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Empirical essays on education and social insurance policies

Caroline Hall

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Postal address: P O Box 513, 751 20 Uppsala
Visiting address: Kyrkogårdsgatan 6, Uppsala
Phone: +46 18 471 70 70
Fax: +46 18 471 70 71
ifau@ifau.uu.se
www.ifau.se

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Abstract

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This thesis consists of four self-contained essays.

Essay 1: This paper evaluates the effects of the introduction of a more comprehensive upper secondary school system in Sweden in the 1990s. The reform reduced the differences between the academic and vocational educational tracks through prolonging and substantially increasing the academic content of all vocational tracks. The effects of this policy change are identified by exploiting a six year pilot scheme, which preceded the actual reform in some municipalities. The results show that the prolongation of the vocational tracks brought about an increased probability of dropping out among low performing students. Though one important motive behind the policy change was to enable all upper secondary school graduates to pursue a university degree, I find no effects on university enrolment or graduation. There are some indications, however, that attending the longer and more academic vocational tracks may have led to increased earnings in the long run.

Essay 2: (co-authored with Peter Fredriksson, Elly-Ann Johansson and Per Johansson) We examine whether the impact of pre-school interventions on cognitive skills differs by immigrant background. The analysis is based on Swedish data containing information on childcare attendance, rich family background information, the performance on cognitive tests at age 13, and long-run educational attainment for cohorts born between 1967 and 1982. We find that childcare attendance reduces the gap in language skills between children from immigrant backgrounds relative to native-born children. We find no differential effects on inductive skills, however. Nor does childcare appear to affect the distribution of long-run educational attainment.

Essay 3: (co-authored with Laura Hartman) This paper studies a specific type of moral hazard that arises in the interplay between two large public insurance systems in Sweden, namely the sickness insurance (SI) and the

unemployment insurance (UI). Moral hazard can arise from the structure of the benefit levels as for some unemployed persons benefits from the SI are higher than benefits from the UI. We use a reform of the SI system that came into force on 1 July 2003 to identify the effect of economic incentives arising from the different benefit levels. The purpose of the reform was to eliminate the difference in benefits between the two insurance systems. Our results from a duration analysis show clearly that the higher the sickness benefits, the higher the probability of reporting sick.

Essay 4: Previous research suggests that there are substantial interactions between the unemployment insurance (UI) and the sickness insurance (SI) in Sweden. Moral hazard arises in the interplay between these two social insurance systems, since by reporting sick an unemployed person can postpone the UI expiration date and sometimes also receive considerably higher benefits. This paper examines whether these interactions affect the transition rate from unemployment to employment. To study this question I utilize a reform which greatly reduced the incentives for unemployed persons to transfer to the SI. While there is evidence that this reform substantially lowered the incidence of sick reports among the unemployed, I find no evidence suggesting that the reduced sick report rate in turn affected the transition rate to employment.

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While working on this thesis I have had the opportunity to spend time both at the Department of Economics at Uppsala University and the Institute for Labour Market Policy Evaluation (IFAU). I find that these two places together offer a very stimulating research environment, characterized by a friendly and supportive atmosphere as well as a high research standard. I would like to thank all former and present colleagues with whom I have had the chance to discuss research and who have given me advice and commented on my work throughout my time in the Ph D program. The administrative staffs at both the department and the IFAU also deserve special recognition for their professional assistance. Åke Qvarfort's and Jörgen Moen's swift, excellent and reliable computer assistance has been invaluable.

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Caroline Hall, January 2010, Uppsala

Contents

Introduction

This thesis consists of four self-contained empirical essays focusing on essential aspects of the Swedish welfare state: publicly financed education (Essay 1), pre-school/childcare interventions (Essay 2) and social insurance (Essay 3 and 4).

Central to all the essays is the attempt to uncover the causal relationships between the policies studied and the outcomes of interest. Needless to say, understanding the causal impact of different policies is critical in order to make wise policy decisions. Establishing causality is however not easily done in the social sciences. It is difficult from retrospectively collected data, from registers or surveys, to determine if a correlation actually reflects the impact of one variable on another. The correlation could also arise due to some unobserved variable(s) which affects them both, or the variables may be determined simultaneously.

The effect of pre-school/childcare interventions on the development of cognitive skills, which is the theme of the second essay, may serve as an illustration. An observed positive correlation between childcare attendance and children's cognitive achievement at a later age does not necessarily mean that attending childcare, rather than being cared for at home, actually enhances cognitive ability. The parents who enroll their children in childcare may be different from those who stay at home in ways which are also related to children's development of cognitive skills. For instance, high cognitive ability among parents may be positively related both to their children's cognitive skills and childcare attendance. Not accounting for parental ability in the analysis would then give rise to a spurious relationship between childcare and children's cognitive achievement. It is also possible that a child's cognitive development during their pre-school years influences the parents' decision on childcare, implying that the direction of causality is, at least partly, the other way around.

In order to resolve whether a change in one variable actually causes a change in another, all other relevant factors need to be held constant. In e.g. the medical sciences, such situations are created through experiments where treatment is randomly assigned among individuals. However, in the social sciences experiments are difficult to conduct. Instead researchers often try to find some naturally occurring phenomenon which has the properties of an experiment. Such circumstances, generally referred to as 'natural experiments' or 'quasi-experiments', often arise due to policy changes which only affect parts of the population and thereby generate natural treatment and comparison groups.

Three of the essays in this thesis exploit such natural experiments in order to identify causal relationships: the first essay uses a pilot scheme and the third and forth use a reform. In the second essay we have not found any experiment-like situation to make use of. Our empirical strategy is instead to account for as many relevant variables as possible. We then use sensitivity analyses to evaluate the credibility of this approach.

Below I present the four essays one by one. I begin each section by putting the research question into a broader context and describing how my work contributes to the existing literature. Thereafter I discuss the empirical strategies used to estimate causal relationships. Each section ends with a summary of the results and their implications for policy.

Comprehensive versus selective upper secondary schooling

The first essay studies the consequences of introducing a more comprehensive upper secondary school system for students' educational and labor market outcomes.

A much debated issue within education policy is whether, and at what age, students should be separated between academic and more vocationally oriented educational tracks. Across countries there is great diversity in these policies. In some countries students are separated into different tracks already in primary school; in others the school system remains comprehensive all through secondary school. Moreover, countries differ in terms of the number of different educational tracks available as well as regarding the extent of curricular differences among the available tracks.

Since the mid-20th century there has been a tendency in many OECD countries towards adopting more comprehensive school systems (see e.g. Leschinsky and Mayer 1990). The effects of these policy changes are generally difficult to evaluate, most importantly since such reforms usually take place simultaneously across a whole nation. Such implementation schemes naturally make it difficult to separate the effects of these particular reforms from those of other concurrent changes and time trends. What effects a shift towards a more comprehensive school system has on students' outcomes is therefore disputed in the literature (see e.g. Manning and Pischke 2006, and Waldinger 2007).

I evaluate a major educational reform in Sweden in the 1990s in which the upper secondary school was made more comprehensive. The reform was unusual in that it was preceded by an extensive pilot scheme. The pilot scheme only existed in parts of the country, which makes it possible to avoid the methodological difficulty described above.

The reform of interest made the school system more comprehensive through substantially reducing the differences in curricula between the academic and vocational tracks. This was done by considerably increasing the academic content as well as the length (from two to three years) of all vocational tracks. As a result of these changes, vocational students attained basic eligibility for university studies. In theory there are compelling arguments both for and against this type of policy change. Opponents of the reform argue that not all students may benefit from an upper secondary education with a substantial academic content. Proponents tend to stress that it is advantageous that students no longer have to choose a definitive educational direction at an early age, a decision which otherwise might have restricted them in the future.[1]

By exploiting the pilot scheme I can disentangle the effects of the reform from the effects of other concurrent changes and time trends which also affect student outcomes. The pilot scheme took place during a six year period and was introduced at different times in different municipalities. The included municipalities also participated to different extents. The idea behind the empirical strategy is to compare the development of educational attainment and earnings over time for students from municipalities that participated to a large extent, with how the same outcomes developed for students from municipalities that did not participate, or that participated to a more limited extent.

The results show that the introduction of more comprehensive upper secondary schooling – through prolonging and adding more academic content to the vocational tracks – increased the drop out rate. The probability of dropping out of upper secondary school is estimated to have increased by 3.8 percentage points as a consequence of attending a new rather than an old vocational track. This increase is entirely driven by students who finished compulsory school with a below-average grade point average. This finding thereby gives support to the fear held among opponents to the reform; that not everyone benefits from an upper secondary education with a substantial academic content.[2]

Although an important motive behind the policy change was to enable vocational students to study at the university, the results give no indication that attending the new tracks induced more vocational students to pursue university studies. There is some weak evidence, however, that the longer and more academic vocational tracks led to increased earnings in the long run.

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[1] See e.g. Brunello and Checchi (2007) for a general discussion of the relative merits of a comprehensive versus a selective school system.

[2] This finding is in line with the results of a few studies investigating the effects of raised graduation standards on high school dropout decisions in the US; see e.g. Lilliard and DeCicca (2001), and Dee and Jacob (2006).

Pre-school interventions and the development of cognitive skills

The second essay (co-written with Peter Fredriksson, Elly-Ann Johansson and Per Johansson) examines whether pre-school interventions reduce the gap in cognitive achievement between immigrant and native students.

Immigrant students typically perform substantially worse than native students in the OECD countries. According to e.g. the Program for International Student Assessment (PISA), there are considerable performance differences in mathematics, reading and science (OECD 2006). The size of the performance differences between immigrants and natives varies across countries and the gaps are particularly large in Middle and Northern Europe (Schneeweis 2009).

The differences in immigrant/native educational gaps across countries depend, partly, on the characteristics of immigrants; in particular, immigrant source countries are likely to be important. But host-country educational institutions should also matter. It is intuitively plausible that pre-primary education is one important factor. Indeed, Schneeweis (2009), in her analysis of aggregate cross-country data, finds that achievement gaps between immigrant and native students are smaller in countries that make extensive use of pre-primary education.

The main contribution of this paper is that we directly examine whether pre-school interventions reduce the immigrant/native gap in cognitive performance. To do this we use Swedish individual data with information on enrolment in pre-school/childcare interventions[3], rich family background information, measures of cognitive achievement at age 13 and long-run educational attainment. We thus study the medium and long-run effects of pre-school interventions.

There are several recent studies which also analyze the effects of (universal) pre-school/childcare interventions; see e.g. Baker et al. (2008), Berlinski et al. (2009), Datta Gupta and Simonsen (2007), Gormley and Gayer (2005), and Havnes and Mogstad (2009). The findings are mixed[4] and most studies focus on short-run effects[5]. Apart from Schneeweis (2009) we are not aware of any other paper focusing on immigrant students.

As described in the introduction, the methodological challenge we face is to disentangle the effects of childcare from other factors which affect children's cognitive ability and at the same time are related to childcare atten-

[3] The terms 'pre-school intervention' and 'childcare' are here used synonymously. Note that childcare staff in Sweden often have pedagogical training.

[4] Studies focusing on cognitive outcomes tend to find positive short-run effects, but the results in Magnuson et al. (2007) suggest that these may dissipate in the medium run. Studies focusing on short-run non-cognitive outcomes suggest that the effects may be negative, at least as indicated by parents.

[5] Havnes and Mogstad (2009) is a recent exception. They find substantial positive effects of childcare attendance on long-run education attainment. See Jonsson (2004) for a study of the effects of pre-school interventions on educational attainment using Swedish data.

dance. Our empirical strategy is to control for as many such 'confounding variables' as possible. We then use sensitivity analysis to evaluate the credibility of this method.

Our main approach for examining whether our empirical strategy is credible is to vary the set of control variables. If the estimates of interest do not vary with the set of controls we view them as being robust. This sensitivity analysis leads us to conclude that we cannot credibly estimate the average effect of childcare attendance. However, the effect on the immigrant/native gap in language skills seems to be credibly identified.

Our results suggest that childcare attendance narrows the gap in Swedish language ability between children with an immigrant background and children with a native background. Our estimates imply that a year of childcare experience reduces the gap between immigrants and natives by 10 percent. We find no differential effects on inductive skills, however. Nor do we find any effects on the distribution of long-run educational attainment.

In contrast to the majority of US states, pre-school interventions in Sweden are not targeted at the disadvantaged. Rather childcare is universally available; during the time period we study it was in fact targeted at the employed. Disadvantaged (particularly immigrant) children are less likely to participate in pre-school interventions. Our findings suggest that increased participation in childcare among immigrant children will close some of the gap between natives and immigrants in Swedish language skills.

Moral hazard in the Swedish social insurance system

The third and forth essay deal with moral hazard within the Swedish social insurance system.

Social insurance is the common term used for compulsory insurance programs which are handled by the government and which provide economic assistance to e.g. the unemployed, sick, or disabled. The purpose of social insurance, as of all insurance, is to compensate for unexpected adverse events. Through spreading the risks across a large number of individuals, insurance is a much more efficient way of creating security than the alternatives of saving up for personal buffers or relying on family or friends. That is, through insurance we are able to attain the same level of security at a much lower cost.

There are some principal problems that an insurance program has to handle, however, in order to keep the costs down. These problems stem from an asymmetry in information: individuals have more information about their own actions than has the provider of the insurance. Since an insured person no longer has to bear the full consequences of his or her actions, he or she may be tempted to act less carefully than he/she would if not covered by insurance. The provider of the insurance cannot observe such behavior. This phenomenon which may result in excess use of insurance programs is called *moral hazard*.

The insurance literature generally distinguishes between *moral hazard ex ante* and *ex post*. *Moral hazard ex ante* refers to when the behavior of an insured person is influenced by the insurance before an adverse event takes place. For instance, if covered by sickness insurance a worker may take less effort to prevent illness, e.g. by eating less healthy or exercising less, than he/she would if fully exposed to the risk. *Moral hazard ex post*, which is generally the main concern in social insurance programs, instead refers to the behavior after an adverse event. An example concerning sickness insurance is if generous sickness benefits induce a worker to report sick more often. Monitoring, sanctions, and limited rather than full compensation levels are examples of ways to reduce problems with moral hazard.

Moral hazard arising in the interplay between the unemployment and sickness insurance

Moral hazard is a common problem with insurance; its extent has been explored in numerous studies.[6] While almost all previous studies consider one insurance program at a time, the third essay in this thesis (co-written with Laura Hartman) studies moral hazard that arises in the interplay between two large social insurance programs – the Swedish sickness (SI) and unemployment insurance (UI).

Just as employed persons, the unemployed in Sweden are entitled to SI benefits in case of illness. The rational behind this rule is the view that job search is comparable to work. In order to be eligible for UI benefits, an unemployed person should actively search for jobs and be able to accept a job offer at short notice. Unemployed persons who lose their work (search) capacity due to illness should therefore receive benefits from the SI rather than the UI. Moral hazard can arise in this context since benefits from the SI sometimes are considerably higher than benefits from the UI. By reporting sick, an unemployed person can also postpone the UI expiration date (see Larsson 2006). The specific question addressed in this paper is whether a difference in benefit generosity between the two insurance programs affects the probability that an unemployed person reports sick.

The methodological challenge is here to separate the effects of the economic incentives from the effects of other factors which also affect an individual's sickness absence. Whether a person would benefit economically from receiving SI benefits, rather than UI benefits, is related to his or her pre-unemployment wage. However wages are associated with many factors which are likely to affect claims of SI benefits, for instance education and

[6] See e.g. Krueger and Meyer (2002) for a survey of the international literature on social insurance programs. Swedish studies include Johansson and Palme (1996, 2002 and 2005), Henreksen and Persson (2004), Carling et al (2001), and Bennmarker et al (2007).

health. We are unlikely to be able to observe and take all such factors into account.

To identify the effects of economic incentives we therefore exploit a reform of the SI. The reform took place in 2003 and its purpose was to eliminate the difference in benefit generosity between the two insurance programs. The reform implied that some unemployed persons – those whose pre-unemployment wage exceeded a certain cut-off – had their SI benefits reduced. This means that we can study how the sickness absence changes before and after the reform for those affected by the policy change, compared to those not affected. If the sickness absence declines for those who had their benefits reduced but not for the others, this would indicate responsiveness to economic incentives.

The results show that, due to the harmonization of benefits, the sick report rate declined by as much as 36 percent among the unemployed affected by the policy change. Hence, the probability that an unemployed person reports sick seems to be heavily influenced by the relative compensation size in the two insurance systems. The results thereby give evidence of moral hazard arising in the interplay between the UI and the SI.

This finding is interesting for several reasons. First, it indicates that monitoring is lax as it allows the choice of benefits to be determined to a large extent by economic incentives rather than health status alone. This further suggests that monitoring in these insurances programs in general may be quite lax, which could imply a rather widespread misuse. Second, the result illustrates the importance of taking the whole social insurance system into account when designing its different parts, in order to avoid undesired incentive effects in terms of flows between programs.

Does this type of moral hazard matter for the job finding rate among the unemployed?

The previous essay is not alone in drawing attention to undesired incentive effects arising from the interplay between different social insurance programs; see Fortin and Lanoie (1992), Larsson (2006), Henningsen (2008), and Karlström et al (2008) for more examples. Many countries have complex social insurance systems and their various programs sometimes overlap in ways that generate unintended flows between them. Several academics have suggested that this may be an overlooked and financially costly phenomenon (see e.g. Krueger and Meyer 2002, Pellizzari 2006, and the European Economic Advisory Group 2007). The costs will, however, to a large extent depend on whether or not this type of benefit shifting tends to extend the time individuals spend out of work. Little research has been done on this issue.

The forth essay explores whether the interplay between the Swedish UI and SI affects the length of time unemployed persons spend out of employment. In the previous essay we showed that the sick report rate among the unemployed is sensitive to differences in benefit generosity between the two programs. However, does it really matter for the transition rate to employment whether the unemployed – given their health status – receive benefits from the UI or the SI?

There are in fact several reasons for why the source of funding may matter for the incentives to find work. Receiving UI benefits is associated with a number of rules, the purpose of which is to increase transitions to employment: the worker is obliged to apply for and accept jobs, otherwise a sanction may be imposed; benefits are reduced after 100 days; and there is a limit on how many days benefits can be received. SI benefits, on the other hand, are not associated with any similar requirements and have in principle unlimited duration.[7] Hence, if the UI rules work as intended, funding from the UI rather than the SI could be expected to be associated with higher search effort.

It is difficult to determine whether the benefit type affects the amount of time it takes to find employment without extremely comprehensive data, in particular on individual health. But even with access to very detailed data it would be difficult to say with certainty that any observed difference in job finding rates between the unemployed receiving UI and SI benefits was solely due to the rules of the programs, and not to that the two groups of unemployed were different in ways we were unable to observe.

To study whether it matters for the job finding rate if the unemployed claim SI rather than UI benefits, I therefore again use the reform in 2003 which changed the relative compensation size between the two programs. The question of interest this time is if the reduced sick report rate due to the reform also translated into a higher rate of job finding. The method used is thereby similar to the previous essay: I study how the job finding rate changes before and after the reform for the group that was affected by the policy change – and as a result reduced its sick report rate – compared to the group that was not affected. If the job finding rate increases for the group that due to the reform increasingly claimed UI rather than SI benefits, but not for the others, this suggests that transitions to SI among the unemployed prolong the time out of employment.

I find no evidence suggesting that the reduced sick report rate translated into faster transitions to employment; for the group that reduced its sick report rate due to the reform, spending more time receiving benefits from the UI rather than the SI did not seem to shorten the time out of employment. Hence, the results give no evidence that the type of moral hazard which arises in the interplay between the UI and the SI prolongs the time out of work for unemployed persons.

This finding indicates that the financial costs of interactions among these insurance programs are perhaps not as large as one could have expected.

[7] These were the rules in place during the time period for which I have data in this paper.

However, we should probably not, based upon this, conclude that the interplay between them is without economic significance, and that it would not matter if there perhaps was excess use of the SI among the unemployed. Making sure that the insurance programs are used in the ways intended is likely to be important per se. If the citizens have the perception that the benefits are misused this could undermine the legitimacy of the social insurance system.

Moreover, the fact that I do not find any effect of the reform on the job finding rate can have different explanations, which would have different policy implications. First, the reason could be that, for those affected by the reform, search effort did not differ depending on receiving benefits from the UI or the SI. This would then indicate that monitoring in at least one of these insurance programs is lax. If these persons did not search actively in *either* program, this would suggest lax monitoring in the UI, as active search is a formal requirement for UI benefits. If they in fact searched actively in *both* programs, this would instead indicate lax monitoring in the SI, as the SI is intended for those who have lost their work (search) capacity due to illness.

Second, it is possible that spending more time receiving UI benefits in fact *did* increase search effort, but that more active search still did not result in faster transitions to employment for this particular group. If this is the case, it could indicate that those who changed their sickness absence behavior due to the reform were not very attractive on the labor market.

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Essay 1: The effects of making upper secondary school more comprehensive on dropout rates, educational attainment and earnings: Evidence from a Swedish pilot scheme[♦]

1 Introduction

A much debated issue within education policy is whether, and at what age, students should be separated into academic and more vocationally oriented educational tracks. Across OECD countries, there is great diversity regarding the age at which this type of tracking[1] is done. In some countries, e.g. Austria and Germany, pupils are tracked into different types of schools already at the age of 10, while in some others, e.g. the US and Spain, the school system remains comprehensive all through secondary school. Likewise, countries differ in terms of the number of educational tracks available and the extent of curricular differences among the available tracks.[2] Since the Second World War, there has been a tendency in many OECD countries towards adopting more comprehensive school systems (see e.g. Leschinsky and Mayer, 1990). The effects of such policy changes have generally been difficult to evaluate as the reforms have often coincided with other major changes of the education systems[3] or been implemented at the same time across a

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[1] The meaning of the term 'tracking' differs between Europe and the US. While in Europe tracking generally refers to the streaming of students into academic and vocational educational tracks, in the US it rather signifies ability grouping within schools. In this paper, I use the term to refer to the former type of selection.

[2] See e.g. OECD (2004, p. 262) for a comparison of structural features of school systems across the OECD countries.

[3] For instance, in the comprehensive school reforms in the Scandinavian countries in the 1940-1970s, postponing division of students into different educational tracks coincided with increases in the number of compulsory years of education, and the introduction of new national curricula. See e.g. Meghir and Palme (2005), Aakvik et al. (2003) and Pekkarinen et al. (2009) for studies of these reforms in Sweden, Norway and Finland, respectively.

whole nation. Such implementation schemes naturally make it difficult to separate the effects of reduced tracking *per se* from those of other concurrent changes or cohort effects. The consequences of these reforms therefore remain disputed.

This paper evaluates the effects of introducing a more comprehensive upper secondary school system in Sweden by making use of a pilot scheme, which preceded a major educational reform. In 1991, the Swedish Parliament decided on a reform which substantially reduced the differences in curricula between the academic and vocational tracks in upper secondary school. This was done by considerably increasing the academic content of all vocational tracks. The length of these tracks was at the same time extended from two to three years, giving them the same length as the academic tracks. As a result of these changes, students graduating from a vocational track attained basic eligibility for university studies. The reform was preceded by a six-year pilot-period in which the new vocational tracks were tried out in some municipalities. As the pilot scheme did not coincide with any other changes of the upper secondary school system, it can be used to identify the effects of introducing more academic vocational tracks on students' later educational and labor market outcomes.

In theory there are compelling arguments both in favor of and against this type of shift towards a more comprehensive school system. Opponents of the 1991 reform argue that not all students may benefit from an upper secondary education with a substantial academic content. To make everyone pursue such a track may be a waste of resources, since it retains into academic education individuals who may lack the ability to benefit from it or who have already made up their mind to pursue a non-academic career (Brunello and Checchi, 2007). This could potentially even cause some students to drop out of upper secondary school and consequently have a negative impact on their educational attainment.

Proponents of the reform instead argue that it is advantageous that students no longer have to choose a definitive educational direction, which may restrict them in the future, at an early age. Educational systems with early tracking are exposed to the risk of students ending up in the wrong track as it may be difficult to anticipate future educational performance at an early point in the educational career (see e.g. Brunello et al., 2004). Also, the willingness to proceed up the educational ladder may not yet be well formed. The consequences of choosing the wrong track will be mitigated when all tracks enable students to continue to higher education. This argument is frequently brought up with regard to students whose parents have low education, as they are thought to often end up in vocational tracks for reasons unrelated to their ability.[4],[5]

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[4] A country's choice of tracking policy is likely to also affect students' performance through peer effects; see e.g. Hanushek and Wössman (2006) for a discussion. Moreover, a selective

There are some previous studies which also investigate the effects of adopting a more comprehensive school system using within-country variation in tracking regimes. The abolishment of tracking in British secondary schools during the 1960s and 1970s is a particularly well-studied case; see e.g. Galindo-Rueda and Vignoles (2005) and Kerkhoff et al. (1996). As this reform took place only gradually, it produces a set-up with regional variation in students' exposure to the comprehensive system. However, in a recent paper Manning and Pischke (2006) show that the regions that chose to implement the reform early were systematically different from the late adopters. They argue that studies exploiting this variation are unlikely to remove the selection bias between students attending different types of schools.

Other researchers have tried to estimate the effects of different tracking regimes by utilizing cross-country variation in these policies; see e.g. Hanushek and Wössman (2006). Such studies face the difficulty of accounting for all unobserved country-specific factors which are correlated with the choice of tracking regime, and which also affect students' performance. Failure to do this will lead to biased estimates. Waldinger (2007) finds that the results of this literature are very sensitive to model specification as well as to which countries are included in the analysis.

The pilot scheme studied in this paper was implemented gradually over a 3-year period. Moreover, different regions came to participate to different extents as not all vocational tracks were converted to 3-year tracks within all participating municipalities. This generates a setting with temporal as well as regional variation in students' exposure to the pilot tracks, which I exploit to identify the effect of introducing a more comprehensive upper secondary school system. Unlike the British case described above, the participation decision was not given to the municipalities or schools themselves, but to a central government agency. As the selection of municipalities was based on stipulated criteria and observed characteristics, I am able to address selection bias in a more reliable way compared to previous studies of similar reforms.[6]

The results show that the introduction of more comprehensive upper secondary schooling – through prolonging and adding more academic content to the vocational tracks – increased the drop out rate. The probability of dropping out of upper secondary school is estimated to have increased by 3.8 percentage points as a consequence of attending a 3-year rather than a 2-year

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school system is generally favored by the view that it is easier to teach homogenous classes. The 1991 reform did not necessarily alter the composition of students in academic versus vocational tracks. Thus, we should not expect any major effects of the reform operating through these channels.

[5] See e.g. Brunello and Checchi (2007) for a more extensive discussion of the relative merits of a comprehensive versus a selective school system.

[6] The effects of this pilot scheme have previously been studied by Ekström (2003). However, she uses data for only a single cross section of students and thus cannot control for unobserved differences across municipalities. Moreover, only individuals who have graduated from upper secondary school are included in her analysis.

vocational track. This increase is entirely driven by students who finished compulsory school with a below-average grade point average (GPA). Although an important motive behind the policy change was to enable all upper secondary school graduates to pursue a university degree, the results give no indication that the extra year of schooling increased the transition rate to university studies. There is some weak evidence, however, that the extra year of education may have led to increased earnings in the long run.

The outline of the rest of the paper is as follows. The next section describes the 1991 reform of the Swedish upper secondary school as well as the pilot scheme preceding the reform. Section 3 presents the empirical strategy and the data. The results are reported in Section 4. Finally, Section 5 discusses the results and concludes.

2 The 1991 reform of upper secondary school and the pilot scheme

All individuals who have completed nine years of compulsory schooling are entitled to upper secondary education. Schooling at the upper secondary level is voluntary although the vast majority of students choose to attend. Among those who finished compulsory schooling in 1988, almost 90 percent continued directly to upper secondary education (Palme 1992, p. 207). Upper secondary school comprises several different educational tracks to which individuals apply based on their grades from compulsory school. Students generally attend a school in their municipality of residence, but if the desired track is not offered they can instead choose to attend it in a nearby municipality.

Individuals who are older than 20 when they begin upper secondary education are not entitled to attend a general upper secondary school, but instead enter the adult education system. Within this system, both those who lack any upper secondary education and those who dropped out before graduating can finalize a degree. It is also possible to supplement e.g. a 2-year upper secondary degree in order to obtain a 3-year degree.

In 1991 the Swedish Parliament decided on a major reform of upper secondary education. The reform can be categorized as a step from a selective towards a more comprehensive upper secondary school system.[7] Before the reform, upper secondary education consisted of a few academic and several vocational tracks. The vocational tracks were two years long and consisted mainly of vocational training. The academic tracks typically lasted three years and prepared the students for higher education.[8]

[7] The reform is thoroughly described in e.g. National Agency for Education (2000).
[8] There were also a large number of short, more specialized vocational courses.

The general aim of the 1991 reform was to bring about a higher quality of education as well as to increase the flexibility of the upper secondary school system. The largest changes concerned the vocational tracks, which through the inclusion of several general theoretical subjects in the curriculum received a considerably higher academic content. The length of the vocational tracks was also extended from two to three years, hence giving them the same length as the academic tracks. These changes were motivated by the view that there was an increasing need for a broader education in working life as well as by the desire to enable everyone to continue to university studies. As a result of the reform, all students graduating from a vocational track attained basic eligibility[9] for university studies.[10]

2.1 The pilot scheme with prolonged vocational tracks

Concerns about the need to modernize the vocational upper secondary education had been raised all through the 1980s (see e.g. Prop. 1983/84:116). In 1984 the government appointed a committee with the task of reviewing the vocational education and putting forward suggestions for improvements. The proposals led to a nation-wide pilot period between 1988 and 1993 in which new 3-year vocational tracks were tried out in some municipalities.[11]

The 3-year vocational tracks in the pilot scheme had increased academic content compared to the ordinary 2-year tracks. While Swedish was the only general theoretical subject included in all 2-year tracks, the pilot tracks also contained English, social studies and an elective course. Math appears to be by far the most common choice of elective.[12] As a result of these additions, students graduating from the pilot tracks attained basic eligibility for university studies. Another difference between the 2- and 3-year tracks is that the latter located a larger share of the vocational training in workplaces rather than in schools.[13]

[9] 'Basic eligibility' does not mean eligibility to all university studies as some programs have special requirements.
[10] The reform contained more elements than those described here. There were, for instance, several organizational changes in the upper secondary school system as well as changes in the structure of the different educational tracks.
[11] See e.g. Prop. 1987/88:102 for a description of the pilot scheme.
[12] The National Board of Education (1990a) reports that 86 percent of the students in 1988 chose to study math.
[13] Compared to the pilot tracks, the 3-year vocational tracks that were implemented after the 1991 reform contained even more academic subjects and somewhat less training in workplaces.

2.1.1 The implementation of the pilot scheme[14]

The pilot scheme contained about 6,000 available places in 1988, 10,000 in 1989, and 11,200 in 1990. This represented approximately 11-20 percent of the total number of available places in vocational tracks (National Board of Education 1989a, 1989b, and 1990b). A class in the pilot scheme would always replace a class in a corresponding 2-year track, implying that the total number of available places in vocational tracks was not expanded. On top of this, in 1987 there was a very limited pre-pilot scheme only including 500 places. The tracks in the pre-pilot scheme differed somewhat from those in the pilot scheme as they did not contain more extensive workplace training.

The National Board of Education was responsible for allocating the pilot scheme among the different vocational tracks, as well as for deciding in which municipalities it should be located each year. The allocation of places among the different tracks was done primarily on the basis of proportionality; the goal was that each track should receive the same share of available 3-year places as they received of 2-year places. There were however some deviations from this principle, e.g. tracks with a small number of places were somewhat overrepresented. The allocation decision was further restricted by the fact that in the beginning of the pilot period no curricula had yet been prepared for some of the 3-year tracks. This meant that out of the 18 3-year tracks available, the pilot scheme could include only 10 in 1988 and 17 in 1989. In 1990 all 18 tracks were included. Table 1 shows which tracks were included each year as well as each track's number of available and share of vacant places.[15]

[14] This section is mainly based on SOU 1989:106, which describes the implementation process in 1988 and 1989. The implementation in 1990 has not been documented, but was most likely carried out according to the same principles. Regarding the pre-pilot scheme in 1987, there is no available documentation of the implementation process.

[15] The share of vacant places was in general somewhat lower for the 3-year vocational tracks than for the 2-year tracks. For the 2-year tracks the share of unfilled places amounted to 0.07 in 1987, 0.08 in 1988, 1989 and 1990.

