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Institute for Evaluation of Labour Market and Education Policy

Benefits or work? Social programs and labor supply

Ulrika Vikman

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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 I: This essay evaluates how access to paid parental leave affects labor market entrance for immigrating mothers with small children. Paid parental leave together with job protection may increase labor force participation among women but if it is too generous it may create incentives to stay out of the labor force. This incentive effect may be especially true for mothers immigrating to a country where having small children automatically makes the mothers eligible for the benefit. To evaluate the differences in the assimilation process for those who have access to the parental leave benefit and those who do not, Swedish administration data is used in a difference-in-differences specification to control for both time in the country and the age of the youngest child. The results show that labor market entrance is delayed for mothers and that they are less likely to be a part of the labor force for up to seven years after their residence permit if they had access to parental leave benefits when they came to Sweden. This reduction in the labor force participation is to some extent driven by unemployment since the effect on employment is smaller. But there is still an effect on employment of 3 percentage points lower participation rates 2–6 years after immigration.

Essay II: This essay examines if the probability of leaving unemployment changes for unemployed parents with young children when childcare is available. To investigate this, I use the heterogeneity among Swedish municipalities before the implementation of a 2001 Swedish childcare reform making it mandatory for municipalities to offer childcare to unemployed parents for at least 15 hours per week. The results indicate a positive effect on the probability of leaving unemployment for mothers when childcare is available, but no effect is found for fathers. For mothers, some heterogeneous effects are also found, with a greater effect on the probability of leaving unemployment for work when childcare is available for mothers with only compulsory schooling or university education and mothers with two children.

Essay III (with Helge Bennmarker and Oskar Nordström Skans): In this essay we estimate the effects of conditioning benefits on program participation among older long-term unemployed workers. We exploit a Swedish reform which reduced UI duration from 90 to 60 weeks for a group of older unemployed workers in a setting where workers who exhausted their benefits received unchanged transfers if they agreed to participate in a work practice program. Our results show that job finding increased as a result of the shorter duration of passive benefits. The time profile of the job-finding effects suggests that the effects are due to deterrence effects during the program-entry phase. We find no evidence of wage reductions, suggesting that the increased job-finding rate was driven by increased search intensity rather than lower reservation wages.

Essay IV (with Anna Persson): Previous literature shows that activation requirements for welfare participants reduce welfare participation. However, the dynamics have not been fully examined. In this essay we use a rich set of register data covering the entire population in a Swedish municipality to study how the introduction of mandatory activation programs aimed at unemployed welfare participants affect the probability of entering and exiting welfare. Our results indicate that the reduction in the caseload of welfare participants was mainly due to an increase in welfare exits. The effect is larger for unmarried individuals without children and for young individuals where we also find a reduction in welfare entries. It thus seems that individuals with fewer family responsibilities are more responsive to the reform.

In memory of my mother
Christina Bernspång

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First and foremost I would like to thank my main supervisor Matz Dahlberg. The times I left his office after a meeting with increased confidence and joy in doing research, are uncountable. His advice when it comes to not only my work with this thesis, but also discussions we have had about the following academic life, have been of great appreciation. I would also like to thank my assistant supervisor Eva Mörk. Eva's ability to see the small things is amazing. Both Matz and Eva have undoutbtedly helped me in doing better research and in improving my essays, but I have also appreciated the many laughs and discussions about other things, if it has been books by Arne Dahl or how to prepare for the races in a Swedish classic.

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I have had the opportunity to write two of the papers with other researchers, co-work I have really enjoyed. The cooperation with Anna Persson was a relief and I truly appreciated our discussions and her ability to find relevant literature for our work. I am also very thankful that Oskar Nordström Skans and Helge Bennmarker let me join in their work. They have always given me time and answered my questions and I really learned a lot from working with them.

During my fourth year as PhD student I was invited by Olof Åslund to stay at the Institute for Evaluation of Labour Market and Education Policy, Ifau, which I am indeed grateful for. Both the Department of Economics and Ifau host really good researchers who have shown interest in my work, given me constructive criticism during seminars and happily answered my questions. I especially would like to thank Anders Forslund, Per Johansson and Johan Vikström whose doors are always open. Thanks to Mattias Nordin and Daniel Avdic with whom I have shared room. The discussion with them definitely made me a better economist. Thanks also to Adrian Adermon who have been my Stata and LaTeX support. Niklas Bengtsson, P-A Edin, Susanne Ek, Mikael Elinder, Mattias Engdahl, Caroline Hall, Kajsa Hanspers, Arizo Karimi, Erica Lindahl, Elly-Ann Lindström, Lars Lindvall, Martin Nilsson, Kristina Sibbmark, Håkan Selin, Daniel Waldenström and Göran Österholm are present or former colleagues that also specially have helped or encourage me in the work with this thesis.

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Uppsala, January 2013
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Contents

Introduction

An individual's decision to work is affected by several things. For example how the individual value her time, the wage the individual can get, and what other income sources that is available for the individual. The incentives to work are thereby also affected by the design of different social programs.

In this thesis I will empirically evaluate four different policies and how they affect labor supply. The first essay studies how parental leave affects the labor force participation of immigrant mothers. The second essay evaluates how access to childcare affects unemployed parents' job finding rates. The two last essays concern effects of workfare, where the third essay studies the effect on job finding rates for old long-term unemployed, and the fourth essay studies the effect on entry and exit rates to social assistance.

In order to put the empirical findings in the thesis into perspective I will in this introductory chapter take a theoretical approach to the questions asked in my essays. The structure of this introduction is organized as follows: I will start by describing a simple job search model and then continue to use this model as a framework to discuss the essays in this thesis.

Job search model

The economic science typically uses theoretical models to explain the world. Even if the models are simplified versions of the real world, they can often intuitively explain different behaviors. The model I will describe in this introduction is a simple job search model describing the behavior of an unemployed individual. This model will be used to analyze how different policies may affect the probabilities for an unemployed to find work.

Studying the match between unemployed individuals and vacancies, there are different parameters affecting the supply side (the unemployed),

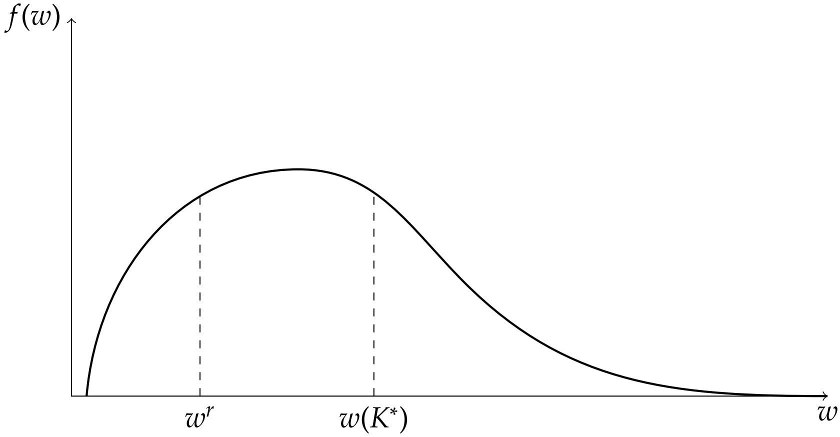

Figure 1: Distribution of wage offers in the economy.

the demand side (the firms) or both. Even if policies may affect parameters on both sides, the demand side will be considered as given. This assumption of no general equilibrium effects is less likely if polices affect a large share of the work force but more credible for reforms only affecting few workers. The demand for a given unemployed may however change if the characteristics of that unemployed change, through, e.g., more education.

Starting with what is given by the demand side, I assume that wages, w, are decided depending on what qualifications (K) is needed for a certain job, rather than on the qualifications of the individual getting the job. Different jobs require different qualifications implying a distribution of wage offers according to figure 1. An unemployed knows his or her own qualifications (K^*) and will therefore prefer the wage $w(K^*)$. If there is uncertainty about which of the employers that offer jobs with those qualifications, an unemployed will search for jobs but will only be offered those with wages less or equal to $w(K^*)$ (Mortensen, 1970).

The distribution of wage offers, $F(w)$, is thereby decided by the demand for labor, i.e. by the employers, and will, from the job searcher's perspective be considered as given. However if an unemployed invest in his or her human capital, e.g. by studying, and thereby get better qualifications, the highest possible wage will be higher and the probability of receiving a job offer will increase. The arrival rate of job offers, λ also depends on the search intensity of the unemployed, s, and can be described as:

$$\lambda = f(F(w), K^*, s) \tag{1}$$

where increased search intensity, s, and higher qualifications increase the job arrival rate; otherwise the arrival rate is seen as given.

Let us next turn to the supply side and the decisions faced by the unemployed. The unemployed has to, at each point in time, decide how much to search and what the minimum acceptable wage should be. Unemployed workers should choose a search intensity that maximizes their utility, U, given the cost of searching and the job offer arrival rate. A simple way to summarize this is through the following equation:

$$max \quad U = b - a(s_i) + s_i\lambda(E - b) \quad w.r.t. \quad s_i \tag{2}$$

where b is the utility of being unemployed, which can be seen as utility derived from both leisure time and unemployment benefits; $a(s_i)$, is the search cost (which is increasing in s_i); and the probability of receiving a job offer, λ, is multiplied by how many jobs the individual has searched for and the difference in utility between being employed and unemployed $(E - b)$. This maximization problem gives the following first order condition:

$$a'(s_i) = \lambda(E - b) \tag{3}$$

that is the marginal cost of more search shall be equal to the expected gain of a search. The cost of search is assumed to be convex indicating that the marginal cost of searching is increasing in s. So given the first order condition this imply that if the cost of searching increases the individual will search less and if the utility of being employed compared to unemployed increases the individual will search more.

But what wage should the unemployed accept? If only accepting $w(K^*)$, λ will be very low and the individual will stay unemployed for a very long time. The individual should therefore choose a reservation wage that maximizes the expected future earnings. The reservation wage fulfilling this is the one when the cost equalizes the expected gain of extended search (Mortensen, 1977). For a given level of search intensity, the reservation wage is given by the following equation:

$$U_e(w^r) = U_u(b - a(s), \lambda, F(w, K^*)) \tag{4}$$

where the left hand side, U_e is the utility of all future incomes when accepting a job offer with the reservation wage w^r, and the right hand side, U_u is the expected utility of continue searching. Hence, unemployed should only accept job offers above the reservation wage, w^r. Higher benefits increase the utility of continuing search and thereby the reservation wage. The cost of searching for job, $a(s)$, lowers the utility and thereby the reservation wage. An increased arrival rate of job offers increases the probability of getting job offers with a high wage and thereby increases the reservation wage. Higher qualifications increase the mean of expected wage offers and thereby also increase the reservation wage. Going back to figure 1, individuals are not offered jobs with wages above $w(K^{*})$ and rejects jobs with wages below w^{r}.

The model described above concerns individuals who have decided to be in the labor force. The utility of taking part in the labor force (U_{LF}) depends on the utility of being employed (U_e), unemployed (U_u) and the probability to be in each state, p_e and $(1-p_e)$. If this utility is lower than the utility when staying out of the labor force (U_{NLF}) an individual will choose not to join. An individuals participate in the labor force if:

$$p_e U_e + (1 - p_e) U_u \geq U_{NLF}(I_{NLF}) \tag{5}$$

where U_{NLF} depends on what income is available for the individual when not working or receiving unemployment benefits, I_{NLF}.

Different social programs in a welfare state will affect different parameters in this model. For example, according to Mortensen (1977) the expected effect on unemployment duration of unemployment insurance (UI) is ambiguous since those who are not yet eligible for UI will try to find work fast to be eligible while those with UI will have higher reservation wages and thereby stay unemployed for a longer time.

The social programs evaluated in this thesis affect the incentives faced by the individuals and thereby their labor supply. While some effects may be obvious, other effects may be clear first after studying specific parameters. For example, providing childcare to unemployed parents makes it easier for the parents to search for work but at the same time also changes the utility of being unemployed and thereby the reservation wage (discussed more below). Next, having the theoretical model laid out in this section in mind, I will discuss in more detail what effects the policies that I evaluate in this thesis might have on the studied individuals' labor supply.

Paid parental leave to immigrants

The first essay studies labor supply decisions by mothers immigrating to Sweden with young children and how these decisions are affected by access to paid parental leave.

Many countries have some sort of paid parental leave (Moss, 2010) to make it easier for parents, especially mothers, to combine family and work. One motive for governments to provide parental leave benefits is to maintain high fertility rates with a high female labor force participation and many studies have indicated that paid parental leave systems give higher birth rates (for an overview see Björklund, 2007). With a system where the benefit is based on earlier income women also have incentives to join the labor force before they give birth to a child. When the benefit is combined with job protection it is also easier for the parents to return to earlier employers after they have been on parental leave (Baker and Milligan, 2008). However, if the parental leave benefit is too generous the benefit may make parents to stay out of the labor force for a long time and potentially make women's return to the labor market difficult.

In Sweden the parental leave system is generous with 480 days of paid parental leave (mainly income based) but 60 days are quoted for each parent (Lindström, 2010). Immigrants coming to Sweden with children below the age of eight get access to the same parental leave benefits as parents whose children are born in Sweden. Most of the immigrants coming to Sweden with small children are not entitled to income related benefits. Instead they receive a fixed amount that today is 225 SEK per day[1]. Mothers use most of these benefits. I therefore focus on the decisions made by the mothers.

So what effects should we expect? What are the predictions from the job search model of the parental leave benefits on the immigrated mothers' labor supply decisions?

Immigrant mothers receive money if they stay out of the labor force, which increase I_{NLF} and thereby U_{NLF} in equation (5). But what will these mothers expected utility be if they take part in the labor force. Initially, these mothers must be considered as having low qualifications since many of them do not speak Swedish. This will give few job offers when searching for jobs. The benefit received when unemployed is the introduction benefit for refugees or, for those mothers with no income or assets, the social assistance. Regarding the benefit level, the parental leave benefit may actually be higher than the benefit available when being unemployed or in the end be the same. This is because the parental leave benefit is fixed and the same for most of these mothers while the introduction benefit or social assistance benefit, for those immigrants who are eligible, follows a national norm and depends on the number of individuals in the household in relation to other income sources the family has. Some mothers will also be eligible for social assistance even if they claim the parental leave benefits and will therefore end up with the same amount as with social assistance but will still meet the same requirements as if they only had parental leave benefits. If the probability of finding a job, p_e, is very low mothers joining

[1]Varied between 60 and 180 during the period I study.

the labor force will be unemployed and the relevant comparison will then be if the utility of being unemployed is greater or equal than staying out of the labor force.

At the same time, when the mothers are unemployed, they have to take part in introduction programs for immigrants and thereby increase their qualifications. So as time goes by, their probability of finding a job will increase and thereby increase the expected utility of joining the labor force to begin with. However, to conclude, having parental leave benefits available for immigrating mothers will increase I_{NLF} and thereby their utility of staying outside the labor force, U_{NLF}, making the decision rule to join the labor force in equation (5) to hold for fewer mothers. The expected effects from the theoretical model is therefore that more immigrant mothers will stay out of the labor force.

To evaluate this question empirically I have compared the labor force participation of mothers who had access to the parental leave benefit (treated group) when they arrived to Sweden (i.e. had small children) with those mothers who had older children and therefore didn't have access to the benefit when arriving to Sweden. Since it can be expected that also the ages of the children affect labor force participation, a difference-in-difference specification is used. The additional control group that had been added consists of mothers who immigrated earlier to Sweden and have given birth to all their children in Sweden. The data used in the analysis mainly originates from Statistics Sweden and the Public Employment Service.

The identifying assumptions are that only the age of the child makes the treated group different from immigrants who come with somewhat older children, and that the effect of child age on labor force participation is the same for all immigrants, irrespectively of when they immigrated to Sweden. If these assumptions are fulfilled the results can be given casual interpretations.

The prediction of the theoretical model is confirmed by the empirical analysis in Essay 1. Immigrated mothers have lower probabilities of being in the labor force up to seven years after immigration if they had access to parental leave benefits when they came to Sweden. If mothers have access to paid parental leave when they immigrate they are 7-8 percentage points less likely to be in the labor force 2 years after immigration. This effect is then slowly decreasing each year until year seven when no differences can be seen. The result is to some extent driven by unemployment since the effect on employment is smaller. But there is still an effect on employment rates two to six years after immigration of about 3 percentage points.

