGURE 2.2 Geographical representation of the implementation of the reform. Source: Pekkarinen (2008)



2.3 Data and Empirical Strategy

2.3.1 Data

The complete population register data from Statistics Finland was used for this study. The pertinent information was collected from the Longitudinal Census Files, Longitudinal Employment Statistics Files, and Statistics Finland's Register of Completed Education and Degrees. First, using birth dates, individuals born between 1960 and 1966 were selected for the study. Then, using an individual specific ID code, each individual's municipalities of residence for the years 1972–1977 were gathered. With these two variables and the information on the year in which the reform was adopted in each municipality, I coded an indicator variable for whether or not the individual was exposed to the reform. Further, the number of years that they spent in comprehensive school was also determined. This information is summarised in Table 2.2.

TABLE 2.2 Implementation of the reform, number of observations and years spent in comprehensive school

	Reform year						
Birth cohort	1972	1973	1974	1975	1976	1977	Total
1960	7113	10482	15351	15110	15337	8829	72222
1961	7312 5 years	10353	15362	15034	15393	8821	72275
1962	7091 6 years	10228 5 years	14921	15048	15432	9148	71868
1963	6879 7 years	10009 6 years	14720 5 years	15137	15653	9345	71743
1964	6531 8 years	9607 7 years	14143 6 years	14751 5 years	15527	9591	70150
1965	6068 9 years	9021 8 years	13631 7 years	14425 6 years	14781 5 years	9532	67458
1966	5870 9 years	8717 9 years	13312 8 years	13935 7 years	14925 6 years	9522 5 years	66281
Total	46864	68417	101440	103440	107048	64788	491997

Note: The shaded cells adopted the comprehensive school reform. The first line in a cell reports the number of observations, while the second line in the shaded cells reports the number of years that each cell was exposed to the comprehensive system.

The treatment status was then linked to labor market and educational outcomes. Data on the employment status of the individuals at the end of the year are available on an annual frequency from 1987 to 2015. Annual labor earnings are available from 1987 to 2016. Annual labor earnings include wages and income from self-employment, which deflated to 2015 euros. Considering the birth cohorts chosen in the data as well, data on employment is available from a min-

imum age of 21 to a maximum age 55 and data on earnings from a minimum of 21 to a maximum of 56. Finally, data on all completed degrees are available from 1970 to 2015.

Observations had to be excluded for various reasons. First, those who had deceased before the year 1987 were dropped. Second, people whose information on municipality of residence was missing for any of the years between 1972 and 1977 were dropped. Also, those who moved between municipalities that adopted the reform in different years than the period of 1972–1977 were dropped from the data because the treatment for these individuals could not be unambiguously determined. Finally, people living in the autonomous Åland Islands were dropped. Altogether, these exclusions reduced the sample size by ∼16% from the whole population of individuals born between 1960 and 1966. From the remaining sample, 45.8% belong to the treatment group. The sample contains observations from 496 municipalities. The median number of observations per municipality-birth cohort combination is 247.

An examination of employment and earning patterns over age shows differences in labor market outcomes between those affected and not affected by the reform (see Figures 2.B.1 and 2.B.2 in Appendix 2.B). The employment rates of the non-treated group were higher than those treated in the earlier years; however, this difference vanishes with age. For earnings, the pattern is not as clear. However, a simple analysis such as this fails to account for the differences between municipalities and birth cohorts which may distort the results, as argued in Section 2.2. The figures also capture period-specific effects such as depression in the early 1990s in Finland. Thus, a more sophisticated approach should be adopted.

2.3.2 Empirical Strategy

Following Hanushek et al. (2017), the following difference-in-differences model was estimated to test out the hypothesis of a trade-off between early- and late-career employment. To this end, age-profiles of the outcome variable are allowed to linearly differ between the treated and non-treated[2]:

$$\begin{aligned} y_{icmt} = {} & \alpha_0 + \alpha_1 Age_{it} + \alpha_2 Age_{it}^2 + \beta_1 Reform_{icm} + \beta_2 Reform_{icm} \times Age_{it} \\ & + \gamma_0 D_c + \delta_0 D_m + (\gamma_1 D_c + \delta_1 D_m) Age_{it} + \epsilon_{icmt}, \end{aligned} \tag{2.1}$$

where y_{icmt} is the relevant outcome for individual i belonging to birth cohort c and living in municipality m observed in year t, and $Reform$ is an indicator for having attended comprehensive school. D_c and D_m are dummies for each cohort and municipality, respectively. Cohort- and municipality-specific linear age

[2] In addition to being easier to interpret, a linear model fits the data better than higher polynomial models based on AIC and BIC. In the case of employment, the estimates obtained from the higher polynomial models are qualitatively similar to those of the linear model. Estimated effects on earnings are more sensitive to model specification. In the robustness checks below, estimates from separate regressions for each year of age are reported to further address concerns related to model specification.

trends were also added to the regressions[3]. Age was added to the equation as both a linear and squared term to capture the typical age-profile of employment and income. As seen in Figure 2.B.1, it was found that the second-degree polynomial may not fit the employment data well until after age 30; therefore, this assumption was later relaxed as a robustness check.

As evident from Figure 2.2, the implementation of the reform varied geographically. Since the northern and eastern parts of Finland have lower income per capita and level of education than the southern and western parts, the indicator variable for the reform may be endogenous in the absence of the municipality fixed effects and time trends. Further, similar differences may exist among the birth cohorts, which is why fixed effects and time trends were also added for each cohort. Year dummies were omitted from the equation to avoid perfect multicollinearity. With a fixed age, cohort effects effectively control for year effects, but, as a result, they are indistinguishable from each other in the model.[4] However, these estimates are not of importance since the main focus of this paper lies in the effects of the reform.

The parameters of interest in Equation 2.1 are β_1 and β_2. Together, these parameters describe the lifecycle effects of the reform. β_1 measures the initial impact of the reform on the outcome of interest (normalized to age 21 in this paper). β_2 captures the effect of the reform with each additional year of age. The identification of the causal effect of the reform is based on the usual common trends assumption. In this case, the assumption states that the timing of the reform across regions is not systematically related to other factors that affect labor market outcomes over the lifecycle. Thus, in the absence of the reform, differences in the age profiles of the outcome would have remained constant among the municipalities that adopted the reform at different times. To test the validity of the assumption, Figure 2.B.3 plots the average employment and annual labor earnings in the different implementation regions using data on the pre-reform 1960 birth cohort over the lifecycle. A visual inspection suggests that the age profiles of both the employment and annual labor earnings were similar between the regions prior to the reform, which supports the validity of the common trends assumption.

All the models were separately estimated by gender. This approach was adopted because earlier papers on the reform indicate heterogeneous effects for men and women. All the models were estimated by OLS, which means that linear probability models were used for binary outcome variables. A linear probability model was used to simplify the results since the marginal effects for linear probability models corresponded to the estimates[5].

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3 Alternatively, one could use dummies and trends for each implementation region instead of municipalities since the variation in reform status within birth cohorts occurs at both levels. Using municipalities instead of the implementation region in the model specification does not affect the estimates but does produce smaller standard errors.

4 This situation is also known as the age-period-cohort problem. See for example Heckman and Robb (1985).

5 For robustness, the models were also estimated using a probit model. The marginal effects from probit models were similar to those from the linear probability models

TABLE 2.3 Effects of the reform on employment over the lifecycle

	(1) Men	(2) Women	(3) Men	(4) Women
Reform	-0.048***	-0.061***	-0.002	-0.004
	(0.002)	(0.004)	(0.002)	(0.003)
Reform × (Age/10)	0.028***	0.036***	0.001	0.002
	(0.001)	(0.002)	(0.001)	(0.001)
Observations	6,931,132	6,691,826	6,931,132	6,691,826
Municipality and birth cohort FE	YES	YES	YES	YES
Municipality and birth cohort trends	NO	NO	YES	YES

Note: Dependent variable: Employed (= 1, otherwise 0). Employment status is measured at the end of the year. Age in the sample ranges from 21 to 55. Age is subtracted by 21 in the regressions. Standard errors clustered at the municipality level reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

2.4 Results

2.4.1 Main results

Table 2.3 reports regression estimates of Equation 2.1 with employment status as the dependent variable. Columns 1 and 2 present the estimates from a model including municipality and cohort fixed effects for men and women separately. The results show that the reform had a negative initial effect on employment (normalized to age 21). The estimated effect is -4.8 percentage points for men and -6.1 percentage points for women. However, the reform's interaction with age was found to be positive, meaning that the employment gap between the treated and non-treated cohorts narrows by 2.8 percentage points for men and 3.6 percentage points for women with every decade. These estimates imply that, in terms of employment, attending comprehensive school goes from being a disadvantage to an advantage at approximately age 38 for both men and women. Columns 3 and 4 include municipality and cohort-specific linear age trends with the model. After the addition of these trends, the effects of the reform can be seen to practically vanish. Although the estimates still show a negative initial impact and positive linear interaction with age, the estimates are close to zero and statistically insignificant.

As argued by Wolfers (2006), a model using a single dummy to capture the full effect of the reform may be misspecified if the actual treatment effect is not constant. This misspecification might result in the model being unable to separate the effect of the reform from the effects of the cohort and municipality. Since the students were exposed to the reform for varying numbers of years depending on which grade they were in when the reform was adopted in their municipality, the effects of the reform are likely heterogeneous with respect to treatment intensity.

To circumvent this issue, a richer variation in the treatment intensity was exploited instead of assuming a constant treatment effect. To this end, the treatment effects were allowed to linearly depend on the number of years spent in compre-

TABLE 2.4 Effects of the reform on employment by treatment intensity over the lifecycle

	(1) Men	(2) Women	(3) Men	(4) Women
Intensity	-0.016***	-0.023***	-0.005***	-0.008***
	(0.001)	(0.002)	(0.001)	(0.002)
Intensity × (Age/10)	0.011***	0.014***	0.003***	0.005***
	(0.000)	(0.001)	(0.001)	(0.001)
Observations	6,931,132	6,691,826	6,931,132	6,691,826
Municipality and birth cohort FE	YES	YES	YES	YES
Municipality and birth cohort trends	NO	NO	YES	YES

Note: Dependent variable: Employed (=1, otherwise 0). Employment status is measured at the end of the year. The age range of the sample is from 21 to 55. The variable *Intensity* measures the number of years spent in comprehensive school (ranging from 0 to 5). Age is subtracted by 21 in the regressions. Standard errors clustered at the municipality level reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

hensive school.[6] The variable *Intensity* in Table 2.4 takes on the integer values from 0 (not treated) to 5 (spent the full nine years in comprehensive school). The estimates in Table 2.4 are based on an OLS estimation of Equation 2.1, with the only difference being that the reform indicator is replaced by a measure of treatment intensity. Finally, using treatment intensity instead of a single indicator requires no further identifying assumptions, and the discussion on the identification strategy in Section 2.3.2 still applies.

Table 2.4 reports the reform's effect on employment by the intensity of treatment. Columns 1 and 2 show the estimations of the model without the municipality- and cohort-specific trends, separated by gender. Without the trends, the obtained estimates are similar to those in Table 2.3. Specifically, they show that treatment intensity has a negative initial impact and then a positive interaction with age. The estimates indicate that the initial impact of the reform on employment ranges from -1.6 to -8 percentage points for men and from -2.3 to -11.5 percentage points for women, depending on treatment intensity. The interaction terms show that the gap in employment narrows every ten years by 1.1 to 5.5 percentage points for men and 1.4 to 7 percentage points for women. Consequently, the crossover age is around 36 for men and 37 for women.

In Columns 3 and 4, the municipality and cohort-specific trends were added to the model. Adding the trends had an attenuating impact on the estimates but not to the degree as in Table 2.3. The estimates show that the reform had an initial effect on employment of -0.5 to -2.5 percentage points for men and -0.8 to -4 percentage points for women, depending on the number of years they spent in comprehensive school. With every ten years, the probability of employment increases by 0.3–1.5 percentage points for men and 0.5–2.5 percentage points for women. With this specification, the crossover in the employment probability oc-

[6] Linearity was assumed to simplify the results. Using dummies for the number of years spent in comprehensive school produced similar estimates and particularly showed that treatment effects grow with treatment intensity, so the assumption of linearity here was not too restricting.

TABLE 2.5 Effects of the reform on annual labor earnings over the lifecycle

	(1) Men	(2) Women	(3) Men	(4) Women
Reform	-399.06	-1279.49***	95.39	-37.48
	(416.61)	(251.71)	(148.67)	(79.51)
Reform × (Age/10)	227.95***	719.24***	-105.08	-14.88
	(223.95)	(104.20)	(110.86)	(69.18)
Observations	7,137,671	6,891,804	7,137,671	6,891,804
Municipality and birth cohort FE	YES	YES	YES	YES
Municipality and birth cohort trends	NO	NO	YES	YES

Note: Dependent variable: Annual labor earnings (includes wages and income from self-employment.) Earnings are deflated to 2015 prices using the CPI. 0 earners are not dropped. Age in the sample ranges from 21 to 56. Age is subtracted by 21 in the regressions. Standard errors clustered at the municipality level reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 2.6 Effects of the reform on annual labor earnings by treatment intensity over the lifecycle

	(1) Men	(2) Women	(3) Men	(4) Women
Intensity	235.16	-267.22***	315.63***	-40.18
	(197.08)	(92.42)	(112.50)	(39.79)
Intensity × (Age/10)	-62.45***	251.78***	-87.00	65.10**
	(85.64)	(34.76)	(65.78)	(28.60)
Observations	7,137,671	6,891,804	7,137,671	6,891,804
Municipality and birth cohort FE	YES	YES	YES	YES
Municipality and birth cohort trends	NO	NO	YES	YES

Note: Dependent variable: Annual labor earnings (includes wages and income from self-employment.) Earnings are deflated to 2015 prices using the CPI. 0 earners are not dropped. The variable *Intensity* measures the number of years spent in comprehensive school (ranging from 0 to 5). Age in the sample ranges from 21 to 56. Age is subtracted by 21 in the regressions. Standard errors clustered at the municipality level reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

curs approximately at age 37 for men and age 36 for women.

Tables 2.5 and 2.6 report estimates of the reform's effect on annual labor earnings. For women, the estimates from models without trends show a trade-off between early and late-career earnings. Column 2 in Table 2.5 shows that initially, the reform decreased annual earnings by around 1300 euros, but every ten years the annual earnings increased by around 700 euros. A similar effect is reported in Column 2 of Table 2.6 using treatment intensity as the explanatory variable. However, these estimates were not robust to adding the municipality- and cohort-specific trends. For men, the estimates in Tables 2.5 and 2.6 are even more sensitive to the model specification. The initial effect of the reform was found to be positive and the interaction with age to be negative in most models. However, none of these results are statistically significant.

2.4.2 Robustness checks

A possible concern related to the main results is that they may be sensitive to the model specification which was especially seen in the earnings results. Thus, to allow for a more flexible model, the reform's effect on employment and earnings were separately estimated for each year of age. The regressions included either the reform indicator or the measure of treatment intensity and fixed effects for the birth cohorts and municipalities. The estimates from these regressions are presented in Appendix 2.B.

The results show a similar age–employment pattern as suggested by the main specification. The estimates support the hypothesis of a trade-off between early- and late-career employment, although some non-linearities are present in the early career period. As with the linear specification, using the simple reform indicator (Figure 2.B.4) failed to produce significant results. However, as shown in Figure 2.B.5, treatment intensity has a negative effect on the probability of employment until the late 30s (with some exceptions in the late 20s), after which the effect is positive. This crossover age is close to the one suggested by the main specification.

The estimated effects on earnings are noisier than the effects on employment, which may explain the poor fit of the linear model in this case, especially for men (see Figures 2.B.6 and 2.B.7). The preferred point estimates using treatment intensity as the explanatory variable (Figure 2.B.7) are mostly positive for men and women from the late-30s onward, although the estimates for men are not as precise as for women. For women, the point estimates are mostly negative prior to this age but for men, there is no clear pattern. The sensitivity of these results to different model specifications makes it difficult to conclude that a trade-off is present in the data related to earnings, although the estimates from this model point to possible earnings gains beginning from late-30's as a result of the reform[7].

Several additional robustness checks were used to evaluate the estimated effects on employment. The results can be found in Appendix 2.B. First, given that the reform increased the probability of attending higher education, it is likely that the treated would have entered the labor market later. Similarly, the non-treated were less likely to obtain higher education, which could have led them to enter the labor market earlier and even exit it earlier by retiring. To ensure that the results were not driven by this kind of behavior, the model was estimated using a sample of only those individuals who were either employed or unemployed. Second, observations from the Helsinki region were dropped; the reform was met with fierce resistance and most of the students were already attending general secondary school, making the reform largely redundant (Pekkarinen, 2008; Pekkarinen et al., 2009). Third, the data was balanced such that only persons

[7] The findings here are in line with Pekkarinen (2008) and Pekkarinen et al. (2009) who find the average effect of the reform on taxable income in the year 2000 (which corresponds to ages 34 to 40) to be insignificant. In my results, the point estimates for ages 34–40 include both negative and positive values.

observed in each year were retained. Finally, the model was estimated using a sample that included all the birth cohorts from 1960 to 1970. In the original sample, all treatment intensities were not observed in all municipalities, leading to the estimates reflecting differences between the municipalities. By including the birth cohorts from 1967 to 1970, all treatment intensities were observed in all the municipalities. All of the aforementioned robustness checks left the results qualitatively unchanged.

2.4.3 Effects of the reform on post-compulsory education

Since the reform increased the tracking age and updated the curriculum to include more academic content, an interesting question arises of whether the reform also affected post-compulsory schooling. Changes in educational attainment or the share of students choosing the academic track over the vocational track are likely to affect labor market outcomes over the lifecycle. To investigate these effects, Table 2.7 presents estimates of the reform's effect on education at age 30. The reported estimates are only from a model of treatment intensity because using the reform indicator produced statistically insignificant estimates close to zero, as with the previous results.

The estimates show, on average, an increase in the probability of attaining an upper secondary degree. This effect was found to be 0.6–3 percentage points (significant at 1% level) for men and 0.7–3.5 percentage points for women (significant at 5% level), depending on the treatment intensity. Relative to the mean of the outcome, these estimates correspond to increases of around 2–11 percent and 1–7 percent for men and women, respectively.

Conversely, the reform's estimated effects on the probability of completing a vocational degree were found to be negative, ranging between -0.2 and -1 percentage points for men based on the treatment intensity. However, the estimate was not significantly different from zero. For women, the resulting effect was between -0.6 and -3 percentage points (around -1 to -6 percent relative to the mean), and it was significant at the 5% level.

Finally, the reform also had a positive effect on the probability of completing a tertiary degree. In the Finnish context, these include bachelor's and master's degrees from universities and vocational universities. The reform increased the probability of completing a tertiary degree by 0.6–3 percentage points for men (around 2–11 percent relative to the mean) and 0.8–4 percentage points for women (around 2–10 percent relative to the mean), depending on the treatment intensity. Both the estimates were found to be significant at the 1% level.

The reform's effects on post-compulsory education are likely to contribute to its effect on labor market outcomes over the lifecycle. Based on previous literature on general and vocational education (e.g., Hanushek et al. (2017)), the shift from upper secondary vocational to general education found here is in line with the estimated negative effect on employment early in the career and a positive effect later on. Similarly, the increase in the probability of completing tertiary education might explain the estimated age-employment pattern: Participating in

TABLE 2.7 Effects of the reform on education at age 30

	Upper secondary degree		Vocational degree		Tertiary degree	
	(1)	(2)	(3)	(4)	(5)	(6)
	Men	Women	Men	Women	Men	Women
Intensity	0.006***	0.007**	-0.002	-0.006**	0.006***	0.008***
	(0.001)	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)
Outcome mean	0.279	0.477	0.556	0.516	0.267	0.383
Observations	250,660	241,337	250,660	241,337	250,660	241,337

Note: The variable *Intensity* measures the number of years spent in comprehensive school (ranging from 0 to 5). Standard errors clustered at the municipality level reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All columns control for municipality and cohort fixed effects.

tertiary education is likely to postpone labor market entry, thus decreasing employment in early career. However, the increased educational attainment could provide a better employment probability later in the individual's career.

2.5 Conclusion

This paper focused on the labor market effects of the Finnish comprehensive school reform over one's lifecycle using complete population administrative registry data. The reform transitioned the Finnish education system towards more general education by increasing the age at which students were tracked, into academic and vocational tracks, from 11 to 16 and by updating the curriculum to include more academic content. The municipalities' gradual implementation of the reform was exploited as a natural experiment to estimate the causal effects of the reform.