Table 1: Number of available places and share of vacant places by pilot track and year

	1987		1988		1989		1990	
	No. of places	Share vacant	No. of places	Share vacant	No. of places	Share vacant	No. of places	Share vacant
Electrical engineering	48	0.00	528	0.02	656	0.03	776	0.02
Health care	46	0.02	2 182	0.03	2 918	0.03	3 072	0.10
Heating, ventilation and sanitation	64	0.11	64	0.00	72	0.00	104	0.00
Industry	352	0.01	1 608	0.09	1 952	0.13	1 968	0.12
Business and services			210	0.01	660	0.03	990	0.05
Caring services: children, youth			256	0.01	420	0.01	420	0.08
Construction			296	0.08	408	0.02	432	0.01
Textile and clothing manufacturing			136	0.11	208	0.22	224	0.17
Transport and vehicle engineering			752	0.04	992	0.03	1 056	0.02
Use of natural resources			352	0.12	640	0.09	720	0.04
Constructional metalwork					56	0.14	56	0.05
Food manufacturing					224	0.08	256	0.11
Handicraft					32	0.03	64	0.05
Painting					56	0.04	88	0.05
Process technology					176	0.17	208	0.23
Restaurant					336	0.00	416	0.00
Wood technology					144	0.10	168	0.09
Graphic							112	0.00
Total	510	0.03	6 384	0.05	9 950	0.06	11 130	0.07

Notes: Share of vacant places by September 15th each year. Source: National Board of Education (1988), (1989a), (1989b), and (1990b).

Regarding the geographical location of the pilot scheme, the Government stipulated that it should be distributed among regions with different industry and population structures. There should also be variation regarding the extent to which different regions participated. In some participating regions, all or a large share of the vocational tracks should be converted to 3-year tracks, while in other regions only a few of the tracks should be prolonged. The motive behind these requirements was to get an idea of how the more extensive work place training worked in different types of labor markets, as well as of the strain on the labor market if it was implemented on a large scale. On top of these criteria, the National Board of Education emphasized whether or not the local labor market was prepared to arrange the extended workplace training. In judging this they relied upon recommendations from employer and union representatives in different sectors.

The initiative to participate always came from the municipalities themselves, as they had to apply to the county school board in order to be considered. The board then made recommendations based on which municipalities they believed had the best prerequisites to participate. With the help of these suggestions, the National Board of Education made the final selection according to the criteria listed above. The same procedure was repeated each year, with the exception that schools that had participated in the pilot scheme one year always were included the following years. The interest to participate was large; each year there were applications for far more places than what was available.

Sweden had 284 municipalities during this time period. Only about 68 percent of them offered vocational tracks. Students residing in the other municipalities hence had to attend school in a nearby municipality if they wanted to obtain a vocational degree. Figure 1 illustrates the share of (all) municipalities that participated in the pilot scheme each year, as well as how the extent of their participation varied over time. The pre-pilot scheme in 1987 only involved 22 municipalities, all of which participated to a quite small extent. When the actual pilot scheme was introduced in 1988, about 40 percent of the municipalities were granted participation. In 1990, this share had increased to about 52 percent. The extent to which the municipalities participated also increased each year. Thus, throughout the pilot period the pilot scheme was extended both to new municipalities as well as often to more tracks within already participating municipalities.

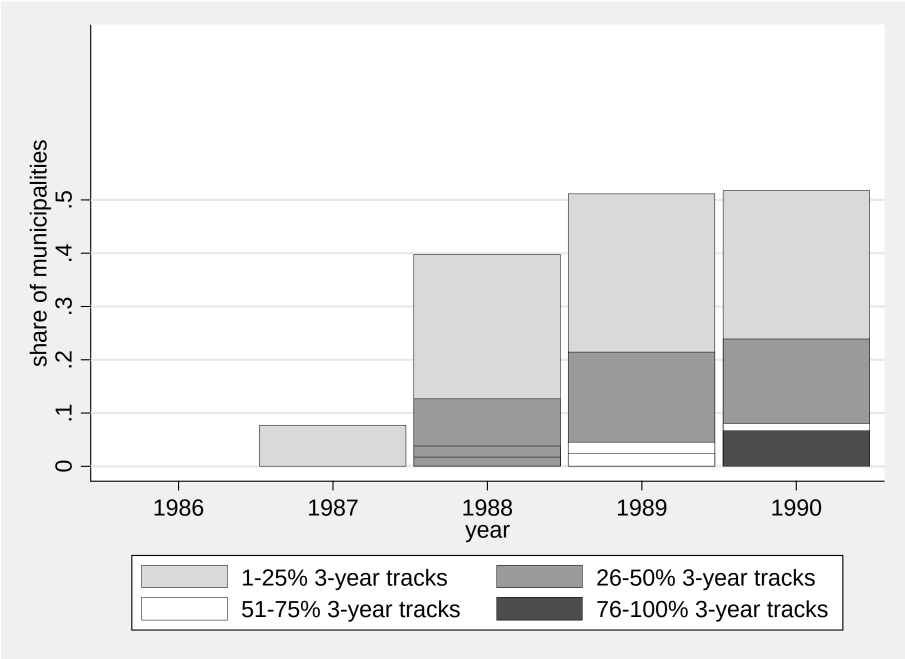

Figure 1: Share of municipalities that participated in the pilot scheme each year, and the extent of their participation

Notes: '% 3-year tracks' is the percent of all vocational tracks available in a municipality which were part of the pilot scheme. Source: Own calculations based on the Upper secondary school application record.

All through the pilot period most participating municipalities came to offer both 2- and 3-year vocational tracks. Sometimes the exact same track was offered in both lengths within the same municipality. Even in municipalities only offering either 2- or 3-year tracks, students could in some cases have a choice of program length if a nearby municipality offered tracks of a different length. The design of the pilot scheme thus generates a setting where some students were given *the choice* of attending a 3-year rather than an ordinary 2-year vocational track. The degree to which a student had this choice depended on where he or she lived as well as on which year he or she began upper secondary school.

3 Estimating the impact of the prolongation of vocational upper secondary education

3.1 Data

The data used come from different administrative records maintained by Statistics Sweden.[16] The records cover the entire Swedish population during the period 1985-2006. One of the most important registers for this study is the *Upper secondary school application record*, which contains information on when and where an individual began upper secondary school as well as what track (type and length) he or she started. I use this record to construct the sample of individuals, but also to acquire information on which educational tracks each municipality offered each year.

The population of interest consists of all persons who finished compulsory school during 1986-1990 and who thereafter continued directly to upper secondary school. Only individuals who began a vocational track – a regular 2-year track or a 3-year pilot track – are included. An additional restriction imposed is that only pilot tracks which corresponded to regular 2-year tracks are included, and vice versa.[17] The population consists of 202,072 individuals. However, almost 9 percent are excluded due to missing information on some of the variables[18], giving me a final sample of 184,101 persons.

These data are matched with information on the individuals' later educational attainment and earnings, as well as with some background variables. More specifically, I consider the following outcomes: whether the person has dropped out of upper secondary school; whether his/her highest education level is at least three years of upper secondary education; whether he/she has begun university studies; whether he/she has completed a university degree; and the natural logarithm of annual earnings.

To determine whether an individual has dropped out of upper secondary school, I use data from the *Upper secondary school graduation record*. A person is considered to have dropped out if he or she still has not graduated six years after being admitted. As mentioned before, it is possible for those who have dropped out to later supplement their education within the adult education system. Such complementary courses are not included in the graduation record, implying that these individuals are still considered dropouts.

To determine whether a person has obtained at least three years of upper secondary education, I use data collected by Statistics Sweden on the indi-

[16] Table A1 presents all variables and which registers they originate from.
[17] This restriction excludes students in the 3-year Graphic and Handicraft tracks since they do not correspond to any of the 2-year tracks. Table A2 lists the included 2- and 3-year tracks.
[18] The majority of these individuals are excluded due to missing information on either municipality of residence or grades from compulsory school.

vidual's highest education level. This measure includes courses obtained within the adult education system. Data on initiated and completed university studies are obtained from the *University enrolment* and *graduation records*. These three educational outcomes are measured 15 years after the person began upper secondary school. Most students begin upper secondary education at age 16, implying that these outcomes are generally measured at age 31.

The earnings measure used is defined as the annual sum of the individuals' gross wage earnings (in SEK). I consider earnings for 16 years after the person started upper secondary school.

The individual background variables include: sex; immigrant background[19]; municipality of residence the year before applying to upper secondary school; and GPA the last year of compulsory school. The students have also been linked to their biological parents in order to obtain information on the parents' highest education level and their immigrant background.

Table 2 presents descriptive statistics for the individual background variables. It also shows the local unemployment rate during the relevant time period. The individuals in the sample are separated into two groups based on the degree to which their municipality of residence participated in the pilot scheme. Municipalities with an above-average share of 3-year tracks in 1990 are considered 'high level' regions, while those with a below-average share are referred to as 'low level' regions.[20] The average share of 3-year tracks in 1990 was 0.18. We see from the table that the two groups are very similar in terms of all observed individual level characteristics. The local unemployment rate is in general somewhat higher in the municipalities that participated to a high degree in the pilot scheme. It is also important to note the dramatic increase in the unemployment rate in the beginning of the 1990s. This was a very turbulent period on the Swedish labor market. Descriptive statistics for the individual outcome variables are presented in the next section.

[19] A person is considered to have immigrant background if he or she is born in a Non-Nordic country.

[20] The 'low level' group includes the municipalities that did not participate in the pilot scheme at all.

Table 1: Sample characteristics

	Level of pilot scheme (1990) in municipality of residence♦	
	Low	High
Individual characteristics:		
Average GPA compulsory school	2.88	2.86
Prop. of females	0.41	0.40
Prop. of immigrants (=born in non-Nordic country)	0.02	0.02
Prop. whose parents are both immigrants	0.02	0.03
Prop. whose parents have upper secondary education	0.55	0.55
Prop. whose parents have post upper secondary education	0.16	0.17
Municipality characteristics:		
Average unemployment rate♣		
1988	3.6%	3.9%
1989	3.0%	3.3%
1990	3.0%	3.3%
1991	5.0%	5.4%
1992	8.7%	9.1%
1993	12.9%	13.3%
Number of individuals	97,671	86,430

Notes: 'High level of pilot scheme' municipalities are defined as municipalities where the share of 3-year tracks was above the average (=0.18) in 1990. The sample consists of all individuals who finished compulsory school 1986-1990 and the same year began a vocational track in upper secondary school. ♦Municipality of residence is measured the year before the individual started upper secondary school. ♣The unemployment rate is measured at the county level and includes participants in labor market programs.

3.2 Identification strategy

The purpose of this paper is to evaluate the effects of the prolongation of vocational upper secondary education on individuals' educational attainment and earnings. The following econometric model characterizes the effect of attending a 3-year rather than a 2-year vocational track on an individual's educational or labor market outcome:

$$Y_{ijk} = \gamma_j + \mu_k + \beta D_{ijk} + \mathbf{\delta X}_i + \varepsilon_{ijk} \qquad (1)$$

where Y_{ijk} is the outcome of interest for individual i, beginning upper secondary school in year k, and residing in municipality j. γ_j and μ_k denote municipality-of-residence and cohort fixed effects, respectively, and $\mathbf{X}_i$ is a vector of individual characteristics. D_{ijk} is a dummy variable, where D_{ijk} =1 if the individual chose to attend a 3-year vocational track, and D_{ijk} =0 if he or she attended a 2-year track. β is thus the effect of attending the longer and more academically oriented vocational track.

Estimating model (1) with Ordinary Least Squares (OLS) may lead to biased estimates as ε_{ijk} and D_{ijk} are likely to be correlated. Even if we have a rich data set of individual characteristics, we are unlikely to be able to control for all factors which are correlated with the individual's choice of track length and which also affect his or her later educational or labor market outcomes.

The pilot scheme however provides us with a potential source of exogenous variation in track length, which can be exploited in order to estimate the causal effect of attending a 3-year vocational track. As we have seen, the pilot scheme gave some students the choice of attending a 3-year rather than a regular 2-year vocational track. The extent to which a person had this choice depended on which year he or she finished compulsory school – as the pilot scheme was introduced gradually over time – as well as on where he or she lived – as the degree to which municipalities participated in the pilot scheme greatly varied. After controlling for cohort and municipality of residence (during compulsory school), individuals' exposure to the pilot scheme is potentially exogenous to the unobserved component of the outcomes of interest and can consequently be used as an instrument for the length of the chosen track (D_{ijk}).[21]

I will estimate the model using Two Stage Least Squares (2SLS). The first stage can be written as:

$$D_{ijk} = \gamma_j + \mu_k + \alpha P_{jk} + \boldsymbol{\lambda}\mathbf{X}_i + v_{ijk} \qquad (2)$$

The instrument, denoted by P_{jk}, is the extent to which the individual's municipality of residence participated in the pilot scheme by the time he or she began upper secondary school, measured as the share of the available vocational tracks which constituted 3-year tracks.[22],[23] I measure municipality of residence during the fall semester of the individual's last year of compulsory school. This means that, unlike municipality of upper secondary school attendance, it is likely to be exogenous with respect to the location of the pilot scheme. In general it seems implausible that students would move already during compulsory school as a consequence of the introduction of 3-year vocational tracks, especially as it was already possible to apply to upper secondary schools in municipalities other than ones own. This assertion is also supported by the fact that the decision of where to locate the new available

[21] Similar identification strategies have been used in earlier studies; see e.g. Duflo (2001).

[22] P_{jk} is zero for municipalities not offering any vocational tracks.

[23] Ideally, P_{jk} would be measured as the share of the available *places* in vocational tracks which represented 3-year tracks. However, such data are not available at the municipality level.

pilot places each year was not taken until during the following spring, i.e. after the point in time when I measure municipality of residence.[24]

P_{jk} is a valid instrument for the length of the vocational track under the assumption that it is not correlated with any unobserved variables affecting the outcomes of interest, and that it had no impact on the outcomes other than through influencing whether the person attended a 2-year or a 3-year track. This entails assuming that the availability of pilot tracks did not affect the individual's choice of whether to begin a vocational track at all. If the pilot tracks attracted students from the academic tracks or students who would otherwise have chosen not to attend upper secondary school, the individual's untreated state is not clear and the results will consequently be hard to interpret. This part of the identifying assumption is tested below (see Section 3.2.2). Of course, for the method to work P_{jk} must also have explanatory power in the first stage.

If the effects of obtaining additional education vary across individuals, the estimated coefficient for D_{ijk} (in equation 1) should be interpreted as the average effect of attending a 3-year vocational track for those who, due to the availability of pilot tracks in their municipality of residence, chose to begin a 3-year rather than a 2-year track (see Imbens and Angrist, 1994). For this to be the correct interpretation, increased availability of pilot tracks in a municipality must never have decreased participation in the 3-year tracks among those living in that municipality.

The vector of individual characteristics ($\mathbf{X}_i$) in the model includes GPA for the last year of compulsory school, sex, immigrant background, the parents' highest education level and whether both of the parents have immigrant background.[25] An additional factor which is potentially important to account for is the local unemployment rate when the individual finished upper secondary school. As was shown in Table 2, the unemployment rate rose steeply in the beginning of the 1990s. This means that students following a 3-year track systematically graduated under worse labor market conditions than those attending a regular 2-year track. Not accounting for this implies that the estimated effect of attending a 3-year track will include the effect of graduating in a worse labor market situation. I will return to this issue in Section 4, where I present the results.

[24] At least this was the case with the localization decision in 1988 and 1989, which were the years that involved the largest increases of available places in the pilot scheme. Details are found in SOU 1989:106. The decision process in 1987 and 1990 has not been documented.

[25] I have also estimated a model that includes controls for what type of track (five categories) a person attended. The estimated effect of attending a 3-year track is virtually identical for this model. These results are not reported but can be obtained from the author.

3.2.1 Descriptive analysis

Table 3(a) illustrates the basic idea behind the identification strategy. It shows means of the outcome variables for the cohort that started upper secondary school the year before the pilot scheme was introduced (in 1986) as well as for those who started when it was fully implemented (in 1990). The individuals are additionally separated into ‘high’ and ‘low level of pilot scheme’ regions, based on where they lived the year before they started upper secondary school. As in the previous section, municipalities with an above average share of 3-year tracks in 1990 are considered ‘high level’ regions, and those with a below-average share are referred to as ‘low level’ regions. The table shows the difference in average outcomes between the two cohorts, in both types of regions.[26]

Let us start by looking at the share of individuals whose highest education level is at least three years of upper secondary education. This share will include everyone who has completed some type of post-secondary education. At least partly, this explains why about 27 percent of the vocational students already in the 1986 cohort – when all vocational tracks were 2-year tracks – obtained this level of education. Some of them could also have attended supplementary courses within the upper secondary school system after completing their 2-year vocational degree or have changed from a 2-year vocational to a 3-year academic track during their course of study. Regarding this outcome, there is no visible difference between the two types of regions for the cohort starting upper secondary school in 1986. From 1986 to 1990, this share more than doubled in both high and low level regions. However, it increased significantly more in the high level regions. Under the assumption that general trends in educational attainment would not have been systematically different between the two types of regions in the absence of the pilot scheme, this difference-in-difference estimate can be interpreted as a causal effect of the pilot scheme.

For the 1986 cohort, there is no significant difference between the high and low level regions regarding any of the other outcomes either. We see that the share of students dropping out decreases significantly over time in the regions that participated in the pilot scheme to a low extent, while there is no significant change in the high level regions. This indicates that the prolongation of the vocational tracks may have increased the dropout rate from upper secondary school. Regarding the share of students obtaining a university degree and log earnings, the changes over time are not significantly different between the two types of regions.

[26] This analysis is inspired by Duflo (2001).

Table 3: Descriptive analysis: Means of outcome variables by cohort and level of pilot scheme

	Share with at least 3 years of USE			Share dropping out[♣]		
	Level of pilot scheme (1990) in municipality of residence			Level of pilot scheme (1990) in municipality of residence		
	High	Low	Difference	High	Low	Difference
(a) *Experiment of interest: Vocational students*						
Starting USE (Upper secondary educ.) in 1990	0.651	0.588	0.063***	0.119	0.105	0.014*
	(0.014)	(0.007)	(0.015)	(0.007)	(0.004)	(0.008)
Starting USE in 1986	0.267	0.266	0.000	0.125	0.126	-0.002
	(0.007)	(0.005)	(0.008)	(0.008)	(0.004)	(0.009)
Difference	0.384***	0.321***	0.063***	-0.006	-0.022***	0.016**
	(0.012)	(0.009)	(0.015)	(0.006)	(0.004)	(0.007)
(b) *Control experiment: Academic students*						
Starting USE in 1990	0.947	0.945	0.002	0.055	0.054	0.001
	(0.002)	(0.002)	(0.003)	(0.004)	(0.002)	(0.004)
Starting USE in 1986	0.870	0.869	0.001	0.076	0.079	-0.003
	(0.005)	(0.005)	(0.007)	(0.006)	(0.005)	(0.007)
Difference	0.077***	0.076***	0.001	-0.020***	-0.025***	0.004
	(0.005)	(0.004)	(0.006)	(0.003)	(0.005)	(0.006)

Table 3, cont.	Share with university degree			Log annual earnings♦		
	Level of pilot scheme (1990) in municipality of residence			Level of pilot scheme (1990) in municipality of residence		
	High	Low	Difference	High	Low	Difference
(a) *Experiment of interest: Vocational students*						
Starting USE in 1990	0.097	0.097	-0.000	12.026	12.026	-0.000
	(0.004)	(0.003)	(0.005)	(0.011)	(0.009)	(0.014)
Starting USE in 1986	0.065	0.068	-0.003	11.936	11.926	0.010
	(0.003)	(0.003)	(0.005)	(0.011)	(0.009)	(0.014)
Difference	0.032***	0.029***	0.003	0.090***	0.100***	-0.010
	(0.004)	(0.003)	(0.005)	(0.014)	(0.011)	(0.018)
(b) *Control experiment: Academic students*						
Starting USE in 1990	0.419	0.405	0.014	12.144	12.163	-0.019
	(0.013)	(0.006)	(0.014)	(0.009)	(0.008)	(0.012)
Starting USE in 1986	0.362	0.342	0.020	12.099	12.113	-0.014
	(0.014)	(0.008)	(0.016)	(0.012)	(0.013)	(0.018)
Difference	0.057***	0.063***	-0.006	0.045***	0.049***	-0.005
	(0.007)	(0.006)	(0.009)	(0.012)	(0.011)	(0.016)

Notes: 'High level of pilot scheme' municipalities are defined as municipalities where the share of 3-year tracks was above the average (=0.18) in 1990. Robust standard errors in parentheses, allowing for clustering by municipality of residence. */**/*** denotes significance at the 10/5/1 percent levels respectively. ♣Dropping out is here defined as to not complete USE with grades in all subjects. ♦Earnings are here measured 16 years after the person began upper secondary school. All individuals with positive wage earnings are included.

3.2.2 Evaluating the identifying assumptions

The assumption that, in the absence of the pilot scheme, trends in educational attainment and earnings would not have differed systematically between high and low level of pilot scheme municipalities is of course impossible to test directly. However, it is possible to examine an implication of this assumption as I also have data for students attending academic tracks in upper secondary school. The length of the academic tracks was not altered during the pilot period. Hence, there should not be any systematic differences in the changes of the outcome variables between the two types of regions for these students.

Table 3(b) presents means of the outcome variables in the same fashion as 3(a), but for students attending academic tracks. We see that for none of the outcome variables are there any significant differences between the two types of regions. This is the case for both cohorts. This suggests that the statistically significant difference-in-difference estimates for the dropout rate and share completing at least three years of upper secondary education for the vocational students, are not results of unsuitable identifying assumptions.

As discussed earlier, it is possible that the introduction of 3-year vocational tracks affected the individual's choice of whether to obtain an academic or a vocational upper secondary degree. If this is the case, the individuals' untreated state is not clear, which would make the results hard to interpret. The same problem would arise if the pilot scheme affected individuals' decisions of whether or not to begin upper secondary school.

Table 4 shows the share of students in upper secondary school that began vocational as well as academic tracks before and after the full implementation of the pilot scheme, i.e. in 1986 and 1990.[27] The shares are calculated separately for municipalities that participated in the pilot scheme to different extents. We see that the share of students starting a vocational track decreases slightly over time, while the share starting an academic degree increases. There are, however, no significant differences in these patterns between municipalities that participated in the pilot scheme to a higher or a lower degree.

[27] Upper secondary school also comprised a number of short more specialized vocational courses. These are not included in the table.

Table 4: Share of students in vocational and academic tracks by cohort and level of pilot scheme

	Share starting vocational tracks			Share starting academic tracks		
	Level of pilot scheme (1990) in municipality of residence			Level of pilot scheme (1990) in municipality of residence		
	High	Low	Difference	High	Low	Difference
Starting USE (Upper secondary educ.) in 1990	0.411	0.427	-0.015	0.546	0.536	0.010
	(0.015)	(0.008)	(0.017)	(0.014)	(0.009)	(0.016)
Starting USE in 1986	0.430	0.442	-0.012	0.517	0.513	0.004
	(0.018)	(0.009)	(0.020)	(0.016)	(0.010)	(0.018)
Difference	-0.019**	-0.015***	-0.004	0.029***	0.023***	0.006
	(0.008)	(0.005)	(0.009)	(0.007)	(0.005)	(0.008)

Notes: 'High level of pilot scheme' municipalities are defined as municipalities where the share of 3-year tracks was above the average (=0.18) in 1990. Robust standard errors in parentheses allowing for clustering by municipality of residence. */**/*** denotes significance at the 10/5/1 percent levels respectively.

Table 5 shows the share of students finishing compulsory school who start upper secondary school the same year. Again, the individuals are separated based on their municipality of residence's level of participation in the pilot scheme. Since the first year for which there are data on students finishing compulsory school is 1988, I here compare changes between 1988 and 1990. The number of available places in the pilot scheme in 1990 was almost double that of 1988. Thus, any divergent trends in upper secondary school participation arising as a consequence of the pilot scheme are likely to be visible also by comparing these years. We see from the table that there are no significant changes in the share starting upper secondary school in any of the two groups of municipalities.

To sum up, according to the checks performed in this section the identifying assumptions imposed seem appropriate.

Table 5: Share of students finishing compulsory school who continue directly to upper secondary school, by cohort and level of pilot scheme

	Share starting upper secondary school		
	Level of pilot scheme (1990) in municipality of residence		
	High	Low	Difference
Starting upper secondary educ. in 1990	0.853	0.856	-0.003
	(0.006)	(0.004)	(0.007)
Starting upper secondary educ. in 1988	0.856	0.857	-0.001
	(0.005)	(0.004)	(0.006)
Difference	-0.002	-0.001	-0.001
	(0.003)	(0.004)	(0.005)

Note: 'High level of pilot scheme' municipalities are defined as municipalities where the share of 3-year tracks was above the average (=0.18) in 1990. Robust standard errors in parentheses allowing for clustering by municipality of residence. */**/*** denotes significance at the 10/5/1 percent levels respectively.

4 Results

4.1 The effect of the pilot scheme on the choice of track length

Before presenting the results from the instrumental variables estimation, I report estimates for the first stage relationship, showing that the intensity of the pilot scheme in an individual's municipality of residence is a good predictor for his or her choice of track length. This section also gives some additional evidence indicating that the pilot scheme is a valid instrument.

The first two columns of Table 6 present the results for the first stage relationship. Column (1) reports estimates from a regression containing only the instrument, cohort fixed effects and municipality-of-residence fixed effects. In column (2) all individual characteristics are included. Comparing column (1) and (2), we see that the coefficient for the instrument is very robust to the inclusion of additional control variables. The estimate reveals that the pilot scheme intensity in a person's home municipality has a clear impact on the probability that he or she begins a 3-year, rather than a 2-year, track. The coefficient is statistically significant at the 1 percent level and suggests that increasing the share of 3-year tracks by, for instance, 50 percentage points in a person's home municipality on average increases the probability that he or she begins a 3-year track by nearly 30 percentage points. The fact that there is not a one to one correspondence between the share of 3-year tracks and the probability that a person begins such a track is likely to be mainly explained by the possibility for students to attend schools outside their municipality of residence. The last row of Table 6 reports the F-statistic for the null hypothesis that the coefficient for the instrument is zero. The F-statistic is about 184, which indicates that a weak instrument is not a concern.[28]

Some of the other variables also have a significant impact on the probability of beginning a 3-year track. As can be expected, students with a high GPA from compulsory school and students with highly educated parents are more likely to choose the longer and more academically oriented vocational track. Gender and immigrant background have no significant impact on the choice of track length, while having immigrant parents is associated with a significantly lower probability of choosing the 3-year track.

It is also important to mention that the location of the pilot places seems to have been exogenous to the individuals. The fact that the addition of individual controls does not affect the estimate for the instrument suggests that any excluded individual variables are unlikely to be correlated with the pilot scheme intensity. Moreover, the selection of pilot municipalities appears to have been based largely on characteristics that are likely to have remained relatively constant throughout the relevant time period (see Section 2.1.1), e.g. industry and population structure. The relevant municipality characteristics should therefore be captured by the municipality fixed effects. However, as an additional test for endogeneity, I also estimate the first stage regression for the sample of students attending academic tracks.[29] Since the academic tracks were not part of the pilot scheme, there should not be any effect of the pilot scheme intensity on the choice of track length for these students. A

[28] Staiger and Stock (1997) suggest that an F-statistic less than 10 indicates weak instruments.

[29] As one of the academic tracks lasted four years, this regression estimates the effects of the pilot scheme intensity on the probability of choosing a 3- or a 4-year, rather than a 2-year, track.

significant 'effect' would instead suggest the presence of unobserved variables, which are correlated with the pilot scheme intensity.[30] The results from this test are reported in column (3) and (4) in Table 6. The test gives no sign of an endogenous relationship as the estimate for the pilot scheme is statistically insignificant.

Table 6: Effects on the probability of beginning a 3-year track

	Vocational students		Academic students♦	
	(1)	(2)	(3)	(4)
Pilot scheme intensity in municipality of residence	0.577*** (0.050)	0.577*** (0.050)	-0.015 (0.016)	-0.019 (0.015)
Cohort fixed effects	Yes	Yes	Yes	Yes
Municipality-of-residence fixed effects	Yes	Yes	Yes	Yes
GPA compulsory school		0.026*** (0.005)		0.298*** (0.011)
Female		-0.005 (0.008)		-0.079*** (0.003)
Immigrant background		0.011 (0.008)		-0.002 (0.005)
Parents with immigrant background		-0.008* (0.005)		0.045*** (0.005)
Parent with upper secondary education		0.007*** (0.001)		0.030*** (0.002)
Parent with post-upper secondary education		0.026*** (0.002)		0.038*** (0.003)
Sample size	184,101	184,101	224,337	224,337
F-statistic on the instrument	131.32	131.61	0.81	1.67

Notes: Robust standard errors in parentheses allowing for clustering by municipality of residence. */**/*** denote significance at the 10/5/1 percent levels respectively. ♦As one of the academic tracks lasted four years, these regressions estimate effects on the probability of beginning a 3-year, or longer, track.

In sum, the results presented in this section show that the pilot scheme intensity had a significant and substantial effect on the probability of beginning a 3-year vocational track, and that it can be used as an instrument for the students' choice of track length. Let us therefore move on to the effects of attending the prolonged vocational tracks.

[30] If these unobserved variables are also related to the unobserved variables in the outcome equation, the instrument would most likely be invalid.

4.2 The effect of attending a 3-year track on the probability of dropping out

I start by presenting the estimated effect of beginning a 3-year, rather than a 2-year, vocational track on the probability of dropping out of upper secondary school for the full sample of vocational students. I use two different measures of dropping out: to have still not graduated six years after admittance, and to have not graduated with complete grades before this point in time. A person will graduate with incomplete grades if he or she has not attended school enough to obtain grades in all subjects, meaning that he/she is likely to have dropped out of some, but not all, of the classes.

Table 7 shows 2SLS as well as OLS estimates. In both cases, the model is estimated both with and without the individual covariates.[31] The OLS estimates for the effect of attending a 3-year track would be biased if e.g. those choosing to attend a 3-year track would differ in motivation from those attending a 2-year track. However, when the individual covariates are included in the model, the OLS estimates turn out to be quite similar in size to the 2SLS estimates.

The preferred specification (column 4) suggests that choosing to attend a 3-year rather than 2-year vocational track increased the probability of dropping out by 3.8 percentage points, and the probability of not finishing with grades in all subjects by as much as 7.5 percentage points. The estimates are statistically significant at the 5 and 1 percent levels, respectively. Hence, the prolongation of the vocational tracks seems to have caused a large increase in the dropout rate from upper secondary school. It is important to note, however, that if students dropped out during the third year, they still received more education compared to if they would have chosen to attend a 2-year track. Unfortunately, there are no data on when a person dropped out. Thus, I am not able to estimate the effect on the amount of upper secondary education received.

It is tempting to jump to the conclusion that the increased dropout rate was caused by the increased length and/or increased academic content of the program. Another possibility is that individuals actually dropped out because they found employment. As described in Section 2.1, the 3-year tracks contained more training in workplaces compared to the ordinary 2-year tracks, which naturally would imply more contacts with potential employers. In order to get some idea of whether this explanation seems likely, I have estimated the effect of starting a 3-year track on annual wage earnings during the first two years following admittance, for the sub sample who did not graduate (using the preferred 2SLS specification). This analysis gives no indication that attending a 3-year track implied increased earnings for these

[31] Results for the control variables are available upon request.

students during the time period preceding expected graduation.[32] Note however that this analysis is very tentative as there is no way of knowing when a person dropped out. Limiting the sample based on an outcome variable may also introduce some sample-selection issues that could bias the results.

Table 7: Effects on the probability of dropping out of upper secondary school (full sample)

	Dependent variable: Dropping out of upper secondary school			
	OLS	OLS	2SLS	2SLS
Effect of attending a 3-year vocational track	0.030*** (0.005)	0.044*** (0.004)	0.041** (0.019)	0.038** (0.019)
Cohort fixed effects	Yes	Yes	Yes	Yes
Municipality-of-residence fixed effects	Yes	Yes	Yes	Yes
All individual covariates	No	Yes	No	Yes
Mean of dependent variable	0.109	0.109	0.109	0.109
	Dependent variable: Not finishing with complete grades			
Effect of attending a 3-year vocational track	0.070*** (0.006)	0.086*** (0.005)	0.079*** (0.021)	0.075*** (0.021)
Cohort fixed effects	Yes	Yes	Yes	Yes
Municipality-of-residence fixed effects	Yes	Yes	Yes	Yes
All individual covariates	No	Yes	No	Yes
Mean of dependent variable	0.126	0.126	0.126	0.126
Sample size	184,101	184,101	184,101	184,101

Notes: Robust standard errors in parentheses allowing for clustering by municipality of residence. */**/*** denotes significance at the 10/5/1 percent levels respectively. The following covariates are included in column (2) and (4): final GPA in compulsory school, sex, immigrant background, the parents' highest education level and their immigrant background.

