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Hannu Karhunen

Economic Studies on Higher Education and Productivity

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ABSTRACT

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This dissertation consists of an introductory chapter and four separate empirical studies which study higher education and productivity in Finland. Introductory chapter presents shortly the motivation, background and main results of this thesis. First three articles focus on the relationship between working while studying, dropout behavior and later labor market outcomes at individual level. Fourth article examines how subsidies affect labor productivity at the firm level.

The first article of this thesis studies how working while studying relates to earnings after graduation from university or polytechnic. Analysis focuses on working while studying and earnings over two year period after graduation. The results show that early work experience is related to higher earnings right after graduation year, but instrumental variable estimations find no significant causal effect for university students.

The second article explores on the relationship between working while studying and migration. Migration propensities are studied up to three years after graduation. Results show that working while studying can partly explain why the mobility of highly educated people has declined. We find that that there are differences by study region and earlier migration behavior.

The third article examines the relationship between the decision to drop out from university and labor market outcomes. Results indicate that compared to similar university graduates, the dropouts' annual earnings are on average 11,000 euros lower four years after the dropout decision. Dropouts have also a higher probability of being self-employed than similar graduates.

The fourth article examines the effect of R&D subsidies on labour productivity. Study uses firm-level data on Finnish SMEs from 2000 to 2012 and apply a combined matching and difference-in-differences method to control for selection bias. Study does not find significant positive effect on labour productivity over the five-year period after a subsidy is granted.

Keywords: Work and study, higher education, earnings, migration, labour supply, productivity.

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The relationship between thesis supervisors and doctoral students is mainly based on trust. Supervisors offer guidance, help find funding and emphasize career possibilities after dissertation. Graduate students invest years of their lives studying and researching without really knowing what it means and what are the chances to succeed. Risks could feel overwhelming if one is left alone. Thus, it is important to know that supervisors trust you even if sometimes you don't believe in yourself. Here, I would specially like to thank my supervisors Professor Hannu Tervo and Professor Jaakko Pehkonen for all the trust they have placed in me during these years. I would like to thank Professor Kari Heimonen for encouragement and Professor Ari Hyytinen for constructive comments. Realistic encouragement and just criticisms are necessary to develop one's ideas as a researcher.

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In the past six years I have had the privilege of getting to know many interesting individuals from Jyväskylä University School of Business and Economics and other research communities. Many have become dear friends for me. Dr. Tuomo Suhonen and I shared a room for almost a year in the beginning of my PhD career. One of the papers in this thesis is coauthored by Dr. Mika Haapanen who patiently offered time and well-structured guidance for me every single time I asked for help. I'm deeply thankful to Dr. Signe Jauhiainen for connecting me with Janne Huovari leading to creation of one the papers in this thesis. Juho Jokinen is just an unbelievably good person. I thank you and others for your support and I hope that we will not only work together in the future, but also continue to spend quality time outside of work.

This thesis would not have been possible without the support of my family. I thank my parents, Kaarina and Kyösti, my sister Heli, her husband Esa and my godchild Leo, who helped me to cope with life during this project. I also thank dear childhood friends from Keitele, who have always remembered me and understood why I'm doing this. Finally, above all I want to thank Terhi who brought peace and love in my soul. When I'm with you, I feel complete.

This thesis is devoted to my parents whose work ethics I have always admired. My parents had to start to work at the age of 15, but they believed that education is a key to better life. In our family, work was highly valued and the children's work was to study. My mom and dad worked very hard to make a better future for their children. I was hoping that my dad would see the day when both of his children have a doctoral degree, but unfortunately fathers die too soon in Finland. Love stays.

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CONTENTS

CHAPTER 1
INTRODUCTION

1 Background of the thesis

Economic theory suggests that productivity growth can be enhanced in market economies by directing resources to education and to research and development (R&D). There are two main efficiency rationales that can be used to justify the redistribution of public funds to these sectors (see, e.g., Lerner, 2002; Hanushek, 2002; Scott-Clayton, 2013). First, social returns from education can be higher than private returns, e.g., education might have positive externalities on health and the crime rate (Lochner, 2011). As public investments decrease the private costs of education, individuals will acquire more education than they would without the support of the public sector. This enhances possible social returns. A similar argument applies to public R&D investments. For example, private firms have little incentives to invest in basic research that benefits their market competitors and the surrounding society, and public funds are needed to raise overall investments to a more socially optimal level. Second, there are inefficiencies that result from a lack of information. Private financiers cannot reliably predict the future performance of firms that apply for loans for R&D projects or whether students will eventually be able to repay their student loans. Incomplete information also affects the decisions of firms and individuals. Firms will avoid beginning risky R&D projects if it is difficult to predict the possible returns on investments. Students who are uncertain of their own capabilities do not necessarily enter higher education (HE) because there may be substantial costs following a possible failure to graduate; there is no insurance for human capital production. Again, these inefficiencies can be reduced by public sector investments that lower the risks taken by firms and individuals (e.g., by offering loans and direct funding).

The public sector can influence individual- and firm-level decision-making processes by affecting market prices and by regulation. How and to what extent the public sector should intervene in HE and private R&D is theoretically and empirically unclear. The basic problem is that the socially optimal level of investment in these areas is unknown and there are no clear guidelines on how public funds could be most efficiently distributed (e.g., Barr, 2004). There is a large literature showing that the public sector should concentrate it investments in basic and secondary education because the private and social returns are estimated to be higher at lower levels of education than those that can be obtained from HE (e.g., Acemoglu and Angrist, 1999; Psacharopoulos and Patrinos, 2004). Research evidence also suggests that the public sector should invest in universities and research institutes because the majority of private firms do not finance their own basic research (for an exception see, e.g., Roosenberg, 1990). Market imperfections are not particularly clear in business environments in which private firms offer new services and design new consumer products. Governments are thus often forced to determine the scale of their intervention without prior information on market inefficiencies. This raises the need to evaluate different policies to ensure that scarce resources are allocated in the most efficient way.

The public sector disbursed 523 million euros in direct subsidies for Finnish firms in 2013, which is 30 per cent more than the figure in 2006 (OSF, 2013a). However, the level of student aid for HE has not increased in Finland in the past two decades. In fact, after the student grant reform in 1992, the real value of student aid (the maximum value of the study grant and housing benefit) has decreased relative to the value in the year of the reform. When voluntary student loans are included, the maximum level of support was only marginally higher in 2014, when grants were tied to the price index ("Kansaneläkeindeksi"). Thus, students have been compelled to finance their time spent studying by other means, namely through work. However, the increase in student employment raises the question of how this affects "in-school" human capital accumulation and subsequent labour market success. Time spent working could have been used studying and networking through student activities. The literature does not offer conclusive evidence on the benefits of working while studying relative to full-time human capital accumulation. A significant share of the costs of HE has been thus transferred to students, but whether working while studying is the most efficient way to finance private costs is not well understood.

In Finland, the combined expenditures in the HE sector (expenditures and student allowances) represented 2 per cent of GDP in 2012, and nearly 30 000 firms were directly subsidised by government agencies (OSF, 2012a, 2012b, 2013c). This thesis presents four separate micro-level studies that focus on publicly funded HE and R&D subsidies granted to private firms in Finland. The articles focus on productivity at the individual and firm level. The first two articles examine student employment and labour market outcomes after graduation. There are substantial incentives to work during HE in Finland, but how this relates to productivity-enhancing outcomes is not well understood. The third article focuses on university dropouts. As the direct costs of university education in Finland are still born by the public sector, the individual incentives to enter university are also high. Information on dropouts and the extent of their success in the labour market is crucial when evaluating the effectiveness of publicly funded university education. Finally, public funds are also directed to private R&D projects to foster economic growth. High-skilled labour is necessary to conduct R&D projects, but firms also receive direct subsidies and loan guarantees from the public sector. The fourth article of this thesis examines how R&D subsidies affect firm productivity in low- and high-skill firms.

The remainder of the introductory chapter is organised as follows. Section 2 presents a review of the theoretical background on education, productivity and the association between student employment and subsequent labour market outcomes. This section also provides information on HE funding schemes and how students are supported in Finland. An overview of HE and student employment is also offered in this section. Section 3 provides an overview of the thesis, including specific research questions, main findings and concluding remarks.

2 Higher education, work and productivity

2.1 Education and productivity

Educated individuals earn, on average, more than individuals with less education. This phenomenon is perhaps the most studied issue in labour economics. Theoretically, there are two main approaches that strive to explain the positive correlation between education and earnings. Human capital theory (developed by Schultz, 1961; Becker, 1962) emphases that resources spent on education improve individuals' skill level and that a higher level of skills leads to higher productivity, and thus higher earnings. Education can be understood to contribute to the stock of human capital, which is directly used in complex production processes (e.g., Becker, 1964). It can also enhance skills that are needed to adapt and diffuse new technologies (e.g., Schultz, 1961; Nelson and Phelps, 1966). Alternatively, the positive correlation between education and earnings does not reflect the accumulation of skills that are utilised in labour markets. Signalling theory, beginning with Spencer (1973), suggests that firms cannot observe which individuals are productive but that education is a signal that helps firms to recognise the most productive individuals. Thus, in the extreme version, firms do not hire educated employees because of the skills accumulated during their studies but because of their inner productivity as demonstrated by their educational achievements.

The overwhelming empirical evidence shows that individuals obtain positive returns to education even after controlling for individual-level heterogeneity (see, e.g., Angrist and Krueger, 1991; Card, 1999). Previous studies support both human capital and signalling theory (e.g., Harmon, Oosterbeek and Walker, 2003). This is not surprising, as there is a wide variety of occupations requiring different skills. The most common method in the empirical literature is to examine the returns to education by estimating Mincerian wage regressions (formally constructed by Mincer, 1958, 1974; Ben Porath, 1967). A simplified equation estimates the relationship between individual earnings and individual-level factors such as work experience and education. Naturally, a wage regression can be further augmented with additional covariates.

Traditional Mincerian wage regressions are still used in applied work, but the approach has attracted widespread theoretical and econometric criticisms. The assumptions of a static environment and perfect expectations by individuals with respect to future earnings (i.e., the internal rate of return on education) are not plausible when one makes schooling decisions (e.g., Heckman, Lochner and Todd, 2006). Endogenous educational decisions also complicate the identification strategy in attempts to estimate the returns to education. As individuals self-select into different levels of education, it is possible that the observed positive correlation between education and wages is driven by unobserved factors such as motivation and innate ability (e.g., Card, 1999). The problem of self-selection also applies to other research question using observational data. It is not surprising that research efforts have been made to find valid identification strategies (e.g., Murnane and Willet, 2011).

2.2 Student's decision to work

There are many reasons that students might decide to work during their studies. From the perspective of the traditional human capital model (e.g., Becker, 1962), an individual's schooling decisions are well-informed investment decisions, where expected lifetime returns to education are compared to the costs of acquiring more education. An individual will invest time and resources in education until the marginal returns to education equal the marginal costs. The decision to spend time studying has opportunity costs because individuals could also work full-time instead of studying. Students can recoup part of these costs by working while studying. The traditional model becomes more realistic when the possibility of incomplete information is recognised (e.g., Manski 1989; Altonji, 1993; Keane and Wolpin, 1997; Cameron and Heckman, 1998; Stinebrickner and T. Stinebrickner, 2012). Students might be uncertain of their own abilities, and it is difficult to assess the expected lifetime returns to education. Students benefit from employment opportunities because it enables them to update their beliefs on their own productivity in the labour market, which further improves their decisions on schooling and work.

Student employment itself might also have a positive effect on future labour market outcomes, as it could enhance one's productivity similarly to education. Student employment could be an important factor for an individual's career development. The literature suggests that work experience accumulated during the studies (specific skills and knowledge obtained in the labour market) is an important element of human capital that further affects labour market outcomes. Empirical evidence indicates that internship periods have a significant effect on individual earnings after graduation from university because training programs smooth the transition to the labour market (e.g., Saniter and Siedler, 2014). Early employment opportunities might enhance early career development, which has a significant effect on later outcomes (e.g., Topel and Ward, 1992; Rayan, 2001; Cockx and Picchio, 2012). However, students often work in low-skilled jobs (so called "Mac jobs") that are not related to their field of education. Empirical studies on how beneficial student employment is for students in HE remain relatively limited and inconclusive. In the US, Light (2001) finds that those who accumulate 2 years of work experience before graduation from college have 10 per cent higher wages after graduation, while Molitor and Leigh (2005) show that in-school work experience is more important for two-year college students than for four-year students. Hotz et al. (2002) also find a positive correlation between student employment (during high school and college) and subsequent wages, but the heterogeneity analysis reveals that the relationship is primarily explained by unobserved background factors. Structural modelling of student decisions in Denmark shows that a moderate amount of work during university enhances future labour outcomes but that excessive student employment can be detrimental to academic achievement (Joensen, 2009). In Finland, Häkkinen (2006) finds that student employment has a positive effect on earnings immediately after graduation, but when accounting for the effect on study duration, the former effect is insignificant.

It is also possible that students are credit constrained (for a review, see Lochner and Monge-Naranjo, 2012). Students might be forced to work to finance their studies in countries where HE is not free and the availability of student support is limited. It is also possible that students finance higher levels of consumption through work. The empirical literature indicates that lowering the costs of HE can improve university access and completion rates (e.g., Dynarski and Scott-Clayton, 2013). Dynarski (2003) finds that aid eligibility has a positive effect on enrolment and completion in the US, where eligibility rules were changed during the 1980s. An interstate comparison also reveals that merit-based aid programs have improved university completion (Dynarski, 2008). There is also recent evidence that student performance can be improved by increasing student aid in countries where HE is supported by the public sector. Glocker (2011) finds that German students who receive student aid graduate faster. In Norway, Gunnes, Kirkebøen and Rønning (2013) show that increased financial aid positively affects on-time graduation rates, while Arend (2008) finds that aid has a negative effect on dropout rates in Denmark.

Student employment can also be harmful for individuals and society. There are at least four possible channels for the negative associations between student employment and labour market outcomes (see Figure 1). First, earlier research shows that working while studying is associated with longer study durations (e.g., Häkkinen and Uusitalo, 2003; Triventi, 2014; Darolia, 2014). Postponed graduation has significant costs for individuals because earnings increase rapidly after graduation but not before. Holmlund et al. (2008) estimate that one additional gap year before entering university in Sweden is associated with 2 per cent lower earnings at age 35. The cumulative loss from two years of postponed entry amounts to 40–50 per cent of annual earnings at age 40. As a result, postponed graduation could have high social costs. An evaluation report using Swedish data on university graduates indicates that the social costs of postponed graduation might even be 50 per cent higher than the private costs (Uusitalo, 2011).

Second, student employment might impact "in-school" human capital accumulation, but the empirical evidence on this issue remains inconclusive. Kalenkoski and Pabilonia (2010) find that employment during the first semester has a negative impact on grade performance, while Darolia (2014) finds no significant effect of part- or full-time work on grades. Ehrenberg and Shermen (1989), R. Stinebrickner and T. Stinebrickner (2003) and Body et al. (2014) find that only extensive student employment has an impact on study performance, while a small amount of work might even improve student performance. One might also suspect that the time spent working could constrain student's extracurricular activities and social interaction, which could enhance subsequent labour market success. Third, student employment might affect labour mobility. While the relationship between education and labour mobility is examined extensively in the literature (see, e.g., Greenwood, 1997, Chapter 12; Dustmann and Glitz, 2011), no study to date addresses how working while studying affects the propensity to migrate. The concern is that student employment might decrease migration after graduation, thereby worsening the match between jobs and employees. Fourth, student employment might increase the dropout propensity. As students have incomplete information on future returns on education and work, short-run employment opportunities during their studies might induce students to drop out. The literature indicates that students who work full time during their university studies are less likely to complete their studies than students who work part time or not at all (e.g., Hovdhaugen, 2014).

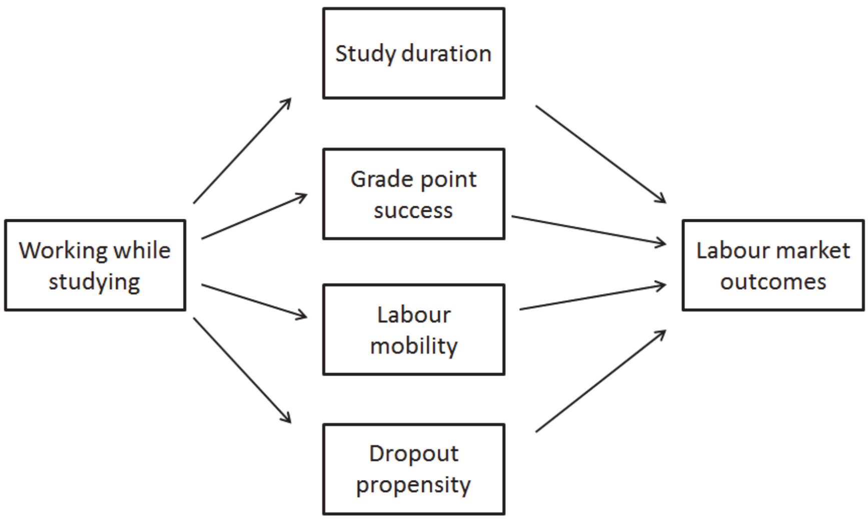

FIGURE 1 Possible adverse effects of student employment. Note: There may be interactions between factors (e.g., grade success and dropout propensity). For simplicity, these are not included in the figure.

2.3 Financing higher education

There are no tuition fees in Finland. The main argument for free HE is that individuals with different socioeconomic backgrounds should have an equal opportunity to access HE (e.g., Asplund, Adbelkarim and Skalli, 2008). International empirical evidence supports the observation that decreasing costs of university education increases the attendance and completion of schooling in low socioeconomic groups (see, e.g., Dynarski, 2002; Deming and Dynarski, 2009; Dynarski and Scott-Clayton, 2013). However, recent empirical studies indicate that poor performance in secondary school might explain lower HE participation rates better than barriers at the point of entry (e.g., Chowdry et al., 2013; Denny, 2014). Publicly funded HE might also be considered a potential mechanism to equalise the income distribution, but this could lead to significant inefficiencies (see, e.g., Hanushek, Leung and Yilmaz, 2003). [1]

There are at least three arguments questioning whether HE should be solely financed by public funds. First, the empirical literature clearly demonstrates that HE has large returns for individuals (e.g., Koerselman & Uusitalo, 2014). Thus, individuals who receive a free HE are also those who benefit from it significantly over their lifecycle. Second, HE is financed via taxes on labour income. Thus, low-skilled labour that never enters HE (and high earners without HE) bears part of the costs related to HE (e.g., Eckwert and Zilcha, 2012). In effect, HE, which is financed by general taxation, will redistribute funds from the poor to the wealthy if there is too little progressivity in the tax system (e.g., Garcia-Penalosa and Wälde, 2000). Third, there is a question of the efficient use of public funds for HE. The use of public funds for HE is inefficient if free HE attracts low-quality students (e.g., Viaene and Zilcha, 2013). Additionally, if education imposed costs on individuals, it would create incentives for students to graduate on time (e.g., Johnstone, 2004). A study by Brunello and Winter-Ebner (2003) indicates that excess time-to-graduation is higher in European countries where tertiary education is largely subsidised by the public sector.

In many of the member countries of the European Union, HE is at least partly financed by tuition fees (see Table 1). While tuition fees are relatively modest in most of these countries, the differences compared to free Nordic-style system are significant. For example, in Estonia and Austria, tuition fees are tied to student performance. Estonian students are exempt from tuition if they achieve a minimum of 30 student points (ECTS) per semester. Austrian students need to pay tuition fee only when the duration of their studies exceeds the defined maximum time by one year. In the UK (except Scotland) annual tuition fees are normally over 4 000 euros, but depending on the field of education, the fee can reach over 10 000 euros. Tuition fees in the UK can be financed through state-guaranteed loans that are repaid on a monthly basis after the individual's annual income rises above a threshold level. [2] These so-called "income-

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[1] See Jacobs and Van der Ploeg (2006) on problematic issues related to education subsidies, equity and optimal taxation.

[2] In the UK and Ireland, the level of tuition fees has changed several times since they were introduced in the 1990s. This created an opportunity to conduct natural exper-

contingent loans" transfer part of the costs of education to students such that the loan repayments are conditional on future labour market success (e.g., Barr, 2004). Recent empirical evidence suggests that income-contingent loans might be a preferable approach to obtaining greater resources for HE in Europe (e.g. e.g., Chapman and Ryan, 2005; Vandenberghe and Debande, 2007; Courtioux, 2012).

TABLE 1 EU countries that collect tuition fees from their citizens for the 2013/2014 academic year (bachelor's degrees only)

Country	Tuition fee (euro)	Comments
Austria	363	Only if one exceeds the maximum study duration by one year.
Belgium	0 – 837	Exceptions and regional differences (70 % of students pay a maximum fee)
Bulgaria	59 – 741	Almost all pay (exclusions, i.e., orphans)
Germany	200 – 1000	Students can be exempt from fees based on need or merit.
Estonia	0 – 7200	No fee if the student achieves a minimum of 30 ECTS per semester.
Ireland	2500 – 6000	Student contribution of 2500 euro (all pay)
Spain	713 – 2011	Regional differences and exemptions
France	183 – 2000	Exceptions (65 % of students pay fees)
Croatia	665 – 1329	1st year students do not pay fees
Italy	0 – 1300	Exceptions (88 % of students pay fees)
Latvia	903 – 4876	Exceptions (55 % of students pay fees)
Lithuania	625 – 5260	Exceptions (48 % of students pay fees)
Hungary	795 – 5532	Exceptions (43 % of students pay fees)
Portugal	631 – 1066	All students pay
Romania	525 – 2819	Exceptions (45 % of students pay fees)
Slovenia	1210 – 2800	Exceptions (20 % of students pay fees)
Slovakia	10 – 1960	Based on study duration
UK	4409 - 11099	Regional differences; all pay except in Scotland (income-contingent loans)

Note: Students in Sweden, Denmark, Finland, the Czech Republic and Poland pay only modest registration fees. Students in Malta, Cyprus and Greece pay tuition fees only for master's level studies. Information was not available for the Netherlands and Luxemburg. Source: European Commission, 2013.

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iments on the effect of tuition fees on participation decisions. Nevertheless, conclusive empirical evidence on the effect of tuition fees on participation rates is rare. Bradley and Migali (2013) show that the tuition fee reform of 2006 raised the dropout probability by one per cent. In Germany, Bruckmeir and Wigger (2014) and Baier and Helbig (2014) find no evidence that tuition fees affect the likelihood of enrolling in university.

In addition to an absence of tuition fees, countries such as Finland, Sweden and Denmark have direct support schemes for HE students (see Table 2). In each of these countries, the maximum amount of support is bounded by annual income. In Finland, the largest monthly expenditures for HE students are rent payments. Living expenditures represent approximately 50 per cent of all expenditures for a student living alone (Ministry of Education and Culture, 2010). The direct allowance payments and employment opportunities during studies are important, as only 42 per cent of university and polytechnic graduates used student loans to finance their studies in 2012 (OSF, 2013a). According a survey conducted by the Ministry of Education, the major reason that students do not take out loans is that they wish to avoid indebtedness as a matter of principle (Viuhko, 2006).

TABLE 2 Higher education and student support systems in selected countries in 2012/2013

Country	Monthly student grants and loans*	Income limit (2014)
Finland	- Study grant (max. 298 euro) - Housing benefits (80 % of the total rent, max. 171 euro) - Student loan (300 euro) - For 64 months in total, but there are differences by field of education.	660 euro per support month and 1 970 per aid-free month.
Sweden	- Study grant 1/3 (max. 350 euro) - Student loan 2/3 (max. 770 euro) - Further student loans possible - Typically for 12 semesters in total.	For six aid months, 7550 euro if a full-time student and 13 220 euros if a half-time student.
Denmark	- Study grant (max. 770 euro) - Student loan (max. 395 euro) - For planned study duration (field specific) plus 12 months.	8630 euro for six aid months.

* Monthly grants and maximum duration also often depend on other factors such as whether an individual lives with his/her parents, how much the person earns during studies or whether the student has children or is disabled. Note: conversion rates as of 1.6.2014. Sources: European Commission, 2013; Student support system in Finland [accessed 1.6.2014]. (KELA) Available: <URL: http://www.kela.fi>; Student support system in Sweden [accessed 1.6.2014]. Centrala studiestödsnämnden (SCN), available: <URL: http://www.csn.se>; Student support system in Denmark [Referred accessed 1.6.2014]. Statens Uddannelsesstøtte (SU), available: <URL: http://www.su.dk.

The Finnish student aid system has been reformed multiple times since the early 1990s. Nevertheless, the real value of student benefits has remained relatively fixed since 1992, when the student aid system was overhauled. Before the reform, student aid was more based on loans and direct grant payments were low (for HE, the maximum grant was 108 euros per month for studies lasting seven years).[3] After the reform, direct grant payments were increased and students were entitled to separate housing benefits. The share of student loans in available student aid initially declined but has since increased. Without adjusting for inflation, the maximum amount of student aid (grant and housing benefits) was 423 euros in 1992 and 502 euros in 2014. Over the same period, the monthly student loan levels doubled from 202 euros to 400 euros. If adjusted for inflation, the sum of student aid (including loans) declined by 10 per cent between 1992 and 2013, but they increased by 4 per cent if one accounts for the most recent changes in 2014. For comparison, the average rent per square metre (EUR/m2/month) increased by approximately 67 per cent between 1992 and 2006 (OSF, 2013c). The consumer price index shows a 42 per cent increase in consumer prices between 1992 and 2013 (OSF, 2014). Thus, while there are no direct costs (i.e., tuition fees) related to HE in Finland, one might argue that the costs of HE have been gradually transferred to the student.

2.4 Higher education and student employment in Finland

The HE system in Finland consists of polytechnic schools, representing approximately 45 per cent of HE students, and universities, representing approximately 55 per cent of total students in 2011 (OSF, 2012c). Polytechnic schools were formed gradually after 1991 by combining existing vocational schools and colleges to create a more uniform system (for further details, see Lampinen, 2000). Polytechnics offer higher vocational education at the bachelors' level with a focus on vocational skills (e.g., nurses, engineers, business degrees). Graduation from the polytechnic schools is designed to require 3 to 4 years. Universities are more focused on academic skills, and after completing a master's degree, students can continue on to Ph.D. studies. The designed duration of master's studies in the university system is 5 to 7 years, depending on the field of study (for further details, see Ministry of Education (2005)). In total, the Finnish HE network consists of 15 university and 26 polytechnic institutions (including the Police academy and Åland polytechnic). These institutions are spread across over ten city regions.

So how much do students work during HE in Finland? Unfortunately, few statistics are available to address this question. Statistics Finland produces statistics on year-end employment for students that are available beginning in 2000 (see Table 3). Approximately 57–58 per cent of university and polytechnic students are employed during the last week of the year. While student employment among university students has been relatively stable during this period, work among polytechnic students has increased 7.5 per cent. It also notable that employment among vocational students has increased in this period. Unfortunately, the information from the last week of the year does not necessarily indicate how intensively students work during the academic year.

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[3] Source: Finlex online database (FINLEX) [accessed 1.10.2014]. Available: <URL: http://www.finlex.fi/fi/laki/alkup/1991/19910790> (in Finnish).

TABLE 3 The share of employed students aged (at least 18 years old) of all students in the period 2000–2012 by educational sector

Educational sector	2000	2002	2004	2006	2008	2010	2012
High school	31.3	30.8	31	29.8	32.3	29.1	28.4
Second stage vocational	42.3	43.7	54.4	58.6	62	56.3	53.2
Polytechnic	49.5	50.2	52.7	55.8	59.5	58.9	57.1
University	57.6	55.9	56.9	59.1	61.4	61.2	58.4
Total	48.2	48.2	52.5	55.6	58.7	56.1	53.7

Source: OSF (2008, 2012d). Note: student employment is observed in the last week of the calendar year.

One way to study how intensively students work during their studies is to examine annual wage earnings from the study period. Figure 2 plots average earnings (measured two years before graduation) by university (1992–) and polytechnic (1995–) graduate cohorts (deflated using the 2000 consumer price index). Figure 2 indicates that earnings have increased significantly since the end of the deep recession in 1993. Gradate cohorts earned on average 4 500 euros two years prior to graduation in 1995. In 2006, university (polytechnic) graduate earnings increased to 9200 euros (8000 euros).

Figure 2 also demonstrates an income limit (dashed horizontal line) beginning in 1998 for a student who received study benefits for a nine-month period during the calendar year (students do not automatically receive student benefits for the summer months). After 1997, students were allowed to earn 505 euros per support month and 1515 euros per month when not receiving benefits. Before 1998, the earnings limit was calculated using the average earnings for study months in the spring and fall semesters instead of yearly income. From 1995 to 1997, a student could earn 303 euros per support month (or 2 727 euros for nine months). In addition, a student could have earnings from summer months. While labour markets improved after the end of the 1993 recession, average student earnings also increased to near the income level specified in student financial aid policy for the nine study months.

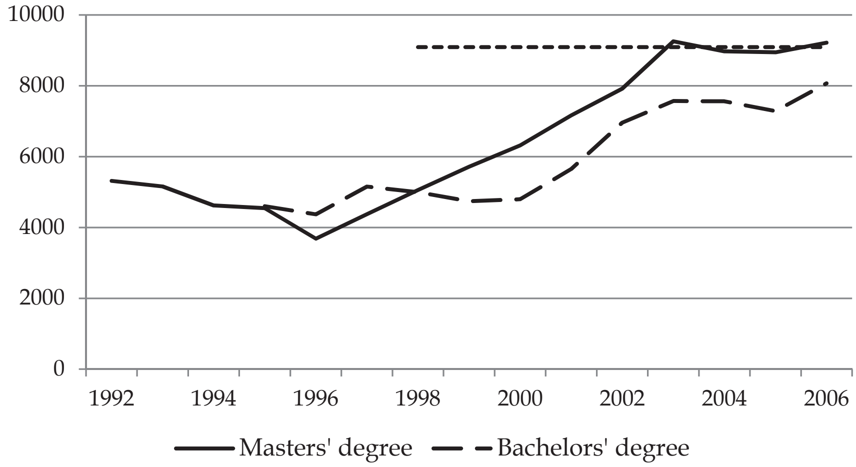

FIGURE 2 Annual wage earnings (in euros) by graduate cohort two years before the graduation year (Earnings are deflated using the 2000 consumer price index). Source: own calculations based on a random sample from Statistics Finland. Note: The sample includes only high school graduates who were under 36 years old when graduating from universities or polytechnic institutes. Note: The dashed horizontal line is an income limit for students who study for nine months in a given year.

3 Overview of the thesis

3.1 Research questions and aims

This thesis builds on four research articles organised into separate chapters. Chapters 2 and 3 study the relationship between working while studying and earnings and labour mobility, respectively. Chapter 4 focuses on average labour market outcomes after an individual drops out of university. These three chapters study HE and labour market outcomes at the individual level. The final chapter focuses on outcomes at the firm level; specifically, Chapter 5 presents an evaluation of the effect of R&D subsidies on labour productivity in low- and high-skill firms.

By chapter, the research questions of this thesis are:

- Chapter 2: Does working while studying affect earnings after graduation from HE? How significant are the returns are compared to the possible costs of postponed graduation?

- Chapter 3: Does working while studying relate to labour mobility after graduation? Are there any differences by level of education or region?

- Chapter 4: Is the decision to drop out from university related to positive labour market outcomes after the dropout decision? On average, are there large differences relative to the outcomes of university students and graduates?

- Chapter 5: Do public R&D subsidies affect labour productivity over the five-year period after a subsidy is granted? Are there differences between low- and high-skill firms?

Chapter 2 investigates whether working while studying is related to higher earnings after graduation from a polytechnic institute or university in Finland. While there is an extensive empirical literature on the returns to education per completed degree or by years of education, less attention has been directed to student time use (i.e., work) and how it relates to later outcomes. The aim of the research is to determine the average extent of the returns that individuals receive from working while studying over the two-year period after graduation. By comparing possible private returns to the cost of postponed graduation, one might assess how beneficial it is to work before graduation.

Chapter 3 studies the relationship between student employment and labour mobility over the three-year period after graduation from vocational school, a polytechnic institute or university. This research contributes to the economic literature on education and migration, which has not yet considered the role of students working when studying the determinants of migration. The aim of the research is to study how student employment relates to migration propensity and whether the effect of work is heterogeneous across levels of education, region and prior migration behaviour. Understanding how student employment might affect migration propensity could reveal whether working while studying has any indirect effects on later labour market outcomes.

Chapter 4 focuses on university students who decide to leave university before graduation. Individuals with different innate capabilities have substantial incentives to enrol in university in Finland because university education is free and the returns to HE are relatively high. However, little is known about university dropouts and how they succeed in the labour market after deciding to drop out. Thus far, the literature has primarily studied the factors leading to the dropout decision. The aim of the research is study how the dropout decision relates to earnings, employment, unemployment and self-employment over the five-year period after the dropout decision. The possible returns are evaluated relative to university students and graduates.

Chapter 5 analyses whether public R&D subsidies affect labour productivity at the firm level. The specific focus of this study is to compare low- and high-skill firms, as subsidies might have a more substantial effect on productivity in firms that have higher levels of human capital. Additional examinations focus on employment, firm survival and whether the size of the subsidy matters, which is rarely explored in the existing literature. This study also evaluates whether R&D subsidies enhance firms' human capital intensity. The aim of the study is to provide empirical evidence for policy makers on the effectiveness of R&D subsidies.

3.2 Data, methods and limitations

The analysis in this thesis exploits register-based datasets at the individual and firm level. The empirical studies in Chapters 2, 3 and 4 are based on a seven-per cent random sample from Finland's 2001 population census (a total of 363 643 individuals). Using individual-specific identification codes, Statistics Finland connected this sample to variables from the Longitudinal Census Files and the Longitudinal Employment Statistics File from 1970 to 2006. Data from the Social Insurance Institution File on student benefits from 1997 to 2000 were also combined with the sample. Thus, the panel data allow one to follow the same individuals across multiple periods over the lifecycle. The second strength of this data set is that it includes rich background variables not only on sample individuals but also on parents, spouses and regional labour markets. The weakness of this data set is that not all variables are available for the full period. Thus, the empirical analysis in the research articles contained in this thesis utilise different sub-samples based on the research question at hand. The final chapter uses data from the Business Register database administered by Statistics Finland. The Business Register database includes nearly all firms in Finland between 1988 and 2012. The research in Chapter 5 focuses on a sub-sample of SMEs between 1999 and 2012, as public R&D subsidies were available only during this period. With the help of firm identification codes provided by Statistics

Finland, a number of background variables were combined with data on individual firms from different sources (e.g., from the Financial Statement database and the Patent database).

The sub-sample used in the empirical analysis in Chapter 2 includes a total of 6 768 high school graduates who graduated from a polytechnic institute or university between 1997 and 2005. When evaluating the research's external validity, it is important to note that this sub-sample does not include individuals who dropped out before graduation. In Chapter 3, the sub-sample includes all high school graduates between 1991 and 1996, and both graduates and non-graduates are included in the analysis (for a total of 10 077). In Chapter 4, research data are collected for the period from 1999 to 2002. This sub-sample includes high school graduates who are university students (8,518), graduates (2,372) or who dropped out (670) according to records on public student grant payments and completed degrees. Finally, the firm-level data in Chapter 5 include over 30 000 firm-year observations that are used to construct a control group for subsidised firms (1 221).