The results show that the reform decreased the affected population's employment probability in early career stages, but from their mid-30s onward, having attended comprehensive school turned into an employment advantage. Results for labor earnings are sensitive to different model specifications, but a positive effect was found in the late career by running separate regressions for each year of age. These results resemble findings from earlier literature comparing the lifecycle effects of general and vocational education. Changes in post-compulsory educational outcomes are likely mechanisms through which the reform affected labor market outcomes. The reform increased the probability of completing an upper secondary degree, instead of a vocational degree, as well as the probability of completing a tertiary degree. The obtained results related to the labor market and educational outcomes over the lifecycle were similar for men and women.

This paper provides one of the first estimates of the effects of a comprehensive school reform on labor market outcomes over the lifecycle. A limitation of the study is that it was impossible to disentangle the relative importance of the tracking age change from the curriculum changes. However, I argue that, in practice, comprehensive school reforms are likely to change both the curriculum and

tracking age, making the results here relevant for evaluating the lifecycle effects of past and future reforms.

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2.A Appendix: Identification Strategy

Without loss of generality, consider two municipalities, m_j, where $j \in \{0,1\}$ denotes the year in which the municipalities adopt the reform. Within both these municipalities, there are two cohorts, c_k, whose birth years are denoted by $k \in \{0,1\}$. Assume that the reform r_{jk} is implemented such that it affects only cohort c_1 and only in municipality m_0.

Suppose we are interested in estimating the effect of the reform β at age a on an outcome y. Assume that there is a linear relationship between municipality, birth cohort, year, reform status, and y:

$$y_{ijkl} = a + m_j + c_k + t_l + \beta r_{jk} + \epsilon_{ijkl}, \tag{2.A.1}$$

where $E[\epsilon_{ijkl}|j,k,l] = 0$. t_k denotes year effects. Since there are two cohorts observed at the same age, there must also be two years in which they are observed, so that $k \in \{0,1\}$. This exercise also highlights the age-period-cohort problem mentioned in Section 2.3. With age held constant, it impossible to disentangle the cohort and year effects. Thus, from now on, only age- and cohort-related effects are included, and the year effects are omitted, although these are implicitly included in the cohort effects. The model now simplifies to:

$$y_{ijk} = a + m_j + c_k + \beta r_{jk} + \epsilon_{ijk}, \tag{2.A.2}$$

Note that it is also assumed that the common trends assumption holds. This assumption states that the outcome y would solely be determined by the age, cohort, and municipality effects in absence of the reform, i.e., $E[y_{0ijk}|j,k] = a + m_j + c_k$, where $y_{0ijk} = y_{ijk}$ when $r_{jk} = 0$.

To identify the effects of the reform, I use a difference-in-differences estimator (DiD). To see that this estimator indeed identifies the effect of the reform, first, the difference between the expectations of the two cohorts in each municipality was taken, beginning with m_0:

$$\Delta_0 \equiv E[y_{ijk}|j=0,k=1] - E[y_{ijk}|j=0,k=0] = (c_1 - c_0) + \beta \tag{2.A.3}$$

Similarly, for m_1:

$$\Delta_1 \equiv E[y_{ijk}|j=1,k=1] - E[y_{ijk}|j=1,k=0] = (c_1 - c_0) \tag{2.A.4}$$

Finally, taking the difference between Equations 2.A.3 and 2.A.4, we have the DiD-estimator:

$$\Delta_0 - \Delta_1 = (c_1 - c_0) + \beta - (c_1 - c_0) = \beta \tag{2.A.5}$$

which is an unbiased estimator of the reform's effect. In practice, the estimator is implemented by including municipality and cohort fixed effects as regressors. This simple exercise can also be generalised to consider age-dependent effects. To comply with the model in Equation 2.1, interactions with age were added. The common trends assumption now states that $E[y_{0ijk}|j,k] = a + m_j^0 + c_k^0 + a(m_j^1 + c_k^1)$, and the outcome at age a can be written as:

$$y_{ijk} = a + m_j^0 + c_k^0 + \beta r_{jk} + a(m_j^1 + c_k^1 + \delta r_{jk}) + \epsilon_{ijk} \tag{2.A.6}$$

Δ_0 and Δ_1 can now be expressed as:

$$\begin{aligned} \Delta_0 &\equiv E[y_{ijk}|j=0,k=1] - E[y_{ijk}|j=0,k=0] && (2.A.7) \\ &= (c_1^0 - c_0^0) + a(c_1^1 - c_0^1) + \beta + \delta a \\ \Delta_1 &\equiv E[y_{ijk}|j=1,k=1] - E[y_{ijk}|j=1,k=0] && (2.A.8) \\ &= (c_1^0 - c_0^0) + a(c_1^1 - c_0^1) \end{aligned}$$

The DiD-estimator is the difference between Equations 2.A.7 and 2.A.8:

$$\Delta_0 - \Delta_1 = (c_1^0 - c_0^0) + a(c_1^1 - c_0^1) + \beta + \delta a - (c_1^0 - c_0^0) - a(c_1^1 - c_0^1) = \beta + \delta a \tag{2.A.9}$$

which, again, is unbiased. It is worth noting that in Equation 2.A.9, the effect of the reform at age a consists of the initial impact β and the age-dependent effect δa.

2.B Appendix: Figures and Tables

FIGURE 2.B.1 Employment by age and reform status.

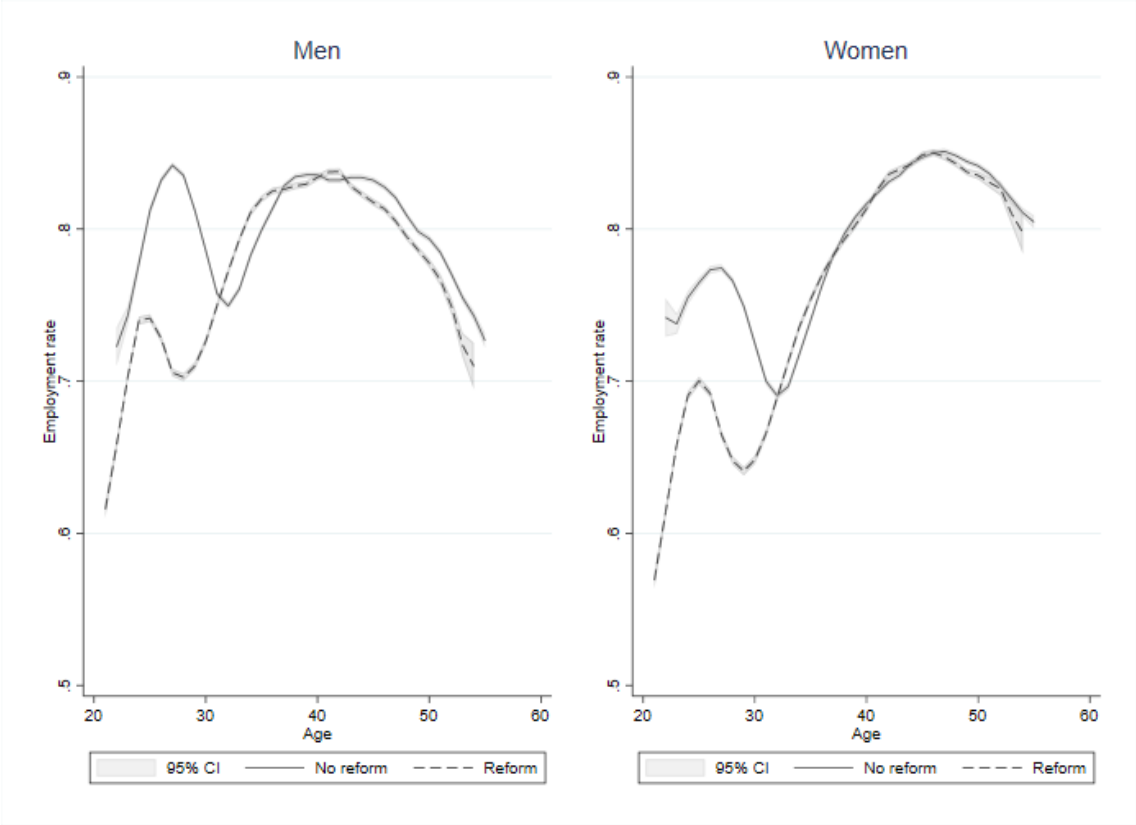


FIGURE 2.B.2 Annual labor earnings (2015 euros) by age and reform status.

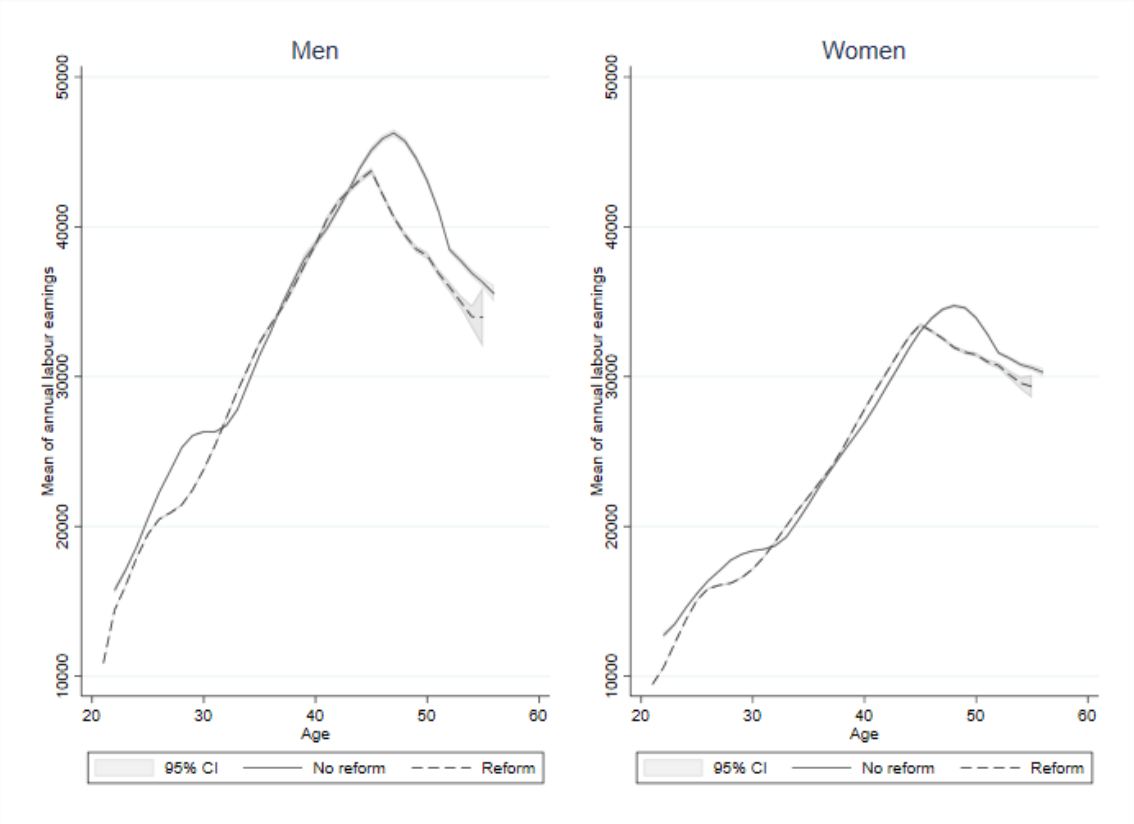
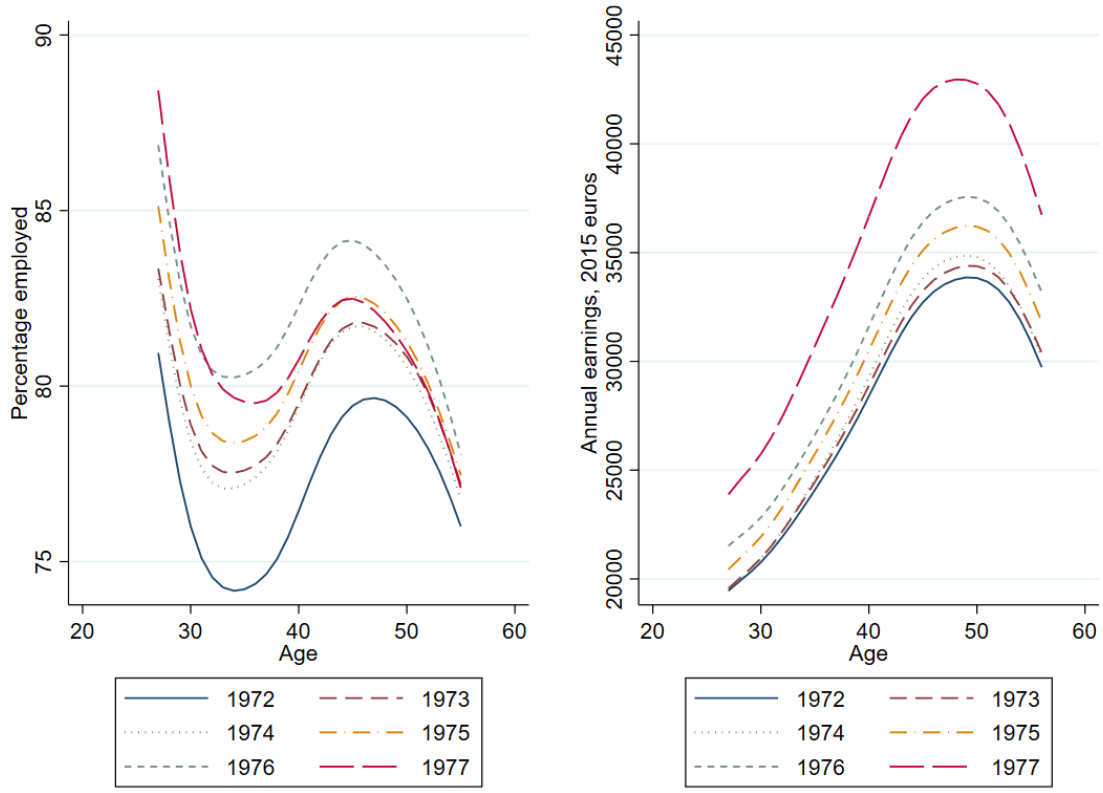
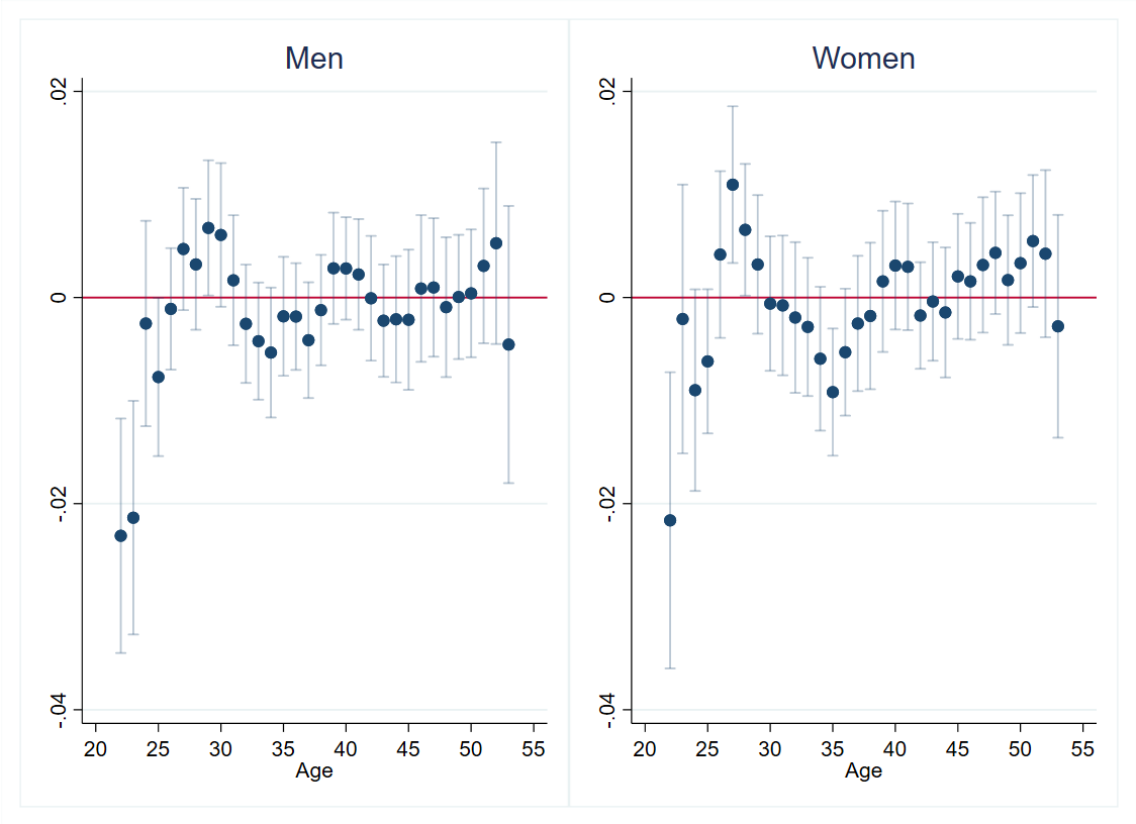


FIGURE 2.B.3 Employment and annual labor earnings by age and implementation region, 1960 birth cohort.



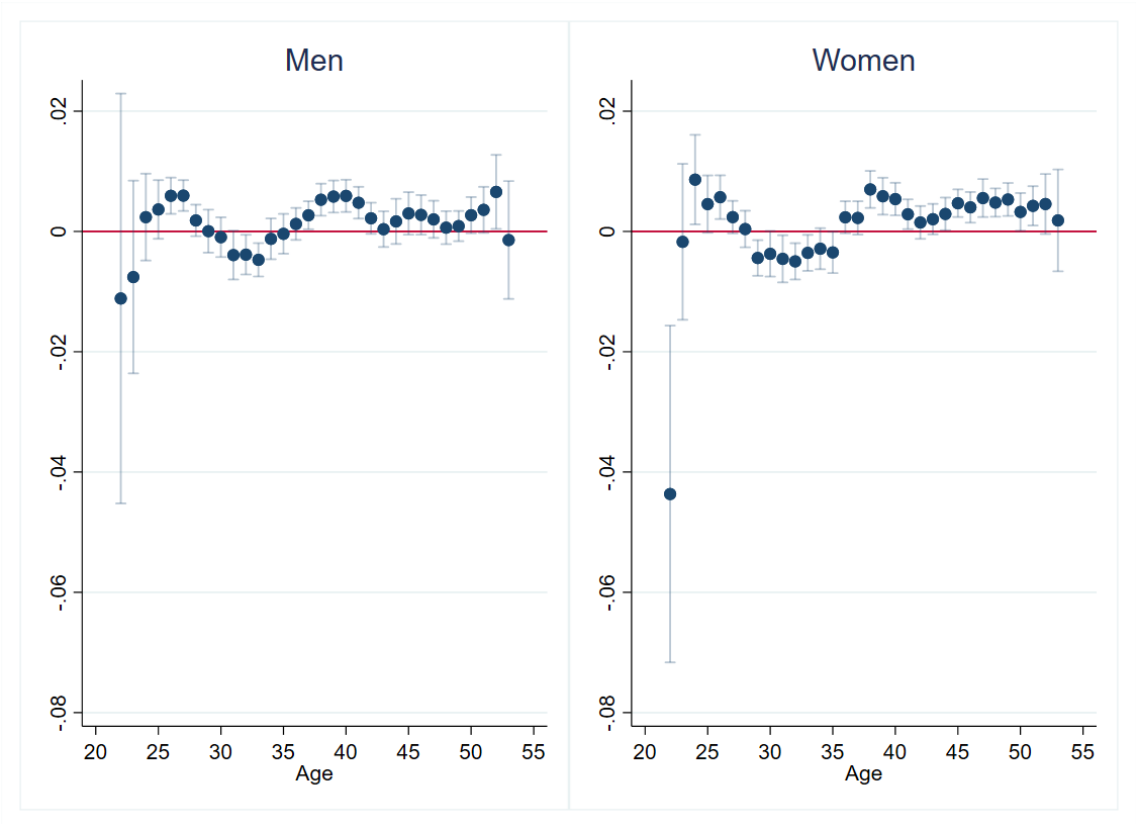
Note: Smoothed scatter plots using locally weighted regressions.

FIGURE 2.B.4 Effect of the reform on employment over the lifecycle.