Childcare to unemployed

The second essay also concerns the labor supply of parents or a match between an unemployed parent and some vacancy. One explanation to the high labor force participation in Sweden among women is the access to universal childcare for all employed parents (Lundin, Mörk, and Öckert, 2008). For unemployed parents without childcare, taking care of their children may however be an additional obstacle to enter employment.

In July 2001 it became mandatory for all municipalities in Sweden to offer unemployed parents childcare for at least 15 hours each week. Before 2001 a majority of the municipalities offered childcare to unemployed parents, but not all did. The aim with the reform was mainly to make childcare available for the children, but the government also argued that it would be easier for the unemployed parents to find work (Swedish National Agency for Education, 1999).

Turning to the predictions from the theoretical model there is first no reason to expect the qualifications of the parent to change. The wage distribution faced by the parent will thereby not be affected.

When an unemployed parent gets access to childcare this will make it easier to search for work and meet potential employers. The search cost will thereby be lower and increase the search effort.

The increased search effort will increase the arrival rate of job offers, λ. Getting access to childcare already when unemployed will also make it possible to accept job offers right away instead of waiting for a childcare placement. Since it may take up to three months in Sweden to get a childcare place, not all employers may accept this delayed start if an unemployed do not have a childcare placement when unemployed. Given everything else equal, parents without childcare may then face a lower λ and receive fewer job offers. Giving unemployed parents access to childcare may therefore increase the number of job offers.

However, it will probably also increase their reservation wage if unemployed parents are offered childcare when they are unemployed. If the parent appreciate time at home without the child, the utility of being unemployed, b, will increase when the child is at childcare and thereby also increase the reservation wage. The utility of being unemployed may also increase if the parent appreciate that the child gets to childcare and meet other children or get high quality childcare. If the parent does not appreciate that the child is at childcare, there is no need for the parent to put the child in childcare. But then the reform does not provide any changes for the parent.

Another reason for an increased reservation wage is that the cost of searching, $a(s)$, will decrease, implying that the expected utility of continuing searching increase and therefore also the reservation wage.

So even if the reservation wage may increase when unemployed parents are offered childcare, and thereby lower the probability of a match, the possibility to increase the search effort and to accept job offers at once from all employers will increase the number of matches. The total effect can therefore go in either direction, or the countervailing effects can cancel each other out.

The empirical analysis in the second essay focuses on all parents with their youngest child between 2 and 10 years old and who registered at the public employment service between July 2000 and June 2002. The analysis is preformed using a triple difference specification. The first difference is over time, before and after the reform. The second difference is between municipalities, those who did not offer childcare before the reform and those who did. But since these municipalities may differ in other aspects as well, a third difference within municipalities but over age of youngest child is also added. Parents with older children, aged 6-10, have childcare irrespectively of the reform since their children are in pre-school and school. The triple difference specification is then used in a Cox proportional hazard model to study the effect of the reform on the parents' probability to find work.

The result in this second essay is that the probability of finding a job increased with 16 percent for mothers while no effect were found for fathers. Within the group of mothers the effects are heterogeneous. When it comes to education, the effects are U-shaped. Mothers with only compulsory school or any university education had a higher probability of finding work when childcare was available, while no effect could be found for mothers with a high school education of two years or less. Likewise, no effect could be found for mothers with only one child, while mothers with two children had a 32 percent higher probability of finding work when childcare was available during unemployment.

Workfare

The two last essays in this thesis examine the effects of workfare or activation requirements. In both these essays the unemployed only receives identical benefit levels as long as they agree to participate in some program. The aim of these programs are to increase the probability to find employment.

Simplified, we may think of two types of programs when workfare

are analyzed using the job search model. The first type has a specified duration and increases the qualifications of the unemployed, e.g. an active labor market program (ALMP), giving the unemployed some education, or some work practice where the unemployed get work experience. The second type consists of coaching or help in searching, thereby decreasing the cost of search, $a(s)$ and may either have specified duration or continue until the unemployed find some work.

The two types of programs will have some different expected effects, which also may vary depending on whether one studies the effects before, during or after the program.

Independent of which type of program the unemployed facing workfare will attend, the programs confiscate leisure and thereby increase the cost of being unemployed. The expected effects from the theoretical model is hence that if the unemployed do not want to participate in a program, he or she may increase the search intensity and lower the reservation wage before the program start, or in the beginning of a program. This pre-program effect will thereby make more unemployed to find work.

During a program, unemployed individuals participating in different types of coaching or search help programs are expected to increase their job search, since the cost of searching will decrease, and thereby increase the probability of getting a job offer above their reservation wage. For participants in ALMPs and work practice the programs will confiscate their time making it harder to search and thereby lower the probability of getting a job offer.

If the programs who aim at increasing the participants' qualifications are successful, more unemployed will find work after the programs ends since they may be offered jobs from a greater part of the wage offer distribution.

The expected effect of workfare is therefore that more unemployed will find employment for three reasons. The utility of being unemployed, b, will decrease and thereby lower the reservation wage and increase the search effort. For participants in ALMPs their qualification will increase which will increase λ and for participants in coach programs the cost of searching, $a(s_i)$, will be lower also increasing λ.

In the third essay (joint with Helge Bennmarker and Oskar Nordström Skans) we study an unemployment insurance reform in 1998 in Sweden that changed the time when long-term unemployed, aged 55 or 56, had to enter a program to receive identical benefit levels. Before the reform the unemployed in this age span received benefits without participation for 90 weeks. After the reform, time to program was changed to 60 weeks. The programs these unemployed individuals faced were mainly work practice that continued for 6 months but could also consist of ALMPs.

While young workers often face different types of workfare (see e.g. Forslund and Skans, 2006, and de Georgi, 2005), older workers often have extended durations of passive benefits (see e.g. Tatsiramos, 2010). There is no apparent theoretical reason for this division. Graversen and van Ours (2008) studies mandatory programs in Denmark and actually find largest effects for the old unemployed.

By studying unemployed workers that are slightly younger and slightly older than the unemployed affected by the reform we are able to find a good control group to be used in a difference-in-differences specification in a proportional hazard model. The data mainly comes from a register at the Public Employment Service and we follow all new unemployment spells (with some restrictions) that began between 1996 and 1999.

We find that the probability of finding employment increased by 11 percent, mainly before the program started (a pre-program effect). We also estimate the effect on post unemployment wages but do not find any effects of lower wages, indicating that the unemployed increased their search behavior. Workfare may thereby be a way to increase job finding rates among old unemployed without removing their benefits.

The final essay (joint with Anna Persson) does not study employment or unemployment directly as the outcome. Instead the focus in this essay is how workfare affects social assistance utilization, and especially if workfare mainly affects the entry to or exit from social assistance. The program studied is however targeted at unemployed social assistance recipients.

The reform studied was implemented in different years in different city districts in Stockholm between 1998 and 2004. The program implemented required unemployed social assistance recipients in Stockholm to visit a job center regularly several days a week. At the job center the recipients could search and apply for jobs. Since the aim of the program was to help the participants to search for work, there shouldn't be any lock-in effects for the unemployed[2]. This program was therefore mainly a program that decreased the costs of searching and confiscated their leisure, thereby decreasing the utility of being unemployed and receiving social assistance. The predictions from the theoretical model is therefore more matches between unemployed and work and reduced social assistance dependency.

The effect may be both that social assistance recipients find work and do not need any support longer (an exit effect) or that potential social

[2]Thorén (2005) studies one of the centers and conclude that the program mainly checked the participants willingness to work instead of helping them in searching and applying for jobs, but the conclusions from the model is the same.

assistance recipients search harder before they need to search for income support (an entry effect). In the long run, individuals may also get more education, to increase their qualifications and thereby the job offer arrival rate as unemployed, to decrease the probability of claiming social assistance.

In earlier work, Dahlberg, Johansson, and Mörk (2008) have shown that the same reform, reduce social assistance utilization. What we do in Essay 4 of this thesis is to study if the reduced utilization comes from reduced entry rates or increased exit rates.

The data, mainly originating from Statistics Sweden, include yearly information on social assistance benefits and individual characteristics as education, age and birth region. The population wide registers makes it possible to not only study individuals who are dependent on social assistance, and thereby only looking at exit effects as many earlier studies have done (for an overview see Blank, 2002), but also to study if the program affected the inflow to social assistance.

Since the reform was implemented at different points in time in different city districts we are able to use a difference-in-difference specification where those city districts that have not yet implemented the activation requirements can be used as controls for those city districts that have.

In the empirical analysis we found that the reduced utilization was due to increased exit rates rather than reduced entry rates. The effect is largest for young and unmarried individuals without children, a population that can be expected to be most mobile and therefore are able to search for work within a larger area.

In the end, it hence turns out that the empirical findings in all essays of this thesis are in line with the predictions from the theoretical model laid out in the beginning of this chapter.

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Essay I

Paid Parental Leave to Immigrants: An Obstacle to Labor Market Entrance?

1 Introduction

With an aging population in many countries, it is important to have high rates of labor force participation. Two groups among which labor force participation may be increased are women and immigrants. This paper studies labor force participation among immigrated women and how this is affected by a generous welfare system.

One way to increase female labor force participation is to have a flexible parental leave insurance together with job protection during leave, and then childcare availability after leave (Bennett and Tayler, 2006). However if the parental leave insurance is too flexible and generous it may create incentives to stay out of the labor force. This may be especially true for mothers with small but not newborn children, immigrating to a country where having children automatically makes the mothers eligible for the benefit.

In this paper I will evaluate how access to parental leave benefits (PLB) in Sweden affect labor market participation for immigrant women. Sweden has in general a very high female labor force participation rate[1]. There is also a very generous PLB system, where parents get 480 days of paid parental leave to be used before the child's eighth birthday. Most parents use a majority of the days during the child's first two years, but since it is

[1]In 2009 the labor force participation among women aged 15–64 was 77.7 percent in Sweden, compared to an average of 62.8 percent in the OECD countries.

possible to claim the days until the child's eighth birthday, it is possible for immigrants coming with older children to claim the benefit. The access to this benefit may be a smaller problem if it only delays the labor market entrance, but it is more problematic if the delayed entrance excludes these mothers from the labor market for a long time. Such exclusion is likely if their experience in the first year in a country is crucial for later outcomes.

When parents receive paid parental leave, they are not allowed to work or participate in any introduction program or language courses. Treatment may thus be seen as a composite effect of financial incentives and missing or delayed program participation. The outcomes studied will be both labor force participation and employment.

I perform the evaluation by studying mothers immigrating to Sweden between 2000 and 2005 (Late immigrants), comparing the assimilation process for those who had access to PLB when they received their residence permit (Treated group) with those whose youngest child was older than the age cut-off and therefore didn't have access to the benefit[2].

To be sure that the difference in labor force participation is not just due to differences in the age of the children, I control for the age of the children using an additional control group consisting of women immigrating to Sweden earlier and who give birth to all their children in Sweden. These women used most of their days of PLB during their children's first two years and will therefore not be able to stay home for long periods when the children are older, as can the treated group.

The identifying assumptions are that only the age of the child makes the treated group different from immigrants who come with somewhat older children, and that the effect of child age on labor force participation is the same for both Late immigrants and Earlier immigrants.

This paper contributes to two important strands of the literature: the effects of parental leave benefits and immigrant assimilation. Parental leave benefits have in many studies been shown to increase fertility (Lalive and Zweimller, 2009; Milligan, 2005; Björklund, 2007) and paid parental leave together with job protection have made it easier for mothers to stay home with their newborns and then return to their earlier work (Baker and Milligan, 2008; Bergemann and Riphahn, 2010; Ruhm, 1998). But for parents who are not attached to the labor market and arrived in Sweden with somewhat older children, this system may prevent them from entering the

[2]The data only include information on when the individuals register at the tax authorities after they received their residence permit in Sweden, not when they actually arrived. It is not possible to claim any PLB before this registration, which is why this registration date is preferred. For simplicity, I will use the words immigration or date for residence permit even if more correct would be, date for registration at the tax authorities.

labor market, an effect that is related to the other relevant literature about immigrant assimilation.

Starting with Chiswick (1978), the assimilation process among immigrants in different labor market outcomes, such as employment and earnings, have been studied by many economists. As pointed out by Borjas (1985, 1989), it is important to use panel data to evaluate immigrants assimilation patterns, since using cross-sectional data may capture differences between immigrant cohorts. Where Chiswick (1978) and Borjas (1985, 1989) study the assimilation pattern for American immigrants, there have been studies for many different countries (Amuedo-Dorantes and de la Rica (2007) for Spain, Clark and Lindley (2005) for the UK, and Longva and Raaum (2003) for Norway). The main conclusions from these studies are that immigrants have lower employment rates and earnings the first years when they immigrate to a new country, assimilate over time, but never reach the participation or wage levels of natives. The assimilation, however, differs, depending on gender, education, and origin.

When it comes to Sweden, there are two different studies of employment assimilation. Nekby (2002) finds that employment convergence between immigrants and natives occurs during the first 10–15 years after immigration to Sweden, but a significant difference from natives still remains after 15 years. Lundborg (2007) studies labor force and non-labor force immigrants separately, and finds that the former face almost immediate employment assimilation, while it takes approximately 20 years for the non-labor force immigrants to reach the same employment status as natives.

The assimilation pattern when it comes to welfare use differs between countries. In the US, immigrants increase their welfare use over time (Borjas and Trejo, 1991, 1993) while immigrants in Sweden assimilate out of welfare (Hansen and Lofstrom, 2003), but after 20 years the share of immigrants receiving welfare is about the same in both countries. The difference between the countries is probably due to the difference in their institutions. In the US, as shown by Bertrand, Luttmer, and Mullainathan (2000), welfare use is spread within social networks. Welfare use increases if there are many speaking the same language using welfare around an individual, and therefore it seem to be a behavior that can be learned. In Sweden, all refugees who receive a residence permit are offered social assistance for the first 18 months to be able to attend introduction programs and therefore get information about the welfare system, often before they have received a residence permit. After the large welfare reform in the US in the 1990s, immigrants were not allowed to collect welfare. This reform led to a sharp decrease in welfare recipients among immigrants in the US, but this reduction was only driven by California. In the rest of the country, many states offered state-funded programs to immigrants, or the immigrants became naturalized citizens and then got access to the benefits (Borjas, 2002).

The results in this paper show that labor force participation for mothers who had access to PLB when they came to Sweden is 7.7 percentage points lower two years after residence permit, compared to mothers with older children that did not have access to PLB. The difference then decreases to 3.6 percentage points lower participation rates due to PLB in year six, before the effect disappears in year seven. The effect of PLB on employment is about 3 percentage points lower, two to six years after the residence permit and then reduces to no effect.

The effect is larger for mothers with their youngest child between two and four than for mothers with five and six year old children. When performing a heterogeneous analysis by region of origin, no negative effect is found for mothers from the Middle East and Africa when it comes to employment, since few mothers, irrespectively of the age of the children, find work.

This paper is organized as follows: Section 2 describes some institutional settings in Sweden and the parental leave utilization by newly arrived immigrants. Section 3 describes the sample, the data, and descriptives of the outcomes, before Section 4 presents the econometric specification. Section 5 show the results and sensitivity analysis, which are finally then discussed in Section 6.

2 Institutional setting and parental leave benefit utilization

2.1 Parental leave benefits in Sweden

Sweden has a very generous system of paid parental leave. When a child is born, the parents can claim 390 days of paid parental leave to be home with the child. Of these days, 60 days are quoted for each parent.[3] The benefits correspond to about 80 percent of the parents' salaries up to a ceiling.[4] In addition to these days, the parents can claim an additional 90 days for which they are only paid 60 SEK (7 USD) per day. The system is very flexible in the sense that the parents decide for themselves for which

[3]For children born before 2002 the parents got 360 days. At that time only 30 days were quoted, making 330 days available for the mothers during the whole period.

[4]The ceiling increased from about 600 SEK (70 USD) per day in 2000 to 900 SEK (105 USD) per day in 2009.

days they want to claim paid parental leave, or even part of a day, making it possible to extend the leave to a very long period. The only restrictions are that the parent is not allowed to work and benefits are only paid out until the child attains the age of eight or finishes his or her first school year.[5] This is the basic structure but many workers have additional insurance in collective agreements. If a parent has no income or a very low income, the parent gets a fixed amount per day, which has been increasing over the years from 60 SEK (7 USD) before the year 2002 to 180 SEK (21 USD) from the year 2004 (Lindström, 2010).