Table 8 reports separate estimates of the effect on the probability of dropping out, by final GPA from compulsory school and levels of parental education (using the preferred model specification). The results indicate that the large increase in the probability of dropping out is entirely driven by a higher dropout rate among the low performing students. A GPA lower than 3, on the scale 1-5, is here considered 'low'.[33] The estimated effect is large in

[32] The effect on both the log of earnings and the probability of having positive earnings is insignificant for these years.

[33] During this time period, Sweden used relative grades on the scale 1-5 (with 5 being the highest grade). The scale was supposed to follow a normal distribution, with a mean of 3, on the national level.

magnitude for this group; the point estimates suggest that the probability of dropping increased by 8.3 percentage points and the probability of not obtaining grades in all subjects by as much as 13.6 percentage points. For students with higher previous grades, attending the longer track does not seem to have affected the likelihood of dropping out. If the model is instead estimated separately for students with academic versus non-academic parents, the results exhibit a similar pattern. 'Academic parents' is here defined as to have at least one parent with more than two years of upper secondary education. Thus, it seems to be mainly among students who, for study purposes, were relatively less advantaged that the probability of dropping out increased substantially as a consequence of the prolongation of the vocational tracks.

Table 8: Effects on the probability of dropping out for different sub samples

	Dependent variable: Dropping out of upper secondary school			
	High GPA	Low GPA	Academic parents	Non-academic parents
Effect of attending a 3-year vocational track	-0.017 (0.023)	0.083*** (0.023)	0.006 (0.026)	0.052** (0.021)
Mean of dep. variable	0.049	0.158	0.086	0.118
	Dependent variable: Not finishing with complete grades			
Effect of attending a 3-year vocational track	0.001 (0.024)	0.136*** (0.026)	0.037 (0.029)	0.092*** (0.023)
Mean of dep. variable	0.055	0.184	0.102	0.137
Sample size	82,558	101,543	53,697	130,404
Method	2SLS	2SLS	2SLS	2SLS

Notes: Robust standard errors in parentheses allowing for clustering by municipality of residence. */**/*** denotes significance at the 10/5/1 percent levels respectively. All models include cohort fixed effects, municipality-of-residence fixed effects, and controls for final GPA in compulsory school, sex, immigrant background, the parents' highest education level and their immigrant background. 'High GPA' refers to students with at least grade 3 (on the scale 1-5) in compulsory school. 'Academic parents' means that at least one parent has a long degree from upper secondary education or a higher degree.

4.3 The effect of attending a 3-year track on educational attainment

Table 9 presents the estimated effects of attending the longer and more academic vocational tracks on students' long-term educational attainment. Again, I start by showing the results for the full sample of vocational students. The top part of the table shows the effect on the probability that the individual's highest education level obtained is at least three years of upper secondary education. Note that the OLS estimates are very similar to the

2SLS estimates for this outcome. The results for the preferred specification (column 4) suggest that beginning a 3-year track increased the likelihood of obtaining at least 3 years of upper secondary education by about 40 percentage points. This estimate is significant at the 1 percent level.

One important motive behind the decision to prolong the vocational tracks was to enable all upper secondary school graduates to continue to university studies. The bottom part of Table 9 shows the estimated effect of attending a 3-year track on the probability of beginning as well as completing a university degree. The OLS estimates for these effects will be biased upwards if students choosing the longer track in general are more motivated to pursue higher education than those choosing the shorter option. The results confirm this: while the OLS estimates show a significant and positive effect on both outcomes, the 2SLS estimates suggest that there is no effect of attending the 3-year track; neither on the probability of starting nor completing a university degree. The results are very similar if I limit the sample to only include students who actually graduated from upper secondary school.[34]

As was discussed earlier (see Section 3.2), students who completed a 3-year track systematically graduated during worse labor market conditions than those completing a 2-year track. A higher unemployment rate is likely to increase the transition rate to higher education, which implies that the effect of attending a 3-year track may be overestimated in these regressions. However, we may still conclude that the results give no indication that beginning a 3-year track would increase the probability of enrolling in, or graduating from, university studies. Hence, the prolongation of the vocational tracks seems to have increased educational attainment through increasing the amount of upper secondary schooling received, but the additional year of schooling does not seem to have caused more students to pursue a university degree.

[34] Results are available upon request.

Table 9: Effects on educational attainment (full sample)

	Dependent variable: At least 3 years of upper secondary education			
	OLS	OLS	2SLS	2SLS
Effect of attending a 3-year vocational track	0.411*** (0.006)	0.388*** (0.005)	0.403*** (0.026)	0.403*** (0.023)
Cohort fixed effects	Yes	Yes	Yes	Yes
Municipality-of-residence fixed effects	Yes	Yes	Yes	Yes
All individual covariates	No	Yes	No	Yes
Mean of dependent variable	0.435	0.435	0.435	0.435
Sample size	181,445	181,445	181,445	181,445
	Dependent variable: University enrolment			
Effect of attending a 3-year vocational track	0.056*** (0.009)	0.033*** (0.005)	-0.005 (0.018)	-0.006 (0.016)
Cohort fixed effects	Yes	Yes	Yes	Yes
Municipality-of-residence fixed effects	Yes	Yes	Yes	Yes
All individual covariates	No	Yes	No	Yes
Mean of dependent variable	0.184	0.184	0.184	0.184
Sample size	184,101	184,101	184,101	184,101
	Dependent variable: University degree			
Effect of attending a 3-year vocational track	0.039*** (0.007)	0.026*** (0.004)	0.003 (0.011)	0.004 (0.010)
Cohort fixed effects	Yes	Yes	Yes	Yes
Municipality-of-residence fixed effects	Yes	Yes	Yes	Yes
All individual covariates	No	Yes	No	Yes
Mean of dependent variable	0.083	0.083	0.083	0.083
Sample size	184,101	184,101	184,101	184,101

Notes: Robust standard errors in parentheses allowing for clustering by municipality of residence. */**/*** denotes significance at the 10/5/1 percent levels respectively. The following covariates are included in column (2) and (4): final GPA in compulsory school, sex, immigrant background, the parents' highest education level and their immigrant background.

Table 10 shows results from separate estimations by compulsory school GPA and parents' education. We see that the effect on the probability of completing at least three years of upper secondary education is significant and substantial for all four groups. The point estimate is somewhat higher for low performing than for high performing students, and is higher for students with non-academic parents than for those with academic parents. This pattern seems reasonable since high performing students and students with academic parents are more likely to obtain this level of education even without access to the longer vocational tracks. For none of the groups are there any significant effects on the probability of beginning or completing a university degree.

Table 10: Effects on educational attainment for different sub samples

	Dependent variable: At least 3 years of upper secondary education			
	High GPA	Low GPA	Academic parents	Non-academic parents
Effect of attending a 3-year vocational track	0.356*** (0.033)	0.445*** (0.030)	0.330*** (0.037)	0.432*** (0.026)
Mean of dep. variable	0.544	0.347	0.534	0.395
Sample size	81,318	100,127	52,748	128,697
	Dependent variable: University enrolment			
Effect of attending a 3-year vocational track	-0.010 (0.030)	-0.002 (0.016)	0.027 (0.031)	-0.019 (0.018)
Mean of dep. variable	0.288	0.100	0.273	0.148
Sample size	82,558	101,543	53,697	130,404
	Dependent variable: University degree			
Effect of attending a 3-year vocational track	0.009 (0.022)	-0.002 (0.009)	0.007 (0.024)	0.004 (0.012)
Mean of dep. variable	0.149	0.028	0.127	0.064
Sample size	82,558	101,543	53,697	130,404
Method	2SLS	2SLS	2SLS	2SLS

Notes: Robust standard errors in parentheses allowing for clustering by municipality of residence. */**/*** denotes significance at the 10/5/1 percent levels respectively. All models include cohort fixed effects, municipality-of-residence fixed effects, and controls for final GPA in compulsory school, sex, immigrant background, the parents' highest education level and their immigrant background. 'High GPA' refers to students with at least grade 3.0 (on a scale from 1.0-5.0) in compulsory school. 'Academic parents' means that at least one parent has a long degree from upper secondary education or a higher degree.

4.4 The effect of attending a 3-year track on earnings

Finally, let us turn to the effect of attending the longer vocational tracks on annual wage earnings. As was shown in the previous section, the prolongation of the vocational tracks caused many individuals to acquire an extra year of upper secondary education. This naturally means that they would enter the labor market one year later than those from the same cohort who attended a 2-year track, and would consequently have less work experience. Here I estimate the effect of attending a 3-year track on earnings without controlling for education or experience. The estimated effect will thus depend on, among other things, how more schooling is valued relative to more experience on the Swedish labor market.

The effects on earnings are estimated in separate regressions for different years after the person began upper secondary school, starting with two years after admittance and including the subsequent 14 years. Figure 2 shows 2SLS estimates of the effects on the natural logarithm of annual wage earnings for the full sample (with positive earnings). The parameter estimates (2SLS as well as OLS) are also reported in Table 11, which additionally includes the estimated effect on the probability of having positive earnings.

Figure 2 shows a significant negative effect of attending a 3-year, rather than a 2-year, track on annual earnings the second and third year after admittance. This captures the fact that those completing the 3-year track entered the labor market one year later. Except for the last year – 16 years after admittance to upper secondary school – none of the other 2SLS estimates are significantly different from zero. The earnings estimate for the 16th year is positive and significant at the 10 percent level, suggesting a 6.4 percent increase in annual wage earnings due to attending a longer and more academically oriented vocational track. The estimated return to an additional year of education is thereby somewhat higher than what is found by Meghir and Palme (2005), who studies the effects of the prolongation of the Swedish compulsory school. The estimated return is however in the lower range of estimates in international comparisons.[35],[36]

The figure indicates that the return to the additional year of schooling may increase somewhat over time, which suggests that there potentially could be positive effects on earnings later on in the individuals' labor market career. However, without access to data for more years I cannot examine this hypothesis. One possible explanation to the pattern portrayed in the figure is that the loss of experience matters more than the extra year of education in the beginning of a person's working life, but that the positive effect of more schooling dominates later on. The absence of positive effects for most of the time period could potentially also be driven by lower earnings for those who dropped out prematurely as a consequence of the prolonged education. However, the pattern turns out to be similar if I limit the sample to only include those who actually graduated from upper secondary school, which does not support this explanation.[37]

As in the previous section, these regressions ignore the fact that students completing a 3-year track systematically graduated under worse labor market conditions than those completing a 2-year track. This implies that these individuals faced a higher risk of being unemployed and consequently of having

[35] See e.g. Card (1999) for a survey of empirical studies estimating the returns to schooling.
[36] A recent Dutch study that focuses on vocational students however estimates zero returns to an additional year of (general) education; see Oosterbeek and Webbink (2007).
[37] Results are available upon request.

low wage earnings in the beginning of their labor market career. Not accounting for this means that the estimated effect of attending a 3-year track on earnings could be underestimated. Unemployment after graduation may also have scarring effects on employment and earnings later on in a person's career. Analyzing roughly the same cohorts of students, Nordström Skans (2004) finds that being unemployed the year after graduating from upper secondary school has negative effects on earnings and employment during the subsequent five years. The negative effect however seems to decrease over time and is not significantly different from zero six years after the graduation date. This suggests that the effects of attending a 3-year track presented in this section may be underestimated, but that the bias is likely to decrease over time.

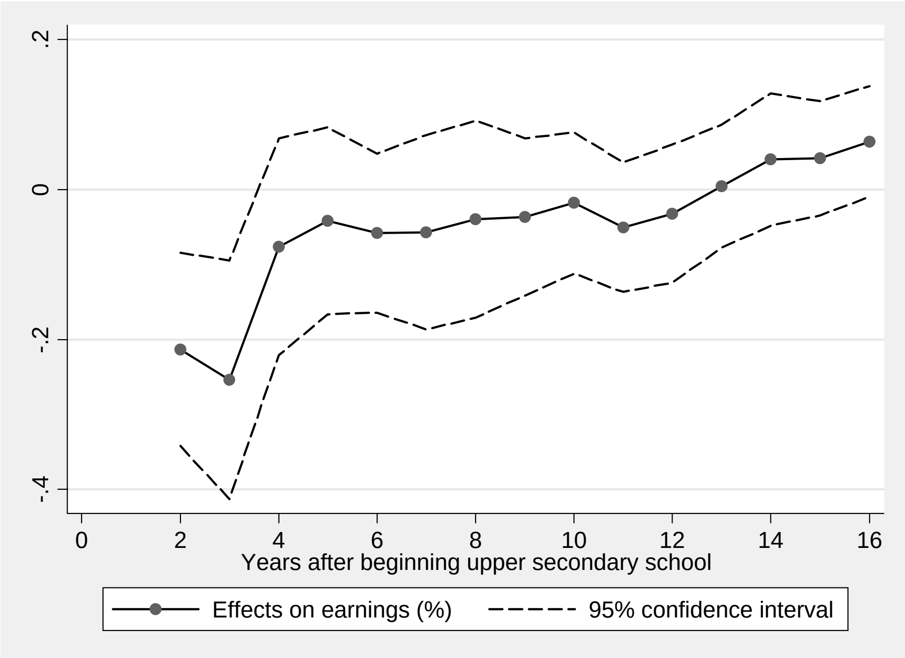

Figure 2: Effects on ln earnings (sample with positive earnings)

Notes: The regressions are estimated with 2SLS. The following covariates are included: cohort and municipality-of-residence fixed effects, compulsory school GPA, sex, immigrant background, the parents' highest education level and their immigrant background. Robust standard errors, clustered by municipality of residence.

Moving on to the rest of the results in Table 11, we see that the 2SLS estimates of the effect on the probability of having positive earnings do not reveal any clear pattern, at least if we recognize that these estimates may be biased downwards during the beginning of the time period (see column 4). The table further indicates that the OLS estimates may be biased for both outcomes as they suggest quite different patterns compared to the 2SLS estimates (see column 1 and 3). If the students who choose to attend the longer tracks in general were more able or had a higher level of career ambition than those attending the 2-year tracks, we would expect an upward bias in the OLS estimates. This is in line with the results for most years.

The earnings effects have also been estimated separately by compulsory school GPA and parental background. The estimates do not show any clear differences between the different sub groups.[38]

Table 11: Effects of attending a 3-year vocational track on annual wage earnings

	Dependent variable:			
	Ln earnings	Ln earnings	Positive earnings	Positive earnings
Years after admittance to upper secondary school:				
2 years after admittance	-0.423***	-0.213***	-0.024***	-0.014
	(0.013)	(0.065)	(0.003)	(0.018)
Sample size	171,669	171,669	184,052	184,052
3 years after admittance	-0.380***	-0.253***	-0.012***	-0.019
	(0.015)	(0.081)	(0.004)	(0.017)
Sample size	170,051	170,051	184,022	184,022
4 years after admittance	-0.013	-0.076	0.009**	-0.014
	(0.014)	(0.073)	(0.004)	(0.022)
Sample size	165,027	165,027	183,995	183,995
5 years after admittance	0.037***	-0.042	0.015***	-0.016
	(0.013)	(0.063)	(0.004)	(0.015)
Sample size	161,555	161,555	183,960	183,960
6 years after admittance	0.055***	-0.058	0.021***	-0.032*
	(0.012)	(0.054)	(0.003)	(0.017)
Sample size	160,301	160,301	183,964	183,964
7 years after admittance	0.059***	-0.057	0.016***	-0.029*
	(0.013)	(0.066)	(0.003)	(0.016)
Sample size	160,370	160,370	183,830	183,830
8 years after admittance	0.058***	-0.040	0.019***	-0.030*
	(0.013)	(0.067)	(0.002)	(0.016)
Sample size	162,295	162,295	183,520	183,520
9 years after admittance	0.061***	-0.036	0.014***	-0.017
	(0.011)	(0.053)	(0.003)	(0.016)
Sample size	163,402	163,402	183,188	183,188

[38] These results are not reported but are available upon request.

Table 11, cont.	Ln earnings	Ln earnings	Positive earnings	Positive earnings
10 years after admittance	0.051***	-0.018	0.016***	-0.005
	(0.009)	(0.048)	(0.002)	(0.013)
Sample size	163,903	163,903	182,790	182,790
11 years after admittance	0.042***	-0.050	0.010***	0.001
	(0.010)	(0.044)	(0.002)	(0.014)
Sample size	164,949	164,949	182,422	182,422
12 years after admittance	0.039***	-0.032	0.011***	0.012
	(0.010)	(0.047)	(0.002)	(0.014)
Sample size	165,731	165,731	182,137	182,137
13 years after admittance	0.038***	0.004	0.009***	-0.001
	(0.009)	(0.042)	(0.002)	(0.013)
Sample size	165,934	165,934	181,888	181,888
14 years after admittance	0.030***	0.040	0.011***	-0.006
	(0.010)	(0.045)	(0.003)	(0.012)
Sample size	165,553	165,553	181,677	181,677
15 years after admittance	0.041***	0.042	0.009***	0.016
	(0.009)	(0.039)	(0.003)	(0.011)
Sample size	164,822	164,822	181,449	181,449
16 years after admittance	0.020**	0.064*	0.011***	0.017
	(0.009)	(0.038)	(0.003)	(0.012)
Sample size	164,207	164,207	181,226	181,226
Method	OLS	2SLS	OLS	2SLS

Notes: Robust standard errors in parentheses allowing for clustering by municipality of residence. */**/*** denotes significance at the 10/5/1 percent levels respectively. All models include cohort fixed effects, municipality-of-residence fixed effects, and controls for final GPA in compulsory school, sex, immigrant background, the parents' highest education level and their immigrant background.

5 Conclusions

The results presented in this paper suggest that the introduction of a more comprehensive upper secondary school system, through prolonging and adding more academic content to the vocational tracks, brought about a higher dropout rate. The probability of dropping out of upper secondary school is estimated to have increased by 3.8 percentage points as a consequence of attending a 3-year, rather than a 2-year, vocational track. This increase is entirely driven by a higher dropout rate among students with below-average grades from compulsory school. These findings are well in line with the results of a few studies investigating the effects of raised graduation standards on high school dropout decisions. For example, Lilliard and DeCicca (2001) find that higher graduation requirements in the US led to increased dropout rates and Dee and Jacob (2006) that the use of exit exams reduced the probability of graduating among disadvantaged groups of students.

Although one important motive behind the decision to prolong the vocational tracks was to enable all upper secondary school graduates to pursue a university degree, the results give no indication that the extra year of schooling increased transitions to university studies. There are some indications, however, that the prolonged education may have led to increased earnings in the long run.

The absence of an effect on university enrolment is at odds with the findings of an earlier study of the same pilot scheme; Ekström (2003) finds that attending a pilot track significantly increased the probability of beginning university studies. The difference between my findings and hers seems to be largely explained by the fact that my regression model includes municipality fixed effects. It may also be interesting to compare my findings to those of Meghir and Palme (2005) who study the Swedish comprehensive school reform in the 1940s. This reform improved access to higher education by increasing compulsory schooling to nine years, and by abolishing the division of students into academic and non-academic schools after grade six. Meghir and Palme find that this reform increased the education level even beyond the new compulsory level. The difference between my results and theirs suggest that the effects of de-tracking a school system may differ for students of different ages. However the content of the two reforms differ in several other aspects, making it hard to rule out other possible explanations.

I have estimated the effects of attending a prolonged and more academic vocational track in upper secondary school by exploiting a pilot scheme which took place in parts of the country. Later on, after 1991, longer and more academic vocational tracks were implemented on a national scale. It is possible that the effects of attending a 3-year track after a nationwide prolongation would differ from the effects of the more limited pilot scheme. The first thing to note in this regard is that since 2- and 3-year vocational tracks coexisted in many regions during the pilot period, the students who attended pilot tracks had at least to some extent chosen to do so. After the reform, no choice of program length existed. If the effects of attending a prolonged track vary across individuals, the average effect for those attending pilot tracks may differ from the average affect for the population of vocational students. It is conceivable that the individuals who took advantage of the opportunity to study an extra year had high expected returns for doing so. This means that the estimated effect on the probability of dropping out is potentially a lower bound of the effect for the whole population of vocational students, while the estimated effect on university attendance and earnings may be an upper bound.

The coexistence of 2- and 3-year vocational tracks may also have implied altered peer groups compared to both the pre-pilot and post-reform periods, when only one program length existed. Thus, if peer group effects are important for the outcomes studied, the effects of attending a 3-year track during the pilot period could differ from the effects of a 3-year track after the nationwide prolongation.

A final point to note regarding the validity of my results for the effects of a national implementation is that a prolongation of all vocational tracks could have general equilibrium effects on the returns to education, in which case the earnings effects may differ from the earnings effects of attending a pilot track.

Many countries have taken steps towards more comprehensive school systems. These types of policy shifts can be accomplished in many different ways, e.g. by delaying the tracking age or reducing the number of tracks. This paper provides evidence on a particular type of transition, where a less selective school system is obtained through making all educational tracks academic enough to prepare the students for university studies. The consequences of this policy change appear not to be straightforwardly positive or negative. The increased probability of dropping out among weak students gives support to the fear held among people opposing this policy shift; that not all students may benefit from an upper secondary education with a substantial academic content. On the other hand, the average educational attainment among vocational students did increase, and there are indications that this may have led to increased earnings in the long run. The absence of an effect on university enrolment suggests that, perhaps already in the old system, the costs of changing direction from a vocational path to pursuing university studies were not large enough to prevent individuals who changed their mind from doing so. After all, even before the pilot period it was possible to supplement a 2-year vocational degree in order to obtain university eligibility within the adult education system.

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Appendix

Table A.1: Variable definitions

Variables	Definitions
Instrument:	
Pilot scheme intensity in municipality of residence	Share of available vocational tracks which constituted 3-year tracks in the person's municipality or residence♦, when he/she began upper secondary school. (The Upper Secondary School Application Record)
Outcome variables:	
Dropped out of upper secondary school	Dummy variable = 1 if the person has not graduated six year after admittance; 0 otherwise. (The Upper Secondary School Graduation Record)
Did not finish upper secondary school with complete grades	Dummy variable = 1 if the person has not graduated six year after admittance, or has graduated but with one or more grades coded as missing; 0 otherwise. (The Upper Secondary School Graduation Record)
At least three years of upper secondary education	Dummy variable = 1 if the person's highest education level is three years of upper secondary education or higher; 0 otherwise. Measured 15 years after admittance to upper secondary school. (LOUISE)
University enrolment	Dummy variable = 1 if the person has enrolled at a university; 0 otherwise. Measured 15 years after admittance to upper secondary school. (The University Enrolment Record)
University degree	Dummy variable = 1 if the person has completed a university degree; 0 otherwise. Measured 15 years after admittance to upper secondary school. (The University Graduation Record)
Ln earnings	The natural logarithm of annual gross wage earnings. (LOUISE)
Individual characteristics:	
Female	Dummy variable = 1 if female; 0 otherwise. (The Multi-Generation Register)
Immigrant background	Dummy variable = 1 if born in non-Nordic country; 0 otherwise. (LOUISE)
GPA compulsory school	GPA the last year of compulsory school. (Cohort 1986-87: the Upper Secondary School Application Record. Cohort 1988-90: the Compulsory School Graduation Record)

Table A.1, cont.

Parental characteristics:	
Immigrants	Dummy variable = 1 if both biological parents are born in non-Nordic countries; 0 otherwise. (LOUISE)
Upper secondary education	Dummy variable = 1 if the parents' highest education is upper secondary education; 0 otherwise. Measured the year the student finished compulsory school. (LOUISE)
Post-secondary education	Dummy variable = 1 if the parents' highest education is post-secondary education; 0 otherwise. Measured the year the person finished compulsory school. (LOUISE)

Notes: Statistics Sweden registers in parenthesis. ♦Municipality of residence is measured on December 31st the person's last year of compulsory school.

Table A.2: Vocational tracks in upper secondary school

Regular 2-year vocational tracks	3-year pilot tracks
Agriculture Forestry Gardening	Use of natural resources
Business & administration Distribution & administration	Business & services
Caring services Social services	Health care
Caring services: children & youth	Caring services: children & youth
Clothing manufacturing	Textile & clothing manufacturing
Construction	Construction Constructional metalwork Heating, ventilation & sanitation Painting
Consumer studies[♦]	
Electrical engineering	Electrical engineering
Food manufacturing	Food manufacturing Restaurant
Operation and maintenance engineering[♦]	
Process technology	Process technology
Vehicle engineering	Transport & vehicle engineering
Wood technology	Wood technology
Workshop techniques	Industry
-	Handicraft[♣]
-	Graphic[♣]

Notes: [♦]Tracks which do not directly correspond to any of the pilot tracks, but are still included in the analysis as important elements of them appear to be present on one or more of the pilot tracks. [♣]Tracks which are not included as they do not correspond to any of the 2-year tracks.

Essay 2: Do pre-school interventions further the integration of immigrants? Evidence from Sweden*

Co-authored with Peter Fredriksson, Elly-Ann Johansson and Per Johansson

1 Introduction

Immigrant students typically perform substantially worse than native students in the OECD countries. According to PISA (Program for International Student Assessment), the performance gap between first generation immigrants and natives amounts to around half a standard deviation in math, reading, and science (OECD 2006a). The achievement gaps between immigrants and natives are particularly large in Middle and Northern Europe (Schneeweis 2009).

The size of the achievement gaps across countries depends on the characteristics of immigrants; in particular, immigrant source countries are likely to be important. But the characteristics of (host-country) educational institutions should also matter. It is intuitively plausible that pre-primary education is one important factor. Indeed, Schneeweis (2009), in her analysis of aggregate cross-country data, found that immigrant/native achievement gaps are lower in countries that make extensive use of pre-primary education.

The main contribution of this paper is that we directly examine whether pre-primary interventions reduce the immigrant/native gap in school performance. We use individual data containing information on childcare attendance, measures of cognitive achievement at age 13, and long-run educational attainment.[1] We thus examine the medium and long-run effects of pre-primary interventions.

* We thank Tuomas Pekkarinen for very helpful suggestions. We also thank seminar participants at the IFAU and Uppsala University for useful comments.

[1] The data come from the so-called UGU-project which is run by the Department of Education at Göteborg University; see Härnqvist (2000) for a description of the data. To these data we have matched educational attainment from the Educational Register (*Utbildningsregistret*) maintained by Statistics Sweden.

We are thus contributing to the recent flurry of papers analyzing the effects of (universal) pre-school interventions; see, e.g., Baker et al. (2008), Berlinski et al. (2009), Datta Gupta and Simonsen (2007), Gormley and Gayer (2005), and Havnes and Mogstad (2009). The literature has examined both cognitive and non-cognitive outcomes. The findings are mixed. Studies focusing on cognitive outcomes tend to find positive short-run effects, but the analysis in Magnuson et al. (2007) suggests that these may dissipate in the medium run. Studies focusing on short-run non-cognitive outcomes suggest that the effects may be negative, at least as indicated by parents; little is known about the longer-run effects on non-cognitive outcomes.[2] Thus, most studies of universal pre-school interventions have focused on short-run effects. Havnes and Mogstad (2009), however, is a recent exception.[3] They find substantial positive effects of pre-school attendance on long-run education attainment. Apart from Schneeweis (2009) we have seen no other paper focusing on immigrants.

Pre-school interventions are likely to reduce inequality in education performance if the alternative to pre-schools (usually the home-environment) is worse for disadvantaged children than for advantaged children. For comparison, we also provide estimates for children with low-educated parents. We thus examine whether any effects are particular to immigrants or whether they apply to disadvantaged groups in general.

Our data cover cohorts born between 1967 and 1982. The time period spanned by these data involve changes in policy which have affected female labor supply and the demand for childcare. The past 40 years have seen a remarkable rise of female labor force participation in Sweden which is intimately tied to an increase in childcare enrolment.[4] The increase in female participation rates and the build-up of pre-schools/childcare were partly the responses to a tax reform in 1971. In 1971, the tax system changed from family taxation to individual taxation. This reform improved the incentives for women – particularly high-skilled women – to enter the labor market.

We are interested in the question of how childcare attendance affects the cognitive achievement gap between immigrants and natives in the medium and the long run. Ideally we would have liked to estimate mean impact of

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[2] Note that this statement pertains to the effects of universal childcare/pre-schools. The studies of the Perry Preschool and Abecedarian programs suggest substantial and favorable longer run effect on non-cognitive (behavioral) outcomes for the particularly disadvantaged groups that participated in these experiments; see Karoly et al. (2005).

[3] See Jonsson (2004) for a study on the effects of pre-schools on educational attainment using Swedish data.

[4] Daycare centers/pre-schools have both caring and school preparatory elements in Sweden. The official terminology changed from daycare to pre-schools in 1998 when a curriculum was introduced. Note that children in daycare/pre-schools have always been "taught" by staff with some pedagogical training. In the sequel we try to adhere to the following terminology. We use "childcare" to refer to both "pre-schools" and "family daycare"; the latter two concepts are defined more closely in the next section.

childcare attendance as well. But the fact that childcare attendance is so intimately tied to female labor force participation makes such an analysis much harder. We will rely on a selection on observables assumption to estimate the effect on the achievement gaps between immigrants and natives. We perform sensitivity analyses to evaluate the credibility of this assumption. Our conclusion from the sensitivity analyses is that the impact of childcare attendance on the achievement gaps seems credibly identified.

We find that childcare attendance reduces the gap in language skills between children from immigrant backgrounds relative to native children. We find no differential effects by mother's education, however. Nor does childcare affect the distribution of inductive skills or long-run educational attainment.

2 Background facts

The purpose of this section is to provide some background facts. We provide these facts along three dimensions: first, we describe childcare, its expansion and the nature of the "treatment"; second, we describe the evolution of female labor supply; and, third, we describe how the composition of children enrolled in childcare has evolved over time.

2.1 Childcare – expansion, content and alternatives

Prior to the late 1960s, childcare was available on a small scale and distinctively targeted at disadvantaged children. The words of a public committee (SOU 1944:20) illustrate the prevailing view. The committee advocated the introduction of pre-schools arguing that "Children from disadvantaged backgrounds should have the possibility to spend time in an activity that furthers their development. [Therefore] pre-schools should be introduced, where children through play (and other activities) enhance social skills, perception, and verbal skills". This policy prescription has been echoed by Heckman (and coauthors) in a series of papers (e.g., Cunha et al. 2006).

A major change in tax policy in 1971 changed the composition of children enrolled in childcare substantially. The policy reform moved income taxation from joint to individual taxation. The tax reform improved the incentives for women (typically the second earners) to enter the labor market, since marginal income tax rates were reduced substantially. In fact, the reform was preceded by the introduction of optional individual taxation in 1966, where couples could move to individual taxation if this minimized total tax payments (Selin 2008). This policy change seems to have spurred the demand for childcare. Pre-school enrolment rates started to increase in the second half of the 1960s; see Figure 1.

Since the late 1960s there has been an impressive increase in pre-school enrolment. In 1970, 4.5 percent of children aged 1–5 were enrolled in pre-schools. By 1985, the share had increased to 32 percent and by 2007 it had increased further to 80 percent.

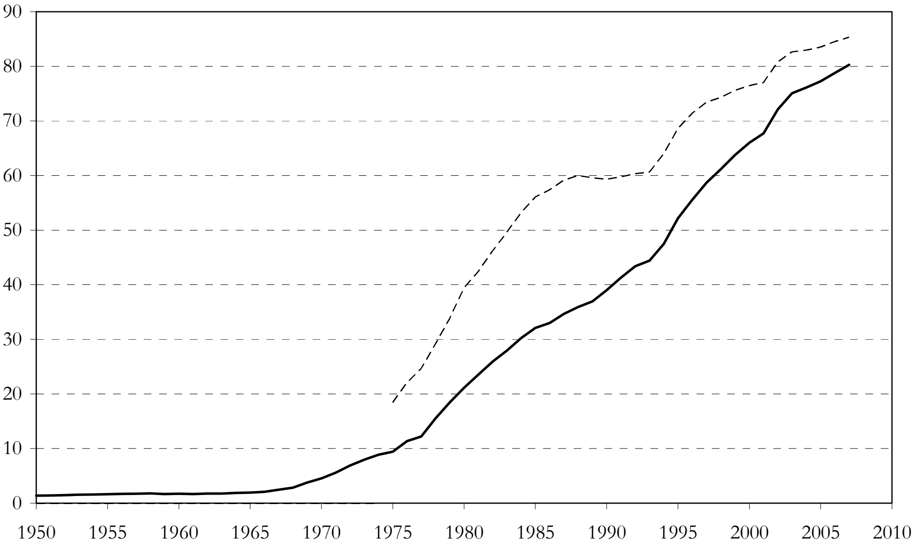

Figure 1: Share of population aged 1–5 enrolled in pre-schools (solid) and pre-schools plus family day-care units (dashed), percent, 1950–2007

Notes: From 1975–2007, pre-school enrolment is reported by age. Before 1975 only total enrolment is available. We have used 1975 data on the share of children above age 5 and below age 1 to adjust the pre-1975 data. Pre-1968, there is only information on the number of slots in pre-schools. We have used the relationship between the number of slots and the number of enrolled children in 1968 to adjust the pre-1968 data.