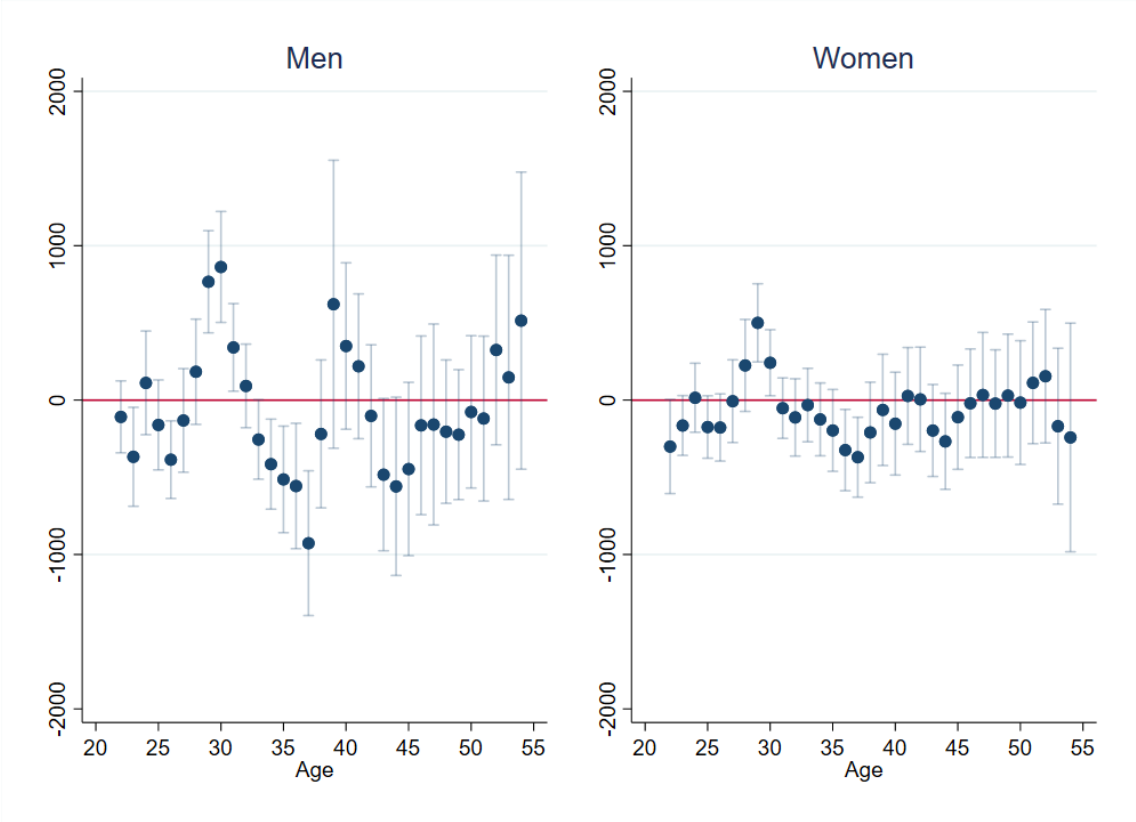
Note: Point estimates of the school reform's effect with 95% confidence intervals from separate regressions for each year of age. Dependent variable: Employed (= 1, otherwise 0). Employment status is measured at the end of the year. Each regression includes fixed effects for municipalities and cohort. Standard errors clustered at the municipality level.

FIGURE 2.B.5 Effect of the reform on employment by treatment intensity over the life-cycle.



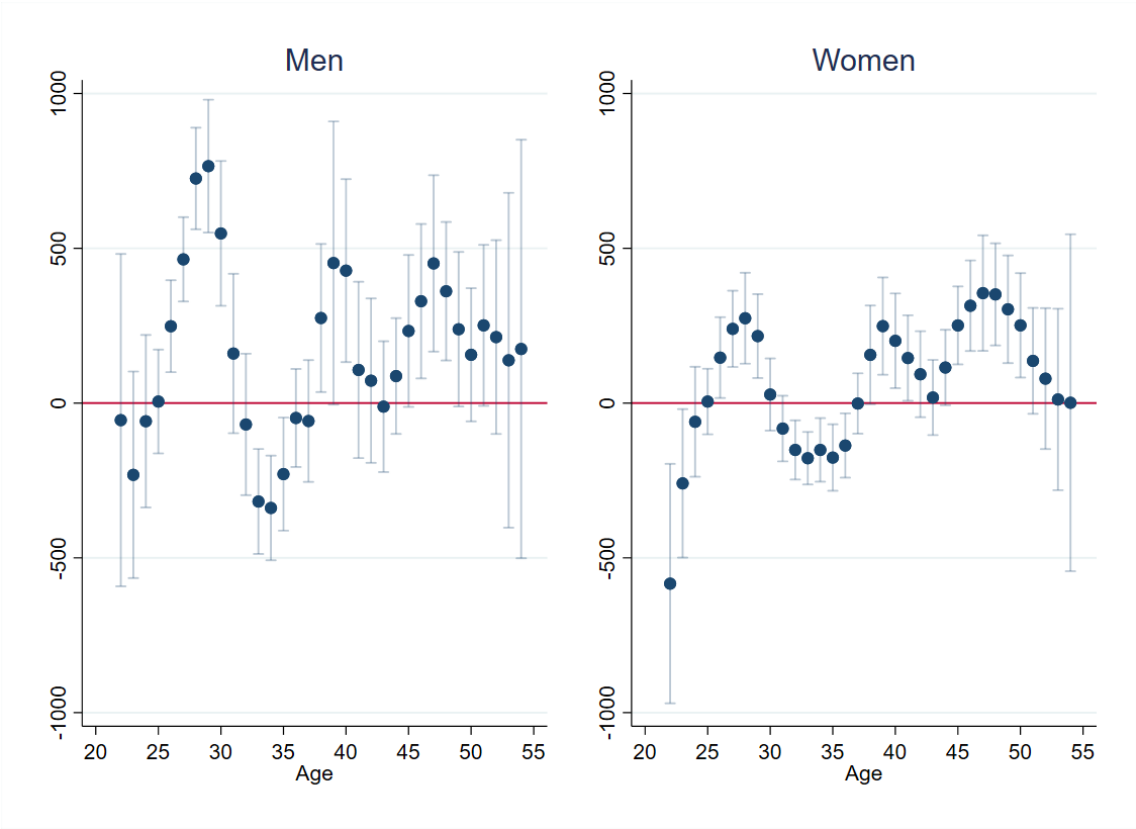
Note: Point estimates of the effect of treatment intensity with 95% confidence intervals from separate regressions for each year of age. Dependent variable: Employed (= 1, otherwise 0). Employment status is measured at the end of the year. Treatment intensity measures the number of years spent in comprehensive school (ranging from 0 to 5). Each regression includes fixed effects for municipalities and cohort. Standard errors clustered at the municipality level.

FIGURE 2.B.6 Effect of the reform on labor earnings over the lifecycle.



Note: Point estimates of the school reform's effect with 95% confidence intervals from separate regressions for each year of age. Dependent variable: Annual labor earnings (includes wages and income from self-employment.) Earnings are deflated to 2015 prices using the CPI. 0 earners are not dropped. Each regression includes fixed effects for municipalities and cohort. Standard errors clustered at the municipality level.

FIGURE 2.B.7 Effect of the reform on labor earnings by treatment intensity over the lifecycle.



Note: Point estimates of the effect of treatment intensity with 95% confidence intervals from separate regressions for each year of age. Dependent variable: Annual labor earnings (includes wages and income from self-employment.) Earnings are deflated to 2015 prices using the CPI. 0 earners are not dropped. Treatment intensity measures the number of years spent in comprehensive school (ranging from 0 to 5). Standard errors clustered at the municipality level.

TABLE 2.B.1 Robustness checks

	Employed and unemployed		No capital region		Observed in all years		1960-1970 cohorts	
	(1) Men	(2) Women	(3) Men	(4) Women	(5) Men	(6) Women	(7) Men	(8) Women
Intensity	-0.004***	-0.003**	-0.003*	-0.005***	-0.006***	-0.009***	-0.002**	-0.003***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
Intensity × Age	0.002***	0.002***	0.003***	0.003***	0.004***	0.005***	0.002***	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	6,246,957	5,742,807	6,021,906	5,821,638	6,437,681	6,361,237	10,266,447	9,894,291

Note: The variable *Intensity* measures the number of years spent in comprehensive school (ranging from 0 to 5). Age is subtracted by 21 in the regressions. Standard errors clustered at the municipality level reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

3 A TALE OF TWO TRADE-OFFS: EFFECTS OF OPENING PATHWAYS FROM VOCATIONAL TO HIGHER EDUCATION

Abstract*, **

This paper studies the effects of a vocational secondary school reform implemented in Finland between 1999 and 2001. The reform extended vocational two-year programs to three years and made all graduates eligible to apply for university. For identification, we exploit the gradual implementation of the reform, and use a differences-in-differences approach and administrative register data up to 13 years after the reform. We find no long-term effect on enrollment in further education or labor market outcomes. However, our results illustrate that the reform increased the dropout probability. Thus, the benefits of opening pathways from vocational to higher education may be outweighed by the cost of a more demanding curriculum.

Keywords: vocational education, reform, dropout, difference-in-differences
JEL Classifications: I21, I28

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3.1 Introduction

Compared to general education, vocational education offers students an easier entry into the labor market. However, in a changing economy, job-specific skills are argued to be at a risk of becoming obsolete, which may lead to worse labor market prospects later (Hanushek et al., 2017; Krueger and Kumar, 2004). To alleviate this trade-off, countries have implemented reforms that open pathways from vocational to higher education. Allowing vocational school students access to further education could make switching occupations easier, thus reducing the risks associated with vocational education. However, if, as a result of providing access to higher education, the vocational programs grow more demanding, another trade-off may emerge: students might be more likely to struggle in the studies and drop out (Hall, 2016).

In this paper, we study the consequences of Finland's vocational education reform that sought to ease the transition from vocational to higher education. We identify the reform's effects using a difference-in-differences strategy based on the reform's gradual implementation across vocational school tracks. Our analysis makes extensive use of the Finnish administrative register data on study completion, higher education enrollment, and labor market outcomes.

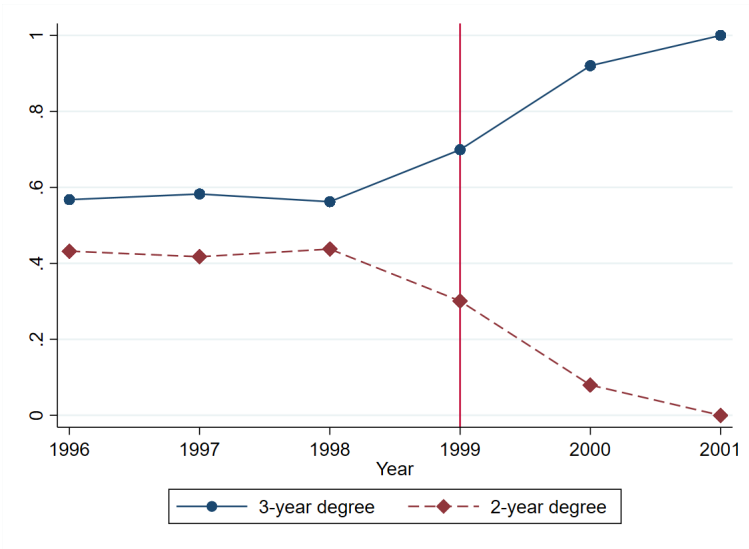
We find that the reform did not affect vocational students' enrollment in higher education. However, our results show an increase of 1.8 p.p. in dropout probability (6.5% increase relative to the mean). Further, we find negative effects on the probability of employment and annual earnings 2 to 4 years after enrollment and no significant effects up to 13 years after enrollment. Our results are consistent with studies on similar reforms (Bertrand et al., 2019; Felgueroso et al., 2014; Hall, 2012, 2016; Oosterbeek and Webbink, 2007; Zilic, 2018).

3.2 Vocational qualifications reform 1999-2001

The vocational secondary education reform changed vocational qualifications and their respective National Core Curricula. It caused two visible structural changes in vocational education. First, it combined 77 narrow study programs to 55 broader ones. Second, all two-year programs were extended to three years. Before the reform, the two-year programs gave eligibility to apply only for vocational higher education (known as polytechnic universities) in the same field while three-year programs gave eligibility for vocational higher education in any field. After the reform, graduates were eligible to apply for polytechnics or to universities regardless of their educational field.

On average, around 48 percent of the cohort graduating comprehensive school attended vocational education in the years 1998-2001. Compared to students in general education, vocational students have much lower GPA from the comprehensive school (see Figure 3.A.1). Figure 3.1 illustrates that, between 1996

FIGURE 3.1 Shares of two- and three-year degrees of vocational degrees by enrollment year.



Note: The vertical line represents the first phase of the reform.

and 1998, on average, 42 percent of enrolled students in vocational education started studying in two-year programs. The reform was implemented in three waves between 1999 and 2001 to ease institutional challenges. Table 3.A.1 in Appendix 3.A depicts the number of new students enrolled in new programs by the reform year.

In addition to enabling vocational graduates continue their higher education studies, the reform sought to increase the quality of vocational education so that graduates' skills better align to firms' needs. To achieve these somewhat conflicting goals, the new programs included six months of on-the-job learning, while the other six months consisted of vocational and optional studies depending on student preferences and the specific program. As a result, the new programs resembled the three-year programs before the reform. Surprisingly, though higher education was made more accessible, the amount of general education in vocational programs did not increase. Additionally, to support students' individualized development and learning, hoping it would result in a decrease in dropout rates, the new programs included a personalized study plan to give more attention to their individual development.

3.3 Data and methodology

We use data from the Finnish National Board of Education (the Joint Application Registry) and Statistics Finland (Register of Completed Education and Degrees, Finnish Longitudinal Census Files, and the Longitudinal Employment Statistics Files). Our analysis focuses on applicants aged 15 to 17 who were admitted for the first time to vocational track between 1998 to 2001 and who had passed through

the basic education syllabus in the comprehensive school. Table 3.A.2 presents the descriptive statistics of our estimation sample.

We use a difference-in-differences approach to estimate the effect of the vocational program reform on study completion, subsequent education, and labor market outcomes. Our identification strategy exploits the staggered implementation of the reform across different study programs between 1999 and 2001. Our baseline estimates are based on the following regression model:

$$y_{ijt} = \gamma_j + \delta_t + \beta D_{jt} + \zeta' X_i + \epsilon_i, \tag{3.1}$$

where y_{ijk} is the outcome of interest y for individual i who enrolled in study program j in year t. γ_j and δ_t are the study program and enrollment year fixed effects, respectively. D_{jt} is an indicator variable for the reform, and X_i is a vector of controls.

β identifies the average causal effect of the reform under the assumption that the timing of the reform in different programs is unrelated to other program-specific changes in the outcome variables. Note, that by including study program fixed effects, we don't assume similarity of the programs or random assignment of the reform years. To relax our identifying assumption somewhat, we add program-specific linear trends to our model in Table 3.A.3 and Figure 3.A.2. Adding these trends has little effect on our estimates and does not alter our conclusions.

During the implementation period, depending on which study track they choose, the students have the option to apply to a two-year or a three-year program. The reform could have caused students with lower academic skills to choose a shorter program to minimize their effort. To ensure that selection on observables does not drive our results, we add controls for parental education, 9th grade GPA, and gender. Since the reform had little effect on the content of these programs, we also add a dummy to indicate the study programs that were three-year programs before the reform. The reform could also have impacted students' decisions on whether to attend vocational school at all. To address these concerns, we show in Table 3.A.4 that the reform did not impact student characteristics in the reformed programs. However, we still cannot rule out the possibility that selection on the unobservable characteristics is reflected in our estimates. Our estimates, we believe, are still relevant for educational policy since vocational education is not compulsory in most countries. As such, changes in educational policy could cause changes in students' educational choices.

3.4 Results

Table 3.1 reports the estimated effects of the reform on dropout probability. Panel A presents our main results. Column 1 is the most basic model with fixed effects for study programs and enrollment year. Column 1 suggests that, as a result of the reform, students were 1.3 percentage points more likely to drop out.

TABLE 3.1 Effects of the vocational program reform on study completion

Dependent variable: Dropout (no degree in five years=1, otherwise=0)	(1)	(2)	(3)	(4)
Panel A: Average effect				
Reform	0.013	0.018**	0.015*	0.018**
	(0.010)	(0.007)	(0.008)	(0.007)
Panel B: Interaction with gender				
Reform	0.029**	0.027***	0.024**	0.027***
	(0.011)	(0.008)	(0.009)	(0.008)
Reform × female	-0.027***	-0.013*	-0.013	-0.015*
	(0.009)	(0.007)	(0.009)	(0.008)
Observations	107,455	107,455	107,455	107,455
Outcome mean	0.276	0.276	0.276	0.276
Year dummies	YES	YES	YES	YES
Program dummies	YES	YES	YES	YES
Other controls	NO	YES	YES	YES
Municipality fixed effects	NO	NO	YES	YES
Municipality specific trends	NO	NO	NO	YES

Note: The standard errors clustered by study program. Other controls include parental education, 9th grade GPA, gender and age dummies, and a dummy indicating if the study program was a three-year program before the reform. Panel B also includes the interactions between gender and year and program dummies. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Column 2 adds controls to the regression model. According to these estimates, the reform increased the probability of dropout by 1.8 p.p. (6.5% in relation to the mean). The estimate is statistically significant at the 5% level. In columns 3 and 4, we add municipality-specific fixed effects and trends as robustness checks which leave the estimates virtually unchanged. We also estimate the reform's effect on the probability of graduating at different times in Table 3.A.5. These estimates demonstrate a negative effect on the probability of graduating up to seven years after enrollment, suggesting that the reform did indeed cause an increase in dropout rates instead of merely postponing graduation.

We interact the reform dummy with gender in Panel B of Table 3.1 to allow for heterogeneous effects. The results illustrate a greater effect of the reform on the dropout probability for males. However, this effect is only significant at the 10% level, and it disappears if we instead define dropout as no degree in six or seven years (see Table 3.A.5). These results suggest that, after the reform, though men were slower to graduate than women, they were not more likely to entirely drop out.

Table 3.2 estimates the effect of the reform on further education using our preferred model (column 2 of Table 3.1). We find no effect of the reform on completing a general secondary degree or enrolling in higher education. Before the reform, applying to universities was only possible with the general degree, and we hypothesized that the reform might have led to a decrease in the share of vocational students completing it in addition to a vocational degree. The fact that

TABLE 3.2 Effects of the vocational program reform on further education

Dependent variable:	General secondary degree by age 24	Enrolled in polytechnic university by age 24	Enrolled in university by age 24
Panel A: Average effect			
Reform	-0.000	-0.007	-0.001
	(0.004)	(0.006)	(0.000)
Panel B: Interaction with gender			
Reform	0.004	-0.007	-0.001
	(0.005)	(0.007)	(0.001)
Reform × female	-0.009	0.001	0.000
	(0.006)	(0.010)	(0.001)
Observations	107,455	107,455	107,455
Outcome mean	0.065	0.113	0.003

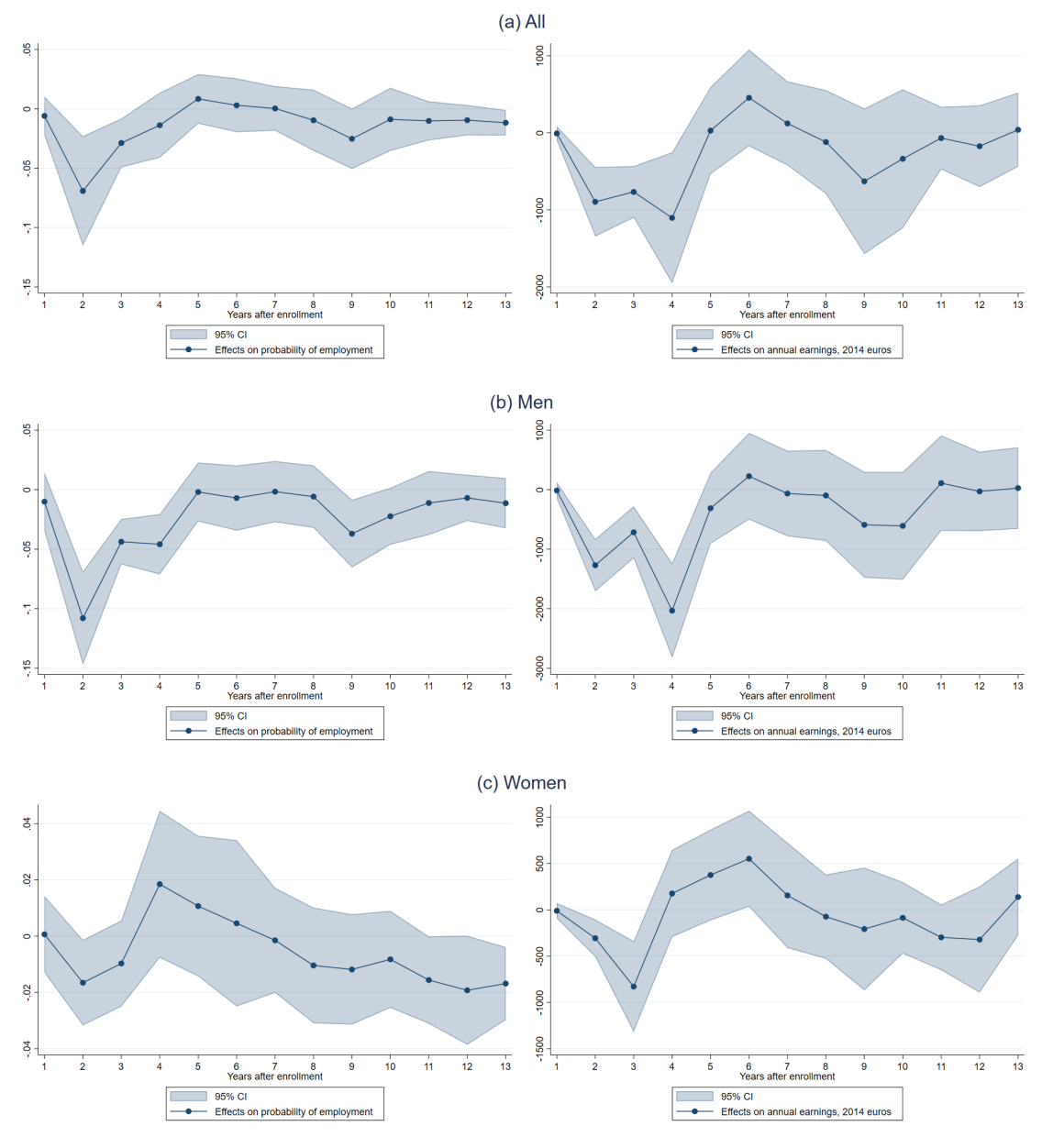
Note: The standard errors clustered by study program. Regressions include program and enrollment year fixed effects and controls for parental education, 9th grade GPA, gender and age dummies, and a dummy indicating if the study program was a three-year program before the reform. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

easing transition from vocational to higher education had no effect on enrollment could indicate that most students chose vocational education because of its relevance to the labor market, and the possibility of further studies does not factor into this decision. In our data, 11.3% of vocational students were enrolled in a polytechnic university and only .03% in a university by age 24 (see Table 3.A.2). These results could also suggest that vocational education even after the reform did not provide the necessary skills for further studies.