A substantial problem affecting empirical research in economics and other social sciences is that one can only observe a single outcome at a given time. For example, if a student drops out of university, the researcher can only observe labour market outcomes following that decision. However, it is impossible to observe what would have happened if the student had chosen otherwise. This problem is called as the "fundamental problem of causal interference" (by Holland, 1986). A random experiment would solve this problem because randomisation creates a group of non-dropouts, who are – except for dropping out – identical to dropouts. Thus, the researcher would not need to observe an individual student in both states, which is impossible, but he/she can use those who do not drop out as a comparison group. Understandably, the social costs related to randomised experiments, e.g., on dropouts, are prohibitive, and it is necessary to adopt other approaches to identification. (Murnane and Willet, 2011)

This thesis employs different econometric approaches to correct the results for observed and unobserved individual heterogeneity. As all of articles in this thesis use observational data, it is necessary to recognise that the results are conditional on the quality of the covariates available for the analysis.

Chapter 2 closely follows earlier work by Häkkinen (2006). The aim is to estimate the effect of work (measured in years of work during studies) on annual earnings after graduation from university or a polytechnic institute. Even if the amount of work would be randomly distributed across students, there are several issues to consider. First, as we observe only those who graduate from HE, the low external validity does not allow us to apply the results to the total student population. Second, we observe earnings only for those graduates who are employed after graduation. Fortunately, the share of zero earners following graduation from HE is low (less than 5 per cent). We also evaluate possible bias resulting from this using Tobit estimation. Third, there is the question of how the dependent and independent variables are measured. Annual earnings were the only measure available for the analysis. It should be noted that students exhibit different working habits across the months of the year, which we cannot observe. Our independent variable of interest is also somewhat problematic. We measure work in years during studies by summing annual working months and dividing them by 12 (excluding the enrolment and graduation years). It is possible that this measurement of work is an overestimation of the true amount of work because student employment periods are brief and irregular, which could create inaccurate official statistics on months of employment. Finally, the timing of the measurement of the control variables is crucial. Covariates used in the analysis should not be outcomes. To avoid the so-called problem of "bad controls", the control variables have to be measured before they become possible outcomes (e.g., Angrist and Pischke, 2009).

The empirical methods employed in Chapter 2 strive to control for student background factors such that the only difference between the groups who work different amounts is the amount of work they perform. In our basic ordinary least squares (OLS) estimations, this assumption is called "selection on observables", i.e., the expected error term is zero when conditioned on the covariates used in the analysis: [$E(\varepsilon|X) = 0$]. Nevertheless, it is plausible that there are unobserved factors (e.g., individual motivation) that are correlated with the amount of student work and earnings after graduation (e.g., Hotz et al., 2002). This would bias our results. Our covariates could also be measured inaccurately. Following Häkkinen (2006), we use an instrumental variables (IV) approach to correct for unobserved heterogeneity resulting from these problems. Our IV approach is based on the assumption that changes in intra-regional unemployment rates and intra-regional service sector size are correlated with the amount of work students perform during their studies (assumption 1) but have no direct impact on earnings after graduation (assumption 2). We consider the validity of these assumptions in detail in Chapter 2.

Chapter 3 studies the relationship between student employment and migration propensity. The sub-sample includes vocational school, polytechnic and university graduates from 1991 to 2004. The dependent variable is a binary variable indicating migration from the study region over the three-year period after graduation. We focus on conditional migration propensities to evaluate whether work experience is associated with decreased or increased migration propensities within different groups. Our variable of interest (work experience) is defined using annual earnings instead of months of employment. The identification strategy is based on the assumption that the control variables employed successfully minimise the selection bias, an assumption that we cannot verify. Thus, these results cannot be interpreted as a causal relationship but instead must be regarded as a conditional correlation between work experience and subsequent migration decisions.

Chapter 4 studies how the dropout decision is related to different labour market outcomes. This chapter uses matching methods to identify a similar comparison group member for each individual who decides to drop out. We compare dropouts to similar students and graduates. The matching approach has multiple advantages over traditional regressions (Imbens and Wooldridge, 2009). Nevertheless, we cannot exclude the possibility that there are some unobserved factors that might bias the results.

In Chapter 5, we study whether public R&D subsidies affect firm productivity. The identification strategy is based on the common trend assumption. After matching on observed covariates, the assumption is that the trends of subsidised firms develop in parallel to those of similar firms in the unsubsidised firm group. We evaluate this assumption descriptively and by testing mean differences of firm groups before and after matching. Chapter 5 also offers multiple robustness checks that further strengthen the empirical approach.

3.3 Main findings and suggestions for future research

This section summarises the main findings of this thesis (see also Table 4). Before proceeding, it is necessary to reiterate that the internal and external validity of the estimation results presented in the various chapters are conditional on the samples, control variables and methods employed (see the limitations above). When interpreting the results, one should bear in mind that the true cause-and-effect relationship can only be identified through a random experiment or an appropriate quasi-experimental setting. At the time the research was conducted, experimental settings appropriate for the research questions of interest were not available.

Chapter 2 investigates the relationship between working while studying and earnings after graduation from university or a polytechnic institute in Finland. The OLS estimates indicate that one year of work before graduation is, on average, associated with 6–8 per cent (11–13 per cent) higher annual earnings one and two years after graduation from university (polytechnic). IV estimations confirm the positive effect on earnings for polytechnic graduates one year after graduation, but no significant effect is found two years after graduation. No significant effect is found for university graduates, but the size of the coefficient is similar to the OLS results one year after graduation. Thus, IV estimations suggest that OLS estimates are biased upwards, especially for university students, which might be because university students work in jobs not related to their field of education. Additional analysis supports the conclusion that the returns from work related to a graduate's field of education are higher.

The results in Chapter 2 suggest that working while studying has, on average, a low and short-term return for individuals. Häkkinen (2006) also finds a short-term earnings effect for university graduates. This might indicate that working while studying eases the transition from studies to the labour market. However, if working while studying leads to postponed graduation (as previous literature would suggest), then the estimated positive effect of work might be biased upwards. Future research should investigate the possible effect of student employment on lifetime earnings. Second, the quality of student- job matches should be studied in greater detail. A continuous career that begins during one's studies might be more beneficial than working for firms on a temporary basis. Third, research literature is still scarce on how working while studying affects subject student achievements.

Chapter 3 focuses on the relationship between working while studying and graduates' migration propensity after graduating from vocational school, polytechnic or university. The results suggest that working while studying is negatively related to migration propensity even after controlling for many background variables. While those who work full time have, on average, a migration propensity that is 7 percentage points lower than those who do not, the relationship is stronger at higher levels of education (- 12 per cent for university graduates). There are considerable differences with respect to the region in which a student pursues studies. Graduates who work full time during their studies are considerably more likely to remain in the region where they studied than those who work less, but the negative relationship stronger for those who live outside the Helsinki region. A graduate's migration history also has an effect. Particularly outside the Helsinki region, working while studying attaches movers (those who moved to study region) to the study region.

Future research on graduate migration should focus on student mobility between firms, industries and occupations before graduation in greater detail. The results of Chapter 3 suggest that working while studying might have an effect on the graduate-job matches, as there is a significant relationship between working while studying and migration propensity. To understand migration decisions after graduation, it is necessary to study individual decisions during education. Future analyses should utilise institutional changes or other possible exogenous variation to achieve identification, which was not possible in this study.

Chapter 4 studies how the decision to drop out of university is related to labour market outcomes (employment, earnings, unemployment and self-employment propensity) relative to graduates and to the enrolled student population. The matching results show that individuals gain only a short-term earnings premium relative to the student population. Four years after the dropout year, university dropouts work significantly less and earn 12,000 euro less than graduates per year. Dropouts are also three per cent more likely to be self-employed.

The results in Chapter 4 indicate that individuals who drop out of university earn significantly less (four years later) than individual who graduate or continue to study. There are at least two recommendations for future research in this regard. First, research should study (in a quasi-experimental setting) the long-term effects of the dropout decision. The timing of the dropout decision might also be important. Second, it would be interesting to estimate the possible indirect costs for society related to dropping out. The number of students accepted in each semester is fixed in Finland, and the incentives to apply to university are high because the HE is free and offers significant benefits after successful enrolment.

Chapter 5 examines the effect of R&D subsidies on labour productivity in Finnish SMEs. Conditional difference-in-differences estimates indicate that subsides have no positive effect on labour productivity over the five-year period after a subsidy decision. Instead, the estimation results suggest that subsidies have a negative short-term effect on productivity. However, subsidies have a significant effect on employment, firm-survival and human capital intensity.

In future research, it would be interesting to identify different channels through which R&D subsidies affect firm productivity. Chapter 5 finds that R&D subsidies enhance employment growth but not productivity. It would be interesting to study the source of this employment growth. For example, if subsidised firms hire new employees from similar competing firms, then it is possible that the total employment effect of R&D subsides is overestimated in typical regression or matching models. A more detailed analysis of new employees might also provide new evidence on possible externalities of such subsidies.

TABLE 4 Summary of the studies and results in different chapters

Chapter	Question(s)	Data and Methods	Results
Chap2	The effect of working while studying on earnings after graduation from university or polytechnic.	• 7 % random sample of high school graduates. • OLS and IV methods	• Positive earnings effect one year after graduation (only polytechnic graduates) • Indications that work experience from the student's field of education might have larger returns
Chap3	The relationship between working while studying and mobility from the study region.	• 7 % sample of high school graduates 1990-1996. • Probit –model	• Working while studying is negatively related to migration propensity • The negative relationship is more significant outside the Helsinki region
Chap4	The relationship between the decision to drop out of university and labour market outcomes.	• 7 % sample of high school graduates who dropped out of university between 1999 and 2002. • Matching method.	• A negative relationship between the decision to drop out and earnings (and employment) four years after the dropout decision. • Positive relationship with self-employment
Chap5	The effect of R&D subsidies on firm-level labour productivity, value added, employment and human capital intensity.	• Private Finnish SMEs 2000-2012. • Matching and conditional difference-in-differences.	• No positive effect on labour productivity over a five-year period • Positive effect on employment, human capital intensity and firm survival.

3.4 Concluding remarks

The public sector allocates significant resources to HE and private R&D in Finland. Scarce public resources should be used as efficiently as possible, but there is no clear empirical evidence on the success of various support schemes. Different public policies should be constantly evaluated, thereby allowing policy makers to reform the system if necessary. Evidence-based reforms require additional investments in empirical research. The results of this thesis suggest that research should further evaluate how beneficial working while studying is for individuals and society and whether R&D subsidies have any long-term effect on productivity.

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CHAPTER 2
DOES IT PAY TO WORK DURING HIGHER EDUCATION? *

Abstract
This study investigates the returns on work experience acquired during higher education using random sample from Finnish register based data. Sample includes over 6 700 graduates from universities and polytechnic schools with unique set of background variables from 1987 to 2006. Analysis focuses on working while studying and earnings over two year period after graduation. The regression results show that early work experience is related to higher earnings especially right after graduation year. This indicates that working while studying eases the school-to-work transition which results higher annual earnings in the first labour market years. Instrumental variable estimations (IV) confirm causal effect on earnings for polytechnic graduates immediately after graduation, but no significant causal effect is found for university students. However, additional results indicate that there are some returns for university students to having a job before graduation if the job is related to the student's field of education. This might also explain why IV estimations are more significant for polytechnic students as work from the own field of education is more common for polytechnic than university students.

Keywords: Work and study; labour supply; school–work transition, higher education; youth.

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1 Introduction

Standard human capital theory states (Mincer, 1958, 1962; Schultz, 1960, 1961; Becker, 1964) that education is an investment that increases labour productivity and lifetime earnings. The age at which education is capitalised on the labour market is central because that age determines to what degree the costs of education will be recouped. Postponing graduation lowers the number of possible working years and lifetime earnings (Ben-Porath, 1967). Thus, factors that contribute to late graduation, such as working while studying, lower the returns on education unless these factors increase productivity and lifetime earnings. If there is no positive effect of in-school work on future earnings or the effect is small, it might be socially beneficial to limit working while studying more forcefully. This might create incentives for students to study full-time and graduate quickly from higher education.

Studies have sought to identify the effect of working while studying on individual earnings after graduation (e.g., Light, 1995, 2001; Ruhm, 1997; Monks, 1997; Hotz et al. 2002; Häkkinen, 2006). These studies have used samples from different countries and with different levels of education, so the results are not comparable. Nonetheless, normal ordinary least square estimates (OLS) often show that working while studying has a significant and positive effect on future earnings. However, after controlling for selection and individual-specific unobserved heterogeneity, the results are not significant or the estimated effect is small. Thus, it is questionable whether working while studying is beneficial at all, especially if one considers the possible adverse effects work might have on education (e.g. Curtis and Shani, 2002; Stinebrickner and Stinebrickner, 2003; Broadbridge and Swanson, 2005; Kalenkoski and Pabilonia, 2010; Callender, 2010) and how work might decrease lifetime earnings by raising the age at graduation.

This paper focuses to assess whether working while studying affects earnings after graduation from higher education. Working while studying may affect student achievements and lifetime earnings, but this study is interested in labour market outcomes immediately after graduation, as the effect should be most visible at that time. If there is a clear, positive effect of work on labour market outcomes after graduation, it should lead individuals to further success in the labour market. This analysis has a significant policy importance, as policy makers are considering options to speed up graduation from higher education.

This paper contributes to existing literature on working while studying in several ways. Used dataset is larger than in the earlier studies on in-school work experience. It includes students from universities and from universities of applied science (polytechnic schools). As the entire higher education sector is studied the results from different levels of education are more comparable than the earlier results. We also focus on student employment in more detail than the previous studies. Different types of work experience are studied to obtain a more comprehensive picture of what type of work experience matters the most.

Polytechnic schools are well connected to the labour markets because training programs and mutual student-firm projects are common throughout the duration of studies. Early work experience may have a larger positive effect for polytechnic students, although relationship with future earnings is less clear for the university students, whose studies are more academic. Finally, this paper acknowledges the selection issues affecting estimations. It is obvious that it is not random who choose to work during studies.

The results show that young polytechnic students benefit substantially from working while studying immediately after the graduation. However, the results for university students differ from the earlier study by Häkkinen (2006) in two ways. When the regional size of the service sector is used as a source of exogenous variation, the instrumental variable (IV) coefficient is remarkably close to the OLS coefficient one year after graduation (approximately seven per cent) but not significant. Secondly, the IV coefficients are close to the OLS coefficients only if the time-to-degree variable is not included in the estimation, whereas Häkkinen (2006) found a high and positive effect that was conditional on the time-to-degree being controlled. Thus our results seem to be more robust. When comparing the results of polytechnic and university graduates, work experience related to the field of education has a more substantial effect on the earnings of university graduates than what the aggregate measurement of the work months might indicate. For polytechnic students, the coefficient for aggregate work experience might be significant because it measures the combined effect of total and relevant work experience in a more precise way.

The remainder of the paper is organised as follows. The next section reviews earlier studies on the subject while the following section gives basic background on the Finnish system of higher education. Data and methodological section introduce the data and evaluation methods used in this study. The results are presented before the last section which concludes this paper.

2 Previous literature

Two aspects of the timing of education are important: the timing of enrolment and the timing of graduation. While graduating during the low points of the business cycles has a persistent negative impact on future earnings (Oreopoulos, Wachter and Heisz, 2006; Kahn, 2010), the timing of work before and during education has an additional significant effect. Light (1995) found that young men who delay their education with a discontinuous schooling pattern earn less than their counterparts who attended school continuously. Monks (1997) studied the timing issue with panel data and a rich set of variables, finding that those who completed their studies at a later age received significantly smaller compensation compared to younger graduates. A more recent paper by Homlund, Liu and Nordström Skans (2008) showed that working before enrolment has no positive relationship with earnings; instead, the relationship is negative and relatively persistent due to postponed graduation. Using Swedish register-based data, Homlund, Liu and Nordström Skans (2008) estimated that one additional gap year is associated with 2 per cent lower earnings at age 35, but this effect disappears at approximately age 40. They found that the negative effect of the gap year on earnings is a result of different returns on experience before and after education. Postponed education results an earnings penalty due the loss of work experience after graduation. Work experience before university is not significant compared to post-university experience, but Homlund, Liu and Nordström Skans (2008) conclude that the timing of education should be given a more explicit role in the studies of the returns on education, especially when work experience is used in the estimations.

Empirical studies on education have started to consider in-school work experience as an important control variable that could distort the schooling estimates if ignored. Light (2001) found that schooling coefficients are 25-44 per cent higher if in-school work experience is removed from the estimation. It is less clear if the selectivity problem is still present in these estimates because estimates of in-school work experience are sensitive to the econometric method. Hotz et al. (2002) found that the effect of working while studying disappeared when individual-specific unobserved heterogeneity was considered. This concern also applies to earlier studies that found a significant and positive relationship between in-school work experience and future earnings (see Ruhm, 1997) for a survey of earlier papers on the relationship of in-school experience and labour market outcomes).

While there is no empirical research on gap years in Finland, there is one recent empirical study with Finnish data concerning employment during university studies. Häkkinen (2006) utilised the instrumental variable (IV) method following Light (2001) to obtain exogenous variation from regional indicators. Häkkinen (2006) used the average regional unemployment rate as an IV for work experience during university enrolment and showed that while IV and OLS estimates show a positive relationship one year after graduation, the IV coefficient for work experience is not significant two or three years after graduation. The IV estimates showed that a one-year increase in work experience results in 17.9 per cent higher earnings one year after graduation from university, but the impact is not significant after that. Häkkinen (2006) concluded that the returns on in-school work experience are relatively insignificant compared to the social costs.

Finally, earlier studies have shown that there is good reason to be interested in youth labour market success immediately after graduation. Early employment status is a source of state dependency because good and bad labour market experiences accumulate for individuals (e.g., Mroz and Savage, 2006; Doiron and Gørgens, 2008). Job stability in the early phase of a career is a source of substantially higher wages in adult ages (Neumark and Joyce, 2001). As the transition period from school to work has become even more complex and fragmented (e.g. Graaf and van Zenderen, 2013), studies suggest that work experience acquired during the studies eases students to realise their own skills and employability (Stiwne and Junger, 2010) and that youths with earlier work experience have shorter jobless period after graduation (Vanoverberghe et al., 2008).

3 Institutional framework

The higher education system in Finland is a dual model with two institutional actors: polytechnic schools, with approximately 43 per cent of total students, and universities, with approximately 57 per cent of total students (in 2008). Graduating from the polytechnic schools takes 3 to 4 years, and polytechnic education offers higher vocational education at bachelor level (e.g., nurses, engineers, and BBA). Universities are focused on academics, and after completing a degree such as Master of Science, students can continue on to Ph.D. studies. The duration of the basic studies is approximately 5 to 7 years, depending on the field of study. Higher education is highly subsidised by the public sector. Education is nearly free for accepted students, and the students are supported with student allowances and housing benefits, with some fixed constraints concerning their annual earnings and the progress of their studies (Ministry of Education and Culture, 2010).

Despite the structure of higher education and access to free higher education, Finnish students are, on average, two years older when they enter the labour market with a complete degree than are students in other OECD countries (25 years)[1]. There are two main reasons why Finnish graduates enter the labour force at such a late age. First, Finnish students have a long time-to-degree duration, partly because many of them work while studying (Häkkinen, 2006, Häkkinen & Uusitalo, 2003). The second factor that raises the average graduation age is the time gap before starting higher education. The median starting age for higher education in Finland is approximately 21 years (Ministry of Education and Culture, 2010), close to the average graduation age found in some European countries. The transition from high school to higher education can be just a few summer months, but frequently, there is a wider time gap between the stages of education. Competition for free educational slots is fierce, as less than a quarter of individuals who apply to university earn a slot[2]. According to a recent survey (Ministry of Education and Culture, 2010), 63 per cent of university and 83 per cent of polytechnic students have had at least a one year break before starting their studies. With the exception of military service (males), the time is mostly spent working or being inactive (unemployed).

While in school, students can earn approximately 12000 euro per year (approximately 9000 euro before 2008) and still receive full student benefits for a nine-month period, which is less than 500 euro per month, including the student allowance and housing benefit. In addition, there are several student dis-

1 See Education in Glance 2010, Table A3.1 for comprehensive picture.

2 Of students who applied to university in 2008, only approximately 20 percent of applicants received a spot (Kota –database, Ministry of Education and Culture).

counts for public transportation, gyms, shops and cafeterias. The extent to which the increasing graduation age is explained by working while studying is not yet empirically clear, but the student allowance system is structured so that students can have substantial earnings during the year and still receive full student benefits. Traditionally, this advantage has been viewed as a policy to smooth the transition from school to work, as there are no specific national programs designed to help with this transition in Finland.

4 Data

The data for this analysis are from a seven per cent random sample of permanent residents in Finland in 2001. This sample is based on the Longitudinal Census Files and the Longitudinal Employment Statistics File constructed by Statistics Finland. The sample includes approximately 364 000 individuals for whom we have a large amount of information from 1987 to 2006. This study focuses on individuals who graduated from high school after 1986 and completed a degree from a polytechnic school or from a university during the nine-year period 1997-2006 because most of the variables are fully available for this period. Our sample contains 12 898 individuals who graduated from higher education in this period. Individuals who graduated from high school before 1987 were excluded from the study (923), as were those individuals who were over 35 years of age upon graduating from higher education (214). Labour market outcomes (the logarithm of annual earnings) are studied after the graduation, so the empirical analysis is performed on graduated individuals one and two years after the graduation year. Table A1 in the Appendix describes the variables and how the sample size varies for different outcome periods. The sample includes graduate students only. The data are normally collected by the registers after graduation, as those who postpone their graduation might eventually graduate. As such, the results are conditional on receiving a degree from an institute of higher education.

In the literature on the economics of education, an emphasis has been placed on individual ability. In this study, the high school grades for math and language are used to control for ability, which might be correlated with early work experience and have an impact on future earnings. Unfortunately, the grades are not available for all sampled individuals. For some individuals, the data are missing because it is possible to enter higher education without attending high school first. We have, in total, 6 768 individuals in our data three years after their graduation year for whom we have complete information regarding individual, parental and regional characteristics and secondary school grades, which are used to control for ability. Detailed definitions of the variables are in Appendix Table A2.

The annual earnings after the year of graduation were used to measure individual returns. Earnings consist only of earned income and do not include any transfer payments. To perform a logarithmic transformation, individual earnings must be above zero; therefore, some observations are excluded from the analysis. This should not distort the results as the portion of zero earners is low (less than 5 per cent of the sample; see Appendix Table A1), but this possibility is still carefully explored (see section on robustness). This study uses a wide set of control variables to estimating the effect of early work experience on earnings. The data set includes controls for non-time variant variables such as graduation age, gender, language, high school grades for math and language, duration of studies and the region where the educational institution is located. The variables for fathers' and mothers' educational levels are defined as the highest degree in the data set for a graduation year. Variables indicating fathers' and mothers' socioeconomic status were available for the years 1995 and 2000; the socioeconomic status closest to the school enrolment year of the sampled individual was used. Regional controls after graduation were included for each year of observation when used in the robustness checks. Used instrumental variables (more details on the instrumental variables are in the next section) were obtained from the ALTIKA database administrated by Statistics Finland (at the NUTS4 regional level where the educational institution is located).

Measuring work while studying is not a simple task. Students are a distinguishable part of the labour force, but the data collection procedures follow the same rules as those used for the rest of the labour force. When experience is measured for a specific time period, it is the sum of total work months during the calendar year. For our estimation, the sum of work months is scaled to years by dividing the months by 12. As such, summed work months include all the work months during the calendar year, as it is not possible to distinguish work months during the terms from the time worked during the vacation time. Häkkinen (2006) noted earlier that there is potential measurement error in the estimated work months because student employment opportunities vary and months of employment may not be measured in a consistent way. The available data from the registers do not allow us to control for the specific month of enrolment in education or month of graduation. Even if there were a variable for graduation month, it might not reflect the real timing of graduation, as the graduation date marked in the registers is subject to administrative proceedings. Therefore, months of employment from the enrolment year or from the graduation year are not added to in-school work experience.

The crude measurement of work months during higher education does not reveal how relevant the work experience is to one's education. To obtain more insight on the qualitative side of the work experience, a proxy variable indicates whether the work experience is related to a student's field of education. The work relevance variable takes the value of one if the individual has worked in an industry relevant to the field of education during the two-year period before the graduation year; otherwise the variable is zero. The match between the field of education and the industry code can be found in Appendix Table A4.

5 Evaluation methods

The individual decision to work before graduation is not necessarily random, as is not the original decision to acquire specific education or to graduate in designed target time (see, e.g., Card, 1999). If there is a common factor that explains some part of the earnings gain after graduation but is systematically higher for those who work before graduation (e.g., work motivation), the OLS coefficients for work experience from a normal wage regression are biased upwards. The resulting omitted variable bias is similar to the bias found in the schooling coefficients if the work experience during prolonged studies is not considered (e.g., see Light, 2001).

In addition to self-selection to work while studying, also the whole sample can be biased as we observer only those individuals who graduate. If the wage offer during the studies is sufficiently high, individuals might choose to drop out from education. Sample selection problem is similar to self-selection problem and both problems require exogenous variation to deal with (Wooldridge 2002, 567). Also when early work experience is measured imprecisely, our explanatory variable is correlated with the error term, which causes our variable of interest to be biased upwards or downwards.

5.1 Basic equation

To estimate the earnings effect, this paper uses a traditional Mincerian model, which is used extensively in the previous literature on student work experience (e.g., Hotz et al., 2002; Light, 2001; Häkkinen, 2006). Let y_i be individual i's annual earnings after graduation, which is explained by the vector of control covariates X_i and by work experience EXP_i which are measured before graduation. Vector G_i includes the variables measured after the graduation and thus are used with caution when assessing the robustness of the results.

$$ln(y_i) = \beta_i{}'X_i + \delta EXP_i + \alpha_i{}'G_i + \varepsilon_i \quad (1)$$

In equation (1), the vector of coefficients β_i summarises the effect of exogenous control variables on earnings. Variables in X_i include age, age squared, gender, marital status, the graduation year, the school region, the number of gap years before the studies, the field of education, parental educational and socioeconomic background and high school grades on math and language. In basic regressions it is often assumed that the effect of explanatory variable is homogenous, meaning that the work experience has a similar effect for all als $\delta_i = \delta$. $G_i$in equation (1) includes the variables measured simultaneously or after graduation, such as time-to-degree, the region of residence, the type of region regional, the regional service sector size and the regional unemployment rate. Reason why endogenous control variables are used in the estimations relates to important conditions concerning the instrumental variables used in the estimations (see below). Term ε_i is the error term.

The OLS coefficient for work experience is valid if all the necessary covariates are controlled in the estimation (including response heterogeneity), so that the error term is uncorrelated with the explanatory bles $[E(\varepsilon|EXP, X) = 0]$. In the linear form, the model also assumes that the linear functional form is the best description of the described relationship. The estimation does not include a variable for school years, as the model is estimated for university and polytechnic students separately. Thus, years of schooling are in that sense fixed.

5.2 Instrumental variables strategy

Unfortunately, despite a wide set of control variables, all the factors that affect the decision to work may not be controlled and our results may be driven by measurement error or sample selection bias. In the absence of social experiments, other methods are needed for a successful evaluation. The basic instrumental variables (IV) method uses a two stage estimation procedure to correct the endogenous variable. The validity of the method is based on the assumptions concerning the instrumental variable (see, e.g., Bound et al. 1995; Heckman and Urzua, 2010). First, the instrumental variables should correlate strongly with the endogenous variable. If the instrument is not sufficiently strong, the IV method substantially reduces the precision of the estimates, as the standard errors of the IV estimates are determined by the correlation between the instrumental and endogenous variable. Second, it is essential that the instrumental variable affect the outcome only through the instrumented variable. Otherwise, the instrumental variable is correlated with the error term, as it affects the outcome thru other channels. The validity of this assumption cannot be tested if there is only one instrumental variable. In a multiple instrument case, the test would require that at least one instrumental variable is valid (Cameron and Trivedi, 2006, 103). What effect IV method can identify is related to the section of the population that is affected by the IV variable (e.g. a rise in the minimum wage or change in the unemployment rate) so that they change behaviour as a result (e.g. work more). Thus, valid IV coefficient and OLS coefficient are the same only when the individual responses to the changes are homogenous (see e.g. Heckman and Urzua, 2010).

Earlier studies have used regional indicators, such as the unemployment rate, as an instrumental variable (Light, 2001; Neumark, 2002; Häkkinen, 2006). In this study the main instrumental variable uses the regional size of the retail and wholesale sector to generate exogenous variation (Ind_serv_{rti}).[3] This seg-

[3] Regional unemployment rate is also used in this study when two instrumental variables are required and when studying the robustness of the results. When used, instrumental variable ($Unemp_{rti}$) is formed by calculating the mean unemployment rate during school for each region r where individual i studies. The regional unemployment rate is calculated for 20- to 34-year-old individuals, as these cohorts are competing with students for the same jobs.

ment of industry provides employment opportunities for the students, especially on weekends and when the regular employees have their holidays. The variable is calculated in three steps: Firstly, the relative regional size of the sector is calculated using the number of employees in the region $\left(\overline{Ind\_S_{rti}}\right)$. Second, the same calculation is performed at the national level $\left(\widehat{Ind\_S_{rti}}\right)$. Finally, the instrumental variable is formed as a deviation from the mean $\left(Ind\_serv_{rti} = \overline{Ind\_S_{rti}} - \widehat{Ind\_S_{rti}}\right)$. Identification assumption is based on the assumption that the regional opportunities to work in the retail and wholesale sector during the studies correlate strongly with the student work, but sector variation has no direct effect to work after graduation.

Regional variation in the regional sector size during the studies may not be truly exogenous, but it might also have a direct effect on earnings after graduation. We want to evaluate if the exclusion restriction is satisfied by comparing results when possible direct channels are controlled to the case when they are not. This is done by adding a relative sector size after graduation (and unemployment rate when used) in the estimation equation. Also other possible endogenous variables, such as time-to-degree, dummy for NUTS3 region and for region type after the graduation is used to evaluate how results are affected by a more prudent approach. One need to remember that this might change the causal interpretations of the results, as outcome controls adds one possible source of selection to the estimates (Angrist and Pischke, 2009, 64).

6 Results

This paper is interested in how working while studying affects earnings after graduation. As estimations contain a large set of control variables, it is impractical to show all of the coefficients in each table. In this section, Tables 1 – 4 show only the variables of interest, and the majority of the control variables are omitted from the tables. The results are estimated for different time periods and for different groups of interest. In section 6.1, the aggregate work experience during higher education is examined with different instrumental variables. In section 6.2, relevant work experience is studied separately from aggregate work experience. Section 6.4 includes robustness checks.

6.1 Aggregate work experience and earnings after graduation

Table 1 shows the OLS and IV results for individual earnings using the aggregate work experience (in years) obtained during school to measure working while studying. Earnings are measured one and two years after graduation. OLS results indicate is that work experience has a significant and positive relationship with earnings. The coefficients for working while studying indicate that work experience has a high return immediately after graduation, but the effect declines over time. Work experience during studies is related to approximately 11 – 13 per cent and 6 – 8 per cent higher annual earnings over two year period for polytechnic and university students, respectively[4]. This means that polytechnic students who work one and half years (average work experience is 1.52 years for polytechnic students) receive around 13 per cent higher annual earnings one year after graduation compared to those graduates who worked only six month time during the studies. Returns seem to be considerable lower for university graduates as those who worked 2.35 years received only an 8 per cent return compared to those who worked one year less[5].

The OLS results in Table 1 do not describe necessarily a causal relationship between the variables because we cannot control for unobservable variables. In Table 1, IV coefficients for work experience are estimated by a 2SLS estimator with the regional service and retail sector size as an instrumental variable while controlling sector size after graduation. This estimation should correct potential problems resulting from selection and from measurement error if our assumptions concerning the instrument hold. The IV estimation is performed with the same variables as the OLS estimation, except the study region variable is not used because it coincides with our instrumental variable. IV results show that the effect of work experience for polytechnic graduates is positive and significantly higher than the OLS results one year after graduation (First-stage results are presented in Table A3). The higher coefficient can result from two factors: the OLS results are biased downwards by the measurement error and/or our OLS estimates measure different effects than our IV estimates. Whereas the OLS estimate measures the average treatment effect (ATE) on the entire sample, the IV method measures the local average treatment effect (LATE) for the subsample of individuals affected by the unemployment rate changes during their studies (see, e.g., Angrist and Pischke, 2009, 151).

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4 The only control variable shown in the Table 1 is the measure for gap years (the time gap between the high school and higher education). This measures the potential work experience before enrollment. OLS results indicate a positive relationship between gap and earnings for polytechnic students.

5 The estimations were done without the disaggregation by sex as the main conclusions do not differ qualitatively for males and females.

TABLE 1 Work experience during higher education and earnings after graduation

Dependent variable: Log of annual earnings				
	One year after graduation		Two years after graduation	
Panel A: *Polytechnic*	(OLS)	(IV)	(OLS)	(IV)
Work experience (years)	0.129 **	0.264 **	0.113 **	0.007
	(0.017)	(0.083)	(0.020)	(0.099)
Gap (years)	0.076 **	0.135 **	0.064 **	0.022
	(0.017)	(0.039)	(0.016)	(0.043)
Sample size	3501	3501	2930	2930
F-value (1st stage)	-	104.7	-	94.35
Endog. test (p-value)	-	0.107	-	0.279
Panel B: *University*	(OLS)	(IV)	(OLS)	(IV)
Work experience (years)	0.077 **	0.067	0.055 **	- 0.053
	(0.011)	(0.078)	(0.012)	(0.080)
Gap (years)	0.011	0.010	0.011	- 0.019
	(0.010)	(0.025)	(0.010)	(0.026)
Sample size	3654	3654	3114	3114
F-value (1st stage)	-	54.15	-	38.74
Endog. test (p-value)	-	0.960	-	0.197

Robust standard errors are in parentheses. ** Significant at 1%,* significant at 5%.
Controls: age, age squared, gender, language, marital status, field of education, graduation year, gap years, math and language scores, fathers' and mothers' education and socioeconomic class and the NUTS3 study region. Note that the regional service sector size after graduation (NUTS4 level) is controlled in every IV-estimation.

Contrary to the earlier study by Häkkinen (2006), the 2SLS estimator fails to find any significant effect for university graduates. Still, the point estimate of IV coefficient is nearly identical to the OLS results one year after graduation. Two years after graduation the IV coefficient is negative and insignificant. IV coefficients do not show any relationship two years after graduation[6].

Results in Table 1 indicate that polytechnic graduates benefit from early work experience more than university graduates, which is plausible considering the institutional differences between universities and polytechnic schools.[7] Results also indicate that work experience has a relatively large effect on earnings one year after graduation. The results are based on the assumption that our instrumental variable strategy is valid. Next, we try to assess whether this is the case. In Tables 2 and 3, the validity of the IV estimates is examined separately

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6 First stage F-value for IV variable is high (well above 10) in all cases which indicates that our IV variable is strong (correlates strongly with work experience). The test for endogeneity indicates that we cannot rule out the possibility that our OLS estimates might be more efficient than our IV results.

7 Probit and IV-Probit models show a similar employment effect year after the graduation: IV-Probit coefficient of work experience is 0.49 for polytechnic graduates (significant) and 0.06 for university graduates. Two years after graduation these coefficients are close to zero and insignificant.

for polytechnic and university graduates. The difference from the earlier estimations is that now we use also the regional unemployment rate as an IV variable and other control variables after the graduation to study whether the estimated relationship is truly causal.