Sources: Statistics Sweden (*Utbildningsstatistisk Årsbok*, 1978, 1999, 2002, 2009; *Befolkningsförändringar*, 1950–1967, *Befolkningen* 1968–2007).

In terms of the increase in the total number of children involved in childcare activities the solid line is somewhat misleading. Since 1970, so called family daycare units have been available. By the mid 1980s, these daycare units hosted a substantial share of children in the pre-school ages. In 1985, 56 percent were enrolled in some childcare activity (either pre-schools or family daycare units); see the dashed line in Figure 1.

The municipalities provide for both pre-schools and family daycare. Pre-schools are organized facilities, with regular opening hours, while family daycare takes place in private homes. In order to shed light on the nature of treatment, we provide some information on, *inter alia*, resources, staffing and staff qualifications at pre-schools and family daycare.

Relative to the rest of OECD (see OECD 2009a), expenditures on pre-primary education appears to be about average. For example, expenditure per student relative to GDP per capita was slightly below average in 2006 while expenditure per student in PPP converted US Dollars was slightly above average. Looking instead at the number of children per teacher (the student/teacher ratio) this was below the OECD average in 2006: 12.5 students per teacher in Sweden while the OECD average amounted to 14.9.

How has the student/teacher ratio evolved over time? The available data (see Johansson and Åstedt 1996) suggest no major changes over time. The number of students per staff was almost the same in 1970 as in 1994. The most relevant period for our purposes, however, is the period 1970–85. During this period there seems to have been a slight reduction in the student/teacher ratio.

Basically, there are two kinds of employees in Swedish pre-schools: teachers and child minders. Pre-school teachers have tertiary education (currently 3.5 years) while child minders, at the time, had 2 years of upper-secondary education. In 1980, 45 percent of all employees had pre-school teacher training while 46 percent had child minder training (see Johansson and Åstedt 1996). Between 1970 and 1990, there appear to have been no major changes in the relative shares of pre-school teachers and child minders.

The fact that almost half of the staff employed in pre-schools have pedagogical training arguably suggests that pre-school activities have (and have had) pedagogical content. In fact, the first Kindergarten was established in the late 1800s.[5] Another hallmark was the public commission (*Barnstugeutredningen*) established in 1968. The public commission had a distinct developmental psychologist or educationalist approach. The work of the commission eventually led to the law on public pre-schools (*Lag om allmän förskola*) which was implemented in 1975. At that time, pre-school activities were the responsibility of the Ministry of Health and Social Affairs and monitoring was conducted by the National Board of Health and Welfare, which, *inter alia*, issued pedagogical guidelines. In 1996, the responsibility for pre-schools was transferred to the Ministry of Education and in 1998 a national pre-school curriculum was introduced. While the transfer of responsibility to the Ministry of Education may have been an important signal, there is little evidence to suggest that the pedagogical emphasis changed. Relative to the earlier guidelines and pre-school programs issued by the National Board of Health and Welfare, the curriculum emphasized (in general terms) the pedagogical goals, rather than how they should be attained.

In a couple of reports the OECD has compared early childhood education and care across a selection of countries; see OECD (2001, 2006b). The

[5] Richardson (2004) describes the historical evolution of the Swedish schooling system. The remainder of the text draws on this source unless explicitly stated.

OECD (2001) emphasizes that Swedish pre-schools appear to be of high-quality: the fraction of pre-school teachers with tertiary education is high, almost the entire staff is trained to work with children, and child/staff ratios are low. We have not been able to detect any major changes in these quality indicators since 1970. Therefore, we infer that the relative quality of Swedish pre-schools is likely to have been high during the 1970s and 1980s – the time period most relevant to our empirical analysis – as well.

Family daycare units have in common with regular pre-schools that they offer an environment which is different from the home environment. Thus, for example, it is more likely that immigrant children interact with native born individuals in both pre-schools and family daycares than in the home environment.

But family daycare units differ somewhat from pre-schools in other respects. Family daycare units are typically staffed by individuals without (tertiary) pedagogical training; nevertheless, the providers are typically trained to take care of children.[6] Furthermore, by construction, group sizes are smaller in the daycare units than in regular pre-schools.

2.2 The evolution of female labor supply

The tax reform (alluded to above) improved the incentives for women to participate on the labor market. The 1970s saw other changes which may have contributed to increasing female labor supply. In 1974, a parental leave system was introduced. The system involved parental leave compensation which was proportional to individual earnings (prior to child birth) up to a ceiling. The policy created an incentive to enter the labor market prior to giving birth. During the 1970s, there was also a rather dramatic reduction in wage dispersion. Since men were usually the prime wage-earners, wage compression may have induced an increase in labor supply among married women.

All in all, the changes during the 1970s improved the incentives for women to supply labor. Figure 2 illustrates how married women responded to the change in tax policy. In the mid 1960s, 50 percent of married women aged 25–54 participated in the labor force. Following tax policy changes, changes in parental leave policy (foremost the introduction of a parental leave system in 1974) and the build-up of childcare, the participation rate among married women converged to the participation rate among single women by the second half of the 1980s.[7]

[6] OECD (2001) reports that 72 % of family daycare providers have either a child minder certificate or have taken a mandatory child minder course from their municipal employers.

[7] We cannot update Figure 2 since the Labor Force Surveys have stopped reporting labor market status by marital status.

Female participation rates in Sweden are among the highest in the world. In 2008, 87.5 percent of the female population aged 25–54 participated in the labor force. The OECD average at the same point in time was 70.2 percent (OECD, 2009b).[8] Male participation rates, on the other hand, are about average: in 2009, the male participation rate (for males aged 25–54) was 93.1 percent which should be compared to an overall OECD average of 92.2 percent.

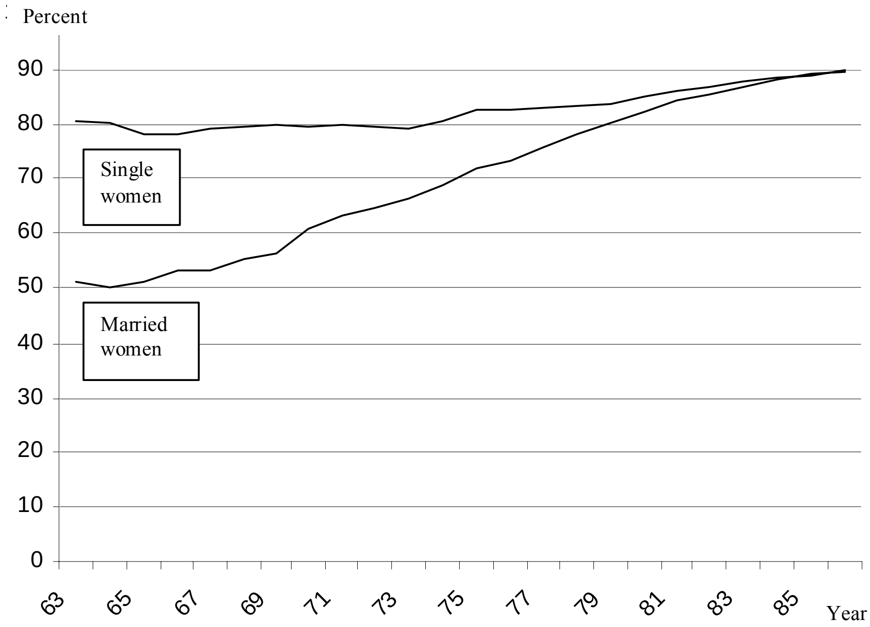

Figure 2: Female labor force participation rates by marital status, 1963–86

Source: Labor Force Surveys, Statistics Sweden.

2.3 Changes in the composition of children in childcare across cohorts

As explained earlier, childcare was originally targeted at the disadvantaged. But following policy changes in the 1970s, they have become part of an overall policy-package designed to increase (and maintain) female labor force participation rates. One would expect that the increase in labor supply has contributed to change the nature of selection of children into childcare, such that children are increasingly drawn from the higher end of the distribu-

[8] This is in line with the cross-country evidence in Jaumotte (2003), which suggests that individual taxation and childcare are two policy tools that contribute to increasing female labor supply. Note also that participation rates among females with small children are higher than among other females, although this to some extent reflects age or cohort effects.

tion of parental background characteristics starting in 1970. Here we analyze this question in greater detail.

To fulfill this objective we have run earnings regressions using data from LINDA (see Edin and Fredriksson 2000) in 1970. We conduct this exercise for a single year because it is more convenient to work with a given set of "skill prices"; note, however, that we obtain very similar results if we use additional years.

The LINDA database includes register information on annual earnings, census information on the education level of the subjects, and standard population characteristics derived from the population registers. We restrict attention to males and females, aged 18–59, who are married and have positive earnings. The earnings regression is specified as follows:

$$\ln w_i = \alpha + \gamma ED_i + \beta_1 age_i + \beta_2 age_i^2 + \varphi IM_i + \kappa_1 \ln w_i^p + \kappa_2 (\ln w_i^p)^2 + \varepsilon_i \qquad (1)$$

where w denotes annual earnings, γED education level fixed effects, IM controls for immigrant status, and w^p denotes the annual earnings of the partner.[9] Apart from the inclusion of w^p, this is a standard earnings regression. We control for the earnings of the partner since we want to free the other coefficient estimates of the variation in labor supply coming from households with different characteristics.

The estimated coefficients on education, age, and immigrant status are used to predict the earnings of the mothers and fathers in the dataset containing an indicator of whether their child has attended childcare (these data come from the UGU project; we describe the data in the next section). We think of predicted earnings as a one-dimensional measure of observed skills and use this single index to illustrate how varying labor supply incentives have affected the selection of children into childcare.

Figure 3 illustrates how the changes in labor supply incentives for mothers have affected the composition of children in childcare. It shows how the probability of participating in childcare varies with the potential earnings of the mother (in 1,000s of 1970 SEK) for successive birth cohorts.

In the cohort born 1967, there is no (or even negative) selection of children. By the cohort born 1972 – i.e., in just five years – this has changed to positive selection with respect to the earnings potential of the mother. The positive selection becomes even clearer for successive cohorts. Figure 3 thus illustrates that the children attending childcare become more selected over the time-period spanned by these cohorts.[10]

[9] Note that all the results are invariant to estimating the earnings regression in levels.
[10] An analysis along these lines is also presented in Jonsson (2004).

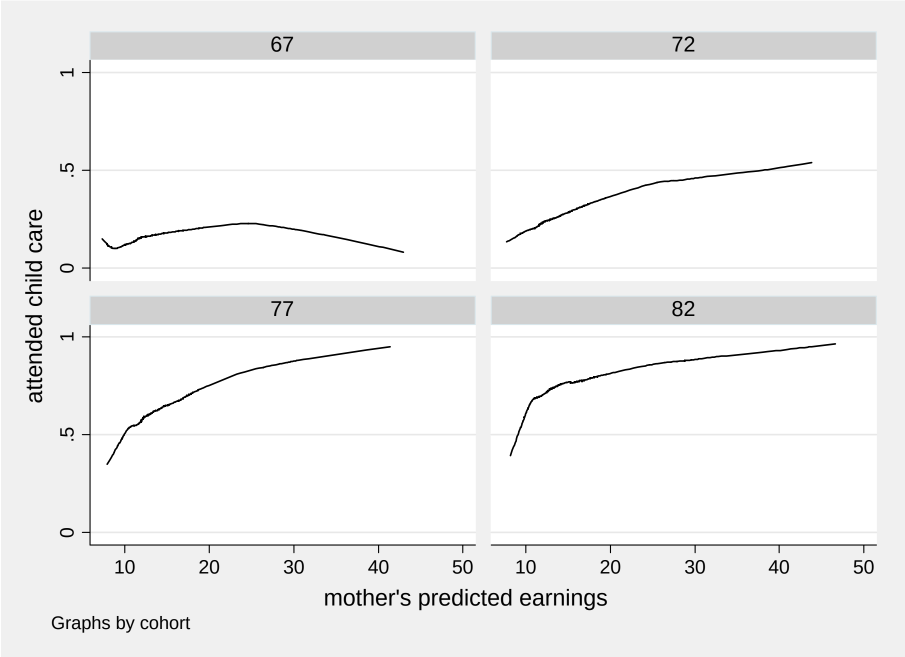

Figure 3: The probability of childcare attendance by mother's skills and cohort

Notes: Predicted earnings in 1,000s of 1970 SEK. Own calculations based on LINDA- and UGU-data as described in the main text. The graph is produced using local linear smoothing.

Figure 4 presents the results of a similar exercise but this time for fathers. The relationship between childcare attendance and the earnings potential of the father is also changing across cohorts. Relative to the mother, the father's earnings potential is not as important in determining childcare attendance of the child. The characteristics of the mother thus appear to be mainly responsible for the changes in the composition of children in childcare that we observe over time.

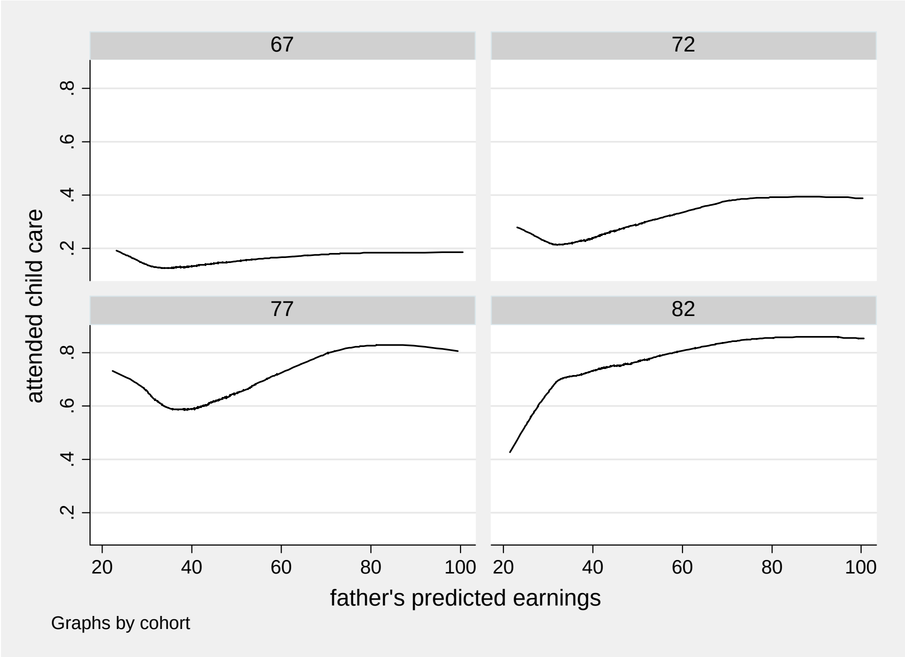

Figure 4: The probability of childcare attendance by father's skills and cohort

Notes: Predicted earnings in 1,000s of 1970 SEK. Own calculations based on LINDA- and UGU-data as described in the main text. The graph is produced using local linear smoothing.

3 Individual data

We use data from the so-called UGU-project maintained by the Department of Education at Göteborg University; see Härnqvist (2000) for a description of the data. The UGU-data have some features which are very useful for our purposes. Importantly, they include the results of cognitive tests conducted at age 13 for roughly 10 percent of the birth cohorts "born" 1967, 1972, 1977, and 1982.[11] Moreover, the data include information on whether the individuals have attended childcare (pre-schools or family daycare) as well as rudimentary information on how many years they have spent in childcare.

To these data we have matched register information on (individual) educational attainment and information on parental age, education, and immigrant status as well as the number of siblings (in addition we of course have information on the gender and age of the child). The link between parents and child come from the multi-generational register (*Flergenerationsregistret*) which links children to their biological parents. The multi-generational

[11] From 1967 and onwards, the children are sampled in the grade which we would normally expect individuals born a certain year to attend. So, for instance, the "1967-cohort" contains individuals in 6th grade in 1980. Some 95 % of these individuals are actually born in 1967.

register also provides the information on the number of (biological) siblings. Individual and parental education comes from the educational register (*Utbildningsregistret*) which records educational attainment in the Swedish population. Basic demographic information originates from the Population register (*Registret för totalbefolkningen*). These register data are of high-quality; it is unlikely that measurement error is an issue.

Table 1 reports descriptive statistics by childcare status and cohort. Since the data were collected using stratified sampling, we present the weighted means and standard deviations. The descriptive statistics are only reported for the sample which we will use in the regressions. Throughout we condition on the child living in Sweden when he or she is 24 years old.[12] Moreover, we condition on the there being complete information about the mother. However, we retain observations where the father is either unknown or there is missing information about the educational attainment of the father.

Table 1 indicates that childcare children are favorably selected in terms of their observed characteristics. In particular, the share of mothers with tertiary (compulsory) education is substantially higher (lower) for children who have attended childcare. Between the cohorts born 1967 and 1972 there is a remarkable increase in the share of mothers and (to some extent) fathers with tertiary education who have used childcare, which is much higher than the corresponding increase among parents in general. This is consistent with the view that the tax reform of 1971 improved the incentives for high-skilled mothers to enter the labor market.

Table 1 also illustrates the trend increase in childcare attendance. Between the cohorts born 1967 and 1982, the share who attended childcare rises from 15 percent to 76 percent.

[12] The sample is reduced by 216 individuals by conditioning on the individuals being alive and in Sweden at age 24 rather than at age 12. Note that this additional sample reduction has no implications for the effects we estimate on cognitive test outcomes.

Table 1: Descriptive statistics by cohort and childcare status

	Childcare status							
	No childcare				Some childcare			
	1967	**1972**	**1977**	**1982**	**1967**	**1972**	**1977**	**1982**
Characteristics of mother								
Compulsory education	0.500	0.364	0.347	0.307	0.312	0.231	0.174	0.133
Upper-secondary education	0.368	0.460	0.510	0.506	0.395	0.379	0.444	0.473
Tertiary education	0.132	0.176	0.143	0.187	0.293	0.390	0.382	0.394
Age at childbirth	26.5	26.5	27.6	28.2	25.5	26.3	27.0	28.1
	(5.5)	(4.9)	(4.8)	(5.1)	(5.4)	(4.6)	(4.7)	(4.9)
Born outside the Nordic countries	0.024	0.023	0.042	0.099	0.022	0.026	0.039	0.061
Characteristics of father								
Missing education	0.068	0.040	0.030	0.051	0.115	0.059	0.039	0.037
Compulsory education	0.420	0.399	0.381	0.322	0.336	0.271	0.237	0.211
Upper-secondary education	0.365	0.395	0.418	0.438	0.353	0.368	0.419	0.436
Tertiary education	0.147	0.166	0.171	0.189	0.196	0.302	0.305	0.316
Age at childbirth*	29.7	29.3	30.5	31.1	28.5	28.8	29.5	30.9
	(6.5)	(5.7)	(5.8)	(5.6)	(6.4)	(5.5)	(5.3)	(5.4)
Born outside the Nordic countries*	0.030	0.034	0.036	0.091	0.074	0.047	0.055	0.072
Father missing	0.011	0.004	0.005	0.013	0.025	0.004	0.008	0.007
Child characteristics								
Female	0.508	0.492	0.479	0.486	0.497	0.490	0.479	0.497
# of siblings (age 12)	1.6	1.5	1.8	2.0	1.3	1.3	1.4	1.6
	(1.0)	(1.0)	(1.1)	(1.2)	(0.9)	(0.9)	(1.0)	(1.0)

Table 1, cont.	No childcare				Some childcare			
	1967	**1972**	**1977**	**1982**	**1967**	**1972**	**1977**	**1982**
Child outcomes								
Language ability (rank)	49.6	48.5	47.5	45.5	52.5	54.8	51.5	51.7
	(28.9)	(28.7)	(29.1)	(28.8)	(28.5)	(28.7)	(28.5)	(28.7)
Inductive ability (rank)	50.0	49.3	46.7	46.3	50.0	52.3	52.1	51.4
	(29.0)	(29.0)	(29.0)	(28.7)	(27.9)	(28.3)	(28.5)	(28.8)
Academic upper-secondary education	0.404	0.454	0.438	0.478	0.484	0.551	0.577	0.600
# observations	4933	4317	1104	1330	871	1433	1889	4189
(share of cohort)	(0.85)	(0.75)	(0.37)	(0.24)	(0.15)	(0.25)	(0.63)	(0.76)

Notes: The table reports weighted means and standard deviations, using the sampling probabilities in each strata as weights.
* Descriptive statistics are only reported for fathers who are not missing.

The lower half of the table reports the means of our outcome variables – the percentile ranked results on two cognitive tests taken at age 13 as well as the probability of having attained a 3-year "academic" – i.e. university-preparatory – upper-secondary degree (at age 24). The inductive test requires the respondent to fill in the next number in a sequence of numbers. The language test involves finding a word having the opposite meaning as a given word.

We have percentile ranked the results of the cognitive tests within cohort. Across cohorts, the test results fall for children who have not attended childcare and there are corresponding increases for children who have participated in childcare. This pattern may reflect the fact that the children in childcare get more favorably selected over time.

Since we focus on the gap between children with an immigrant and native background, it is interesting to examine the immigrant/native gaps by childcare attendance. Throughout the paper, we define immigrant background as both parents being born outside the Nordic countries. Table 2 reports these outcomes.

Table 2: Differences in outcomes by immigrant background and childcare attendance

Childcare	**Immigrant background♦**		**Difference**
	Outcome: Language ability		
	No	**Yes**	
No	49.0	23.7	–25.3***
			(1.3)
Yes	53.2	32.7	–20.5***
			(1.6)
Difference	4.2***	9.0***	**4.8****
	(0.4)	(2.1)	**(2.1)**
	Outcome: Inductive ability		
No	49.1	37.2	–12.0***
			(1.6)
Yes	52.0	42.9	–9.1***
			(1.6)
Difference	2.8***	5.7***	**2.9**
	(0.4)	(2.2)	**(2.2)**
	Outcome: Academic upper-secondary degree		
No	0.436	0.505	0.069**
			(0.028)
Yes	0.581	0.619	0.038
			(0.028)
Difference	0.145***	0.114***	**–0.030**
	(0.007)	(0.039)	**(0.040)**

Notes: ♦Both parents are born outside the Nordic countries. Robust standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. "Difference-in-differences" estimates (bold numbers) are based on regressions including 20,216 observations.

Table 2 conveys several messages. First, individuals with an immigrant background have substantially lower test performance at age 13 than individuals with a native background; the gap in language ability is particularly large. Second, the gaps in cognitive test results between immigrants and natives are smaller among children who have attended childcare; however, it is only the reduction in language ability which is statistically significant. Third, despite the gaps in cognitive test performance, the probability of attaining an academic upper-secondary degree is higher among individuals with an immigrant background; moreover, among the individuals attending childcare the advantage in favor of immigrants is lower than among individuals with no childcare experience, although not significantly so.

Our purpose next is to examine whether these preliminary conclusions hold up to more rigorous analysis.

4 Empirical analysis

The main purpose of this section is to examine how childcare attendance affects future cognitive outcomes and long-run educational attainment. For various reasons we will rely on a selection of observables assumption. The main reason for making this assumption is that we have found no credible instrument which can be used to estimate the effects of interest. Furthermore, the virtue of an instrument is not all that obvious in this case. Since the nature of the selection differs across cohorts, a valid instrument will most likely yield different estimates across cohorts just because the set of “compliers” vary across cohorts (see Imbens and Angrist 1994).

Our main approach for examining whether selection on observables is a reasonable assumption is to vary the set of conditioning variables. If the coefficients of interest do not vary with the conditioning set we view the results as being robust.

To preview our results, we conclude that we cannot credibly estimate the average effect of childcare attendance. However, we consistently find that childcare attendance reduces the gap in language skills by immigrant background.

4.1 Empirical set-up

We specify the outcome equations as follows

$$y_{ijc} = \alpha_j + \alpha_c + \beta CC_{ijc} + \gamma(s \times CC)_{ijc} + \lambda s_{ijc} + \varphi_1 X_{1,ijc} + \varphi_2 X_{2,ijc} + \varepsilon_{ijc} \qquad (2)$$

where i indexes individuals, j municipalities, and c cohorts; thus α_j (α_c) denotes a municipality (cohort) fixed effect.

y_{ijc} denotes the outcome of interest, i.e., either the percentile ranked results on the (two) cognitive tests or educational attainment. The tests were conducted in 6th grade, when the children were aged 13. Educational attainment is measured at age 24; we specify this outcome as the probability of having at least 3 years of university-preparatory upper-secondary education.

CC_{ijc}, the treatment of interest, is defined to equal unity if the parents respond that their child has attended childcare; it equals zero if the parents have responded not at all. We also interact the treatment with indicators of the family background of the children (s). We consider two such interactions. We estimate a separate effect for children: (i) whose parents are both born outside the Nordic countries; and (ii) whose mother has only compulsory education.

We will also examine whether there are differential effects across childcare modes. Thus we define separate indicator variables for children who have attended pre-schools and family daycare units and interact these alternative treatment indicators with the family background of the children.

The two last pieces of notation in equation (2) concern the control variables that we include in the regression. The first set of variables ($X_{1,ijc}$) includes predetermined characteristics which should be included to control for selection on observed characteristics. The variables included in $X_{1,ijc}$ are basically the ones listed in Table 1. The other set of variables ($X_{2,ijc}$) include the variables that we will use to "test" our selection-on-observables assumption. The underlying idea is that if the estimates are plagued by selection (or omitted variables) bias and if the inclusion of $X_{2,ijc}$ moderates (or eliminates) this bias we should see substantial changes in the coefficients of interest when we control for $X_{2,ijc}$. In practice, this idea has been around for quite some time; Altonji et al. (2005) provides a formal justification for such sensitivity analyses.

In the current application, we will include the result on a spatial ability test. The inclusion of this test arguably controls for selection. The problem with including it is that the test is conducted at age 13. Therefore, the variation in spatial ability is potentially an outcome of childcare attendance. However, evidence reported by Cahan and Cohen (1989) as well as the recent evidence presented in Öckert (2009) suggests that spatial ability is less malleable to schooling than inductive and language ability. According to Öckert, a year of schooling improves inductive and verbal ability by 0.17–0.18 standard deviations, but spatial ability "only" increases by 0.07 standard deviations.

4.2 The distributional impact of childcare

Table 3 presents the results. Columns (1)–(3) report the results for language ability, while columns (4)–(6) contain the results for inductive ability ("number series"). Panels A–B consider the interaction between childcare attendance and immigrant background (panel A) and mother's education (panel B), respectively.

Looking at Table 3 it is clear that the main effect of childcare attendance is not credibly identified. While the correlations presented in columns (1) and (4) are all positive and significant, they are all rendered insignificant just by controlling for observed characteristics; see columns (2) and (5). If selection on observed and unobserved characteristics works much in the same way it is not hard to imagine that the main effects would be reduced further. The evidence reported in columns (3) and (6) is consistent with this conjecture. Here we include the measure of spatial ability which reduces the size of the main effect further.

Under the assumption that selection is the same across groups (we will relax this assumption below), it may still be meaningful to examine the distributional impact of childcare attendance. The second row in each panel thus contains the estimates of the interaction between childcare attendance and immigrant background and the mother being less educated, respectively.

Table 3: Effects of childcare attendance on cognitive outcomes by family background

	Language ability			Inductive ability		
	(1)	(2)	(3)	(4)	(5)	(6)
	A. Immigrant background (both parents born outside the Nordic countries)					
Childcare, main effect	4.25***	0.645	0.319	3.36***	0.458	0.048
	(0.63)	(0.620)	(0.584)	(0.63)	(0.618)	(0.564)
Childcare interaction	7.92***	9.04***	8.11***	0.488	0.817	–0.356
	(2.57)	(2.43)	(2.42)	(2.580)	(2.547)	(2.382)
Main effect	–25.0***	–20.7***	–18.8***	–11.9***	–14.0***	–11.5***
(immigrant background)	(1.7)	(2.6)	(2.5)	(1.9)	(2.9)	(2.8)
Spatial ability	--	--	0.344***	--	--	0.431***
			(0.008)			(0.008)
Cohort FE:s	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE:s	Yes	Yes	Yes	Yes	Yes	Yes
Basic covariates	--	Yes	Yes	--	Yes	Yes
Adjusted R^2 (within municipality)	0.025	0.126	0.233	0.015	0.073	0.250
# observations	20,126	20,126	20,126	20,126	20,126	20,126

Table 3, cont.	Language ability			Inductive ability		
	(1)	(2)	(3)	(4)	(5)	(6)
			B. Mother less educated (no more than compulsory education)			
Childcare, main effect	4.41***	1.34**	0.808	3.40***	1.13*	0.452
	(0.69)	(0.68)	(0.634)	(0.69)	(0.68)	(0.612)
Childcare interaction	–4.66***	–1.68	–0.977	–4.64***	–2.46**	–1.58
	(1.22)	(1.21)	(1.139)	(1.22)	(1.22)	(1.11)
Main effect (low education)	–9.13***	–12.9***	–10.1***	–7.52***	–10.6***	–7.04***
	(0.68)	(0.8)	(0.8)	(0.69)	(0.8)	(0.77)
Spatial ability	--	--	0.344***	--	--	0.431***
			(0.008)			(0.008)
Cohort FE:s	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE:s	Yes	Yes	Yes	Yes	Yes	Yes
Basic covariates	--	Yes	Yes	--	Yes	Yes
Adjusted R^2 (within municipality)	0.040	0.120	0.232	0.031	0.073	0.250
# observations	20,126	20,126	20,126	20,126	20,126	20,126

Notes: Linear regression models estimated using the inverse sampling probabilities as weights. Robust standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. Basic covariates include: gender; number of siblings; the mother's educational attainment, age at childbirth, and immigrant background; the father's educational attainment, age at childbirth, immigrant background, and an indicator for unknown father; the immigrant status of both parents.

It seems that childcare attendance narrows the distribution of language skills for children with different immigrant backgrounds. The effects on language skills do not vary by mother's education, however. Moreover, there is no distributional impact of childcare attendance on inductive skills.

It is noteworthy that the interaction estimate for children with immigrant background stays almost the same when we control for spatial ability. This suggests that selection is not driving the interaction estimate.

What does the estimate on the interaction between childcare and immigrant background imply? Jonsson (2004) shows that, on average, the children with some childcare experience in the cohorts born 1966–81 have spent roughly three years in childcare. The estimate in column (3) thus suggests that each year of childcare experience reduces the gap in language ability between immigrants and natives by 2.7 (8.1/3 = 2.7) percentile ranks. The raw gap between immigrants and natives with no childcare experience amounts to 25 percentile ranks (see Table 2 or column (1), panel A). Thus, each year of childcare experience closes 10 percent of the gap between immigrants and natives; 5 years of childcare experience reduces the gap by 50 percent. These effects are rather substantial, suggesting that childcare is an important vehicle for closing the gap between immigrants and natives in terms of language ability.

Next, let us turn to the effects on long-run educational attainment. Constraints related to data quality force us to focus on the probability of having at least a 3-year university-preparatory – an "academic" – upper-secondary degree. We measure this outcome at age 24.[13]

Table 4 presents the results. Despite the fact that childcare improves the language ability of immigrants relative to natives, there is no differential effect on the probability of attaining an academic upper-secondary degree. Moreover, there is no differential effect by mother's education (see panel B), which is consistent with there being no differential effects of childcare attendance on the cognitive outcomes by mother's education.

Determining exactly why there are no differential effects by immigrant background is, to some extent, a matter of speculation. But it seems that the effect on language skills is too small to alter the choices made by children with an immigrant background. Note, in this respect, that the main effect of having an immigrant background is consistently positive, despite the fact that immigrants have both lower test results at age 13 and lower grade point average (GPA) when leaving compulsory school. Thus, cognitive skills (as

[13] High-quality information on educational attainment is available to us 1991–2006. In 1991, the oldest cohort (those born 1967) are 24 years-old, while, in 2006, the youngest cohort (those born in 1982) is 24 years-old. It would have been preferable to record educational attainment at a higher age, because then we could have included tertiary education. Alternatively, a more "discriminatory" outcome would be the probability of having an upper-secondary degree at age 19 (which is the normal graduation age). However, none of these two options are open to us because of data constraints.

measured by the tests or GPA) have a smaller impact on subsequent educational choices among immigrants than among natives.