Lastly, we examined the effects of the reform on labor market outcomes up to 13 years after enrollment. The estimates from our preferred model (column 2 of Table 3.1) are presented in Figure 3.2. The results for the entire sample (Panel A) show that the reform reduced the probability of employment by 7 p.p. (25% in relation to the mean) two years after enrollment and 3 p.p. (8% in relation to the mean) after three years. Annual labor earnings reduced by around 1000 euros two to four years after enrollment (14% to 30% in relation to the mean). After these years, none of the estimates differ significantly from zero. The results reflect the fact that the two-year degrees were extended to three-year programs and, thus, entry to the labor market was postponed. The negative effects also suggest that the increased dropout we observed in Table 3.1 is not due to the students entering the labor market.

The reform's short-term effects on labor market outcomes are different for men (Panel B) and women (Panel C). For men, there are significant negative effects on employment probability and annual earnings two to four years after enrollment. For women, these effects are smaller in size and only significant two to three years after enrollment. These differences could be caused by the slower graduation time for men, which, we observe, is a consequence of the reform. Five or more years after enrollment, the effects for both men and women are found to

FIGURE 3.2 Effects of the reform on labor market outcomes by years after enrollment.



Note: Point estimates of the school reform's effect with 95% confidence intervals from separate regressions for each year after enrollment. The dependent variable is the employment status at the end of the year (=1 if employed, 0 otherwise) on the left-hand side and annual labour earnings (deflated to 2014 euros using the CPI) on the right-hand side. Regressions include program and enrollment year fixed effects and controls for parental education, 9th grade GPA, gender and age dummies, and a dummy indicating if the study program was a three-year program before the reform. The standard errors are clustered at the study program level.

be insignificant. The reform having zero effect on labor market outcomes could be explained by the increased dropout probability. The results could also imply that the extra year of vocational education was no more beneficial in terms of labor market outcomes than one-year work experience, a result also found by Hall (2012) in a similar reform in Sweden.

3.5 Conclusions

This paper investigates the consequences of a Finnish vocational school reform which made all graduates eligible to apply for university by extending two-year programs to three years. To improve the relevance of vocational education in the labor market, the reform introduced workplace learning. Our identification strategy exploits the gradual implementation of the reform. We estimate the reform's effects on the probability of dropout, subsequent educational attainment, and labor market outcomes.

Based on our results, the reform did not achieve its goals and, in fact, had negative consequences. The reform increased the probability of dropout by 1.8 percentage points, and no effect on later employment, income, or educational attainment was found. The results highlight a trade-off between higher demands placed on students and increased dropout rates, which should be acknowledged by policy makers when designing educational reforms.

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3.A Appendix: Tables and Figures

TABLE 3.A.1 Number of slots in the new 3-year programs by track and year.

		Number of slots		
	Vocational Qualification Programs	1999	2000	2001
1	Study Programme in Construction	1317	1465	1326
2	Study Programme in Land Survey Technology	65	29	34
3	Study Programme in Building Maintenance Technology	598	899	539
4	Study Programme in Social and Health Services	1837	1805	1712
5	Study Programmes in Textiles and Clothing		449	340
6	Study Programme in Metalwork and Machinery		2067	2282
7	Study Programmes in Vehicle Technology & Logistics		2181	1954
8	Study Programme in Electrical Engineering		3827	3448
9	Study Programme in Wood Processing		537	479
10	Study Programmes in Surface Treatment Tech. and Painting		248	259
11	Study Programmes in Paper and Chemical Engineering		434	408
12	Study Programme in Food Production		451	383
13	Study Programme in Business and Administration		2917	2695
14	Study Programme in Hotel and Restaurant Services		1494	1580
15	Study Programme in Catering and Customer Services		2138	1894
16	Study Programme in Household and Cleaning Services		282	158
17	Study Programmes in Hairdressing and Cosmetics		553	669
18	Study Programme in Agriculture			500
19	Study Programmes in Fishery, Forestry and Env. Protection			495
20	Study Programme in Publishing and Printing			209
21	Study Programme in Audio-Visual Communication			326
22	Study Programmes in Youth and Leisure Instruction			238
23	Study Programme in Crafts and Design			499
		3699	21611	22327

Source: Number of slots were calculated from the Finnish National Board of Education Joint Application Registry data using the number new students 15 to 17 years old.
Note: To conserve space, some smaller study programs are excluded or included in to the larger tracks. Study Programmes in boat-building, cutter maintenance and draping are included in to the Study Programme in Wood Processing; Study Programmes in Textiles and Clothing includes programs for footwear, milliner and dressmaker.

TABLE 3.A.2 Descriptive statistics for the estimation sample.

	Men	Women	All
Reform	0.488	0.510	0.497
	(0.500)	(0.500)	(0.500)
Dropout	0.285	0.263	0.276
	(0.452)	(0.440)	(0.447)
Enrolled in polytechnic by age 24	0.107	0.121	0.113
	(0.310)	(0.327)	(0.316)
Enrolled in university by age 24	0.002	0.004	0.003
	(0.045)	(0.064)	(0.053)
General secondary degree by age 24	0.046	0.094	0.065
	(0.209)	(0.291)	(0.246)
Employed at age 27	0.759	0.696	0.734
	(0.428)	(0.460)	(0.442)
Labor earnings at age 27	24052.65	15195.87	20578.92
	(16377.57)	(12920.53)	(15722.51)
Age when applying to vocational school	16.193	16.251	16.215
	(0.397)	(0.438)	(0.415)
9th grade GPA	6.525	6.986	6.706
	(0.789)	(0.840)	(0.840)
Parents with no higher than compulsory education	0.141	0.156	0.147
	(0.348)	(0.363)	(0.354)
Observations	65471	42249	107720

Note: The sample consists of individuals aged between 15 and 17 who enrolled for the first time in vocational secondary education between 1998 and 2001. The cells report the mean and standard deviation (in parenthesis) of the variable.

TABLE 3.A.3 Effects of the reform on educational outcomes with program-specific linear time trends.

Dependent variable:	Dropout (no degree in five years)	General secondary degree by age 24	Enrolled in polytechnic university by age 24	Enrolled in university by age 24
Reform	0.013*	-0.001	-0.006	-0.001
	(0.007)	(0.004)	(0.006)	(0.001)
Observations	107,455	107,455	107,455	107,455
Outcome mean	0.276	0.065	0.113	0.003

Note: The standard errors clustered by study program. Regressions include program and enrollment year fixed effects and controls for parental education, 9th grade GPA, gender and age dummies, a dummy indicating if the study program was a three-year program before the reform, and program-specific linear time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 3.A.4 Effects of the reform on study program characteristics.

Dependent variable:	Parental education	9th grade GPA	Application age	Female	Native speaker
Reform	-0.006	-0.042	-0.002	0.008	-0.001
	(0.004)	(0.028)	(0.042)	(0.007)	(0.001)
Observations	92	92	92	92	92

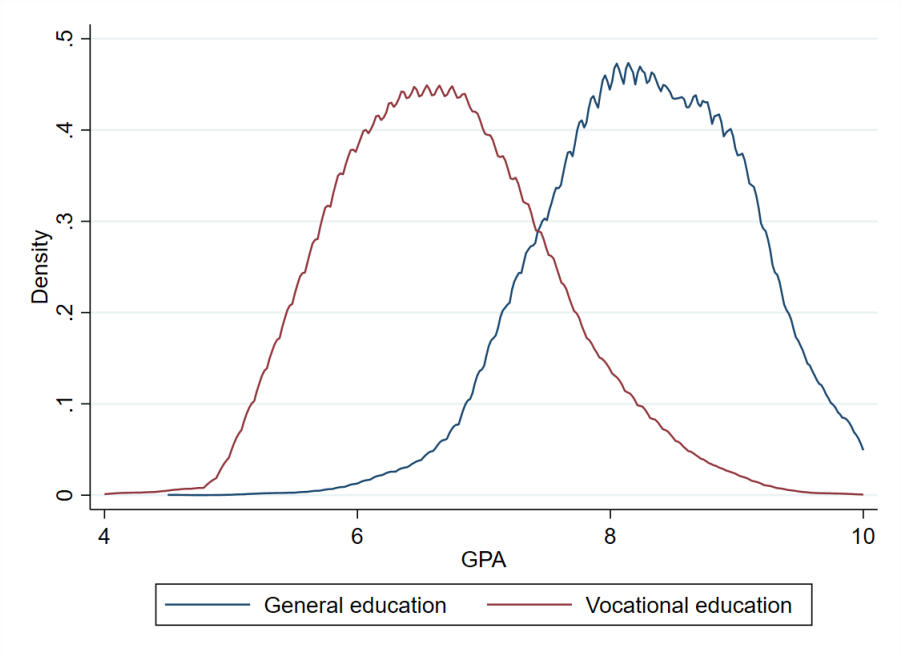
Note: Robust standard errors. All dependent variables are yearly program-level means from 1998 to 2001. Regressions include the reform dummy and program and enrollment year fixed effects. We use the number of students in each program-year combination as weights in the regressions. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 3.A.5 Effects of the vocational program reform on graduation.

Dependent variable:	Graduated in two years	Graduated in three years	Graduated in four years	Graduated in five years	Graduated in six years	Graduated in seven years
Panel A: Average effect						
Reform	-0.377***	-0.060***	-0.030***	-0.018**	-0.017**	-0.017**
	(0.037)	(0.013)	(0.009)	(0.007)	(0.007)	(0.007)
Panel B: Interaction with gender						
Reform	-0.404***	-0.074***	-0.043***	-0.027***	-0.022**	-0.020**
	(0.033)	(0.014)	(0.008)	(0.008)	(0.008)	(0.007)
Reform × female	0.052	0.021	0.021**	0.013*	0.005	0.002
	(0.032)	(0.014)	(0.010)	(0.007)	(0.007)	(0.007)
Outcome mean	0.110	0.598	0.688	0.724	0.746	0.762
Observations	107,455	107,455	107,455	107,455	107,455	107,455

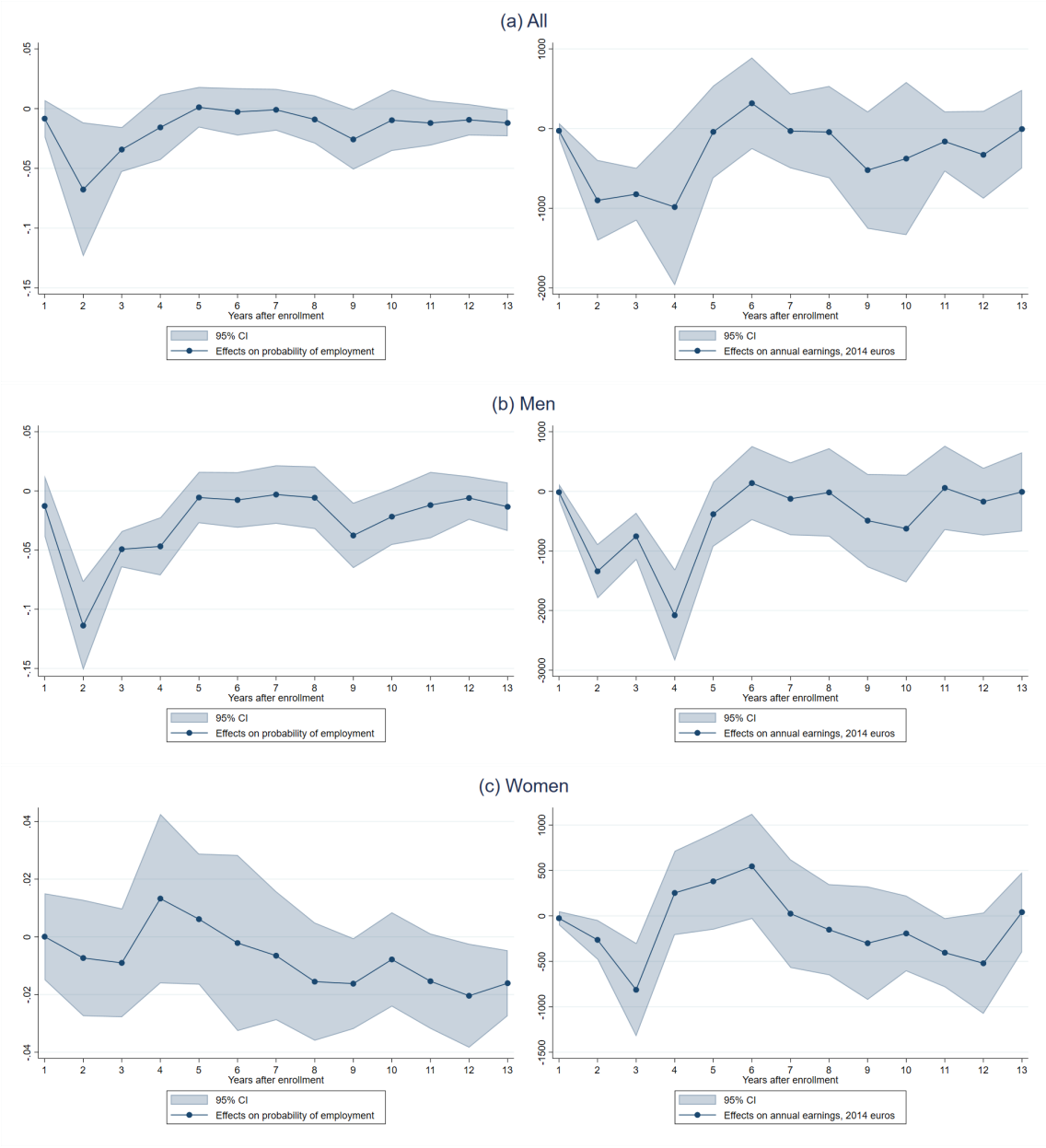
Note: Standard errors clustered by study program. All regressions include program and enrollment year fixed effects. Other controls include parental education, 9th grade GPA, gender and age dummies and a dummy indicating if the study program was a three-year program already before the reform. Panel B also includes interactions between gender and year and program dummies. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

FIGURE 3.A.1 Distributions of grade point averages from 9th grade of comprehensive school for vocational and general education.



Note: We use data on applicants aged 15 to 17 who were admitted for the first time to secondary education between 1998 to 2001.

FIGURE 3.A.2 Effects of the reform on labor market outcomes by years after enrollment with program-specific linear time trends.



Note: Point estimates of the school reform's effect with 95% confidence intervals from separate regressions for each year after enrollment. The dependent variable is the employment status at the end of the year (=1 if employed, 0 otherwise) on the left-hand side and annual labour earnings (deflated to 2014 euros using the CPI) on the right-hand side. Regressions include program and enrollment year fixed effects and controls for parental education, 9th grade GPA, gender and age dummies, a dummy indicating if the study program was a three-year program before the reform, and program-specific linear time trends. The standard errors are clustered at the study program level.

4 ENHANCING CONTINUOUS LEARNING WITH A GROUP INTERVENTION: A RANDOMIZED CONTROLLED TRIAL

Abstract*
This paper aimed to investigate whether continuous learning in mid-career could be supported by a psychological peer group-based intervention to enhance employees' career management skills. The study was implemented as a randomized controlled trial in which 1119 graduates in business and administration and engineering (average age 49) participated in an educational institute in Finland. The intervention increased the treatment group's participation in tertiary education by 11 percentage points after one year. Our results suggest that employees can be encouraged to develop their competences with a short and cost-effective group-based intervention.

Keywords: continuous learning, randomized controlled trial, group intervention, education
JEL Classifications: J24

4.1 Introduction

The transformation the nature of work and work life, aging of the workforce, and accelerating technological development call for new ways to organize and implement continuous learning (or lifelong learning). The European Commission (2017) estimated that less than half of the European working population pos-

* This essay is a joint work with Salla Toppinen-Tanner, Niina Jallinoja, Mervi Ruokolainen, and Jukka Vuori. I am the corresponding author and had the main responsibility for the following elements: data analysis and writing. I shared joint responsibility for finding related literature and interpreting the findings. This study was supported by the Strategic Research Council at the Academy of Finland (grant 303690). Jani-Petteri Ollikainen received funding from the Strategic Research Council at the Academy of Finland (grant 293445).

sesses the critical digital basic skills required in the current competitive labor markets. Even though the need for developing professional competencies throughout one's career is widely acknowledged, there is a lack of methods turning the idea of lifelong learning at mid-career and beyond into reality. Another challenge is that aging employees and employees with low levels of educational attainment are significantly less likely to pursue adult education. On the whole, many employees who would benefit most from learning, in terms of maintaining work ability and employability, do not participate in it (Bassanini et al., 2005).

This study evaluated the effects of the mid-career peer group intervention called "Skills for Work" on participants' subsequent education and employment. The intervention involved career self-management training for supporting skills development and employability among employees facing or experiencing a need to manage career changes mid-career. This training included two four-hour group meetings, which were organized at the Institute for Business Education in Haaga-Helia, Finland. These meetings consisted of the participants identifying competence- and career-related goals and setbacks and then finding and practicing solutions for them.

The individual-focused psychological intervention aimed to increase the participants' mid-career competence management preparedness, a cognitive motivational construct comprising specific self-efficacy and preparation against setbacks (Vuori and Vinokur, 2005). Employees who are well prepared and motivationally ready to manage their careers also possess the confidence (self-efficacies) in their career management skills as well as the knowledge and emotional readiness to deal with the setbacks that are frequently encountered during their careers. Earlier studies show that similar theory-based interventions targeting career management preparedness have been successful in enhancing the management of the different phases of one's career. These interventions have had positive long-term effects on work ability, well-being, motivation, and career choices (Caplan et al., 1997; Koivisto et al., 2010; Vuori and Vinokur, 2005; Vuori et al., 2012, 2019).

We studied the short-term effects of the intervention on employment and subsequent tertiary education. Our estimates from the randomized controlled trial suggest that the intervention increased participants' probability of attending tertiary education by 11 percentage points. We found no effects on employment. The results are encouraging since they demonstrate that continuous learning can be encouraged with a relatively short psychological intervention, focusing on the preparedness to learn.

Previous studies suggest that engaging in adult education may have a positive effect on individual outcomes. For instance, Stenberg (2011) found an average return to earnings of 4.4% from one year of education for low skilled adults. Blanden et al. (2012) demonstrated using data from the UK positive returns for women who obtained qualifications as adults but not for men. Hällsten (2012) estimated the returns to a tertiary degree obtained in those aged above 30. The study revealed strong positive effects on employment and small positive effects on earnings for those employed. Looking at both pecuniary and non-pecuniary

returns to adult education, Ruhose et al. (2019) found that engaging in work-related training had positive impacts on earnings as well as participation in civic, political, and cultural activities. Finally, two studies by Jacobson et al. (2005a,b) revealed positive returns for displaced workers from attending a community college. The studies found that although older workers were less likely than younger workers to attend community college, the effect on earnings was similar.