Columns 1 and 2 in Table 2 show the normal OLS results for the graduates. Time-to-degree and other control variables are shown separately in the table. The F-statistics from the first stage indicate that that our instrumental variables are not weak; all of the F-values are over 50. In column 3, the second stage estimates of work experience are shown when using service sector size (Ind_serv_{rti}) as an IV variable. The coefficient is high, but when the current service sector size is included in the model (column 4), the coefficient is substantially lower. When time-to-degree and current regional variables are controlled (column 5), the coefficient is a few percentage points lower than that estimated with the unemployment rate as the IV variable. For the rest of the table, both IV variables are included in the estimation. When the current unemployment rate is not controlled, the instruments fail or nearly fail the over-identification test. The results in columns 8 through 10 do show that the coefficient for work experience remains close to the earlier estimates. This steadiness further confirms that our instrumental strategy is valid.

TABLE 2 Work experience during the studies and earnings after graduation: Instrumental variables for polytechnic students

Dependent variable: Log of annual earnings one year after graduation										
	OLS		IV							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Work experience (years)	0.129** (0.017)	0.139** (0.018)	0.682** (0.083)	0.264** (0.083)	0.224** (0.085)	0.723** (0.083)	0.349** (0.081)	0.278** (0.082)	0.278** (0.079)	0.239** (0.077)
Gap (years)	0.076** (0.017)	0.019 (0.034)	0.318** (0.040)	0.135* (0.039)	0.015 (0.034)	0.336** (0.041)	0.172** (0.038)	0.141** (0.039)	0.010 (0.034)	0.015 (0.034)
Time-to-degree (years)	-	0.070 (0.040)	-	-	-0.118* (0.058)	-	-	-	0.153** (0.056)	-0.126* (0.055)
Sample size	3501	3501	3501	3501	3501	3501	3501	3501	3501	3501
F-value (1st stage)	-	-	145.2	104.7	97.80	79.52	88.25	52.11	58.32	58.08
Over.id. test (p-value)	-	-	-	-	-	0.126	0.031	0.268	0.545	0.670
Endog. test (p-value)	-	-	0.000	0.107	0.310	0.000	0.005	0.062	0.074	0.194
Instrument: Ind_serv_{rti}	-	-	x	x	x	x	x	x	x	x
Instrument: $Unemp_{rti}$	-	-	-	-	-	x	x	x	x	x
Current $\text{Unemp}_{\text{rti}}$	-	-	-	-	-	-	-	x	x	x
Current $\text{Ind\_serv}_{\text{rti}}$	-	-	-	x	x	-	x	x	x	x
Current region	-	x	-	-	x	-	-	-	-	x
Current region type	-	x	-	-	x	-	-	-	-	x

Robust standard errors are in parentheses. ** Significant at 1%,* significant at 5%. *Controls:* age, age squared, gender, language, marital status, field of the education, graduation year, math and language scores, fathers' and mothers' education and socioeconomic class and NUTS3 study region (omitted in the IV estimation).

TABLE 3 Work experience during the studies and earnings after graduation: Instrumental variables for university students

Dependent variable: Log of annual earnings one year after graduation										
	OLS		IV							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Work experience (years)	0.077** (0.017)	0.090** (0.012)	0.160** (0.060)	0.067 (0.078)	0.001 (0.089)	0.159** (0.060)	0.071 (0.077)	0.073 (0.076)	0.033 (0.080)	-0.018 (0.079)
Gap (years)	0.011 (0.010)	-0.027 (0.016)	0.038 (0.021)	0.010 (0.025)	-0.017 (0.020)	0.037 (0.020)	0.011 (0.024)	0.012 (0.024)	-0.016 (0.020)	-0.015 (0.020)
Time-to-degree (years)	-	-0.050** (0.017)	-	-	-0.007 (0.051)	-	-	-	-0.020 (0.046)	0.004 (0.045)
Sample size	3654	3654	3654	3654	3654	3654	3654	3654	3654	3654
F-value (1st stage)	-	-	99.99	54.15	49.49	50.15	27.62	27.66	31.38	32.64
Over.id. test (p-value)	-	-	-	-	-	0.852	0.782	0.732	0.345	0.730
Endog. test (p-value)	-	-	0.147	0.960	0.339	0.150	0.989	0.991	0.517	0.179
Instrument: Ind_serv_{rti}	-	-	x	x	x	x	x	x	x	x
Instrument: $Unemp_{rti}$	-	-	-	-	-	x	x	x	x	x
Current $\text{Unemp}_{\text{rti}}$	-	-	-	-	-	-	-	x	x	x
Current $\text{Ind\_serv}_{\text{rti}}$	-	-	-	x	x	-	x	x	x	x
Current region	-	x	-	-	x	-	-	-	-	x
Current region type	-	x	-	-	x	-	-	-	-	x

Robust standard errors are in parentheses. ** Significant at 1%,* significant at 5%. *Controls:* age, age squared, gender, language, marital status, field of education, graduation year, math and language scores, fathers' and mothers' education and socioeconomic class and the NUTS3 study region (omitted in the IV estimation).

The difference in returns between polytechnic and university graduates becomes even clearer when different instruments and controls are added step by step for university graduates. The first two columns in Table 3 show the OLS estimates with (0.077) and without (0.090) the controls for time-to-degree and current region. Columns 3 and 6 show the second stage estimates when the current unemployment rate or the current service sector size is not controlled. The coefficients are significant and double of the size of the OLS coefficients. In columns 4, 7 and 8 current controls are added to the estimation. The IV coefficient for work experience is close to the OLS coefficient (0.067 - 0.073), but the results are not statistically significant. The essential difference compared to polytechnic students is that time-to-degree has a large impact on the IV results. When time-to-degree is controlled, the effect of work experience is insignificantly small, whereas the OLS results show a significant and positive effect (column 2). This finding might indicate that work that does not affect time-to-degree (e.g., work during the holidays) has no impact on earnings after graduation for university graduates. When time-to-degree is not controlled, the effect on earnings is considerably larger. Thus, work while studying that affects time-to-degree has an effect on earnings after graduation. This is feasible result because individuals who work in non-holiday periods probably have a better than average job, and the incentives to graduate on time are lower. This result for university graduates is the reverse of the findings by Häkkinen (2006), who noted that working while studying has an impact on earnings only when time-to-degree is controlled.

6.2 Relevant work experience and earnings after graduation

Work experience in the earlier estimations was measured by annual work months during school. Häkkinen (2006) also measured work experience for university students in this manner. Students work in a wide variety of jobs, and it might be informative to explore whether work experience from their field of education has a substantial effect on future earnings. The aggregate work months during higher education might also measure a different kind of work for polytechnic and university graduates. The results above indicate that this might be the case. To study this hypothesis further, a dummy variable for work relevancy was created using industry codes for the last week of the calendar year. A dummy variable indicating job relevancy gets a value of one if the work over the two-year period before graduation year is related to the field of education; otherwise the variable is zero.

The OLS results in columns 1-3 of Table 4 indicate that one year of work experience is associated with 12 – 14 per cent higher earnings after graduation for polytechnic students. The second order term is small and negative, as expected. The coefficient for relevant work is similar in size but not as significant. For university students, the results are somewhat different. While the coefficient for work experience is approximately 7 – 10 per cent, the coefficient for relevant work is significantly higher, approximately 20 – 22 per cent. For university students, the relevance of the work matters. Jobs that are relevant for university students are difficult to obtain during school. Finding a relevant job is easier for polytechnic students, whose studies are more work-orientated than university students. Those polytechnic students, who have done many work months before graduation, might already have found their future employer or at least obtained important experience that is helpful for finding a job immediately after graduation. In that sense, the work months might measure relevant work experience for polytechnic students, whereas for university students, work months are a bad proxy for relevant work.

In columns 4 to 6, different IV variables are experimented for endogenous variables. The IV variable for relevant work is similar to the IV variable of the regional service sector size variation, but now the relative size of the sector employment is calculated separately for each field of education (Ind_all_{rti}). The results show that work experience is not anymore significant for polytechnic graduates, and the coefficients are negative and imprecisely estimated. The variable for relevant work becomes positive and large, but it is imprecisely estimated. For university graduates, the IV coefficient for work experience is close to the OLS coefficient. The coefficient for relevant work is large and positive but imprecisely estimated because the IV variables fail to correct the results. This result is not surprising, as the instrumental variables are only weakly correlated with the relevant work experience variable. When relevant sector size (Ind_all_{rti}) is used as an IV variable, the partial R-squared of the excluded instruments for relevant work becomes somewhat larger and the coefficient is nearly significant at the 10 per cent level (column 5). Adding an interaction term to estimations (column 6) makes weak instrument problem more obvious[8].

[8] Also other instrumental variables (e.g. interaction and second order terms) were experimented, but this did not remove the problem of weak instruments

TABLE 4 Work experience, work relevancy and earnings after graduation

Dependent variable: Log of annual earnings one year after graduation						
	OLS			IV		
Panel A: *Polytechnic*	(1)	(2)	(3)	(4)	(5)	(6)
Work experience	0.120** (0.017)	0.122** (0.019)	0.136** (0.035)	-0.047 (0.367)	-0.165 (0.304)	-0.232 (0.307)
Work experience ^2	-	-	-0.003 (0.007)	-	-	-
Relevant work	0.106** (0.026)	0.118* (0.050)	0.119 (0.068)	1.343 (1.457)	1.877 (1.056)	1.194 (1.814)
Interaction	-	-0.007 (0.023)	-0.011 (0.056)	-	-	0.280 (0.648)
Interaction^2	-	-	0.001 (0.010)	-	-	-
Sample size	3501	3501	3501	3501	3501	3501
Over.id. test (p-value)	-	-	-	-	0.673	0.884
(1) Partial R-sq.	-	-	-	0.027	0.029	0.005
(2) Partial R-sq.	-	-	-	0.006	0.008	0.003
(3) Partial R-sq.					-	0.001
Panel B: *University*	(1)	(2)	(3)	(4)	(5)	(6)
Work experience	0.068** (0.011)	0.071** (0.013)	0.095** (0.031)	0.059 (0.089)	0.056 (0.085)	-0.574 (1.183)
Work experience ^2	-	-	-0.004 (0.004)	-	-	-
Relevant work	0.195** (0.026)	0.217** (0.048)	0.208** (0.069)	0.385 (1.119)	0.544 (0.337)	-4.100 (6.015)
Interaction	-	-0.009 (0.015)	-0.007 (0.041)	-	-	1.712 (2.949)
Interaction^2	-	-	0.000 (0.005)	-	-	-
Sample size	3654	3654	3654	3654	3654	3654
Over.id. test (p-value)	-	-	-	-	0.879	0.595
(1) Partial R-sq.	-	-	-	0.013	0.016	0.003
(2) Partial R-sq.	-	-	-	0.001	0.007	0.002
(3) Partial R-sq.	-	-	-	-	-	0.001
Instrument: Ind_all_{rti}				-	x	x
Instrument: Ind_serv_{rti} A	-	-	-	x	x	x
Instrument: $Unemp_{rti}$ B	-	-	-	x	x	x
Instrument: (A)*(B)	-	-	-	-	-	x
Current $\text{Unemp}_{\text{rti}}$	-	-	-	x	x	x
Current $\text{Ind\_serv}_{\text{rti}}$	-	-	-	x	x	x

Robust standard errors are in parentheses. ** Significant at 1%,* significant at 5%. Controls before graduation are used in estimations. (1) Partial R-sq.: partial R-squared of excluded instruments for work experience. (2) Partial R-sq.: partial R-squared of excluded instruments for relevant work. (3) Partial R-sq.: partial R-squared of excluded instruments for interaction term.

7 Robustness

The results are relatively consistent, but this consistency might be caused by misspecifications in the estimations. To exclude this possibility, multiple different sources of misspecifications were considered. As the estimations use a wide range of control variables, different combinations of variables were considered for robustness. A common result was that coefficients grew larger and more inaccurate when control variables were excluded from the estimation. The coefficients for work experience remained consistent with our earlier estimations. All of the estimations were also repeated with a different sample that is larger and does not include the controls for high school grades. This second estimation was performed because one can graduate from higher education without first graduating from an upper secondary school. Again, the results differed only slightly. For robustness, the samples of university and polytechnic graduates were also trimmed from the tails of earnings distribution to exclude outlier observations. Two per cent from each end of the earnings distribution was excluded. The coefficients for work experience become slightly smaller compared to the earlier estimates.

Estimated effects could also result from positive selection to employment after graduation. While the portion of zero earners is low (around 5 per cent), it is still important to exclude this possibility. Estimations were repeated with a modified dependent variable (zero earnings were replaced by a value of one) and with Tobit models (left -censored). With a modified dependent variable the coefficients and the standard errors for work experience became a bit larger. Left –censoring had even smaller effect on the results. Thus, it seems that the found positive effect is not a result of selection to employment.

8 Conclusions

Working while studying eases the transition from school to work as the positive effect is largest immediately after graduation. However, the returns on working during higher education are not equal sizes for polytechnic and university graduates in Finland. Returns seem to be different because the accumulated work months are more connected to the field of education for polytechnic students. A separate investigation supports the conclusion that relevant work experience, which likely affects time-to-degree, is more important for university graduates than aggregate work experience.

The benefits of work during the studies are questionable as the estimated earnings effect is small and short-lived. Finding supports earlier conclusion by Häkkinen (2006) who noted that working while studying has no significant effect on graduate earnings. It is difficult to argue for work before graduation, as working while studying can cause students to postpone their graduation. If the amount of work while studying is constrained more forcefully in Finland to accelerate the graduation from higher education, then the positive role of work experience connected to the field of education should be emphasised. Policies that increase incentives and opportunities to accumulate work experience in the field of education before graduation could ease the transition from school to work. This type of policy might minimise possible negative effects resulting from more constrained opportunities to work during studies.

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Appendix A

TABLE A1 Descriptive statistics of selected variables

	POLYTECHNIC		UNIVERSITY	
	Average (s.d.)	Obs.* (zero)	Average (s.d.)	Obs. (zero)
Dependent variables				
Earnings one year after graduation year (euro)	19,927 (10,741)	3,698 (197)	25,262 (13,404)	3,805 (151)
Earnings two years after graduation year (euro)	21,929 (11,824)	3,117 (187)	28,550 (14,996)	3,258 (144)
Work experience				
Relevant work.	0.31		0.32	
Work months during enrolment	18.2 (13.67)	-	28.2 (21.06)	-
Instrumental variables				
Average unemployment rate during the studies	14.2 (5.66)	-	15.1 (5.55)	-
Average share of service sector employment rate during the studies	0.32 (0.47)		0.33 (0.47)	
Other control variables				
Age at the graduation year	25.4 (2.59)	-	26.6 (2.35)	-
Swedish-speaking	0.04	-	0.06	-
Female	0.59	-	0.54	-
Married (1st year)	0.21	-	0.31	-
Married (2nd year	0.27	-	0.39	-
Education	0.02	-	0.12	-
Humanities and arts	0.08	-	0.14	-
Social sciences	0.30	-	0.27	-
Science	0.00	-	0.12	-
Technical	0.28	-	0.25	-
Agriculture	0.03	-	0.02	-
Health and Welfare	0.23	-	0.07	-
Services	0.06	-	0.01	-
Gap	2.25 (2.58)	-	1.38 (1.92)	-
Time-to-degree	4.02 (1.33)	-	6.44 (2.11)	-
Unemployment rate (1st year)	11.0 (4.68)	-	10.3 (4.70)	-
Unemployment rate (2nd year)	10.7 (4.39)	-	9.96 (4.37)	-
Ind_serv_{rti} (1st year)	0.42 (0.29)	-	0.12 (0.29)	-
Ind_serv_{rti} (2nd year)	0.41 (0.29)	-	0.11 (0.29)	-

* Number of observation with zero earnings in the period of interest. *Note:* All variables are not presented in Table A1.

TABLE A2 Variable Description

Dependent Variables	
Log of annual earnings	Log of annual earnings for the period of interest (does not include transfer payments).
Individual Background	
Age	Age in the graduation year.
Age^2.	Age squared.
Language	A dummy variable (Swedish = 1, other =0).
Female	A dummy variable (female = 1, male=0).
Married	A dummy variable (married =1, not married = 0).
Individual working experience	
Relevant work	A dummy variable indicating if the work over a two- year period before the graduation year was related to own field of education (formed by industry code).
Work experience	Sum of months of employment during enrolment in school divided by 12 (not counting the enrolment and the graduation year).
Individual educational variables	
Math Scores	A set of 10 categories for high school math grades from the basic and advanced level.
Language Score	A set of five variables indicating the high school language grade of native language.
Time-to-degree	Average time-to-degree in years.
Gap	Break (measured in years) between high school and higher education.
Graduation Year	A dummy variable indicating the graduation year.
Field of education	A dummy variable indicating the field of education (Fields: Education; Humanities and arts; Social sciences; Science; Technical; Agriculture; Health and Welfare, Services)
Level of education	A set of 2 dummy variables (examined separately in the analysis)
Parental characteristics	
Father's education	A set of 5 dummies indicating the father's highest level of education. (Primary, secondary, upper secondary, tertiary, upper tertiary)
Mother's education	(as previous)
Father's socioeconomic class	A set of 6 dummies indicating father's socioeconomic class (farmer, self-employed, low-rank worker, high-rank worker, worker, no classification available)

TABLE A2 (Cont.) Variable Description

Mother's socioeconomic class	(as previous)
Regional Characteristics	
Study Region	A set of 18 dummies indicating the continental NUTS 3 region where the educational institution is located.
Region of living after graduation.	A set of 18 dummies indicating the continental NUTS 3 region.
Type of the region after graduation	A set of 5 dummies indicating the type of region. (university region, large city region, medium-sized city region, industrial region, rural region)
Regional unemployment rate after graduation	Annual unemployment rate (20-34-year-old population) at NUTS 4 regional level.
Regional share of retail sector after graduation	The regional (NUTS 4) size of retail and wholesale sector employment calculate as a deviation from the mean (compared to national level).
Instrumental variables	
Regional unemployment rate during the studies ($Unemp_{rti}$)	Average unemployment rate (20-34-year-old population) over the period of interest (on NUTS 4).
Regional share of retail sector two year before graduation (Ind_serv_{rti})	The regional (NUTS 4) size of retail and wholesale sector calculate as a deviation from the mean (compared to the national level).

TABLE A3 First stage results: Working while studying and the relative size of the retail and service sector.

	Polytechnic		University	
Regional retail and service sector size (IV-variable)	0.675**	0.264**	0.160**	0.067
	(0.082)	(0.083)	0.060	(0.078)
	F=147	F=105	F=100	F=54
Control variables :				
Regional retail and service sector size after grad. year	NO	YES	NO	YES
Individual specific covariate	YES	YES	YES	YES
Fathers and mothers characteristics	YES	YES	YES	YES
Graduation year dummies	YES	YES	YES	YES

TABLE A4 Industry code and the field of education.

The Field of Education	Industry code (TOL02)
Teacher Education and Educational Science	M: 80 – 80429
Humanities and Arts	O: 91310 – 92720
	K: 74810 – 74859
	M: 80 – 80429
Social Sciences and Business	G: 50 – 52740
	J: 65- 67200
	K: 72- 74509
Natural Sciences	M: 80 – 80429
	K: 73 – 73200
Technology	DD - F: 21110 - 45450
Agriculture and Forestry	A - B: 10 – 2019
Health and Welfare	N: 85 – 85329
Services	G: 50 – 52740
	H: 55 – 55520

CHAPTER 3
WORKING WHILE STUDYING: DOES IT LEAD TO GREATER ATTACHMENT TO THE REGIONAL LABOUR MARKET? *

Abstract**

In this chapter, we will study the link between working while studying and migration. Understanding this link is critical because many politicians are calling for policies that would cut down the hours students spend on working to shorten the graduation time. Our analysis focuses on Finnish graduates from universities, polytechnics and vocational schools in 1991–2004. We use rich register-based longitudinal microdata constructed by Statistics Finland. Migration propensities are studied up to three years after graduation. Our results show that working while studying can partly explain why the mobility of highly educated people has declined. We find that that there are differences by study region and earlier migration behaviour. We also offer possible avenues for future research on working while studying and graduate migration.

Keywords: migration, working while studying, higher education, regional labour markets.

JEL classification: J61, I22, R23

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1 Introduction

Extensive theoretical and empirical research indicates that highly educated individuals are more likely to migrate, as investments in human capital increase the expected returns on migration (e.g., Greenwood, 1997, Chapter 12). Recent empirical evidence, however, has shown that internal migration rates have fallen for highly educated people since the beginning of the 1990s in the United States[1] (Molloy et al., 2011, 2014); below, Figure 1 documents a similar observation for university and polytechnic graduates in Finland. It is not clear whether the decline in mobility originates from changes in individual behaviour or characteristics or is connected to wider shift in the labour market. Traditionally, the high mobility of labour is thought to be a sign of a dynamic economy and an essential part of a well-functioning labour market. Hence, it is important to understand the underlining factors that affect migration propensity.

We argue that the decline in the migration propensity of highly educated people might be related to changes in the labour market, particularly the way individuals work during their studies. Short-lived jobs before graduation can work as a stepping stone to long-lasting jobs (Cockx and Picchio, 2012). Thus, work experience accumulated during their studies might attach youths to their study region. The transition from education to work happens often even before graduation, when students work to finance their education or to gain a higher standard of living. Thus, students begin to accumulate work-related human capital before graduation.[2] Occupational and industry-specific human capital can accumulate considerably for individuals with a high level of education, which is often needed to perform specialised tasks (Sullivan, 2010; Yamaguchi, 2012). If human capital is not easily taken to the new occupation or work, this accumulation can decrease migration due to the increased cost of moving from the home region.

1 In the United States, population level migration rates have also declined dramatically since the end of the 1990s (Partridge et al., 2012).

2 A study by Molloy et al. (2014) indicates that labour market transitions (switching employers or occupations) and geographical mobility are strongly correlated (positively) in the US. Hence, the stability in human capital accumulation might explain at least partly why migration has declined.

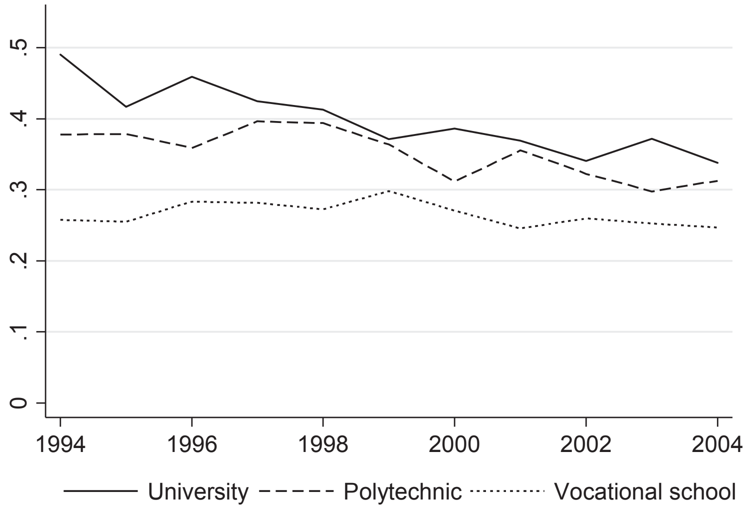

FIGURE 1 Migration rates for graduates by level of education

This chapter examines how working while studying is related to migration from the study region. The analysis focuses on individuals who graduate from higher education (universities, polytechnics or vocational schools) after high school in Finland from 1994–2004. Our first observation is that the mobility of universities and polytechnics graduates has declined in Finland since the first half of the 1990s (see Figure 1). Surprisingly, the decline of migration rates coincides with a period when the Finnish economy expanded significantly, even though an economic growth period is normally understood to relate to higher labour mobility (Saks and Wozniak, 2011). This finding is worrisome; high labour mobility is believed to be crucial for economic development because it improves the allocation of labour across regions.

Our main contribution is that we show that the increase in working while studying can partly explain why the mobility of highly educated people has declined. It has also meant that peripheral regions have gained more educated labour, which is in accordance with the goals of higher education policy in Finland. However, this decline may have worsened the match of higher education and regional labour markets. Second, there are currently plans underway in Finland to make working while studying less tempting, but it is not known how more restricted employment during the studies might affect youth migration. Our results suggest that restrictions on work could potentially lead to the increased migration of highly educated people to the capital region (Helsinki), which could affect regional development. Third, this chapter offers suggestions for future research on graduate migration.

The rest of the paper is organised as follows. Section 2 briefly discusses the theory and closely related literature. Section 3 gives basic facts on the Finnish education system. Section 4 introduces the data and methods and shows descriptive evidence. Section 5 presents the estimation results. Section 6 summarises the results and collects recommendations for future research. Section 7 concludes the paper.

2 Working while studying and internal migration

2.1 How does working while studying affects migration after graduation?

To our knowledge, prior literature has not studied how working while studying affects migration behaviour. This observation is surprising because migration is often seen as an essential part of the job search, mediating the effects on other labour market outcomes. Following Sjaastad (1962) and Bowles (1970), we consider migration as investment in human capital, implying that working while studying affects the costs and benefits of migration after graduation. The direction of the effect on migration is theoretically undetermined.

On the one hand, working while studying expands local labour market networks because students' employers and fellow employees can share information about local jobs. The increased stability (e.g., the lower risk of unemployment) is likely to reduce the propensity to move after graduation. Furthermore, students who do work during their studies might disproportionately consist of individuals whose field of study does match well with the employment needs of the local industries, and vice versa. Therefore, the wage-opportunity cost of moving is relatively high for these matched workers and low for mismatched workers who might invest in migration as a means of improving their job matches (cf. discussion in Nakosteen et al. 2008, p. 772).

On the other hand, a good employment history might be seen as indicator of productivity, which elicits job offers across the regions and thus increases the graduates' propensity to move. Migration also requires financial resources. Graduates who have worked during their studies are better able to finance to their migration efforts after graduation. However, because students in higher education are relatively wealthy in Finland and the government support is generous (see Section 3), financial restrictions are unlikely to play major role in the decision to move. Overall, the propensity to migrate is thus likely to be negatively related to working while studying. However, a positive relationship is also plausible a priori.

Finnish university students have the highest migration propensity close to their graduation year, but the propensity declines dramatically in the following years (Haapanen and Tervo, 2012). Although the majority of graduates stay in their study region after graduation, migration rates are significantly higher for those who did not grow up in the study region. As working while studying can provide essential information on local labour markets already before graduation, it is likely to an important factor (along with prior mobility) in determining whether the graduate stays in their study region. Especially in the slow-growth regions, where employment opportunities are scarce, gained work experience is a competitive advantage over graduates without experience. Nonetheless, very little is known of how working and other work-related activities while studying at different levels of education are related to future mobility.[3]

2.2 Related literature on other labour market outcomes

Although prior evidence on the relationship between working while studying and migration is lacking, there are related studies that examine how early work experience affects other labour market outcomes (e.g., Neumark and Joyce, 2001; Rayan, 2001). Empirical research has found little or mixed evidence that working while studying has significant positive effects on labour market outcomes at the individual level.[4] Some studies have observed a positive earnings effect (see the early survey by Ruhm, 1997), but the effect might be explained by individual selection and unobserved heterogeneity (e.g., Hotz, Xu, Tienda and Ahituv, 2002). The positive effect is normally found to be small and evident right after graduation. Light (2001) finds that students who accumulate two years of work experience before graduating from college have 10 % higher wages after graduation. In Finland, Häkkinen (2006) finds that student employment has a positive effect on earnings right after graduation (19 % one year after graduation), but when she accounts for the effect on the duration of studies, the earnings effect is insignificant. Her finding suggests that working while studying can ease the transition from school to work but has no long-term effect on earnings.

It is also plausible that the effect of work varies with the level of education and work intensity. Molitor and Leigh (2005) show that in-school work experience is more important for two-year college students than for four-year students. A study by Joensen (2009) suggests that a moderate amount of student employment enhances labour outcomes but that excessive amount of work might have a negative effect on academic achievement.

Work and migration decisions might have profound long-term effects on youths' lives. The early career period is often "chaotic"; changing jobs is common among youths (Neumark and Joyce, 2001; Ryan, 2001). The long-term outcomes depend on how young individuals succeed in these first years on the labour market. Unemployment at a young age or after graduation has a significant negative effect on future earnings (Doiron and Gørgens, 2008; Mroz and Savage, 2006; Oreopoulos, von Wachter and Heisz, 2012; Wachter and Bender, 2006). On the contrary, stability in the early career and the successful transition from school-to-work increases future labour activity (Neumark and

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3 Migration behaviour after graduation has gained a noticeable amount of attention in the recent literature (e.g. Venhorst, Van Dijk and Van Wissen, 2011, 2010; Faggian and McCann, 2006; Faggian, McCann and Sheppard, 2006, 2007; Faggian and McCann, 2009; Faggian, Corcoran and McCann, 2013; Abreu, Faggian and McCann, 2015), but studies do not consider how students' employment and other activities during their studies are related to migration behaviour.

4 Nevertheless, empirical evidence indicates that mandatory internships have a positive wage effect (Saniter and Siedler, 2014).

Joyce, 2001) and adult earnings (Neumark, 2002). Migration is essential part of the job search in the early career, and the migration decision is often made simultaneously with job acceptance.[1] Graduates choose a labour market region at the beginning of their work life, and their mobility declines rapidly after the individual finds a suitable job, establishes a family and develops new social networks in the region (Gordon & Molho, 1995; Huff & Clark, 1978; Molho, 1995).

3 Institutional background

After completing comprehensive schooling, approximately 50 % of students in Finland will continue to high school, which lasts for three years and ends with a matriculation examination. High school gives qualifications for applying to institutions of tertiary education, i.e., universities and polytechnics. Approximately 36 % of applicants to polytechnics and 29 % of those to universities are successful and commence studies (Statistics Finland, 2013). Because entry to tertiary education is highly competitive, many students also complete vocational school degrees after high school. Nonetheless, only 12 % of new vocational school students had a high school degree in 2012 (OSF, 2012). Note that degrees from vocational schools and colleges were more common for high school graduates in the early 1990s than they are today because the vocational schooling system was more fragmented and no polytechnics existed in Finland. The polytechnics were formed gradually after 1991 by merging 215 vocational colleges and schools. This expanded higher education network to 26 polytechnics in addition to the pre-existing 15 universities. Regionally, universities are spread over ten city regions, whereas the networks of polytechnics and vocational schools cover the entire country.

In Finland, education is practically free at all levels, and students can benefit from state support, which consists of three parts: direct allowances, housing benefits and state guaranteed loans. Because the student support system reform in 1992 (extending to secondary education in 1994), the subsidy levels and the rules guiding the use of entitlements have been relatively stable for the majority of the students. From 1992 to 2012, the state support for students in higher education has increased the following: direct monthly allowances from €264 to €298, monthly housing benefits from €149 to €202 and the maximum of state guaranteed loans from €202 to €300 (see Ministry of Education and Culture, 2012). At the same time, the living costs have increased by 40 % and the general income level by 93 %, and much of the increase in

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1 For example, Detang-Dessendre and Molho (1999) find that unemployed individuals are more likely to undertake contracted migration as opposed to speculative long-distance migration.

benefits has been the state-guaranteed loans Finnish students are reluctant to take; therefore, incentives for working have increased.[2]

Student benefits have always been constrained by individuals' taxable income. The support system allows students to have substantial earnings during the academic year while receiving full student benefits. From 1998 to 2007, students were allowed to work and earn €505 for each month they received full student benefits and/or housing benefits, and this earnings limit was €1,515 for each benefit-free month.[3] Thus, the incentives to work during the studies have been high in Finland; students who collect full benefits from the typical nine study months could still annually earn €9,090 without the need to repay the allowances.

4 Descriptive analysis

4.1 Data and methods

The analyses are based on the Longitudinal Census Files and the Longitudinal Employment Statistics File constructed by Statistics Finland. These annually updated register-based datasets consist of a large set of variables from 1987 to 2006. This study uses a seven per cent random sample of permanent residents (in Finland in 2001). We will restrict the analysis to individuals who completed their first master's, polytechnic or vocational school degree after high school in 1991–2004.

We investigate graduates' propensity to move from their study region within three years after graduation. Here, study region refers to the NUTS3 region where the graduate receives his/her first degree after high school. Thus, the earlier high school region and the study region are not necessarily the same region. Following earlier research (Böckerman & Haapanen, 2013; Nivalainen, 2004), migration is defined as a long-distance migration between 19 NUTS3 regions (i.e., large labour market areas). Whereas short-distance migration is also common near the graduation year (e.g., because of student housing and requirements for house ownership), we focus on migration between the NUTS3 regions, which constitute a distinct labour market and cultural and geographic areas in Finland. Thus, we believe that this definition of migration constitutes a reliable measure for the individual's decision to change the labour market area and not just to commute over a longer distance.

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2 According to a survey by the Ministry of Education and Culture, the major reason why students do not take student loans is to avoid indebtedness on principle reasons (Viuhko, 2006).

3 From 1995 to 1997, students were allowed to earn €303 per subsidy month. Allowances were cut by 10 % on every €50 above the limit. Students who earned more than €1,180 per month on average were not entitled to student benefits. In 2008, earnings limits were raised by 30 %.

We have constructed two measurements of work that are used to study the relationship between working while studying on migration. First, we define full-time work over a three-year period before the graduation year by using wage earnings, which were deflated using the consumer price index (base year 2000). Statistics Finland classifies an individual as a full-time worker for the year if his/her earnings exceed €8,409 (Statistics Finland, 2000). This definition of full-time work is also used in a recent study (Böckerman, Hämäläinen, and Uusitalo, 2009). We apply this earnings threshold using average annual wage earnings over the three-year period before the graduation year. Second, we define an individual as a part-time worker if his/her average earnings from the same period are less than €8,409 but more than €6,060. The lower threshold for part-time work is not an official definition but is based on rules related to student allowance payments, as discussed above. Students can earn a maximum of €505 per month and receive student benefits each month. Nonetheless, we have also assessed how different lower-bound limits for earnings affect our conclusions (results available from authors).[4]

In estimation, we will restrict the analysis to individuals who completed high school in 1990-1996 because we only know the matriculation examination results from 1990 onwards. We will not consider later years to allow for sufficient time for them graduate from higher education. We also exclude (124) individuals who were older than 25 when they graduated from high school to increase the homogeneity of the sample. After deleting a few observations with incomplete data, we are left with 10,077 graduates who completed their first master's degree, polytechnic or vocational school degree by 2004.

We use a set of probit models to estimate how the completed level of education, working while studying and their interactions relate to the propensity to move from the study region. The models use a number of background variables to control for individual-specific heterogeneity (such as high school grades to control for individual ability). Table 1 displays the variables used in the models and their definitions and mean values.

4 Months of employment are also available in our data, but we believe that earnings are a more accurate measurement of work, particularly for students. Students can work irregularly a few hours per day. Although data are based on administrative files, one hour of work per day is counted as a workday, and fourteen workdays is registered as a work month. This might distort the measurement of work particularly for students who are relatively distinct from the other labor force.

TABLE 1 Description of variables and their mean values.