Immigrants who come to Sweden with children aged below eight are eligible for the same benefits as those parents whose children are born in Sweden. This implies that even if the child is five years old when a family immigrates to Sweden, one of the parents is able to be at home and collect money from the parental leave system for over a year. Paid parental leave benefit days utilized in another country are removed from the potential days used in Sweden. Even if many countries in the world have some sort of paid parental leave, the number of days paid are seldom as many as in Sweden, except from mainly the other Nordic countries[6]. Most immigrants who have children when they come to Sweden and collect parental leave benefits get the fixed rate. The most obvious reason for why they get the fixed rate is that they don't have any employment and therefore no income the benefit could be based on. There is also an additional rule that makes it hard for immigrants to receive any higher payment for the first 180 days they collect benefits. According to this rule, the benefits for the first 180 days are only based on the current income if the parent had an income during the 240 days preceding the birth of the child.[7] This rule makes it even harder for immigrants to get higher benefits than the fixed rate for the first 180 days.

2.2 Immigrants' first time in Sweden

In Sweden the composition of the immigrant group has changed over the years. After World War II, immigrants coming to Sweden were mainly labor force immigrants, but during the 1970s, immigrants due to labor market reasons were replaced by refugees and immigrants due to humanitarian reasons. This change in immigrant composition makes the assimilation much slower today than earlier (Eriksson, 2010; Lematre, 2007).

When immigrants receive a residence permit in Sweden, they have to

[5] It's not possible to collect parental leave benefits if the child is in school or childcare.

[6] Immigrants from Nordic countries will therefore be excluded in the analysis.

[7] Only income in Sweden, other EU, or EES countries counts.

register at the tax authorities and are then eligible for social security benefits of which the paid parental leave benefits are one part. All individuals with a residence permit in Sweden are also eligible for social assistance from the municipalities if they don't have any other possibility of supporting themselves. This implies that immigrants who arrive in Sweden can get social assistance if they have no job or assets. The municipalities may, however, require recipients of social assistance to participate in different activation programs. For refugees, this will be the introduction programs, see below. The main part of social assistance is called income support and consists of a standard plus the cost the individual has for housing. Although the municipalities are responsible for the social assistance system, the lowest level of the standard is decided by a national norm, which in practice has been the benefit level in many municipalities.[8]

During the studied period, however, the municipalities had another option when it came to refugees. Instead of paying social assistance to refugees, the municipalities could pay introduction benefits. The aim of these benefits was to encourage refugees to participate in introduction programs and increase the responsibility for their own finances. The motivation for the programs that was introduced in 1993 was that many refugees, instead of only receiving social assistance temporarily, stayed on benefits for many years. The idea with introduction benefits was that these should be somewhat higher than the social assistance and not means tested. In practice, even if many municipalities introduced introduction benefits to refugees, this was only by name and in reality these benefits worked in exactly the same way as social assistance in most municipalities (SOU 2003:75).

All municipalities in Sweden have introduction programs for newly arrived refugees who have received a residence permit. These programs mainly consist of language training courses (SFI) which also are available for all grown-ups that don't have a basic knowledge of the Swedish language. Normally, the introduction program should be two years and the refugee should start a program within one year after receiving a residence permit. But the program is not mandatory and the programs can be extended if it's necessary for the individual.

The access to PLB for immigrants with children is a potential problem for maternal labor market attachment, which has been discussed in Sweden. This discussion started with a report from The Expert Group on Public Economics (ESO) in the summer of 2011 (Olli Segendorf and Teljosuo, 2011). The purpose of the report was to draw conclusions about initiatives and measures to improve integration in Sweden. The report

[8]The level of the norm for two types of families are shown in Figure 8 in subsection 5.2.

discussed how both general and targeted policies affect labor market entrance for the foreign-born. When it came to parental leave insurance, the authors concluded that this insurance reduces the incentives to work and creates lock-in effects.

The problem was then raised by many politicians and in October 2011 the government initiated an inquiry to investigate how labor market attachment among newly arrived female immigrants may increase. As a special part, the inquiry was to make a survey of the PLB claims of recently arrived women and men. The inquiry studied the claims from all parents who arrived to Sweden in 2006 with children aged below eight. Among the women who were born outside Europe, there were 40 percent who claimed PLB for at least 200 days the year after their arrival. Of those women who gave birth to additional children in Sweden and arrived from countries outside Europe, 25 percent claimed over 200 days of benefits for two consecutive years and 10 percent for three consecutive years. However, for the women born outside Europe who did not give birth to any more children in Sweden, 25 percent did not claim any PLB. Surprisingly, even 7 percent of those who gave birth to new children in Sweden did not claim any days. Some of these may have emigrated again (SOU 2012:9).

From questionnaires to the municipalities, who deal with social assistance, the inquiry also found that many municipalities require immigrants who need social assistance to claim PLB if they have days left to claim before they get social assistance. This means that parents who are unemployed and therefore need social assistance get excluded from the labor force and have to take care of their children instead of joining language courses or searching for work[9].

In September 2012 the Swedish government announced that they will propose a law change, putting a restriction on the parental leave insurance that 80 percent of the available days have to be utilized before the child's fourth birthday. The motivation for this law is to increase labor market attachment for newly arrived immigrant mothers.

2.3 Parental leave utilization among immigrants

As a first step in the analysis, this subsection describes the utilization of PLB by mothers immigrating to Sweden between 2000 and 2005.

The data used are mainly registers from The Swedish Social Insurance Agency and contains information about PLB utilization, such as which

[9]In July 2001 it became mandatory for municipalities to offer unemployed parents in Sweden chidcare for at least 15 hours each week, but a majority of the municipalities offered childcare to unemployed even before this reform (Vikman, 2010).

days a parent has claimed the benefit for and how much money the parents have been paid. By parent ID, it is possible to link the PLB data to some other register data to find the month of birth of biological children, and yearly data (available from 1985) containing individual characteristics such as country of birth and latest immigration year. With these data it is possible to find mothers immigrating to Sweden between 2000 and 2005 with children born outside Sweden, and where the children at immigration were between one and seven years old[10].

It is not surprising that mothers of newborns stay home with them, which is why I choose to study PLB utilization by age of the youngest child at immigration and only show PLB utilization for mothers with their youngest child between one and seven. I only follow mothers until they give birth to a new child, for the same reason. Mothers who have a new child will be included in the main analysis since the decision to have a new child may be endogenous to access to the benefits.

Figure 1 shows the parental leave utilization for mothers immigrating to Sweden between 2000 and 2005 with their youngest biological child between one and seven years old[11]. The first figure shows the share of mothers (who have not given birth to a new child) utilizing the benefit in the year of residence permit (year 0) and the following two years. For example, looking at mothers who came to Sweden between 2000 and 2005, whose youngest children then were five years old, less than 20 percent claimed PLB during the year of immigration, but in year one, about 35 percent and in year two, 38 percent claimed the benefit.

The second figure shows how many days on average the mothers claimed PLB (of those who utilized the benefit). For mothers coming to Sweden with five year old children, the figure shows that those who claimed PLB did it for 120 days on average in the year of immigration, 180 days in year 1, and 125 days in year 2.

As can be seen, a larger share of mothers with younger children claimed some benefit compared to mothers with older children all years and more mothers claimed the benefit in year 1 compared to year 0. In these figures it is not possible to see how many mothers who claimed PLB for just one year and how many claimed for several years but an overall measure is that of all these mothers there were 43 percent who claimed some PLB

[10]In my main analysis when I evaluate the effect of having access to PLB, the data come from another source with some variables in common, but the greatest difference is the immigration data where I have all registered in and out migration since 1985 giving me a somewhat different sample.

[11]Mothers from other Nordic countries are excluded since they have access to many days of PLB in their home countries and the number of days they used in their home countries are taken away from the possible days to claim in Sweden.

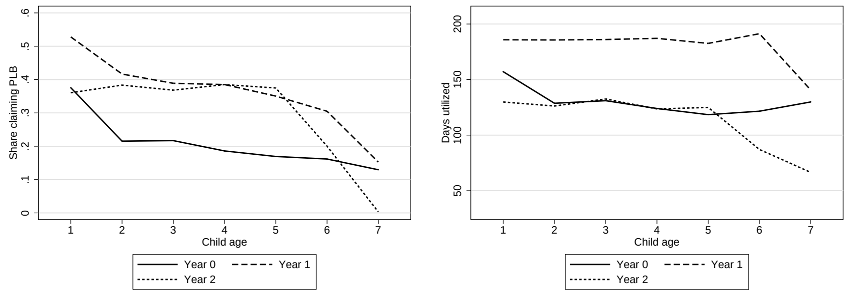


Figure 1: Share utilizing the PLB of late immigrants and the average days claimed, year of immigration and the following two years.

during the year of migration or the following two years for children they had when immigrating to Sweden.

For mothers coming to Sweden with their youngest child seven years old it shouldn't be possible to collect any PLB in year 2, since the child turns eight in year 1, and as can be seen, almost no mothers of seven year old children utilized the benefit in year 2. An explanation to why this number is not exactly zero (easiest to see since there is a value for average days in the second figure) is that it is only possible to determine whether the mother has biological children and the birth month of those in the data but a mother may have non-biological children she claimed PLB for. This error may create some measurement errors in my main analysis if Late immigrant mothers with older children have younger non-biological children to use PLB for. This potential error, if it exists, causes attenuation bias.

The mean number may seem high since mothers are only able to collect 420 days in total for each child (480 days if they are single parents) but the mothers are able to claim parental leave benefit days for all children below the age of eight and may therefore collect benefits for several children.

Almost all mothers who utilized the benefit got the lowest fixed amount. In year 0, 98 percent of the mothers claiming PLB got the lowest fixed amount. In year 1 the share was 97 percent and in year 2 decreased somewhat to 85 percent.

Even if the benefits are for both parents and some part of the benefit is quoted for each parent, the share of immigrating fathers utilizing the benefit is lower (not shown). Among immigrating fathers coming to Sweden with their youngest child between one and seven years old, 24 percent claimed the benefit sometime during the year of immigration or the following two years.[12]

Table 1 shows the utilization of the PLB for some of the mothers who will be used in the control group of Earlier immigrant mothers. Data on utilization is only available from 1994, which is why I am only able to show the utilization for mothers with children aged 2–6 years, even if those with older children also had access to the benefit since their children were born in Sweden. Since only the latest immigrating year is available in the parental leave data, I am only able to find about two-thirds of the control population of early immigrant mothers with children aged 2–6 years. As seen in Table 1, as expected, almost all used the benefits during the year of birth and the following two years, and about 47 percent got the fixed amount sometime during these years.

Table 1: Parental leave utilization among early immigrants with young children (aged 2–6)

	Year of birth	Year 1	Year 2	Years 0–2	*N*
Share collecting benefit	0.908	0.896	0.332	0.965	22038
Mean number of days	171	190	58	357	21269
Share receiving fixed amount	0.451	0.427	0.205	0.468	21269

To conclude this section, we have seen that far from all immigrating mothers who received a residence permit in Sweden with children aged below eight (i.e. eligible for PLB) used the benefits. Still there was a substantial share that used the benefits at least to some extent, and many immigrants who used it to such extent that it made it unlikely for them to be able to attend language courses or other introduction programs. Almost all mothers who immigrated between 2000 and 2005 and used the benefits recieved the low fixed amount.

3 Sample and data description

In this section, I start by describing how I define the sample (3.1) before the data that will be used in the analysis is summarized (3.2). I then continue, in subsection 3.3, by looking at the share of immigrants starting a language

[12]Even if mothers use the PLB to a greater extent than fathers, the PLB may still have an effect on fathers' labor force participation. The analysis has also been performed on fathers but no clear effects could be found, mainly insignificant results, which is why this paper focuses on immigrant mothers.

course within five years, in order to investigate whether participation differs depending on the age of the children. Finally, subsection 3.4 describes the outcomes that will be used and shows some first descriptive results.

3.1 Sample description

To evaluate how access to parental leave benefits (PLB) affect labor market entrance, I study two groups of women who have immigrated to Sweden. The first group is mothers who immigrated to Sweden between 2000 and 2005, whose youngest child then was between two and 15 years old, and who did not give birth to a new child within nine months after their immigration. I will call this group, *Late immigrants*. In the data the date immigrants register at the tax authority after they received a residence permit is available, not the date when they arrived. This registration makes it possible to claim the Swedish social insurance, of which the parental leave benefit is one part, and is therefore the date of interest, even if some mothers arrived in Sweden earlier and therefore had the opportunity to make contact with potential employers before they registered[13]. For simplicity, this date is referred to as the *date for immigration* or *residence permit*.

The reason why I do not include mothers with younger children is that municipalities do not offer childcare until a child has reached the age of one. Therefore there is no real alternative for one parent than taking care of the child until the child's first birthday and thereby, for some part of the year, at least one parent is not able to work. In the group of late immigrants, those with their youngest child between their second and sixth birthday in the year of immigration will be considered as *treated*, while those with older children (7–15) are used as the first *control group*. The reason why I cut between six and seven, even if mothers are able to claim the PLB if the child was up to eight years old, is that all children in Sweden start school the year they turn seven. So even if the mothers got access to the benefit, they were not able to collect it when the children were in school and were therefore able to attend language courses or search for work.

Since it is likely that the age of the child affects mothers' labor force participation, an additional control group is needed. This second group of women in my sample consists of women who received a residence permit in Sweden between 1985 and 1995 and gave birth to their first child after they received their residence permit, referred to as *Earlier immigrants*. To make up a good control group I want to have a group of immigrants that

[13]There would be a problem if the time waiting for a residence permit were different depending on the age of the children. This is, however, not the case.

have not spend a long time in Sweden, since I do not want them to be too different from Late immigrants. At the same time, they must have had time to have children in Sweden. The migration data are also much more detailed from 1985, before this year I only have latest immigration year. To have comparable mothers when it comes to age of the children, the Earlier immigrants have to have had their youngest children aged between 2–15 years between 2000 and 2005.

This construction of the sample implies that there is an inflow of Late immigrants with children between two and 15 in every year between 2000 and 2005, while for earlier immigrants, there is an inflow of mothers with their youngest child between two and 15 in 2000. Between 2001 and 2005 only Earlier immigrant women with a youngest child that is two enter the sample. The sample is summarized in table 2.

Table 2: Sample description

	Children Born:	Age of youngest child 2-6	7-15	Immigration Year
Late Immigrants	Outside Sweden	Treated group	Control	2000-2005
Earlier Immigrants	In Sweden	Additional Control		1985-1995[a]

[a] These year will be varied in the sensitivity analysis.

Two groups of immigrants are excluded from the analysis, immigrants from other Nordic countries and those who do not have any citizenship or where the Swedish government does not know the immigrant's origin. Since the other Nordic countries also have many days of paid parental leave, mothers coming to Sweden from these countries are not treated since they had access to PLB even in their home countries, and days utilized in other countries are removed from the days available in Sweden. Unfortunately I am not able to see the specific country of origin in the data, since countries are grouped, and are not able to remove mothers from other countries who also pay many days of paid parental leave, e.g., Slovenia (Moss and O'Brien, 2006). What I have observed in the parental leave benefit data is that for all groups of countries, there are mothers that used the benefit. The second group which is excluded consists of immigrants where the origin is unknown, which is a very small group, and does not affect the estimations if included.

3.2 Data description

The data used in this paper are all drawn from population-wide registers in the IFAU database. The data mainly originate from Statistics Sweden but also unemployment records from the Public Employment Service (PES) are used.

More specifically, to pick out the sample, two main data registers were used. The first contains all registered migration data since 1985 and was used to find the initial immigration date. Even if this is far from a perfect register, since not all emigration is registered, the first time they come to Sweden will be included, since they need to register to get a Swedish ID to be able to have contact with the authorities or employers.

The second register is a multi-generation register linking all parents with their children and thereby providing me with the birth month of the children. It is not always the case that the Swedish authorities are able to get the exact birthday for all immigrants, which is seen in the data since many immigrants having January 1st or July 1st as their birthday. But even if the this date is not the exact birthday, the date given will be the date that controls when a child starts school and how long the parents are able to claim PLB. I also have access to a register with a rough categorization by country of birth, which makes it possible to exclude mothers who are born in Sweden but have given birth to their children in another country.

To get information about the background characteristics, an income and population wide register (Louise) was used. Louise contains yearly data of all transfers to individuals but also information about education and age, and it links individuals in the same household to each other. For the different outcomes, the Louise database and data from the PES register (Händel) were used. The PES register contains spell data of when unemployed register at the PES and why they leave (work, studying, other authorities etc.) but also what labor market programs they attend and for how long.