Our study contributes to a small experimental literature on promoting continuous learning. To the best of our knowledge, this is one of the first papers estimating the effects of a psychological group intervention on continuous learning. Previous studies in this literature have looked at training vouchers as a method to promote continuous learning, but there is little evidence of their efficacy in terms of promoting continuous learning or subsequent labor market outcomes. Schwerdt et al. (2012) analyzed a randomized field experiment in Switzerland and found that training vouchers did not affect employment, earnings, or educational attainment. Similarly, Hidalgo et al. (2014) examined the effects of training vouchers in a Dutch experiment. They revealed that while the voucher did increase training participation, it had no effects on job mobility or earnings. In Germany, Görlitz and Tamm (2017) studied whether providing information to eligible employees about a training voucher had an effect on training outcomes and found that while the intervention increased the treatment group's knowledge of the training voucher, it did not affect voucher take-up or participation in training.

4.2 The experiment

4.2.1 Group intervention for enhancing mid-career competence management

The theoretical background for the "Skills for Work" intervention was based on social cognitive theories on social action, behavioral control, and individual coping resilience (Ajzen, 1991; Bandura, 1986; Meichenbaum, 1985). The aim was to enhance participants' preparedness for mid-career competence management, which comprises two intertwined dimensions, i.e., self-efficacies related to competence management and preparation for dealing with career-related setbacks (Vuori and Vinokur, 2005). The group program was built utilizing earlier experiences in preventive group methods aimed at increasing preparedness and motivation for different educational and work-life transitions (Koivisto et al., 2007; Vinokur et al., 1995; Vuori et al., 2002; Vuori and Vinokur, 2005).

The self-efficacies related to competence management were strengthened in a stepwise procedure in peer groups. First, the participants identified in peer groups their competence- and career-related goals required for progress. Second, they defined solutions and tasks for performing these goals, and third, they practiced the required skills and actions in small groups. Inoculation against setbacks in competence management was strengthened according to the principles of (Me-

ichenbaum, 1985, 2007) stress inoculation training. First, participants shared experiences of setbacks and barriers in their career and competence environment and empathized with feelings aroused by these conversations. Second, they defined possible solutions for these setbacks and barriers in peer groups, and finally, practiced these solutions in small groups.

In practice, the intervention combined face-to-face group training and self-learning on an online learning platform. Each intervention group met two times for four-hour group sessions organized within a month, facilitated by two trained instructors. Earlier, in between the two meetings, and after the second meeting, the participants created short exercises themselves and returned their completed assignments on the platform for the trainers.

The focus of the intervention was on increasing individual independence and awareness of abilities and choices and possibilities for competence development during one's working career. The intervention could be seen as a means to empower people to maintain and advance their skills during their careers. Therefore, we wanted to implement this preventive intervention as a universal intervention in an occupational institution, also providing a direct link to its courses and programs.

4.2.2 Study design and participants

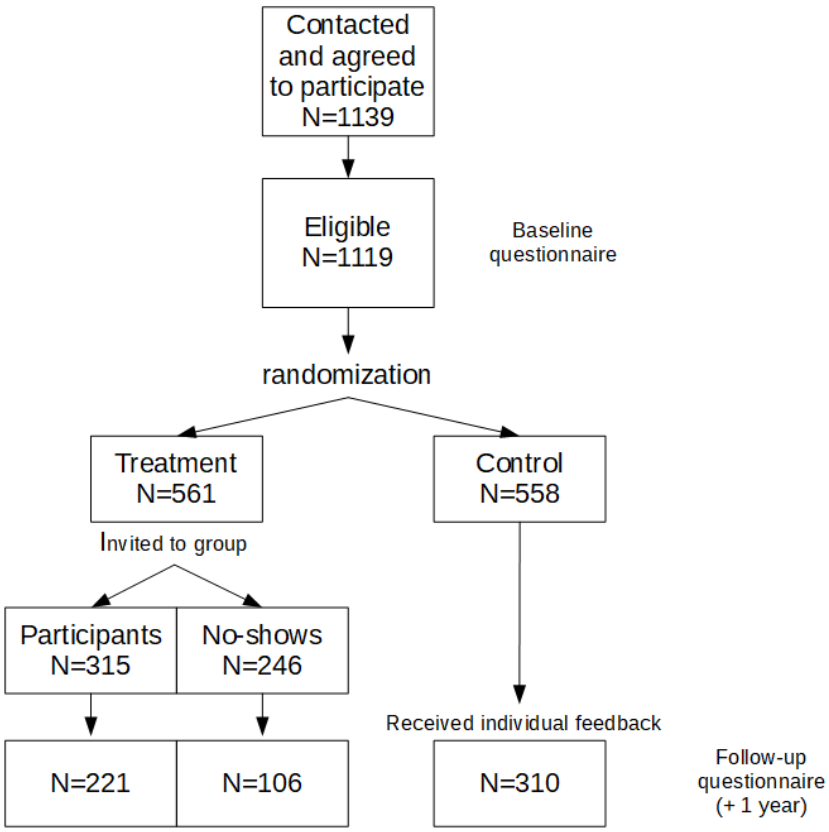
The two populations of the RCT study were the alumni of Haaga-Helia University of Applied Sciences (business, IT, and service professions) and members of the Union of Professional Engineers residing in the Helsinki metropolitan area, both populations aged over 40, constituting a total of about 11,000 persons. These two populations were selected because they are among the largest occupational groups, represent the same educational level, and are in occupations that face rapid changes in the labor market due to technological development. They were contacted by mail and provided the opportunity to participate in the study by filling up and returning an online baseline questionnaire survey. The invitation stated that participation would benefit employees who face changes in their work life or have a need to manage their careers. A total of 1139 individuals filled in and returned the questionnaire, out of whom 1119 completed this task successfully. The study was approved by the Ethical Committee of the Finnish Institute of Occupational Health and the participants provided their informed consent.

The respondents (n = 1119) were randomized into treatment (n = 561) and control (n = 558) groups. The members of the treatment group were sent an invitation to participate in the mid-career peer-group intervention. The baseline questionnaire, randomization, and the intervention were implemented in two stages so that the first half of the participants received their invitation in early 2017 and the other half six months later. The control group received individual feedback on their responses to the baseline survey. Both groups also received a follow-up questionnaire one year later. These questionnaires were returned by 327 (58.2%) of the treatment group and 310 (55.6%) of the control group participants. Figure 4.1 presents the study design, with information regarding the various subgroups

at each stage of the study.

The background characteristics of the study participants at the baseline have been presented in Appendix 4.A. The majority of the participants were female (63.5%), the mean age being 48.6 years and the average duration of tenure 24.7 years. Almost 75% of the participants had completed a bachelor level degree. A vast majority of the participants were employed on a permanent contract (92.9%) and worked in private companies (75.2%). The participants worked in several different occupational sectors (e.g., industry, trading, construction).

FIGURE 4.1 Experimental design



4.2.3 Viability of the randomization and attrition

We did not find any statistically significant differences between control and treatment groups at the baseline in background characteristics (see Table 4.1). Moreover, the participants in the control and treatment group did not differ from each other in any of the outcome variables (i.e., attendance at tertiary education and employment) at the baseline. Thus, the randomization process of the study participants proved to be successful.

Additionally, we compared the means of background variables between different measurement points to understand the attrition rate (see Appendix 4.A). The only statistically significant difference between measurement points

TABLE 4.1 Differences in variables between treatment and control group at baseline (n=1119).

Variable	Treatment group (mean)	Control group (mean)	t-test for difference
Male	0.38	0.38	0.22, p = .83
Age	49.05	48.69	-0.99, p = .32
Business and administration degree	0.66	0.68	0.70, p = .49
Unemployed	0.09	0.10	0.54, p = .59
Temporary employment	0.04	0.03	-0.78, p = .43
Permanent employment	0.79	0.80	0.19, p = .85
Attendance in tertiary education	0.16	0.18	0.75, p = .45
Answered follow-up questionnaire	0.58	0.56	-0.92, p = .36

we found was that the share of women was greater in the intervention than in answering the baseline questionnaire. However, this analysis suggests that answering the follow-up questionnaire did not depend on the background characteristics of the participants. Therefore, any issues related to attrition are unlikely.

4.2.4 Measures

As an effect of the intervention, we assessed participants' attendance in tertiary education and employment status one year after the intervention. Attendance at tertiary education was measured with one question (i.e., "Have you participated in university-level education within 12 months?"). The rating scale was binary, with 0 = no and 1 = yes.

The employment status was assessed from a question concerning the participants' current labor market status (i.e., "Which of the options best describes your current labor market status?"). Options included, for example, permanent employment, temporary employment, unemployed, retired, and student. We defined employment as permanent employment so that employment = 1 if permanent and 0 otherwise.

4.3 Empirical strategy

Our primary interest was in estimating the effects of the intervention on labor market outcomes and education one year after the intervention. To this end, we began by regressing an outcome variable on the random assignment:

$$y_{i,t+1} = \beta_0 + \beta_1 z_{i,t} + \gamma' X_{i,t} + \epsilon_{i,t}, \tag{4.1}$$

where $y_{i,t+1}$ is the outcome of interest for individual i at time $t+1$ (one year after the intervention), $z_{i,t}$ is a dummy variable indicating the random assignment (0 = control group and 1 = treatment group), $X_{i,t}$ is a vector of control variables, and $\epsilon_{i,t}$ is an error term.

We estimated Equation 4.1 using OLS. The parameter of interest here is β_1, which measures the effect of assignment to the treatment group on the outcome

variable. All the outcome variables of interest are dummy variables, so β_1 may be interpreted as an effect on the probability of the outcome variable equaling 1. The successful random assignment ensures that β_1 is an unbiased estimate of the treatment effect since the error term $\epsilon_{i,t}$ was not correlated with any of variables on the right-hand side of Equation 4.1. Even though the randomization was successful, we add a vector of control variables $X_{i,t}$ to address any potential bias arising from attrition in the experiment. $X_{i,t}$ includes gender, age and field of education as reported in the baseline questionnaire. Where possible, also the outcome variable at the baseline questionnaire was added as a control variable.

Alternatively, we could use an instrumental variable estimator (IV) with the random assignment as an instrument for attendance in the intervention. Because a notable share of the treatment group did not attend the group meetings, the intention-to-treat (ITT) effect of the intervention identified by β_1 was bound to produce smaller estimates than the IV.

The resulting IV estimator identified the local average treatment effect (LATE) of the intervention, which is the treatment effect for the individuals who attended the intervention because they were assigned to the treatment group but would not have attended had they been assigned to the control group (Angrist et al., 1996). In this instance, since there were no treated individuals in the control group (one-sided non-compliance), we could also consider the resulting estimate from the model as the average treatment effect on the treated (ATET) (see for example Bloom (1984)).

The IV estimator is unbiased, provided that the instrument used is valid, i.e. uncorrelated with the error term (exclusion restriction) and correlated with the outcome variable of the first stage. These conditions were satisfied in our study since the random allocation was successful and was a predictor of attending the group meetings.

4.4 Results

We begin the section with estimates of the first stage of the IV estimator. The estimates presented in Table 4.2 suggest that random assignment to the treatment group increased the probability of participating in the group meetings by $\sim$57 percentage points (p.p.; significant at 1% level). This is in line with the number of participants reported in Figure 4.1. The F-statistic for the first stage was 189.25 > 10, so the null hypothesis of a weak instrument can be rejected.

Since we used survey data, a further concern was whether the random assignment affected the survey response rate. This could cause the instrument to be correlated with the error term and thus violate the exclusion restriction crucial to IV estimation. The estimates in Table 4.2 also examine this possibility. Based on the estimates in Table 4.2, the random assignment to the treatment group does not affect the probability of replying to the one-year follow-up survey in which the outcome variables are measured.

TABLE 4.2 First stage estimates

	Dependent variable:	
	Attended at least one group meeting	Answered one-year follow-up questionnaire
Treatment group (instrument)	0.57***	0.03
	(0.02)	(0.03)
Age	-0.003	0.001
	(0.002)	(0.003)
Business and administration degree	-0.01	-0.07*
	(0.03)	(0.04)
Female	0.11***	0.03
	(0.03)	(0.04)
Constant	0.28***	0.53***
	(0.10)	(0.13)
Observations	1,103	1,103
R^2	0.41	0.004
F Statistic	189.25***	1.237

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.3 presents estimates of the intervention's effect on our outcome variables of interest: attendance at tertiary education and employment. All three outcome variables were measured one year after the intervention. We present estimates from three separate model specifications: model 1 includes no control variables, model 2 adds controls for age, gender, and field of education, and finally, model 3 further includes a control variable for the outcome variable at the baseline questionnaire. Table 4.3 contains estimates from both the OLS and the IV model.

We found a positive and significant effect (1% level) on attendance in tertiary education one year after the intervention. The intervention amounted to an 8 to 11 p.p. increase in the probability of attending tertiary education based on the OLS estimates and a 12 to 14 p.p. increase based on the IV estimates. To interpret the magnitude of the effect, we define a baseline as the mean of the outcome variable in the control group. For attendance in tertiary education, this baseline is 0.11 (or 11%). Thus, the ITT estimates imply that the intervention approximately doubled the probability of attending tertiary education.

We proposed two mechanisms through which the intervention could have caused an increase in educational participation. First, the intervention could have caused an increase in attending tertiary education by first increasing the competence management preparedness of the participants which could have mediated the effects. Alternatively, since the intervention was organized at an institute for business education, that itself could have affected the participants, for example by providing them with information on courses and programs. In this case, the intervention could have caused an increase in the more general career and competence management preparedness. This along with the intervention environment could have caused the effect. Further studies will be required to disentangle these

TABLE 4.3 Main effects

Panel A: Attendance at tertiary education	*OLS*	*IV*	*OLS*	*IV*	*OLS*	*IV*
Treatment effect	0.08***	0.12***	0.08***	0.13***	0.11***	0.14***
	(0.03)	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)
Control variables	NO		YES		YES	
Outcome at baseline questionnaire	NO		NO		YES	
Baseline	0.11					
Observations	578		572		276	
R^2	0.01	0.02	0.04	0.05	0.21	0.21
Panel B: Employment	*OLS*	*IV*	*OLS*	*IV*	*OLS*	*IV*
Treatment effect	0.02	0.03	0.03	0.04	0.03	0.05
	(0.03)	(0.05)	(0.03)	(0.05)	(0.03)	(0.04)
Control variables	NO		YES		YES	
Outcome at baseline questionnaire	NO		NO		YES	
Baseline	0.80					
Observations	636		630		627	
R^2	0.00	0.00	0.01	0.01	0.40	0.40

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Control variables include age, gender, and field of education. The baseline is the mean of the dependent variable in the control group. The dependent variables were measured one year after the intervention.

possible mediation effects.

Furthermore, we found a positive but statistically insignificant effect on employment, measured as being permanently employed. As shown in Table 4.A.1 in Appendix 4.A, around 80% of the study participants were already in permanent employment and around 10% were unemployed. Any effects on employment could then possibly have been driven by the unemployed and thus too small to detect using our rather limited sample size.

Further information on the education of the participants following the intervention would enrich our understanding of the intervention's effects and mechanisms. Unfortunately, a limitation of the survey data we used was that we have had to restrict our analysis to whether educational participation increased. Since we observed an increase in participation rates, it would be equally important to understand which fields of education saw an increase in participation. With the present data, we cannot determine if the study participants enrolled in studies in the same field as their previous degree or in another field. Furthermore, we know that participation in tertiary education increased, but with the survey data set, we cannot differentiate between university education and vocational higher education. In the Finnish context, vocational higher education also includes vocational master's programs. As shown by Böckerman et al. (2019), there are positive returns associated with these. Thus, these programs could be a relevant option for the study participants. They are eligible for the programs as they have completed

a vocational bachelor's degree and possess at least three years of work experience (the average tenure in the data was over 24 years).

In addition to the limited information on educational outcomes, the data also lacks information on other relevant outcomes such as earnings. Another concern is possible issues with self-reporting often associated with survey data. These issues should be addressed in a follow-up study using administrative register data to further gain a richer understanding of the intervention's effects on education and labor market outcomes. The participants were asked for permission to link the experimental data to registers, which means that a follow-up study using register data is plausible.

When generalizing the results, it should be noted that the invitation to the intervention was aimed at those workers who faced or had a need to manage changes in their careers. The participants were primarily bachelors of business and administration and engineers, and thus do not represent the entire Finnish working-age population. These two target groups were selected because they are among the largest occupational groups, represent the same educational level (most had a lower tertiary degree), and are engaged in occupations that face rapid changes due to technological development. Because educational attainment is itself a predictor of positive labor market outcomes, it is possible that workers with lower levels of education could benefit from such interventions even more than those with higher education. Further research could focus on the effectiveness of the intervention in other selected occupations that are expected to face changes with the transformation of working life. For example, according to Frey and Osborne (2017), occupations at risk of automation include transportation and logistics, office and administrative support, and production. Evidence also shows that those who benefit the most from interventions are the most vulnerable groups of employees at risk (for a review, see Seymour and Grove (2005)) or at risk of leaving their jobs (Heaney et al., 1995).

4.5 Conclusion

In this study, we investigated if employees' competence development and labor market outcomes can be supported with an intervention aimed at enhancing career and competence management mid-career. Participants in the study (with an average age of 49) had at least a bachelor level degree in business or engineering.

Our results from a randomized controlled trial revealed that the "Skills for Work" -intervention increased the participants' attendance in tertiary education in the following 12 months. Participation in group intervention increased the probability of attending tertiary education by 11 percentage points on average. At the same time, 11% of the control group took up tertiary education, implying that the intervention doubled the probability of attending. We found no effect on employment. The results are significant considering that the intervention was relatively short, consisting of two four-hour group meetings and four self-study

assignments.

The results are encouraging as they show that employees can be steered during mid-career to develop their professional competencies with short and cost-effective group training. From a public policy perspective, this type of intervention could be useful for supporting continuous learning later on in people's careers, especially among those groups of employees who are not active by nature in educating themselves. It could be utilized in a more targeted fashion than, for example, adult education subsidies or as a complement to them. The intervention could also be used as more of a preventive measure by helping the participants gain a better understanding of their skills and possible development needs for the future.

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4.A Appendix: Tables

TABLE 4.A.1 Background characteristics of employed study participants (%) (N = 1119)

Measures	N	%
Gender		
Female	684	62.0
Male	420	38.0
Age in years (M/SD)	1118	48.9 / 5.9
Tenure (M/SD)	1107	24.6 / 7.1
Education		
College or lower	25	2.2
Bachelor of business and administration	557	49.8
Bachelor of engineering	299	26.8
Upper polytechnic degree	146	13.0
Master degree or higher	92	8.2
Employment situation		
Permanent	894	79.9
Temporary	43	3.8
Notice	10	0.9
Unemployed	109	9.7
Entrepreneur	29	2.6
Student	10	0.9
On family leave	8	0.7
Other	16	1.5
Employer		
State	89	8.0
Municipality	77	6.9
Private company	837	75.2
Entrepreneur	31	2.8
Other	79	7.1
Field of business		
Industry	201	18.1
Construction	89	8.0
Trade	178	16.0
Information and communication	152	13.7
Finance and insurance	130	11.7
Public administration	162	14.6
Education	32	2.9
Health and social care	23	2.1
Other	143	12.9

TABLE 4.A.2 Means of background variables at different stages of the study

	Answered baseline questionnaire (mean)	Attended intervention (mean)	t-test for difference from baseline	Answered 1-year follow-up (mean)	t-test for difference from baseline
Female	0.62	0.71	-3.03, $p<.001$	0.62	0.13, $p = .90$
Age	48.87	48.56	0.87, $p = .38$	48.98	-0.38, $p = .71$
Bachelor of business and administration	0.67	0.70	-1.33, $p = .18$	0.64	0.94, $p = .35$
Unemployed	0.09	0.11	-0.77, $p = .44$	0.10	-0.39, $p = .69$
Temporary employment	0.04	0.03	0.42, $p = .68$	0.03	0.40, , $p = .69$
Permanent employment	0.79	0.78	0.39, $p = .70$	0.80	-0.33, $p = .74$

5 EFFECT OF EDUCATION ON COGNITIVE AND NON-COGNITIVE SKILLS

Abstract*
We exploit admission cut-offs to study the effects of secondary schooling on cognitive and non-cognitive skills. We measure these skills using the Finnish Defence Forces Basic Skills Test that due to compulsory military service still covers the vast majority of Finnish men. This test is designed to predict performance at military tasks but it is also a strong predictor of later labor market success. We compare academic and vocational tracks and find that differences in skills across tracks are mainly due to selectivity rather than causal effects of schooling on either cognitive or non-cognitive skills.