Table 4: Effects of childcare attendance on educational attainment (at least academic upper-secondary degree at age 24) by family background

	(1)	(2)	(3)
	A. Immigrant background (both parents born outside the Nordic countries)		
Childcare, main effect	0.097***	0.020*	0.017
	(.011)	(0.011)	(0.010)
Childcare interaction	0.005	0.021	0.012
	(.051)	(0.048)	(0.048)
Main effect	0.055	0.082	0.101*
(immigrant background)	(0.036)	(0.053)	(.054)
(Spatial ability)/100	--	--	0.337***
			(0.014)
Cohort FE:s	Yes	Yes	Yes
Municipality FE:s	Yes	Yes	Yes
Basic covariates	--	Yes	Yes
Adjusted R^2 (within municipality)	0.032	0.160	0.196
# observations	20,126	20,126	20,126
	B. Mother less educated (no more than compulsory education)		
Childcare, main effect	0.077***	0.023**	0.018
	(.012)	(0.012)	(0.011)
Childcare interaction	–0.055***	–0.010	–0.003
	(0.021)	(0.020)	(0.020)
Main effect	–0.203***	–0.286***	–0.258***
(low education)	(0.012)	(0.014)	(0.014)
(Spatial ability)/100	--	--	0.337***
			(0.014)
Cohort FE:s	Yes	Yes	Yes
Municipality FE:s	Yes	Yes	Yes
Basic covariates	--	Yes	Yes
Adjusted R^2 (within municipality)	0.071	0.160	0.196
# observations	20,126	20,126	20,126

Notes: Linear probability models estimated using the inverse sampling probabilities as weights. Robust standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. Basic covariates include: gender; number of siblings; the mother's educational attainment, age at childbirth, and immigrant background; the father's educational attainment, age at childbirth, immigrant background, and an indicator for unknown father; the immigrant status of both parents.

4.3 Robustness checks and extensions

The purpose of this section is to present some robustness checks and extensions of our baseline specification. Throughout, we focus on the differential effect on language ability by immigrant background. We view the estimate presented in column (3) of Table 3 as our baseline result. In Table 5 we examine whether this estimate is robust to alternative assumptions and alternative definitions of treatment; for convenience the first row reproduces the baseline estimate.

Table 5: Effect of childcare attendance on language ability by immigrant background, variations of the baseline specifications

		Main effect	Interaction with immigrant background	# obs.	Adj. R2
(1):	Baseline estimate	0.319 (0.584)	8.11*** (2.42)	20,126	0.233
(2):	Allowing for differential selection by immigrant background	0.226 (0.583)	9.16*** (2.39)	20,126	0.237
(3):	(2) with separate treatment effects for pre-school and family daycare			20,126	0.236
	Pre-school	0.282 (0.579)	5.72** (2.52)		
	Family daycare	–0.201 (0.676)	10.5** (4.9)		
(4):	(3) estimated by cohort				
	<u>1967</u>			5,864	0.237
	Pre-school	–0.689 (1.354)	21.3*** (6.3)		
	Family daycare	1.87 (1.89)	28.3*** (3.9)		
	<u>1972</u>			5,750	0.222
	Pre-school	1.26 (1.19)	0.290 (7.382)		
	Family daycare	3.72* (2.05)	22.6 (18.4)		
	<u>1977</u>			2,993	0.253
	Pre-school	–0.400 (1.222)	9.00 (5.83)		
	Family daycare	–0.669 (1.223)	13.1 (9.2)		

Table 5, cont.	Main effect	Interaction with immigrant background	# obs.	Adj. R2
1982			5,519	0.260
Pre-schools	0.502	7.22**		
	(0.916)	(3.05)		
Family daycare	–1.09	10.0*		
	(0.92)	(5.1)		

Notes: Model (2) includes an interaction between immigrant background and spatial ability, the predicted earnings of the mother, and the predicted earnings of the mother interacted with immigrant background. The models in (4) are estimated separately by cohort. Robust standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. Regressions are weighted using the inverse of the sampling probabilities. Covariates include: gender; number of siblings; the mother's educational attainment, age at childbirth, and immigrant background; the father's educational attainment, age at childbirth, immigrant background, and an indicator for unknown father; the immigrant status of both parents; and spatial ability.

An identifying assumption underlying our baseline results is that the process determining selection into childcare is the same on average for immigrants and natives. To be more precise, we assume that the correlations of the unobserved and observed variables (e.g. ability) are linear in the covariates and the same across the two groups. In model (2) we relax these assumptions by allowing the coefficient on spatial ability to vary by immigrant background; moreover, we introduce the predicted earnings of the mother which is allowed to have a separate effect for children with an immigrant background.[14] If the estimates are unaffected by these extensions we interpret this as saying that (potential) unobserved variables varying across the two groups does not bias the baseline estimates.

Row (2) shows that allowing for differential selection by immigrant background has no implications for the baseline result (if anything the result is strengthened). The estimate on the interaction between immigrant background and childcare attendance equals 9.2 with a t-ratio of 3.8.

The model in (3) includes separate treatment effects for pre-schools and family daycare. As explained earlier these two childcare modes imply different kinds of treatments. The pedagogical content may be higher in pre-schools but group sizes are also higher. The magnitudes of the estimates imply that family daycare reduces the gap between immigrants and natives more than pre-schools, although the estimates are not different from each other in the statistical sense.

[14] The reason for introducing the linear earnings index, rather than interacting observed covariates fully with immigrant status, is that we want to save on degrees of freedom. To obtain the earnings predictions we have estimated equation (1) separately for immigrant and native mothers. Note that it does not matter for the results if we use a single set of estimates for both groups.

Finally, in (4) we estimate the model in (3) separately by cohort. There is some variation in the estimate of the treatment interactions across cohorts. The lower bounds of the (95 percent) confidence bands estimated for the 1967 cohort are higher than the point estimate of the pooled regression in model (3). The confidence bands of all other cohorts cover the corresponding estimate of model (3). It is also noteworthy that family daycare consistently appears to reduce the language gap more than pre-schools.

In sum, we view the variations reported in Table 5 as lending support to the baseline estimates reported in panel A) of Table 3. Childcare attendance thus reduces the gap in language skills between immigrant and native children.

4.4 Summary and discussion of the results

Let us summarize the results. Comparing children of immigrant and native background we find that:

I childcare attendance reduces the gap in language skills across the two groups

II family day care appears to reduce the gap more than regular pre-schools

III there is no effect of childcare on the gap in inductive skills across these two groups

Comparing children with low and high-educated mothers we find that:

IV there are no effects of childcare on the gaps in language and inductive skills

What is the rationale for these (medium-run) findings? We think that the configuration of the results suggests that what childcare offers is mainly an arena for interaction with other children as well as staff. Any pedagogical treatment effects appear to be limited – or at least not substantial enough to alter medium-run cognitive achievement.

If pedagogical content would have been an important part of the treatment we would expect to see a reduction in the gap in inductive skills as well, a decrease in the cognitive ability gaps along the educational dimension, and regular pre-schools to have greater effects on the achievement gaps than family daycare.

Rather we observe: a reduction in the language ability gap only; this effect only shows up along the immigrant/native comparison; and, if anything, family daycare has a greater effect on the language ability gap among immigrants and natives. This suggests to us that childcare furthers the language ability of immigrants since it opens up for closer interaction with native-born children and Swedish speaking staff.

We have not been able to detect any differential long-run effect of childcare on educational attainment. It may be that the effect on language skills is too small to alter the choices made by children with an immigrant background.

5 Concluding remarks

In this paper we have estimated the relationship between childcare attendance and medium and long run educational outcomes. We have done this using data on individuals born between 1967 and 1982.

The time period spanned by these cohorts featured a substantial expansion of childcare: in 1975, 18 percent of children aged 1–5 attended childcare; by 1985 (in just 10 years) the share of 1–5 year-olds participating in childcare had increased to 56 percent. The childcare expansion was intimately tied to the increase of the labor force participation of women. We have illustrated that, across cohorts, children in childcare were increasingly drawn from the higher end of the distribution of family background characteristics.

The changes in the composition of participating children raise issues regarding the selection into childcare. For that reason we have focused on whether childcare attendance has differential effects by immigrant background. We have found that childcare participation narrows the language ability gap between children with an immigrant background and children with a native background. Our estimates imply that a year of childcare experience reduces the overall gap between immigrants and natives in language ability by 10 percent. This conclusion is robust to allowing differential selection across immigrants and natives.

We have found no differential effects on inductive skills, however. Nor does childcare affect the distribution of longer-run educational attainment. The latter result is somewhat surprising, given that the gap in language skills is affected by childcare attendance. Taken seriously, it is perhaps due to the effect on language skills being too small to alter the educational choices of immigrants; educational choices of immigrants in Sweden seem to be driven by cognitive ability to a lesser extent than among natives. But for (at least) two reasons it would be premature to conclude that childcare has no differential long-run effects. First, since some of the individuals included in the analysis are born in the 1980s, we measure educational attainment at a relatively young age (24 years-of-age). Therefore, we have focused on the probability of attaining a university-preparatory upper-secondary degree. Since we cannot account for tertiary education we may miss some of the potential effect on educational attainment. Second, we are perhaps ultimately interested in whether the differential effects on language ability feed on to long-run earnings outcomes. Again, the time-span of our data precludes such an analysis.

In contrast to the vast majority of U.S. states, pre-school interventions in Sweden are not targeted at the disadvantaged. Rather they are universally available; during the time period we have considered they were in fact targeted at the employed. Disadvantaged (particularly immigrant) children are less likely to participate in pre-school interventions. The evidence we have offered suggests that increasing the childcare participation rates among immigrant children will close some of the gap between natives and immigrants in language skills.

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Essay 3: Moral hazard among the sick and unemployed: Evidence from a Swedish social insurance reform[♦]

Co-authored with Laura Hartman

1 Introduction

Moral hazard arises when the behavior of an insured person is affected by how the insurance is constructed. It is a common problem associated with insurance and the extent of it has been explored in many empirical studies. However, most studies consider one insurance program at a time. Moral hazard that arises in the interplay between various programs is a largely unexplored research area, as pointed out by Krueger and Meyer (2002) in their *Handbook of Public Economics* chapter on labor supply effects of social insurance, as well as by the European Economic Advisory Group (2007).

This paper looks at a specific type of moral hazard that arises in the interplay between two large public insurance systems in Sweden, namely the sickness insurance (SI) and the unemployment insurance (UI). More specifically, we address the question of whether differences in benefit generosity affect the hazard rate from UI to SI benefits. Flows between the UI and the SI that arise due to differences in benefit generosity are interesting for several reasons. First, such a finding can be taken as evidence for insufficient monitoring as it indicates that economic incentives rather than unemployment (UI), health (SI), or some other condition determine the choice of benefits. In that case we should expect that monitoring in these programs in general is insufficient, implying a rather widespread misuse.

Second, it is crucial to take such flows between programs into account when evaluating the effects of reforms that only change the structure of a

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single program. Simply looking at the benefit size and the use of one system at a time may not be enough for a fair cost-efficiency analysis of the reform. Previous estimates of, for example, the effect of reduced UI benefits on job finding rates may be biased if the reduced benefits induce the unemployed to shift to some other program instead of inducing them to search harder for jobs.

Turning to the specific case studied in this paper, it is possible for an unemployed person in Sweden to report sick and receive SI benefits even for a short period. This rule is based on the idea that job search is comparable to work. In order to be eligible for UI benefits, an unemployed person should actively search for jobs and be able to accept a job offer at short notice. Unemployed persons who lose their work (search) capacity due to sickness should therefore receive benefits from the SI rather than the UI.

There are at least two sources of moral hazard in this context. First, UI benefits are limited to 300 work days whereas SI benefits, until very recently, have had no time-limit. By reporting sick an unemployed person has been able to postpone the UI expiration date. A previous study from Sweden (Larsson 2006) shows that the probability of reporting sick among the unemployed increases drastically as the UI expiration date approaches. Henningsen (2007) finds the same pattern in Norway, where the institutional setting is similar to Sweden. However, whether these results are due to economic incentives or actual health deterioration caused by stress remains to be explored.[1]

Second, moral hazard can arise from the benefit size structure. For some unemployed persons, benefits from the SI are higher than benefits from the UI. Both benefits are determined by the worker's pre-unemployment wage, the replacement rate being approximately the same, whereas the cap – i.e. the maximum amount – for most periods has been higher in the SI than in the UI system. Thus, the high-wage unemployed workers have been able to receive higher benefits from the SI than from the UI. In the early 2000s, SI benefits could be up to 20 percent higher than maximum UI benefits. For unemployed persons who have received UI benefits for 100 days, the UI benefit cap drops by approximately 7 percent, implying that the SI benefits could be nearly 30 percent higher for such 'long-term' unemployed persons. Larsson (2006) looks into this potential source of moral hazard as well and finds that the difference in benefits seems to increase the probability of reporting sick.

In this study, we use a reform of the SI system that came into force on 1 July 2003 to identify the effect of economic incentives arising from the dif-

[1] Several empirical studies indicate that exit rates from unemployment to employment increase as workers approach the benefit expiration date. Evidence from the United States is reported by Moffitt (1985), Meyer (1990), and Katz and Meyer (1990). Swedish evidence is found in Carling et al (1996).

ferent benefit sizes. By exploiting some unique features of this reform, we are able to identify the effects of economic incentives in a more reliable way compared to the previous study by Larsson (2006). In essence, the purpose of the reform was to eliminate the difference in benefits by lowering the SI benefit cap to the same level as the UI benefit cap during the first 100 days of unemployment. We would expect sickness absence to decrease due to the reform as the benefits from the SI no longer exceed the benefits from the UI.

We identify the effect of economic incentives using the fact that the reform affected various groups of unemployed persons differently and at different durations of unemployment. First, as workers become unemployed at different dates, the reform affected them at different lengths into their unemployment period. This enables us to separate the reform effect from the effect of unemployment duration. Second, the reduction of the SI benefit cap affected only those who had a previous wage above the new, lower cap. Persons with a lower previous wage can be used as a comparison group. Finally, our data contains repeated unemployment spells, allowing us to test for unobserved individual heterogeneity.

Our results suggest strong negative effects on the incidence of sickness absence. Due to the lowered benefit cap, the incidence of sick reports was reduced by about 36 percent more among the treated compared to the comparison group. As the average drop in benefits in our sample was roughly 9 percent, we estimate an elasticity of sick reports with respect to sickness benefits of about 3.9. The result is very robust across various specifications.

The remainder of this paper is organized as follows: Section 2 presents the central features of Sweden's UI and SI systems; Section 3 discusses identification issues; Section 4 presents the data; Section 5 shows the empirical results; Section 6 discusses the economic significance of the reform and concludes.

2 Unemployment and sickness insurance in Sweden[2]

SI and UI form an integral part of the public social insurance system in Sweden. Benefits from the public social insurance are income-related and for the most part financed by taxes. The system, being a part of the Swedish welfare state, can be characterized as general rather than selective. That is, most citizens are comprised by the system, and the degree of means testing is low. Moreover, the Swedish system is often perceived as generous with high replacement rates by international standards.

[2] This section describes the systems as they were in 2003.

2.1 Description of the unemployment insurance

The UI provides income-related compensation for a maximum period of 60 weeks. During 2003, the replacement ratio was 80 percent up to a cap, approximately equal to the mean wage of a Swedish worker. For income-related benefits, the unemployed person has to fulfill three conditions:[3]

- *The basic condition* that the unemployed person is available for vacant jobs. In practice this means that he has to be registered at the public employment office as a job seeker and that he is willing to accept a job.
- *The membership condition* that the unemployed person has been a member of a UI fund for at least twelve months prior to unemployment. Membership is voluntary.
- *The working condition* that the unemployed has worked at least six months during the last twelve months preceding unemployment.

If the unemployed person has been a member of a UI fund for a shorter period than a year but still fulfills the other two conditions, he is entitled to a fixed basic amount of compensation.

The UI is administered by 36 UI funds representing workers from different occupational groups. All together, the UI funds have approximately 3.8 million members, corresponding to 85 percent of the work force and 65 percent of the adult population. The funds are formally independent, but they must be officially approved by the state and follow common regulations in order to receive a grant from the state. Until lately, the main source of finance for the UI benefits has been the state grant, the remaining part being financed by membership fees.

The UI funds work closely with the local public employment offices, especially in controlling whether the unemployed person fulfills the rules concerning job search. The unemployed person has to meet his employment officer regularly and he is obliged to apply for any job the officer assigns him. If he does not meet these requirements, the employment officer must write a report to the UI fund, which then decides on a suitable sanction. In short, either the unemployed person is suspended from the UI, or his benefits are reduced. These sanctions are time-limited or permanent, depending on if the person has violated the rules before, and the expected duration of the employment he refuses to accept.

UI benefits are time-limited to 300 workdays, corresponding to 60 weeks. These benefit days can be received either continuously or with breaks in the unemployment period. If working long enough – basically at least six months – during a break, a person can qualify for a new period of 300 days.

The UI benefit amounts were changed by the new Government in January 2007. The description below concerns the rules during 2003. The income-related UI benefits were 80 percent of the worker's average earnings during

[3] For a detailed description, see e.g. www.aea.se.

the last six months of work, with a lower and an upper limit. Figure 1 illustrates. The fixed basic amount of SEK 7,040 (≈ € 750)[4] per month constituted the minimum, corresponding to 80 percent of a monthly wage of SEK 8,800. The upper limit varied depending on how long the person had been unemployed. During the first 100 days of unemployment, the maximum benefits were 80 percent of a monthly wage of SEK 20,075. After the first 100 days, the cap was reduced to 80 percent of SEK 18,700.[5]

The first five days of involuntary unemployment are uncompensated. If the unemployment is voluntary, i.e. if the person has left his job without a valid reason or if he has been laid off because of improper behavior, the uncompensated period is up to 45 benefit days.

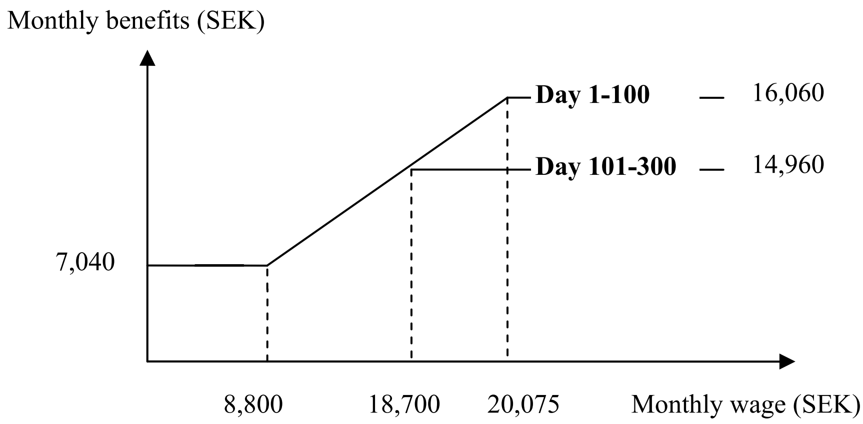

Figure 1: UI benefits in 2003

2.2 Description of the sickness insurance

The purpose of the SI is to provide economic maintenance when the worker is too sick to work and support himself. Benefits are income-related and, until 2008, there has not been any formal time-limit. In recent years, the replacement ratio has been around 80 percent. Just like the UI benefits system, the SI system contains a benefit cap.

All employed workers are automatically covered by the SI. Students and unemployed workers are also eligible for SI benefits as long as they fulfill certain conditions. An unemployed person must be registered at a local employment office as a job seeker. The size of an unemployed person's SI

[4] Exchange rate April, 2006.
[5] These amounts were constant between 1 July 2002 and 31 December 2006.

benefits is not based on his UI benefits but on his wage before unemployment. Thus, unemployed persons without any employment history do not receive SI benefits.

The SI is administered by the Swedish Social Insurance Agency and financed by payroll taxes. The first day of sickness is uncompensated. Employers are responsible for the employees' sickness compensation during the following 13 days of sickness, a period which was extended to 20 days between 1 July 2003 and 31 December 2004; after that the Social Insurance Agency takes over. For unemployed persons, the Social Insurance Agency is responsible for the sick pay from day two.[6]

The SI system contains some control instruments to prevent unjustified use of the insurance. After reporting sick by contacting either his employer (employed workers) or the Social Insurance Agency (non-employed), the person must visit a doctor within seven days of sickness in order to receive additional compensation after the first week. Again after four weeks, a doctor's certificate must be provided to the SI authorities.

A reform on 1 July 2003 changed the marginal replacement rate in two ways, the effect being different for employed and unemployed workers. Figure 2 illustrates the case for an unemployed worker. The size of the SI benefits depends on the person's wage prior to the sick period. For unemployed workers, it is based on the wage prior to unemployment. Before the reform, the replacement rate was 80 percent of the previous (pre-unemployment) wage. The minimum wage for receiving any SI benefits was SEK 775 per month, and the maximum SEK 24,125 per month. In other words, SI benefits varied between SEK 620 and SEK 19,300 per month.[7] The reform implied two changes: First, it reduced the marginal replacement rate to 77.6 percent. This concerned all insured, employed as well as unemployed. Second, for the unemployed insured, the maximum SI benefits were reduced to SEK 16,060 per month, which corresponded to the maximum monthly UI benefits.

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[6] This asymmetry in rules has important implications for data and thus for our study. The data from the Swedish Social Insurance Agency includes all sick spells for unemployed persons, whereas sick-spells shorter than or equal to the two (or three) weeks during which the employer is responsible are not included. Thus, we cannot use employed workers as a comparison group.

[7] Not accounting for the first uncompensated day.

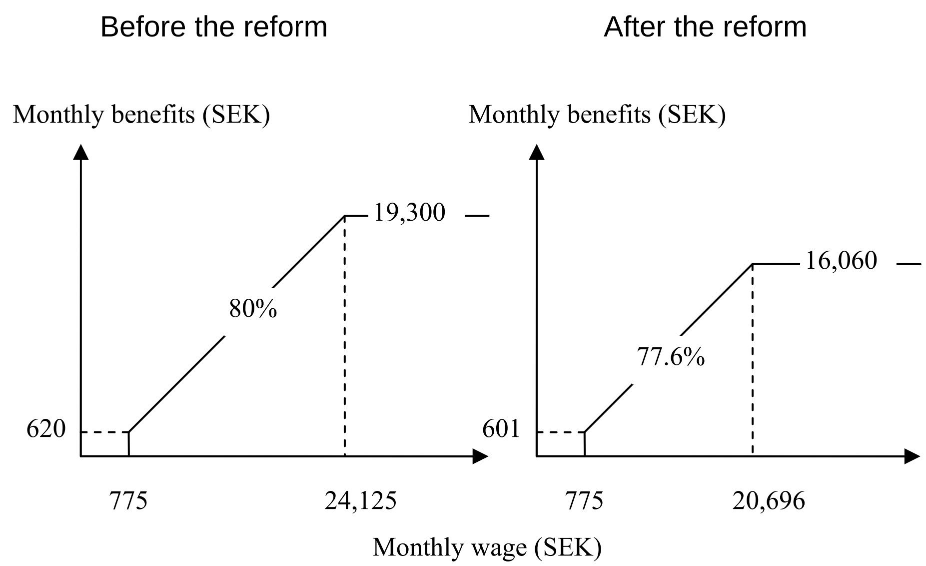

Figure 2: SI benefits for unemployed workers, before and after the reform in July, 2003

An additional aspect of the reform which is important for our study is that it affected *all* unemployed insured, that is, even those with already ongoing unemployment spells had their SI benefits reduced on 1 July 2003. This feature turns out to be important for our identification strategy.

3 Identification strategy

The fundamental research question of interest is how the size of economic compensation affects sickness absence. The reform that reduced the SI benefit cap serves as an ideal tool for identification. First, as workers become unemployed at different dates, the reform affected them at different lengths into their unemployment period. By exploiting this variation, we can separate the reform effect from the effect of unemployment duration. Second, it divides the unemployed population into *treated* and *non-treated* as it only affected persons with previous wages above the UI benefit cap.[8] Finally, our data contains repeated unemployment spells, which allows us to control for unobserved individual heterogeneity.

[8] Lack of data on short sick spells for employed persons prevents us from using the employed as an additional comparison group.

Let us start by looking more closely into how the reform affected the difference between SI and UI benefits for various types of unemployed persons. Recall that the difference depended on i) the previous, pre-unemployment wage, and ii) whether the unemployed person had received UI benefits for less or more than 100 days. Figure 3 illustrates the case of an unemployed person who has not passed the 100-day limit, i.e. before the UI benefit cap drops.

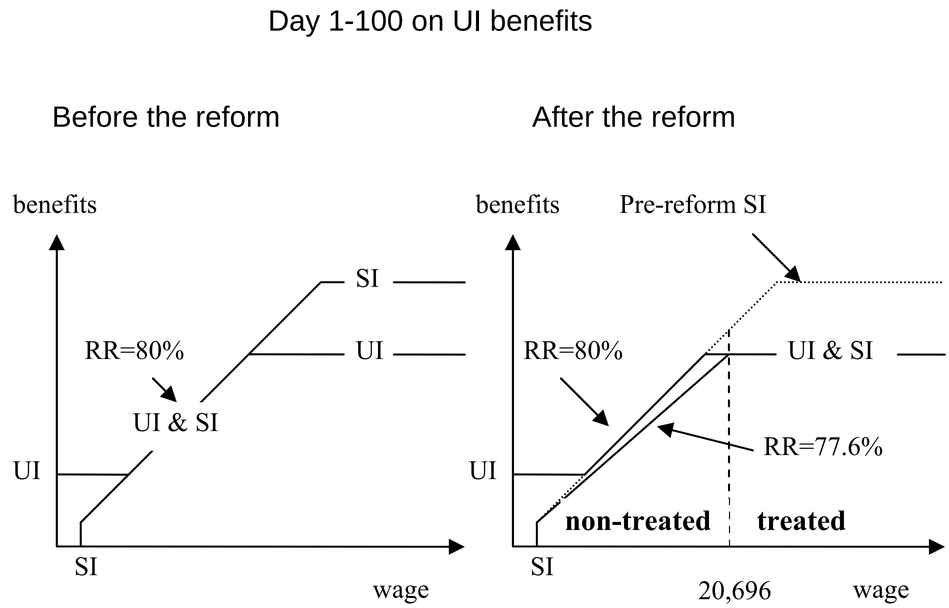

Figure 3: The change in relative benefit size due to the reform, during the first 100 UI benefit days

The reform changed the SI benefits for everybody, as the marginal replacement rate was reduced from 80 to 77.6 percent. Thus, the relative SI benefits (as compared with UI benefits) were reduced for all unemployed persons. However, up to the previous wage of SEK 20,696 the change was relatively small and, more importantly, equal to all.[9] These are the non-treated or comparison persons. For unemployed persons with a previous wage above

[9] The reform reduced the SI benefits with 3 percent for all unemployed persons with a previous wage up to SEK 20,696. Persons with very low previous earnings are an exception, as the reform also implied a marginal reduction of the minimum wage for SI eligibility; from SEK 620 to SEK 601. Hence, persons in this income group got eligible for SI benefits and thus experienced a benefit increase. However, our data do not include observations in this income interval.

that level, the treated, the reform implied a reduction of SI benefits that varied from 3 up to almost 17 percent.

Figure 4 illustrates the case for an unemployed person who has passed the first 100 UI benefit days. The pattern is somewhat different as the UI benefit cap is now lower, implying that even after the reform, benefits from the SI are higher than benefits from the UI for high-wage unemployed persons. But the effect of the reform on the benefit *difference* is similar to Figure 3: up to a previous wage of SEK 20,696 the SI benefits were reduced by 3 percent. From that level upwards, the reduction was larger the higher the previous wage, varying between 3 and almost 17 percent. So again, the population can be divided into *treated* and *comparisons* according to the previous wage, the dividing line being at SEK 20,696.

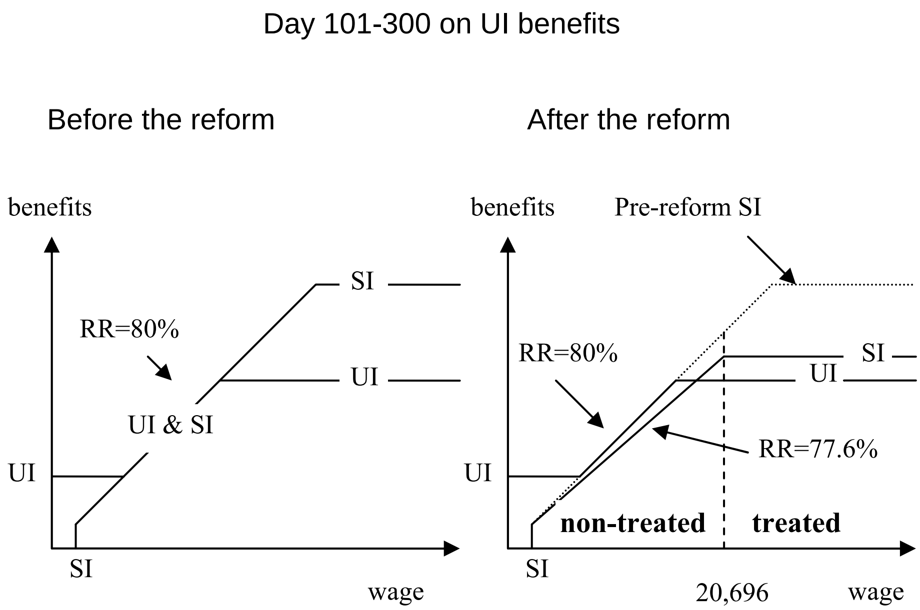

Figure 4: The change in relative benefit size due to the reform, after the first 100 UI benefit days

We will analyze the behavioral response to the change in compensation size in terms of the conditional incidence of sickness absence. That is, the incidence of sickness absence at day t of unemployment, conditional on remaining unemployed up until this day.[10] In addition to making use of the treatment and comparison group, our identification strategy exploits the tim-

[10] In what follows, we will simply refer to this as ‘incidence’.

ing of the reform. The timing feature arises when we use duration data and have a fixed reform date. As workers become unemployed at different dates, the reform affects them at different durations of unemployment. This variation can be used to separate the reform effect from the effect of unemployment duration. We do this by comparing the evolution of hazard rates to sickness for people who experienced the reform at different stages of their unemployment period. For example, the unemployed who experienced the reform 30 days into their unemployment spell are compared with those whose unemployment spells are at least 30 days but who do not experience the reform, either until after day 30 or never.

This strategy enables us to identify the effect of the reform *date*. It is possible that other changes in the environment occurred around the time of the reform affecting transitions out of unemployment. In order to separate the effect of the benefit level from such factors, we compare the reform-date effect for the treated and the non-treated. A larger effect for the treated, who experienced a larger cut in the replacement rate, indicates responsiveness to economic incentives. Hence, the policy change we use to identify the behavioral response to the compensation size is not the entire reduction in SI benefits due to the reform in July 2003, but rather the reduction over and above the general 3 percent reduction in the replacement rate. The effect of the 3 percent reduction cannot be identified as long as we believe that other changes in the environment occurred around the time of the reform.

To estimate the effect of the policy change, we use a Cox regression model. The advantage of imposing this semi-parametric structure instead of estimating fully non-parametric hazard rates is that we can control for some potentially important confounders, such as the time of inflow into unemployment. The baseline specification to be estimated can be written as:

$$\lambda(t) = \lambda_0(t)\exp\{f(x, z(t), \mathbf{\Omega}) + \delta D_t^{July03} + \gamma T + \beta D_t^{July03} T\} \tag{1}$$

where λ_0 is the baseline hazard rate, i.e. the pre-reform hazard to sickness. $f(.)$ is a function of the time-invariant covariates x and the time-varying covariates $z(t)$, and $\mathbf{\Omega}$ is a vector of parameters corresponding to the covariates. D_t^{July03} is a time-varying dummy variable, where $D_t^{July03}=0$ prior to 1 July 2003 and $D_t^{July03}=1$ thereafter. T is a dummy for the treatment group, were T=0 if the previous wage is below SEK 20,696 and T=1 for wages above that. The effect of the reduction in the SI benefit cap is obtained by comparing the change in hazard rates for the treated and the comparisons after the 1st of July 2003. The effect of the policy change is given by the coefficient of the interaction variable, β.[11]

[11] Concerns about the interpretation of interaction terms in non-linear models have been raised by Ai and Norton (2003). However, Puhani (2008) demonstrates that these concerns are not relevant for the estimation of the treatment effect in nonlinear 'difference-in-differences'

The underlying assumption behind this 'difference-in-differences' approach is thus that the development over time of the hazard to sickness in the comparison group, captures the counterfactual development in the treatment group, had the policy change not occurred.[12] This assumption may be violated if, for example, the labor market opportunities developed differently for the two groups around July 2003, leading to divergent changes in the (health) composition of the two groups. It is thus essential to check whether our estimates are affected by compositional changes in unobserved factors.[13] We do this by estimating stratified models. We first use the week of inflow into unemployment as well as the local labor market as stratification units. This model should be less sensitive to compositional changes regarding unobserved factors as the reform effect is identified solely by comparing individuals beginning their unemployment period during the same week *and* in the same local labor market. Moreover, access to repeated spell data for about half the sample allows us to also stratify on the individual. This model hence controls for all unobserved individual heterogeneity that is persistent over time.[14] Our results turn out to be robust in all these respects.