I also have records from the Swedish language course (SFI) showing how long a time it takes before immigrants start taking a language course, see subsection 3.3. I am not able to link the parental leave data, shown in subsection 2.1, to these other data registers, and therefore don't know which mothers claim PLB.

Mothers are followed until 2009, untill they leave Sweden, or turn 65, which is the most common retirement age in Sweden.

The effect of having access to PLB when immigrating to Sweden is probably different for different mothers. Two groups who could be expected to be affected differently are refugees and other immigrants. Unfortunately, I don't know the reason why an individual received a residence permit in Sweden. Immigrants from Eastern Europe, Africa, and Asia are however more likely to have received residence permits as refugees[14]. As mentioned, the data, however, contains a rough categorization by country of birth, which makes it possible to divide the sample into different sub-populations depending on where the immigrants came from. This division will be Western Europe, Eastern Europe, the Middle East and North Africa, Sub-Saharan Africa, Asia, and the last group contains North and South America together with the South Pacific. Heterogeneous analysis will be performed by region of origin but also by child age, educational level, and for single mothers and cohabiting mothers separately.

There would be a problem for the analysis if many immigrants came to Sweden just to claim PLB. Figure 2 shows the distribution of immigrant mothers coming to Sweden between 2000 and 2005, by age of the youngest child. There are more mothers who immigrate to Sweden with younger children but reassuringly there are fewer mothers for each child age, even for older children, and no big jumps around age seven when the time to claim PLB ends.

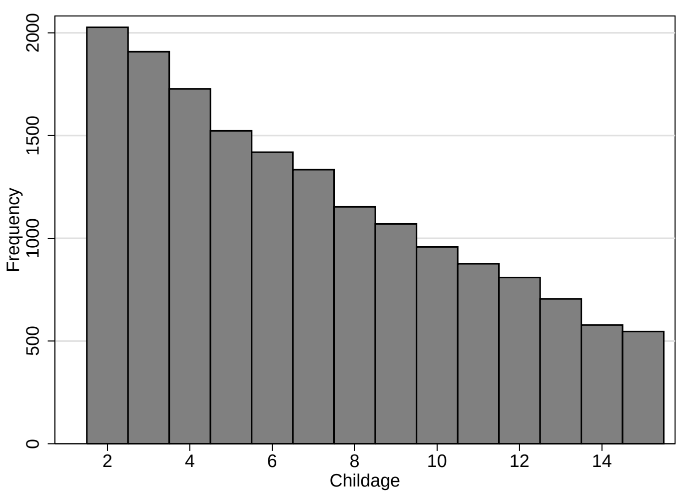


Figure 2: Distribution over child age for late immigrant mothers.

Table 3 shows the descriptive statistics for mothers. Late immigrants

[14]This group is hereafter referred to as the *refugees* even if not all of them have received residence permits as refugees.

are in the first two columns, and Early immigrants are in the last two columns.

Table 3: Sample means for mothers

	Late Immigrants		Early Immigrants	
Age of youngest child	2-6	7-15	2-6	7-15
Age	32.4	38.3	32.0	37.8
Child's age	3.8	10.3	3.0	9.0
Number of children	2.2	2.0	1.9	1.6
New child	0.362	0.148	0.408	0.135
Year between child and new child	6.4	11.8	5.7	11.5
Number of new children[a]	1.3	1.2	1.3	1.2
Share emigrating from Sweden	0.174	0.110	0.066	0.060
Time to leaving Sweden in years[b]	3.4	3.3	4.3	4.1
Other censoring[c]	0.004	0.009	0.004	0.008
Less than compulsory school	0.141	0.149	0.101	0.103
Compulsory school	0.054	0.046	0.158	0.126
Up to 2 years of High school	0.079	0.091	0.192	0.221
Up to 3 years of High school	0.105	0.117	0.230	0.192
Tertiary, less than 3 years	0.121	0.124	0.120	0.147
Tertiary, more than 3 years	0.218	0.222	0.159	0.186
Doctoral studies	0.024	0.025	0.012	0.011
Western Europe	0.095	0.066	0.049	0.072
Eastern Europe	0.273	0.366	0.265	0.326
N. Africa and the Middle East	0.319	0.270	0.370	0.277
Sub-Saharan Africa	0.068	0.045	0.100	0.052
Asia	0.164	0.173	0.131	0.171
N. and S. America and the S. Pacific	0.080	0.080	0.086	0.103
Descriptive statistics for mothers living with a partner first year[d]				
Share living with partner	0.791	0.677	0.791	0.651
Swedish-born partner	0.078	0.092	0.184	0.211
Partner immigrated:				
more than 5 years earlier	0.078	0.104	0.546	0.431
1–5 years earlier	0.209	0.157	0.058	0.006
same year	0.425	0.324	0.004	0.003
Observations	8604	8029	32429	7431

[a] Of parents who have more children. [b] Of parents who emigrate from Sweden. [c] Including parents reaching the age of 65, dying, or leaving the register for unknown reason. [d] First year is the year of immigration for (late) immigrants and the first year of analysis for the control group with early immigrants.

As seen in Table 3 the control group consisting of Earlier immigrant mothers has a lower mean child age, especially for mothers with older children, compared to Late immigrants. This difference is due to the restrictions put on this group, that they have to have had all their children after they received a residence permit in Sweden[15].

For about 25 percent of the late immigrants, the highest completed education is missing in the data. This is also the case for some of the earlier immigrants. When people receive a residence permit in Sweden, Statistics Sweden sends mail to the newly arrived, asking for their education, but not all of them answer that mailing. The share of those who, in their first year of immigration, reported their education, is even less. To increase this share, I have, to replace the information that is missing in the first year, taken as the education that was reported in the year after immigration. What can be seen is that despite the missing information, the educational level reported is somewhat different from the control group. Earlier immigrants mostly have education in the middle of the distribution, while late immigrants have higher shares both in the bottom and the top of the distribution.

In all groups, most mothers are living with a partner and, as expected, these partners are more likely to have immigrated at the same time as the mothers.

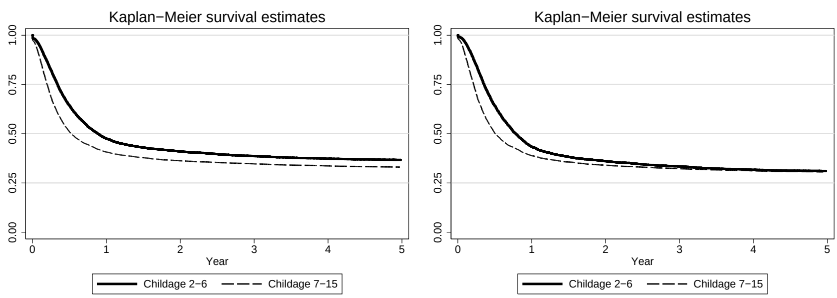
Table 3 also shows that a substantial part of the late immigrants leave Sweden within a few years. As discussed by Edin, Lalonde, and Åslund (2000), the emigration of immigrants is probably not random, which causes bias in the estimates of assimilation. If those immigrants who have the least attachment to the labor market leave, assimilation will appear to be larger than it is. In this paper, when I am comparing the assimilation pattern for two groups, bias arises if the emigration pattern is different between these groups. Therefore a sensitivity analysis without those who leave will also be performed.

[15]This restriction is because I do not want them to have been treated when they immigrated to Sweden, that is, have been able to collect parental leave benefit for a child born outside Sweden. However, the children may still have been born outside Sweden since I do not restrict them to have stayed in Sweden for the entire span of time since their first immigration, as this reduces the sample size. But even if there is a risk of them being treated, they immigrated to Sweden before their first child and therefore had the possibility of attaching themselves to the labor market before they gave birth to their children.

3.3 Language course

As mentioned in subsection 2.2, all refugees are offered language training courses when they come to Sweden and these courses are also available for all grown-ups that don't have basic knowledge of the Swedish language. In this subsection, figures of time to starting a language course is shown and I study if this differs depending on the age of their children. The reason for focusing on language courses is that Dustmann and Fabbri (2003) and Ferrer, Green, and Riddell (2006) show that language proficiency often is crucial for becoming established in the labor market. Immigrants who attend the language course in Sweden (SFI) have 5 percentage points higher employment 10 years after immigration than comparable immigrants who didn't attend the language course (Kennerberg and Åslund, 2010).

Figure 3 shows the Kaplan–Meier estimates for language course participation, that is, the share who haven't started a language course after obtaining their residence permit, by age of children among late immigrants. The first figure is for all late immigrant mothers in the sample (a), while the second figure is for refugee mothers (b).



(a) All mothers

(b) Refugee mothers

Figure 3: Survival until language course.

As seen in the figure, mothers with younger children begin the Swedish language course later and fewer attend the course, compared to those with older children (a). There may be several explanations for this difference, not only that those with younger children are able to stay home and collect parental leave benefits. Even in the absence of parental leave benefits in Sweden, the age of a mothers's child may still affect the participation rate.

For the sub-sample of immigrants that are more likely to be refugees, the patterns are a little bit different (b). After five years, about 70 percent have started a language course, irrespectively of the age of the youngest child, but those with younger children start later.

3.4 Labor force participation and employment

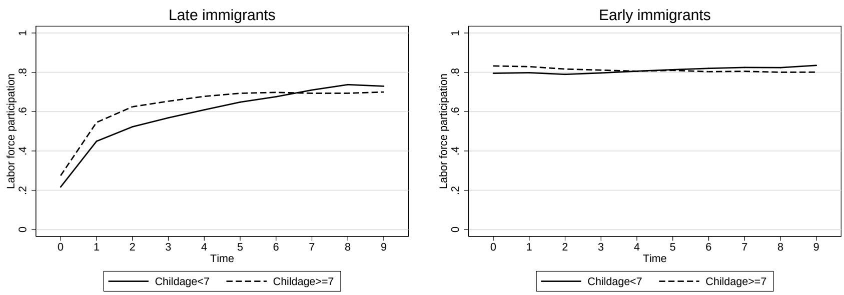
Many earlier studies of immigrant assimilation have studied earnings assimilation (Borjas, 1985, 1989; Clark and Lindley, 2005; Longva and Raaum, 2003; Edin, Lalonde, and Åslund, 2000). But before an individual has employment, and thereby some earnings, the decision to enter the labor market has to be made; and not all who choose to try to enter the labor market find employment. Therefore I will study both labor force participation and employment in this paper. While labor force participation includes all individuals who want to work, employment show those who have been able to find work. Since many municipalities require recipients of social assistance to register at the PES, a labor force measure may also capture individuals without any possibility of finding work, why both labor force participation and employment is interesting to study.

There are different ways of defining employment and labor force participation. In the data, yearly income from work and days registered at PES are available. But when should we consider an individual to be part of the labor force or employed? Is it enough to just earn a small amount of money during a year to be seen as employed that year, or is it necessary for the individual to earn enough to support themselves during the whole year to be considered as employed? The same considerations can be made when it comes to unemployment and thereby the definition of being a part of the labor force. Since many mothers in Sweden only work part-time, I will use rather low thresholds for employment and unemployment.

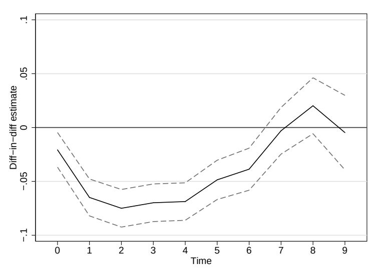
A mother will be considered as employed if she earns at least one month of minimum wage during a calendar year[16]. To be able to define labor force participation, I add a threshold for unemployment and this will be at least 30 days registered at the Public Employment Service (PES). But do all the unemployed register at the PES? There are several reasons to register at the PES. For the unemployed with a working history, this registration is mandatory to receive UI. Even if this is not a reason for newly arrived immigrants, they have to register to be able to take part in active labor market programs. If they need social assistance it is also in the municipalities' interest to require them to register since the PES then can help them find work and be able to support themselves. Mörk and Liljeberg (2011) also show that a large share of recipients of social assistance in 2009 is registered at PES, which is especially true for immigrants and young people.

[16] The minimum wage is calculated as the 10th percentile in the overall wage data (monthly fulltime wages) using data from the Structure of Earnings Statistics and varies between 14275 SEK (≈1680 USD) in 2000 and 19403 SEK (≈2280 USD) in 2009.

Figures 4 (a) and 5 (a) show labor force participation and employment according to these definitions for Late and Early immigrant mothers each year of analysis, where year 0 is the year of residence permit. Mothers are also divided by age of youngest child in the beginning of the analysis.



(a) Labor force participation



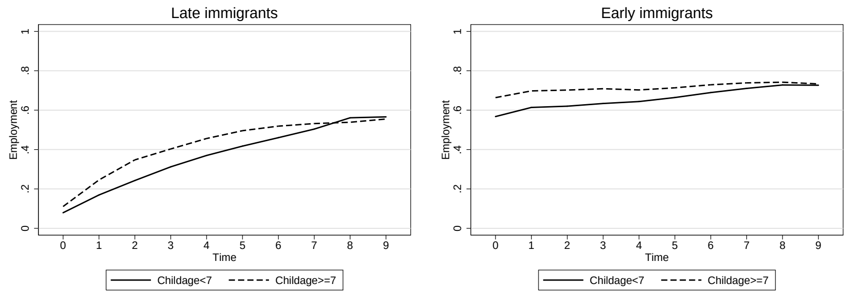
(b) Raw diff-in-diff estimates

Figure 4: Share in labor force and difference-in-difference figure.

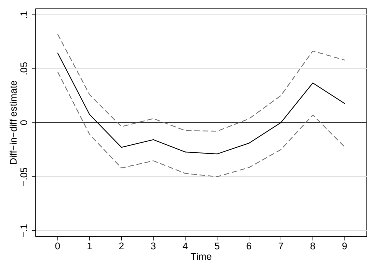
Starting with labor force participation in Figure 4, we see that Early immigrants have participation rates of about 80 percent. In the first years, mothers with older children participate to a greater extent, but from year 4 this changes. Late immigrant mothers have very low participation rates the year of migration (year 0), but after one year, the rates are much higher: around 50 percent, even if mothers with older children participate to a greater extent.

In Figure 4 (b), a diff-in-diff estimate of the four groups in (a) for each year is shown, with 95 percent confidence intervals. These diff-in-diff estimates show a raw measure of what cannot be explained by immigration and the age of the children. The differences between these estimates and the results from the estimations later is that these mean values do not take different child age compositions and different years into account. These diff-in-diff estimates indicate that access to PLB reduces labor force participation for some years, but from year 7 no difference can be seen.

When it comes to employment in Figure 5, early immigrant mothers with younger children have lower employment rates at all times than mothers with older children. For late immigrant mothers, the employment rates are low for the first years and do not approach 40 percent until year 2 for mothers with older children.



(a) Employment



(b) Raw diff-in-diff estimates

Figure 5: Share employed and difference-in-difference figure.

The diff-in-diff estimates for employment, shown in figure 5 (b), are negative and significant between year two and six but smaller in magnitude than the diff-in-diff estimates for labor force participation.

Mothers immigrating to Sweden seem to face obstacles to entering the labor market in their first years, irrespectively of whether they have access to PLB or not. Since the labor participation rates are so much lower in the year of immigration, Early immigrants may not be a good control group for that year, which is why estimates for the first year probably should not be given a causal interpretation. For employment, the same is true for the year of immigration and the first year after. The diff-in-diff estimates shown in Figure 5 (b) are also large and positive in years 0 and 1, indicating that the effect shouldn't be seen as causal until year 2.

4 Econometric specification

To answer the question how access to PLB affects labor market participation it is clear from subsection 3.4 that it is not possible to simply compare the participation rates for mothers coming to Sweden with different ages of their children. The reason is that also the age of the individual's children affect the outcomes. The following difference-in-differences specification will therefore be used in the estimations[17]:

$$\begin{aligned} y_{it\tau} = &\sum_{\tau} \beta^{\tau} D(Time_{it} = \tau) + \sum_{a} \beta^{a} D(Childage_{it} = a) \\ &+ \sum_{b} \beta^{b} D(Year_{t} = b) + \sum_{\tau} \delta^{\tau} PLB * D(Time_{it} = \tau) \\ &+ \beta' \mathbf{X_i} + \varepsilon_{it\tau} \end{aligned} \tag{1}$$

where $y_{it\tau}$ is the outcome variable of interest for individual i, year t, τ years after their residence permit.