Keywords: non-cognitive skills, regression discontinuity, secondary schooling
JEL Classifications: J24, I21

5.1 Introduction

The importance of both cognitive and non-cognitive skills in the labor market is now a widely accepted fact. Both cognitive and non-cognitive skills affect employment and earnings and explain an empirically important fraction of variation in labor market success between individuals.[1]

Despite this newly acquired consensus on the importance multidimensional skills, there is relatively little empirical research on the factors that affect the for-

* This essay is a joint work with Tuomas Pekkarinen, Roope Uusitalo, and Hanna Virtanen. I am the corresponding author and had the main responsibility for the following elements: designing the research and analyzing the data. I shared joint responsibility for formulating the research questions and interpreting the findings. I had a supportive role in writing the paper and finding related literature. This study was supported by the Strategic Research Council at the Academy of Finland (grant 293445).

[1] For an authoritative survey see e.g. Cunha et al. (2006).

mation of these skills. In particular, even though it is well-known that both cognitive and non-cognitive skills are highly correlated with educational attainment, the causal effect of schooling on skills and particularly on the non-cognitive skills is still an open question.

Recent research has produced evidence suggesting that cognitive abilities may be set at a relatively young age leaving only limited scope for schooling to affect them. Non-cognitive skills may be also affected by interventions at somewhat older age.

These studies are often based on randomized controlled trials and hence provide convincing evidence that interventions can improve valuable skills. However, most existing studies mainly focus on small scale interventions and do not tell whether these skills could also be effectively fostered in the regular schooling system.[2]

In this paper, we estimate the effect of general secondary schooling on both the cognitive and non-cognitive skills of young Finnish men. We measure skills based on an extensive psychological test battery used by the Finnish armed forces. Finland is one of the few western countries where military service is still compulsory and where the vast majority of Finnish men enter military service. All conscripts are tested at the beginning of service and access to the military test data therefore provides measures of both cognitive and non-cognitive skills for almost entire cohorts of young men. We will also demonstrate that these skills are highly relevant in the labor market by showing that the military skill test scores are strongly correlated with later earnings and educational outcomes.

Finnish men enter military service typically at age 19 or 20. At this age there are already important differences in completed schooling. Almost no one has a college degree when entering military service, but by age 19 academically oriented men have usually already completed their secondary schooling and taken the matriculation examination that universities use as an admission criteria. Others have participated in vocational education for two or three years or dropped out of school at the end of comprehensive school at age 16.

There are large differences both in the cognitive and in the non-cognitive skills between men that differ in their education level at the time of entering military service. Using the effect of skills on later earnings to calculate skill prices generates an estimate according to which the skills gap between general secondary school and vocational school graduates is worth about 10 000 euro annually.

Naturally, the difference in skills across groups that differ in their education does not imply that education has an impact on these skills or that these skills have been fostered at school. The skill differences could equally well reflect selectivity to the different levels of education or selectivity to different schooling tracks. In this paper, we use admission cut-offs as an exogenous source of variation to remove selectivity bias and to identify causal effects of education on skills.

The Finnish youth finish compulsory comprehensive school at age 16 after which most students apply to the secondary schools. Admission is based on

[2] Useful surveys on current stage of knowledge include Kautz et al. (2014) and Schurer (2017).

grade point average at the end of comprehensive school. In the oversubscribed schools, this system creates a discontinuous threshold with very similar applicants on both sides of the threshold. We use these thresholds to study the effect of admission to the general secondary school on the cognitive and non-cognitive test scores in the Finnish Defence Forces Basic Skills Test.

Admission to general secondary school has a large effect on school environment in Finland. Compulsory comprehensive schools educate the entire cohort, do not track by ability and have roughly similar curriculum in all schools. Post-compulsory education in the secondary schools is the exact opposite. The cohort is split roughly in half at age 16. Students continuing in the general secondary schools have an ambitious academic program preparing for the tertiary education that focuses on foreign languages, math and humanities. Other students who continue in vocational education specialize in practical skills needed in specific occupations. Large share of vocational training also takes place at the workplaces rather than in the classroom.

Also the peer groups are quite different. Admission to some vocational programs is highly competitive but, on average, general secondary school students have much higher grade point average from the comprehensive school. Also gender composition is different. The majority of the general secondary school students are girls while the vocational programs are more popular among boys. Vocational programs are also strongly segregated by gender so that an average boy attending a vocational program will have mostly male classmates.

By the time Finnish men enter military service and take the battery of psychological tests, they have spent three years in a very different school environment. The main question in this paper is how these schooling years at ages between 16 and 19 affect cognitive and non-cognitive skills of young men.

Our results indicate that schooling has surprisingly little effect on the cognitive skills. We detect no effect on tests of logical, mathematical or verbal reasoning. Similarly, we find no significant effects on non-cognitive skills. Based on our estimates, schooling seems to have no effect on the non-cognitive skills that are most highly correlated with future earnings, such as measures related to achievement motivation and self-confidence. Interestingly, we observe that admission to general secondary school, or perhaps exposure to female classmates, decreases measures of masculinity.

Compared to earlier research, our results indicate smaller effects of schooling at secondary school age on non-cognitive skills than many previous papers surveyed in Schurer (2017). Exact comparison is difficult as measures of non-cognitive skills vary from study to study and very few studies anchor their skill measures to later outcomes so that comparison of the results would be meaningful. Naturally also methods vary. Identifying causal effects using natural experiments requires large data sets and these rarely contain psychological test results for representative samples of population.

Comparison to earlier research on the effects of schooling on cognitive skills is easier as measures of cognitive skills are more comparable, and there is more research on the effect of schooling on cognitive skills. Our setting is most com-

parable to a Swedish study by Carlsson et al. (2015) who examine the effects of length of schooling on cognitive skills measured with a test battery used in the Swedish military. Interestingly they find a rather large effect of length of schooling on the cognitive skills test scores. Effects of schooling on non-cognitive skills could be evaluated with Swedish military test data.

5.2 Institutional background

5.2.1 Finnish secondary schooling system

Compulsory comprehensive school lasts for nine years in Finland. The comprehensive school usually ends in May of the calendar year when the students turn sixteen. After comprehensive school, most students apply to secondary education.

There are two main options at the secondary level. General upper secondary schools (lukio) offer an ambitious academic program that prepares students for tertiary education either in traditional universities or in universities of applied sciences. Completing general upper secondary education requires passing 75 courses each consisting of 38 hours in class plus homework. The target duration is three years, but the students can study at their own pace and many graduate only after four years. General secondary school students study Finnish, math, natural sciences, humanities and on average 2.5 foreign languages. The general secondary school ends in matriculation examination that provides general eligibility to university-level studies but no professional qualifications.

The main alternative is vocational secondary education that provides practical training and vocational competences in specific occupations. The largest fields are manufacturing and construction (26%); business and administration (21%), health and welfare (19%) and services (19%). The students also take some general courses but over 80 % of training is concentrated on practical skills. A large share of training takes place at workplaces under supervision of a more experienced worker.

Currently 54% of those who finish comprehensive school continue in the general secondary school and 40% in vocational education. The remaining 6% either participate in the extra 10th grade of comprehensive school or quit school at age 16. General secondary school is more popular among girls. Only 43% of boys go to general secondary school while 54% of boys continue in vocational education after comprehensive school.

5.2.2 Applications and admission to secondary schools

Application to secondary education takes place through a centralized application system maintained by the Finnish National Board of Education (FNBE). The process starts in February-March of the final 9th year of comprehensive school. The

students can apply to up to five different post-compulsory programs (programs in different schools or different programs within schools).

Admission is based on program-specific admission scores. For most general secondary school program this score is solely based on the comprehensive school grade point average (GPA). Many vocational programs grant extra admission points for work experience or use aptitude tests in addition to grades.[3]

The students receive their final grades in May and therefore do not know their exact admission points or admission cut-offs at the time when they apply. As the cut-offs vary from year to year, students cannot either accurately predict whether they will be admitted into a particular program, making strategic application behaviour difficult.

The supply of slots in each educational program is fixed and announced before the application process begins. Student selection follows a DA algorithm. The algorithm terminates when every applicant is matched to a track or every unmatched candidate is rejected by every track in her application. At the end of this automated admission stage, in June of the final year of comprehensive school, applicants receive an offer according to the allocation result. Admitted applicants have two weeks to accept their offer, while the rejected applicants are placed on a waiting list in rank order based on their admission scores. After these two weeks schools start to fill their remaining vacant slots by inviting applicants on their waiting list in the rank order. This updating process affects roughly 10 percent of applicants in our period of study.

This paper focuses on applicants who are at the margin of being admitted into the general upper secondary schools. On average, the entry requirements to the general upper secondary schools are substantially higher than the entry requirements to vocational training so that the students who are not admitted to the general secondary schools are typically admitted to the vocational schools if they have listed a vocational alternative in their application.

The main educational options for those not accepted to any secondary education programs are the optional 10th grade of comprehensive school and preparatory vocational training. After this most students apply again to secondary education in the following years. However, as failed applicants have already completed their compulsory schooling, they are under no obligation to continue in education.

5.2.3 Military service

According to the Conscription Act, all Finnish men have to participate in either armed or unarmed military training or non-military (civil) service. Women can apply to military service on a voluntary basis.

Currently all Finnish men are called to the draft in the fall of the year they turn 18. At this point they are assigned a starting date and location where to report for service. In most cases men enter service during the two calendar years

[3] In our data, we do not observe the points for these different admission criteria. Therefore, we focus on admission into the general track.

after the draft year but it is possible to apply as a volunteer to service at age 18 of request for a postponement due to e.g. an on-going education program. There have been some changes in the draft system over time. Cohorts born in 1970 or earlier were drafted at age 19 and typically entered military service during the year they turned 20.

The draft also includes a physical examination. Those not fit for service can be exempt either temporarily or permanently. It is also possible to be exempt due to religious or ethical conviction.

In the years that we examine, the duration of armed military service was either 8 or 11 months (those trained as officers had longest service times). Non-military service lasted for 12 months. With few exceptions detailed below, the tests that we use in this paper were taken during the military service. Hence, we have no test data on those in civil service nor on those exempt from service.

5.3 Data and descriptive statistics

5.3.1 Test data

Data on the cognitive and non-cognitive skills used in this study are obtained from the Basic Skills Test of the Finnish Defence Forces. All conscripts are tested at the beginning of their military service with a battery of cognitive and non-cognitive skills tests. At the time of the test these conscripts are typically 19 or 20 years old. Currently Finnish men are free to choose between military service and slightly longer civil service at e.g. hospitals. It is also possible to be exempt from service due to ethical or religious conviction or more commonly for medical reasons. During the period covered in our data on average 70% of men performed military service and took the skill test battery. As we demonstrate later in Table 5.3, admission to general secondary school has no effect on the likelihood of entering military service.

The test contains two main sections: one for the cognitive and one for the non-cognitive skills. The cognitive skills test resembles aptitude tests used in college admissions (SAT) and very similar to the ability test used in Swedish military described in e.g. Grönqvist et al. (2017). It has three forty-question sets that measure verbal and numerical skills and logical reasoning. In particular, the logical reasoning part that is based on Raven's progressive matrices is closely related to common IQ tests.

The non-cognitive test section was developed by the Finnish Defence Forces in late 1970's. It has been used in an unchanged format from 1982 to 2001. Also this test has several parts. We use data from the leadership inventory which contains eight measures of traits that the army psychologists judge to be important characteristics for the military leaders.[4] Each trait is measured with 20 to 30 state-

[4] In addition the test contains a section based on Minnesota Multiphasic Personality Inventory (MMPI) that is used for screening for mental health conditions.

ments with which the test-taker is asked to agree or to disagree. The individual test items are not published and the entire test is a military secret. However, the Defence Forces have released some sample statements for each trait.

The test battery is rather extensive. The cognitive test has 120 items and the leadership inventory part of the non-cognitive test 218. During the period of 1982 to 2000 that we use in this study, the test was a paper and pencil test that took about two hours to complete. The test is conducted at the military base in standardized conditions. Between 1995 and 2000 the test was conducted already at the draft with the intention that it could be used in task placement during military service. The process turned out to be too slow and conditions at testing sites not sufficiently comparable and therefore the military reverted back to the practice of testing conscripts at the beginning of service. (Nyman, 2007)

The Defence Forces use the test results as one of the criteria when selecting conscripts to officer training. According to a validation study (Nyman, 2007), the test scores are correlated with other assessments of performance during military training and predict scores in final evaluations conducted after officer training.

More importantly for this study, the military test scores are also strongly correlated with various labor market outcomes. Jokela et al. (2017) demonstrate that men scoring higher in the military tests obtain higher level of education and earn more between ages 30-34. Jokela et al. (2017) also validate measures of the leadership inventory also against more commonly used personality test BIG5 by administering short versions of both tests to a sample of students. According to their results, subscales of the test are highly correlated with measures of extroversion, neurotism and conscientiousness of BIG5.

Psychological test scores have no natural scale. To make the magnitudes of the estimates easier to interpret we follow the example in Cunha et al. (2010) and anchor the test scores to later earnings data. This also reduces the dimensionality of the tests in a natural way and makes comparisons of the magnitude of differences in different skills meaningful. We will describe the anchoring procedure in more detail after first introducing the other data sources.

5.3.2 Data on earnings and education

Our earnings data are based on tax records. The earnings definition that we use in this paper contains annual wage earnings excluding taxable benefits. Data are available from 1987 onward on the annual basis. Linking tax data across years as well as linking tax data to other data sources is relatively easy using person id's. For the main part of our analysis, we use average log annual non-zero earnings at ages between 35 and 39. As shown by Böhlmark and Lindquist (2006) earnings at this age are highly correlated with lifetime earnings. Tax data have practically no measurement errors but taking an average over five years still reduces the effects of random fluctuations and avoids some issues with zero earnings during periods outside the labor force.

Education data come from two main sources. Data on completed degrees are based of Statistics Finland Register of Degrees and Examinations. They covers

all degrees completed in Finland and are based on direct reports from all degree-granting institutions. Data on applications and admissions to secondary education are based on records from Joint National Application Register maintained by National Board of Education. They contain all applicants to the secondary schools with information on applications in preference ranking, final grades from the comprehensive school and admission decisions to all secondary school programs.

We use Statistics Finland family relation tables to link the men in data to their parents. Information on completed education and earnings of the parents is based on same registers than information on education and earnings of the men in the sample.

Information in different registers is linked together using person id numbers. All data files then are stored on Statistics Finland remote access system and are used in this study in an anonymous form.

5.3.3 Estimation sample

We restrict our estimation sample to include conscripts who were between 18 and 22 years old in the end of the year than they took the Basic Skills Test. This mainly omits those who postponed their service due to participation in college-level education and naturally those who were exempt from military service or applied to civil service.

Finland is a bilingual country with a small Swedish-speaking minority. We exclude all Swedish-speakers from our analysis. Swedish-speakers typically attend different schools and take the test in Swedish and are therefore not strictly comparable. As only about 5% of conscripts are Swedish-speakers removing these from the sample has practically no effect on the key results.

The results from the full Basic Skills test are currently available from the year 1982 when the non-cognitive skills test was adopted up to the year 1999 after which the test was reformed. Cognitive test results also exist for the later years up to 2015, but Defence Forces have refused to release data on the new non-cognitive test that was adopted in 2000.

Data on application register are available from 1985, 1989 and from 1991 onwards. Due to changes in vocational education system, the observations from the 1980's may not be fully comparable with the later years. Therefore, we only use data from 1991 onwards.

Tax records on annual earnings are available at the time of writing from 1989 until 2018. As we measure earnings at ages from 35 to 39, access to earnings data does not restrict the range of cohorts available for the analysis.

To maximize the sample size while maintaining comparability we restrict data on cohorts who applied to secondary school in years from 1991 to 1995. As we study the effects of general upper secondary schools the sample is naturally also restricted to individuals who applied to the general upper secondary school. The men in final data were born between 1973 and 1979 and performed their military service between 1992 and 1999.

We also make the following restrictions to our estimation sample. First, we focus our analysis on first time applicants who are between 15 and 17 in age at the end of the year when applying. Second, we exclude programmes that do not reject any applicants. Finally, we need at least two applicants on each side of the cut-offs for our RDD design, so we exclude programmes that do not meet this requirement. Our final estimation sample has 41 164 applicants in 1144 programme-year combinations.

5.3.4 Anchoring

We convert the test scores to an interpretable scale by anchoring the test scores to later earnings. In practice, we calculate the natural logarithm of average earned real income between ages 35 and 39 and then regress this earnings measure on all cognitive and non-cognitive test scores as well as on the cohort dummies.[5]

In Table 5.1, we report the results from these anchoring regressions. In the first column, we explain average earnings with the scores in the three subsections of the cognitive test. We have access to the raw scores i.e. the number of correct answers in each tests but for easier interpretation we have normalized these scores to have standard deviation of one in this regression and use these normalized scores as explanatory variables in the anchoring regressions.

The cognitive test scores have a substantial effect on earnings. In particular, the arithmetic test scores are highly predictive of later earnings. One standard deviation increase in the arithmetic test increases earnings at ages 35 to 39, ceteris paribus, by 12.5 percent. Also the partial correlations of both the visuospatial and the verbal tests are positive and statistically significant. Jointly the three cognitive test scores explain 3.9 percent of the variation in earnings at ages between 35 and 39.

In the second column, we repeat the exercise using the non-cognitive test scores. Also these scores display strong correlation with future earnings. In particular, measures related to achievement motivation and self-confidence are highly correlated with future earnings. Predictive power of the non-cognitive test scores is only slightly lower than that of cognitive skills.

In the third column, we include both the cognitive and the non-cognitive test scores as explanatory variables in the log earnings regression. The measures are generally positively correlated and therefore coefficients of individual measures smaller than in columns 1 and 2. Coefficients of most cognitive test scores and most non-cognitive scores remain significant even in regression where both scores are simultaneously included. Jointly the test scores explain about 5 percent of variance in earnings measured 15 to 20 years after taking the test.

Finding that both cognitive and non-cognitive skills measured in aptitude and the personality tests taken before entry to labor market or college-level education explains a substantial fraction of the variance in earnings is interesting but not a particularly new finding. Numerous studies have reported similar results

[5] As noted by Jokela et al. (2017) both cognitive and non-cognitive test scores improve over time reflecting a phenomena known as the Flynn effect.

TABLE 5.1 Anchoring test scores to log average earnings at ages 35 to 39

	(1)	(2)	(3)
Cognitive:			
Visuospatial	0.068***		0.064***
	(0.004)		(0.004)
Verbal	0.058***		0.043***
	(0.004)		(0.004)
Arithmetic	0.125***		0.105***
	(0.004)		(0.005)
Non-cognitive:			
Leadership motivation		-0.000	-0.012**
		(0.006)	(0.006)
Activity-energy		-0.013***	0.012***
		(0.005)	(0.005)
Achievement striving		0.095***	0.044***
		(0.004)	(0.004)
Self-confidence		0.112***	0.046***
		(0.005)	(0.005)
Deliberation		0.034***	0.047***
		(0.004)	(0.004)
Sociability		0.012**	0.044***
		(0.005)	(0.005)
Dutifulness		0.014***	-0.009**
		(0.004)	(0.005)
Masculinity		0.017***	0.015***
		(0.003)	(0.003)
N	137 495	146 685	136 387
R^2	0.039	0.031	0.051

Note: Test scores are standardized to have mean 0 and standard deviation 1. For the anchoring regressions, we use data on birth cohorts 1974-1979. All columns include birth cohort fixed effects. The dependent variable is the natural logarithm of average annual earnings at ages 35-39 measured in 2018 euros. Robust standard errors are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

earlier (see e.g. Borghans et al. (2008); Kautz et al. (2014); Jokela et al. (2017) ; Edin et al. (in press)).

In this paper, the main focus is in the effect of schooling on skills. The estimates on the correlation between skills and earnings serve two main purposes. First, they verify that the test scores we use in this paper have external validity i.e. that the measured skills are relevant in the labor market. Second, they help to interpret the results by creating a meaningful scale for the test scores.[6]

Anchored test scores are simply predicted values from a regression model explaining earnings with the test scores. Note that these scores can also be calculated for the men (12%) with zero earnings or no valid earnings information as long as they have non-missing data on the test scores. Effectively anchoring simply weights the different sub-scores in a natural way and rescales the test scores so that the effects on skills are easier to interpret.

5.3.5 Descriptive statistics

Figure 5.1a plots the anchored test scores by educational background at the time of taking the test. In these figures, we restrict our estimation sample to include persons who were aged 18 to 22 at the end of the year when they take the test but make no other restrictions to data.

Figure 5.1 reveals large differences across men with different education at the test date. Apparently the difference in cognitive skills across men with different schooling levels is substantially larger than the difference in non-cognitive skills.