4 Data

We combine data from different sources for the empirical analysis. The database *ASTAT*, originating from the unemployment insurance funds and the *Sickness Benefit Register* (SFR) from the Social Insurance Agency constitute the two main sources. These two datasets are a part of *LINDA*, which is a register-based longitudinal database that includes about 3 percent of the Swedish population.[15] LINDA additionally contains several demographic variables collected from e.g. tax registers.

ASTAT contains information on benefit payments for all unemployed persons who have been entitled to either basic-amount or income-related UI benefits. It is most common to receive income-related benefits; during 2003 only about 9 percent of all benefit days were on the basic-amount. Each week ASTAT registers the number of benefit days received, together with

models. As is shown by Puhani, the treatment effect is here the incremental effect of the coefficient of the interaction term.

[12] Note that this assumption entails assuming that both groups responded in the same way to the general 3 percent reduction in the replacement rate.

[13] It is well known that problems with unobserved heterogeneity are particularly important to handle when estimating duration models. Contrary to usual regression models, even unobserved heterogeneity which is uncorrelated with the included covariates may cause biased coefficients.

[14] This method has the advantage of not requiring the strong assumption of independence of observed and unobserved explanatory variables, which is necessary in order to take unobserved heterogeneity into account using single spell data. See e.g. van den Berg (2001) for detailed discussions of identification issues in duration models.

[15] For a detailed description of LINDA, see Edin and Fredriksson (2000).

information on benefit amounts and the number of days left until a person's UI benefits expire. For unemployed with income-related benefits the database also includes information on the previous wage.

SFR contains information on SI benefit payments for all people who have been sick and entitled to such benefits, hence both employed and unemployed persons. For employed workers, however, sick spells shorter than or equal to the employers' responsibility period are not included in the data. For each sick spell, SFR records the start and end date, the income on which the benefits are based, and if benefits were given on a full or part-time basis. The SI benefits can be of a few different types: regular benefits for illness, compensation for work related injury, rehabilitation benefits, and benefits for preventive care. Regular SI benefits for illness are the most common, covering about 83 percent[16] of the sick spells starting in 2003.

Using ASTAT as the data source for unemployment spells means that the condition for being defined as unemployed is to receive funding from the UI. This implies that participants in labor market programs and people who are registered at the public employment office as unemployed but who are not qualified for UI benefits[17] are not included in our sample. The main reason for excluding these groups is that we neither have information on their benefits (if any) nor on their previous wage, which we need in order to know their SI compensation in case of sickness. Since data on the previous wage is lacking also for the unemployed who are only entitled to the basic-amount of UI benefits, we also exclude this group.

4.1 Sampling and descriptive statistics

We construct our sample by selecting all individuals who began an unemployment period with income-related UI benefits during the period 1 December 2002 – 30 June 2003 (i.e. up until the reform date). Unemployment spells beginning after the reform are left out in order to avoid changes in the sample composition caused by the reform.[18] The rationale for not sampling before December 2002 is that the wage information is incomplete before this point in time.[19] An unemployment period is considered to begin when a person who has not received UI benefits during the last 7 days, starts to receive benefits.

[16] About 89 percent if we also count those periods where regular benefits for illness were given together with one of the other benefit types.
[17] That is, people who have not fulfilled the *working condition* (see section 2.1).
[18] If the reform also affects the duration of SI spells, it may affect the composition of the unemployed population through its effect on the hazard rate from sickness back to unemployment. UI spells beginning after the reform are, however, included in parts of the sensitivity analysis when the individual is used as stratification unit (see section 5.3.2).
[19] Before this date, the wage variable is capped for individuals belonging to some of the UI funds.

Each unemployment spell beginning during the sampling period is followed until it ends, or at most, until the end of 2003. A transition to SI benefits or an interruption in the UI benefit payments for more than one week defines the end of an unemployment period. If a person who has transferred to the SI later returns to the UI system, a new unemployment period starts. For simplicity, we make no distinction between different types of SI benefits or between full and part-time sick leave. That is, we regard all SI periods the same. If a UI period ends for some other reason than sickness, e.g. because the person finds a job or starts a labor market program, the spell is treated as censored.

Our sampling procedure results in a sample of 10,845 individuals. For about 36 percent of them, the data includes multiple unemployment spells.[20] Table 1 and Table 2 below present some descriptive statistics. Table 1 gives statistics on the incidence and the duration of sick spells, separately for the treatment and the comparison group. We see that the sick report rate is lower among the treated (8.5 %) than the comparison persons (9.9 %). The sick spells are slightly shorter among the treated as well, whereas their UI spells are considerably longer. The latter could be due to a lower sick report rate, which implies fewer interruptions in unemployment and thus fewer but longer UI spells.

Table 1: Descriptive spell statistics

	Treatment group	Comparison group
No. of ind. with a UI spell	2,165	8,680
No. of ind. with an SI spell (%)	184 (8.5)	855 (9.9)
No. of UI spells	3,369	16,990
No. of transition to SI benefits (%)	228 (6.8)	1,012 (6.0)
Average spell length (days)		
UI benefits	54.6	35.7
SI benefits	51.3	53.6
No SI spells lasting:		
1 days	2	10
2-7 days	93	396
8-28 days	50	218
29-89 days	45	191
>90 days	38	197
No censored SI spells	30	143

Notes: The sample consists of all individuals in the LINDA-database who began an unemployment period with income-related UI benefits during 1 December 2002–30 June 2003.

[20] In the stratified analysis that also includes UI spells beginning after the reform (see section 5.3.2) there are multiple UI spells for about 49 percent of the individuals.

From Table 2 we can see that the individuals in the comparison group are, on average, younger, less educated, and have more young children compared to the treated. In general, they have fewer days left until their UI benefits expire in the beginning of the unemployment period. The proportion of women is also higher in the comparison group, as is the proportion of immigrants from non-OECD countries.

If we instead compare the sample of unemployed persons who report sick to the total sample of unemployed persons, we see that the proportion of women is larger among the sick, as is the average age and the proportion that is married. Also worth noting is that the sick individuals are closer to UI benefit expiration, compared to the total sample of unemployed persons.

Table 2: Descriptive covariate statistics (means)

	Sample of unemployed		Sample of sick	
	Treatment	Comparison	Treatment	Comparison
Female	0.27	0.63	0.38	0.68
Age	40.9	37.2	44.0	40.4
Education: High school	0.85	0.83	0.86	0.79
Education: Post high school	0.31	0.19	0.31	0.14
Immigrant: OECD	0.05	0.04	0.07	0.05
Immigrant: other	0.06	0.13	0.05	0.15
Married	0.47	0.46	0.53	0.52
Presence of children<18	0.35	0.44	0.34	0.50
Days left until UI benefit expiration (in the beginning of the UI spell)*	208.2	185.2	193.0	177.7
Average (previous) wage*	25,293	15,538	25,664	15,441
No. of individuals	2,165	8,680	184	855

Notes: The sample consists of all individuals in the LINDA-database who began an unemployment period with income-related UI benefits during 1 December 2002–30 June 2003. Statistics marked by * are averages among spells. The other statistics are averages among individuals.

In Figure 5 we show the (smoothed) weekly inflow to SI benefits for the treatment and comparison group. The inflow rate is here defined as the number of sick reports each week among the UI recipients, divided by the total number of UI recipients that week. We see that the sick report rate in the two groups exhibits a similar seasonal pattern in the pre-reform period, though the share reporting sick generally is higher for the comparison group. The flow to SI benefits decreases for both groups around the time of the reform. This pattern is consistent with a common finding in the Swedish literature on sickness insurance, namely that the share reporting sick generally declines during the summer (see e.g. Larsson 2006; Johansson and Palme 2005). After the summer, the sick report rate increases again for the comparison group, while it remains on a lower level for the treated. This pattern thus suggests that the reform may have been effective in decreasing sickness absence among the unemployed affected. However, these inflow rates do not account for any of the potentially important differences between the two groups, nor do they account for the lengths of the unemployment spells. Separating the reform effect from the effect of unemployment duration is a crucial part of our identification strategy.

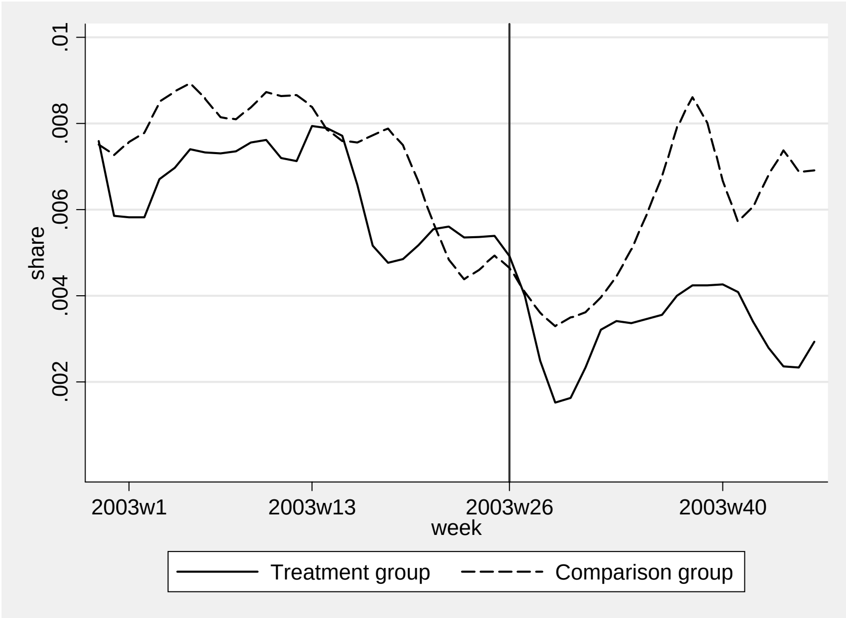

Figure 5: Weekly inflow to SI benefits among UI recipients before and after the reform, separately for the treatment and the comparison group

Note: The inflow rate is smoothed using local linear smoothing, bandwidth 0.15.

5 Empirical results

5.1 Incidence of sickness absence

The results for the Cox regression model are reported in Table 3, which consists of five different specifications estimated with partial maximum likelihood.[21] Let us begin with column (1) which presents the results for a model that only includes a dummy for the reform date, a dummy for the treatment group, and an interaction variable called the 'cap reform effect'. The latter captures the effect of the reduced SI benefit cap on the treated population

[21] Ties are handled using the exact method in SAS, see DeLong *et al* (1994), and Kalbfleisch and Prentice (1980).

and is thus the parameter of main interest. The estimate for the cap reform effect is statistically significant and quite strong; it suggests that the reform reduced the incidence of sick reports among the treated by 33.7 percent.[22]

Column (2)-(5) present results for some further specifications of the model in which we control for a number of covariates (which are discussed below). In essence, including covariates does not change the result concerning the cap reform effect; the coefficient estimate increases slightly and remains statistically significant in all specifications. The estimated parameter in the regression including all covariates (column 5) is -0.451, which suggests that the reduced SI benefit cap lowered the transition rate to sickness absence with about 36 percent in the treated population.

Among the other variables, we notice that the coefficient for the reform date dummy is negative and significant, hence indicating a general decrease in sickness absence among the unemployed around the time of the reform. This variable should partially be picking up the effect of the general 3 percent reduction in SI benefits but also the effect of other changes in the environment occurring around 1 July 2003. The parameter estimate for the reform date dummy decreases substantially when we control for the month of inflow to unemployment (column 4 and 5). We will return to this point in the sensitivity analysis (section 5.3.1), where we stratify on the week of inflow to unemployment.

We also note that the incidence of sickness absence is significantly lower during the first 100 days of unemployment than later in the UI period. Hence, the probability of reporting sick seems to increase as the UI expiration date approaches, which is in accordance with the findings of Larsson (2006). However, as the last three specifications reveal, the hazard does not seem to be monotonically increasing as the expiration date comes closer. It is highest right before the expiration date (the omitted category), but reaches another peak right after the 100 UI day limit has passed (the category 200-151 days until UI-exp.), i.e. at the time when the UI benefits are reduced relative to the SI benefits for many UI recipients.

Some of the demographic variables also obtain statistically significant parameter estimates. Being older is associated with a higher transition rate to sickness absence, and women have considerably higher transition rates than men – the difference being almost 45 percent. This large discrepancy has motivated us to also estimate the model separately for men and women. These estimations give in general less precise estimates (not reported), as should be expected, but the cap reform effect is still significant at the five percent level for the male population. The cap reform effect is, however, not found to be significantly different between men and women.[23]

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[22] The percentage effect is obtained by 100*(exp(β)-1), where β is the parameter of interest.

[23] The hypothesis of equal effects is tested by including an interaction term between the cap reform effect and the female dummy in the regression including both men and women. We

Among the other results presented in Table 3, we note that a post-high school education is associated with a significantly lower transition rate to sickness absence than is an education below the high school level. Moreover, the sick report rate appears to be significantly higher for those who have children living at home.

Table 3: Estimated effects on the incidence of sickness absence

	(1)	(2)	(3)	(4)	(5)
Cap reform effect (t) ($D_t^{July03} * D^T$)	-0.411** (0.185)	-0.433** (0.185)	-0.427** (0.185)	-0.438** (0.185)	-0.451** (0.186)
Post July 2003 (t) (D_t^{July03})	-0.419*** (0.088)	-0.441*** (0.088)	-0.442*** (0.088)	-0.201* (0.111)	-0.233** (0.110)
Previous wage >20,696 (D^T)	-0.208** (0.084)	-0.145* (0.084)	-0.129 (0.084)	-0.126 (0.084)	-0.018 (0.092)
Before 100 UI days limit (t) (=300-201 days until UI-exp.)		-0.524*** (0.064)			
No. of days until UI-expiration					
300-251 days (t)♣			-0.507*** (0.103)	-0.515*** (0.103)	-0.453*** (0.103)
250-201 days (t)♣			-0.355*** (0.105)	-0.356*** (0.105)	-0.320*** (0.106)
200-151 days (t)♣			-0.102 (0.108)	-0.103 (0.108)	-0.076 (0.108)
150-101 days (t)♣			-0.199** (0.095)	-0.198** (0.095)	-0.163* (0.095)
100-51 days (t)♣			-0.202** (0.099)	-0.199** (0.099)	-0.180* (0.100)
Month of entry into unemployment	No	No	No	Yes	Yes
Female					0.371*** (0.064)
Age					0.117*** (0.021)
Age^2					-0.001*** (0.000)
Immigrant: OECD					-0.043 (0.131)
Immigrant: other					-0.096 (0.093)
Education: High school					0.017 (0.076)

have also estimated models where the cap reform effect is interacted with dummies for different age categories. However, we find no evidence for heterogeneous effects among different age groups.

Table 3, cont.					
Education: Post high school					-0.266*** (0.081)
Married					-0.017 (0.068)
Presence of children <18					0.154** (0.075)
ln (previous wage)					-0.015 (0.045)
County dummies					Yes
-2 Log likelihood	17,453	17,383	17,365	17,340	17,163
No of observations	20,359	20,359	20,359	20,359	20,339

Notes: Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels, respectively. (t) denotes time-varying variable. ♣Reference is 50-1 days until UI-expiration.

5.2 Heterogeneous effects

The size of the decrease in SI benefits due to the reduced benefit cap depends on the person's pre-unemployment wage. Unemployed persons with a previous wage ranging between SEK 20,696–24,125 (hereafter referred to as T1) experienced a 3–16.8 percent cut in benefits, and those with a previous wage above SEK 24,125 (T2) experienced a benefit cut of 16.8 percent. We would thus expect the largest response to the reform to be found among those in the highest wage group (T2). In Table 4, column (2), we present results from a regression in which we have separated the treatment group (T) into these two groups. To facilitate comparison, column (1) reproduces the average effect, i.e. Table 3, column (5).

As expected, it is in the highest wage group (T2) that we find the largest responsiveness to the reform: the cap reform effect is statistically significant and suggests a 47 percent decrease in sick reports due to the reduced benefit ceiling. No statistically significant effect is found for the middle wage group (T1).

In column (3) we have instead divided T into two groups based upon the number of days left until the UI expiration date. More specifically, we have interacted the cap reform effect with an indicator of whether the person has between 200–300 UI days left (has *not* passed the 100 day-limit), or if he or she has less than 200 UI days left (has passed the 100 day-limit). Recall that passing the 100 day-limit implies a drop of the UI benefit cap by approximately 7 percent, as is shown in Figure 1. This created an even larger discrepancy between the SI and UI benefits before the reform and still creates a small difference in benefits after the reform for high wage unemployed persons. We see that the cap reform effect only appears significant for the 'after 100 days group', that is for the unemployed closest to UI expiration.

Finally, column (4) shows results when T is split up both along the wage and the expiration date dimension. As should be expected (given the results above), it is among the unemployed in the highest wage group and with relatively few UI days remaining that the responsiveness to the cap reform seems strongest. This is the group which before the reform had the largest incentives to report sick – their SI benefits were substantially higher than their UI benefits and they had a relatively short period left before UI benefit expiration.

Table 4: Interacting the cap reform effect with previous wage and duration until UI benefit expiration

	(1)	(2)	(3)	(4)
Cap reform effect $(D_t^{July03} * D^T)$ (t)	-0.451**			
	(0.186)			
Cap reform effect*Middle wage group $(D_t^{July03} * D^{T1})$ (t)		-0.316		
		(0.227)		
Cap reform effect*High wage group $(D_t^{July03} * D^{T2})$ (t)		-0.643**		
		(0.288)		
Cap reform*Before 100 days-limit $(D_t^{July03} * D^T * \textit{Before 100 days})$ (t)			-0.337	
			(0.339)	
Cap reform*After 100 days-limit $(D_t^{July03} * D^T * \textit{After 100 days})$ (t)			-0.484**	
			(0.205)	
Cap ref*Bef 100d-limit*Middle wage $(D_t^{July03} * D^{T1} * \textit{Before 100 days})$ (t)				-0.011
				(0.402)
Cap ref*After 100d-limit*Middle wage $(D_t^{July03} * D^{T1} * \textit{After 100 days})$ (t)				-0.405
				(0.255)
Cap ref*Bef 100d-limit*High wage $(D_t^{July03} * D^{T2} * \textit{Before 100 days})$ (t)				-0.836
				(0.598)
Cap ref*After 100d-limit*High wage $(D_t^{July03} * D^{T2} * \textit{After 100 days})$ (t)				-0.594*
				(0.313)
Post July 2003 effect (D_t^{July03}) (t)	-0.233**	-0.234**	-0.236**	-0.236**
	(0.110)	(0.110)	(0.111)	(0.111)
Previous wage>20,696 (D^T)	-0.018		-0.018	
	(0.092)		(0.092)	
Middle wage (20,696-24,125) (D^{T1})		-0.017		-0.016
		(0.108)		(0.108)
High wage (>24,125) (D^{T2})		-0.027		-0.026
		(0.135)		(0.136)
Six categories for # days until UI benefit expiration (t)	Yes	Yes	Yes	Yes
Month of entry into unemployment	Yes	Yes	Yes	Yes
All other covariates included	Yes	Yes	Yes	Yes
-2 Log likelihood	17,163	17,162	17,163	17,161
No of observations	20,339	20,339	20,339	20,339

Notes: Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels, respectively. (t) denotes time-varying variable.

5.3 Sensitivity analysis

5.3.1 Effects of the time of inflow into unemployment

The effect of the reduced benefit cap is partly identified by comparing the evolution of hazard rates into sickness for people who experienced the reform at different lengths of unemployment. This means most importantly that we compare people based upon when they became unemployed. If there is (health) heterogeneity among the unemployed with respect to the time of entry into unemployment this could potentially affect our results. In particular, it may matter if such heterogeneity differs over time in divergent ways for the treatment and the comparison group.

In order to check whether this type of heterogeneity affects our findings, we perform a stratified analysis, using the week of entry into unemployment as the stratification unit. This means that the baseline hazard is allowed to differ across weeks of entry. The reform effect is still identified by the variation in unemployment duration until the reform occurs. However, now variation within a given entry-week is used for each stratum. This model is estimated with a stratified partial maximum likelihood estimator (see e.g. van den Berg 2001, section 6). The results from this analysis are presented in Table 5, column (2). In column (3) we show results when the week of inflow into unemployment *and* the local labor market (county) are used as stratification units. This allows there to be heterogeneity with respect to inflow week that differs between different local labor markets. The estimate for the 'cap reform effect' is very similar to that obtained earlier (shown in column 1) for both regressions. Hence, heterogeneity with respect to the time of inflow into unemployment does not seem to distort our findings.

Table 5: Estimated effects on the incidence of sickness absence, using the week of inflow into unemployment and the local labor market as stratification units

	(1) Main results	(2) Stratification by week of inflow	(3) Stratification by week of inflow *and* local labor market
Cap reform effect (t) $(D_t^{July03} * D^T)$	-0.451**	-0.497***	-0.502**
	(0.186)	(0.187)	(0.203)
Post July 2003 (t) (D_t^{July03})	-0.233**	-0.807***	-0.966***
	(0.110)	(0.252)	(0.263)
Previous wage >20,696 (D^T)	-0.018	-0.011	-0.014
	(0.092)	(0.092)	(0.097)
Six categories for # days until UI benefit expiration (t)	Yes	Yes	Yes
Month of entry into unemployment	Yes	No	No
All other covariates included	Yes	Yes	Yes
Stratification by week of inflow	No	Yes	Yes
Stratification by local labor market	No	No	Yes
-2 Log likelihood	17,163	12,700	6,578
No of observations	20,339	20,339	20,339
No of strata	-	31	673

Notes: Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels, respectively. (t) denotes time-varying variable.

5.3.2 Effects of unobserved individual heterogeneity

There may of course be individual heterogeneity due to other factors than time of entry into unemployment and local labor market. One example relates to the outflow to employment. If people who find jobs on average have better (or worse) health than those who remain unemployed, this will cause the composition of our sample regarding health status to change over time. Divergent labor market opportunities for the treated and the comparisons may then imply different compositional changes regarding health status in the two groups, which in turn could lead to bias in our estimates.

In order to improve on this part, we use the fact that we have multiple unemployment spells for about half (49 percent) of the individuals in our sample (if we also sample unemployment spells beginning after the reform) and estimate the model only using within individual variation. Hence, we reestimate the model using the stratified partial maximum likelihood estimator, but this time we use the individual as the stratification unit.[24] The estimates produced by this approach are robust with respect to individual heterogene-

[24] A similar approach has been used in previous empirical studies; see e.g. Johansson and Palme (2004) and Lindeboom and Kerkhofs (2002), though the latter uses the workplace rather than the individual as a stratification unit.

ity that is persistent over time.[25] The obvious drawback with this method is that we now identify the cap reform effect using only a selected sample of the unemployed.[26]

The results from this robustness check are presented in Table 6, column (2). We see that the cap reform effect is similar in size to the effect obtained earlier.[27] The estimate is somewhat less precise, but still significantly different from zero at the five percent level. Hence, our previous results seem robust with respect to this type of compositional change.

Table 6: Estimated effects on the incidence of sickness absence, using the individual as a stratification unit

	(1) Main results	(2) Stratification by individual
Cap reform effect (t) $(D_t^{July03} * D^T)$	-0.451**	-0.496**
	(0.186)	(0.245)
Post July 2003 (t) (D_t^{July03})	-0.233**	-0.336*
	(0.110)	(0.173)
Previous wage >20,696 (D^T)	-0.018	0.920
	(0.092)	(0.584)
Six categories for # days until UI benefit expiration (t)	Yes	Yes
Month of entry into unemployment	Yes	Yes
All other covariates included	Yes	-
-2 Log likelihood	17,163	2,159
No of observations	20,339	35,044
No of strata	-	14,525

Notes: Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels, respectively. (t) denotes time-varying variable.

[25] Unobserved heterogeneity could also lead to underestimated standard errors as it may introduce dependence between spells belonging to the same individual. This approach also corrects for this potential problem.

[26] This method needs at least two UI spells for each individual to identify the reform effect, out of which at least one exists before the reform and one after. Moreover, in order for a censored spell to contribute to the likelihood it must be longer than an uncensored spell for the same individual. See e.g. Ridder and Tunali (1999) for a thorough explanation of stratified partial likelihood estimation.

[27] As before, the baseline model (column 1) is estimated only including spells beginning before the reform. If we also include spells beginning after the reform, the estimate for the cap reform effect is somewhat smaller in size: -0.346 (0.129). The difference is probably a consequence of that the reform also affected the hazard rate from SI back to UI. Reduced SI benefits imply that we can expect sickness periods to be shorter (on average) after the reform, compared to what they would have been if the benefits had remained unchanged. Thus, people may return to UI while still sick. This means that the average health status among the UI recipients most likely is worse after the reform. Worse health on average in the UI recipient population would naturally imply a higher transition rate to SI. Hence, the cap reform effect is likely to be downward biased if we also include UI spells beginning after the reform in the baseline model. The within-individual estimates in this section should not suffer from this bias.

5.3.3 Pre-treatment effects

The reduction of the SI benefit ceiling was announced already in April 2003, soon after a debate had arisen on the harmonization of the SI and UI system. Hence, it is possible that there would be a change of behavior regarding sickness absence among the high-wage unemployed during the months prior to the reform. In order to examine the existence of such 'pre-treatment' effects, we have re-estimated the model instead including variables for a hypothetical reform in the beginning of June 2003, as well as in the beginning of May, April and March of the same year. Table 7 presents the results from this analysis.

We see that the point estimate for the 'cap reform effect' during the pre-reform period is only 22–46 percent of the point estimate for the actual reform effect and is never significantly different from zero. We conclude that there is no evidence of anticipatory behavior during the months preceding the reform.

Table 7: Pre-treatment effects

	Reform:	Hypothetical reforms:			
	July, 1st	June, 1st	May, 1st	April, 1st	March, 1st
Cap reform effect	-0.451**	-0.209	-0.145	-0.101	-0.141
$(D_t^{Reform} * D^T)$ (t)	(0.186)	(0.160)	(0.151)	(0.152)	(0.164)
Post reform effect	-0.233**	-0.403***	-0.477***	-0.286***	-0.105
(D_t^{Reform}) (t)	(0.110)	(0.112)	(0.108)	(0.106)	(0.107)
Previous wage	-0.018	-0.052	-0.061	-0.063	-0.020
>20,696 (D^T)	(0.092)	(0.098)	(0.105)	(0.122)	(0.142)
Six cat. for # days until UI exp. (t)	Yes	Yes	Yes	Yes	Yes
Month of entry into unemployment	Yes	Yes	Yes	Yes	Yes
All other covariates incl.	Yes	Yes	Yes	Yes	Yes
-2 Log likelihood	17,163	17,159	17,153	17,168	17,175
No of obs.	20,339	20,339	20,339	20,339	20,339

Notes: Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels, respectively. (t) denotes time-varying variable.

5.4 Duration of sickness absence

Reduced economic compensation in case of sickness could be expected to affect not only the probability of reporting sick, but also the duration of sickness absence. Given that an individual transfers to the SI system, it seems reasonable to expect his or her sickness period to be shorter (on average) after the reform compared to what it would have been if the compensation size had remained unchanged.

However, given that the reform had a strong effect on the incidence of sickness absence, we would not necessarily expect the average sick spell length to decrease among observed sickness spells. If the reduction of the incidence is (mainly) due to reduced moral hazard, we would expect the average health of the treated population on SI benefits to be worse after the reform than before. In other words, the threshold for a few days' sick period due to minor illness is higher after the reform, thereby increasing the average length of SI periods among the population on sick leave.

When estimating the effect of the reform on the hazard rate out of sickness, we get no significant estimates (not reported), neither for the reform date nor for the reduced SI benefit cap.[28] Hence, the two counteracting effects seem to balance each other out, leaving the average duration of sick spells unchanged.

6 Discussion

Our results suggest a strong behavioral response to changes in sickness compensation among the unemployed. Using a reform within the sickness insurance that only affected some of the population – the treated – we estimate that the incidence of sick reports was reduced by about 36 percent more in this group compared to the comparison group that was not affected by the reform.

Up to this point we have made inference regarding the total behavioral response to the cap reform, without relating it to the magnitude of benefit reduction. In order to say something about the *economic* or *policy* significance of our estimates, we use the estimates from section 5.1 to calculate the elasticity of the sick report rate with respect to SI benefits. Furthermore, this makes it possible to compare our results with results from previous studies on sickness benefits.

The elasticity measure used is given by:

$$\hat{e} = (1 - \exp(\hat{\beta}) / \Delta) \qquad (2)$$

where Δ is the percentage decrease in (potential) benefits due to the reduced SI benefit cap (i.e. the reduction on top of the general 3 percent reduction due to the reduced replacement rate). The decrease in benefits is computed for each individual based on the factual difference between his or her old and

[28] To do this we have created an additional dataset by following the sub-sample of unemployment spells that has ended in sickness, until they end, or at most, until the end of 2003. The effect of the reform on the duration is then estimated using model (1), as specified in section 3.

new SI benefits. On average, SI benefits were reduced by 9.3 percent in our sample. The estimated elasticity is therefore 3.9.

Previous studies that estimate the effect of economic compensation on absence incidence among employed workers report lower elasticities: just below 1 in Johansson and Palme (2005) and 1.72 or 2.45 in Pettersson-Lidbom and Skogman Thoursie (2006), depending on whether monthy or weekly data is used.[29] These estimates can be interpreted within the traditional labor supply framework as elasticities between leisure and consumption. Whether our estimate can be compared with them thus depends on whether unemployment in this context is assumed to be leisure or work, which in turn is not clear. However, it seems plausible that unemployed persons are more sensitive to changes in the SI compensation size than employed persons.

Our estimate is high even if compared to previous results concerning unemployed workers. Larsson (2004) and (2006) use data from the late 1990s and report an elasticity of around 1–1.5.[30] Whether this difference is due to a different time period or a different identification strategy is difficult to say.

One aspect that might affect our elasticity estimate concerns supplementary compensation for sickness and unemployment. The most common type of such benefits is insurance schemes regulated by collective agreements between unions and employers' organizations. These agreements vary across sectors and in some sectors even across firms. In general, they contain supplementary benefits above the cap for high-wage workers, implying that the denominator – the percentage decrease in benefits due to the reform – is overestimated and the elasticity underestimated. How much the elasticity is biased is difficult to estimate as we do not have data on which scheme the individual is covered by, and due to variation across schemes. For the identification of the cap reform effect however, the supplementary schemes do not pose any problem. Even though they imply that some high-wage unemployed receive considerably higher total benefits from the SI than from the UI even after the reform, they do not change the fact that the reform affected the treatment group and the comparison group differently. There was no concurrent change in the rules for these supplementary schemes that would potentially bias our estimate of the reform effect.

To conclude, our results show evidence of moral hazard in the Swedish sickness insurance system. In fact, the moral hazard revealed by our study may be of two kinds: First, assume that the drop in SI benefits made (rela-

[29] The Swedish SI system does not have any clear public counterpart in North America. The most similar counterpart is the short-term disability benefits offered by the workers' compensation (WC) programs. Krueger and Meyer (2002) survey the evidence of effects of WC benefits on the incidence of claims and find that the estimated elasticities cluster around 0.2-0.3 in studies using aggregate data. The few studies using individual data find considerably larger effects.

[30] The papers do not include an estimate of the elasticity but the results can be used to calculate it.

tively) healthy unemployed persons refrain from reporting sick. In that case, our results suggest that the reform led to *decreased* moral hazard within the sickness insurance.

Second, it could of course be the case that the drop in compensation made some truly sick persons refrain from reporting sick when this no longer was economically advantageous. Without access to health information we cannot determine this with certainty. If this is the case, our results actually suggest that the reform led to *increased* moral hazard within the unemployment insurance system. Active job search is a formal requirement for UI eligibility, and unemployed persons who are too sick to apply for jobs should receive benefits from the SI instead. The difficulty of determining whether the reform actually led to decreased moral hazard illustrates the importance of taking the whole social insurance system into account when designing reforms.

Economic incentives seem to be important for the use of sickness insurance among the unemployed. This in turn raises the question of whether interactions between the SI and the UI also matter for the job finding rate; does being financed by the UI rather than the SI matter for transitions to employment? Investigating the effects of interactions between the UI and the SI on the job finding rate is thus an interesting topic for further research.

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Essay 4: Do interactions between unemployment insurance and sickness insurance affect transitions to employment?[♣]

1 Introduction

In recent years, several academics as well as policy makers have pointed out that undesired incentive effects arising from the interplay between different social insurance programs within a welfare state may be an overlooked and financially costly phenomenon (see e.g. Krueger and Meyer 2002, and the European Economic Advisory Group 2007). Many countries have complex social insurance systems and their various parts sometimes overlap in ways that can generate unintended flows between them. This has, for instance, been noted with regard to unemployment (UI) and sickness insurance (SI) in Sweden and Norway (Larsson 2006, and Henningsen 2007), UI and disability pensions in Sweden and Finland (Karlström et al 2008, and OECD 2006), and UI and workers' compensations programs in Canada (Fortin and Lanoie 1992). In the presence of such interactions, limiting access to one program may just result in an overflow to other programs. Reforms intended to increase transitions to employment by a change of a single program may then not be very effective. For example, reducing the amount or the duration of UI benefits may not be an efficient way of inducing the unemployed to search harder for jobs if they can easily shift to other benefit programs.