Variable	Description	Mean
Dependent variable		
Migration	1 if moved from the NUTS3 study region by the end of second year after graduation. 0 otherwise	0.32
Working while studying		
Full-time work	1 if average annual earnings over €8,409 during 3-year period before graduation year, 0 otherwise	0.19
Part-time work	1 if average annual earnings €5,666–8,409 during 3-year period before graduation year, 0 otherwise	0.14
Less than part-time work	1 if average annual earnings under €5,666 during 3-year period before graduation year, 0 otherwise	0.67
Level of education		
Master's degree	1 if graduated with master's degree, 0 otherwise	0.30
Polytechnic degree	1 if graduated with polytechnic degree (lower-degree level tertiary education), 0 otherwise	0.26
Vocational school degree	1 if graduated with vocational school degree (upper secondary level education or lowest level tertiary education), 0 otherwise	0.44
Control variables		
Age	Graduation age	25.08
Age squared	Graduation age squared	635.2
Female	1 if female, 0 otherwise	0.61
Swedish	1 if Swedish speaking, 0 otherwise	0.05
Married*	1 if married, 0 otherwise	0.24
Children*	1 if at least one child, 0 if no children	0.06
Female with children*	Interaction term (Female × Children)	0.16
Spouse's education*	0 if not married, 1 if basic education,…, 5 if higher education	0.55
Spouse's employment*	1 if spouse is employed, 0 otherwise	0.09
Spouse's income*	Annual income of spouse, €10,000	0.09
Flat/house owner*	1 if owns a flat or house, 0 otherwise	0.21
HS work experience	Sum of work months over 3-year period during the high school	6.26
HS work exp. squared	HS work experience squared	92.08

TABLE 1 (Cont.) Description of variables and their mean values.

Math score	Matriculation score for math grades at the basic and advanced level from high school (1–10; 10 is the best), 0 if missing.	3.27
Language score	Matriculation score for the grade of the native language (1–5; 5 is the best), 0 if missing	2.39
Missing scores	1 if matriculation score(s) missing, 0 otherwise	0.33
Migrated for studies	1 if study region is not high school region (NUTS3), 0 otherwise	0.20
Parent's region	1 if mom or dad is living in the study region	0.79
Unemployment rate*	Average unemployment rate of 20–34-year-old population in the NUTS4 study region	18.55

Notes: Estimations also use study region dummies and graduation year dummies for high school and the next degree. Variables marked (*) are measured three years before graduation year. Earnings were deflated using consumer price index in 2000.

Control variables are measured three years before graduation year or earlier. Thus, they are determined before our work measurements to avoid an endogeneity problem. Nonetheless, our results should not be interpreted as causal effects of student work on migration behaviour.[5] Rather, the coefficients of our analysis reflect conditional correlations between factors of interest. The control variables consist of information on individual, family, parent and regional characteristics that have been linked to migration decisions in the prior literature. The controls include, e.g., individual and household factors such as marriage (e.g., Newbold, 2001), spouse earnings (e.g., Haapanen and Tervo, 2012), education in general (e.g., Machin, Salvanes and Pelkonen, 2012), high qualifications (Venhorst, Van Dijk and Van Wissen, 2010) and earlier migration behaviour (e.g., DaVanzo, 1983), which all have an effect on migration propensity.

4.2 Descriptive view on student employment and graduate migration

Before presenting our estimation results, we will first provide descriptive results on student employment and graduate migration. Figure 2 illustrates how much Finnish graduate cohorts (1994–2004) worked over the three-year period before their graduation. After a severe recession in the early 1990s, the share of students who worked full-time and part-time grew substantially. For example, in 1996, only 11 % of university graduates worked full-time before graduation, but in 2004, this share of students was 36 %. The change is similar in magnitude for polytechnic and vocational school graduates. Unfortunately, our data do not enable us to investigate whether students work for the same firm or industry before and after graduation. Successful employee-job or

5 We cannot exclude the possibility that some unobserved factor (additional to observed covariates) correlates with student employment and further migration behaviour.

employee-industry matches before graduation could be a crucial factor in determining migration decisions in the future. Because we only observe the amount of work measured by annual earnings, it should be stressed that employed students might have multiple employment periods and employers during their studies. We cannot observe their importance with our individual-level data.

Increased student employment indicates that many of the students enter the labour markets before graduation. Next, we will consider whether high student employment is related to a lower migration propensity. Each group of graduates (university, polytechnic and vocational school graduates) is studied separately. Figure 3 shows graduates' propensity to move from the study region within three years after graduation. Migration rate is clearly lower for graduates who work full-time before graduation. In 2004, approximately 19 % of university graduates who also worked full-time before graduation migrated from the study region. Those university students who did not work part-time or full-time had a significantly higher propensity to migrate (33 %). Although this negative relationship between student employment and migration is relatively similar across the graduate groups, the decline in the migration propensities is most noticeable for polytechnic and university graduates during the study period, as illustrated by Figure 1. Next, we will investigate whether the relationship holds even after controlling for other factors.

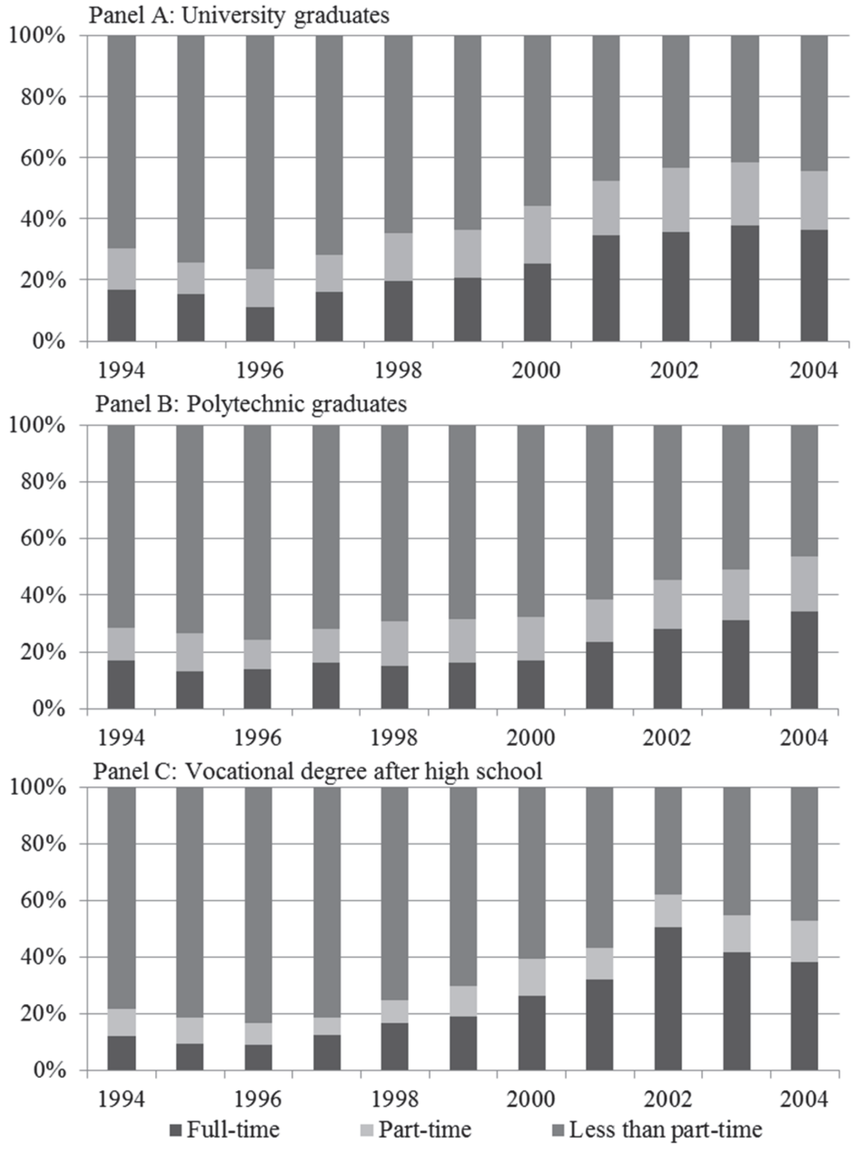

FIGURE 2 Proportion of graduates working full-time, part-time or less over a three-year period before their graduation year (1994–2004). *Source:* Own calculations based on a 7 % random sample. No restrictions are placed on the matriculation year.

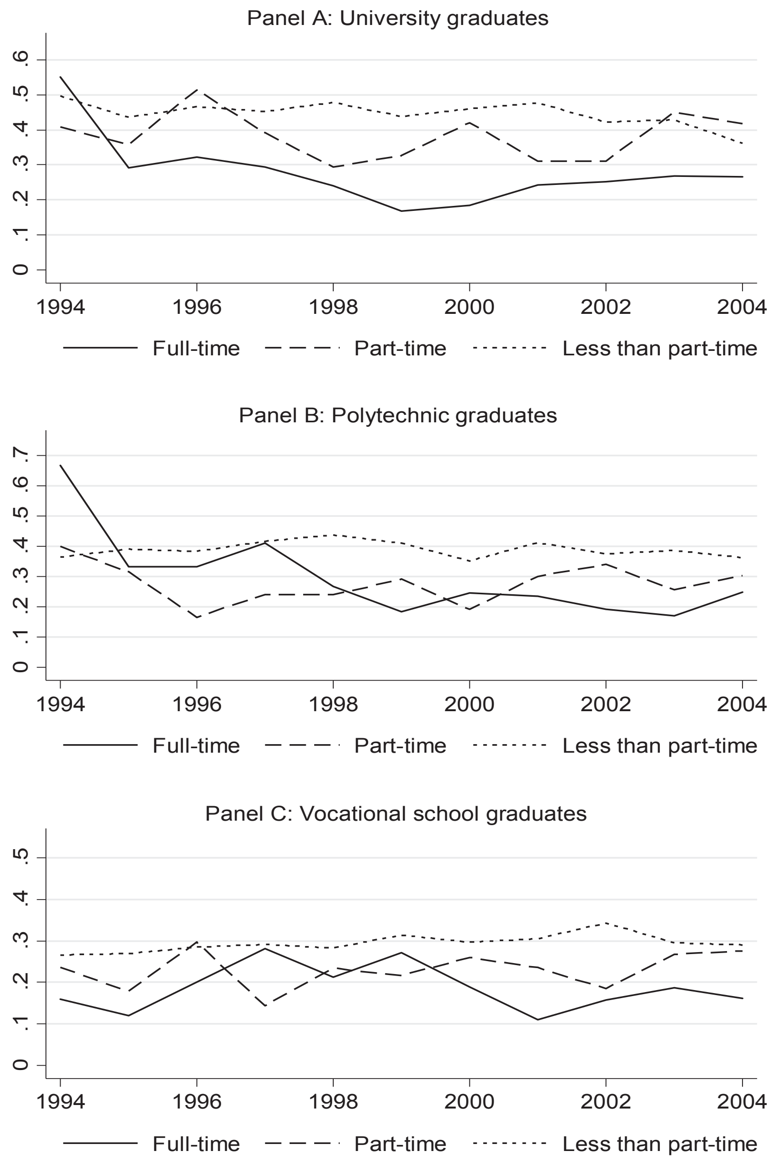

FIGURE 3 Migration rates from the study region over the three-year period after their graduation year (1994–2004). *Source:* Own calculations based on a 7 % random sample. No restrictions are placed on the matriculation year.

5 Estimation results

Next, we will present estimation results that show how the completed level of education and working while studying relate to the propensity to move from the study region. The estimated probit models use a full set of interaction terms between the level of education and working status. They also use the number of background variables to control for confounding factors (see Table 1). First, Table 2 shows the baseline results for the entire country and regionally disaggregated results (Helsinki metropolitan region vs. the rest of the country). Then, Table 3 considers the relevance of the prior (school-to-school) mobility on the results. Throughout, we report average marginal effects on migration; probit estimates are available on request from the authors.

The results for the entire country show, as expected, that the level of education is positively related to migration propensity (see column 1 in Table 2). The estimated average marginal effects indicate that university graduates with a master's degree have, on average, a 12 percentage point higher propensity to migrate from the study region than vocational school graduates. The corresponding figure for the polytechnic graduates is 5 percentage points. Instead, student employment is negatively related to migration propensity. Students who work full-time (part-time) are 7 (4) percentage points less likely to migrate than those who do not work significantly prior to graduation (i.e., the reference group). These findings are consistent with the view that better labour market opportunities during studies significantly decrease migration later on.

Given that our probit model contains interaction terms between the level of education and working status, we can also investigate how the relationship between student employment and migration varies with the level of education. Although students who work full-time have, on average, a 7 percentage point lower migration propensity, the conditional marginal effects show that the negative relationship is particularly strong for university graduates (-12 %) and polytechnic graduates (-9 %) but not significant for vocational school graduates. Part-time work also hinders migration, particularly for students graduating from polytechnics but for also those from universities.

TABLE 2 Average marginal effects on migration by study region

Dependent variable: Migration (1/0)	Entire country	Study region is Helsinki	Study region is not Helsinki
	(1)	(2)	(3)
Level of education			
Master's degree[a]	0.119***	0.041**	0.159***
	(0.014)	(0.019)	(0.018)
Polytechnic degree[a]	0.049***	0.004	0.068***
	(0.012)	(0.016)	(0.015)
Working while studying			
Full-time work[b]	-0.070***	-0.038**	-0.095***
	(0.014)	(0.015)	(0.019)
Part-time work[b]	-0.039***	-0.052***	-0.030
	(0.014)	(0.016)	(0.019)
Conditional on master's degree			
Full-time work[b]	-0.118***	-0.068***	-0.134***
	(0.020)	(0.024)	(0.029)
Part-time work[b]	-0.047**	-0.077***	-0.017
	(0.022)	(0.027)	(0.030)
Conditional on polytechnic degree			
Full-time work[b]	-0.094***	-0.045*	-0.117***
	(0.022)	(0.024)	(0.031)
Part-time work[b]	0.062***	-0.049*	-0.068**
	(0.024)	(0.028)	(0.031)
Conditional on vocational school degree			
Full-time work[b]	-0.022	-0.006	-0.057**
	(0.021)	(0.024)	(0.028)
Part-time work[b]	-0.020	-0.030	-0.016
	(0.022)	(0.024)	(0.030)
Log likelihood	-5,343	-999	-4,285
Pseudo R-squared	0.152	0.095	0.097
Average predicted migration rate	0.318	0.118	0.405
Number of observations	10,077	3,046	7,031

Notes: Average marginal effects (AMEs) are based on probit models that include main effects for the level of education and working status and their full set of interactions. All models also contain the control variables described in Table 1. Marginal effects are computed as averages over all relevant observations. Conditional AMEs are computed only for the selected graduate population (e.g., master's). [a] Reference education is a vocational school degree (e.g., "full-time work" displays its AME on migration relative to working less than part-time for all graduates). [b] Reference working status is less than part-time.

There are good reasons to suspect that migration propensity is differently affected by working experience in the Helsinki region than elsewhere in the country. The Helsinki region is the only metropolitan area in Finland, and approximately one third of all economic activity occurs there. Therefore, columns (2) and (3) in Table 2 present the average marginal effects that have been estimated separately for subsamples of students living inside and outside the Helsinki region. The results confirm our expectations: There are considerable differences by study region. Having a master's degree is related to only a 4 percentage point increase in migration propensity in the Helsinki region, whereas outside the Helsinki region, having a completed master's (polytechnic) degree is on average related to a 16 (7) percentage point higher migration propensity than having a vocational school degree. This finding is in line with earlier observations by Haapanen and Tervo (2012): Highly educated graduates tend stay in the Helsinki region, where migration rates are lower than outside Helsinki. Furthermore, the average predicted migration rate is considerably smaller in the Helsinki region (12 %) than elsewhere in the country (41 %).

Furthermore, Table 2 shows that graduates who work full-time during their studies are considerably more likely to stay in their study region than those who work less, but the negative relationship is stronger for those who live outside the Helsinki region (see columns 2 and 3). There are many reasons why working while studying may have a smaller effect in the Helsinki region. The demand for student work is greater in the vicinity of the capital region, and high living expenditures, such as rent, can force students to work to finance their daily life. Elsewhere, rent and other living expenditures are more modest, but jobs are also harder to find. Thus, a local work experience might be appreciated more outside the Helsinki region. It is also possible that other unobservable factors might explain the observed difference in student work and migration.

Finally, in Table 3, we have further divided the regional samples according to graduates' prior mobility (stayers vs. movers). In particular, stayers (movers) are defined as graduates whose study region is (not) the same as their high school region. The results show that graduates' migration history does matter. Looking at the average predicted migration rates across the four subsamples, we can see that graduates who have stayed to study in the Helsinki region are, on average, the least likely to move after graduation, whereas those who have moved to study outside the Helsinki region are the most migratory; see the bottom of Table 3. In both regions, people who moved to study are more mobile after graduation (movers) than those who have not changed their region after high school (stayers). Nonetheless, both stayers and movers studying in Helsinki are less mobile than those studying outside Helsinki.

Next, the average marginal effects show that education only has a level effect for stayers studying in the Helsinki region: Migration rates are 5 percentage points higher for people who complete a master's degree than for the other stayers. Outside Helsinki, stayers with a master's degree or a polytechnic degree both have higher migration rates after graduation than vocational school graduates (17.5 % and 7.1 %). Outside Helsinki, movers with a master's degree have a higher migration rate than other graduates.

Individuals who work during their studies are less likely to migrate after graduation than those who do not work. Note, however, that it seems that working while studying outside the Helsinki region particularly strongly attaches movers to the region. Once we condition the level of education, we can see that full-time work is negatively correlated with the migration of stayers outside Helsinki with all levels of education. Other than for stayers outside Helsinki, working while studying is not significantly related to mobility after graduating from vocational education. Part-time work is also a strong signal of the decreased mobility of students who have moved to study at a university in the Helsinki region. In other regions, and for stayers studying in Helsinki, working full-time while studying is more strongly negatively related to migration propensity after graduation.

Notably, working while studying at a polytechnic decreases migration propensity significantly for the stayers but not for the movers. This result applies both to graduates from Helsinki and other regions. The reverse is true for university students: Working while studying at a university slows down migration more for movers than for stayers. In terms of regional policy, these findings suggest that expanding local labour market opportunities before graduation is particularly important for high school graduates who decide to study at the local polytechnic. On the contrary, university students who have moved to study in the region are more affected by local labour market opportunities than stayers. Nonetheless, it is hard to say whether the match between graduates and jobs is improved or deteriorated as a result of early labour market opportunities. Such an investigation requires detailed information on the quality of the jobs that is lacking in our data.

TABLE 3 Average marginal effects on migration by study region and prior migration for studies

Dependent variable: Migration (1/0)	Study region is Helsinki		Study region is not Helsinki	
	(1) Stayer	(2) Mover	(3) Stayer	(4) Mover
Level of education				
Master's degree[a]	0.053**	-0.002	0.175***	0.082**
	(0.021)	(0.043)	(0.020)	(0.038)
Polytechnic degree[a]	0.012	-0.033	0.071***	0.038
	(0.016)	(0.042)	(0.017)	(0.039)
Working while studying				
Full-time work[b]	-0.041***	-0.035	-0.086***	-0.116***
	(0.016)	(0.038)	(0.021)	(0.041)
Part-time work[b]	-0.032**	-0.096**	-0.012	0.094**
	(0.016)	(0.039)	(0.021)	(0.042)
Conditional on master's degree				
Full-time work[b]	-0.065***	-0.068	-0.090**	-0.201***
	(0.024)	(0.049)	(0.035)	(0.054)
Part-time work[b]	-0.028	-0.146***	0.036	-0.149**
	(0.031)	(0.048)	(0.035)	(0.059)
Conditional on polytechnic degree				
Full-time work[b]	-0.068***	0.002	-0.122***	-0.056
	(0.023)	(0.065)	(0.034)	(0.072)
Part-time work[b]	-0.068***	0.022	-0.077**	-0.000
	(0.024)	(0.086)	(0.034)	(0.079)
Conditional on vocational school degree				
Full-time work[b]	-0.011	-0.001	-0.064**	-0.050
	(0.024)	(0.075)	(0.031)	(0.075)
Part-time work[b]	-0.015	-0.103	0.000	-0.104
	(0.024)	(0.084)	(0.033)	(0.078)
Log likelihood	-570	-402	-3,506	-736
Pseudo R-squared	0.082	0.068	0.096	0.094
Average predicted migration rate	0.080	0.223	0.377	0.547
Number of observations	2,233	813	5,852	1,179

Notes: Stayers (movers) are individuals whose study region is (not) the same as their high school region (NUTS3). See also notes to Table 2.

6 Discussion

Our results have shown that working while studying is negatively related to migration propensity even after controlling for many background variables. Hence, our results are consistent with the theoretical view that working while studying expands local labour market opportunities such as networks (and thus stability in the region), which is likely to reduce the propensity to move after graduation. Furthermore, students who work during their studies might disproportionately consist of individuals whose study field matches well with the employment needs of the local industries. Further research is needed in this respect.

6.1 Methodological challenges and possible solutions

A major challenge in empirical research is that individuals make their educational and locational choices according their own preferences and capabilities, which are practically impossible to observe. This self-selection problem can bias estimation results despite the excessive use of individual and region-specific background variables. Thus, we cannot claim that we have established a causal relationship between student employment and later mobility. Instead, our results are primarily descriptive, revealing conditional correlations, and future research should be conducted to identify a causal relationship between student employment and migration propensity.

Causal relationships can be identified, for example, by utilising institutional changes. Many countries have made changes to their education system and related rules during recent decades. These changes – exogenous to individuals' education decisions – can provide opportunities for robust results. The identification of causal effects is also enhanced by the increased availability of micro-level register data that hold information on all individuals (i.e., population) in a specific country (see, e.g., Koster and Venhorst, 2014, for Netherlands). Furthermore, many educational institutions collect extensive student registers that could be linked with employee-employer data. When these registers become available for research, they will allow for controlling for individual heterogeneity in a more prudent manner.

6.2 Avenues for future research

The goal of this chapter has been to draw attention to working while studying, which can hinder the matching between graduates and jobs. Education is a long investment that takes many years to complete, but little is known how students' activities during this period affect their future outcomes. To understand individual decisions after graduation and the mechanisms by which decisions are formed, one needs to consider what happens during the studies and even earlier.

Although there are many avenues for future research, we will provide three suggestions. First, does it matter when the labour market career starts? Many students begin to work at an early age, but not much is known about the benefits of working while studying over studying full-time. In essence, the question is whether youths are making the right labour market decisions. Second, more information is needed on the quality of jobs in which students work. Industrial or occupational mobility during the studies might improve employment opportunities significantly. However, low-skilled jobs (i.e., "McJobs") might place a negative stigma on the students. Third, it would be interesting to study how working while studying relates to firm-level outcomes. Do firms have incentives to invest in the students' human capital because they are particularly prone to moving after graduation? Students might just be seen as a cheap workforce to the firms, in which it is not worth investing.

7 Concluding remarks

Our study has provided one possible explanation of why migration rates have decreased recently over time and why prior studies have found little effect of working while studying on labour market success. We argue that the increase in working while studying has decreased migration rates after graduation, which may have worsened the match between jobs and graduates. Our results show that the negative relationship between working while studying and graduate migration is stronger for higher levels of education and outside the Helsinki region. Prior mobility also plays an important role in determining the relationship between working while studying and graduate migration.

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CHAPTER 4
DROPPING OUT OF UNIVERSITY AND LABOUR MARKET OUTCOMES: EVIDENCE FROM STATE-FUNDED UNIVERSITY SYSTEM*

Abstract

This study examines the relationship between the decision to drop out from university and labor market outcomes. Utilizing a rich register-based random sample of high school graduate cohorts, the research constructs comparison groups for university dropouts in Finland. Matching results indicate that the decision to dropout is related to short-term returns when compared to enrolled student population. If compared to similar university graduates, the dropouts' annual earnings are on average 11,000 euros lower four years after the dropout decision. We find that dropouts have a higher probability of being self-employed than similar graduates.

Keywords: Dropouts, Universities, Earnings, Self-employment.

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1 Introduction

The list of famous names of university dropouts, such as Mark Zuckerberg and Bill Gates, might indicate to undergraduate students that degree from a university is not necessary for later labour market success. Unfortunately, early professional endeavours rarely turns out to be a billion dollar business. Lifetime earnings of college dropouts in the U.S. are only $70,000 above the earnings of high school graduates (Hendricks and Leukhina, 2011). Still, the U.S. Department of Education (2013) reports that less than 60% of undergraduate students who began their studies in 2005 completed a four year bachelor's degree by 2011. Universities in Europe are facing similar problems. Only 46% of students who enrol in Tertiary (A) programs in Italy eventually graduate, while in Finland and Denmark the completion rate is close to 80% (OECD, 2008; 2013). Although country specific statistics on completion rates show that a significant share of student cohorts in Europe never finish their studies, little is known about university dropouts (see NESET, 2013).

From the standpoint of state-funded universities, a high dropout rate is a problem that wastes scarce resources.[1] Starting with Tinto (1975, 1993), higher attention has focused onto identifying a possible mismatch between individuals and educational resources (e.g., Light and Strayer, 2000; Bound and Turner, 2006; Stratton, O'Toole and Wetzel, 2008; Opheim, 2011). Empirical studies, which use exogenous variation for identification, show a causal relationship between financial support and student performance. Dynarski (2003) finds that aid eligibility has a positive effect on college enrolment and completion in the U.S where eligibility rules were changed during the 1980s. A comparison between states reveals that merit-based aid programs have improved college completion (Dynarski, 2008). Gunnes, Kirkebøen and Rønning (2013) show that increased financial aid affects positively on-time graduation in Norway. Garibaldi et al. (2012) find that increases in continuation tuition reduce late graduation in Italy. The study by Arendt (2013) uses Danish student grant reform to study the causal relationship between student grants and dropout behaviour. The results indicate that an increase in student grants decreases the likelihood of dropout, especially among students from low socioeconomic backgrounds. Using a rare randomised experiment, Leuven, Oosterbeek, and van der Klaauw (2010) show that financial incentives have only a small or insignificant effect on course pass rates at the University of Amsterdam. Studies have also utilised educational reforms to study how new educational processes

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1 Although different resource constrains may partly explain the decision to dropout, Manski (1989) argues that dropping out is a result of rational decision making based on learning own abilities rather that social problem which would need policy actions (see also Altonji, 1993; Keane and Wolpin, 1997; Cameron and Heckman, 1998). T.Stinebrickner and R.Stinebrickner (2012) show that around 40% of dropout decisions (in the first and second year of college) could be explained by the fact that individuals learn their academic ability and decide to drop out

could change student behaviour. The introduction of bachelor degrees has not decreased dropout rates among German university students (Horstschräer and Sprietsma, 2013), while a greater degree flexibility in Italian universities has led to a decline in dropout risk (Di Pietro and Cutillo, 2008).

Earlier literature finds that the decision to drop out of a university is related to numerous background factors, such as academic preparedness and ability (e.g., Montmarquette, Mahseredjian and Houle, 2001; Arulampalam Naylor and Smith, 2004; Johnes and McNabb, 2004; Belloc, Maruotti and Petrella, 2010); family backgrounds (e.g., Lassibille and Gomez, 2008; Vignoles and Powdthavee, 2009; Gury, 2011; Aina, 2013); regional labour markets (e.g., Di Pietro, 2006) and working while studying or financial aid (e.g., Glocker, 2011; Hovdhaugen, 2013). Nevertheless, existing empirical literature offers little evidence on how individuals succeed after they dropout from a university.[2] Literature shows that university students tend to overestimate their expected income, especially if they do not succeed in completing their studies (Jerrim, 2013). Thus, the decision to dropout might not lead to a smooth transition into labour markets, and specific policy measures (i.e., providing information on labour market prospects without a degree) might be necessary to support students before they decide whether to drop out.

The purpose of this paper is to examine the relationship between the dropout decision and labour market outcomes over a five-year period after the decision is made. While our main focus is to evaluate possible short-run returns of dropouts compared to university students and university graduates, we also explore important heterogeneities within the dropout group. We expect that the dropout decision is related to larger returns for those who work extensively during their studies (e.g., Hovdhaugen, 2013), while the empirical literature is not clear on whether working while studying has any returns (e.g., Hotz et al, 2002; Häkkinen, 2006). It is also likely that dropping out benefits academically less able students more than able students (e.g., T. Stinebrickner and R. Stinebrickner, 2012) and that male students are more likely to dropout than females (e.g., Arulampalam, Naylor and Smith, 2004).

This paper contributes to the existing literature on university dropouts in four ways. First, little is known about dropouts and how successfully individuals move from universities to labour markets. This study focuses on employment, earnings, unemployment and self-employment over a five-year period after the dropout decision. Second, a new innovative method is used to define the university students and dropout group. We focus on active student populations based on student grant payments data and register information on possible graduation. Third, our paper focuses closely on heterogeneities within the group of dropouts, which could guide future research and educational policy. Fourth, it is clear that the individual decision to dropout is not a random process. Fortunately, our register-based longitudinal data enable us to control a

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2 There is an extensive literature studying the returns to education measured by years of education or by finished degrees (see e.g. Bound and Turner, 2011).

rich number of background covariates that are used to correct possible selection bias.

The remainder of the paper is organised as follows: the next section presents some features of the institutional environment; Section 3 introduces the data; Section 4 presents empirical methodology, results and robustness analysis, and Section 5 concludes the paper.

2 Institutional context

The Finnish university network consists of 15 university-level institutions in 10 city regions. After acceptance, students can choose to study for a bachelor's degree and/or a master's degree. Education is free, and 60% of the total university student population received student aid in 2006 from the Social Insurance Institution. Public financial aid directed to university education consists of three support forms: a monthly student allowance (around 260 euros in 2006, raised to 298 euros in 2008), monthly housing benefits (80% of monthly housing costs, but not more than 252 euros) and a state guaranteed loan (300 euros per month). (SII, 2007, 2008).

There are four constraints to receiving student aid. First, student aid is not paid if a person receives other transfer payments such as pensions. Second, there is an upper limit on the number of support months (mainly 55 months before 2005/2006, then modified to more depending on the field of education). Third, there is a limit on how much a student can earn annually if the student receives monetary aid, without the need to repay the support money. Fourth, students are eligible for student aid if they have completed 5 ECTS per support month in the previous semester. This rule was formed to ensure that student aid is given for full-time studies. (SII, 2008).

There is no official definition of university dropouts (see Rodríguez-Gómez et al. 2013). However, Statistics Finland has statistics on discontinuity of education. A university student is defined to be in the discontinuity group if the student was registered at a university last year but has not graduated or registered in the same institution this year. According to Statistics Finland (2012), 6% of university students belonged to a discontinuity group in 2010.

3 Data

The data are based on a 7% random sample of permanent Finnish residents in 2001. For each sample individual, a rich set of annual variables were collected from register-based files (Longitudinal Census Files and the Longitudinal Employment Statistics Files) by Statistics Finland from the period 1987 to 2006. We constrain our sample in three ways. First, the sample includes only high school graduate cohorts from 1987 to 1998 consisting of students under 21 years old when graduating. Second, we have information on official statistics on public student grant payments for university studies only from 1997 to 2006. We use these payment records with register data on graduation to define three groups: the active university population (= control group 1), university graduates (= control group 2), and university dropouts (= treatment group) for the four year period from 1999 to 2002. The last four years are preserved as a follow-up period for our variables of interests. Third, we further constrain our sample to those who were 24–32 years old when dropping out (see Appendix B Figure B1 for total age distribution) to ensure that our matching variables measured three years before (t-3) the dropout year do not coincide with covariates measured during the high school period. Moreover, if all older cohorts were included, there would be an increasing amount of missing values for some of our variables.

We construct active university populations and dropouts in the following manner. First, our sample of university students consists of individuals who finance their studies (at least partly) by public study allowance during the academic year or in a previous two-year period. This definition of student population does not include those individuals who used only wage earnings or other income sources to finance their studies. The definition also excludes those individuals who are registered in a university level institution but who do not complete enough courses to be eligible to receive any student allowance during the calendar year. Incentives to register in a university are high in Finland, even without any desire to attend courses, because registered individuals enjoy a wide variety of student discounts (e.g., 50% discount for public transportation) after paying a modest registration fee (annually around 100 euros). Second, the individual decision to dropout is defined by combining register information on student allowance payments and completed university degrees. An individual is defined as a dropout if the person received a student allowance in the previous year but did neither received any allowance and nor graduated from any university during the next five years.[3] It is not possible to determine exactly when an individual decided to dropout, as one can receive student allowance payments throughout the semester even when taking courses just during the first few months. Still, we have acknowledged this situation in our empirical analysis as our control variables were measured well before and thus should not be endogenous on the individual decision to drop out.[4]

We perform our analysis using covariates year before the high school graduation year and three years before the dropout decision. Following suggestions by Lechner and Wunsch (2013), our propensity score approach utilises rich information on basic socio-demographic variables, pre-treatment

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3 Individuals can dropout from university and continue studying at another level of education. Still, if we would exclude individuals who continue studying in non-university institution (such as polytechnics), our results would not chance qualitatively.

4 In our study period, percentage of dropouts relative to active university population is 5% (1999–2002) which is 1% point less than official figures on study discontinuity in Finland (Statistics Finland, 2012). See Appendix B Figure B2 for more details.

outcomes, regions and short- and long-run labour market histories that are important for removing biases. In addition, our control variables contain covariates on parents' education, as well as high school grades in mathematics and language. To be more precise, covariates for prior high school graduation include individual (high school graduation age, sex, language, marital status, children, earlier work experience, earlier vocational studies, high school grades and region) and parental information (education and socioeconomic class). We also control multiple variables three years prior to the base year (work experience, earnings, earnings growth, self-employment, unemployment, marital status, number of children and house ownership). Detailed variable definitions are listed in Appendix Table A1.

Table A2 in the Appendix A displays the descriptive statistics for the dependent and independent variables. Dependent variables are measured at t+4. Annual wage earnings for dropouts are on average 19,000 euros, which is 8,000 euros less than what graduates earn on average (prices are deflated using Consumer-price-index 2006). Dropouts' self-employment propensity is 4 %, which is double the average for self-employment propensity among the graduates group. Average independent variables indicate, for example, that dropouts are more likely to be males who are not married (at t-3 period) and who work more than the average population (during the high school but not three years before dropout year). There are no large differences in parents' education or socioeconomic status, but high school grades indicate that dropouts have lower success in high school.

4 Empirical strategy and results

4.1 Econometric approach

We are interested in estimating the average treatment effect on treated (ATT). Individuals' either graduate ($T_i = g$), dropout ($T_i = d$) or stay enrolled ($T_i = s$), but no more than one outcome is possible at the same time. We estimate propensity for dropouts ($P^d(X)$) relative to the student population and graduates. Variable specific coefficients from these selection equations are shown in Appendix Table A3.[5] The average treatment effect for the treated is measured by comparing weighted outcomes of those who dropout ($Y^d | P^d(X), T_i = d$) to those who remain enrolled ($Y^s | P^s(X), T_i = s$) or to those who graduate ($Y^g | P^g(X), T_i = g$). Thus, ATT for dropouts is (see e.g., Imbens and Wooldridge, 2009):

$$ATT = \left(Y^d - Y^s \middle| P^d(X),\ P^s(X),\ T = d\right) = \frac{1}{\mathrm{N_d}} \sum_{\mathrm{i=1}}^{N_d} \left(Y_i^d - \sum_{\mathrm{j=1}}^{N_s} \mathrm{W}(i,j)\, Y_j^s\right) \qquad (1)$$

5 Marginal effects in Appendix in Table A3 show, for example, that males have a higher propensity to dropout, while higher grades in math and language are related to lower dropout propensity (especially if compared to graduates).