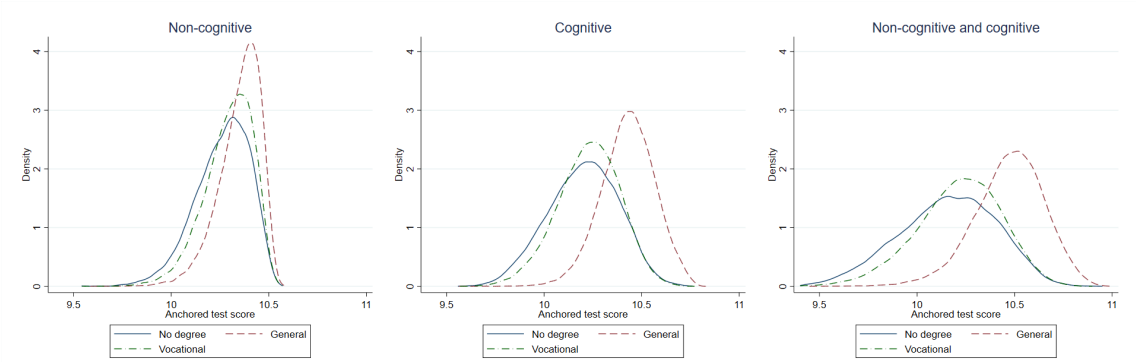
The men who have completed a general secondary education have much higher scores in both cognitive and non-cognitive tests than men who have completed a vocational degree or have no post-compulsory degree by the test date. On the other hand, the difference between men with vocational education and men with no completed education by the time of entering the military service are rather similar. In what follows, we therefore mainly compare men with secondary education to the rest of men.

Figure 5.1b displays the differences in the three components of the cognitive skill test and Figure 5.1c in the eight components of the non-cognitive skill test. We find large differences in the cognitive skill distribution between those admitted to the general secondary schools and the other two groups. The differences are of roughly equal magnitude (about 1 standard deviation) in all three components of the cognitive skills test.

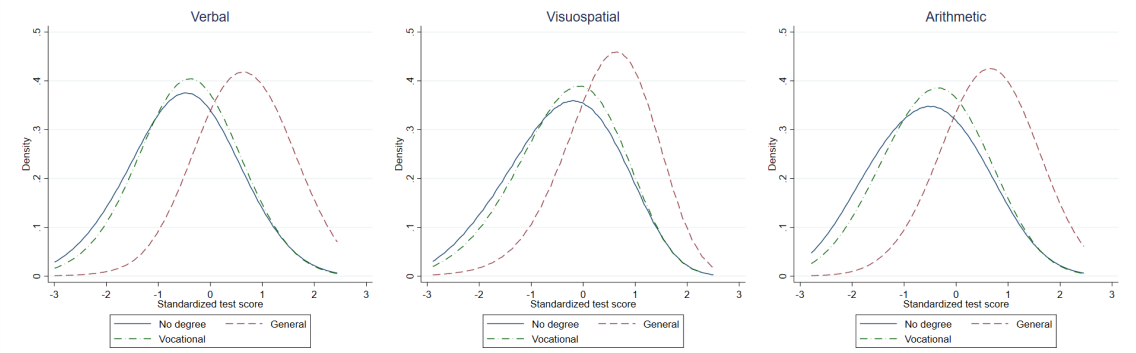
Figure 5.1c demonstrates that there are also large differences in several non-cognitive traits across education groups. Those who have participated in the general secondary school have substantially higher scores in measures related to

[6] Note that to generate a common scale that is comparable across different levels of education, we need to estimate a pooled regression model for all schooling levels. Naturally it is possible that some skills are more relevant for those with academic education and other skills for those with vocational education and that skills affect the choice of education as in the Roy model (Roy, 1951).

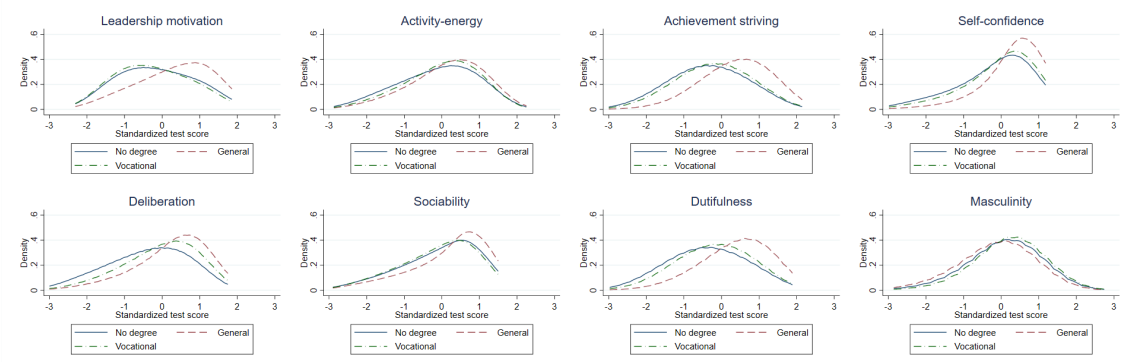
FIGURE 5.1 Distributions of test scores by education



(a)



(b)



(c)

Note: Figure 5.1 shows the distributions of the test scores by completed education at the time of taking the test. The sample includes men aged 18 to 22 at the end of the year in which they take the test. Panel (a) plots the anchored test scores, while panels (b) and (c) plot the standardized scores from each subtest.

motivation (leadership motivation and achievement motivation) but also in self-confidence, deliberation, sociability and dutifulness. As all these skills are valued at the labor market, those with general secondary education clearly are in an advantageous position. On the other hand, no major differences in skills can be detected between those with vocational education and those with no completed education after comprehensive school.

We collect means of the key variables used in the analysis to Table 5.2. In addition to differences in test scores displayed also in Figure 5.1, there are also large differences in student characteristics across schooling levels. The men who have a general secondary school degree have substantially higher grade point average in comprehensive school than men with vocational degree or no degree (8.3 vs. 6.7 or 6.5 on scale from 4 to 10). They also have more educated parents who have some 50% higher earnings than in the other two groups.

TABLE 5.2 Means of outcome and background variables by completed education.

	General	Vocational	No secondary
GPA (scale 4 to 10)	8.34	6.69	6.45
Average earnings at 35-39	46 000	33 600	26 300
Mother has at least secondary education	0.81	0.64	0.62
Father has at least secondary education	0.78	0.57	0.55
Parental income	320 200	233 200	230 200
Cognitive test score, anchored	10.43	10.23	10.20
Non-cognitive test score, anchored	10.35	10.29	10.26
Cognitive and non-cognitive test scores, anchored	10.47	10.21	10.15
Visuospatial	0.46	-0.29	-0.43
Verbal	0.63	-0.42	-0.52
Arithmetic	0.62	-0.40	-0.54
Leadership motivation	0.33	-0.25	-0.16
Activity energy	0.14	-0.02	-0.14
Achievement striving	0.39	-0.22	-0.35
Self-confidence	0.30	-0.11	-0.31
Deliberation	0.25	-0.05	-0.40
Sociability	0.19	-0.12	-0.07
Dutifulness	0.39	-0.19	-0.38
Masculinity	-0.15	0.20	0.06
N	59 394	59 572	24 468

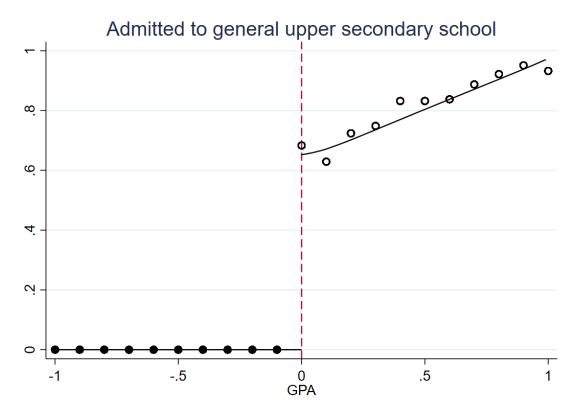
Note: Earnings and income are measured in 2018 euros. Parental income is the sum of the mother's and father's annual taxable incomes in 1991 to 1995.

5.4 Identification strategy and results

5.4.1 Identification strategy

Identifying the effect of education on skills is challenging for at least two reasons. First, education may foster skills, but skills may also affect educational aspira-

FIGURE 5.2 cut-off and admission into general upper secondary school



Note: Figure 5.2 shows the share of applicants admitted to general upper secondary education, plotted against the program-specific running variable. The dots depict sample means of the dependent variable for 0.1 GPA unit wide bins. The lines show local linear regressions weighted using an edge kernel and bandwidth 1.

tions and admission prospects to different schools. Solving this reverse causation issue requires some variation in education that is not affected by skills. Second, educational choices are likely to be correlated with various factors that are also correlated with skills (e.g. parent characteristics). Some of these factors can be controlled, but not all background characteristics can be measured in a reliable way. The resulting omitted variable problem generates a bias in the estimates.[7]

We identify the effects of admission into general upper secondary education on non-cognitive and cognitive skills by using admission cut-offs in a regression discontinuity design. We compare the outcomes of students who **all applied** to these schools but ended up on different sides of the admission threshold. Figure 5.2 shows that being above the admission cut-off increases the likelihood of being admitted to the general track by approximately 65 percentage points. Note that since we use the cut-off for the general upper secondary school with the lowest cut-off, it is possible for an applicant to be above the cut-off and not be admitted to the general track because they could have been accepted to a vocational track ranked higher in their preferences.

We define the cut-off for each school (k) in each year (t) as the GPA of the last accepted student. Our running variable for applicant i is then defined as:

$$r_{ikt} = c_{ikt} - \tau_{kt}, \tag{5.1}$$

where c_{ikt} is the applicant's GPA and τ_{kt} the cut-off to school k in year t.

To identify the effect of being above the cut-off on cognitive and non-cognitive skills, we pool data on each school and year, and estimate the following reduced form regression:

7 Table 5.A.1 summarizes OLS estimates of the effect of graduating the general track on the test scores using different the sample restrictions and control variables. In general, the OLS estimates show much larger effects on the test scores than the RDD estimates.

$$y_{ikt} = \alpha_{kt} + \beta Z_{ikt} + (1 - Z_{ikt}) f_0(r_{ikt}) + Z_{ikt} f_1(r_{ikt}) + \Gamma' X_i + e_{ikt}, \tag{5.2}$$

where y_{ikt} is the test score for applicant i to track k in year t. Z_{ikt} is an indicator variable for being above the cut-off to school k in year t, and r_{ikt} is the running variable centered at the cut-off (value 0). We allow the slope of the running variable (f_n) to differ on either side of the cut-off. We include fixed effects for each cut-off and their interactions with the running variable. The error terms e_{ikt} are clustered at the cut-off level. X_i is a vector of control variables that includes birth year fixed effects and the first and second polynomials of age at test measured in days. Between 1996 and 1998, the non-cognitive test was conducted at the draft instead of after entering military service. Since the two testing sites may not be entirely comparable, X_i also includes a dummy indicating if the individual took the non-cognitive test at the draft.

We also employ an instrumental variable strategy (fuzzy RDD) to convert the reduced form estimates to effects of general upper secondary schooling. Using this strategy, we estimate the local average treatment effect (LATE) of general upper secondary school on the cognitive and non-cognitive skills. We report first stage and LATE estimates for two separate treatment variables D_i. The first treatment variable indicates that the applicant was admitted to general upper secondary education, and the second, that the applicant has completed general upper secondary school by the time of entering military service. The first stage of this fuzzy RD design is Equation 5.2 where the outcome variable is D_i.

We estimate Equation 5.2 using non-parametric local linear regression with triangular kernel weights:

$$K(r_i) = (1 - \frac{r_i}{h}) \mathbb{1}(\frac{r_i}{h} \leq 1), \tag{5.3}$$

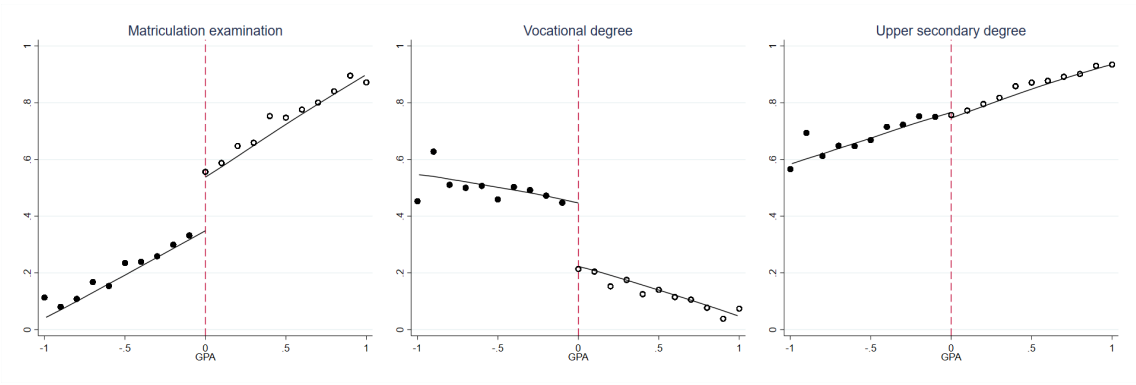
where h is the bandwidth determining the observations that are sufficiently close to the threshold to be used in estimating the effect of admission. We estimate the optimal bandwidth estimated using the selection procedure in Calonico et al. (2014). However, to make estimates with different outcomes comparable, we use a bandwidth of 0.5 GPA units in all baseline specifications. [8]

5.4.2 Results

Figure 5.3 illustrates the effect of exceeding the admission threshold on completed degrees. The likelihood of completing general upper secondary education by the time of entering military service increases with comprehensive school

[8] Optimal bandwidths vary between 0.3 and 1.3 depending on the outcome. In general, the optimal bandwidths are lower below than above the admission thresholds. Table 5.A.2 presents RDD estimates on our main outcomes of interest. Our main results are largely unaffected by the choice between the optimal bandwidths or a fixed bandwidth of .5 GPA units.

FIGURE 5.3 Admission cut-offs into general upper secondary school and completed upper secondary degrees.



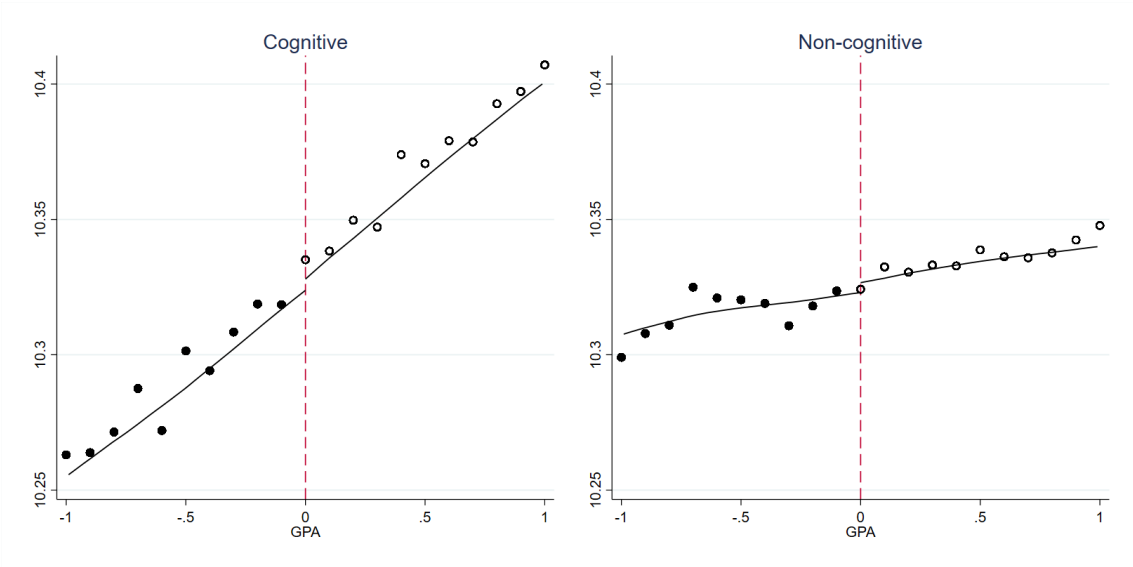
Note: Figure 5.3 shows the share of students completing the matriculation examination, vocational degree, or either of these, plotted against the program-specific running variable. The dots depict sample means of the dependent variable for 0.1 GPA unit wide bins. The lines show local linear regressions weighted using an edge kernel and bandwidth 1.

GPA. However, there is a clear discontinuity at the admission threshold of the general upper secondary school with the lowest admission criteria (dashed line in the figure). Some students who scored below this threshold still enter general upper secondary school either by applying directly for schools after the centralized admission system is completed or by re-applying in the following years. Also some students above the admission threshold never complete general upper secondary school or at least have not done so by time of entering military service. Some of these students are admitted but drop out of program at some stage. Others have ranked a vocational program higher in their secondary school application and hence end up in vocational school even though they would have been admitted to general upper secondary school.

The middle panel of Figure 5.3 shows the likelihood of completing a vocational school as a function of (re-scaled) comprehensive school GPA. This is a mirror image of the left panel. Likelihood of completing vocational training decreases with the comprehensive school GPA and displays a clear drop at entry threshold to general upper secondary school. The rightmost panel of Figure 5.3 confirms that exceeding the admission threshold of general upper secondary school mainly affects the type of school rather than amount of schooling. Exceeding the admission threshold has no effect on the likelihood of completing some upper secondary education.

In Figure 5.4 we show the effect of schooling on skills - the main question analysed in this paper. While cognitive and non-cognitive skills are both positively correlated with comprehensive school GPA, cognitive skills display a stronger correlation with the comprehensive school GPA. Based on Figure 5.4, admission into general upper secondary education has little, if any, effect on cognitive and non-cognitive skills. There is a visible yet economically insignificant jump at the upper general secondary school admission threshold for both skill

FIGURE 5.4 Anchored test scores and admission cut-offs into general upper secondary education.



Note: Figure 5.4 plots the anchored test scores against the program-specific running variable. The dots depict sample means of the dependent variable for 0.1 GPA unit wide bins. The lines show local linear regressions weighted using an edge kernel and bandwidth 1.

measures.

In Table 5.3 we verify the validity of our approach by examining the effect of exceeding the admission threshold on pre-determined variables. According to these results, our treatment is uncorrelated with parents' education and living in an urban area. However, there is a discontinuity in father's earnings at the cut-off that is significant at the 10% level. Adding controls for parents' earnings and education does not change our results.

In the middle part of Table 5.3 we show that exceeding the admission threshold has a large effect on school environment. Average peer GPA increases by almost one unit (roughly one standard deviation). Share of women among classmates increases by 15 percentage points. Exceeding the admission threshold also significantly increases the average test scores of the classmates. Finally, peers above the threshold have, on average, parents with higher earnings and more education.

In the bottom section of Table 5.3, we replicate the results already shown in Figure 5.3. Exceeding the admission threshold increases the likelihood of completing general secondary school by about 20 percentage points and has roughly equal negative effect on the likelihood of obtaining a vocational secondary degree. Hence, exceeding the threshold mainly affects the type of education and has no significant effects on completing secondary school by the time of entering military service. As the main purpose of general secondary school is to prepare students for higher education it is not really surprising that exceeding the threshold increases the odds of later completing a tertiary degree.

TABLE 5.3 Effects of the admission threshold on pre-determined variables, peer characteristics and subsequent outcomes

Pre-determined variables		
Urban	0.004	(0.015)
Semiurban	-0.009	(0.011)
Rural	0.005	(0.013)
Mother's earnings	11	(3 300)
Mother has a secondary degree	0.039	(0.025)
Father's earnings	9 200*	(5 200)
Father has a secondary degree	0.014	(0.026)
Peer characteristics		
GPA (scale 4 to 10)	0.818***	(0.058)
Share of women	0.149***	(0.017)
Cognitive test score, anchored	0.064**	(0.021)
Non-cognitive test score, anchored	0.109**	(0.052)
Cognitive and non-cognitive test scores, anchored	0.083***	(0.021)
Mother's earnings	9 600***	(1 200)
Mother has a secondary degree	0.071***	(0.009)
Father's earnings	18 200***	(3 100)
Father has a secondary degree	0.083***	(0.010)
Subsequent outcomes		
General secondary degree	0.179***	(0.026)
Vocational secondary degree	-0.219***	(0.027)
Secondary degree	-0.014	(0.024)
Tertiary degree	0.046*	(0.027)
Average annual earnings at ages 16-19	-10	(100)
Average annual earnings at ages 20-24	-1 000**	(400)
Average annual earnings at ages 25-29	-1 200	(800)
Average annual earnings at ages 30-34	200	(1 000)
Average annual earnings at ages 35-39	13	(1 300)
Attended military†	0.018	(0.014)
Age at non-cognitive test	0.018	(0.032)
Age at cognitive test	0.040	(0.044)

Note: Each entry in the table is an estimate from a local linear regression using triangular kernel weights and a bandwidth of .5 GPA units. Standard errors clustered by cut-off are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Earnings and income are measured in 2018 euros. Mother's and father's earnings are the sum of annual taxable incomes in 1991 to 1995. All regressions include fixed effects for each cut-off, interactions between each cut-off and the running variable, birth year fixed effects, and the first and second polynomials of age at test measured in days. We include age at test as a control to maintain the same specification as in our main estimates. † We do not include the age at test as a control in the regression for attending military, since this information is only available for those individuals that attended military and took the test.