$D(Time_{it} = \tau)$ is an indicator variable that equals one if it is τ years since individual i immigrated, and is always zero for individuals in the control group of earlier immigrant mothers. β^{τ} thereby captures the assimilation process for immigrants and shows how fast they assimilate to the control group already living in Sweden.

$D(Childage_{it} = a)$ equals one if the youngest child at immigration, or first year of analysis, is a years old in year t and thereby captures the effect the age of the child has on labor market participation. Since the decision to have more children is endogenous, controls for new children are not included in the main analysis, but to be sure that it is not immigrants who gave birth to new children that drive the results, additional controls for new children will be included in one of the sensitivity analysis.

$D(Year_{t} = b)$ equals one if it is year b, and hence captures the business cycle.

Finally, PLB equals one for those who have a child under the age of seven when they received their residence permit and therefore δ^{τ}, the parameter of interest, captures the difference in assimilation between those immigrants who have or had access to parental leave benefits at immigration, after controlling for the age of the child.

[17]Since the treatment depends on the age of the children, some may think that a regression discontinuity approach would be appropriate. There are several reasons why an RD does not work. Treatment is not sharp at the age discontinuity, instead it is fuzzy in one direction since parents coming with seven year olds are not able to use all the days before the child turns eight. Since seven year olds also will attend school, it is even harder to say who will be treated.

To investigate how much individual characteristics affect the results and to increase the precision of the results, some additional control variables will also be added (X_i) including seven dummy variables for education, five dummy variables for the number of children, nine dummy variables for the different age groups of the mother, six dummy variables for the region of origin, 21 dummy variables for the county the individual lives in during the first year of analysis, and four dummy variables for partner status the first year of analysis[18]. The variables used are defined in Appendix A.

The identifying assumptions are that only the age of the child makes the treated group different from immigrants who come with somewhat older children and that the effect child age has on labor force participation is the same for both Late immigrants and Earlier immigrants.

Are there ways to examine whether these assumptions are plausible? The first assumption is connected to why people with children immigrate to Sweden in a certain year and whether the reasons depend on the age of the children. For refugees, there is less reason to expect that there is any difference between parents with younger and with older children. To receive a residence permit in Sweden for refugee reasons, it's the conditions in the home country that determines the decision, not the age of the child. For non-refugees however, the reason may be family connections or labor market reasons, since an older child probably has stronger connections to the home country there may be unobserved differences between immigrants with younger and older children. Since refugees are more likely to come from certain regions, a heterogeneity analysis will be performed by region of origin. Parents with younger children may also plan to come and work in Sweden for a few years and then return to their home country when it is time for the child to start school. These parents will then emigrate after a few years and, as mentioned above, emigration may then cause biased results if this emigration changes the composition of the groups. Therefore, also estimations without immigrants who leave Sweden will be performed in the sensitivity analysis.

It may be hard to find a good control group to control for child age and thereby fulfill the second assumption. Does the child's age affect labor force participation in the same way for those mothers coming to Sweden as those mothers already living in Sweden? Labor force participation among mothers differs in different countries. This may be due to both values being connected with raising a child and working in each country, but also if the various institutions in the country make it easier for mothers to combine

[18]These include whether the partner was born in Sweden, immigrated more than five years earlier, immigrated 1–4 years earlier, and immigrated the same year.

work and family life. Values and institutions are probably correlated and affect each other. In Sweden many mothers work, facilitated by access to childcare and the possibility of working part-time when children are small. All mothers in Sweden naturally face the same institutions, irrespectively of when they immigrated. The family–work values may however differ, even if the mothers come from the same region and this will they compromise the second assumption. Even if a mother wants to be home and take care of the household, she may be forced by the authorities to search for work if the family is not able to support themselves and needs to rely on social assistance. In that case, the Swedish institutions will probably affect the labor force participation more than the values of the mothers.

The effect of child age may be correlated with the degree of connection to the labor market. If the child's age has less effect on labor force participation when the mother already has some contact with an employer, the control group of earlier immigrants may not be able to fully control for child age and the estimates will be negatively biased.

There is also a constructional problem within the group of earlier immigrants. With the restriction that this group has to have immigrated before their first child but no later than 1995, the older the children are, the longer the immigrants have been in Sweden, creating stronger labor market attachment depending on the child's age. This may lead to a greater difference between mothers with older and with younger children than which is due to the age of their children. The early immigrant mothers will thereby overcompensate for the effect of the child's age and produce positive biased estimates. To reduce this specific constructional problem and to evaluate if the second assumption is fulfilled, two different sensitivity analyses will be done. The first is conducted with immigrants who have immigrated sometime before 1990. The second sensitivity analysis will be done with a control group consisting of Swedish-born mothers. If these estimations yields similar results it is less likely that the effect child age has on participation rates differs between different populations of mothers in Sweden.

Another problem when controlling for child age appeared when studying labor force participation and employment in subsection 3.4. Even if late immigrant mothers want to work, they may not have access to the labor market from the beginning. Controlling for child age may therefore overestimate the effect of access to parental leave if mothers among late immigrants are excluded from the labor market their first years in Sweden. The figures in subsection 3.4 suggest that for labor force participation, the group of early immigrants is a good control group from year 1, while for employment, it is not until year 2 that the estimates should be given causal interpretations.

5 Results

In this section, the estimation results will be presented. First I will show (subsection 5.1) the main results for the effect of access to parental leave benefits (PLB) on both labor force participation and employment. The next subsection will discuss how the results should be interpreted (5.2). Subsection 5.3 will try to look at the effect of only the economic incentives. I then continue, presenting the results from a sensitivity analysis in subsection 5.4 and the results of the heterogenous analysis in subsection 5.5. All subsections will begin by studying the labor force participation outcome before studying the employment participation outcome, since mothers first face the decision to enter the labor force.

5.1 Main results

Figure 6 shows the estimated effects of access to parental leave benefits (PLB) on labor force participation, with 95 percent confidence intervals, each year after immigration.[19] This is δ^{τ} in equation (1), where τ goes from 0 to 9. Reassuringly, the estimates are similar both with and without the additional control variables, indicating that the earlier immigrants are good controls. The results show that the year after immigration, mothers who had access to PLB had a probability of being in the labor force about 6.5 percentage points lower than that of mothers immigrating at the same time but with older children. This gap then increases to about 7–8 percentage points in year 2, but then slowly decreases until year 7, when no differences can be seen. These results indicate that the PLB delays labor force participation for some years, but that these women later catch up with the women who immigrated at the same time but didn't have access to PLB. As mentioned before, the increasing gap in the beginning is probably due to the obstacles late immigrant mothers with older children face when they come to Sweden and are attending language courses instead of searching for work.

As with labor force attachment, the graphs displaying the effect of PLB on employment are very similar, independently of whether additional control variables are included, see Figure 7.[20] The labor force attachment results are to some extent driven by older mothers who are unemployed

[19] For point estimates and standard errors see Table 4, first column.

[20] Point estimates and standard errors are shown in the first column of Table 5.

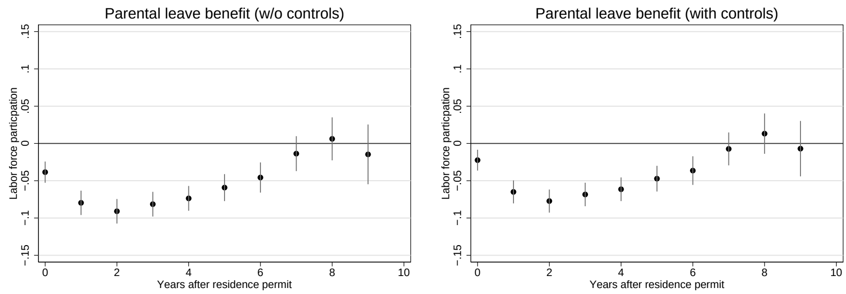


Figure 6: Estimated results: effect of access to parental leave benefits on maternal attachment to the labor force (without and with additional controls).

and registered at the PES since the estimates for employment are smaller than those for labor force attachment. Here the estimates for the year of immigration are positive but these are driven by the fact that very few mothers with older children have obtained employment in the first year. In years 2–6 the estimated effect of PLB on employment is about 3 percentage points lower employment rates.

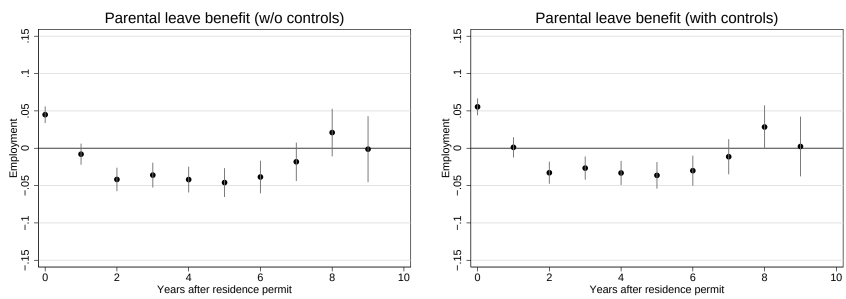


Figure 7: Estimated results: effect of access to parental leave benefits on maternal employment (without and with additional controls).

Table B 1 in Appendix B show estimation results and standard errors for the effect of PLB, the assimilation process, and the estimated effect of child age on the different outcomes.

5.2 How to interpret the coefficient: What is the treatment?

To understand the results it is important to know what treatment is. Access to paid parental leave for these immigrant mothers may be seen as two things. First, the PLB is an economic incentive to stay out of the labor force. As seen in subsection 2.3, most mothers immigrating to Sweden between 2000 and 2005 and who claimed the PLB received the low fixed amount. For mothers claiming the benefit in 2000, this amounted to 60 SEK per day. The fixed amount was then increased to 120 SEK in 2002, 150 SEK in 2003, and finally 180 SEK for days claimed after January 1st 2004. Hence, the amount paid depended on which day the benefit was claimed for, not when the mother arrived or when the child was born. Is this enough money to create economic incentives? Figure 8 shows the social assistance norm per month each year for two types of families. The first one consist of two adults and two children, aged four and seven, and the second family consists of a single parent with a child that is four years old. Families who receive these norms also get additional money for housing. The fixed amount is much lower than the social assistance norm

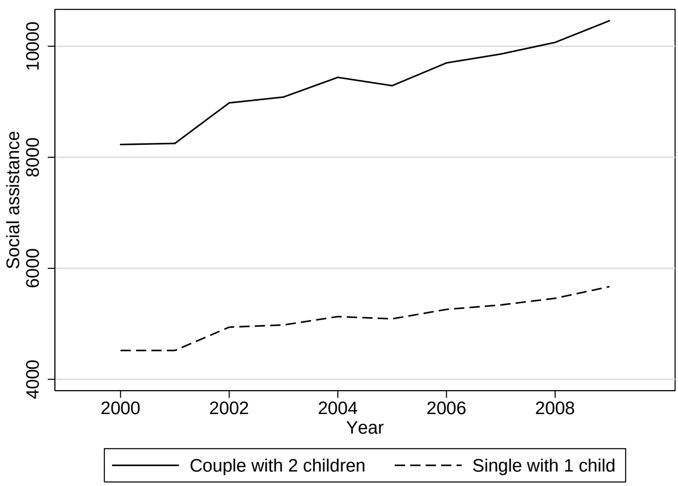


Figure 8: Social assistance norm in Sweden.

in Sweden, especially in the beginning of the period of analysis. In 2000, a mother claiming PLB received about 1800 SEK each month, compared to 8230 SEK and 4520 SEK plus costs for housing for a family of four and two, respectively, receiving social assistance. Even for a single mother with one child, the parental leave benefit is lower after 2004 when the additional social assistance for housing is taken into account (PLB gives about 5600 SEK and social assistance 5130 SEK without housing).

But the immigrants do not necessarily compare the parental benefits with the social assistance norm, for several reasons. If the husband works or if the family has other assets, they may not be eligible for social assistance and the PLB may then be a good complement. They may also compare the money to the income level in their home country.

The second way to see the treatment is as an interruption in the introduction program or language courses. Parents who claim the PLB are not allowed to work or study and are hence not able to participate in programs which would increase their human capital. As mentioned earlier, the governmental inquiry found in surveys to the municipalities that many municipalities actually require parents who need social assistance to first claim the PLB. If a woman with a four year old child takes a language course (SFI) and needs additional social assistance for support, four out of ten municipalities require that she drop the language course and claim PLB instead. Even two of ten municipalities will require a refugee mother to quit an introduction program if she needs additional support and instead claim PLB (SOU 2012:9).

The treatment is therefore a combined effect of both economic incentives and a potential interruption in introduction or language courses. One way to examine the incentive part is to use the change in the fixed amount. Even if those mothers arriving in Sweden in 2000 also got a higher fixed amount if they claimed days for 2002, it is possible to compare mothers who got a residence permit in 2002 with those who got one in 2000. The treatment in this case will then be having access to a benefit of 120 SEK per day the year of immigration and 150 SEK the year after immigration, compared to 60 SEK per day in the control group. This analysis will be performed in the next subsection and may tell us something about how important the economic incentives are.

5.3 Economic incentives

Trying to only study the effect of higher benefits I here present results from an difference-in differences estimation where mothers immigrating in 2002, when the lowest fixed amount was 120 SEK per day, are compared with mothers immigrating in 2000, when the lowest fixed amount was 60 SEK. The treated group are immigrating mothers with small children who got residence permit in 2002. Mothers with older children (immigrating in 2002) are still included to control for time of immigration, and mothers immigrating to Sweden in 2000 are used as the additional control group to control for age of the child. The treatment is thus having a higher parental leave benefit when immigrating as well as in the following years[21]. The

[21]The use of PLB among immigrants with children has increased over the years. There may be two plausible explanations for this. The first explanation is that higher benefits have increased the economic incentive to use the benefit, which is why more immigrants

results for each year are shown in Figure 9 for labor force participation and in Figure 10 for employment.

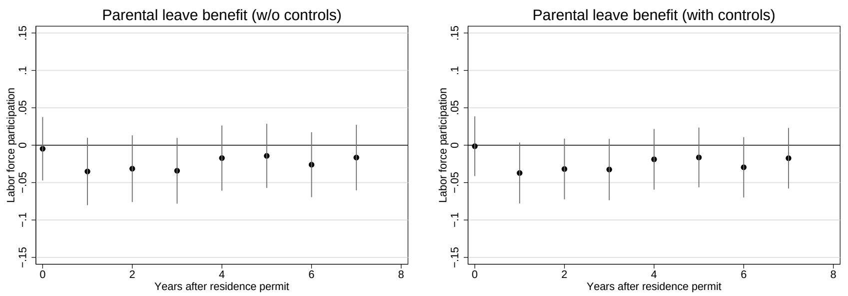


Figure 9: Estimated results: effect on maternal labor force attachment of access to 120 SEK instead of 60 SEK the year of migration (without and with additional controls).

All estimates for the effects on labor force participation are insignificant. The point estimates are negative but the confidence interval covers for example -7 to 1 percentage points in years 1–3. When it comes to employment, in Figure 10 the point estimate in year 2 is significant at the 10 percent level. But due to the large standard errors and the statistical probability that some of the estimates should be significant, there should be some caution regarding this result.

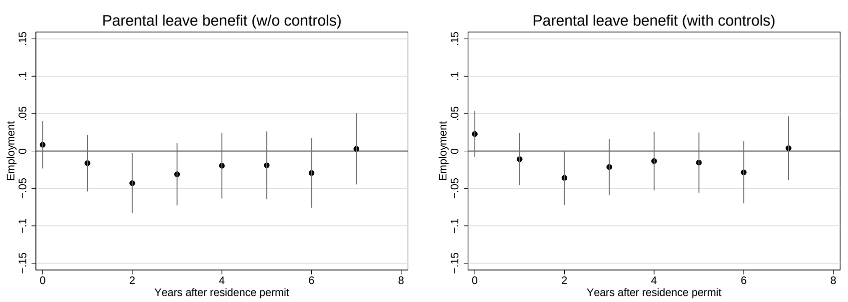


Figure 10: Estimated results: effect on maternal employment of access to 120 SEK instead of 60 SEK the year of migration (without and with additional controls).

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used it. This is what I try to examine here. However, the higher benefits have also increased the incentive for the municipalities to require social assistance recipients to claim the PLB. If the municipalities' behavior has also changed, this would negatively bias the effect of the economic incentives.