Where Y_i^d denotes the outcome for individual (i) who dropouts and Y_j^s denotes the outcome for individual (j) in the control group constructed from the student population (control group can be constructed similarly from graduates). By calculating the propensity to dropout for each individual, one can form a comparison group by weighting the control group individual (j) according the selected matching method. This study uses nearest neighborhood matching with two nearest neighbor. In this case, the weight is one for the two closest control group observations (by propensity score arranged from the smallest to largest value) and zero otherwise. For robustness, we also explore in great detail how different matching methods and trimming strategies affect the conclusions (following suggestions by Huber, Lechner and Wunsch, 2013).

Our identification assumption is based on the condition that by using a rich set of background variables, the conditional propensity to dropout of a university is a mean independent of potential outcomes (conditional independence assumption, CIA).[6] For example, Lechner and Wunsch (2013) show that studies on active labour market programs are not significantly biased if matching analysis includes detailed background variables. It should also be emphasized that our matching variables are measured well before the decision to drop out. To avoid any indigeneity concerns, we do not control for educational field or particular institution, as these covariates could be considered bad controls (see Angrist and Pischke, 2009, 64).

4.2 Main results

Figures 1 and 2 depict dynamic ATT estimates on annual employment months, annual wage earnings, annual unemployment months and year-end self-employment propensity before and after the dropout decision. In Figure 1, dropouts are compared to similar individuals matched to the active student population, while in Figure 2 dropouts are matched to similar university graduates. ATT estimates are performed using the nearest neighborhood matching (n=2). We impose common support conditions that exclude only a few observations. Estimated coefficients with standard errors are shown in Appendix Table A4.

Our results presented in Figure 1 show that dropouts' annual work months and earnings increase significantly the year before the dropout year compared to otherwise similar university students. While annual wage premium (work months) of dropouts is on average 6,900 euros (1.7 months) at period t, returns decline rapidly after the dropout year. Average dropouts' earnings are 1,500 euros lower than in control group (statistically significant at the 10% level) four years after. This outcome is understandable, because students graduate, transition into full-time employment and catch up to the earnings of dropouts. The lower part of Figure 1 shows ATT estimates on the annual un-

[6] Addition to CIA, assumption on overlap is also necessary. One can compare individuals only when there are observations in both groups with similar qualities (see Web Appendix Figures W3 and W4 for propensity score distributions).

employment and self-employment propensity. Dropping out is positively related to unemployment over a two-year period, but this relationship is not significant three or four years after. The relationship between the drop out decision and self-employment emerges strongly form the annual estimates. Dropouts have a 3% point's higher probability of being self-employed four years after the dropout year.

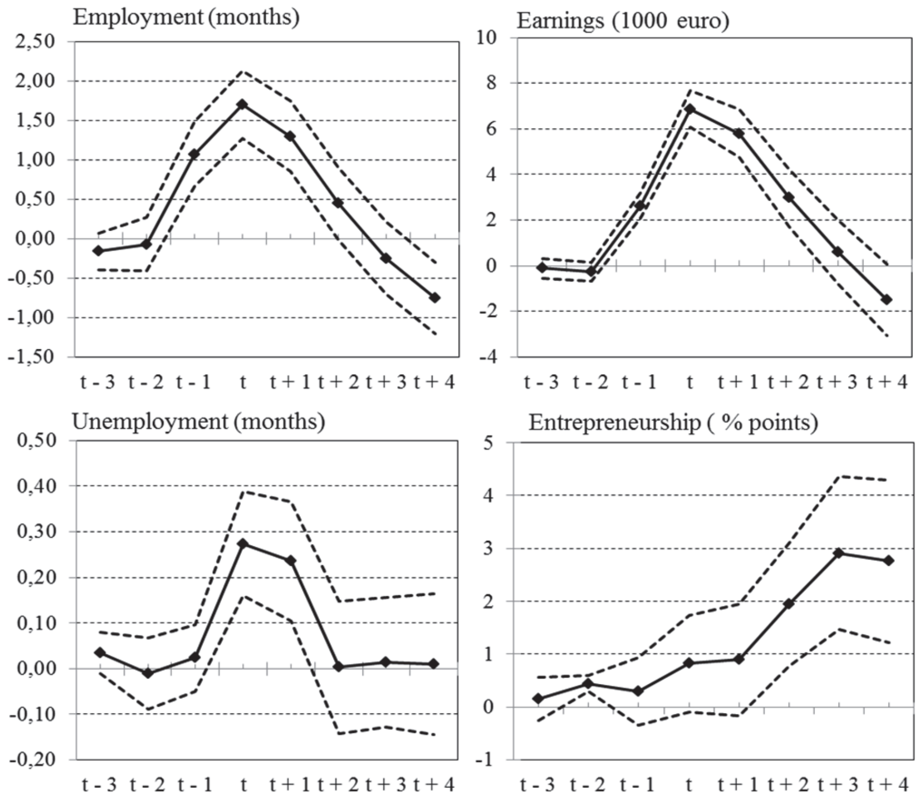

FIGURE 1 Average treatment effects on dropouts relative to active student population. Note: Solid line indicates dropout group and dashed lines show 95% confidence intervals.

In Figure 2, we compare university dropouts to otherwise similar university students who graduate at period t. Contrary to our earlier result, we find that dropping out is related to a significant negative decline in annual work months and earnings one to four years after the dropout year. Dropouts' wages are on average 2,500 euros lower than graduates at period t. Four years after the dropout year, dropouts work two months less and earn 12,000 euros less than university graduates. A modest decline in work months might indicate that dropouts work in temporary jobs, which are recorded as full employment months in labour statistics. Figure 2 also shows that the dropout decision is negatively related to unemployment in periods t and t+1. The difference from our earlier results is explained by the fact that official unemployment benefits are available after graduation, but registered students cannot collect these benefits. Finally, if we compare dropouts to university graduates, the dropout decision is still related to a 3% point's higher self-employment probability four years after the dropout year.

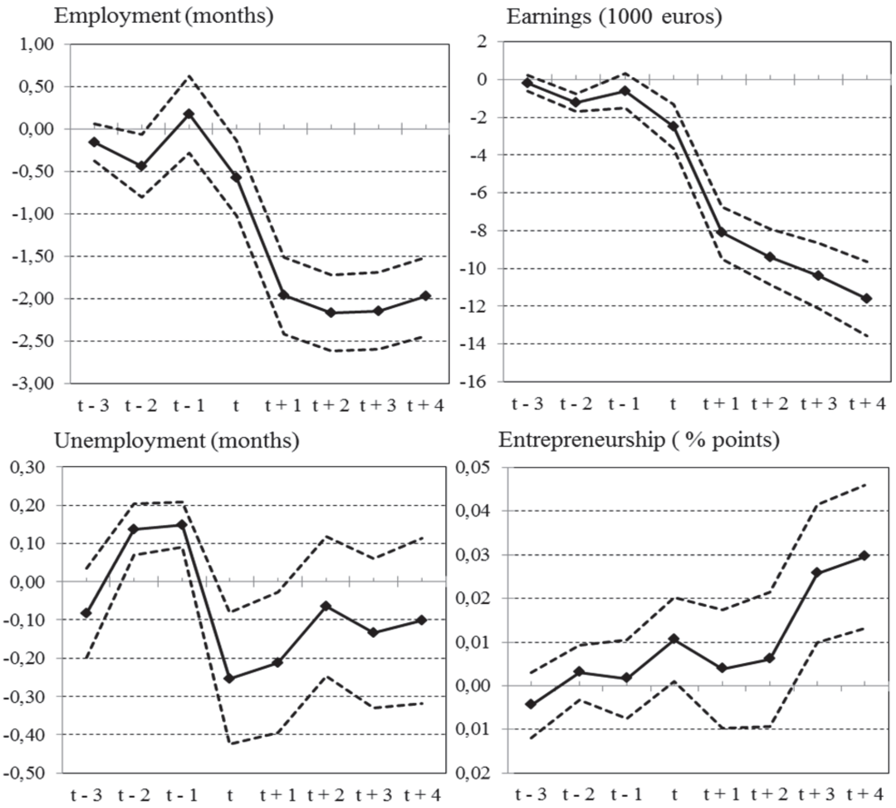

FIGURE 2 Average treatment effects on dropouts relative to university graduates (master). Note: Solid line indicates dropout group and dashed lines show 95% confidence intervals.

4.3 Heterogeneity

Similar to our findings, earlier literature has shown that some groups of individuals are more likely to dropout from university than others (e.g., Lassibille and Gomez, 2008; Gury, 2011; Hovdhaugen, 2013). It is possible that dropping out is related to higher returns for those who dropout in higher probability. Tables 1 and 2 report ATT estimates by gender, earlier work experience and academic ability.

Results in Panel A of Table 1 indicate that females' dropout decision is related to a significant decline in work months and earnings compared to similar university students. We do not find a similar significant relationship for males, although their self-employment propensity is 1% higher than it is for females. Dropouts, who worked 9–12 months three years before the dropout year, do not experience a significant decline in earnings, whereas the decline is larger in groups with less work experience (Panel B). For the same group, the dropout decision is also insignificantly related to self-employment, which might indicate that those who start a career during their studies continue similarly after the dropout decision. Finally, Panel C shows how the results differ by academic ability. As university course grades are not available, we use high school matriculation exam grades as proxies for different academic ability. ATT estimates indicate that the dropout decision is negatively related to employment and earnings for high grade dropouts. It also seems that a positive relationship between dropout propensity and self-employment is not significant for the lower part of the ability distribution.

In Table 2, we compare dropouts to similar university graduates instead of university students. For both genders, the dropout decision is related to a 40% (9,000-12,000 euros) decline in annual earnings four years later. Contrary to the earlier results in Table 1, the dropout decision is not related to female self-employment propensity when compared to otherwise similar university graduates. Still, we observe that the self-employment propensity is higher for those who worked less than nine months three years prior to the dropout year (Panel B) and for those who have average grade success (Panel C).

TABLE 1 ATT for dropouts by gender, earlier work experience and academic ability relative to student population

Panel A: Gender			
	All	Female	Male
Employment (t + 4)	-0.749***	-1.067***	-0.305
	(0.231)	(0.309)	(0.274)
Earnings (t + 4)	-1 507*	-1 912**	-825
	(793)	(878)	(1 092)
Unemployment (t + 4)	0.010	-0.063	0.049
	(0.079)	(0.089)	(0.125)
Propensity of self-employment	0.028***	0.031***	0.041***
(t + 4)	(0.008)	(0.010)	(0.012)
Treated (off support):	670	3234 733	344 (3)
Control group:	8 518		3 781
Panel B: Work experience (t-3)			
	0 – 3 months	4 – 8 months	9 – 12 months
Employment (t + 4)	-0.709**	-0.491	-0.464
	(0.282)	(0.359)	(0.306)
Earnings (t + 4)	-1 018	-4 204***	69
	(913)	(1 500)	(1 357)
Unemployment (t + 4)	-0.014	0.126*	0.004
	(0.110)	(0.073)	(0.127)
Propensity of self-employment	0.023**	0.053***	-0.013
(t + 4)	(0.010)	(0.013)	(0.017)
Treated (off support):	387	170	113
Control group:	4 704	2 273	1 480
Panel C: Academic ability			
	Low grade	Average grade	High grade
Employment (t + 4)	-0.813**	-0.393	-0.980***
	(0.352)	(0.376)	(0.327)
Earnings (t + 4)	-389	-1 509	-2 952**
	(1 120)	(1 416)	(1 196)
Unemployment (t + 4)	-0.072	-0.031	0.133
	(0.191)	(0.117)	(0.099)
Propensity of self-employment	0.018	0.064***	0.028***
(t + 4)	(0.012)	(0.011)	(0.007)
Treated (off support):	256	163	250 (1)
Control group:	2 583	2 265	3 647

*** (**,*) = significant at 1 % (5%, 10%) level; standard errors in parentheses. *Low grade:* No grade or lowest grades 1 to 3 (I, A, B) from matriculation exam from B-level mathematics or from first language. *Average grade:* Matriculation exam grades 4 to 5 (C or M) from B-level mathematics (also I, A or B from A-level) or first language grade C or M. *High grade:* Highest grade 6 (L or E) form A- or B-level mathematics or from first language.

TABLE 2 ATT for dropouts by gender, earlier work experience and academic ability relative to graduates

Panel A: Gender			
	All	Female	Male
Employment (t + 4)	-1.975***	-2.680***	-1.348***
	(0.238)	(0.371)	(0.299)
Earnings (t + 4)	-11 595***	-9 068***	-12 278***
	(997)	(986)	(1 480)
Unemployment (t + 4)	-0.102	-0.266*	0.157
	(0.109)	(0.142)	(0.108)
Propensity of self-employment	0.030***	0.003	0.043***
(t + 4)	(0.008)	(0.011)	(0.009)
Treated (off support):	670 (9)	308 (15)	340 (7)
Control group:	2 376	1 311	1 062
Panel B: Work experience (t-3)			
	0 – 3 months	4 – 8 months	9 – 12 months
Employment (t + 4)	-2.277***	-1.825***	-0.990**
	(0.351)	(0.426)	(0.432)
Earnings (t + 4)	-11 393***	-14 645***	-12 750***
	(1 177)	(2 046)	(2 337)
Unemployment (t + 4)	-0.121	0.141	-0.018
	(0.125)	(0.160)	(0.230)
Propensity of self-employment	0.039**	0.025**	0.004
(t + 4)	(0.011)	(0.011)	(0.012)
Treated (off support):	377 (10)	163(7)	111 (2)
Control group:	1 183	666	506
Panel C: Academic ability			
	Low grade	Average grade	High grade
Employment (t + 4)	-2.413***	-1.991***	-2.067***
	(0.388)	(0.312)	(0.327)
Earnings (t + 4)	-11 794***	-10 889***	-10 602**
	(1 675)	(1 437)	(1 332)
Unemployment (t + 4)	-0.083	0.090	0.096
	(0.167)	(0.137)	(0.134)
Propensity of self-employment	0.022	0.068***	0.012
(t + 4)	(0.015)	(0.018)	(0.014)
Treated (off support):	247 (5)	161 (2)	246 (5)
Control group:	567	625	1178

*** (**,*) = significant at 1 % (5%, 10%) level; standard errors in parentheses. *Low grade:* No grade or lowest grades 1 to 3 (I, A, B) from matriculation exam from B-level mathematics or from first language. *Average grade:* Matriculation exam grades 4 to 5 (C or M) from B-level mathematics (also I, A or B from A-level) or first language grade C or M. *High grade:* Highest grade 6 (L or E) form A- or B-level mathematics or from first language.

4.4 Robustness

The identification strategy of this paper is based on rich set of background variables that are used to control the non-random process of dropping out of a university (CIA assumption). Thus, it should be recognized that our results are based on propensity score matching and are not a proof of causal relationship between the individual decision to drop out and the resulting labor market outcomes. It is not possible to know if some unobserved characteristics (such as inner motivation or inborn tendency to depression) are over or under represented in our treatment groups compared to our control groups (see e.g., Murnane and Willet, 2011, 44).

It is informative to assess whether our results are robust to different possible sources of biases. Our matching procedure succeeds to balance the data so that there are no observed differences between treated and untreated groups, and that visual inspection of propensity score distributions shows that we have relatively good common support for our estimates. We also evaluate how the inclusions of different matching variables affect our results by incrementally adding variables into our propensity estimations. Our concern is that some of our covariates measured three years before the dropout year might be endogenous. We repeat the estimations using variables from the high school period and five years before the dropout decision. In addition, we test different trimming and matching strategies to assess result robustness (e.g., Lechner and Wunsch, 2013). These modifications have only a marginal effect on our results (see Appendix B Table B3).

It is possible to study whether the unconfoundedness assumption (CIA) holds indirectly. A significant "placebo" effect before the actual treatment period (dropout or graduation decision) would indicate that that our results are driven by some other factor than our variable of interest. When we use university student populations as our comparison group, results do not show any significant effect two years before the dropout year. Significant results one year before the dropout year underline the fact that it is hard to point to the exact timing of the dropout decision. Youth can receive student benefits in the first months of the academic year and dropout later in the year without the need to pay back the received student aid. Also, when we compare dropouts to graduates, the placebo effect is significant two years before dropout year, but not significant three and one year before (see Appendix Table A4). This finding might indicate that some unobserved factors might explain, at least partly, the observed differences between the treated and untreated groups.

We also assess how sensitive our results are for hidden bias. We do this by calculating upper and lower bounds on the used test-statistics that show how large unobserved bias should be (upwards or downwards) so that our results become insignificant (see e.g., Rosenbaum, 2002; 2005; Guo and Fraser, 2010, 297). We focus on the study of sensitivity of earnings and self-employment propensity four years after the dropout decision. Two main findings emerge from sensitivity analysis. First, unobserved factor(s) should increase the odds of drop out by 1.4–1.6 so that the relationship between the dropout decision and self-employment would become insignificant at the 5% level. Second, the relationship between the dropout decision and earnings loss four years later is robust, when dropouts are compared to university graduates. Odds change should be over tenfold so that our earnings coefficient would become insignificant for the 5% level (see Appendix B Table B4).

5 Conclusions

This study examines several labor market outcomes after the decision to drop out of a university. Propensity score-matching results indicate that dropping out is related to modest and short-term labor market returns when compared to similar university students who do not dropout or graduate. When dropouts are compared to similar university graduates, the decision to drop out is related to significant annual earnings loss (annually over 11,000 euros four years later). On a positive note, dropouts have only a small amount of official unemployment months over a five-year period, and dropouts have a higher self-employment propensity than university graduates. We also find that there are differences in labor market outcomes by gender, early work experience and academic ability.

There are at least four recommendations to offer for future research and policy makers. First, studies should utilize quasi-experimental settings (e.g., changes in levels of student grants or course requirement rules) to study causal relationship of dropout decision and later labor market outcomes. An important limitation of this study is that our results cannot be interpreted strictly in a causal manner. Second, it is surprising how many of the university students choose to drop out compared to the number of university graduates. In our sample, there is one university dropout per four university graduates. If the number of dropouts is compared to the total student population, one might make a false conclusion about how significant problem the dropout phenomenon is. It is clear that national statistics on university dropouts would need to be improved to analyses dropout phenomena more prudently. Third, results of this study show that average individuals' earnings related to the dropout decision are mainly negative in the short-run. Future research should study the long-run effects of the dropout behavior and compare possible returns to public resources used by the dropouts. Fourth, it would be interesting to estimate how large are the indirect costs the dropout phenomenon could cause for society when the supply of university education is fixed as it is in Finland. University dropouts might displace other individuals, who would have a higher probability to graduate, from accessing the university.

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Appendix A

TABLE A1 Variable Description

Variable	Description
Dependent variables	
Employment	Annual months of employment (0–12).
Annual Earnings	Annual earnings (does not include any transfer payments)
Unemployment	Annual months of unemployment (0–12)
Self-employed	1 = Self-employed, 0 = other. Person is defined as self-employed by the end of the year employment status.
Treatment variable	
Dropout	Individual has dropped out from university (see text)
Control variables	
Age	HS graduation age in years.
Age2	Age squared/100.
Female	Female = 1, otherwise e= 0.
Swedish	Swedish speaking = 1, otherwise = 0.
Children	At least one children year before grad. form HS =1, otherwise = 0.
Married	Married year before grad. form HS =1, otherwise = 0.
HS work experience	Sum of work months over two year period before HS graduation year divided by 12 (= years of experience)
HS experience2	Experience (years) squared
Mother's highest education	1 = basic education, 2 = secondary education, 3 = tertiary education
Father's highest education	1 = basic education, 2 = secondary education, 3 = tertiary education
Parents field of education	Eight categories: 1 = Education, 2 = Humanities/art, 3 = Social science/business/law 4 = Science, 5 = Technical, 6 = Agriculture, 7 = Services 8 = No specific educational field for parents.
Father's socioeconomic background 1990	Six categories: 1= self-employed, 2 = high-ranking official, 3 = low-ranking official, 4 = manual worker, 5 = studies or is retiree, 6 = unemployed.
Math grade (B–level)	Five categories for highest high school matriculation grade on math: 1 (worst), to 5 (best).
Math grade (A–level)	Five categories for highest high school matriculation grade on advanced math: 1 (worst) to 5 (best) and a category for missing grade.
Language grade	Six categories for highest high school matriculation grade on first language: 1 (worst) to 5 (best) and a category for missing grade.
Grade(s) not available	Math or language grade is missing = 1, otherwise = 0.
Vocational degree	Individual has vocational degree = 1, otherwise = 0. (defined two years before)

TABLE A1 (Cont.) Variable Description

Higher vocational diploma	Individual has higher vocational diploma from polytechnic = 1, otherwise = 0. (defined two years before)
Work experience (t-3)	Sum of annual months of employment three years before.
Earnings (t-3)	Sum of annual earnings/1000 three years before
Earnings growth (t-3)	Difference of annual earnings three and four years before/1000.
Unemployment (t-3)	Sum of annual months of unemployment three years before.
Entrepreneur (t-3)	1 = Entrepreneur, 0 = other, three years before.
Married (t-3)	1 = Married, 0 = other, three years before.
Number of kids (t-3)	Number of kids three years before.
Owns a house/flat (t-3)	Owns a house or flat three years before =1, otherwise = 0.
High school region	Dummies for 20 high school region (NUTS3)
Year	Dummy variable indicating years from 1999 to 2002.

Note: Åland Island is not included in our sample (Isolated and small island region in south-west Finland).

TABLE A2 Descriptive statistics on selected variables

Variables:	Control group 1 (Student pop.)		Control group 2 (Graduates)		Dropouts	
	Mean	s.d.	Mean	s.d.	Mean	s.d.
Outcome variables(t+4)						
Employment (months)	8.5	4.65	10.10	3.83	7.87	5.21
Earnings (euro)	20442	15776	27367	19130	19390	17702
Unemployment (months)	0.40	1.51	0.37	1.49	0.39	1.77
Self-employment propensity	0.02	0.12	0.02	0.13	0.04	0.21
Covariates from high school (S) period						
Hs age	19.16	0.40	19.11	0.35	19.16	0.41
Female	0.56	0.50	0.55	0.50	0.48	0.50
Swedish	0.06	0.24	0.07	0.26	0.07	0.25
Children	0.02	0.12	0.02	0.14	0.02	0.13
Married	0.00	0.07	0.00	0.06	0.00	0.07
High school work exp. (X)	0.17	0.34	0.19	0.33	0.24	0.40
X^2	0.14	0.51	0.15	0.45	0.22	0.61
Mother has basic educ.	0.19	0.39	0.20	0.40	0.20	0.40
… has secondary educ.	0.29	0.46	0.30	0.46	0.30	0.46
… has tertiary educ.	0.52	0.50	0.50	0.50	0.50	0.50
Father has basic educ.	0.23	0.42	0.23	0.42	0.23	0.42
… has secondary educ.	0.24	0.43	0.22	0.41	0.24	0.43
… has tertiary educ.	0.53	0.50	0.55	0.50	0.53	0.50
Parent´s field = Education	0.08	0.27	0.08	0.28	0.06	0.25
…= Humanities/art	0.09	0.29	0.08	0.27	0.08	0.27
…= Social sci./business/law	0.32	0.46	0.32	0.47	0.33	0.47
…= Science	0.05	0.22	0.05	0.22	0.05	0.22
…= Technical	0.38	0.48	0.36	0.48	0.39	0.49
…= Agriculture	0.07	0.25	0.08	0.26	0.06	0.23
…= Health	0.24	0.43	0.23	0.42	0.22	0.42
…= Services	0.11	0.32	0.12	0.33	0.11	0.32
…= No educ. field for parents	0.44	0.50	0.43	0.50	0.46	0.50
Father is self-employed	0.15	0.36	0.17	0.37	0.15	0.36
… is high-ranking official	0.38	0.48	0.39	0.49	0.37	0.48
… is low-ranking official	0.18	0.39	0.16	0.37	0.18	0.38
… is manual worker	0.18	0.38	0.17	0.37	0.17	0.38
… studies or is retiree	0.04	0.20	0.05	0.22	0.05	0.21
… is unemp./missing	0.07	0.25	0.07	0.26	0.09	0.28
Math grade missing	0.24	0.43	0.21	0.40	0.31	0.46
Math grade (B-level)=1 (worst)	0.05	0.21	0.03	0.17	0.04	0.19
… = 2	0.06	0.24	0.04	0.19	0.06	0.23
… = 3	0.06	0.24	0.05	0.21	0.04	0.21
… = 4	0.08	0.27	0.08	0.28	0.08	0.27

TABLE A2 (Cont.) Descriptive statistics on selected variables

	Mean	s.d.	Mean	s.d.	Mean	s.d.
… = 5 (best)	0.08	0.27	0.07	0.26	0.06	0.23
Math grade(A level)=1(worst)	0.01	0.09	0.01	0.07	0.01	0.10
… = 2	0.07	0.25	0.05	0.23	0.06	0.23
… = 3	0.11	0.31	0.11	0.32	0.10	0.30
… = 4	0.12	0.32	0.15	0.36	0.11	0.32
… = 5 (best)	0.13	0.33	0.20	0.40	0.12	0.33
Language grade missing	0.06	0.23	0.09	0.28	0.13	0.33
Language grade = 1(worst)	0.02	0.13	0.01	0.09	0.03	0.16
… = 2	0.06	0.23	0.03	0.17	0.05	0.23
… = 3	0.22	0.42	0.18	0.38	0.21	0.41
… = 4	0.32	0.47	0.32	0.47	0.31	0.46
… = 5 (best)	0.32	0.47	0.37	0.48	0.28	0.45
Vocational degree	0.16	0.36	0.10	0.30	0.13	0.33
Higher vocational diploma	0.06	0.25	0.11	0.31	0.04	0.19
Control covariates after high school						
Work experience (t-3) (months)	4.15	4.06	4.74	4.17	3.83	4.17
Earnings (t-3) (1000 euro)	3.98	4.23	5.09	5.23	3.84	4.81
Earnings growth (t-3) (1000 euro)	0.88	3.42	1.13	3.76	0.74	4.13
Unemployment months (t-3)	0.64	1.87	0.01	0.07	0.25	1.21
Entrepreneur (t-3)	0.00	0.07	0.06	0.53	0.01	0.09
Married (t-3)	0.05	0.21	0.09	0.29	0.06	0.24
Number of kids (t-3)	0.19	0.56	0.17	0.53	0.15	0.44
Owns a house/flat (t-3)	0.47	0.50	0.45	0.50	0.45	0.50
Year dummies						
Year 1999	0.23	0.42	0.25	0.43	0.24	0.43
Year 2000	0.25	0.43	0.23	0.42	0.27	0.45
Year 2001	0.26	0.44	0.25	0.44	0.24	0.43
Year 2002	0.26	0.44	0.26	0.44	0.25	0.43
Observations	8518		2381		670	

Note 1: High school regions (20), age squared omitted from the table.
Note 2: Descriptive statistics on annual outcome variables are reported in Table B1 in Appendix B.

TABLE A3 Determinants of dropout (marginal effects)

Dependent binary variable: Dropping out	(A) Control group 1 (Student pop.)		(B) Control group 2 (Graduates)	
	Coeff.	s.e.	Coeff.	s.e.
HS age	-0.378	0.459	-2.001	1.277
Hs age^2	0.942	1.184	5.260	3.294
Swedish	-0.002	0.012	-0.057	0.034
Female	-0.024***	0.006	-0.086***	0.017
Married	-0.006	0.039	0.140	0.113
Children	0.019	0.021	-0.016	0.058
High school work exp. (X)	0.066***	0.018	0.008	0.050
X^2	-0.024**	0.012	0.038	0.038
Father is self-employed	-0.015	0.012	-0.023	0.032
…is high-ranking official	-0.019*	0.011	-0.021	0.032
…is low-ranking official	-0.018	0.011	0.008	0.033
…is manual worker	-0.019	0.011	-0.019	0.033
…studies or is retiree	-0.014	0.015	-0.033	0.044
…is unemployed	Ref.		Ref.	
Math grade missing	0.014	0.014	0.001	0.017
Math grade (B-level) = 1	Ref.		Ref.	
… = 2	0.006	0.017	0.029	0.055
… = 3	-0.012	0.017	0.016	0.054
… = 4	0.012	0.016	0.043**	0.048
… = 5	-0.013	0.017	0.024	0.050
… = 6 (A-level)	0.009	0.029	-0.006	0.095
… = 7	-0.006	0.017	-0.037	0.052
… = 8	-0.009	0.015	-0.071	0.047
… = 9	-0.006	0.015	-0.098**	0.046
… = 10	-0.010	0.015	-0.132***	0.047
Language grade missing	0.021	0.020	-0.182	0.169
Language grade = 1	Ref.		Ref.	
… = 2	-0.022	0.020	-0.011	0.072
… = 3	-0.026	0.018	-0.171***	0.064
… = 4	-0.022	0.018	-0.183***	0.063
… = 5	-0.026	0.018	-0.208***	0.064
Vocational degree	-0.017**	0.008	0.024	0.024
Higher vocational diploma	-0.029**	0.012	1.185***	0.032
Work experience (t-3)	-0.002**	0.001	-0.002	0.002
Earnings (t-3)	- 0.001	0.001	-0.011***	0.003
Earnings growth (t-3)	- 0.001	0.001	0.006**	0.002
Unemployment months (t-3)	-0.012***	0.002	0.044***	0.010
Entrepreneur (t-3)	0.029	0.033	0.036	0.091
Married (t-3)	0.021*	0.012	-0.061**	0.029
Number of kids (t-3)	-0.012**	0.006	-0.006	0.016
Owns a house/flat (t-3)	-0.004	0.005	-0.007	0.016
Parent´s educ. dummies	Yes		Yes	
HS region & base year dummies	Yes		Yes	
Propensity score	0.073		0.220	
Number of observations	9 188		3 051	

*** (**,*) = significant at 1%, (5%, 1%) level; standard errors in parentheses. Marginal effects evaluated at means.

TABLE A4 Labour market outcomes before and after dropout (ATT)

	Matching results (Nearest neighbourhood matching; n=2)							
	Employment (months) (1)		Wage earnings (euro) (2)		Unemployment (months) (3)		Self-employment(% points) (4)	
Panel A: Dropouts relative to student population (Control group 1)								
T - 3	-0.163	(0.118)	-116	(225)	0.035	(0.023)	0.001	(0.002)
T - 2	-0.070	(0.175)	-273	(205)	-0.011	(0.040)	0.004***	(0.001)
T - 1	1.073***	(0.210)	2635***	(281)	0.023	(0.037)	0.003	(0.003)
T	1.699***	(0.217)	6869***	(405)	0.274***	(0.058)	0.008*	(0.005)
T + 1	1.297***	(0.228)	5802***	(535)	0.235***	(0.067)	0.009*	(0.005)
T + 2	0.447*	(0.234)	2966***	(635)	0.003	(0.074)	0.019***	(0.006)
T + 3	-0.250	(0.234)	585	(717)	0.014	(0.073)	0.029***	(0.007)
T + 4	-0.749***	(0.231)	-1507*	(793)	0.010	(0.079)	0.028***	(0.008)
Dropouts (off):	670		670		670		670	
Control group:	8 518		8 518		8 518		8 518	
Panel B: Dropouts relative to graduates (Control group 2)								
T - 3	-0.162	(0.111)	-202	(222)	-0.082	(0.060)	-0.005	(0.004)
T - 2	-0.438**	(0.188)	-1223***	(245)	0.137***	(0.034)	0.003	(0.003)
T - 1	0.169	(0.229)	-602	(472)	0.149***	(0.030)	0.002	(0.005)
T	-0.580**	(0.227)	-2498****	(591)	-0.253***	(0.088)	0.011**	(0.005)
T + 1	-1.964***	(0.230)	-8116***	(701)	-0.211**	(0.094)	0.004	(0.007)
T + 2	-2.172***	(0.228)	-9387***	(757)	-0.064	(0.093)	0.006	(0.008)
T + 3	-2.145***	(0.230)	-10381***	(876)	-0.134	(0.100)	0.026***	(0.008)
T + 4	-1.975***	(0.238)	-11595***	(997)	-0.102	(0.110)	0.030***	(0.008)
Graduates (off):	661 (9)		661 (9)		661 (9)		661 (9	
Control group:	2 372		2 372		2 372		2 372	

*** (**,*) = significant at 1 % (5%, 10%) level. *Note:* AI robust standard errors in parentheses; see Abadie and Imbens (2009).

Appendix B

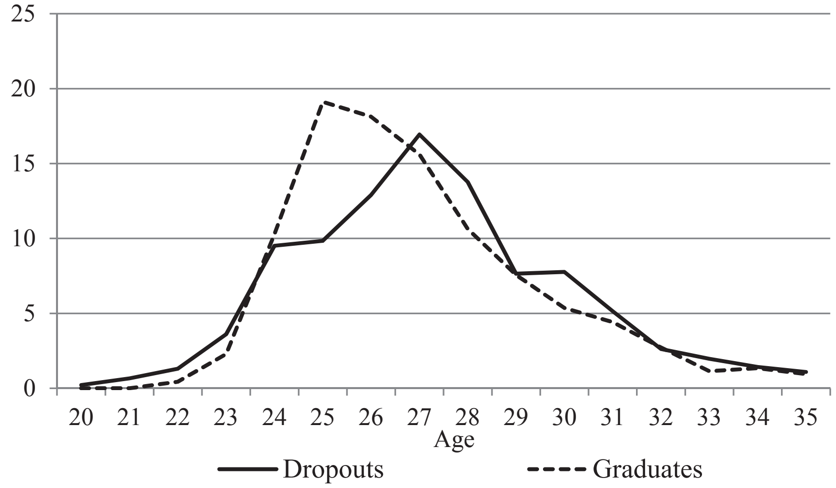

FIGURE B1 Percentage of university dropouts and graduates (master degree) by age (years 1999–2002). Calculations consist of individuals who were 17-20 years old when graduating from high school. *Source:* Own calculations based on random sample from Statistics Finland.

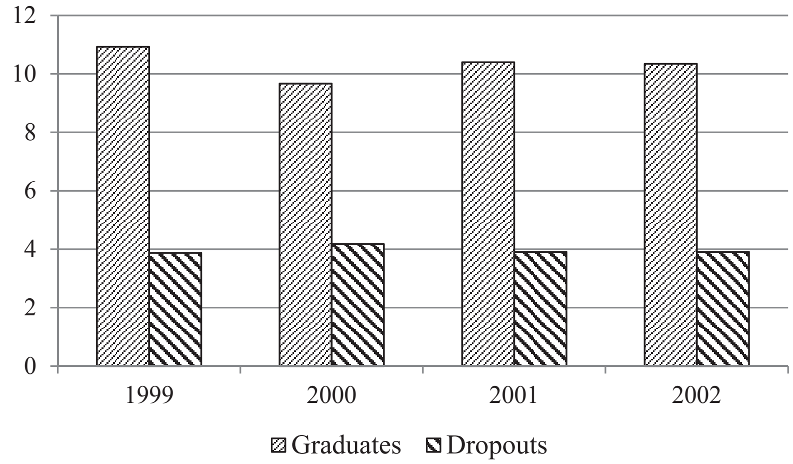

FIGURE B2 Percentage of university graduates (master degree) and dropouts relative to active university population. Calculations consist of individuals who were 17-20 years old when graduating from high school. *Source:* Own calculations based on random sample from Statistics Finland.