While there is evidence that the interplay between different social insurance programs sometimes does give rise to benefit arbitrage (see e.g. Larsson 2006, and Hall and Hartman 2009), little research has been done on whether such interactions actually matter for transitions to employment. Pellizzari (2006), who studies interactions between UI and social assistance in 15 EU countries, is an exception. His findings suggest that UI recipients who are also eligible for social assistance are less sensitive to changes in the level and the duration of their UI benefits, and that the interplay between

[♣] I am grateful to Peter Fredriksson, Erik Grönqvist, Laura Hartman, Pathric Hägglund and Per Johansson for valuable comments and discussions. I would also like to thank Linus Liljeberg for helpful suggestions regarding the data work. The paper has benefited from comments at seminars at Uppsala University/IFAU, University College London and the EALE conference 2008. The financial support from the Wallander and Hedelius Foundation, and the Swedish Council for Working Life and Social Research (FAS) is gratefully acknowledged.

these programs may provide an explanation for the scant success of many labor market reforms in Europe in the past decades. In this paper, I provide Swedish evidence on the interplay between UI and another social insurance program, namely the SI, and on whether this interplay affects transitions to employment.

In Sweden, unemployed individuals are able to report sick and receive SI benefits. The rationale behind this rule is that job search is comparable to work. In order to be eligible for UI, an unemployed person should actively search for jobs and be able to accept employment at short notice. A person who looses his or her work (search) capacity due to sickness should therefore be funded by the SI rather than the UI. Previous research on the interplay between these two insurances, however, suggests that health deterioration is not the sole explanatory factor for transitions to the SI. The probability of transferring to the SI is affected by the relative compensation in the two systems; unemployed persons are more likely to report sick if their replacement rate is higher in the SI than in the UI (Hall and Hartman 2009). The probability of reporting sick is also found to increase drastically as the UI expiration date approaches, suggesting that the SI may be used as a way of postponing the UI expiration date (though it cannot be excluded that the peak is at least partly driven by health deterioration due to stress) (Larsson 2006, and SFU 2007).

If transitions to employment would follow a similar trajectory regardless of shifting to the SI or remaining in the UI system, this type of interplay should perhaps not cause too much concern. Of course, government spending increases if the SI benefits are higher than the individual's alternative benefits, but the costs would be much larger if these UI-SI interactions in general also worked to prolong the individuals' time out of employment.

There are in fact several reasons for why the source of funding (UI or SI) may matter for the incentives to find work. Being on UI is associated with a number of rules, the purpose of which is to increase transitions to employment: the worker is obliged to apply for and accept jobs, otherwise a sanction may be imposed[1]; benefits are reduced after 100 benefit days[2]; and there is a formal time limit on how long benefits can be received[3]. SI benefits, on

[1] Some recent empirical studies from the Netherlands and Switzerland suggest that imposing sanctions in the UI substantially raises the exit rate from unemployment, see e.g. Abbring et al (2005) and Lalive et al (2005).

[2] The question of how the UI benefit level affects job finding has received extensive attention in the economic literature. A survey of the evidence is provided by Krueger and Meyer (2002). The US studies surveyed imply an elasticity of unemployment duration with respect to the UI benefit level in excess of 0.5. The results from other countries are more varied. Carling et al (2001) suggest an elasticity of 1.6 for Sweden.

[3] Several empirical studies find that the transition rate from unemployment to employment increases as the UI expiration date approaches. See e.g. Moffitt (1985), Meyer (1990), and Katz and Meyer (1990) for evidence from the US. Swedish evidence is reported by Carling et al (1996).

the other hand, are not associated with any similar requirements and have in principle unlimited duration.[4] Hence, if the UI rules work as intended, funding from the UI rather than the SI could (for a given health status) be expected to be associated with a higher search effort.

In order to study whether transitions to SI among the unemployed affect the transition rate to employment, I use a reform in July 2003 which changed the relative compensation in the two systems. The reform reduced the SI benefit cap (i.e. the maximum amount) so as to correspond to the UI benefit cap, in order to prevent unemployed workers from receiving higher benefits by reporting sick. Before the reform, SI benefits could exceed UI benefits by up to 20 percent. Hall and Hartman (2009) find that this policy change led to a large (36 percent) decline in the sick report rate among the unemployed affected by the reform. The question of interest here is whether the reduced sick report rate in turn translated into a higher rate of job finding.

To identify the effect of the reduced SI benefits (relative to the UI benefits) on the transition rate to employment, I use the fact that the reform affected various groups of unemployed persons differently and at different durations of unemployment. First, as workers became unemployed at different dates, the reform affected them at different lengths into their unemployment period. This variation can be used to separate the reform effect from the effect of unemployment duration. I do this by comparing the hazard to employment for people who experienced the reform at different stages of their unemployment period. Second, only those with previous wages above the UI benefit cap were affected by the reform. The change in transitions to employment for those with previously lower wages can thus be used to control for calendar time effects (such as business cycle effects) around the time of the reform, which were common to the two groups.[5]

My results suggest that, while the reform significantly reduced sickness absence among the unemployed, this did not matter for the transition rate to employment. For the group who reduced its sick report rate due to the reform, spending more time in the UI rather than the SI did not seem to shorten the time out of employment. This finding is robust across various sensitivity tests. Hence, while there are important interactions between these two social insurance systems, I find no evidence suggesting that these interactions affect the job finding rate among the unemployed workers.

The outline of the rest of the paper is as follows: In Section 2 I describe the central features of Sweden's UI and SI systems, as well as the SI reform in July 2003. Section 3 discusses theoretical issues. The empirical strategy is described in Section 4, and Section 5 presents the data. The results as well as a number of sensitivity checks are shown in Section 6. Finally, Section 7 contains concluding remarks.

[4] These were the rules in place during the time period for which I have data in this paper.

[5] Hall and Hartman (2009) use a similar identification strategy.

2 Unemployment and sickness insurance in Sweden[6]

The UI and SI constitute essential parts of the public social insurance system in Sweden. Their purpose is to insure against income losses due to involuntary unemployment (the UI) and sickness (the SI). Both insurances provide income-related compensation up to a cap and the benefits are for the most part financed by taxes.

2.1 Description of the unemployment insurance

The UI consists of two parts: a *basic insurance* offering a fixed amount of compensation and a voluntary *income-loss insurance* providing income-related benefits. In order to be eligible for any kind of UI benefits, an unemployed person must be registered at the public employment service (PES) as a 'job seeker' and be willing to accept employment. Qualification for income-related benefits additionally requires that the person has been a member of a UI fund for at least twelve months prior to unemployment (the membership condition) and that he or she has worked during at least six of these months (the working condition). If the person fulfills the working condition but not the membership condition, he or she is entitled to the fixed basic amount of compensation.[7]

The UI is administered by a number UI funds that together enroll about 85 percent of the work force. The PES controls that the unemployed fulfill the rules concerning job search. To receive UI benefits, an unemployed person has to meet his or her caseworker at the PES regularly and apply for any job the caseworker assigns him or her. If the person does not meet these requirements, he or she can be submitted to a sanction in the form of reduced or suspended benefits. The sanctions can be time-limited or permanent, depending on if the person has broken the rules before, and on the expected length of the job he or she refuses to accept.[8]

In 2003, when the reform was implemented, the UI benefits were time-limited to 300 workdays (60 weeks) and could be received either continuously or with breaks in the unemployment period. For individuals reaching the end of their benefit period, a PES caseworker would assess the need for intensified counseling. If such a need was found, the person would get assigned to a specific labor market program[9]. Refusing to participate would lead to benefit expiration. If intensified counseling was not found necessary, the unem-

[6] This section describes the rules in place during 2003.
[7] For a detailed description of the UI see e.g. www.aea.se.
[8] An unemployed person does not necessarily have to accept *any* job in order to receive further UI benefits. Factors such as the family situation and the duration of unemployment are taken into account in the judgement.
[9] The program was called *Activity Guarantee* and implied full time activity. Participants were offered counselling and the whole spectrum of programs and services available at the PES. The economic compensation equaled the UI benefits.

ployed would get entitled to a new benefit period of 300 days. Such an extension was however only possible once.

The UI replaced 80 percent of the worker's average earnings during the last six month of employment, with a lower and an upper limit. See Figure 1 for an illustration. The lower limit was SEK 7,040 (≈ € 760)[10] per month and equaled the fixed basic amount. The maximum amount varied depending on how long the person had been unemployed. During the first 100 benefit days, the cap was 80 percent of a monthly wage of SEK 20,075 and after that the cap dropped to 80 percent of SEK 18,700. No compensation was given during the first five days of an unemployment period.[11]

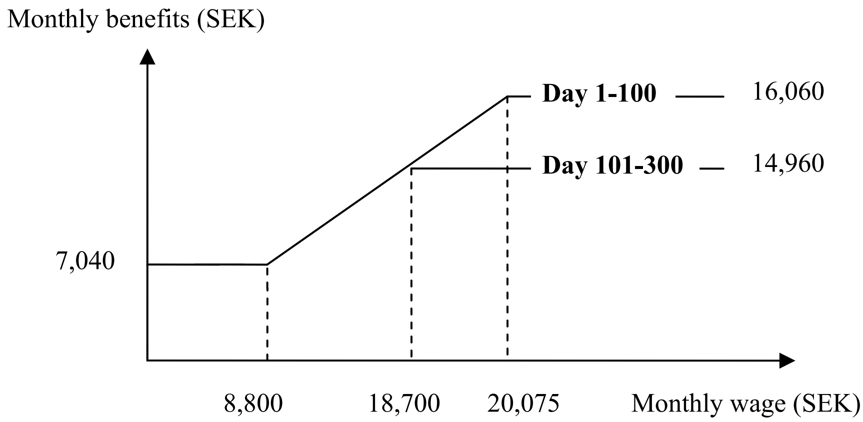

Figure 1: UI benefits in 2003

2.2 Description of the sickness insurance

The SI provides economic compensation when a worker is too sick to carry out his or her regular job. All employed workers are automatically covered by the insurance. Unemployed workers who have previously been employed are also eligible, as long as they are registered as 'job seekers' at the PES. The size of the SI benefits depends on the person's wage prior to the sick period. For unemployed workers however, the benefits are based on the wage prior to unemployment.

The Social Insurance Agency is responsible for the SI compensation for unemployed workers. The first day of a sickness period is always uncompensated. During the first seven days it is up to the individual to judge whe-

[10] Exchange rate May, 2007.

[11] If the unemployment was voluntary, that is if the person had quit his or her job without a valid reason, the uncompensated period was up to 45 benefit days. For those who had been laid off because of improper behaviour, the suspension period could be even longer.

ther he or she is too sick to work (search). Thereafter, the person needs a certificate from a doctor in order to receive additional benefits. In 2003, there was no formal time-limit for how long SI benefits could be received.

In the beginning of 2003, the SI replacement rate was 80 percent of the previous (pre-unemployment) wage. Hence, the replacement rate was the same as in the income-related UI.[12] The minimum wage for receiving any SI benefits at all was SEK 775 per month, and the maximum SEK 24,125 per month. In other words, SI benefits varied between SEK 620 and SEK 19,300 per month.[13] This meant that the maximum monthly SI benefits exceeded the maximum monthly UI benefits. The reform on 1 July 2003 changed the marginal replacement rate in the SI in two ways. Figure 2 illustrates how the changes affected unemployed workers. First, the reform reduced the marginal replacement rate to 77.6 percent. This change concerned all insured, employed as well as unemployed. Second, for the unemployed insured, the maximum SI benefits were reduced to SEK 16,060 per month, which corresponded to the maximum monthly UI benefits. The purpose of the latter part of the reform was to prevent unemployed persons from receiving higher benefits by reporting sick.

[12] The two insurance systems however define the earnings on which the benefits are based somewhat differently. While the UI benefits are based on the worker's average earnings during the last six months of employment, the SI benefits are based on an estimate of the earnings a worker would have had during the sickness period.
[13] The numbers in this section do not account for the first uncompensated day in a sickness period.

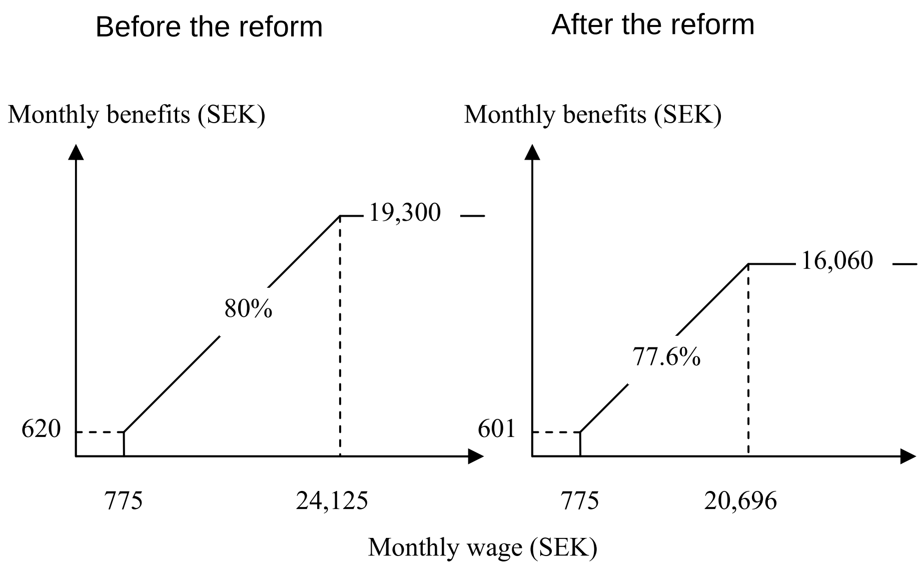

Figure 2: SI benefits for unemployed workers, before and after the reform in July, 2003

An additional aspect of the second part of the reform, which is important for this study, is that it affected *all* unemployed insured, i.e. even those with already ongoing UI spells had their SI benefits reduced on 1 July 2003. This feature turns out to be important for the identification strategy (see Section 4).

3 Theoretical issues

Sickness absence and unemployment, though two states both representing substantial losses of work time, are typically not analyzed jointly. Sickness absence has most commonly been analyzed within the framework of a labor supply model, and the focus has generally been exclusively on employed workers (see Brown and Sessions 1996 for a survey of the work absence literature). Holmlund (2005) develops a theoretical framework that incorporates both unemployment and sickness absence as distinct labor force states. Moreover, sickness absence may occur both among employed and among unemployed workers. This model thus allows for interdependencies between policies concerning unemployment and sickness absence and is suitable for a unified analysis of labor market effects of changes in sickness and unemployment benefits.[14]

The Holmlund (2005) model includes four different labor force states: *work*, *sickness absence*, *unemployment* and *non-participation*. Individuals in the first two states are employed, whereas those in the second two states are non-employed. Sickness absence among unemployed workers is regarded as non-participation. Contrary to the state of unemployment, individuals in this state do not search for jobs actively, and hence the probability of finding employment is lower (though it is still positive since the individuals may be contacted by an employer).

Each of the four labor force states is associated with a particular present discounted value of utility. This value depends on the income in the current state as well as on incomes in all other potential states, since chance and choice induce the (homogenous) workers to move between states. Employed workers face a risk of job loss and non-employed workers face a chance of finding a job. Workers are also exposed to random (daily) shocks of sickness, which affect their disutility of work and job search. The key decision for employed individuals is to choose between work and sickness absence, and for the non-employed, to choose between search and inactivity, i.e. between unemployment and non-participation.

The optimal behavior is characterized by *reservation values of sickness*. The employed prefer sickness absence rather than work for sufficiently severe sickness shocks; and similarly, the non-employed prefer non-participation rather than job search for sufficiently serious realizations of sickness. The critical values of sickness generally differ between employed and non-employed workers, and are determined by benefits and other parameters of the model. For example, the reservation value of sickness is higher, the higher is the relative economic gain of being active rather than inactive. Hence, for non-employed workers, the probability of reporting sick is lower, the lower are SI benefits relative to UI benefits. The reservation level of sickness also depends on differences in transition probabilities; e.g., the higher the relative returns to active job search, the lower the probability that non-employed workers report sick.

A decrease in SI benefits targeting only non-employed workers, such as the one in Sweden in July 2003, has straightforward implications in this framework. First, reduced SI benefits for non-employed individuals will have a direct positive effect on their reservation level of sickness, making non-employed individuals less inclined to report sick. There will also be a wealth effect working in the same direction since the value of non-employment decreases relative to the value of employment, which makes

[14] This model ignores the behavior of firms and focuses on the supply side. See Engström and Holmlund (2007) for an extension of the model that also incorporates firm behavior.

active job search more attractive.[15] Second, since the probability of finding a job is higher in unemployment than in non-participation by assumption, the higher reservation level of sickness will also translate into a higher job finding rate among the non-employed workers.

4 Identification strategy

The question of interest in this paper is whether the reform in July 2003 affected the transition rate to employment, through its effect on the sick report rate. To identify the effect of the reduced SI benefits (relative to the UI benefits), I exploit two features of the reform: (i) As workers became unemployed at different dates, the reform affected them at different durations of unemployment. By exploiting this variation, I can separate the reform effect from the effect of unemployment duration. (ii) Only those with previous wages above the UI benefit cap were affected by the reform. The change in transitions to employment for those with previously lower wages can thus be used to control for calendar time effects (such as business cycle effects) around the time of the reform, which were common to the two groups.

I begin by describing more closely how the reform affected the difference between UI and SI benefits for various types of unemployed persons.[16] Recall that the difference depended on: (i) the wage prior to unemployment, and (ii) whether the person had received UI benefits for less or more than 100 days. Figure 3 shows the case of an unemployed person who has not yet passed the 100-day limit in the UI, i.e. before the UI benefit cap drops.

[15] If the risk of job loss is higher for workers on sick leave than for those at work, the reservation level of sickness will also increase for employed workers, since the incentives to prevent a job loss by attending work increases.
[16] The following paragraphs in this section build extensively on the description in Section 3 in Hall and Hartman (2009).

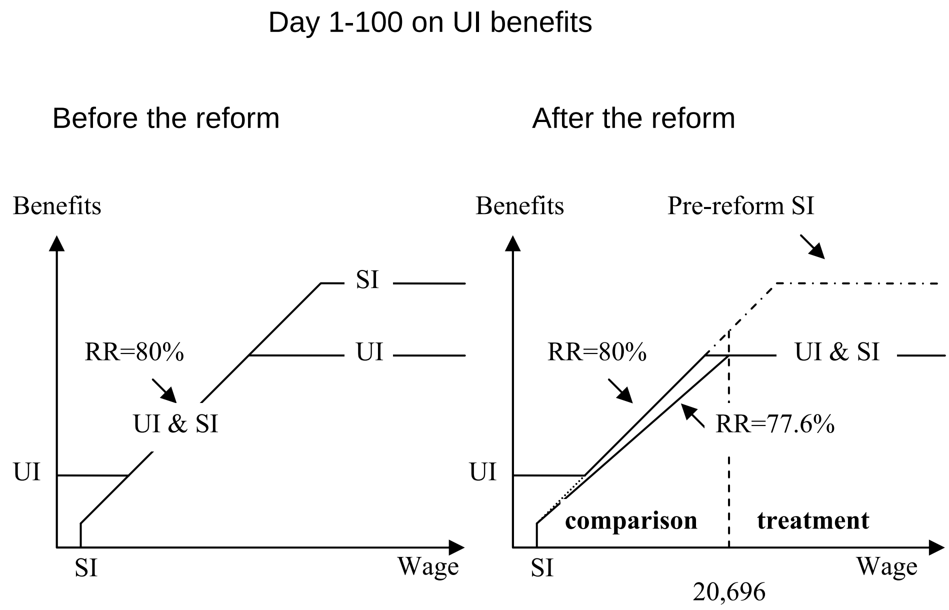

Figure 3: The change in SI benefits due to the reform, during the first 100 UI benefit days

The reform lowered the SI benefits for everybody, as the marginal replacement rate was reduced from 80 to 77.6 percent. Thus, SI benefits were reduced relative to UI benefits for all unemployed persons. However, up to the previous wage of SEK 20,696 the reduction in SI benefits was relatively small; amounting to only 3 percent.[17] I will refer to this group as the comparison group. For unemployed persons with a previous wage above that cut-off, the treated, the reform implied a reduction of the SI benefits that varied from 3 up to almost 17 percent.

The situation is somewhat different for the unemployed who have passed the first 100 UI benefit days, as the cap in the UI then is lower. This implies that even after the reform, SI benefits are higher than UI benefits for high-wage unemployed persons. However, the effect of the reform on the *difference in SI benefits* is similar to Figure 3: up to a previous wage of SEK 20,696 the SI benefits were reduced by 3 percent. From that level upwards, the reduction was larger the higher the previous wage, varying between 3 and almost 17 percent. So again, the population can be divided into treated

[17] Persons with very low previous earnings are an exception, as the reform also implied a marginal reduction of the minimum wage for SI eligibility; from SEK 620 to SEK 601. Hence, persons in this income group became eligible for SI and thus experienced a benefit increase. However, there are no observations in this income interval in the sample studied in this paper.

and comparisons according to the previous wage, the cut-off being at SEK 20,696.

I will analyze the effect of the change in SI compensation on the hazard rate to employment, that is, the rate at which a person finds a job at time t of unemployment, conditional on remaining unemployed up until this point in time. In addition to making use of the treatment and comparison group, the identification strategy used exploits the timing of the reform. The timing feature arises when one uses duration data and has a fixed reform date. As workers become unemployed at different dates, the reform affects them at different durations of unemployment. This variation can be used to separate the reform effect from the effect of unemployment duration. I do this by comparing the hazard to employment for people who experienced the reform at different stages of their unemployment period. For example, the unemployed who experienced the reform 8 weeks into their unemployment spell are compared with those whose unemployment spells are at least 8 weeks, but who did not experience the reform until after week 8 or never.

This strategy makes it possible to identify the effect of the reform *date*. However, it is likely that other changes on the labor market occurred around the time of the reform which also affected transitions to employment. In order to separate the effect of the reduced SI benefits from such factors, I compare the reform-date effect for the treatment and the comparison group. A larger effect for the treated, who experienced a larger benefit cut, will indicate responsiveness to the SI compensation size. The policy change which is used to identify the behavioral response to the SI benefit level is thus not the entire reduction in SI benefits due to the reform, but rather the reduction over and above the general 3 percent reduction in the replacement rate. The effect of the 3 percent reduction cannot be separated from the effects of other changes around the time of the reform.

To estimate the effect of the policy change, I use a Cox regression model. The baseline specification to be estimated can be written as:

$$\lambda(t) = \lambda_0(t)\exp\left\{f\left(\mathbf{x}, \mathbf{z}(t); \mathbf{\Omega}\right) + \delta D_t^{reform} + \gamma D^T + \beta D_t^{reform} D^T\right\} \qquad (1)$$

where λ_0 is the baseline hazard, i.e. the pre-reform hazard to employment. $f(.)$ is a function of time-invariant covariates, $\mathbf{x}$, and time-varying covariates, $\mathbf{z}(t)$, and $\mathbf{\Omega}$ is a vector of parameters corresponding to the covariates.[18] D_t^{reform} is a time-varying dummy variable, where $D_t^{reform} = 0$ prior to the reform and $D_t^{reform} = 1$ thereafter. D^T is a dummy for the treatment group, where $D^T = 0$

[18] The covariates included are sex, age, age^2, immigrant background, marital status, presence of children younger than 18, level of education (4 categories), type of education (10 categories), ln(pre-unemployment wage), number of days left until UI benefit expiration (6 categories), and indicators of the local labor market (county) as well as the month of entry into unemployment.

if the previous wage is below SEK 20,696 and $D^T = 1$ for wages above that. The effect of the reduction in the SI benefit cap is obtained by comparing the change in hazard rates for the treated and the comparisons after the reform. The effect of the policy change is given by the coefficient of the interaction variable, β.

The identifying assumption behind this 'difference-in-differences' approach is thus that the development over time of the hazard to employment in the comparison group, captures the counterfactual development in the treatment group, had the reform not occurred. This assumption is violated if, for example, the labor market opportunities developed differently for the two groups around the time of the reform. Divergent trends at other times during the sampling period may also be problematic as they may lead to divergent compositional changes in the two groups regarding unobserved factors.[19]

In order to check whether the estimates are affected by compositional changes in unobserved factors, I also estimate stratified models. I use the month of entry into unemployment as well as the local labor market as stratification units. This specification should be less sensitive to unobserved heterogeneity as the reform effect is identified solely by comparing individuals beginning their unemployment period during the same month and in the same local labor market. To check whether the results could be biased due to divergent changes in labor market opportunities for the two groups, I try to assess whether such changes have taken place during the relevant time period. I also re-estimate the model for a few different sub-samples which are more similar than the treated and comparisons in the baseline model, in terms of the pre-unemployment wage. Finally, to test whether the results may be biased as a result of divergent seasonal patterns for the two groups, I analyze the effects of a hypothetical reform supposed to have occurred on the same date the year after the actual reform. The results turn out to be robust in these respects.

5 Data and sampling

5.1 Data

I combine data from several different sources for the empirical analysis. The database *ASTAT*, originating from the unemployment insurance funds, *HÄNDEL*, from the PES, and the *Sickness Benefit Register* (*SFR*) from the National Social Insurance Board, constitute the main sources. These databases are all a part of *LINDA*, which is a register-based longitudinal database that

[19] It is well known that problems with unobserved heterogeneity are particularly important to handle when estimating duration models. Contrary to usual regression models, even unobserved heterogeneity which is uncorrelated with the included covariates may cause biased coefficients.

includes about 3 percent of the Swedish population.[20] LINDA additionally contains several demographic variables collected from e.g. tax registers.

ASTAT contains weekly information on UI benefit payments as well as on the number of days left until the benefits run out, for all unemployed persons who have received either basic-amount or income-related benefits. It is most common to receive income-related benefits; during 2003 only about 9 percent of all benefit days were on the basic-amount. For those entitled to income-related benefits, the database also includes information on the previous wage.

I use ASTAT as the data source for unemployment spells, which implies that the condition for being defined as unemployed is to receive funding from the UI.[21] Since data on the previous wage does not exist for those who are only entitled to the basic-amount of UI benefits, I exclude this group from the sample. Data on the pre-unemployment wage is needed in order to determine a person's SI compensation in case of sickness.[22]

SFR contains data on sickness spells for all persons who have been entitled to SI benefits. SI benefits can be given on a full or part-time basis, and they can be of a few different types: regular benefits for illness, compensation for work related injury, rehabilitation benefits, and benefits for preventive care. Regular SI benefits for illness are the most common and were paid out during about 89 percent of all SI spells starting in 2003.

I merge ASTAT with SFR in order to track the length of unemployment spells during which the individual switches to SI benefits. Hence, sickness spells that occur during a UI benefit period (at the latest, start the week after the payments from the UI stop) are considered to be a part of the unemployment spell. Naturally, the same spell continues if the individual later switches back to UI benefits. All types of SI periods are included and, for simplicity, I make no distinction between them.

Neither ASTAT nor SFR contain any information on why the benefits stop, at the end of a spell. Therefore, in order to determine whether or not an unemployment period ends in employment, I use information from HÄNDEL. HÄNDEL consists of data on the individual's labor market status, e.g. unemployed; employed; or participant in a labor market program, and on transitions between such states, for all persons who are registered at the PES. Since registration is compulsory in order to receive UI benefits, the records should include all UI benefit recipients.

[20] For a detailed description of LINDA, see Edin and Fredriksson (2000).

[21] This means that participants in labor market programs and individuals who are registered at the PES as unemployed, but who are not qualified for UI benefits, are not included in the sample.

[22] Since the income measure on which the SI benefits are actually based only exists for those who have reported sick, I use the income measure reported by the UI funds to calculate the individual's SI compensation in case of sickness. Unless the person worked very irregularly before unemployment, the two income measures should be approximately equal.

I use the individual's registered labor market status the week after the UI (or SI) benefits stop to define whether or not a spell ends in employment. If the worker is still registered in a state pertaining to unemployment during this week, I instead use the labor market transition closest in time after as the reason for benefit interruption, given that the transition occurs within the next four weeks.[23] Both permanent and temporary jobs are classified as employment, as long as they cause a break of at least three weeks in the UI benefit payments.

5.2 Sampling and descriptive statistics

I construct the sample by selecting all individuals who began an unemployment period with income-related UI benefits during the period 1 December 2002–31 December 2003. The reason for not sampling before December 2002 is that the wage information is incomplete before this point in time.[24] An unemployment period is considered to begin when a person who has not received UI benefits during the last three weeks, starts to receive UI benefits. I require that the unemployment spells begin with funding from the UI, i.e. I do not include persons who became unemployed during or directly after a sickness period.

Each unemployment spell that begins during the sampling period is followed during, at most, 60 weeks or until the end of 2004. The spell length is measured in weeks. An unemployment period ends when there is a break in the UI payments, other than due to sickness, which is three weeks or longer. That is, very short intervening employment periods or other breaks are treated as part of the unemployment period. If a UI period ends for some other reason than employment, e.g. because the person starts an education; a labor market program (including subsidized employment programs); or if the reason is unknown, the spell is treated as censored.

Quite a large share of the unemployment spells, almost 15 percent, end for unknown reasons; either the PES has registered that they have lost contact with the person, or the data contains no reason for the benefit interruption[25]. It is likely that some of these spells end in employment. People who have found a job may not see any reason to contact the PES. If the job is short term only, such persons are likely to remain registered as unemployed during the subsequent employment period, and hence I do not observe any

[23] The reason for allowing this gap is that ASTAT and HÄNDEL do not match perfectly in this aspect. The discrepancy is most likely due to that there is no flow of information from the UI funds to the employment agencies regarding the individual's benefit payments or labor market status. The information in HÄNDEL is instead given to the employment agency by the individual or his or her employer.

[24] Before this date, the wage variable is capped for individuals belonging to some of the UI funds.

[25] In most of these cases the person is still registered as full time unemployed.

reason for the benefit interruption in the data. If the job is long-term, the PES will at some point report that they have lost contact with the person.[26] To the extent that the fraction of spells ending for unknown reasons that actually end in employment differ systematically between unemployment spells that include sickness spells and those that do not, this may bias the estimate of the reform effect. To check whether the results are sensitive to how these spells are treated, I have re-estimated the model treating all spells ending for unknown reasons as ending in employment. As it turns out, this does not affect my findings.

This sampling procedure results in a sample of 19,291 unemployment spells. However, almost 12 percent are excluded since the person cannot be found in the HÄNDEL register during the relevant time period, or due to inconsistent information in HÄNDEL. I also exclude workers older than 60 and workers with reported work related disabilities. Finally, I exclude a few spells where the worker's previous wage is below the limit for SI eligibility. The resulting sample contains 11,022 unemployed persons and 14,935 unemployment spells. About 24 percent of the individuals have multiple unemployment spells.

Table 1 and Table 2 below present descriptive statistics, separately for the treatment and the comparison group. Table 1 gives statistics on the duration of the unemployment spells as well as on the reason for benefit interruption. We see that the average spell length is about two weeks longer for the treated than for the comparisons. This could be due to that it is more common among those in the comparison group to have breaks of a few weeks in their unemployment periods. Recall that an unemployment spell – as defined here – ends if there is a break in the benefit payments that is three weeks or longer. Since repeated unemployment is more common among the comparisons, this group in general has fewer days left until their UI benefits expire in the beginning of their unemployment spells. The proportion of spells ending in employment also differs between the groups; while 35 percent end in employment for the treated, this share is only 21 percent for the comparisons. Compared to previous Swedish studies on unemployment duration, these shares appear low; e.g. in Carling et al (2001) the proportion of spells ending in employment is nearly 47 percent. There are a couple of reasons for why this share is much lower in my sample: I censor spells at an earlier duration of unemployment; the proportion of spells ending for unknown reasons (which could be employment) is much higher; and I am more likely to observe breaks in the unemployment spells in my data, compared to in the data used in previous studies.[27]

[26] Bring and Carling (2000) have conducted a follow-up study of 'lost contact' individuals. They find that almost 50 percent dropped out due to employment.

[27] Most previous Swedish studies have used HÄNDEL to measure unemployment duration. This data is less appropriate here, since there is no consistent way of handling individuals who transfer to SI benefits in this register. Short sickness spells are likely to be unnoticed in

The most common reason for benefit interruption in the comparison group is ‘other destinations’; 42 percent of the spells end in this category, compared to 22 percent among the treated. Other destinations include e.g. education and part time unemployment (without UI benefits). Among these, the latter is the most common destination in the comparison group.