TABLE 5.4 RDD estimates of the effect of general upper secondary education on anchored test scores

	Non-cognitive	Cognitive	All
Reduced form:	-0.002	0.004	0.002
	(0.007)	(0.008)	(0.010)
Admission:			
First stage:	0.643***	0.643***	0.643***
	(0.022)	(0.022)	(0.022)
LATE:	-0.003	0.006	0.003
	(0.011)	(0.012)	(0.016)
Completed degree:			
First stage:	0.181***	0.181***	0.181***
	(0.027)	(0.027)	(0.027)
LATE:	-0.011	0.020	0.009
	(0.038)	(0.043)	(0.055)
N	8 317	8 317	8 317

Note: Each entry in the table is an estimate from a local linear regression using triangular kernel weights and a bandwidth of .5 GPA units. All regressions include fixed effects for each cut-off, interactions between each cut-off and the running variable, birth year fixed effects, the first and second polynomials of age at test measured in days, and a dummy indicating if the individual took the non-cognitive test at the draft. Standard errors clustered by cut-off are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

An increase in the likelihood of entering tertiary education is also reflected in the effect on later earnings. Earnings are reduced at ages 20 to 24 when those who enter tertiary educational institutions are mostly still at school. After these ages, the effect on earnings decreases and approaches zero by age 39. This finding is roughly in line with findings of Silliman and Virtanen (in press) who use same data for more recent cohorts to evaluate the effect of schooling on earnings.[9]

Finally, we check that exceeding the admission threshold has no significant effect on the likelihood of entering military service (and taking the test) or the age at which the test is taken. We were concerned on selectivity in the test score data but according to these results there is no indication that admission would have an effect on the availability or the timing of the test scores.

The main results are collected in Tables 5.4, 5.5 and 5.6. First, in Table 5.4 we estimate the effect of secondary schooling on aggregate measures of cognitive and non-cognitive skills. As noted in Section 5.3.4 the outcome variables are anchored to later earnings and the effects can therefore be interpreted as proportional effects on economic value of these skills. According to the results in Table 5.4 completing upper secondary school has no significant causal effect on either cognitive or non-cognitive skills. The estimates are, not only insignificantly dif-

[9] The set-up in Silliman and Virtanen (in press) is slightly different as they compare vocational secondary education to general secondary education while we compare general secondary to all others including the group that quits school after compulsory comprehensive school. Exact replication of Silliman and Virtanen (in press) is not possible for the cohorts we use in this paper (and for whom military test scores are available) due to lack of data on exact entry criteria used by vocational schools.

TABLE 5.5 RDD estimates of the effect of general upper secondary education on cognitive skills

	Visuospatial	Verbal	Arithmetic
Reduced form:	0.009	0.029	0.009
	(0.048)	(0.043)	(0.046)
Admission:			
First stage:	0.638***	0.638***	0.638***
	(0.022)	(0.022)	(0.022)
LATE:	0.014	0.046	0.014
	(0.076)	(0.068)	(0.071)
Completed degree:			
First stage:	0.180***	0.180***	0.180***
	(0.027)	(0.027)	(0.027)
LATE:	0.044	0.159	0.049
	(0.268)	(0.242)	(0.254)
N	8 375	8 375	8 375

Note: Each entry in the table is an estimate from a local linear regression using triangular kernel weights and a bandwidth of .5 GPA units. Each outcome variable is standardized to mean 0 and standard deviation 1. All regressions include fixed effects for each cut-off, interactions between each cut-off and the running variable, birth year fixed effects, the first and second polynomials of age at test measured in days, and a dummy indicating if the individual took the non-cognitive test at the draft. Standard errors clustered by cut-off are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

ferent from zero, but also small in magnitude. The reduced form estimates are relatively precise so that effects exceeding 2% both in cognitive skills and in non-cognitive skills fall outside the confidence interval. Comparing these effects to raw differences in skills across school type in Table 5.2 reveals that the differences across school type are mainly due to selection rather than the effects of different school types on skills. Causal effect of education on skills only represents a small fraction of the observed skill differences across the schooling levels.

Table 5.5 presents the results related to individual test sections. Given that we found no effects on the aggregate-level skill measures it is not so surprising that we find no effects on sub-test scores either. Also some cognitive skill measures, particularly the visuospatial test are related to fluid intelligence i.e. the ability to reason and think flexibly rather than crystalized intelligence i.e. accumulation of knowledge, facts, and skills that are acquired throughout life. Finding no effect on visuospatial test scores is consistent with previous results according to which fluid intelligence is independent of learning, experience, and education.

Finding that type of secondary education has no effect on arithmetic or verbal abilities is perhaps more surprising. After all, there is much more training in math and much more reading and writing assignments in the general upper secondary school than in vocational education. However, the Defence Forces Basic Skills Test measures rather basic arithmetic and verbal skills, not skills in differential calculus or essay writing. Note however, that these basic skills still have strong impact on earnings and hence demonstrated value in the labor market.

TABLE 5.6 RDD estimates of the effect of general upper secondary education on personality traits

	Leadership motivation	Activity-energy	Achievement striving	Self-confidence
Reduced form:	0.057	-0.005	0.030	-0.033
	(0.057)	(0.059)	(0.054)	(0.051)
Admission:				
First stage:	0.643***	0.643***	0.643***	0.643***
	(0.022)	(0.022)	(0.022)	(0.022)
LATE:	0.089	-0.009	0.046	-0.051
	(0.089)	(0.092)	(0.084)	(0.079)
Completed degrees:				
First stage:	0.181***	0.181***	0.181***	0.181***
	(0.026)	(0.026)	(0.026)	(0.026)
LATE:	0.313	-0.030	0.161	-0.182
	(0.319)	(0.326)	(0.300)	(0.280)
N	8317	8317	8317	8317
	Deliberation	Sociability	Dutifulness	Masculinity
Reduced form:	0.039	-0.011	0.037	-0.134***
	(0.062)	(0.055)	(0.059)	(0.050)
Admission:				
First stage:	0.643***	0.643***	0.643***	0.643***
	(0.022)	(0.022)	(0.022)	(0.022)
LATE:	0.061	-0.017	0.058	-0.209***
	(0.096)	(0.086)	(0.091)	(0.079)
Completed degrees:				
First stage:	0.181***	0.181***	0.181***	0.181***
	(0.026)	(0.026)	(0.026)	(0.026)
LATE:	0.215	-0.061	0.205	-0.741**
	(0.341)	(0.304)	(0.322)	(0.292)
N	8317	8317	8317	8317

Note: Each entry in the table is an estimate from a local linear regression using triangular kernel weights and a bandwidth of .5 GPA units. Each outcome variable is standardized to mean 0 and standard deviation 1. All regressions include fixed effects for each cut-off, interactions between each cut-off and the running variable, birth year fixed effects, the first and second polynomials of age at test measured in days, and a dummy indicating if the individual took the non-cognitive test at the draft. Standard errors clustered by cut-off are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In Table 5.6, we show the effects on the individual elements of the non-cognitive test. Again we find only one significant effect even for traits where the differences across school type are the largest suggesting again that these differences are mainly due to selection rather that effects of type of secondary schooling completed. The only effect that turns out to be statistically significant is a negative effect on masculinity.

5.5 Conclusion

Admission to general vs vocational education after compulsory comprehensive school at age 16 leads to very different school environment for the following three years. General education is academically oriented and prepares students for higher education while vocational education focuses on practical occupation-specific skills. Also peer groups are quite different - students who end up in general education have much "higher quality" peers when peer quality is measured by average school grades, test scores or parents' education.

According to the results in this paper, these differences in school environment have little effect on basic skills measured in the military tests at age 19 or 20. Despite large differences in the test scores between men with different educational backgrounds, we find no causal effects of education on cognitive or non-cognitive skills using a regression discontinuity design created by a centralized application system in Finnish secondary education. Thus, the differences in skills between the general and vocational tracks arise from selectivity rather than as a causal effect of schooling.

Taken at the face value our results imply that important cognitive and non-cognitive skills are set at relatively young age and are not much affected by schooling after age 16. The finding also suggests that efforts on identifying the effects of schooling on key cognitive and non-cognitive skills should focus on younger children.

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5.A Appendix: Tables and Figures

OLS estimates

Table 5.A.1 presents OLS estimates of the effects of completing the general track on the anchored test scores. In particular, we study how the estimated effects are affected by restricting the sample and accounting for selection on observable characteristics. The effects are estimated separately using either the full sample or the RDD sample as described in section 5.3.3 with and without control variables.

In the first panel, we use the full sample. The estimated effects of completing the general track on the test scores without control variables are large and correspond to the differences in average test scores presented in Table 5.2. According to these estimates, completing the general track has an effect of 5.6 % on non-cognitive skills and 19.4% on cognitive skills. Adding control variables for GPA, age at test, and birth year reduces the size of the estimated effects by approximately a third.

In the second panel, we restrict the estimation sample as we do for our RDD design. The effects with and without controls are now smaller than the corresponding estimates in the first panel. By using the RDD sample, we exclude those individuals who only applied to vocational school or dropped out at the end of compulsory school. These individuals score lower in the skills tests which contributes to the smaller estimates than with the full sample. However, even after adding control variables, especially the estimated effects on cognitive skills are still large compared to our RDD estimates in Table 5.4.

TABLE 5.A.1 OLS estimates of the effects of completing general upper secondary education on anchored test scores.

	(1) Non-cognitive	(2) Cognitive	(3) All	(4) Non-cognitive	(5) Cognitive	(6) All
Full sample:						
Matriculation examination	0.066***	0.208***	0.275***	0.043***	0.139***	0.182***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
N	143 512	143 512	143 512	143 512	143 512	143 512
Controls	NO	NO	NO	YES	YES	YES
RDD sample:						
Matriculation examination	0.031***	0.118***	0.149***	0.010***	0.040***	0.050***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
N	41 164	41 164	41 164	41 164	41 164	41 164
Controls	NO	NO	NO	YES	YES	YES

Note: Full sample includes men aged 18 to 22 at the end of the year in which they take the test from birth cohorts 1974-1979. RDD sample refers to the estimation sample used in our RDD estimates (see section 5.3.3). Control variables include dummies for .5 wide GPA intervals, age at test, age at test squared, and birth year dummies. Robust standard errors are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

RDD bandwidth

In Table 5.A.2, we estimate the effect of general upper secondary education on anchored test scores with optimal bandwidths selected using the selection procedure in (Calonico et al., 2014). The bandwidths are selected separately below and above the cut-off. As in Table 5.4, the estimates are close to zero and insignificant, leaving our conclusions unaltered.

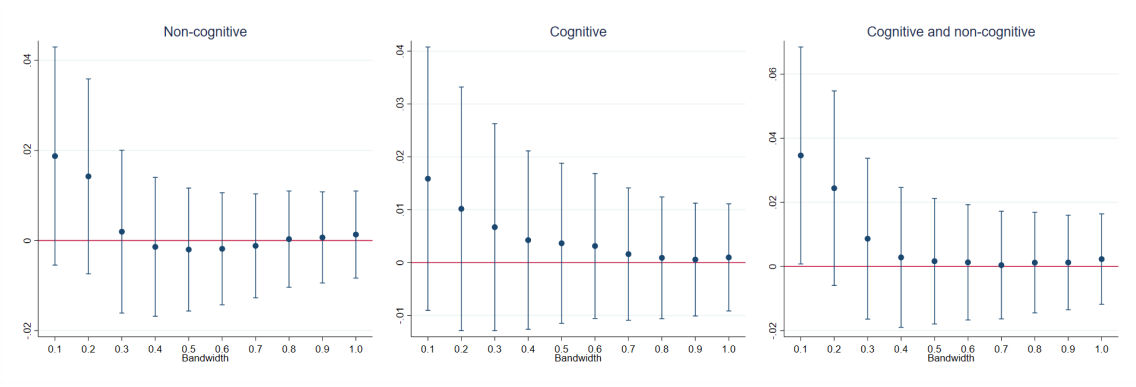
TABLE 5.A.2 RDD estimates of the effect of completing general upper secondary education on anchored test scores using optimal bandwidths

	Non-cognitive	Cognitive	Non-cognitive and cognitive
Reduced form	-0.000	-0.001	-0.003
	(0.006)	(0.006)	(0.009)
Admission:			
First stage:	0.585***	0.589***	0.592***
	(0.021)	(0.021)	(0.021)
LATE:	-0.000	-0.001	-0.005
	(0.011)	(0.011)	(0.015)
Completed degree:			
First stage	0.173***	0.163***	0.171***
	(0.024)	(0.022)	(0.023)
LATE	-0.000	-0.003	-0.017
	(0.037)	(0.039)	(0.052)
N	20 363	17 702	19 913
Optimal bw below/above	.30/1.20	.47/1.02	.34/1.19

Note: Each entry in the table is an estimate from a local linear regression using triangular kernel weights and the optimal bandwidths selected separately below and above the cut-off using the selection procedure in (Calonico et al., 2014). All regressions include fixed effects for each cut-off, interactions between each cut-off and the running variable, birth year fixed effects, the first and second polynomials of age at test measured in days, and a dummy indicating if the individual took the non-cognitive test at the draft. Standard errors clustered by cut-off are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In Figure 5.A.1, we examine the robustness of our main estimates to different bandwidths. Figure 5.A.1 reports the reduced form estimates for a range of bandwidths from .1 to 1 GPA units on both sides of the cut-off along with the corresponding 95 percent confidence intervals. In general, the estimates resemble our main results in that they are close to zero, and except for a single estimate with a bandwidth of .1 in the rightmost subfigure, also statistically insignificant.

FIGURE 5.A.1 Robustness to alternate bandwidths.



Note: Figure 5.A.1 plots the RDD estimates of crossing the admission threshold on the anchored test scores from local linear regressions using triangular kernel weights. We present estimates for bandwidths ranging from .1 to 1 GPA units on both sides of the cut-off. All regressions include fixed effects for each cut-off, interactions between each cut-off and the running variable, birth year fixed effects, the first and second polynomials of age at test measured in days, and a dummy indicating if the individual took the non-cognitive test at the draft. For each point estimate, we also present the 95 percent confidence intervals. Standard errors are clustered by cut-off.

Additional robustness checks

In Table 5.A.3 we perform two additional robustness checks on the main results. First, since the admission cut-offs are defined by the last accepted student into a program, we want to ensure that our estimates are not biased by possible endogeneity arising from this definition. To this end, we use a donut-RDD strategy where we drop the applicants who determine the cut-offs in our sample. The reduced form estimates using this strategy are presented in the first panel of Table 5.A.3. The estimates remain close to zero and insignificant.

Second, since we observed a discontinuity in father's earnings at the cut-off, we test whether our estimates are sensitive to the inclusion of controls for parental background. The second panel of Table 5.A.3 shows estimates from a model with controls for both parents' earnings and whether they had secondary education. The inclusion of these controls does not significantly affect our estimates.

TABLE 5.A.3 Robustness checks

	Non-cognitive	Cognitive	All
Donut:			
	-0.010	-0.002	-0.012
	(0.011)	(0.011)	(0.015)
N	7 295	7 295	7 295
Parental controls:			
	-0.008	0.003	-0.005
	(0.007)	(0.008)	(0.010)
N	7 856	7 856	7 856

Note: Each entry in the table is an estimate from a local linear regression using triangular kernel weights and a bandwidth of .5 GPA units. All regressions include fixed effects for each cut-off, interactions between each cut-off and the running variable, birth year fixed effects, the first and second polynomials of age at test measured in days, and a dummy indicating if the individual took the non-cognitive test at the draft. Standard errors clustered by cut-off are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

YHTEENVETO (FINNISH SUMMARY)

Tässä väitöskirjassa tarkastellaan ammatillisen koulutuksen, inhimillisen pääoman sekä työmarkkinatulemien yhteyttä yli elinkaaren taloustieteen näkökulmasta. Väitöskirja koostuu johdantoluvusta sekä neljästä tutkimusartikkelista. Johdantoluvussa esitellään kunkin artikkelin tutkimuskysymykset, aikaisempi kirjallisuus, käytetyt aineistot ja menetelmät sekä keskeiset tulokset.

Luvussa 2 tutkin, kuinka Suomen vuosien 1972–1977 peruskoulu-uudistus vaikutti työmarkkinatulemiin yli elinkaaren. Uudistuksen keskeisimpinä toimina oppilaat jaettiin vanhaan kansakouluun verrattuna myöhemmin ammatilliseen ja yleisivistävään koulutukseen ja akateemisten oppiaineiden määrä opintosuunnitelmassa lisättiin. Hyödynnän tutkimuksessa Tilastokeskuksen rekisteripohjaisia aineistoja mm. työssäkäynnistä, koulutuksesta ja tuloista. Tutkimuksessa selvisi, että peruskoulu-uudistuksella oli uran alkuvaiheessa negatiivinen vaikutus todennäköisyyteen työllistyä. Tämä vaikutus kuitenkin hävisi iän myötä, ja myöhemmällä työuralla uudistuksen vaikutus työllisyyteen kääntyi positiiviseksi. Havaitsen myös positiivisen vaikutuksen ansiotuloihin myöhemmällä työuralla. Uudistus lisäsi tulosten mukaan myös todennäköisyyttä suorittaa toisella asteella ylioppilastutkinto ammatillisen tutkinnon sijasta, mikä voi osaltaan selittää uudistuksen työmarkkinavaiktuksia.

Luku 3 käsittelee vuosina 1999–2001 toteutetun ammatillisen koulutuksen uudistuksen vaikutuksia opintojen keskeyttämiseen, työmarkkinatulemiin ja jatko-opintojen suorittamiseen. Uudistuksen myötä aiemmat kaksivuotiset ammatilliset tutkinnot korvattiin kolmevuotisilla tutkinnoilla. Uudistuksen jälkeen kaikki tutkinnot antoivat yleisin jatko-opintokelpoisuuden, kun taas uudistusta ennen ammatilliset tutkinnot antoivat kelpoisuuden vain ammattikorkeakouluun. Tutkintoihin liitettiin myös pakollinen työssäoppimisen jakso, millä pyrittiin lisäämään opintojen työelämävastaavuutta. Tutkimusaineistona käytetään Tilastokeskuksen rekisteriaineistoja sekä Opetushallituksen yhteisvalintarekisteriä, joiden avulla voimme seurata tulemia 13 vuotta opintojen aloittamisen jälkeen. Tutkimuksen tulosten mukaan uudistuksella ei ollut vaikutusta työllisyyteen, ansiotuloihin tai jatko-opintojen suorittamiseen. Havaitsemme kuitenkin uudistuksen kasvattaneen todennäköisyyttä keskeyttää opinnot.

Luvussa 4 arvioidaan jatkuvan oppimisen tukemista keskellä työuraa "Taidot työhön" -interventiotutkimuksen avulla. Tutkimus toteutettiin satunnaistettuna kenttäkokeena, johon osallistui 1119 insinööriä ja tradenomia. Osallistujien keski-ikä oli 49 vuotta. Osallistujat arvottiin koe- ja kontrolliryhmiin, ja koeryhmä osallistui ryhmävalmennukseen, jossa pyrittiin edistämään osaamisen kehittämistaitoja. Osallistujat vastasivat kyselylomakkeisiin ennen interventiota sekä 12 kuukautta sen jälkeen. Tulosten perusteella ryhmävalmennus lähes kaksinkertaisti koeryhmän todennäköisyyden suorittaa korkeakouluopintoja verrattuna kontrolliryhmään.

Luvussa 5 verrataan, kuinka ammatillinen ja yleissivistävä toisen asteen koulutus vaikuttavat kognitiivisiin kykyihin ja persoonallisuuspiirteisiin.

Tutkimuksessa käytettävät psykologiset mittarit ovat peräisin Puolustusvoimien peruskokeista. Tulostemme mukaan sekä testien mittaamat kognitiiviset kyvyt että persoonallisuuspiirteet ovat vahvasti korreloituneita myöhemmän työmarkkinamenestyksen kanssa. Hyödynnämme tutkimuksessa toisen asteen yhteisvalinnan sisäänpääsyrajoja ja regressioepäjatkuvuusmenetelmää, joiden avulla pyrimme selvittämään koulutuksen kausaalivaikutuksen kognitiivisiin kykyihin ja persoonallisuuspiirteisiin. Kuvailevan analyysin mukaan testituloksissa on suuria eroja suoritetun koulutuksen suhteen. Ylioppilastutkinnon suorittaneet pärjäävät muita paremmin sekä kognitiivisia kykyjä sekä persoonallisuutta mittaavissa testeissä. Emme kuitenkaan havaitse toisen asteen koulutuksella olevan suoraa kausaalivaikutusta testituloksiin. Tulostemme mukaan erot testituloksissa johtuvat siis pääosin kyvyiltään erilaisten oppilaiden valikoitumisesta yleissivistävään ja ammatilliseen koulutukseen.