The conclusion from this subsection is that the data do not allow drawing any firm conclusions about the effect of economic incentives. I therefore continue studying the total effect of access to PLB. It should be remembered that the effect evaluated in this subsection is the difference of 60 SEK per day, while many mothers received higher benefits in the main analysis.

5.4 Sensitivity analysis

Tables 4 and 5 show the estimations from different sensitivity analyses, evaluating the total effect of access to PLB. The first table is with labor force participation as the outcome, while Table 5 has employment as the outcome. In all the estimations presented, the full model, with all additional control variables, is used. For easier comparison, the first column contains the main results from subsection 5.1.

Estimations without emigrants are displayed in the second columns (w/o Emigrants). As discussed earlier, immigrants who leave Sweden may bias the results if those who leave Sweden are differently affected by child age, for example if mothers with younger children come to Sweden to work for some years and then return to their home countries when the child is about to start school. The results show all very similar results to the main analysis, giving no indication that emigration biases the results.

Mothers who came to Sweden with small children may have postponed the birth of another child when waiting for a residence permit. The results may thereby be driven by mothers having a new child. Since the decision to have another child is endogenous, a control variable for the age of a new child has so far been excluded. But in the third columns (New Child), estimations including indicator variables for the age of the youngest new child are presented. As expected, the negative estimates generally become smaller but only a little, and the results can therefore not only be explained by immigrants in the treated group having new children. I have also estimated the effect on fertility, having a new child each year as the outcome (results available upon request) and the effect is positive and significant in year 3 but the estimated effect is less than 1 percentage point.

As discussed in subsection 4, the construction of the group consisting of earlier immigrants may overestimate the effect of child age. Earlier immigrants with older children have to have immigrated to Sweden earlier than 1995 to be able to have older children born after the first immigration date. Mothers with older children may therefore have assimilated to the labor market more, which will be captured by the variables controlling for child age. Therefore the two last sensitivity analyses will try to deal with this potential problem.

Table 4: Estimation results: sensitivity analysis, maternal labor force participation, earlier immigrants as control

	Main	w/o Emigrants	New Child	Early im. −1990	Swedish born
Access to PLB, each year since immigration:					
0	-0.0224***	-0.0181**	-0.0299***	-0.00536	-0.0322***
	(0.00713)	(0.00774)	(0.00714)	(0.0104)	(0.00667)
1	-0.0650***	-0.0613***	-0.0713***	-0.0559***	-0.0665***
	(0.00785)	(0.00826)	(0.00783)	(0.0104)	(0.00746)
2	-0.0772***	-0.0705***	-0.0758***	-0.0759***	-0.0715***
	(0.00790)	(0.00814)	(0.00778)	(0.0100)	(0.00750)
3	-0.0684***	-0.0652***	-0.0644***	-0.0692***	-0.0571***
	(0.00804)	(0.00818)	(0.00792)	(0.00971)	(0.00763)
4	-0.0614***	-0.0626***	-0.0568***	-0.0635***	-0.0428***
	(0.00810)	(0.00818)	(0.00800)	(0.00948)	(0.00762)
5	-0.0472***	-0.0499***	-0.0446***	-0.0506***	-0.0207**
	(0.00879)	(0.00885)	(0.00870)	(0.00982)	(0.00830)
6	-0.0364***	-0.0391***	-0.0329***	-0.0404***	-0.00112
	(0.00979)	(0.00983)	(0.00971)	(0.0106)	(0.00923)
7	-0.00731	-0.00950	-0.00552	-0.0113	0.0346***
	(0.0113)	(0.0113)	(0.0112)	(0.0119)	(0.0106)
8	0.0132	0.0107	0.0119	0.00817	0.0608***
	(0.0138)	(0.0138)	(0.0137)	(0.0142)	(0.0132)
9	-0.00697	-0.00836	-0.00573	-0.0150	0.0443**
	(0.0190)	(0.0189)	(0.0188)	(0.0192)	(0.0185)
N	463265	444240	463265	183547	7766157

All estimations also include controls for time since immigration, year, child age, and individual characteristics. Standard errors clustered on individual in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The first one changes the population of earlier immigrants by requiring all earlier immigrants to have immigrated earlier. Column 5 (Early im. - 1990) in Tables 4 and 5 therefore show estimations where the additional control group consist of earlier immigrants who received their residence permit before or in 1990 in Sweden. The estimates are somewhat more negative from year 3 for labor force participation (Table 4), and for employment (Table 5) the differences are somewhat larger, indicating that the composition of the group consisting of earlier immigrants overestimates the effect of child age, thus giving positively biased estimates. However, putting the limit for immigration earlier also reduces the sample and probably makes the earlier immigrants more different from the late immigrants.

Table 5: Estimation results: sensitivity analysis, maternal employment, earlier immigrants as control.

	Main	w/o Emigrants	New Child	Early im. –1990	Swedish born
Access to PLB, each year since immigration:					
0	0.0554***	0.0610***	0.0484***	0.0473***	0.0268***
	(0.00577)	(0.00612)	(0.00579)	(0.00919)	(0.00486)
1	0.00121	0.00388	-0.00577	-0.00732	-0.0173***
	(0.00692)	(0.00731)	(0.00691)	(0.00963)	(0.00634)
2	-0.0328***	-0.0301***	-0.0321***	-0.0463***	-0.0463***
	(0.00753)	(0.00783)	(0.00747)	(0.00969)	(0.00708)
3	-0.0266***	-0.0249***	-0.0219***	-0.0408***	-0.0366***
	(0.00793)	(0.00815)	(0.00783)	(0.00961)	(0.00752)
4	-0.0331***	-0.0333***	-0.0272***	-0.0466***	-0.0355***
	(0.00824)	(0.00838)	(0.00814)	(0.00956)	(0.00779)
5	-0.0363***	-0.0388***	-0.0319***	-0.0505***	-0.0305***
	(0.00909)	(0.00920)	(0.00897)	(0.0101)	(0.00864)
6	-0.0301***	-0.0319***	-0.0248**	-0.0422***	-0.0149
	(0.0103)	(0.0104)	(0.0102)	(0.0111)	(0.00976)
7	-0.0114	-0.0130	-0.00799	-0.0197	0.0117
	(0.0120)	(0.0121)	(0.0119)	(0.0126)	(0.0114)
8	0.0284*	0.0275*	0.0283*	0.0214	0.0591***
	(0.0148)	(0.0148)	(0.0146)	(0.0152)	(0.0142)
9	0.00233	0.00167	0.00450	-0.00580	0.0407**
	(0.0204)	(0.0204)	(0.0203)	(0.0207)	(0.0201)
N	463265	444240	463265	183547	7766157

All estimations also include controls for time since immigration, year, child age, and individual characteristics. Standard errors clustered on individual in parentheses, $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

The other way to remove the construction problem is to use Swedish-born mothers to control for the effects which child age has on labor force participation and employment. Estimation results with Swedish-born mothers are shown in column 6 of the tables. Even if Swedish-born mothers could be expected to have different values when it comes to children and work, the results are very similar, at least for the years 2–5.

Reassuringly, the estimated effects do not change much when performing these different sensitivity analyses, which reinforces the effect of PLB reducing labor force participation and employment up to six years after immigration.

5.5 Heterogenous effects

The results may differ for different mothers, which is why some heterogenous analyses are performed in this subsection. First, an analysis by child age is performed in subsection 5.5, before the mothers are divided by region of origin in subsection 5.5. In subsection 5.5 the analysis is performed for different educational levels and finally I study single mothers and mothers living with a partner in subsection 5.5.

By child age

To find the effect by child age, the age of the child has been interacted with the PLB variable. To investigate if the cut-off between ages six and seven is reasonable, even mothers with seven year old children have in these estimations been assigned a PLB variable equal to one.

Figure 11 shows the estimated results for labor force participation. The estimated effect is largest for mothers with children aged 2–4 years, while no effect is found for mothers coming to Sweden with children seven years old.

For employment, smaller point estimates together with larger confidence intervals give fewer significant results (Figure 12). It seems to be the case that mothers with younger children have lower probabilities of being employed when they have access to PLB at immigration, after controlling for the child's age. Mothers of seven years old children do seem not seem to have been affected and, the estimates for mothers coming with children six years of age are almost all insignificant.

The access to pre-school classes and the municipalities' behavior may explain why there are small or no effects for mothers of children six years of age. As found by the inquiry appointed by the government to evaluate how the PLB affects labor market participation, many municipalities require immigrants who are able to claim PLB to do that before they can get social assistance (SOU 2012:9). This requirement implies that immigrants who want to search for work may be excluded from the labor market by the municipality. When the child is seven, the municipalities can not longer force mothers to claim PLB since the child then starts mandatory school. However, even mothers of six year olds may not be required to claim PLB

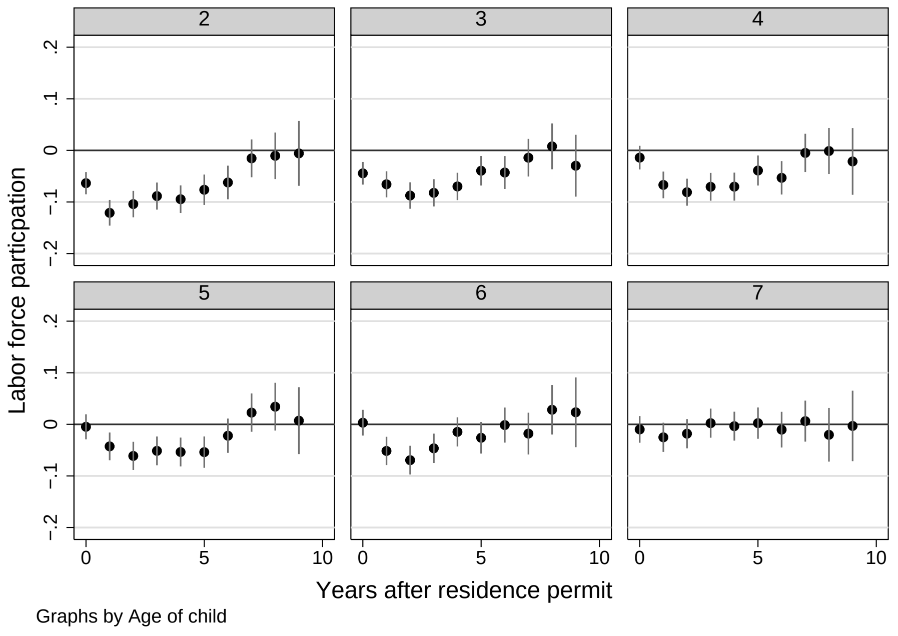

Figure 11: Estimated results: effect of access to parental leave benefits on maternal labor force participation, by age of youngest child.

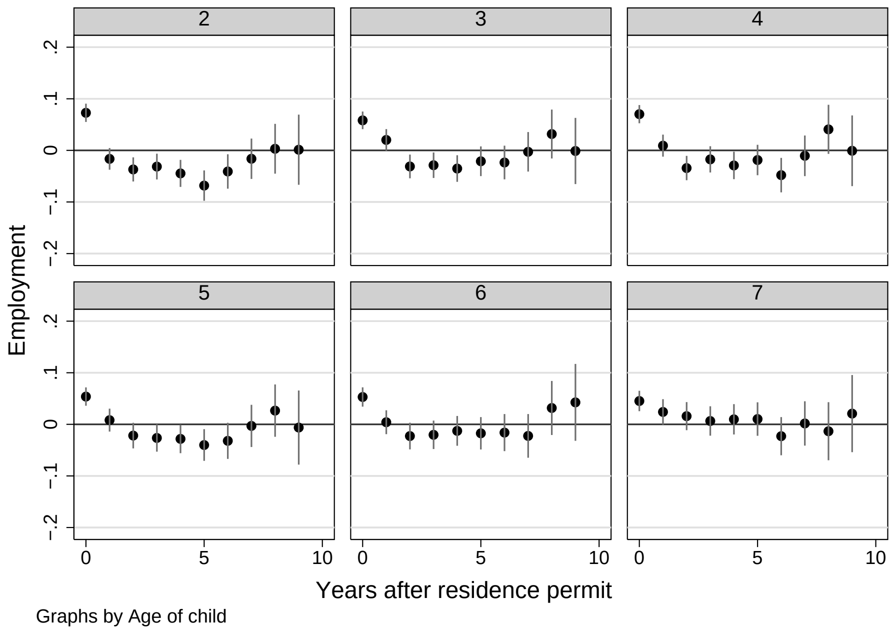

Figure 12: Estimated results: effect of access to parental leave benefits on maternal employment, by age of youngest child.

since most six year olds in Sweden attend pre-school classes. These classes aren't mandatory, but in 2001, 93 percent of all six year olds started this class (Swedish National Agency for Education, 2002).

By region of origin

Figures 13 and 14 show the estimated effect of PLB by region of origin. The sample is divided into sub-samples and the analysis is performed for each region separately. Dividing the sample reduces the sample sizes, making many of the confidence intervals include a zero effect. The point estimates are in many cases still large, of more than 5 percentage points, but the patterns differ to some extent from those found when all regions are estimated together.

The two top sub-figures are for regions where few or no immigrants received residence permits as refugees. Immigrants from Western Europe show a similar pattern as in the main analysis with an estimated negative effect the first years but this effect only lasts to year 3 before the estimates are close to zero for labor force participation and one year earlier in the employment estimation, even if all estimates are insignificant.

The second sub-figure, displaying the estimated results for immigrants coming from North and South America together with the South Pacific, show a negative effect on labor force participation of over 10 percentage points in year 2 after immigration. When estimating the effect of PLB on employment these estimates are somewhat smaller but still large and not until year 7 do the point estimates become close to zero, even if the estimates are smaller and insignificant from year 3. The results for these non-refugee regions indicate that there are effects for the first years but that this effect disappears or at least gets smaller earlier than when estimating all mothers together.

Mothers from Eastern Europe, mothers from Asia, and mothers from the Middle East and North Africa had lower probabilities of being in the labor force for some years if they had access to parental leave benefits when they came to Sweden. For mothers from Eastern Europe and Asia the same pattern is seen for employment. Mothers from Sub-Saharan Africa show similar but smaller estimated effects of PLB on labor force participation and they are far from significant.

Mothers from the Middle East and N. Africa together with Sub-Saharan Africa stands out when it comes to employment. Mothers from these regions have a positive estimated effect of PLB for the first few years, that slowly reaches zero. The explanation for this is the low participation rates among these mothers (not shown in any tables). For mothers from

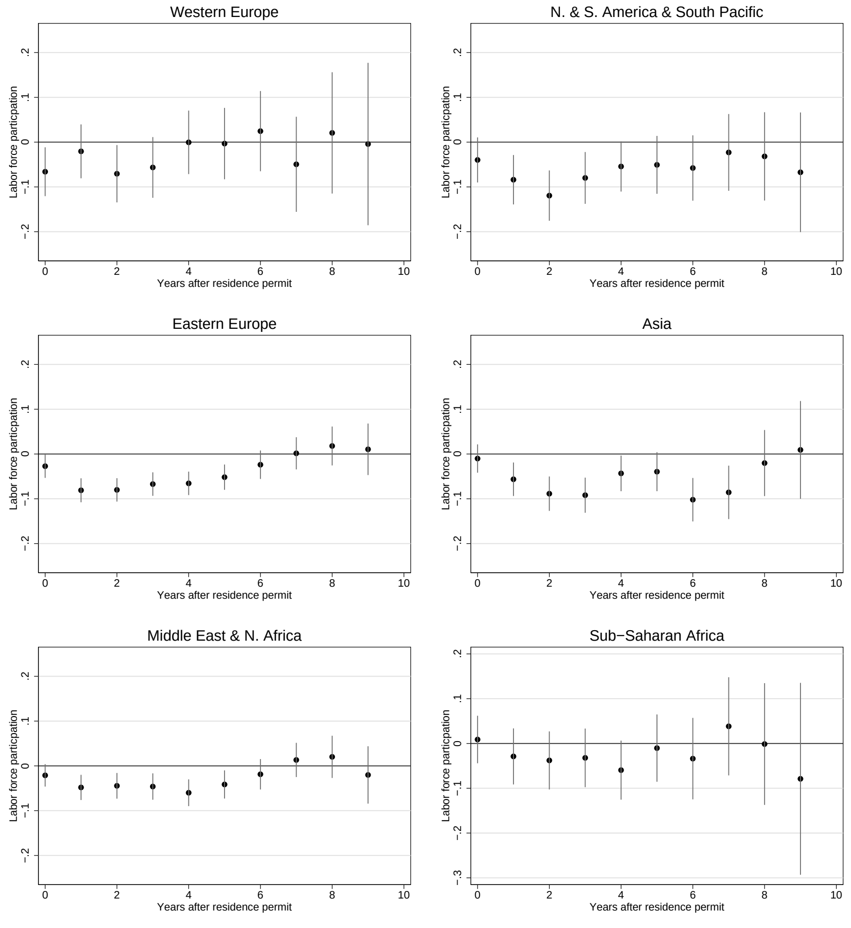

Figure 13: Estimated results: effect of access to parental leave benefits on maternal labor force participation, by region.