Table 1: Spell characteristics

	Treatment group	Comparison group
Number of unemployment spells	3 368	11 567
Number of individuals	2 696	8 408
Proportion of ind. with multiple spells	0.19	0.25
Days left until UI benefit expiration (in the beginning of the spell)	224	202
Proportion of spells lasting longer than		
10 weeks	0.67	0.61
20 weeks	0.44	0.40
30 weeks	0.31	0.28
40 weeks	0.22	0.21
50 weeks	0.16	0.14
Average spell length (weeks)	23.7	21.8
Proportion of spells ending in		
Employment	0.35	0.21
Labor market programs	0.13	0.13
Other destinations♦	0.22	0.42
Unknown destination	0.19	0.14
Censored after 60 weeks or due to end of study	0.11	0.10

Notes: The sample consists of the individuals in the LINDA-database who began an unemployment period with income-related UI benefits during the period 2002-12-01 – 2003-12-31.
♦Other destinations include e.g. education and part time unemployment (without UI benefits).

From Table 2, which presents descriptive covariate statistics, we can see that the unemployed in the comparison group, on average, are younger, less educated, and have more young children compared to the treated. The proportion of women is also higher in this group, as is the proportion of immigrants from non-OECD countries. There are also some differences in type of education between the two groups. The differences in observed characteristics are a natural consequence of having defined treatment status based on the pre-unemployment wage.

HÄNDEL, whereas a person who gets SI benefits for a longer time period either leaves the register at some point during the sickness period, or is moved to the category ‘others registered’. The PES generally has less frequent contact with individuals in this category, which means that the information on unemployment duration is likely to be less accurate also for these individuals.

Table 2: Sample characteristics (means)

	Treatment group	Comparison group
Female	0.28	0.61
Age	38.8	35.0
Immigrant: OECD	0.05	0.04
Immigrant: other	0.06	0.13
Married	0.46	0.44
Presence of children<18	0.38	0.46
Length of education		
Upper secondary education	0.87	0.85
Post-secondary education	0.34	0.21
Missing	0.00	0.00
Type of education		
General	0.20	0.29
Pedagogic, teacher education	0.03	0.05
Humanities, arts	0.04	0.06
Social sciences, law, trade, admin.	0.17	0.16
Science, mathematics, computer science	0.04	0.02
Technical, manufacturing	0.38	0.17
Agriculture, forestry, veterinary	0.02	0.02
Health care, social work	0.04	0.12
Service	0.04	0.08
Missing/unknown	0.03	0.04
Pre-unemployment wage (month), SEK*	25 552	15 968
Number of individuals	2 696	8 408

Notes: The sample consists of the individuals in the LINDA-database who began an unemployment period with income-related UI benefits during the period 2002-12-01–2003-12-31.
* denotes average among spells, rather than among individuals.

Figure 4 below shows how the flow from UI to SI benefits evolves over time for the sample of unemployed workers. Figure 5 shows the evolution of the share of the unemployed finding employment. The shares in both figures are calculated for time intervals of two weeks, and separately for the treatment and the comparison group. Here, we see a first indication of how the reform affected sickness absence and job finding among unemployed workers. The sick report rate is higher for the comparison group for most of the time period. Around the time of the reform it decreases for both groups, a pattern which is in line with a common finding in the Swedish literature on sickness insurance, namely that sick report rates tend to decline in the summer (see e.g. Larsson 2006, and Johansson and Palme 2005). After the summer, the flow to SI benefits returns almost to the pre-reform level for the comparisons group, while it remains on a somewhat lower level for the treated. This pattern is thus consistent with the reform having a negative effect on sickness absence among those unemployed who were affected by the reform, as is found by Hall and Hartman (2009). Regarding the job finding rate on the other hand, there is no indication that transitions to employment increased for the treated relative to the comparisons after the reform. When interpreting these figures it is important to note that they do not adjust for any of the potentially important differences between the two groups. Perhaps most importantly, they do not account for the lengths of the unemployment spells.[28] Separating the reform effect from the effect of unemployment duration is a crucial part of the identification strategy.

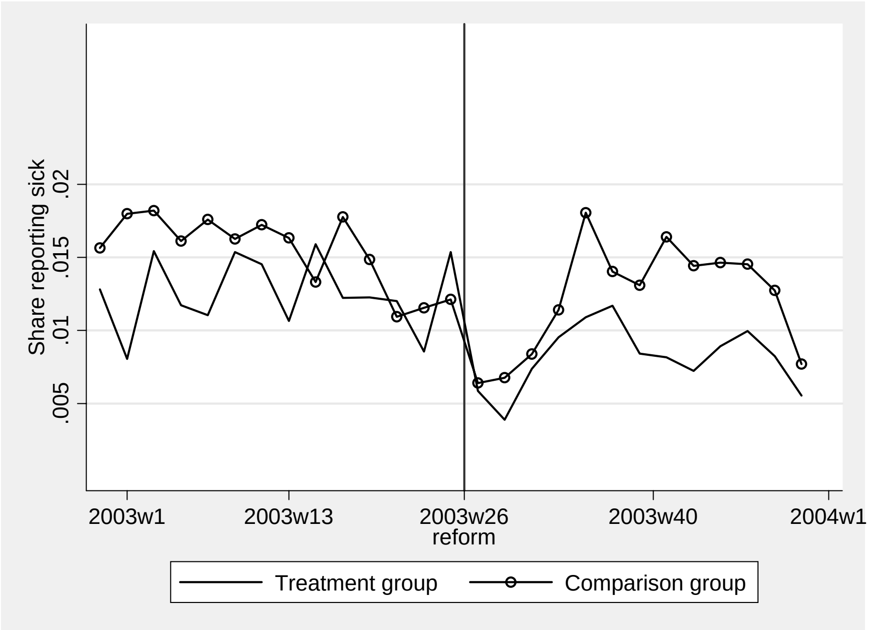

Figure 4: Share of the UI recipients reporting sick (per two-week interval), before and after the reform

Notes: The shares are computed as (number of UI recipients reporting sick within an interval)/(average number of UI recipients each week in that interval). The shares are computed for the individuals in the LINDA-database who began an unemployment period with income-related UI benefits during the period 2002-12-01–2003-12-31.

[28] Not accounting for spell length means that the composition of the two groups with regard to unemployment duration will change over time in divergent ways, since the workers in the different groups leave unemployment at different rates. This may cause the difference between the hazard rate for the treated and the comparisons to change over time.

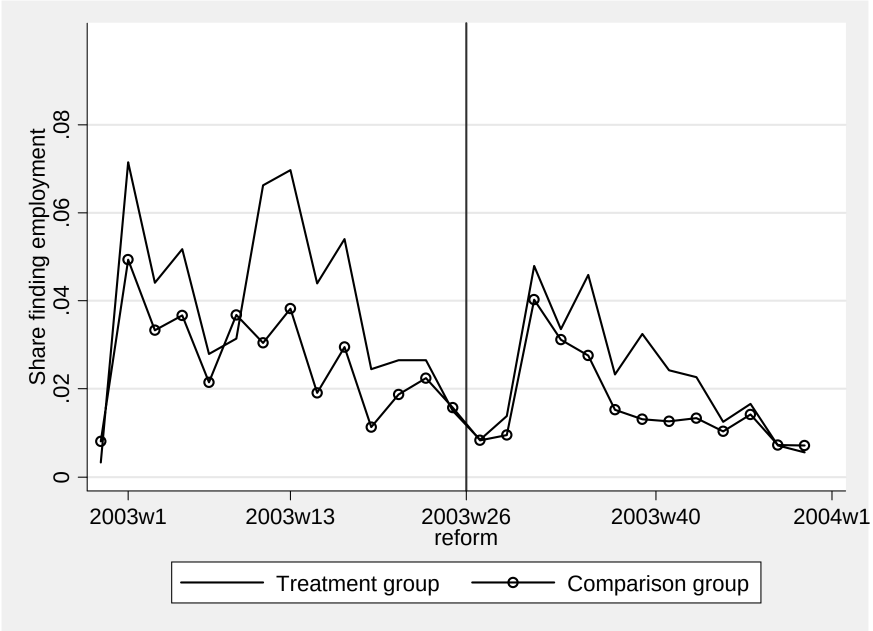

Figure 5: Share of the unemployed finding employment (per two-week interval), before and after the reform

Notes: The shares are computed as (number of unemployed persons finding a job within an interval)/(average number of unemployed persons each week in that interval). The shares are computed for the individuals in the LINDA-database who began an unemployment period with income-related UI benefits during the period 2002-12-01–2003-12-31.

6 Empirical results

6.1 Transitions to sickness insurance

For the reform in July 2003 to have an effect on the job finding rate among unemployed workers, it must first of all have an effect on sickness absence. I therefore begin by examining how the reform affected the incidence of sick reports among the UI recipients, that is, the transition rate from UI to SI benefits. Hence, I replicate the results in Hall and Hartman (2009), though with a somewhat different sample[29].[30],[31]

[29] The most important difference is that I use weekly, rather than daily, data.

[30] Since I allow individuals to return to UI benefits after a sickness period, I here follow Hall and Hartman (2009) and exclude UI spells starting after July 1, 2003. This is to avoid changes in the sample composition that are caused by the reform. If the reform also affects the duration of the SI spells, it may affect the composition of UI recipients through its effect on the hazard rate from SI back to UI.

[31] In this analysis I do not impose the restriction that there must be a three-week break in the UI benefit payments for a spell to end. Instead, a sickness period of *any* length or an interrup-

Table 3 presents results for Cox regression models as described in Section 4, but where the outcome variable is sick leave rather than employment. The table shows the estimated reform effect for four different specifications, estimated with partial maximum likelihood[32]. A table with all parameter estimates can be found in the Appendix. Column (1) shows the results for a model that only includes a dummy for the reform date, a dummy for treatment status, and an interaction variable called the 'cap reform effect'. The latter captures the effect of the reduced SI benefit cap, and is thus the variable of interest. In column (2) I present results for a model to which I have added indicators for the month of entry into unemployment, the number of days left until the UI benefits expire, the local labor market, as well the following individual characteristics: sex, age, level of education (4 categories), type of education (10 categories), immigrant background, marital status, presence of children younger than 18, and pre-unemployment wage. The last two columns show results for two stratified models, where the month of entry into unemployment and the local labor market are used as stratification units.[33] As mentioned in Section 4, these models should be less sensitive to compositional changes in unobserved factors as the reform effect is identified solely by comparing individuals beginning their unemployment period during the same month (column 3), as well as in the same local labor market (column 4).

We first note that the coefficient for the reform date dummy is negative and statistically significant in all four specifications, indicating that there was a general decrease in sickness absence around the time of the reform. This variable should partially be picking up the effect of the general 3 percent reduction in SI benefits but also the effect of other changes around July 1, 2003, such as seasonal variation in sickness absence. The estimate for the 'cap reform effect' is also statistically significant in all specifications and quite large; it suggests that the reduced SI benefit cap lowered the incidence of sick reports among the treated with 31-33 percent[34]. Moreover, this estimate is very stable across the various specifications. Hence, in line with Hall and Hartman (2009), these results suggest that the reform had a strong negative effect on the transition rate to SI among the UI recipients.[35]

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tion in the UI benefit payments which is longer than one calendar week defines the end of a UI period.

[32] Ties are handled using the exact method in SAS, see DeLong et al (1994), and Kalbfleisch and Prentice (1980).

[33] These models are estimated with a stratified partial maximum likelihood estimator; see e.g. van den Berg (2001), Section 6.

[34] The percentage effect is obtained by 100*(exp(β)-1), where β is the parameter of interest.

[35] The size of the effect is very similar to the one found in Hall and Hartman (2009). Their preferred estimate for the 'cap reform effect' implies a 36 percent reduction of the incidence of sick reports. This effect is very robust across various sensitivity tests. They also study the effect on sickness duration, but find no such effects.

Table 3: Estimated effects on the transition rate to sickness insurance

	(1)	(2)	(3)	(4)
Post reform (t) (D_t^{reform})	-0.263***	-0.195*	-0.517**	-0.431*
	(0.086)	(0.107)	(0.255)	(0.257)
Previous wage>20,696 (D^T)	-0.139	0.121	0.119	0.128
	(0.091)	(0.106)	(0.107)	(0.108)
Cap reform effect (t) ($D_t^{reform}*D^T$)	-0.377**	-0.390**	-0.397**	-0.377**
	(0.159)	(0.159)	(0.159)	(0.164)
Month of entry into unemployment	No	Yes	-	-
# days until UI-exp., in the beginning of the spell (six categories)	No	Yes	Yes	Yes
All other covariates included	No	Yes	Yes	Yes
Stratification by month of entry	No	No	Yes	Yes
Stratification by local labor market	No	No	No	Yes
-2 Log likelihood	14,819	14,607	13,524	9,303
No of observations	12,748	12,746	12,746	12,746
No of strata	-	-	7	153

Notes: Estimation using (stratified) partial maximum likelihood estimator. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. (t) denotes time-varying variable. The following covariates are included in column (2)-(4): sex, age, age^2, immigrant background, marital status, presence of children younger than 18, level of education, type of education, ln(pre-unemployment wage), and indicators for the local labor market.

6.2 Transitions to employment

Let us now move on to analyze whether the reduced sick report rate among the treated also translated into a higher rate of job finding. The results for the Cox regression models are reported in Table 4. As in the previous section, I start by presenting results for a model that only includes a dummy for the reform date, a dummy for the treatment group and an interaction variable – the 'cap reform effect' – which captures the effect of the reduced SI benefit cap on the treated population (column 1). The point estimate for the cap reform effect is close to zero in this model and it is not statistically significant. Adding covariates to the model does not alter this finding; the estimate for the cap reform effect is close to zero and insignificant also in the second specification, which includes all covariates. A table with all parameter estimates can be found in the Appendix.

In the last two specifications, I have stratified on the month of inflow into unemployment (column 3) as well as on the local labor market (column 4). This means that the baseline hazard is allowed to differ across months of entry, and across local labor markets. The variation which identifies the reform effect in these specifications thus comes from when, within a given month, a person entered unemployment. These models should be less sensitive to seasonal (column 3), as well as regional (column 4), variations in labor market conditions. A further implication of this approach is that only unemployment spells that start before the reform are used to identify the reform effect (since there is no within-month variation in the time-varying reform variable for spells beginning after the reform). While the estimate for the cap reform effect does not change much as I stratify on the entry month, it becomes more negative when I also stratify on the local labor market. However, it is still very far from being significantly different from zero. In sum, I find no evidence suggesting that the reduced sick report rate in the treatment group affected the transition rate to employment.

Regarding the other variables, we can see that the estimate for the post reform dummy is negative and statistically significant in all four specifications. This result indicates a general decrease in transitions to employment around the time of the reform (as is suggested in Figure 5). We also note that the dummy for the treatment group is positive and statistically significant, showing that the job finding rate in general is higher for the unemployed with previously high wages.

Table 4: Estimated effects on the transition rate to employment

	(1)	(2)	(3)	(4)
Post reform (*t*)	-0.156***	-0.300***	-0.739***	-0.617***
(D_t^{reform})	(0.046)	(0.073)	(0.233)	(0.237)
Previous wage>20,696	0.422***	0.209***	0.197***	0.235***
(D^T)	(0.066)	(0.076)	(0.076)	(0.078)
Cap reform effect (*t*)	0.007	-0.007	0.000	-0.069
($D_t^{reform}*D^T$)	(0.078)	(0.079)	(0.079)	(0.082)
# days until UI-exp., in the beginning of the spell (six categories)	No	Yes	Yes	Yes
Month of entry into unemployment	No	Yes	-	-
All other covariates incl.	No	Yes	Yes	Yes
Stratification by month of entry	No	No	Yes	Yes
Stratification by local labor market	No	No	No	Yes
-2 Log likelihood	39,382	38,670	35,964	25,669
No of observations	14,935	14,932	14,932	14,932
No of strata	-	-	11	239

Notes: Estimation using (stratified) partial maximum likelihood estimator. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. (*t*) denotes time-varying variable. The following covariates are included in column (2)-(4): sex, age, age^2, immigrant background, marital status, presence of children younger than 18, level of education, type of education, ln(pre-unemployment wage), and indicators for the local labor market.

Even in the absence of a significant average effect of the reform, there may be an effect on the job finding rate for sub-groups of unemployed workers. Previous studies on sickness absence among the unemployed find that the flow to SI increases as the UI expiration date approaches (e.g. Larsson 2006). A similar pattern is visible in my results; see Table A.1 in the Appendix. Moreover, Hall and Hartman (2009) find that the decline in sick reports due to the July 2003-reform was largest among those with relatively few UI days left. To check whether there perhaps is an effect on the job finding rate for those who are close to benefit expiration, I have re-estimated the model only including the sub-sample with less than 150 remaining UI days (in the beginning of their unemployment period). However, the estimate for the cap reform effect is insignificant also for this group.[36]

6.3 Sensitivity analyses

The treatment and comparison group are indeed heterogeneous in several respects as treatment status is defined based on pre-unemployment earnings. One concern is whether the estimate of the cap reform effect is biased due to divergent trends in labor market opportunities for the two groups. For instance, if the labor market opportunities worsened for the high-wage relative to the low-wage unemployed during the time period studied, this may bias the estimate of the cap reform effect downwards. This could then explain why we do not observe any effect of the cap reform on the transition rate to employment. A similar problem may arise if the labor market opportunities worsened more for the high-wage than for the low-wage unemployed around July 2003, due to different seasonal patterns. I perform several sensitivity analyses in order to test the robustness of my results in these respects.

I start by examining employment-to-population rates for different educational groups during the relevant period (2002-2004), see Figure 6. Since the average level of education is higher among the treated, this figure should give an indication of how the labor market opportunities developed for the two groups during this time period. Figure 6 gives no support for that these opportunities worsened for the treated relative to the controls; it rather suggests the reverse.

[36] These results are not reported, but are available upon request.

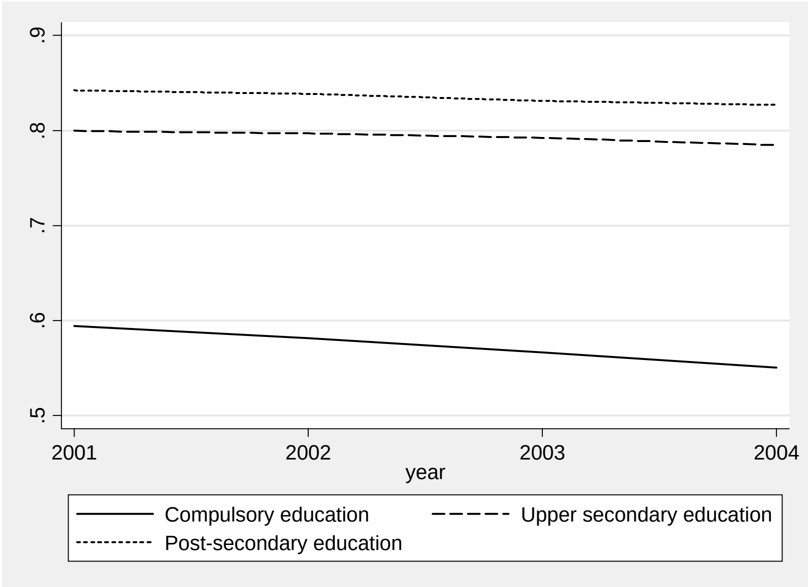

Figure 6: Employment-to-population rates for different educational groups, 2001–2004 (annual averages)

Notes: Calculated for persons aged 16-64. Source: Labour Force Surveys, Statistics Sweden.

The employment-to-population rates are only available as annual averages. As discussed above, the result could also be biased due to divergent seasonal patterns in labor market opportunities for workers with different wages. In order to examine this possibility, I first re-estimate the model for different sub-samples which are more similar than the treated and comparisons in the baseline model, in terms of the pre-unemployment wage. I also test whether there could be different seasonal patterns for the two groups around the time of the reform, by analyzing the effects of a hypothetical reform supposed to have occurred on the same date the year after the actual reform.[37]

To reduce heterogeneity between the two groups, I successively exclude individuals with the 10 percent, 30 percent, 50 percent and 70 percent lowest pre-unemployment wages in the comparison group.[38] Column (2)-(5) in Table 5 present the results from this exercise. For ease of comparison, the first

[37] Due to lack of data on pre-unemployment wages for 2002, I cannot perform the same analysis for the year before the reform.

[38] Ideally, we may also want to exclude individuals with the highest previous wages among the treated. However, by doing this we would at the same time reduce the average amount of 'treatment' received in the treatment group. Recall that the percentage decrease in SI benefits varied among the treated depending on the pre-unemployment wage; the higher the wage, the larger the percentage reduction in benefits. The results from such an exercise would thus be difficult to interpret.

column of the table reproduces my main results (shown in column 2, Table 4). The point estimate for the cap reform effect remains close to zero in all these regressions and the estimate is far from being statistically significant. Hence, limiting heterogeneity between the two groups in this way does not affect my findings.

Table 5: Effects of excluding workers with the lowest previous wages

	(1)	(2)	(3)	(4)	(5)
% of comparison group excluded	0%	10%	30%	50%	70%
Average previous wage					
Comparison group	15,968	16,680	17,573	18,340	19,149
Treatment group	25,552	25,552	25,552	25,552	25,552
Post reform (*t*)	-0.300***	-0.320***	-0.334***	-0.398***	-0.396***
(D_t^{reform})	(0.073)	(0.075)	(0.082)	(0.092)	(0.109)
Previous wage>20,696	0.209***	0.224***	0.261***	0.265***	0.251***
(D^T)	(0.076)	(0.080)	(0.083)	(0.085)	(0.091)
Cap reform effect (*t*)	-0.007	-0.003	-0.006	0.008	0.000
($D_t^{reform}*D^T$)	(0.079)	(0.080)	(0.083)	(0.087)	(0.097)
All covariates included	Yes	Yes	Yes	Yes	Yes
-2 Log likelihood	38,670	36,727	32,145	27,186	21,353
No of observations	14,932	13,777	11,462	9,151	6,838

Notes: Estimation with partial maximum likelihood. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. (*t*) denotes time-varying variable. The following covariates are included: sex, age, age^2, immigrant background, marital status, presence of children younger than 18, level of education, type of education, ln(pre-unemployment wage), number of days until UI-expiration, indicators for the month of entry into unemployment and the local labor market.

To study the effect of a hypothetical reform the year after the actual reform, I construct a new sample of unemployment spells, following the same sampling procedure but instead including spells beginning during the period 1 December 2003–30 December 2004. In case this imaginary reform gives rise to a significant estimate for the 'cap reform effect', this would indicate that the seasonal patterns may indeed differ for the two groups around this time of the year. If there is no significant 'effect' of the hypothetical reform either, divergent seasonal patterns in labor market opportunities seems less likely to be a problem. The results for this exercise are shown in Table 6, column (2). The first column of the table reproduces my main results for the actual reform (shown in column 2, Table 4). The estimate for the hypothetical cap reform is more negative than the estimate for the actual reform, however it is far from being significantly different from zero. Hence, there is no evidence of different seasonal patterns for the two groups around July 1 the year after the reform.

Table 6: Estimated effects of a hypothetical reform July 1, 2004

	(1) Actual reform (July 1, 2003)	(2) Hypothetical reform (July 1, 2004)
Post reform (*t*) (D_t^{reform})	-0.300***	-0.303***
	(0.073)	(0.074)
Previous wage>20,696 (D^T)	0.209***	0.346***
	(0.076)	(0.070)
Cap reform effect (*t*) ($D_t^{reform}*D^T$)	-0.007	-0.056
	(0.079)	(0.073)
No of days until UI-expiration (six cat.)	Yes	Yes
Month of entry into unemployment	Yes	Yes
All other covariates included	Yes	Yes
-2 Log likelihood	38,670	40,136
No of observations	14,932	16,160

Notes: Estimation with partial maximum likelihood. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. (*t*) denotes time-varying variable. The following covariates are included: sex, age, age^2, immigrant background, marital status, presence of children younger than 18, level of education, type of education, ln(pre-unemployment wage), and indicators for local labor market.

In addition to the sensitivity analyses presented above, I have checked whether the results are sensitive to how unemployment spells that end for 'unknown reasons' (which may well be employment) are treated. To do this, I have re-estimated the model treating these spell as ending in employment. I have also estimated a model that account for (persistent) unobserved individual heterogeneity, by using the fact that I have multiple unemployment spells for a part of the sample.[39] The findings are qualitatively the same in these models.

7 Concluding remarks

This paper studies the effects of a reform which substantially reduced the economic incentives for unemployed persons to transfer from the unemployment insurance (UI) to the sickness insurance (SI). While there is evidence that this reform effectively lowered the incidence of sick reports among the unemployed affected, I find no evidence suggesting that the re-

[39] To check whether the results are affected by compositional changes in unobserved factors, I have estimated a model where I stratify on the individual. That is, the reform effect is identified using within individual variation. This method may not be ideal since it only uses a small sub-sample of the unemployed workers (only 24 percent of the individuals in the sample have repeated unemployment spells), as well as rests on the assumption that the unobserved individual characteristics are fixed across spells. However, the results are qualitatively the same. These results are not reported, but are available upon request.

duced sick report rate in turn affected the transition rate to employment. Hence, for those who reduced their sick report rate due to the reform, spending more time in the UI rather than the SI did not seem to shorten the time out of employment.

Should we then conclude that the interplay between these two insurances does not have any economic significance, and that it does not matter whether there perhaps is excess use of the SI among the unemployed? Probably not. First of all, making sure that the insurance systems are used in the way intended is likely to be important per se. If the citizens have the perception that the benefits are misused this could undermine the legitimacy for the social insurance system. Second, we should note that the study in this paper is a partial equilibrium analysis, and that the results do not indicate how the reform affected total employment. Reduced SI benefits for the unemployed may affect employment through other channels than the one studied here, e.g. it may affect transitions to other benefit systems as they become relatively more attractive. Early retirement pension is one example.

Finally and perhaps most importantly, the fact that I do not find any effect on the job finding rate can have two different explanations, which in turn will have very different policy implications. First, it could be due to that, for those affected by the reform, search effort did not differ depending on receiving benefits from the UI or the SI (given their health status). This would then indicate that monitoring in at least one of the insurance systems is lax. If these unemployed persons did not search actively in *either* system, this would suggest lax monitoring in the UI, as active search is a formal requirement for receiving UI benefits. If they in fact searched actively in *both* systems, this would instead indicate lax monitoring in the SI, as the SI is intended for those who have lost their work (search) capacity due to sickness.

Second, it is possible that spending more time in the UI in fact *did* increase search effort, but that more active search still did not result in faster transitions to employment for this particular group. If this is the case, it could indicate that those who changed their sickness absence behavior due to the reform were not very attractive on the labor market. An interesting topic for future research would be to use data on individual search behavior in order to discriminate between these two explanations.

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Appendix

Table A1: Estimated effects on the transition rate to sickness insurance

	(1)	(2)	(3)	(4)
Post reform (t) (D_t^{reform})	-0.263***	-0.195*	-0.517**	-0.431*
	(0.086)	(0.107)	(0.255)	(0.257)
Previous wage>20,696 (D^T)	-0.139	0.121	0.119	0.128
	(0.091)	(0.106)	(0.107)	(0.108)
Cap reform effect (t) ($D_t^{reform} * D^T$)	-0.377**	-0.390**	-0.397**	-0.377**
	(0.159)	(0.159)	(0.159)	(0.164)
Days until UI benefit expiration♣ (*Ref*: 50-1 days until UI-exp.)				
300-251 days until UI-exp.		-0.288***	-0.280***	-0.250***
		(0.092)	(0.092)	(0.094)
250-201 days until UI-exp.		-0.251**	-0.243**	-0.203*
		(0.111)	(0.111)	(0.113)
200-151 days until UI-exp.		-0.075	-0.062	-0.017
		(0.114)	(0.114)	(0.117)
150-101 days until UI-exp.		0.038	0.049	0.096
		(0.115)	(0.115)	(0.118)
100-51 days until UI-exp.		0.102	0.110	0.127
		(0.117)	(0.117)	(0.119)
Female		0.202***	0.213***	0.224***
		(0.070)	(0.070)	(0.072)
Age		0.077***	0.078***	0.075***
		(0.024)	(0.024)	(0.024)
Age^2		-0.001**	-0.001**	-0.001**
		(0.000)	(0.000)	(0.000)
Immigrant: OECD		-0.031	-0.042	-0.053
		(0.128)	(0.128)	(0.131)
Immigrant: other		0.035	0.039	0.064
		(0.088)	(0.088)	(0.089)
Married		-0.119*	-0.118*	-0.127*
		(0.068)	(0.068)	(0.069)
Presence of children<18		0.209***	0.209***	0.201***
		(0.074)	(0.074)	(0.075)
Level of education: (*Ref*: compulsory school)				
Upper secondary education		-0.176	-0.172	-0.174
		(0.110)	(0.110)	(0.112)
Post-secondary education		-0.273***	-0.264***	-0.248***
		(0.094)	(0.094)	(0.096)
Missing		-0.816	-0.824	-0.692
		(1.020)	(1.021)	(1.030)
Type of education (10 categories)	No	Yes	Yes	Yes
ln(pre-unemployment wage)		-0.295***	-0.298***	-0.311***
		(0.106)	(0.107)	(0.107)
Month of entry into unemployment	No	Yes	-	-
Dummies for local labor market (county)	No	Yes	Yes	-

Table A.1, cont.

Stratification by month of entry	No	No	Yes	Yes
Stratification by local labor market	No	No	No	Yes
-2 Log likelihood	14,819	14,607	13,524	9,303
No of observations	12,748	12,746	12,746	12,746
No of strata	-	-	7	153

Notes: Estimation using (stratified) partial maximum likelihood estimator. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. (*t*) denotes time-varying variable.♣Measured in the beginning of the unemployment spell.

Table A2: Estimated effects on the transition rate to employment

	(1)	(2)	(3)	(4)
Post reform (*t*) (D_t^{reform})	-0.156***	-0.300***	-0.739***	-0.617***
	(0.046)	(0.073)	(0.233)	(0.237)
Previous wage>20,696 (D^T)	0.422***	0.209***	0.197***	0.235***
	(0.066)	(0.076)	(0.076)	(0.078)
Cap reform effect (*t*) ($D_t^{reform} * D^T$)	0.007	-0.007	0.000	-0.069
	(0.078)	(0.079)	(0.079)	(0.082)
Days until UI benefit expiration♣				
(*Ref*: 50-1 days until UI-exp.)				
300-251 days until UI-exp.		0.218***	0.219***	0.209***
		(0.067)	(0.067)	(0.068)
250-201 days until UI-exp.		0.147*	0.147*	0.146*
		(0.076)	(0.076)	(0.077)
200-151 days until UI-exp.		0.169**	0.158**	0.145*
		(0.080)	(0.080)	(0.081)
150-101 days until UI-exp.		0.114	0.106	0.088
		(0.084)	(0.084)	(0.085)
100-51 days until UI-exp.		0.002	-0.007	-0.003
		(0.090)	(0.090)	(0.091)
Female		-0.269***	-0.261***	-0.261***
		(0.041)	(0.041)	(0.042)
Age		0.048***	0.048***	0.049***
		(0.013)	(0.013)	(0.013)
Age^2		-0.001***	-0.001***	-0.001***
		(0.000)	(0.000)	(0.000)
Immigrant: OECD		-0.176**	-0.186**	-0.175**
		(0.088)	(0.088)	(0.089)
Immigrant: other		-0.620***	-0.621***	-0.612***
		(0.067)	(0.067)	(0.068)
Married		0.097**	0.095**	0.103**
		(0.047)	(0.047)	(0.048)
Presence of children<18		-0.122**	-0.118**	-0.130***
		(0.047)	(0.048)	(0.048)
Level of education:				
(*Ref*: compulsory school)				
Upper secondary education		-0.201***	-0.195***	-0.182***
		(0.069)	(0.069)	(0.070)
Post-secondary education		0.060	0.062	0.057
		(0.049)	(0.049)	(0.050)
Missing		0.422	0.418	0.453
		(0.398)	(0.398)	(0.407)
Type of education (10 categories)	No	Yes	Yes	Yes
ln(pre-unemployment wage)		0.125	0.125	0.131*
		(0.079)	(0.079)	(0.079)
Month of entry into unemployment	No	Yes	-	-
Dummies for local labor market (county)	No	Yes	Yes	-
Stratification by month of entry	No	No	Yes	Yes
Stratification by local labor market	No	No	No	Yes

Table A.2, cont.

-2 Log likelihood	39,382	38,670	35,964	25,669
No of observations	14,935	14,932	14,932	14,932
No of strata	-	-	11	239

Notes: Estimation using (stratified) partial maximum likelihood estimator. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. (*t*) denotes time-varying variable.♣Measured in the beginning of the unemployment spell.

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