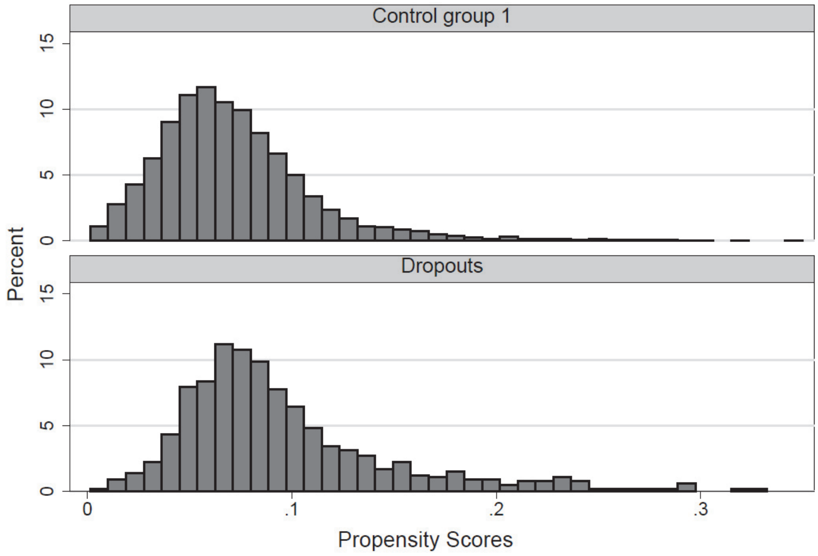

FIGURE B3 Common support for dropouts relative to student population

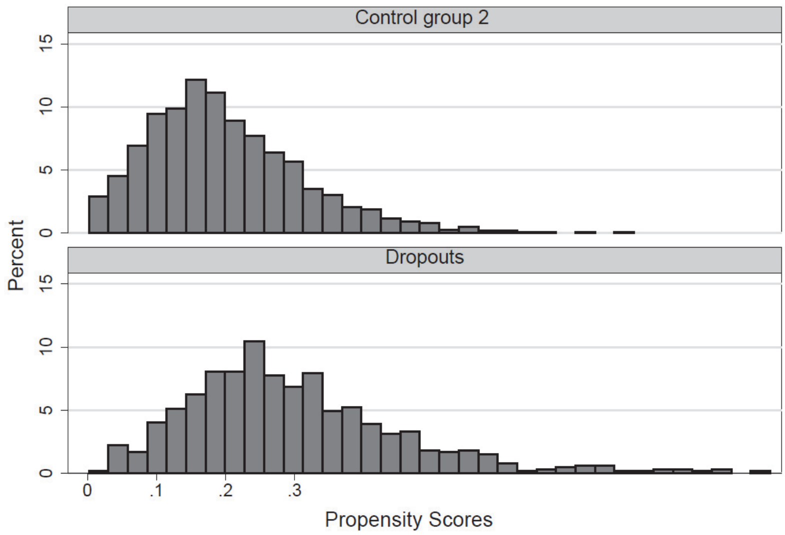

FIGURE B4 Common support for dropouts relative to university graduates (master)

TABLE B1 Descriptive statistics on outcome variables before matching

Variable	Control group 1 (Stud. pop.)		Control group 2 (Graduates)		Dropouts	
	Mean	s.d.	Mean	s.d.	Mean	s.d.
Annual employment (months)						
T-3	4.153	4.064	4.739	4.171	3.833	4.166
T-2	4.750	4.177	5.589	4.296	4.654	4.478
T-1	5.269	4.304	6.384	4.461	6.263	4.752
T	5.631	4.381	8.121	4.080	7.375	4.989
T+1	6.462	4.554	9.856	3.744	7.854	5.138
T+2	7.431	4.690	10.16	3.687	7.879	5.213
T+3	8.125	4.696	10.15	3.763	7.948	5.224
T+4	8.578	4.649	10.10	3.834	7.867	5.210
Annual wage earnings (euro)						
T-3	3980	4233	2952	3628	3837	4809
T-2	4665	4656	3957	4322	4581	4933
T-1	5299	4937	5085	5226	8102	7336
T	6387	5694	6739	6756	13400	11264
T+1	9656	9121	9539	9221	15959	13521
T+2	13835	12056	16803	12041	17286	14910
T+3	17530	14334	24418	13687	18451	16309
T+4	20442	15776	27367	15273	19390	17702
Annual unemployment (months)						
T-3	0.636	1.869	0.059	0.535	0.254	1.206
T-2	0.337	1.398	0.028	0.372	0.182	1.029
T-1	0.204	1.100	0.022	0.307	0.166	0.780
T	0.110	0.757	0.515	1.371	0.360	1.577
T+1	0.181	0.936	0.538	1.666	0.397	1.681
T+2	0.336	1.345	0.350	1.390	0.369	1.609
T+3	0.397	1.508	0.369	1.489	0.390	1.775
T+4	0.432	1.604	0.344	1.532	0.415	1.777
Self-Employed (% points)						
T-3	0.005	0.068	0.005	0.074	0.007	0.086
T-2	0.004	0.064	0.005	0.074	0.009	0.094
T-1	0.005	0.070	0.007	0.084	0.010	0.102
T	0.005	0.068	0.005	0.074	0.016	0.127
T+1	0.006	0.076	0.009	0.096	0.019	0.138
T+2	0.008	0.086	0.011	0.106	0.028	0.166
T+3	0.011	0.105	0.015	0.120	0.040	0.197
T+4	0.016	0.124	0.017	0.130	0.045	0.207
Observations	8518		2381		670	

TABLE B2 Balancing condition before and after the matching procedure with different methods

Panel A: University dropouts relative to graduates				
	Unmatched		NN (2)	
	t-test	Bias>10	t-test	Bias>10
Number of covariates	79	79	79	79
Number of covariates imbalanced	12	10	0	0
Per cent of covariates imbalanced	15 %	13 %	0 %	0 %
Mean bias	3.0		1.9	
Panel B: University dropouts relative to student population				
	Unmatched		NN (2)	
	t-test	Bias>10	t-test	Bias>10
Number of covariates	79	79	79	79
Number of covariates imbalanced	24	18	0	0
Per cent of covariates imbalanced	30 %	23 %	0 %	0 %
Medium bias	4.8		2.4	

Note: two-tailed t-test indicates that covariate means difference at 5 per cent level. NN (2): Nearest neighborhood matching using two nearest neighbors. Bias>10: Rosenbaum & Rubin (1985) standardized bias criteria indicating covariates with standardized bias over 10.

TABLE B3 Labour market outcomes before and after dropout (5 % of the propensity score is trimmed)

	ATT							
	Employment (months) (1)		Wage earnings (euro) (2)		Unemployment (months) (3)		Self-employment (% -points) (4)	
Panel A: Dropouts relative to student population (Control group 1)								
T - 3	0.047	(0.069)	130	(227)	-0.031	(0.026)	0.000	(0.003)
T - 2	0.184	(0.165)	-126	(197)	-0.032	(0.046)	0.005*	(0.00)
T - 1	0.492***	(0.205)	2712***	(287)	0.049	(0.037)	0.004	(0.05)
T	1.643***	(0.213)	7036***	(387)	0.228***	(0.061)	0.012**	(005)
T + 1	1.130***	(0.218)	5951***	(520)	0.226***	(0.066)	0.014***	(.005)
T + 2	0.346	(0.224)	3097***	(613)	0.081	(0.067)	0.016***	0.006)
T + 3	-0.165	(0.226)	758	(688)	0.035	(0.075)	0.026***	(0.008)
T + 4	-0.717***	(0.236)	-1104	(761)	0.006	(0.079)	0.024***	(0.008)
Dropouts (off):	637 (33)		637 (33)		637 (33)		637 (33)	
Control group:	8 518		8 518		8 518		8 518	
Panel B: Dropouts relative to graduates (Control group 2)								
T - 3	-0.054	(0.105)	-43	(127)	-0.013	(0.017)	0.002	(0.001)
T - 2	-0.271	(0.197)	-1033***	(254)	0.096**	(0.021)	0.002	(0.004)
T - 1	0.492**	(0.247)	-176	(475)	0.111***	(0.029)	-0.002	(0.005)
T	-0.411*	(0.247)	-2050***	(668)	-0.330***	(0.094)	0.012**	(0.005)
T + 1	-1.673***	(0.234)	-7824***	(848)	-0.397***	(0.110)	0.010*	(0.007)
T + 2	-1.947***	(0.249)	-9194***	(896)	0.149	(0.098)	0.012	(0.008)
T + 3	-1.892***	(0.248)	-9912***	(960)	-0.116	(0.098)	0.031***	(0.008)
T + 4	-2.049***	(0.246)	-11478***	(1214)	-0.014	(0.093)	0.036***	(0.008)
Dropouts (off):	603 (67)		603 (67)		603 (67)		603 (67)	
Control group:	2 376		8 518		8 518		8 518	

*** (**,*) = significant at 1 % (5%, 10%) level. *Note:* AI robust standard errors in parentheses; see Abadie and Imbens (2009).

TABLE B4 Results sensitivity to hidden bias

		Compared to university students			Compared to university graduates		
Groups		All	Female	Male	All	Female	Male
All	Earnings (t+4)						
		Γ = 1.00	Γ = 1.00	Γ = 1.00	Γ > 10	Γ > 10	Γ >10
		p = 0.057	p =0.057	p=0.578	.	.	.
	Self-employment					Γ= 1.00	Γ= 1.40
	(t+4)	Γ= 1.50	Γ=1.50	Γ=1.20	Γ=1.30	p=0.083	p = 0.046
		p = 0.053	p = 0.051	p = 0.047	p = 0.055		
Group 1	Earnings (t+4)	Γ =1.00	Γ = 1.00	Γ = 1.00	Γ > 10	Γ > 10	Γ > 10
		p = 0.061	p = 0.389	p =0.256	.	.	.
	Self-employment	Γ=1.60	Γ=1.40	Γ=1.40	Γ= 1.35	Γ= 1.00	Γ= 1.40
	(t+4)	p = 0.051	p = 0.055	p = 0.048	p = 0.052	p=0.106	p = 0.049

Note 1: Estimations are done using one-to-one matching without replacement. Note 2: Γ= value of gamma when coefficients are near 5 per cent significance level (p=p-value for corresponding gamma value); e.g. gamma is 1.10 when we assume 10 per cent bias upwards or downwards. Group 1 indicates that 2.5 % of observations are dropped from both upper and lower tails of ps distribution.

CHAPTER 5
R&D SUBSIDIES AND PRODUCTIVITY IN SMES

Abstract*

This paper examines the effect of R&D subsidies on labour productivity. We use firm-level data on Finnish SMEs from 2000 to 2012 and apply a combined matching and difference-in-differences method to control for selection bias. We find no significant positive effect on labour productivity over the five-year period after a subsidy is granted. However, the results vary over time and indicate a 2–4 % negative effect on SMEs' annual productivity growth one to two years after the subsidy year. Nevertheless, subsidies generate a positive employment effect and enhance firm survival. Additional scrutiny reveals that subsidies positively affect the human capital level of low-skill firms.

JEL: D24, O25, O38, C21

Keywords: Productivity, subsidies, R&D, SMEs, industrial policy, conditional difference-in-differences.

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1 Introduction

Governments can subsidise private research and development (R&D) indirectly with tax incentives (e.g. Cappelen et al., 2012) and directly with subsidies. European policy makers are actively promoting innovation policies designed to enhance R&D in small and medium-sized enterprises (SMEs) (Ortega-Argilés et al., 2009). Granted subsidies are aimed at removing market imperfections and generating positive welfare effects (e.g. Hainz and Hakeness, 2012; Takalo et al., 2013). Nevertheless, the effectiveness of R&D subsidies is often questioned because it is not clear how they should be allocated to private firms (e.g. Cantner and Kösters, 2012; Koski and Pajarinen, 2014).

R&D investments and related technological improvements are a major source of productivity growth (e.g. Griliches, 1998). Returns on R&D have been studied extensively in earlier literature (Mairesse and Sassenou, 1991; Hall et al., 2010; Mohnen and Hall, 2013 for surveys). Firms' R&D activities strive to generate higher rates of innovation (product, process or other), which can enhance firms' economic performance, such as their productivity (e.g. Crépon et al., 1998; Hall, 2011).[1] Because of market imperfections, firms might invest less in R&D than would be optimal for the whole society (Arrow 1962). Thus, public R&D subsidies are needed to increase R&D investments.

Empirical studies have found mixed evidence for the effectiveness of R&D subsidies (e.g. David et al. 2000; Zúñiga-Vicente et al. 2014 for surveys). Their effectiveness is evaluated by studying input and output additionality effects. Subsidies have input additionality effects if they attract additional R&D investments from private sources.[2] Output additionality effects materialise in firms' operations, which enhances their market success (e.g., increased patent activity and sales).[3] A growing number of studies examine subsidies' effects on firm productivity, which is a key factor in firm success. The results of these studies are, however, inconclusive. The productivity effect of R&D subsidies is found to be insignificant in high-tech firms in the U.S. (Irwin and Klenow, 1996). Similarly, no evidence is found that regional subsidies improve firm productivity in Britain (Criscuolo et al., 2012) or in Italy (Bernini and Pellegrini, 2011; Cer-

1 Empirical literature indicates that returns on R&D differ by firm characteristics (e.g., Hall et al., 2008; Hall et al., 2009; Ortega-Argilés et al., 2010; Ortega-Argilés et al., 2011).

2 Many authors have reported that R&D subsidies stimulate private investments (e.g. Almus and Czarnitzki, 2003; Hyytinen and Toivanen, 2005; Czarnitzki, 2006; Görg and Strobl, 2007; Özçelik and Taymaz, 2008; Aerts and Schmidt, 2008; Hussinger, 2008; Meuleman and Maeseneire, 2012; Czarnitzki and Lopes-Bento, 2013), but studies have also found that these funds partly or fully crowd out some private investments (e.g. Wallsten, 2000; Lach, 2002; Busom, 2000; González and Pazó, 2008; Gelabert et al., 2009 Bronzini and Iachini, 2014).

3 Subsidies have been found to enhance employment growth (e.g. Girma et al., 2008; Koski and Pajarinen, 2013; Link and Scott, 2013; Moretti and Wilson, 2014), patent development/innovations (e.g. Czarnitzki et al., 2007; Berube and Mohnen, 2009) and sales/investments (e.g. Criscuolo et al., 2012; Einiö, 2014; Cerqua and Pellegrini, 2014).

qua and Pellegrini, 2014). In Finland, Einiö (2014) finds that R&D subsidies from the European Regional Development Fund (ERDF) have a positive effect on productivity three years after the subsidy is granted. Finally, Koski and Pajarinen (2013, 2014) evaluate all business subsidies in Finland, and the results indicate that different subsidies (including R&D subsidies) have a small negative or insignificant effect on productivity growth.

Prior evidence also shows that R&D subsidy effects vary according to firm characteristics, which is valuable information when designing innovation policies. Lach (2002) finds that subsidies stimulate private funding only in small firms. Similarly, González and Pazó (2008) report that public financing is more efficient in small firms that operate in the low-technology sector. Public funding is also found to be disproportionally more important for firms that rely on external sources of financing (Hyytinen and Toivanen, 2005). Surprisingly, the sources of the heterogeneity of R&D subsidy effects are rarely studied relative to employee education levels and skills. Recent evidence, however, shows that founders' (e.g. Honjo et al., 2014) and employees' (e.g. Andries and Czarnitzki, 2014) human capital enhances firms' innovation. Higher education provides a comparative advantage in utilising new technologies (e.g. Bartel and Lichtenberg, 1987), and knowledge accumulation and R&D enhance firms' absorptive capacities (e.g. Cohen and Levinthal, 1989).[4]

In this paper, we contribute to the existing literature on R&D subsidies by examining their effect on productivity. We exploit longitudinal data on Finnish private-sector SMEs to examine the effects over a five-year period after the subsidy is granted. We not only study overall productivity effects but also investigate how subsidies affect firms' employment, value added and employee education levels. At the start of an R&D project, SMEs might recruit new employees (or re-allocate old employees and other resources within the firm), and this activity could reduce productivity growth in the short term (e.g. Bernini and Pellegrini, 2011). Thus, this negative subsidy effect on productivity is understandable if employment growth is attributable to firms' actions to enhance innovation capacity (e.g., firms recruit new employees with more human capital).

Productivity—defined here as the labour productivity—is an important factor in national welfare. From the viewpoint of policy makers, a direct positive effect of R&D subsidies on firm productivity would be a sign, even if not conclusive, of successful innovation policy. We focus on private-sector SMEs that received an R&D subsidy from the Finnish Funding Agency for Technology and Innovation (Tekes). Tekes annually grants over 600 million euros in R&D subsidies to public and private entities, but prudent evaluations of subsidies and firm productivity are still rare. We address the subsidy selection process (i.e., subsidies are not granted randomly) by using matching and conditional difference-in-differences estimators. We utilise rich longitudinal data from different register-based data sources that include all Finnish firms from

4 Recent empirical literature indicates that absorptive capacity (or "learning capacity") at a regional level is essential for subsidy effectiveness (e.g. Griffith et al., 2003, 2004; Becker et al., 2013).

2000 to 2012. We conduct a number of robustness checks on our results and also study whether the subsidies have an effect on firm survival.

The remainder of the paper is organised as follows. The next section presents the basic institutional features and introduces the data and the empirical method. Section 3 presents the results and the sensitivity analysis. Section 4 discusses the results, and the paper ends with concluding remarks in section 5.

2 Institutions, data and empirical methodology

2.1 R&D subsidies in Finland

Finland's R&D expenditures amounted to 3.78 % of GDP in 2011. Over 70 % of these R&D expenditures were funded by private business entrepreneurs, but the public sector offers R&D funding for firms to foster growth and innovation. Approximately 30 % of the public R&D funds are granted by Tekes, which is one of the agencies of the Ministry of Employment and the Economy. This amount is more than what universities or other research organisations receive directly from the state budget for R&D. In 2010, Tekes distributed 611 million euros to public and private parties. Tekes funds come mostly from the Finnish state budget, but some of the funds (less than 10 % of the total) come from the ERDF and are granted to public research projects. These public research projects include joint projects with several private parties.[5]

Tekes plays an important role in Finland because it facilitates the goals of innovation policy, which aims, amongst other things, to support firm renewal and productivity. The selection of firms for the Tekes subsidy programs is a rigorous and well-structured process. First, companies and research organisations that apply for funding must satisfy Tekes' national funding criteria. Companies that are based in Finland are eligible for funding if they meet these criteria according to Tekes experts. Second, all funding is competitive. Third, firms can apply for all funding directly during Tekes' applications rounds, in addition firms can also apply for ERDF funding in the application rounds in the provinces. Tekes funds part of each project with loans and direct subsidies or a combination of funding sources, but over 70 % of funding is in the form of direct subsidies. Grants for SMEs are usually 35 or 50 % of the total cost of the R&D project. [6]

2.2 Data

The data for this study come from a number of register databases. To combine the data sources, we use the firm-specific identifier codes provided by Statistics

5 This information is based official statistics by Statistics Finland. E-publication (in English) is available at: http://tilastokeskus.fi/til/tkke/2011/tkke_2011_2012-10-31_tie_001_en.html.

6 More information on Tekes programs, see: http://www.tekes.fi/en.

Finland. We use firm-level data from the Business Register database (age, region, foreign trade, sector and industry code at the two-digit level), the Financial Statement database (number of full-time personnel, value added and turnover), the Patent database (patents applied for in Finland and in Europe and patents granted in the US), the Concern database (a firm belongs to larger group) and the Statistics on Business Subsidies database (all paid loans, paid subordinated loans and subsidies). These register-based databases are maintained by Statistics Finland. Firm-level data is combined with the Employee Characteristics database (firm-specific employee education characteristics) created using the Finnish Longitudinal Employer-Employee Data (FLEED) by Statistics Finland.

Our sample consists of private-sector firms that have 10 to 249 full-time employees and have an annual turnover not exceeding 50 million euros (or less than 43 million euros on the firm's balance sheet). Our sample thus includes firms defined as SMEs by the European Commission, except that we exclude firms with fewer than 10 employees. These firms are excluded from the sample because the FLEED database includes only those firms that have at least 10 full-time employees. Because it is crucial to control for previously paid subsidies (see, e.g. González and Pazó, 2008), our analysis uses two annual observations to control for these. Firms are followed for a five-year period after the subsidy was granted. After these restrictions, our final sample consists of 1,221 firms that were granted an R&D subsidy over the period from 2002 to 2007. The last observation year is 2012.

This study uses labour productivity to measure firm productivity. Labour productivity is the annual value added divided by the number of full-time employees (as defined by Statistics Finland).[7] Productivity is in logarithm form to level outliers and facilitate the interpretation of the results. Because productivity measures can take negative values, and are thus unspecified for logarithm, some firms were excluded from the sample (less than 5 % of all firm-level observations). As a robustness test, we examine subsidies' effects using absolute values rather than logarithms. The results remained intact (see the online appendix for non-logarithm results).[8] Although the granted R&D subsidy is normally between 35 and 50 % of the total cost of the R&D project, the absolute size of the project can vary by industry. In our analysis, we use a binary treatment variable to capture Tekes' decision to grant an R&D subsidy. The treatment variable takes a value of one if the firm was granted a subsidy (in the form of a loan, subordinated loan or subsidy) and zero otherwise.[9]

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7 Alternatively, productivity can be measured by calculating total factor productivity via a production function rather than calculating labour productivity from the raw data. We use labour productivity to avoid a priori assumptions, which are needed to estimate total factor productivity.

8 The causal interpretation of the results may also be sensitive to log-linearisation (see Fisher and Ciani, 2014).

9 In our sample, 899 firms received only a direct subsidy and 64 firms received only a loan-based subsidy (for more details on R&D subsidies, see Table W1 in the online appendix). The results remained qualitatively similar when we focused on firms that received only a direct subsidy (see online appendix Table W9).

The timing of the variables is important in matching models. There is a possibility that firms anticipate of receiving a subsidy, which could affect firm's behaviour. Therefore, the matching variables are measured one year before a subsidy is granted. Possible earlier subsidies are also measured two years before a subsidy is granted. Overall, when building the data set and key variables, we attempt to control for the possible decrease in firm productivity before the subsidy decision is made. If a firm hires people in anticipation of receiving a subsidy, then the so-called Ashenfelter's dip would distort the difference-in-differences (DID) results upwards.[10]

2.3 Econometric evaluation method

R&D subsidies are not randomly distributed (e.g. David et al., 2000; Klette et al., 2000; Meuleman and Maeseneire, 2012). Because we cannot directly compare group means, we need to find comparable subsidised and unsubsidised firms. We start by calculating a subsidy assignment probability-based probit model, which reduces the matching dimension to a single scalar called the propensity score $P(X)$ (Rosenbaum and Rubin, 1983). The estimated propensity is then used in the matching procedure to estimate the average treatment effect on the treated firm (ATT_{PS}). ATT_{PS} can be formulated as the difference in conditional outcomes between subsidised (Y^S) and unsubsidised firms (Y^C):

$$ATT_{PS} = \left(Y^S - Y^C \middle| P(X),\ T = 1\right) = \frac{1}{\mathrm{N_S}} \sum_{\mathrm{i=1}}^{N_S} \left(Y_i^S - \sum_{\mathrm{j=1}}^{N_C} \mathrm{W}(i,j)\, Y_j^C\right) \qquad (1)$$

where Y_i^S denotes the labour productivity for firm i, which receives a subsidy, and Y_j^C denotes the labour productivity for firm j, which does not. Term $\mathrm{W}(i,j)$ is a weight function that determines how an unsubsidised firm j is weighted relative to a subsidised firm i, and N_S and N_C indicate the number of observed firms in each group. A single treatment case (above) can be straightforwardly extended to multiple treatments to investigate if subsidy size affects our estimations (e.g. Imbens, 2000; Lechner, 2002; Görg and Strobl, 2007).

The matching approach has multiple advantages over traditional regressions (Imbens and Wooldridge, 2009). For example, we do not need to make any functional form or distributional assumptions, which would be the case if we estimated the standard Cobb-Douglas production function. Matching using propensity scores can reduce selection bias, but it is still based on observed covariates. We follow earlier literature and control for multiple background vari-

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10 The DID method is based on differencing outcomes before and after a treatment. Thus, if a firm can anticipate that it will receive a subsidy (and changes its behaviour, e.g., hires new employees), then DID estimates can be biased (for details, see Heckman and Smith, 1999). As robustness checks, we also repeated the estimations by matching firms three and four years before a subsidy was granted. Again, our results remained qualitatively unchanged.

ables that could affect both a firm's probability of receiving a subsidy and its productivity. Unfortunately, we do not have the exact numbers of R&D staff for each firm in our register-based data set (available innovation surveys do not include all small firms). To form proxy variables for firms' R&D staff, we use two firm-level measurements for employee education levels: i) the share of workers with higher-degree tertiary education (masters, licentiates and PhDs) in a technology or natural science field (R&D staff 1) and ii) the share of workers with higher-degree tertiary education in other fields (R&D staff 2). These variables (combined with other matching variables) are important for minimising the selection problem concerning firm-specific R&D variables (e.g. Hussinger, 2008). Our other control variables include firm age (age and age squared) and size (number of employees and turnover) because firm innovativeness is known to be related to these factors (e.g. Veugelers and Cassiman, 1999; Huergo and Jaumandreu, 2004).[11]

It also important to control for outcome variables before actual subsidy year (productivity and employment growth) so that compared subsidised and non-subsidised firms are in similar growth trajectories (e.g. Lechner and Wunsch, 2013). The probability of receiving a subsidy and firm-level outcomes are also related to subsidy history (e.g. González and Pazó, 2008). We use three different subsidy history variables: firm received funding from Tekes one year (prev. sub t-1) and two years (prev. sub t-2) earlier and firm received any other subsidies over a two-year period (other subsidies). We also use covariates that indicate if the firm belongs to a larger firm group (group), has foreign ownership (ownership), is involved in foreign trade (foreign trade), has applied for patents (patents) and is multi-establishment (one location). It is also plausible that there are regional and industry-specific differences between the probability of receiving an R&D subsidy and firm performance (e.g. Almus and Czarnitzki, 2003; Aerts and Schmidt, 2008; Hussinger, 2008). Thus, we include 18 regional dummies in our matching equation, and we also control for whether the firm is located in a regional centre municipality (centre). Different industries are taken into account with 14 dummy variables (see online appendix Figure W1 for industry distribution by treatment status). Finally, we also include subsidy year dummies to account for idiosyncratic time shocks during different subsidy years (see Appendix Table 6 for more details on the covariates used).

However, unobserved macroeconomic and firm-specific shocks might also affect firm productivity. Because we have longitudinal firm data, we can further reduce the possible biases by differencing our outcome variables before and after the treatment. In particular, the DID method removes the time-invariant productivity differences between firms. This is an robust evaluation approach, especially when it is prudently combined with a matching method (e.g., Blundell and Costa Dias, 2000; Imbens and Wooldridge, 2009). To further formulate the combined matching and difference-in-differences approach (CDID), we in-

11 Following Aerts and Schmidt (2008), employment and turnover are in logarithms to avoid potential biases caused by skewness of the data.

sert different time periods into Equation 1. Let t and t' indicate the time periods before and after the R&D subsidy is granted, and the CDID estimator is:

$$ATT_{CDID} = \frac{1}{N_S}\sum_{i=1}^{N_S}\left[Y_{it}^S - Y_{it'}^S - \sum_{J=1}^{N_C} W(i,j)\left(Y_{jt}^C - Y_{jt'}^C\right)\right], \quad (2)$$

where the average treatment effect on the treated (ATT_{CDID}) is examined in terms of differences rather than levels. As before, term $W(i,j)$ is the weighting function for constructing the comparison group from untreated firms j for each treated firm i. For causal interpretation, two basic identification assumptions are needed: the conditional independence assumption ($Y_i(S), Y_i(C) \perp T_i | P(X)$) and the common support assumption ($0 < Pr[S = 1|P(X)] < 1$). The conditional independence assumption (CIA) states that conditional treatment status (T_i) should be independent of potential outcomes $Y_i(S)$ and $Y_i(C)$. Note that this assumption is somewhat weaker when using the CDID estimator than when using the pure matching approach without differencing. If the conditional independence assumption does not hold, we can examine the average treatment effect for the treated firms as long as we can assume that the possible bias is constant (Heckman et al., 1997, 1998). In practise, this means that both the control group and the treatment group must have evolved (conditionally) in a similar manner had they not been treated. [12]

Finally, we need to choose how to compute the weights $W(i,j)$ for the propensity scores and estimators (Equations 1 and 2). Although there are many alternative matching methods for estimating the weights, we use the nearest neighbourhood method as a starting point. The two nearest firms in the control group (as measured by propensity scores) are used as comparisons for each subsidised firm. To examine the robustness of the main results, we also use a different number of neighbours in the matching process, trim the tails of the propensity score and use an alternative matching method.

3 Results

3.1 Firm selection and main results

Before focusing on the main results, it is informative to examine the selection into the treatment. Table 1 reports the results from the probit model, which estimates how different firm characteristics are related to the probability of receiving a subsidy. The results indicate that firm-specific education-level variables

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12 We plot averages of our outcome variables over the three-year period before the subsidy year (and after) to show descriptively that the outcomes in the subsidised firm group developed in parallel to those in the unsubsidised firm group. Figures are available in the online appendix.

are important in the selection process. Firms that have a high share of workers with tertiary education have a higher likelihood of obtaining an R&D subsidy. Productivity growth before the subsidy year is also higher in subsidised firms than in unsubsidised firms.[13] Past R&D subsidies and other subsidies are also positively related to new R&D subsidies. These findings are consistent with Lerner (2002), who suggests that firms might learn from the application process over time. In addition, we observe that foreign trade is associated with a higher probability of receiving a subsidy, whereas foreign ownership decreases this probability. In the sample of SMEs, the average probability of obtaining a subsidy is 3.6%.

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13 This might indicate that funding authorities are attempting to follow the so-called "picking the winner" strategy (i.e., subsidies might be granted to relatively good firms rather than to marginal projects or firms that suffer market malfunctions, e.g. Cantner and Kösters, 2012).

TABLE 1 Selection to treatment (Probit model)

Dependent variable: Treatment	Coefficients (SE)		Marginal effects (SE)	
Age	-0.046	(0.030)	-0.002	(0.001)
Age^2	0.002	(0.002)	0.000	(0.000)
ln(turnover)	0.098**	(0.031)	0.003***	(0.001)
ln(employees)	0.077**	(0.038)	0.003**	(0.001)
Group	-0.060	(0.037)	-0.002	(0.001)
Ownership	-0.452***	(0.071)	-0.016***	(0.002)
Foreign trade	0.222***	(0.043)	0.008***	(0.002)
Patents	-0.003	(0.080)	0.001	(0.003)
R&D staff 1	1.613***	(0.162)	0.057***	(0.006)
R&D staff 2	0.683***	(0.203)	0.015***	(0.002)
Prev. sub t-1	0.534***	(0.046)	0.019***	(0.002)
Prev. sub t-2	0.416***	(0.047)	0.015***	(0.002)
Other subsidies	0.410***	(0.036)	0.014***	(0.001)
ln(prod. growth)	0.341***	(0.088)	0.012***	(0.003)
ln(emp. growth)	0.055	(0.048)	0.002	(0.002)
Centre region	0.028	(0.038)	0.001	(0.001)
One location	-0.034	(0.035)	-0.001	(0.001)
Food industry	0.573***	(0.105)	0.020***	(0.004)
Textile industry	0.550***	(0.124)	0.019***	(0.004)
Wood industry	0.421***	(0.112)	0.015***	(0.004)
Paper industry	0.378*	(0.203)	0.013*	(0.007)
Chemical industry	0.614***	(0.097)	0.022***	(0.003)
Metal industry	0.641***	(0.089)	0.023***	(0.003)
Machine industry	0.671***	(0.090)	0.024***	(0.003)
Electronic industry	0.749***	(0.099)	0.026***	(0.003)
Other industries	0.434***	(0.097)	0.015***	(0.003)
Utilities	0.405*	(0.214)	0.014*	(0.007)
Construction	0.182*	(0.096)	0.006*	(0.003)
Sales	-0.113	(0.095)	-0.004	(0.003)
Private services for business	0.639***	(0.087)	0.023***	(0.003)
Average propensity score	0.036			
Log-likelihood	- 3,909			
Number of observations	33,811			

Significance: 10 % level (*), 5 % level (**) and 1 % level (***). Standard errors are in parentheses. Note 1: Estimations also included dummies for NUTS3 regions (18) and subsidy year (6). Note 2: Marginal effects are evaluated at the means.

TABLE 2 Descriptive statistics for the selected variables before and after matching

	Before matching				After matching			
	Mean				Mean			
Variable	Treat. group	Cont. group	B	p-value	Treat. group	Cont. group	B	p-value
*Pre-treatment outcome variables (levels) **								
Emp. (t-1)	50.12	33.98	43	0.000	50.16	50.73	-2	0.700
Emp. (t-2)	47.82	33.11	40	0.000	47.86	48.40	-2	0.750
Emp. (t-3)	45.77	32.31	37	0.000	45.82	46.34	-1	0.756
V.ad. (t-1)*	2.69	1.71	43	0.000	2.69	2.75	-2	0.335
V.ad. (t-2)	2.47	1.61	40	0.000	2.46	2.55	-3	0.394
V.ad. (t-3)	2.24	1.52	32	0.000	2.24	2.35	-5	0.335
Prod. (t-1)	53,660	50,238	13	0.000	53,665	54,131	-2	0.657
Prod. (t-2)	51,548	48,573	12	0.000	51,511	52,713	-5	0.265
Prod. (t-3)	48,916	47,010	8	0.014	48,949	50,721	-8	0.097
Pre-treatment outcome variables (growth rate)								
V.ad. (t-1)-(t-3)	0.45	0.19	27	0.000	0.6	0.40	7	0.110
Prod. (t-1)-(t-3)	4,744	3,228	10	0.000	4,716	3,421	7	0.114
Matching variables								
Age	16.15	17.86	14	0.000	16.16	16.36	1	0.674
ln(turnover)	1.56	1.22	35	0.000	1.56	1.60	-4	0.326
ln(employees)	3.63	3.27	51	0.000	3.63	3.64	-1	0.759
Group	0.64	0.74	20	0.000	0.64	0.63	3	0.436
Ownership	0.04	0.08	16	0.000	0.04	0.04	0	0.921
ln(prod.growth)	0.06	0.03	15	0.000	0.06	0.05	4	0.432
ln(emp.growth)	10.75	10.70	12	0.000	10.75	10.76	-3	0.523
Foreign trade	0.70	0.44	55	0.000	0.69	0.70	-2	0.629
Patents	0.06	0.01	28	0.000	0.07	0.07	3	0.685
R&D staff 1	0.08	0.02	57	0.000	0.08	0.07	5	0.382
R&D staff 2	0.03	0.02	13	0.000	0.03	0.03	1	0.905
Prev. sub t-1	0.40	0.05	95	0.000	0.40	0.38	6	0.253
Prev. sub t-2	0.39	0.05	86	0.000	0.38	0.36	5	0.304
Other subs.	0.61	0.21	89	0.000	0.61	0.63	-4	0.381
Centre region	0.51	0.54	-7	0.018	0.51	0.50	2	0.557
One location	0.66	0.70	-8	0.003	0.66	0.67	-2	0.637
Observations	1,221	32,590			1,221	1,874		

* Value added is in millions of euros. Note 1: Period (t) indicates subsidy (treatment) year. Note 2: p-values indicate two-sided t-tests for mean equality. Note 3: Other matching variables include age squared and dummies for NUTS3 regions (18), industry (14) and subsidy years (6). Note 4: See Table 6 in the Appendix for definitions of variables. Table W2 in the online appendix shows that matching successfully balanced all covariates used in the analysis. Note 5: Letter B indicates % of bias before and after matching.