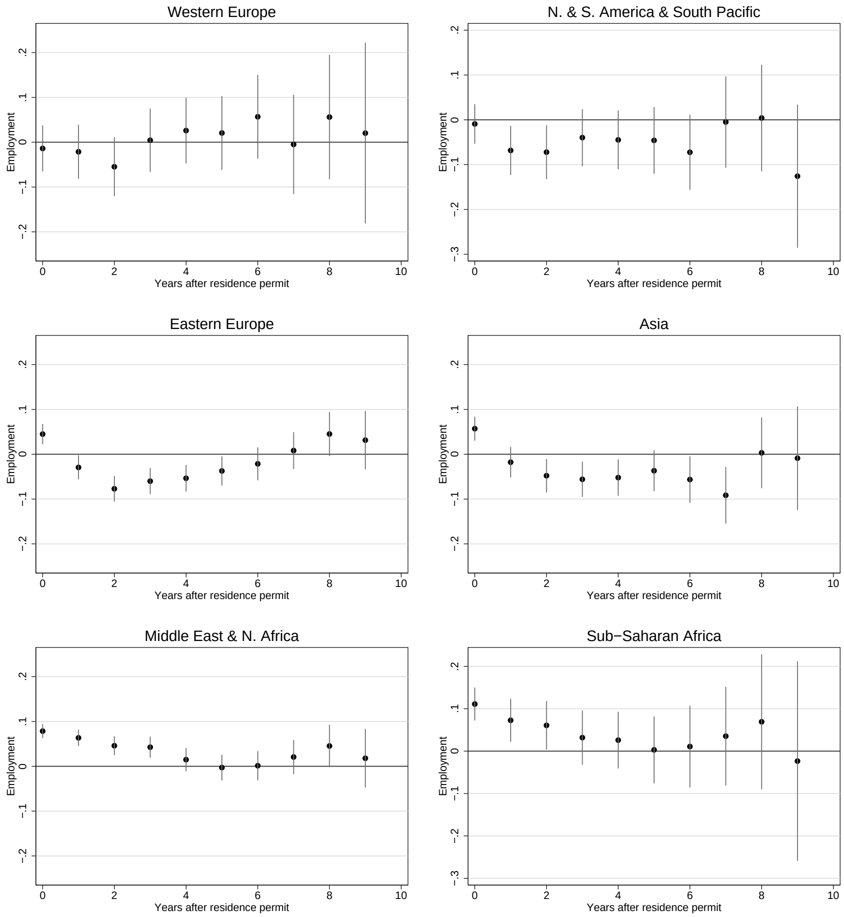

Figure 14: Estimated results: effect of access to parental leave benefits on maternal employment, by region.

N. Africa and the Middle East, the employment rates are lower than 10 percent for the year of immigration, while the difference between the early immigrant mothers from the same region is 12 percentage points. Mothers from Sub-Saharan Africa have somewhat higher employment rates the first year (10.1 and 13.2 percent for the different child age groups) but they are still low compared to early immigrant mothers from this region (61.7 and 72.3 percent). Mothers from these regions seem to face other obstacles to entering into employment. These obstacles may be due to their both lacking country specific human capital, such as speaking Swedish, and discrimination in the labor market, but there may also be a decision made by the mothers.

Mothers from the four regions in the bottom sub-figures are all more likely to receive residence permits in Sweden as refugees. Even if all four groups of mothers show similar patterns when it comes to labor force participation, the effects do not carry over to employment for all groups. These differences are probably driven by late immigrating mothers with older children. Even if mothers with older children from the Middle East and Africa are registered at the PES, they are not able to find employment, which probably would be the case even for mothers with younger children if they didn't have access to PLB.

The estimated effects for labor force participation depend to some extent on where the mothers come from, but the patterns are similar for most of the regions, even if the point estimates are smaller and insignificant for Sub-Saharan Africa. When it comes to employment, the estimated effects are positive in the beginning for both Sub-Saharan Africa and the Middle East and North Africa, probably driven by obstacles to getting employment for the mothers from these regions.

By educational level

The estimated effect of PLB for mothers with different level of education are shown in Figures 15 and 16. Mothers least affected are those with at least some university education. University educated mothers have at most a 5 percentage points lower probability of being in the labor force if they had access to PLB. It may be easier for these mothers to find some employment, and therefore they may use the benefit to a lesser extent.

For mothers with less education, PLB causes the participation in the labor force to be 5 to 10 percentage points lower, depending on educational level, until about year 7. This is a result that carries over to the effect on employment for mothers with some high school education, even if the estimates have smaller magnitudes. For mothers with only compulsory

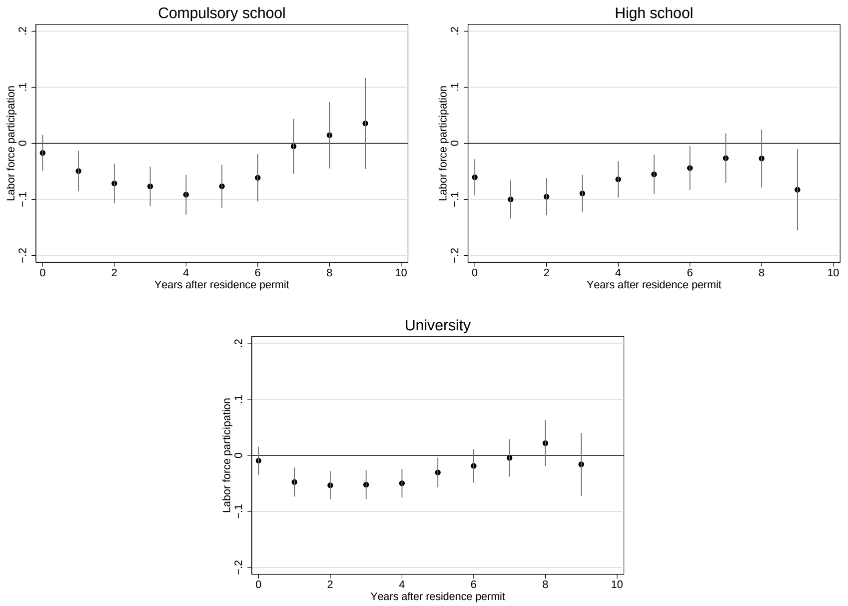

Figure 15: Estimated results: effect of access to parental leave benefits on maternal labor force participation, by educational level.

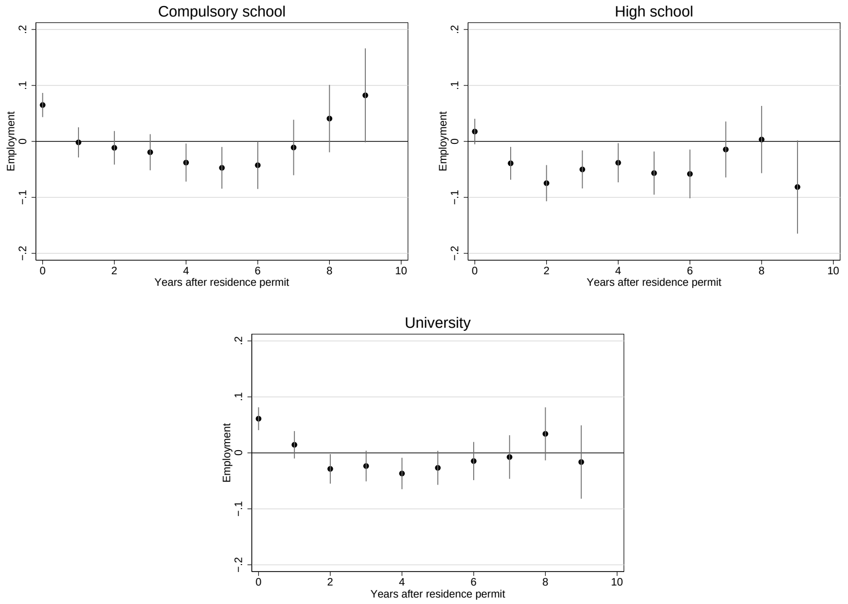

Figure 16: Estimated results: effect of access to parental leave benefits on maternal employment, by educational level.

school, the pattern changes, showing zero estimated effects the first years but from year 4 a negative estimated effect for some years of having access to PLB at immigration.

A possible explanation for the delayed effect could be if low-educated mothers with older children start labor market programs to increase their human capital and then get their first employment only after some years. Then for the first years, mothers with small children are able to stay on parental leave and mothers with older children attend different labor market programs, which is why no differences in employment can be seen.

Singles and in couples

The last heterogenous analysis is performed for single mothers as opposed to mothers living with a partner. Mothers may be affected differently if they have a spouse with which to share both the economic responsibility and the care for the children. The results from these estimations are shown in Figures 17 and 18. For single mothers the estimated effect on labor force participation is greater in year 2 than for mothers living in a couple, but single mothers seem to enter the labor market faster than mothers in couples. For single mothers, the estimates approaches zero in two steps. First, in year 4 the point estimate is about 5 percentage points lower for being in the labor force and then again in year 7 the point estimate drops even more. The reduction in year 4 when it comes to labor force participation does not carry over to employment, where single mothers have negative estimates of 3–5 percentage points until year 7, which is larger than that of mothers in couples.

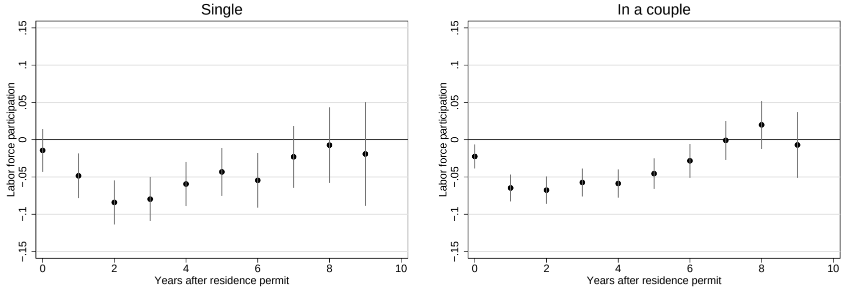

Figure 17: Estimated results: effect of access to parental leave benefits on maternal labor force participation of singles and mothers in couples.

The results in this section indicated that single mothers are more affected the first year but some joined the labor force faster than cohabiting

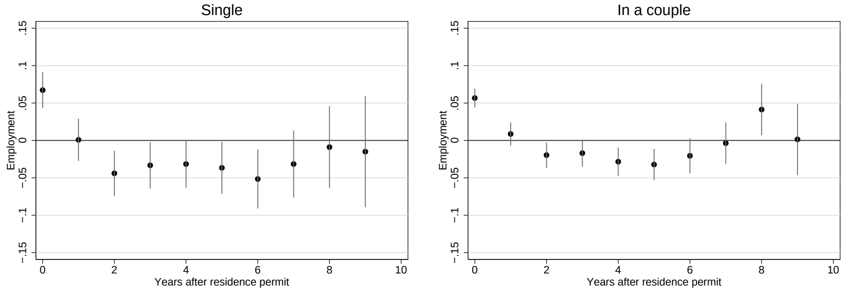

Figure 18: Estimated results: effect of access to parental leave benefits on maternal employment for singles and mothers in couples.

mothers. The differences between single mothers and cohabiting mothers is not statistically different.

6 Discussion

This paper has studied how access to paid parental leave (PLB) affects immigrating mothers' labor market assimilation. All parents who receive a residence permit in Sweden with children aged below eight get access to 480 days of PLB, making it possible for one parent to delay labor market entrance for some years. Many immigrating mothers use the benefit, but far from all. Among mothers immigrating to Sweden between 2000 and 2005 with their youngest child between two and six years old, 43 percent claimed at least some PLB for children they had when they immigrated during the year of immigration or the following two years.

To be able to answer the question, how does the access to PLB affect labor market participation, I have made two key assumptions in this paper. The first assumption is that the only thing affecting labor force participation, or employment, that differs between mothers immigrating to Sweden with children of different ages is the age of the child. To control for this "child age effect," an additional control group consisting of mothers who immigrated earlier to Sweden, and gave birth to their children after their immigration, was added. The second assumption is that the effect which child age has on labor force participation and employment is the same for both mothers immigrating with children and mothers immigrating earlier and gave birth to their children in Sweden. If these assumptions are fulfilled, the estimated effects can be given casual interpretations.

The first assumptions is likely to be fulfilled. There is no reason that mothers with different ages of their children coming to Sweden should differ in any other aspects than the age of their children. The second assumption is however stronger. To check the robustness of this assumption I do sensitivity analyses using different groups of mothers, both mothers immigrating earlier and Swedish born mothers, to control for child age. The result from these estimations yields very similar results as in the main analysis indicating that the effect child age has on labor force participation do not differ to much between these groups of mothers.

The main results indicate that labor force participation is 7.7 percentage points lower two years after immigration due to access to PLB, going to zero effect seven years after immigration. For employment, the estimated effect of access to PLB is about 3 percentage points 2–6 years after immigration. This indicates that it is not only mothers who, without the benefits, would have been unemployed that are the ones who are affected.

Since access to PLB can be seen as a combined effect of economic incentives and an interruption or delayed start of introduction programs or language courses, subsection 5.3 showed the results when immigrant mothers facing different payment schemes were compared. Basically, the treated group had access to a benefit that was about 60 SEK per day higher each year after immigration compared to the control group. This analysis gave negative point estimates but they were insignificant. Sensitivity and heterogenous analyses were therefore conducted for the total affect of access to PLB. All sensitivity analyses were reassuringly very similar to the main estimations.

When studying heterogeneous effects, a few conclusions can be drawn. Mothers with their youngest child five or six years of age are somewhat less affected than mothers with younger children. This difference isn't surprising since fewer of these mothers claim the benefit and they are able to use it for fewer years since their children turn eight earlier. Mothers coming from Sub-Saharan Africa seem to be less affected when it comes to labor force participation. For employment, both mothers from the Middle East and Africa have positive estimates the first years. This is, though, driven by low employment rates among mothers with older children, indicating that there are more obstacles to entering employment for mothers from these regions.

Is the access to PLB then a huge obstacle for labor market entrance for mothers immigrating with small children? During the six years studied, 1400 mothers on average immigrated each year to Sweden with children aged 2–6 years. If the estimations give the true effect of PLB on labor force participation, this corresponds to at most 100 mothers being out of the labor force in the second year after immigration, decreasing to zero in year 7. For employment, about 40 mothers of those immigrating during a year do not have employment due to access to the benefits 2–5 years after immigration. Looking at the number of individuals, it doesn't seem to be that many, even if this is per year. Since only about half of the immigrating mothers obtain employment after five or six years, the percentage effect is twice as large as the percentage point effect and substantial for this group of women. But still, the access to PLB can definitively not by itself explain the low employment rates among immigrant women. This is also clear when studying the participation rates for mothers with older children who do not have access to PLB.

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Appendix A: Data

Below the variables used in the regression estimations are described.

Outcome variables

- *Employed* equals 1 if individual i in year t earns more than the 10th percentile in the full-time wage distribution in Sweden.
- *Labor force attachment* equals 1 if individual i is *Employed* or registered at least 30 days at the Public employment Service.

Explanatory variables

- *PLB* equals 1 if the mother immigrated between 2000 and 2005 with a child who, in the year of immigration, turned 2–6 years old.
- $D(Time_{it} = \tau)$ equals 1 for Late immigrant mothers if year t is τ years after immigration. τ goes from 0 to 9.
- $D(Childage_{it} = a)$ equals 1 for a mother if the youngest child at immigration, or the first year of analysis, is a years old in year t. a goes from 2 to 20 where 20 includes ages 20–24.
- $D(year_t = b)$ equals 1 if the year is year b, where b goes from 2000 to 2009.