Table 2 reports the descriptive statistics of the key variables before and after the matching procedure, which balances the differences between the treated and untreated groups. Before matching, a comparison of the mean values between the subsidised firms and the unweighted control group indicated significant differences between all variables (see the t-test results). Differences are notable in the outcome variables from the pre-treatment period. Subsidised firms are, on average, larger and more productive than unsubsidised firms. This underlines the fact that we cannot compare subsidised and unsubsidised firms directly and that it is necessary to select control firms with similar characteristics for each subsidised firm. The matching procedure introduced earlier strives to balance the differences between the treated and untreated firms so that firms are similar in observed covariates except for subsidy decision. The results in Table 2 indicate that after matching, the bias between the groups is successfully minimised: the p-values indicate that the means of the two groups' variables are nearly identical.[14]

Table 3 presents the results from both the matching and the conditional difference-in-differences approaches (CDID). Panel A reports the results regarding firm productivity, and Panels B and C show the results regarding employment and value added, respectively. The sample size of firms that received a subsidy (treated) and firms that were used to construct the comparison group (all) and actual control group are reported at the bottom of the table. Estimations are performed with a common support restriction, which excludes treated firms with propensity scores that are too high compared with the highest value in the control group (only a small number of the treated firms are off support). Accordingly, the matching year shows the treatment effect before the actual treatment, which should be statistically insignificant in structure. The treatment year equals the year when a subsidy is granted, and the sequential numbers indicate the years afterward. It should be stressed that each coefficient is estimated separately using the estimation procedure outlined in the previous section.

14 Table W2 in the online appendix further shows how the matching method succeeds in removing significant differences for all used covariates between subsidised and unsubsidised firms.

TABLE 3 Impact of R&D subsidies on labor productivity, employment and value added

	Matching		CDID	
	ATT	(s.e.)	ATT	(s.e.)
Panel A - Dependent variable: log (productivity)				
Matching year	- 0.003	(0.014)	.	.
Treatment year	- 0.022	(0.014)	- 0.018	(0.011)
T + 1	- 0.039	(0.015)***	- 0.036	(0.013)***
T + 2	- 0.027	(0.015)*	- 0.024	(0.015)
T + 3	- 0.014	(0.014)	- 0.011	(0.014)
T + 4	- 0.022	(0.015)	- 0.019	(0.015)
T + 5	0.002	(0.016)	0.006	(0.016)
Panel B - Dependent variable: log (employment)				
Matching year	0.005	(0.017)	.	.
Treatment year	0.023	(0.017)	0.019	(0.005)***
T + 1	0.030	(0.018)*	0.025	(0.009)***
T + 2	0.033	(0.018)*	0.028	(0.011)**
T + 3	0.031	(0.019)*	0.027	(0.013)**
T + 4	0.030	(0.020)	0.025	(0.014)*
T + 5	0.036	(0.021)*	0.031	(0.016)**
Panel C - Dependent variable: log (value added)				
Matching year	0.002	(0.021)	.	.
Treatment year	0.002	(0.022)	0.001	(0.012)
T + 1	- 0.010	(0.023)	- 0.011	(0.015)
T + 2	0.006	(0.024)	0.005	(0.018)
T + 3	0.017	(0.022)	0.015	(0.019)
T + 4	0.008	(0.024)	0.006	(0.021)
T + 5	0.038	(0.026)	0.036	(0.022)
Treated (off support): 1,215 (6)			Control (all): 1,874 (32,590)	

Significance: 10 % level (*), 5 % level (**) and 1 % level (***). Robust standard errors are in parentheses (Abadie and Imbens 2009). *Note:* The matching year refers to the year before the subsidy decision when the matching is performed, the treatment year is when a subsidy is granted, and the sequential numbers indicate the years after a subsidy is granted.

The results in Panel A of Table 3 suggest that R&D subsidies have a negative effect on productivity after the subsidy is granted. When potential time-invariant effects are controlled for in the CDID estimations, subsidies decrease average labour productivity by 3.6 % one year after the treatment year. Two years after the subsidy year, the negative effect on labour productivity is 2.4 % (significant at the 12 % level). The results indicate that the productivity of treated firms catches up with that of unsubsidised firms five years after a subsidy is granted (the difference is statistically insignificant).[15] It is important to note that even though we do not find any significant positive effects on productivity, the point estimates show clear differences over time.

The decline in productivity after a subsidy decision is reasonable because new R&D projects might begin by recruiting new employees or re-allocating old employees (and other resources) from the daily business to the R&D project. The increase in the number of staff negatively affects productivity growth if there is no sufficient increase in value added at the same time. This finding is in line with earlier studies by Bernini and Pellegrini (2011) and Koski and Pajarinen (2014). In Panels B and C of Table 3, we study in greater detail why no positive productivity effect is observed over the five-year period after a subsidy is granted. We recalculate the matching and CDID models using a logarithm of full-time employees and value added as dependent variables. The results indicate that an R&D subsidy has a significant positive effect on employment of approximately 2–3 %. Alternately, R&D subsidies contribute mainly positively on value added, but the estimates are statistically insignificant (throughout the period). The results thus indicate that R&D subsidies have a relatively steady, positive effect on employment growth but that the effect on value added is essentially zero; however, this effect might be realised with a significant lag.

3.2 Low- and high-skill firms

The R&D subsidy effect could be heterogeneous because firms have different abilities to carry out R&D projects that can further affect productivity and future R&D efforts (e.g. Cohen and Levinthal, 1989). We would expect that subsidies have a more significant positive effect on productivity in firms with employees who have higher levels of human capital. We divide the total sample into low- and high-skill firms by using the median share of employees with higher tertiary education.[16] Thus, in our subsamples, we compare subsidised low- and high-skill firms more prudently with similar unsubsidised firms. The subsidy effect might also depend on the size of the subsidy (e.g. Görg and Strobl, 2007). In Table 4, we redefine three separate treatment groups: all subsidies (as in our previous estimations), small subsidies (subsidies per employee under the median) and large subsidies (subsidies per employee at the median

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15 We also re-run our estimations by excluding the covariates that measure employees' education and earlier subsidies from the matching model. In this case, productivity growth was more rapid, and the effect was significant and positive five years after the treatment. This indicates that without controlling for firm-specific education variables and covariates regarding earlier subsidies in the estimations, the estimation results would be biased upwards (as noted earlier by Gonzalez and Pazo 2008). This finding highlights the need to consider the selection problem in different evaluations of subsidies.

16 By higher tertiary education, we mean employees who have master's, licentiate or PhD degrees. The median share of employees with higher tertiary education (in our sample of subsidised firms) is 4.6 %. The correlation between firm size and the share of higher tertiary education is relatively small (-0.12); see online appendix Figure W2 for illustration. As a robustness test, we also study the subsidy effect on productivity by firm size, but the results are mainly insignificant (online appendix Table W5).

or above). In Panel A of Table 4, we repeat the estimations for all firms, and in Panels B and C, we study the effects separately for low-skill and high-skill firms.

The results indicate that firms that receive large subsidies experience (on average) greater productivity decline after the treatment year than firms that receive small subsidies. When we divide the firm sample into skill groups, subsidised low-skill firms (Panel B) experience 3 % productivity decline one to four years after the treatment year (the effect is also relatively long-term for the larger subsidies). The initial productivity decline is greater in subsidised high-skill firms (but the effect is statistically insignificant when we divide subsidies by size). Separate analyses of value added and employment reveal that the subsidy effect is more significant and larger among high-skill firms (Appendix Table 7). Thus, although we see no positive effect on productivity in either group, subsidies have significant positive effects on employment and value added among high-skill firms in particular.

What could explain the differences over time between subsidised low- and high-skill firms? A recent study by Wanzenböck et al. (2013) indicates that R&D-intensive firms are less likely to change their innovation behaviour because of subsidies. Earlier literature reports that R&D subsidies might increase R&D intensity more significantly among firms that were not R&D-intensive before public funding (e.g. Özçelik and Taymaz 2008) and that employees' skills are vital for firms' innovation activities (e.g. Leiponen 2005; Mohnen and Röller 2005). We therefore examine whether firms' innovation capacity changes after subsidies are granted by studying firms' employee education levels. We have already observed that subsidies have a positive effect on employment but not on productivity. Still, if employment growth (caused by the subsidies) enhances firms' innovation capacity, the insignificant effect on firm productivity would be understandable.[17]

[17] It should be stressed that the goal of the R&D subsidies is also to enhance firms' innovation capacity, the benefits of which (such as enhanced productivity) are observed in the long term.

TABLE 4 R&D subsidy effect (ATT) on labour productivity by share of employees with higher tertiary education (CDID)

Dependent variable: log (productivity)			
	Relative size of the subsidy		
	All subsidies	Small subsidies	Large subsidies
Panel A: All firms			
Treatment year	- 0.018 (0.011)	- 0.008 (0.012)	- 0.037 (0.021)*
T + 1	-0.036 0.013)***	- 0.011 (0.014)	-0.057 (0.026)**
T + 2	- 0.024 (0.015)	- 0.031 (0.018)*	- 0.041 (0.025)
T + 3	- 0.011 (0.014)	- 0.020 (0.017)	- 0.041 (0.025)*
T + 4	- 0.019 (0.015)	- 0.011 (0.019)	- 0.020 (0.027)
T + 5	0.006 (0.016)	- 0.004 (0.019)	- 0.011 (0.027)
Treated (off):	1,215 (6)	624	596 (1)
Control (all):	1,874 (32,590)	1,124 (32,590)	982 (32,590)
Panel B: Low-skill firms (share of workers with higher tertiary education under the median)			
Treatment year	- 0.022 (0.012)*	- 0.008 (0.013)	- 0.036 (0.022)*
T + 1	- 0.028 (0.015)*	- 0.006 (0.015)	-0.065 0.024)***
T + 2	- 0.027 (0.016)*	- 0.004 (0.018)	- 0.041 (0.025)*
T + 3	- 0.023 (0.017)	- 0.005 (0.019)	- 0.023 (0.025)
T + 4	- 0.025 (0.018)	0.031 (0.022)	- 0.054 (0.029)*
T + 5	0.001 (0.021)	0.031 (0.023)	- 0.043 (0.028)
Treated (off):	632 (1)	388	260
Control (all):	1,049 (24,881)	691 (24,881)	428 (24,881)
Panel C: High-skill firms (share of workers with higher tertiary education above the median)			
Treatment year	- 0.028 (0.020)	0.001 (0.029)	- 0.025 (0.037)
T + 1	-0.059 (0.023)**	- 0.016 (0.032)	- 0.076 (0.041)
T + 2	- 0.046 (0.026)*	- 0.039 (0.036)	- 0.037 (0.043)
T + 3	- 0.017 (0.025)	- 0.021 (0.037)	- 0.021 (0.039)
T + 4	- 0.013 (0.026)	- 0.017 (0.035)	- 0.002 (0.042)
T + 5	- 0.005 (0.025)	0.025 (0.037)	0.014 (0.044)
Treated (off):	580 (8)	234 (2)	333 (4)
Control (all):	785 (7,709)	397 (7,709)	478 (7,709)

Significance: 10 % level (*), 5 % level (**) and 1 % level (***). Robust standard errors are in parentheses (Abadie and Imbens 2009). Small subsidies: Granted R&D subsidy per employee is under 1,650 euros (50 percentile). Large subsidies: Granted R&D subsidy per employee is over 1,649 euros.

We repeat our CDID estimations using three different firm-level measurements for employees' education, which are utilised to measure the firms' human capital intensity.[18] The estimations in the first column of Table 5 utilise the percentage of employees with higher tertiary education in natural science and technology as a dependent variable. The second column examines the subsidy effect on the share of employees with higher tertiary education in other fields. The dependent variable in the third column is the percentage of employees with less than higher tertiary education. Because the different measurements for education are percentages (which sum up to one), significant positive effects in the first two columns would indicate that firms became more human capital intensive.

The reported results in Panel A of Table 5 show that subsidised firms become more human capital intensive because of the subsidies. The percentage of employees with higher tertiary education (in natural science and technology) increases annually 0.3–0.4 %-points after the subsidies. The results in column 3 also indicate that the share of employees without a higher tertiary degree decreases after the subsidy year. There are clear differences between subsidised low- and high-skill firm groups. In low-skill firms (Panel B), subsidies increase the share of employees with a degree in natural science and technology in the subsidy year (0.2 %- points) and the subsequent two-year period. Contrary to the results for all firms (Panel A), the positive effect on the share of employees from other fields is also significant. As the average share of higher tertiary education is only 0.45–0.55 % in subsidised low-skill firms in the year before the subsidy year, the average share of higher tertiary education more than doubles over the four-year period (relative to the base year). The results in Table 5 also indicate that high-skill firms become more human capital intensive, although the effect is smaller and more short-term. In subsidised high-skill firms, the average share of tertiary education is 7–16 %, which underlines how small the point estimates are.[19]

The finding that R&D subsidies affect human capital levels in low-skill firms in particular is consistent with the earlier literature on R&D subsidies. It is understandable that firms that begin R&D work because of the subsidies (i.e. "switch" R&D development on) increase their R&D efforts relatively more than firms that are already R&D intensive (e.g. Almus and Czarnitzki, 2003; Czarnitzki, 2006; Özçelik and Taymaz, 2008). High-skill firms already have relatively high human capital levels, and subsidies enhance growth in employment and value added (as shown in Appendix Table 7), but these firms do not necessarily become more human capital intensive.

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18 We focus on the three-year period after the treatment year because employee education data were not available for later years.

19 In online appendix Table W6, we show that our results by firm skill groups are robust even if we use different cut-off points for low-and high-skill firms.

TABLE 5 R&D subsidy effect (ATT) on employee education levels (CDID)

Dependent variable: The percentage share of workers at different levels/fields of education

	Higher tertiary education		Less than higher tertiary education
	Natural science and technology	Other fields	
Panel A: All firms			
Treatment year	0.269 (0.118)**	0.025 (0.081)	- 0.041 (0.139)
T + 1	0.389 (0.153)**	0.139 (0.103)	- 0.298 (0.184)
T + 2	0.384 (0.170)**	0.080 (0.120)	- 0.481(0.209)**
T + 3	0.309 (0.191)	- 0.515 (0.130)	- 0.514 (0.231)**
Average share (%)	8.03	3.49	88.48
Treated (off support):	1,216 (5)	Control (all): 1,872 (32,590)	
Panel B: Low-skill firms (share of workers with higher tertiary education under the median)			
Treatment year	0.180 (0.053)***	0.030 (0.037)	0.099 (0.062)
T + 1	0.150 (0.070)**	0.201(0.060)***	- 0.143 (0.086)**
T + 2	0.187 (0.074)**	0.260 (0.074)***	- 0.287 (0.110)***
T + 3	0.138 (0.089)	0.219 (0.079)***	- 0.376 (0.121)***
Average share (%)	0.55	0.45	99.00
Treated (off support):	632 (1)	Control (all): 1,049 (24,881)	
Panel C: High-skill firms (share of workers with higher tertiary education above the median)			
Treatment year	0.564 (0.227)**	- 0.004 (0.126)	- 0.191 (0.300)
T + 1	0.432 (0.295)	0.085 (0.214)	- 0.663 (0.364)*
T + 2	0.471 (0.336)	- 0.246 (0.248)	- 0.691 (0.415)*
T + 3	0.407 (0.381)	- 0.300 (0.259)	- 0.425 (0.464)
Average share (%)	15.94	6.78	77.28
Treated (off support):	580 (8)	Control (all): 785 (7,709)	

Significance: 10 % level (*), 5 % level (**) and 1 % level (***). Robust standard errors are in parentheses (Abadie and Imbens 2009). Note 1: Higher tertiary education comprises employees with master's, licentiate or PhD degrees. Note 2: Average share (%) indicates the percentage of workers with specific education in the subsidised firm group before the subsidy year.

3.3 Sensitivity analysis

Our identification strategy is based on the assumption that a large number of firm background variables can minimise the selection bias problem (Assumption 1, unconfoundedness). Moreover, the CDID approach removes unobserved time-invariant effects and is a more robust approach to addressing the selection problem (e.g. Imbens and Wooldridge, 2009). Unconfoundedness cannot be tested directly, but the sensitivity of results can be reviewed indirectly by estimating "placebo effects" before the actual treatment. Negative subsidy effect on firm productivity before the subsidy is granted would indicate that our results are driven by some factor other than the R&D subsidy. The CDID results in Table 8 of the Appendix show that this is not the case.[20] Although we find that there is a positive effect on productivity two years before the subsidy year, no effect is found three or four years before (or one year before). Positive effects two years before might indicate that subsidised firms have higher prior productivity growth than otherwise similar control firms (indicating the so-called "picking the winner" strategy). Because the results in Table 9 are estimated using a different study period (subsidy years 2004 to 2007) than that used for our main results, the estimations also show that the results are not sensitive to the chosen time period. We also estimate placebo effects for our education variables used in Table 5. We find no significant placebo effects in the two years before the subsidy year (online appendix Table W7).

The assumption regarding common support (Assumption 2) is easier to assess and evaluate because the density distribution of the propensity score in both the treated and the untreated groups is available. In our estimations, we impose a common support by dropping the treatment observations with propensity scores higher than the maximum or lower than the minimum for the control firms. This restriction eliminates only a small number of firms from our sample (indicated as "off support" in the tables). We additionally trim our sample estimations at the 2 % and 5 % levels, but the results remain qualitatively the same. The results are also robust when we use different matching methods and when we divide the total sample into separate firm groups by sector. In online appendix (Panel D of Table W3) we match firms exactly by sector, because propensity score matching might compare firms from different sectors. Results show that this has no impact on our results. Table W10 of online appendix shows that initial decline in productivity is, on average, more significant in industrial sector than in service sector. We also repeat the main estimations using different definitions for the treatment variable (only direct subsidies), but this does not affect our conclusions (these results are also available in the online appendix).

This study uses a balanced panel over the study period. Productivity is important for firm survival, but we exclude by structure those firms that did

[20] We focus on subsidy years 2004–2007 to study possible placebo effects due to data constraints. Placebo effect is calculated by moving one year "window" (a difference between reference year and comparison year) to pre-treatment period.

not survive the entire period. Next, we attempt to evaluate if this might affect our results. First, we repeat our estimations using shorter panels over one year and three years after the subsidy year. The obtained productivity results are similar to those from our earlier estimations using a five-year panel (online appendix Table W8).[21] Second, we also study if the R&D subsidies affect firm survival propensity over the five-year period after the subsidy is granted. Earlier literature suggested that public subsidies affect firm survival (e.g. Ebersberger, 2011). Our dependent variable takes the value of one if the firm remains in the market and zero otherwise.[22] We repeat the survival estimations (using the same control covariates as before). Interestingly, the results in Appendix Table 9 show that R&D subsidies have a significant positive effect on firm survival. Although no effect is found one year after the subsidy year, the subsidy effect on survival rates is positive (2–4%) and significant three to five years later (the effect is notably similar for firm groups).

A positive subsidy effect on firm survival might indicate two things. First, if subsidies are allocated to relatively inefficient firms, they might enhance the firms' probability of surviving in the marketplace without positively affecting firm productivity. This could distort the market mechanism (i.e., natural firm exits) in that the survival of inefficient firms might also affect unsubsidised firms (e.g. Santarelli and Vivarelli 2002; Koski and Pajarinen 2014).[23] Second, if subsidies successfully correct market inefficiencies, and thus lead to improvements in firms' innovation activities, then the positive effect on firm survival might indicate that subsidies could have an effect on firms' long-term productivity. Unfortunately, this study is unable to investigate the long-term effects of R&D subsidies.

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21 In our study period, 32 % of subsidised firms exited our sample during the five-year period.

22 From our data, we cannot distinguish between mergers, acquisitions and firms that went out of business. Still, most business exits indicate that the business was unsuccessful (Coad 2013). Firm survival is studied similarly in earlier literature (e.g. Hyytinen et al. 2014).

23 We studied descriptively how starting/initial (year) productivity is related to firm survival. Simple cross tabulations show that subsidized firms whose initial productivity are below median productivity are more likely to survive than subsidized firms whose initial productivity is above the median (see online appendix Table W11). This might indicate that subsidies are more important for low productivity firms. But it should be stressed that we cannot observe whether R&D subsidies also affect unsubsidised firms. Our empirical approach does not allow spillovers (the so-called stable unit treatment value assumption; see Imbens and Wooldridge 2009). If subsidies also affect unsubsidised firms, then the subsidy effect on productivity might be under- or overestimated. When more accurate databases become available for research, it would be interesting to evaluate if different research results that focus on subsidy effects are affected by this possibility.

4 Discussion

4.1 Summary of the results

This study has examined whether the public R&D subsidies granted by Tekes enhance productivity in private-sector SMEs. Annual Tekes funding for private firms is approximately 600 million euros, making Tekes one of the most significant public source of R&D funding in Finland. The results indicate that one to two years after a subsidy is granted, firm productivity declines by 2–4% compared to similar unsubsidised firms. We find that R&D subsidies positively affect firm employment growth. When we study the education levels of firm employees, we find that low-skill firms in particular become more human capital intensive because of the subsidies. The results also suggest that R&D subsidies have a positive effect on firm survival.

4.2 Limitations of the study

There are at least three important limitations to consider. First, the results indicate that firms do not receive subsidies randomly. Our approach (combined matching and difference-in-differences method) might not be able to remove all of the bias resulting from positive or negative selection. Still, our results are robust to many specifications used in the analyses. Second, we focus on the five-year period after the subsidy is granted. However, benefits from R&D projects could materialise after considerable time lags. For example, if subsidies enhance firms' overall innovative capabilities, the positive subsidy effect on productivity might be observed in the longer term. Third, R&D subsidies might have spillover effects (short- and long-term), which would violate the assumptions of our econometric approach. Spillovers could also be part of the assessment of social benefits gained from R&D subsidies. Unfortunately, evaluating spillover effects is out of reach of this study.

4.3 Policy and research implications

Our findings indicate that on average, public R&D subsidies have negative or insignificant short-term effect on firm productivity and that subsidies enhance firm survival. Positively, we find that subsidies foster employment growth and firms become more human capital intensive as the result of subsidies. These are also the goals of Finnish innovation policy. Nevertheless, further empirical scrutiny is needed for the efficient use of scare public resources. If subsidies do not positively affect firm productivity growth in the longer term, subsidies might artificially help inefficient firms to stay in the market and thus hinder aggregate productivity growth. For more prudent evaluations, government agencies should be more transparent regarding their subsidy decision-making processes. It would be beneficial to have more detailed information on the applications of both subsidised firms and firms that apply for but do not receive subsidies.

In future research, it would be valuable to identify in more detail the different channels through which R&D subsidies affect firm productivity. Although earlier research has shown the relationship between R&D and productivity, evidence of the effects of R&D subsidies on productivity is still inconclusive. Empirical research at the firm level also needs to recognise that subsidies can affect unsubsidised firms. For example, this study reports (as have a number of other earlier studies) that R&D subsidies have a significant positive employment effect. It would be instructive to examine the sources of the observed increases in employment. Such an analysis might bring new information not only on the factors that determine subsidies' effects on firm productivity but also on whether public subsidies affect unsubsidised firms.

5 Concluding remarks

We find no evidence of an economically significant positive effect of R&D subsidies on firm productivity over the five-year period after a subsidy is granted, which should arise in the case of grave capital market imperfections. Over the five-year period after a subsidy is granted, public funding contributes significantly to firm growth in terms of number of employees and human capital intensity but does not result in productivity growth.

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Appendix (Tables 6–9)

TABLE 6 Variable definitions

Variables	Definition
Dependent variable	
Productivity	ln(value added / number of full-time employees)
Treatment variable	
R&D subsidy	If the firm was awarded an R&D subsidy = 1, otherwise = 0
Firm-specific variables	
Age	Age/10 of the firm in years
Age^2	(Age/10) squared
ln(turnover)	ln(Turnover, million euros)
ln(employees)	ln(Number of employees)
ln(prod. growth)	ln(productivity (t-1) /productivity (t-2))
ln(emp. growth)	ln(employment (t-1) /employment(t-2))
Foreign trade	If the firm exports or imports or both = 1, otherwise = 0.
Group	If the firm is part of a larger group = 1, otherwise = 0
Ownership	If the firm has foreign ownership (majority) = 1, otherwise = 0
R&D development	
Patents	If the firm has applied for a patent = 1, otherwise = 0.
R&D staff 1	The share of workers with tertiary education (master's, licentiate or doctoral degree) in technology or natural science.
R&D staff 2	The share of workers with tertiary education (master's, licentiate or doctoral degree) in other than technology or natural science.
Prev. sub. t-1	Received R&D subsidy payments one year before the treatment
Prev. sub. t-2	Received R&D subsidy payments two years before the treatment
Other subsidies	Received a subsidy before (in a two-year period) from another source (e.g., Finnvera) = 1, otherwise = 0
Industry classification	
Food industry	Food and drink industry (15–16)
Textile industry	Textile industry (17–19)
Wood industry	Wood industry (20)
Paper industry	Pulp and paper industry (21)
Chemical industry	Pharmaceutical and chemical industry (23–26)
Metal industry	Metal industry (27–28)
Machine industry	Machine industry (29, 34, 35)
Electronic industry	Electronics industry (30,31,32,33)
Other industries	Other industry (22, 36)
Utilities	Utilities (37, 40, 41, 90)
Construction	Construction (45)
Sales	Sales (50,51,52)
Private services for business	Business services (67,72, 73, 74)
Other private services	Other services (55, 60, 61, 62, 63, 64, 65, 66, 70, 71)
Regional variables	
Region	A dummy for each NUTS 3 region (18 regions)
One location	Located only in one region = 1, otherwise = 0.
Centre region	Regional (NUTS 3) centre municipality = 1, otherwise = 0

Note: Estimations also include year dummies for the treatment years (2002–2007).

TABLE 7 R&D subsidy effect (ATT) on employment and value added by employee skill level (CDID)

Dependent variable:	log (employment)	log (value added)
Panel A: Low-skill firms (share of workers with higher tertiary education under the median)		
Treatment year	0.017 (0.007)**	- 0.005 (0.014)
T + 1	0.027 (0.011)**	- 0.001 (0.018)
T + 2	0.035 (0.013)***	0.008 (0.021)
T + 3	0.047 (0.015)***	0.024 (0.022)
T + 4	0.050 (0.017)***	0.025 (0.024)
T + 5	0.047 (0.020)**	0.049 (0.028)
Treated (off support): 632 (1) Control (all): 1,049 (24,881)		
Panel B: High-skill firms (share of workers with higher tertiary education above the median)		
Treatment year	0.029 (0.009)***	0.001 (0.022)
T + 1	0.045 (0.014)***	- 0.014 (0.027)
T + 2	0.046 (0.017)**	0.001 (0.032)
T + 3	0.061 (0.021)***	0.044 (0.033)
T + 4	0.065 (0.023)***	0.053 (0.035)
T + 5	0.080 (0.025)***	0.075 (0.036)**
Treated (off support): 580 (8) Control (all): 785 (7,709)		

Significance: 10 % level (*), 5 % level (**) and 1 % level (***). Robust standard errors are in parentheses (Abadie and Imbens 2009).

TABLE 8 R&D subsidy effect on labour productivity before and after the subsidy (CDID). Constrained sample includes only subsidised firms in the period 2004–2007.

Dependent variable:	Productivity		log (Productivity)	
	ATT	(s.e.)	ATT	(s.e.)
T-4	298	(727)	0.022	(0.021)
T-3	745	(783)	0.004	(0.015)
T-2	1,755	(815)**	0.029	(0.017)*
T-1	515	(595)	- 0.007	(0.008)
Treatment year	- 1,206	(725)*	- 0.017	(0.012)
T + 1	- 2,979	(854)***	- 0.033	(0.015)**
T + 2	- 2,032	(1,003)**	- 0.015	(0.019)
T + 3	- 1,092	(1,023)	0.005	(0.017)
T + 4	469	(1,099)	0.020	(0.020)
T + 5	438	(1,181)	0.008	(0.019)
Treated (off support):	643 (3)		628 (2)	
Control (all):	1,018 (16,751)		1,008 (16,699)	

Significance: 10 % level (*), 5 % level (**) and 1 % level (***). Robust standard errors are in parentheses (Abadie and Imbens 2009).

TABLE 9 R&D subsidy and survival propensity over the five-year period after the subsidy year

Dependent variable: 1 = Firm survives until year T + x, 0 = otherwise.			
	All firms	Low-skill firms	High-skill firms
T + 1	0.004	0.003	0.001
	(0.003)	(0.003)	(0.003)
T + 2	0.026***	0.028***	0.022***
	(0.005)	(0.006)	(0.008)
T + 3	0.033***	0.035***	0.038***
	(0.007)	(0.009)	(0.012)
T + 4	0.042***	0.042***	0.049***
	(0.009)	(0.011)	(0.014)
T + 5	0.042***	0.054***	0.046***
	(0.011)	(0.013)	(0.016)
Treated (off support):	1,800 (8)	878	927(2)
Control (all):	2,896 (52,492)	1,543 (39,561)	1,339 (12,931)

Significance: 10 % level (*), 5 % level (**) and 1 % level (***). Robust standard errors are in parentheses (Abadie and Imbens 2009).

Supplementary Online Appendix

TABLE W1 R&D subsidies and subsidised firms in the used sample

Year	Subsidised firms	Only direct subsidies	Only loans	Only subordinated loans	Average subsidy per employee, €
2002	166	101	14	2	4,385
2003	221	158	9	1	3,826
2004	213	161	10	3	4,009
2005	201	143	6	0	4,911
2006	213	172	5	0	4,505
2007	207	164	14	0	5,671
Total	1221	899	58	6	4,551

TABLE W2 Balancing condition before and after the matching

Panel A: All firms				
	Unmatched		NN (2)	
	t-test	Bias>10	t-test	Bias>10
Number of imbalanced covariates	36	28	0	0
Percent of imbalanced covariates	66 %	52 %	0 %	0 %
Mean bias	10.8		2.2	
Panel B: Low-skill firms				
	Unmatched		NN (2)	
	t-test	Bias>10	t-test	Bias>10
Number of imbalanced covariates	37	28	0	0
Percent of imbalanced covariates	67 %	52 %	0 %	0 %
Medium bias	13.2		2.6	
Panel C: High-skill firms				
	Unmatched		NN (2)	
	t-test	Bias>10	t-test	Bias>10
Number of imbalanced covariates	32	28	1	1
Percent of imbalanced covariates	59 %	52 %	2 %	2 %
Medium bias	11.0		3.1	

Bias>10: Rosenbaum & Rubin (1985) standardised bias criteria indicating covariates with standardised bias over 10. NN (2): Nearest neighbourhood matching using two nearest neighbours by propensity score. Note 1: two-tailed t-test indicates the covariate mean differences at the 5 per cent level. Note 2: the one covariate that is not balanced in the high-skill firm group is a dummy variable for the machine industry. Excluding this one group has no impact on the main results.

TABLE W3 R&D subsidy effect on firm productivity – Trimming the propensity score (2 % and 5 %) and experiments with alternative matching strategies

Dependent variable:	Productivity		Log(Productivity)	
	ATT	s.e	ATT	s.e
Panel A: Nearest neighbour (2), trimmed 2 %				
Treatment year	- 1,057	(707)	- 0.008	(0.012)
T + 1	- 1,903	(895)**	- 0.029	(0.014)**
T + 2	- 1,004	(1,038)	- 0.010	(0.017)
T + 3	- 888	(1,047)	- 0.004	(0.016)
T + 4	265	(1,161)	0.007	(0.017)
T + 5	410	(1,183)	0.031	(0.018)*
Treated (off support):	1,250 (26)		1,197 (24)	
Control (all):	33,077		32,590	
Panel B: Nearest neighbour (2), trimmed 5 %				
Treatment year	- 1,515	(730)**	- 0.012	(0.012)
T + 1	- 2,755	(822)***	- 0.024	(0.013)*
T + 2	- 1,740	(906)*	- 0.016	(0.016)
T + 3	- 1,431	(949)	- 0.002	(0.015)
T + 4	- 182	(1,004)	0.003	(0.016)
T + 5	108	(1,091)	0.018	(0.018)
Treated (off support):	1215 (63)		1,160 (61)	
Control (all):	1,910 (33,077)		1,863 (32,590)	
Panel C: Nearest neighbour (4)				
Treatment year	- 1,441	(640)**	- 0.010	(0.010)
T + 1	- 2,326	(725)***	- 0.026	(0.012)**
T + 2	- 1,347	(817)*	- 0.019	(0.014)
T + 3	- 836	(846)	- 0.002	(0.013)
T + 4	143	(874)	0.007	(0.014)
T + 5	- 7	(948)	0.027	(0.015)
Treated (off support):	1,273 (3)		1,216 (5)	
Control (all):	3,417 (33,077)		3,312 (32,590)	
Panel D: Mahalanobis one-to-one nearest neighbourhood matching (exact industry)				
Treatment year	- 958	(703)	- 0.019	(0.013)
T + 1	- 2,306	(840)***	- 0.029	(0.014)**
T + 2	- 1,743	(939)*	- 0.017	(0.016)
T + 3	- 1,556	(949)	- 0.006	(0.016)
T + 4	- 352	(1,013)	0.001	(0.016)
T + 5	- 1,055	(1,066)	0.007	(0.019)
Treated (off support):	1,273 (3)		1,221	
Control (all):	1,023 (33,077)		1,023 (32,590)	

Significance: 10 per cent level (*), 5 per cent level (**) and 1 per cent level (***). Robust standard errors are in parentheses; see Abadie and Imbens (2009). Note: The treatment year is the year when a subsidy is granted, and the sequential numbers indicate the years after the subsidy is granted.

TABLE W4 R&D subsidy effect (CDID) on firm productivity, employment, and value added without log transformation.

	ATT	s.e
Dependent variable: Productivity		
Treatment year	- 1,282	(624) **
T + 1	- 2,017	(677)** *
T + 2	- 1,560	(781) **
T + 3	- 1,332	(830)
T + 4	- 1,583	(890) *
T + 5	- 657	(917)
Dependent variable: Employment		
Treatment year	0.949	(0.322)** *
T + 1	1.095	(0.556)**
T + 2	1.204	(0.694)*
T + 3	1.191	(0.806)
T + 4	1.252	(0.917)
T + 5	1.444	(1.010)
Dependent variable: Value added		
Treatment year	- 21,127	(29,867)
T + 1	- 33,721	(29,867)
T + 2	- 14,325	(52,485)
T + 3	- 7,963	(60,621)
T + 4	- 65	(69,063)
T + 5	17,550	(75,595)
Treated (off): 1,273 (2)	Control (all): 1,912 (33,077)	

TABLE W5 R&D subsidy effect on labour productivity by firm size (CDID)

Dependent variable: log (productivity)			
	Relative size of the subsidy		
	All subsidies	Small subsidies	Large subsidies
Panel A: Small SMEs (firm size under the median)			
Treatment year	- 0.006 (0.020)	0.014 (0.019)	- 0.012 (0.025)
T + 1	- 0.031 (0.023)	0.005 (0.024)	- 0.043 (0.033)
T + 2	- 0.032 (0.026)	0.018 (0.041)	0.008(0.039)
T + 3	- 0.024 (0.025)	0.016 (0.025)	0.006 (0.034)
T + 4	- 0.031 (0.028)	0.016 (0.030)	0.011 (0.037)
T + 5	- 0.024 (0.026)	0.042 (0.034)	- 0.002 (0.039)
Treated (off support):	583 (3)	245	340 (1)
Control (all):	929 (22,515)	442 (22,515)	527 (22,515)
Panel B: Large SMEs (median sized firm or above)			
Treatment year	- 0.011 (0.014)	- 0.010 (0.014)	- 0.032 (0.023)
T + 1	- 0.019 (0.016)	- 0.008 (0.015)	- 0.050 (0.028)*
T + 2	- 0.003 (0.018)	- 0.007(0.018)	0.003 (0.033)
T + 3	0.005 (0.019)	0.007 (0.020)	0.020 (0.032)
T + 4	0.024 (0.020)	0.005 (0.022)	0.040 (0.032)
T + 5	0.032 (0.021)	- 0.003 (0.021)	0.057 (0.046)
Treated (off support):	633 (1)	379	252 (3)
Control (all):	946 (10,045)	652 (10,045)	408 (10,045)

Significance: 10 % level (*), 5 % level (**) and 1 % level (***). Robust standard errors are in parentheses (Abadie and Imbens, 2009). Note: the median size of the subsidised firms in our sample is 33.1 employees in the year before the subsidy is granted.

TABLE W6 R&D subsidy effect (ATT) on the share of workers by skill level with different cut-off points (re-estimation of Table 5)

Dependent variable: Percentage of workers at different levels/fields of education			
	Higher tertiary education		Less than higher tertiary education
	Natural science and technology	Other fields	
Panel A: Low-skill firms (share of workers with tertiary education under the 40th percentile)			
Treatment year	0.100 (0.056)	0.002 (0.042)	-0.001 (0.049)
T + 1	0.186 (0.071)***	0.163 (0.068)**	-0.104 (0.090)
T + 2	0.114 (0.077)	0.255 (0.082)***	-0.361 (0.115)***
T + 3	0.111 (0.084)	0.257 (0.085)***	-0.318 (0.131)**
Average share (%)	0.44	0.41	99.2
Treated (off): 485 (1) Control (all): 836 (22,381)			
Panel B: High-skill firms (share of workers with tertiary education above the 60th percentile)			
Treatment year	0.510 (0.266)*	-0.170 (0.225)	0.043 (0.321)
T + 1	0.664 (0.375)*	-0.376 (0.275)	-0.227 (0.441)
T + 2	0.355 (0.425)	-0.307 (0.289)	-0.367 (0.518)
T + 3	0.182 (0.478)	-0.601 (0.335)	-0.174 (0.543)
Average share (%)	19.6	8.2	72.2
Treated (off): 471 (6) Control (all): 623 (5,731)			

Significance: 10 % level (*), 5 % level (**) and 1 % level (***). Robust standard errors are in parentheses (Abadie and Imbens, 2009). Note 1: Higher tertiary education comprises workers with master's, licentiate or doctoral degrees.

TABLE W7 Placebo effects – R&D subsidy effect (ATT) on share of workers by education level. Sample is constrained (subsidy years 2004–2007) to show possible placebo effects before actual treatment year

Dependent variable: Percentage of workers at different levels/fields of education			
	Higher tertiary education		Less than higher tertiary education
	Natural science and technology	Other fields	
Panel A: All firms			
T - 2	-0.180 (0.143)	0.145 (0.191)	-0.170 (0.163)
T - 1	0.120 (0.140)	-0.016 (0.037)	-0.130 (0.156)
Treatment year	0.439 (0.132)***	-0.059 (0.097)	-0.205 (0.160)
T + 1	0.501 (0.184)***	0.020 (0.137)	-0.334 (0.203)*
T + 2	0.469 (0.214)**	0.002 (0.149)	-0.295 (0.226)
T + 3	0.586 (0.238)**	-0.058 (0.167)	-0.124 (0.267)
Average share (%)	8.16	3.81	88.03
Treated (off support): 761 (3)	Control (all): 1,188 (19,871)		
Panel B: Low-skill firms (share of workers with tertiary education under the median)			
T - 2	-0.078 (0.078)	-0.001 (0.043)	0.050 (0.081)
T - 1	-0.033 (0.048)	-0.039 (0.042)	0.005 (0.075)
Treatment year	0.188 (0.061)***	0.081 (0.050)	0.028 (0.070)
T + 1	0.288 (0.088)***	0.346(0.075)***	-0.162 (0.106)**
T + 2	0.272 (0.097)***	0.429(0.096)***	-0.403 (0.136)***
T + 3	0.212 (0.109)*	0.413(0.095)***	-0.584 (0.150)***
Average share (%)	0.51	0.50	98.99
Treated (off support): 386 (1)	Control (all): 642 (15,009)		
Panel C: High-skill firms (share of workers with tertiary education above the median)			
T - 2	-0.089 (0.281)	0.334 (0.198)*	-0.396 (0.319)
T - 1	-0.015 (0.284)	-0.092 (0.109)	-0.286 (0.299)
Treatment year	0.506 (0.287)*	-0.067(0.232)	-0.049 (0.310)
T + 1	0.474 (0.336)	-0.226 (0.245)	-0.370 (0.417)
T + 2	0.251 (0.388)	-0.497 (0.282)*	-0.425 (0.465)
T + 3	0.339 (0.450)	-0.678 (0.322)**	-0.158 (0.514)
Average share (%)	15.76	7.16	77.08
Treated (off support): 370 (7)	Control (all): 520 (4,834)		

Significance: 10 % level (*), 5 % level (**) and 1 % level (***). Robust standard errors are in parentheses (Abadie and Imbens, 2009). Note 1: Higher tertiary education comprises workers with master's, licentiate or doctoral degrees. Note 2: Periods (t-1) and (t-2) show the possible placebo effects before matching; data on education were not available for early periods.

TABLE W8 Impact of R&D subsidies on labour productivity over one year and three years (CDID).

Dependent variable: log (Productivity)				
	Sample 1		Sample 2	
	ATT	(s.e.)	ATT	(s.e.)
Treatment year	- 0.021	(0.012)*	- 0.020	(0.012)
T + 1	- 0.039	(0.015)***	- 0.036	(0.015)***
T + 2			- 0.019	(0.016)
T + 3			- 0.007	(0.017)
Treated (off support):	1,798 (1)		1,638 (2)	
Control group (all):	2,828 (50,611)		2,370 (44,213)	

Significance: 10 % level (*), 5 % level (**) and 1 % level (***). Robust standard errors are in parentheses (Abadie and Imbens, 2009).

TABLE W9 R&D subsidy effect on labour productivity (only direct subsidies).

Dependent variable: log (productivity)		
	Relative size of the subsidy	
	All subsidies	Large subsidies
Treatment year	0.006 (0.013)	- 0.024 (0.022)
T + 1	- 0.022 (0.013)*	- 0.050 (0.027)*
T + 2	- 0.023 (0.017)	- 0.059 (0.026)**
T + 3	- 0.022 (0.015)	- 0.045 (0.026)*
T + 4	- 0.020 (0.016)	- 0.067 (0.029)**
T + 5	- 0.001 (0.018)	- 0.035 (0.032)
Treated (off support):	898 (1)	304 (1)
Control (all):	1,500 (32,590)	534 (32,590)

Significance: 10 % level (*), 5 % level (**) and 1 % level (***).
Robust standard errors are in parentheses (Abadie and Imbens, 2009).

TABLE W10 Heterogeneity by sector - Industrial and service sector

Dependent variable: log (productivity)		
	Industrial sector	Service sector
Treatment year	- 0.012 (0.015)	- 0.028 (0.024)
T + 1	- 0.036 (0.018)**	- 0.011 (0.030)
T + 2	- 0.034 (0.020)*	- 0.018 (0.030)
T + 3	- 0.025 (0.020)	0.036 (0.029)
T + 4	- 0.027 (0.022)	0.034 (0.029)
T + 5	- 0.010 (0.023)	0.043 (0.035)
Treated (off support):	790 (3)	380 (2)
Control (all):	1,194 (10,101)	512 (17,370)

Significance: 10 % level (*), 5 % level (**) and 1 % level (***). Robust standard errors are in parentheses (Abadie and Imbens, 2009). Industrial sector: food, textile, wood, paper, chemical, metal, and machinery, electronic and other industries. Service sector: sales, business services and other services. Note: Utility and construction sectors are excluded from both groups.

TABLE W11 Descriptive survival rate cross tabulations by initial (subsidy year) productivity level and treatment status

	Survival rate (T relative to period T+5)	
	Subsidized firms	Unsubsidized similar firms (by matching)
Under median (or median) productivity at period T	88.3 %	83.8 %
Above median productivity at period T	86.5 %	83.7 %

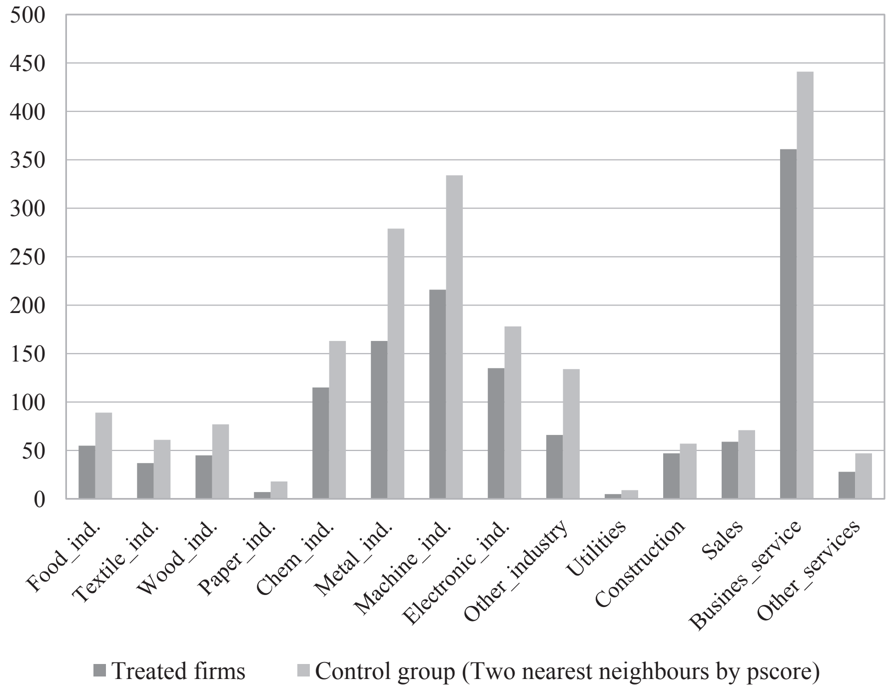

FIGURE W1 Industry distribution in the sample by treatment status.

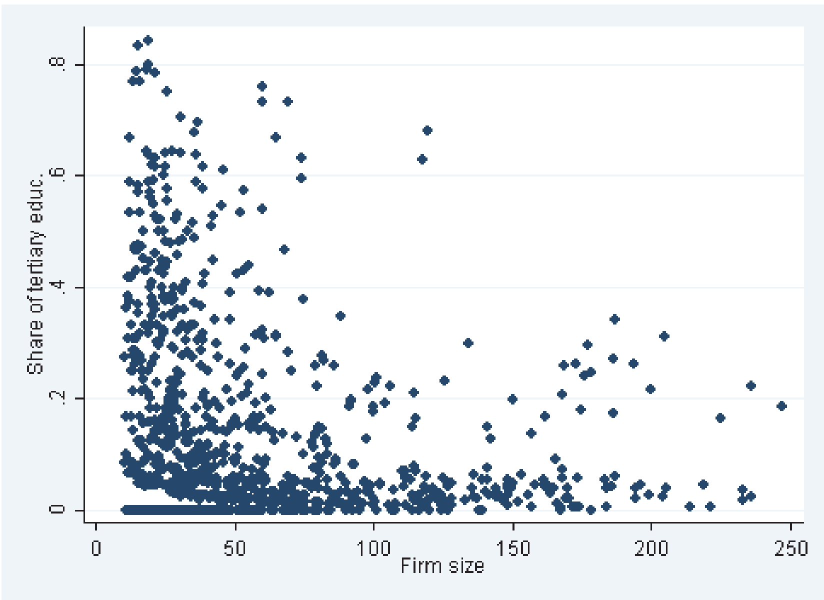

FIGURE W2 Firm size and share of employees with tertiary education.

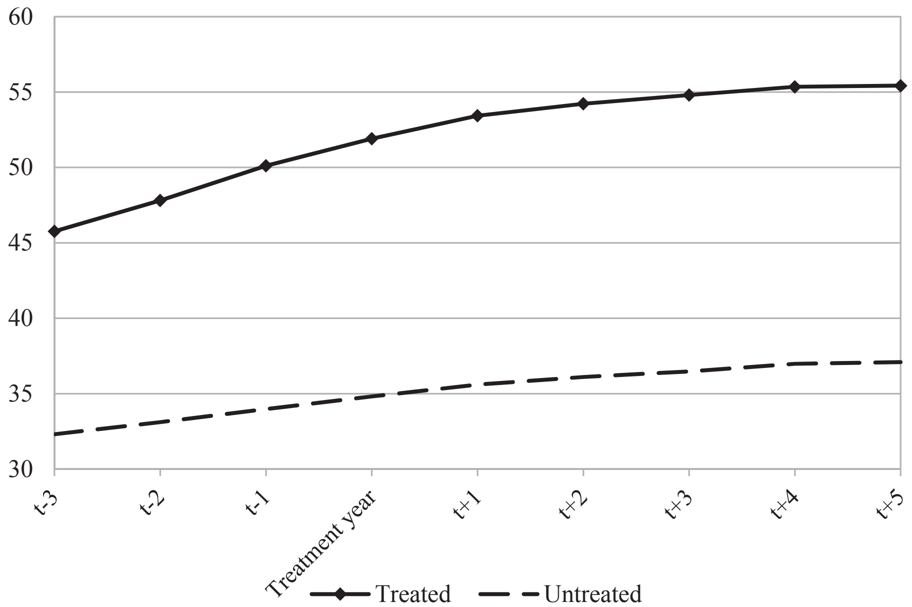

FIGURE W3.1 Average employment before matching.

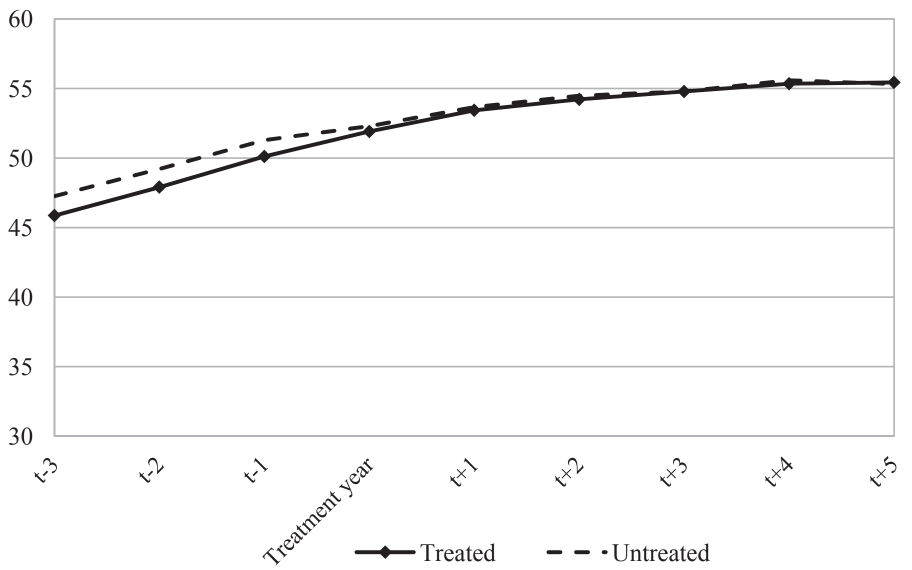

FIGURE W3.2 Average employment after matching.

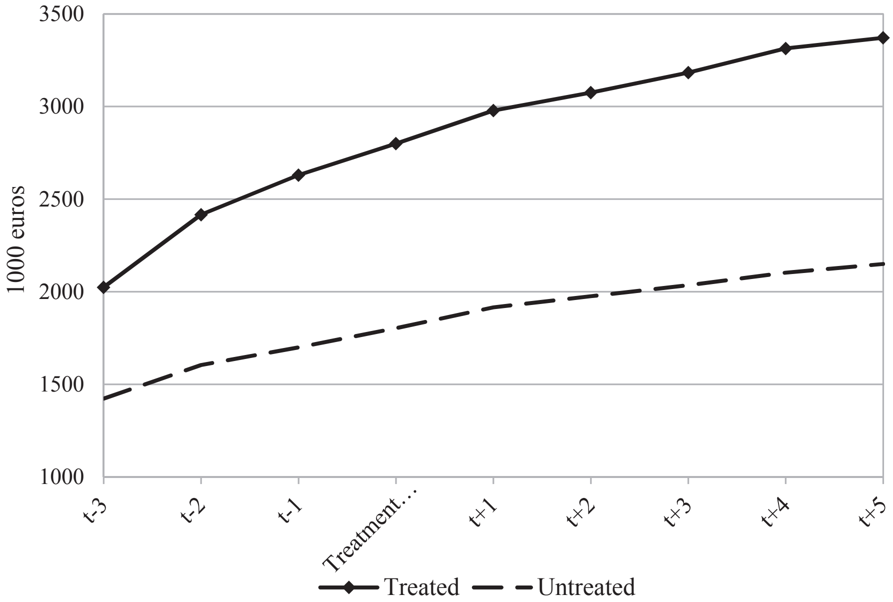

FIGURE W4.1 Average value added before matching.

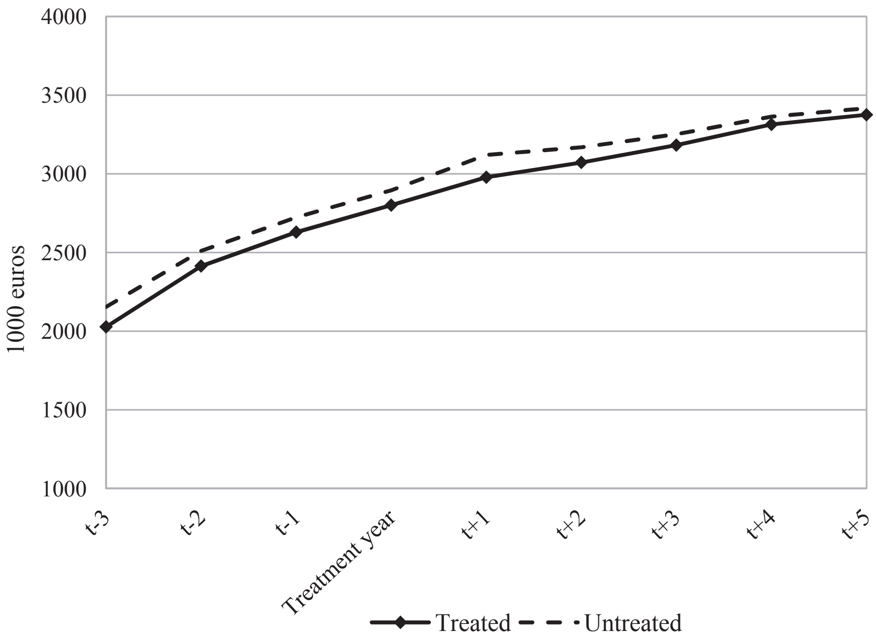

FIGURE W4.2 Average value added after matching.

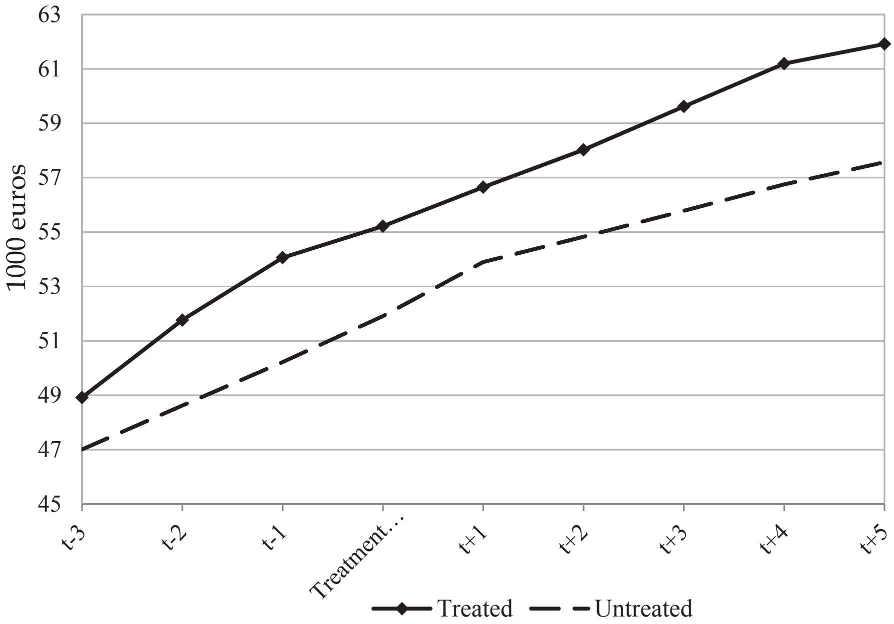

FIGURE W5.1 Average productivity before matching.

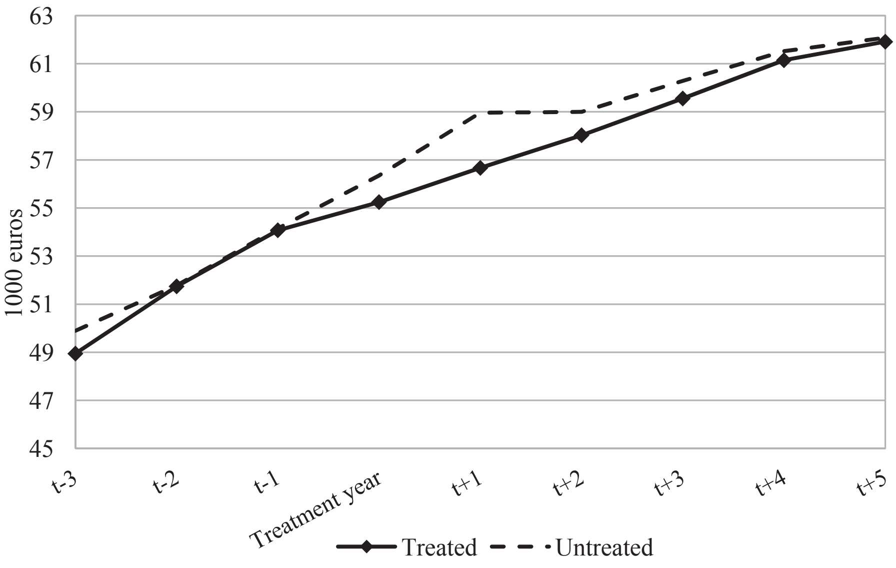

FIGURE W5.2 Average productivity after matching

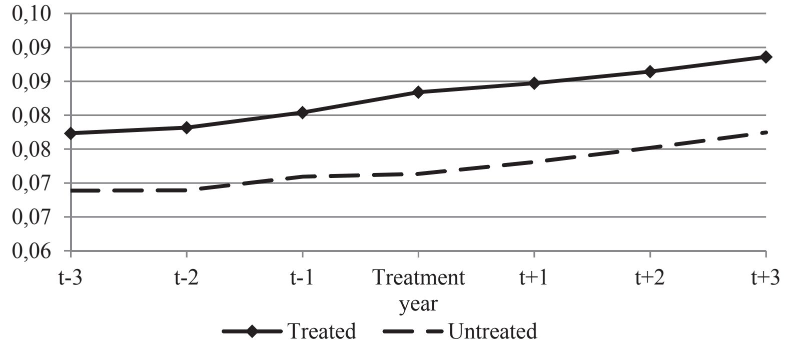

FIGURE W6.1 Percentage of workers with higher tertiary education (science and technology) after matching.

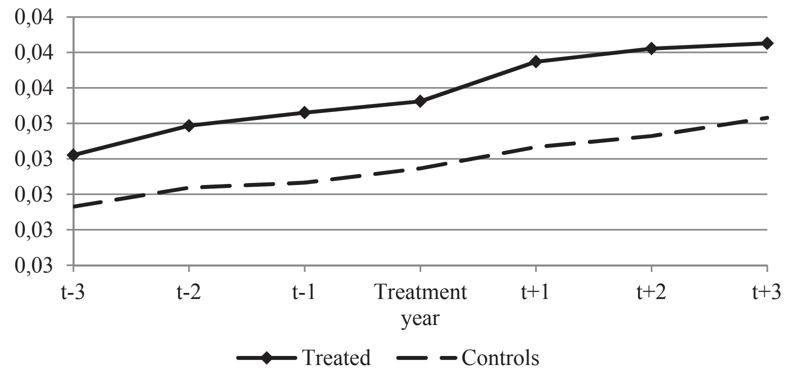

FIGURE W6.2 Percentage of workers with higher tertiary education (other than science and technology) after matching.

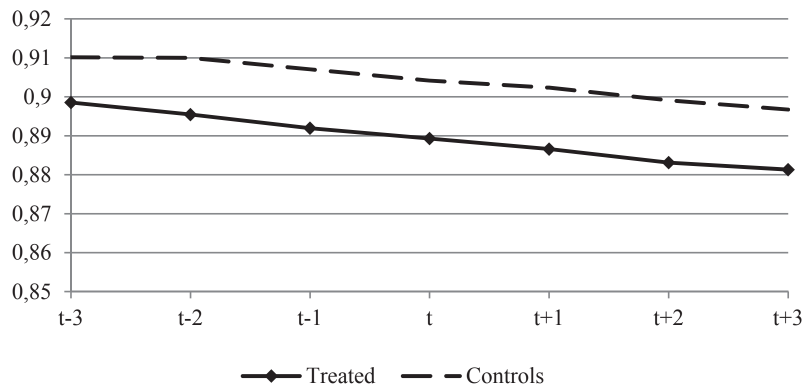

FIGURE W6.3 Percentage of workers with less than higher tertiary education after matching.

SUMMARY IN FINNISH (YHTEENVETO)

Taloudellisia tutkimuksia korkeakoulutuksesta ja tuottavuudesta

Väitöskirja koostuu johdantoluvusta ja neljästä empiirisestä artikkelista. Väitöskirjan kaksi ensimmäistä artikkelia keskittyvät tutkimaan kuinka korkeakouluopiskelijan opiskeluaikainen työssäkäynti on yhteydessä valmistumisen jälkeisiin tuloihin ja alueelliseen liikkuvuuteen. Kolmas artikkeli tarkastelee niiden henkilöiden työmarkkinamenestystä, jotka lopettavat yliopisto-opintonsa ennen varsinaista valmistumista. Neljännessä artikkelissa tutkitaan Tekesin myöntämien yritystukien tuottavuusvaikutusta yritystasolla. Tutkimuksessa käytetyt aineistot pohjautuvat rekisteriaineistoista muodostettuihin otosaineistoihin.

Luvussa 2 tutkitaan opiskeluaikaisen työssäkäynnin vaikutusta valmistumisen jälkeisiin tuloihin. Aikaisempi tutkimuskirjallisuus on havainnut, että opiskeluaikainen työssäkäynti näkyy valmistumisen jälkeen parempana työllisyys- ja tulokehityksenä. Työssäkäynnin positiivinen vaikutus kuitenkin häviää, kun huomioon otetaan yksilöiden valikoituminen työlliseksi opintojen aikana. Tämän tutkimuksen ensimmäinen essee tutkii yliopisto- ja ammattikorkeakouluopiskelijoiden (AMK-opiskelijoiden) työssäkäyntiä opintojen aikana ja kuinka työssäkäynti on yhteydessä valmistumisen jälkeen ansiokehitykseen. Tutkimuksessa käytetty otos sisältää yli 6 700 henkilöä väliltä 1987 ja 2006. Aineisto sisältää useita taustamuuttujia, joita käytetään tutkimuksessa kontrollimuuttujina. Analyysissa keskitytään kahden vuoden periodiin valmistumisen jälkeen, koska tällöin opiskeluaikaisen työn vaikutuksen tulisi olla merkittävimmillään.

PNS-estimaattorilla estimoidut tulokset osoittavat, että opiskeluaikainen työssäkäynti on positiivisessa yhteydessä ansioihin heti valmistumisen jälkeen. Opiskeluaikaisen työssäkäynnin positiivinen yhteys ei ole kuitenkaan kovin merkittävä. Kun tutkimuksessa korjataan henkilöiden valikoitumisesta aiheutuvaa harhaa, niin työssäkäynnin positiivinen vaikutus häviää yliopisto-opiskelijoiden osalta, mutta vaikutus jää merkitseväksi AMK-opiskelijoille. Tulokset ovat yhtenevät aikaisemman kirjallisuuden kanssa. Mikäli henkilöiden havaitsemattomista ominaisuuksista johtuvaa harhaa ei oteta analyysissa huomioon, niin on todennäköistä, että opiskeluaikaisen työssäkäynnin yhteys myöhempään ansiokehitykseen yliarvioidaan.

Luvussa 3 tutkitaan opiskeluaikaisen työssäkäynnin yhteyttä alueelliseen liikkuvuuteen. Tämän yhteyden ymmärtäminen on tärkeää, koska useat julkiset puheenvuorot ovat ehdottaneet työssäkäynnin rajoittamista, jotta keskimääräisiä opintoaikoja saataisiin lyhennettyä. Toisaalta kansainvälinen tutkimuskirjallisuus on havainnut että korkeakoulutettujen alueellinen liikkuvuus on laskenut viimeisten 20 vuoden aikana, mutta tarkkaa syytä tähän ei tiedetä. Tämä tutkimus keskittyy tarkastelemaan henkilöitä jotka valmistuvat yliopistosta, ammattikorkeakoulusta tai ammattikoulusta välillä 1991 ja 2004. Tutkimuksessa käytetyn otos sisältää noin 10000 henkilöä. Muuttoalttiutta yli maakuntarajojen seurataan kolmen vuoden ajan valmistumisen jälkeen.

Tulokset osoittavat että opiskeluaikainen työssäkäynti on negatiivisessa yhteydessä valmistumisen jälkeiseen liikkuvuuteen. Etenkin niiden yliopisto- ja AMK-opiskelijoiden liikkuvuus on alhaisempi valmistumisen jälkeen, jotka työskentelevät täysipäiväisesti opintojen aikana. Työssäkäynnin negatiivinen yhteys on myös tilastollisesti merkitsevämpi Helsingin seutukunnan ulkopuolisilla alueilla kuin Helsingissä. Tulokset tarjoavatkin yhden mahdollisen selityksen siihen, että miksi korkeakoulutettujen liikkuvuus on laskenut viimeisten vuosikymmenien aikana.

Luvussa 4 tutkitaan yliopisto-opintonsa kesken jättäneiden henkilöiden työmarkkinamenestyksen kehitystä neljän vuoden ajalta keskeyttämisen jälkeen. Tutkimusotos koostuu 670 keskeyttäjästä ja noin 8500 kontrolliryhmänä käytetystä henkilöstä vuosina 1999–2002. Opintonsa kesken jättäneitä opiskelijoita verrataan mahdollisimman samankaltaisiin opiskelijoihin ja vastavalmistuneihin. Tulokset osoittavat, että keskeyttäjät ansaitsevat noin 11 000 euroa vähemmän neljä vuotta keskeyttämispäätöksen jälkeen kuin vastaavat opiskelijat, jotka eivät keskeytä opintoja ja valmistuvat. Työllisyys on myös merkitsevästi alhaisempi keskenjättäneiden joukossa. Yllättävää on kuitenkin se, että keskeyttäminen on positiivisessa yhteydessä yrittäjyyteen.

Luvussa 5 tutkitaan Tekesin myöntämien yritystukien vaikutusta työn tuottavuuteen yritystasolla. Yritystukien mahdolliset ulkoisvaikutukset rajataan tarkastelun ulkopuolelle, sillä näiden kausaalinen mallintaminen ei ole mahdollista nykyisillä empiirisillä menetelmillä. Tutkimusaineisto sisältää kaikki suomalaiset pienet ja keskisuuret yritykset väliltä 2000–2012. Tukien vaikutusta työntuottavuuteen tutkitaan niin sanotulla ehdollisella ennen-jälkeen menetelmällä viiden vuoden ajalta tukien myöntämisen jälkeen. Tutkimustulosten syy-seuraussuhteen oikeellisuus perustuu oletukseen siitä, että samankaltaistetut ei-tuetut ja tuetut yritykset omaavat kehityksessään samankaltaisen trendin. Visuaalinen tarkastelu ja yritysryhmien keskiarvojen tilastollinen vertailu tukevat päätelmää, että näin voidaan olettaa.

Tulosten mukaan Tekesin myöntämillä tuilla (sisältäen suorat tuet ja lainapäätökset) ei havaita olevan positiivista vaikutusta työn tuottavuuteen viiden vuoden aikana tuen myöntämisen jälkeen. Tuilla havaitaan olevan tilastollisesti merkitsevä vaikutus työllisyyteen, mutta vaikutus ei realisoidu työntuottavuudeksi (tuen vaikutus arvonlisäykseen on siis liian pieni). Tehdyt lisätarkastelut myös soittavat, että yritystuilla on yritysten selviytymistä parantava vaikutus. Tulokset ovat yhtenevät aikaisemman suomalaisen tutkimuskirjallisuuden kanssa. Tulokset osoittavat myös selvästi sen, että mikäli yritysten valikoitumista ei oteta huomioon, niin yritystukien positiiviset vaikutukset yliarvioidaan.

Tämän tutkimuksen tuloksia tulee käyttää harkiten poliittisen päätännänteon tukena, sillä tarkastelluista tutkimuskysymyksistä on syytä tehdä lisää syy-seuraussuhteisiin keskittyvää tilastollista analyysia. Tästä huolimatta, väitöskirjan kappaleet tukevat seuraavia yleisiä johtopäätöksiä. Erilaisia arviointeja tehtäessä on otettava huomioon henkilöiden ja yritysten valikoituminen arvioitavaan ohjelmaan tai tehtävään. Esimerkiksi, opiskeluaikainen työssäkäynti mielletään laajasti yksilö hyödyttäväksi, koska sen nähdään parantavan valmistumisen jälkeistä työmarkkinamenestystä. Mutta kun empiirisessä tarkastelussa pyritään ottamaan huomioon yksilöiden valikoituminen opiskeluaikaiseen työhön, niin työssäkäynnin vaikutuksesta myöhempään työmarkkinamenestykseen ei voida olla enää varmoja. Opiskeluaikaisella työssäkäynnillä saattaa myös olla ei-toivottuja negatiivisia vaikutuksia joita on vaikea havaita. Tutkimuksen toinen johtopäätös liittyykin politiikkatoimien arvioimiseen. Erilaisten politiikkatoimien arviointi voi olla hankalaa, koska toimilla voi olla epäsuoria vaikutuksia joista ei tiedetä tai joita ei pystytä nykyisillä menetelmillä mittaamaan luotettavasti. Esimerkiksi opintotuen tason realinen alentuminen on saattanut houkutella kyvykkäämpiä opiskelijoita työmarkkinoille. Tämä on vuorostaan saattanut vaikuttaa ei-kyvykkäiden opiskelijoiden työmahdollisuuksiin opintojen aikana. Samantapaisesti yritystukien myöntäminen joillekin yrityksistä voi vaikuttaa niiden yritysten toimintaan jotka eivät saa yritystukea. Olisikin suositeltavaa, että suorien vaikutuksien lisäksi tutkimuskirjallisuus keskittäisi entistä suurempaa huomiota epäsuorien vaikutuksien arvioimiseen.