Additional control variables - $\mathbf{X_i}$

- $D(Edu{=}e)$ equals 1 if the educational level the first year of analysis is e, where e corresponds to
  1. less than compulsory school
  2. compulsory school
  3. up to two years of high school
  4. up to three years of high school
  5. tertiary, less than three years
  6. tertiary, three years or more
  7. doctoral studies
- $D(Number{=}n)$ equals 1 if individual i had n children the first year of analysis. n goes from 1–6 where 6 also includes mothers with more than six children.

- *D(Age group=g)* equals 1 if individual i belongs to age group g, where each age group is a five-year interval.
- *D(region=r)* equals 1 if the region of origin is region r:
  1. Western Europe
  2. Eastern Europe
  3. Asia
  4. The Middle East and North Africa
  5. Sub-Saharan Africa
  6. North America, South America, and the South Pacific
- *D(County=l)* equals 1 if the individual lives in county l the first year of analysis. There are 21 counties in Sweden.
- *D(Partner=p)* equals 1 if the partner the first year of analysis immigrated at time p corresponding to
  1. being Swedish-born
  2. immigrated more than five years earlier
  3. immigrated 1–4 years earlier
  4. immigrated the first year of analysis

Appendix B: Tables

Table B 1: Estimation results: maternal labor force attachment and employment

	Labor force		Employment	
	(1)	(2)	(3)	(4)
Access to PLB, each year since immigration:				
0	-0.0385***	-0.0224***	0.0449***	0.0554***
	(0.00726)	(0.00713)	(0.00570)	(0.00577)
1	-0.0796***	-0.0650***	-0.00797	0.00121
	(0.00833)	(0.00785)	(0.00722)	(0.00692)
2	-0.0909***	-0.0772***	-0.0418***	-0.0328***
	(0.00839)	(0.00790)	(0.00803)	(0.00753)
3	-0.0814***	-0.0684***	-0.0360***	-0.0266***
	(0.00844)	(0.00804)	(0.00848)	(0.00793)
4	-0.0737***	-0.0614***	-0.0419***	-0.0331***
	(0.00848)	(0.00810)	(0.00883)	(0.00824)
5	-0.0592***	-0.0472***	-0.0460***	-0.0363***
	(0.00923)	(0.00879)	(0.00987)	(0.00909)
6	-0.0457***	-0.0364***	-0.0385***	-0.0301***
	(0.0103)	(0.00979)	(0.0112)	(0.0103)
7	-0.0137	-0.00731	-0.0181	-0.0114
	(0.0120)	(0.0113)	(0.0131)	(0.0120)
8	0.00618	0.0132	0.0210	0.0284*
	(0.0147)	(0.0138)	(0.0163)	(0.0148)
9	-0.0147	-0.00697	-0.00127	0.00233
	(0.0204)	(0.0190)	(0.0225)	(0.0204)
Each year since Immigration				
0	-0.533***	-0.467***	-0.559***	-0.497***
	(0.00555)	(0.00603)	(0.00457)	(0.00539)
1	-0.264***	-0.200***	-0.440***	-0.377***
	(0.00615)	(0.00631)	(0.00568)	(0.00619)
2	-0.187***	-0.121***	-0.351***	-0.285***
	(0.00611)	(0.00622)	(0.00621)	(0.00650)
3	-0.161***	-0.0937***	-0.309***	-0.239***
	(0.00612)	(0.00624)	(0.00643)	(0.00666)
4	-0.137***	-0.0695***	-0.266***	-0.193***

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Continued from last page	Labor force (1)	(2)	Employment (3)	(4)
	(0.00614)	(0.00624)	(0.00662)	(0.00678)
5	-0.118***	-0.0467***	-0.233***	-0.151***
	(0.00677)	(0.00677)	(0.00741)	(0.00738)
6	-0.110***	-0.0306***	-0.215***	-0.122***
	(0.00770)	(0.00755)	(0.00844)	(0.00822)
7	-0.110***	-0.0273***	-0.205***	-0.107***
	(0.00911)	(0.00874)	(0.00995)	(0.00945)
8	-0.103***	-0.0252**	-0.198***	-0.102***
	(0.0114)	(0.0107)	(0.0124)	(0.0115)
9	-0.0916***	-0.0102	-0.170***	-0.0697***
	(0.0156)	(0.0144)	(0.0170)	(0.0154)
Child age				
3	0.00103	-0.000575	0.0301***	0.0262***
	(0.00268)	(0.00268)	(0.00298)	(0.00297)
4	0.00102	-0.00285	0.0370***	0.0285***
	(0.00312)	(0.00314)	(0.00353)	(0.00354)
5	0.00550*	-0.000545	0.0468***	0.0340***
	(0.00329)	(0.00335)	(0.00375)	(0.00382)
6	0.0127***	0.00446	0.0614***	0.0444***
	(0.00343)	(0.00355)	(0.00393)	(0.00406)
7	0.0191***	0.0105***	0.0780***	0.0587***
	(0.00362)	(0.00380)	(0.00423)	(0.00439)
8	0.0221***	0.0127***	0.0905***	0.0686***
	(0.00382)	(0.00405)	(0.00451)	(0.00472)
9	0.0260***	0.0153***	0.100***	0.0757***
	(0.00404)	(0.00432)	(0.00480)	(0.00505)
10	0.0256***	0.0131***	0.111***	0.0822***
	(0.00430)	(0.00463)	(0.00514)	(0.00541)
11	0.0253***	0.0117**	0.116***	0.0843***
	(0.00459)	(0.00496)	(0.00548)	(0.00580)
12	0.0286***	0.0128**	0.129***	0.0927***
	(0.00491)	(0.00532)	(0.00585)	(0.00619)
13	0.0199***	0.00357	0.129***	0.0905***
	(0.00521)	(0.00568)	(0.00616)	(0.00656)
14	0.0219***	0.00580	0.137***	0.0972***
	(0.00552)	(0.00604)	(0.00649)	(0.00697)
15	0.0172***	0.00201	0.139***	0.0979***

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Continued from last page	Labor force (1)	(2)	Employment (3)	(4)
	(0.00591)	(0.00648)	(0.00687)	(0.00742)
16	0.0131**	0.000725	0.139***	0.0990***
	(0.00644)	(0.00702)	(0.00741)	(0.00799)
17	0.000288	-0.00908	0.132***	0.0928***
	(0.00711)	(0.00770)	(0.00809)	(0.00869)
18	-0.00841	-0.0144*	0.124***	0.0864***
	(0.00793)	(0.00853)	(0.00897)	(0.00957)
19	-0.0167*	-0.0180*	0.123***	0.0891***
	(0.00906)	(0.00955)	(0.0101)	(0.0106)
≥20	-0.0444***	-0.0328***	0.102***	0.0757***
	(0.0118)	(0.0120)	(0.0129)	(0.0131)
Year effects	Yes	Yes	Yes	Yes
$\mathbf{X_i}$	No	Yes	No	Yes
N	463265	463265	463265	463265

All estimations also include controls for time since immigration, child age, year, and individual characteristics. Standard errors clustered on individual in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Essay II

Does Providing Childcare to Unemployed Affect Unemployment Duration?

1 Introduction

This paper evaluates whether making childcare available for unemployed parents affects their probability of finding work. In Sweden subsidized childcare is available for all families with young children when both parents work. A reform implemented in July 2001 forced Swedish municipalities to also offer childcare to unemployed parents for at least 15 hours per week. The reform was mainly motivated for child investment reasons, but an additional aim with the reform was to make it easier for unemployed parents to search and find work.

With an aging population in many parts of Europe , reforms increasing labor force participation are of great interest. Both lack of childcare availability and the cost of childcare are things that can be seen as barriers to employment, especially for low-income families (Kisker and Ross, 1997). It is therefore interesting to see if the Swedish reform increased the parents' probability to start working.

According to search theory, an unemployed individual may influence his or her probability of receiving a job offer through the intensity and time that the individual devotes to searching for work. An unemployed individual will accept a job offer if the wage is equal to or larger than the individual's reservation wage (for a review of search theory, see Mortensen, 1987).

Offering childcare to unemployed parents may change both their search intensity and reservation wage and therefore the probability of leaving unemployment for work. For an unemployed parent with a young child, two obstacles to leaving unemployment exist when there is no childcare available for them. The first is finding time to search for a job while caring for the child. The second is finding temporary childcare after being offered a job until the child can be put in regular childcare. Although all working parents in Sweden are offered childcare, there is usually some waiting time before a parent entering the workforce can find a childcare placement for his or her child[1]. When unemployed parents are offered childcare, these obstacles are reduced, and the duration of unemployment might decrease.

There may, however, be an opposite effect if an unemployed parent is offered childcare. If the unemployed parent appreciates time at home without the child, this extra leisure time increases the parent's utility and then decreases his or her willingness to start working, or increases the parent's reservation wage, which might increase the duration of unemployment. Thus, childcare for unemployed parents makes it possible for the parent to increase his or her search intensity, but it may also increase the parent's reservation wage. The net effect is therefore an empirical question.

This paper is related to two strands of the literature. The first is search theory and determinants of unemployment duration. Empirically, especially effects of unemployment insurance has been evaluated, but even effects of individual characteristics have been studied (see, for example, Røed and Zhang, 2003, Arulampalam, 2001 and Carroll, 2006, who evaluate the effects of unemployment insurance, scarring from earlier unemployment spells and individual characteristics on unemployment duration). The second is the literature on the effect of subsidized childcare on the female labor supply in particular (for a survey, see Anderson and Levine, 2002). What differs in this study is that the parents have already decided to enter the labor force, and childcare is always available for parents leaving unemployment, even if it may take some time. In this paper, I join the two strands of literature by evaluating how availability of childcare during unemployment affects unemployment duration. To my knowledge, this has not been done before.

Before the reform, implemented in July 2001, a majority of the municipalities offered childcare to unemployed parents, but not all municipalities. This heterogeneity permits the use of a difference-in-differences (DD) approach to evaluate the effects of childcare availability on the probability of leaving unemployment. As the childcare reform did not affect parents

[1] In the majority of the municipalities, most parents who apply for childcare in May are offered a placement in September, when older children leave childcare for preschool. At other times of the year, some municipalities find it harder to offer childcare.

whose youngest child was old enough to be in preschool class[2] or primary school, these parents can be used as a control group in the estimation, making it possible to also use a difference-in-difference-in-differences (DDD) strategy. To include all unemployed parents, not just those leaving unemployment for work, the DD (and DDD) strategy will be applied to a proportional hazard model to determine how the probability of finding work changes for unemployed parents when childcare is available[3].

In the first DD estimation, using parents in other municipalities as the control, positive and significant effects of childcare availability on the probability of finding work are found for mothers with young children. Unfortunately, positive effects are also found in placebo estimations, but the point estimates are smaller. When the second control group is used, parents with older youngest children, the sample size decreases, and all estimates are insignificant. When controlling for several individual characteristics and time effects in the DDD estimation, I find that the probability of leaving unemployment increases by 16 percent for mothers when childcare is available. For fathers with young children, no effects are found in any of the estimations.

For mothers, some heterogeneous effects are also found. Mothers with only compulsory school or any university education had a higher probability of finding work when childcare was available, while no effect could be found for those mothers with a high school education of two years or less. Likewise, no effect could be found for mothers with only one child, while mothers with two children had a 32 percent higher probability of finding work when childcare was available during unemployment.

The remainder of the paper proceeds as follows: section 2 summarizes family policies in Sweden, particularly the Swedish childcare reform; in section 3, the econometric method is described; and section 4 presents the data. The results are discussed in section 5 before concluding in section 6.

2 Childcare and the childcare reform in Sweden

2.1 Family policies in Sweden

Sweden has very generous family policies compared to other European countries. At the time for this study there were, paid parental leave avail-

[2]Preschool class, compared to childcare, is more similar to primary school, but it is not compulsory; see section 2.1.

[3]The same strategy is used by Clotfelter, Glennie, Ladd, and Vigdor (2008), using a policy intervention in North Carolina to evaluate whether higher salaries keep teachers in high-poverty schools.

able for 450 days[4], pay for care of sick children, cash support and subsidized childcare (for an overview, see Björklund, 2006). Both mothers and fathers utilize the paid parental leave, but most parents then return to their employment. In 2001, 43.3 percent of all one-year-old children and 79.3 percent of all two-year-old children in Sweden were in childcare (Swedish National Agency for Education, 2002). The municipalities are responsible for ensuring that childcare is available for those parents who are entitled to childcare according to the law (that is working or studying), and the fees are largely subsidized. The municipalities may make agreements with other parties to provide the actual childcare services (SFS, 1985). To guarantee high-quality childcare, a preschool curriculum including goals and guidelines for the activities offered in childcare was created in Sweden in 1998 (Swedish National Agency for Education, 1998).

In Sweden, municipalities must provide free preschool classes beginning in the autumn of the year in which the child turns six years old. One year later, the child starts compulsory school. In the 2001-2002 school year, 93 percent of all six-year-old children in Sweden attended a preschool class (Swedish National Agency for Education, 2002).

2.2 The childcare reform

The Swedish childcare reform implemented between July 2001 and January 2003 consists of four parts. Making it mandatory to offer 15 hours of childcare per week to unemployed parents was the first part to be implemented (July 2001). Both the second and third parts were introduced in January 2002. The second part made it mandatory for municipalities to offer childcare for at least 15 hours each week, for children whose parents are on parental leave with a younger sibling. The third part introduced a cap on childcare prices, leading to a considerable reduction in childcare costs (for an evaluation of this part, see Lundin, Mörk, and Öckert, 2008). This part was not mandatory for the municipalities, but those that introduced the cap were offered extra grants by the central government. The fourth part, implemented in January 2003, was the introduction of universal free childcare for all four- and five-year-old children for at least 525 hours per year (Swedish National Agency for Education, 2007). The part of the reform used in this study was implemented mainly to prevent isolation of the children of unemployed parents and to increase their opportunity to meet other children and take part in childcare activities, but the government also thought that the reform would allow unemployed parents to search for work more effectively (Swedish National Agency for Education,

[4] In January 2002, this was extended to 480 days.

1999).

During the spring of 1998 and the spring of 2001, the Swedish National Agency for Education conducted surveys to see in which municipalities unemployed parents were offered childcare. Two questions were asked: first, could parents who already had a childcare placement keep the child in childcare if they became unemployed, and second, would childcare be available for unemployed parents where the child had not been in childcare before?

In the analysis, the municipalities are grouped according to their responses to the first question. For some parents in the control group, the variable indicating that childcare is available will then be wrong, indicating that childcare is available when it is not. Because this categorization will put some parents who should be in the treatment group in the control group, the effect of childcare availability will be underestimated[5].

The municipalities can then be divided into three different groups according to their responses to the surveys. The first group consists of those municipalities that did not offer unemployed parents any childcare. This is the clean treatment group of municipalities in the estimations. The second group consists of those municipalities where unemployed parents could keep their childcare placement, but only for a limited number of months (ranging from 2 to 12 months). Parents in this group of municipalities will belong to either the control or treatment group depending on how long they have been unemployed in relation to how many months childcare is available[6]. In the third group of municipalities, unemployed parents could keep their childcare placement with no restrictions in months even before the reform. Therefore, the reform introduced no change, and these municipalities are used as the control group. A total of 208 municipalities were classified into these three groups, and the number of municipalities in each group is shown in table 1. The remaining 81 Swedish municipalities did not respond to one or both surveys or changed their policies. Because it is not possible to know when they changed their policies, they are removed from the analysis.

The control municipalities also offered different amount of hours in childcare to unemployed parents ranging between 3 hours per week to no time restriction. There were however only 9 control municipalities offering

[5]This problem will be minimized by only including those parents with unemployment insurance because to be eligible for UI, the parent must have been employed previously and would thus have had childcare available; see section 4.

[6]Because a proportional hazard model is used, variables may change in the estimation; therefore, it is possible for parents in these municipalities to change from control status to treatment status. See section 3.3

Table 1: Municipality Groups

Municipalities where:	
1. Clean Treatment Group (Childcare was not available for unemployed parents before the reform)	14
2. Treatment and Control (Childcare was available for a limited number of months before the reform)	43
3. Clean Control Group (Childcare was available with no time limits before the reform)	151

less than 15 hours of childcare per week before the reform. In the analysis I only take into account if childcare is available for any hours since the parent then have at least some time to search for work and if offering a job the child has a childcare placement. Even if some municipalities found it hard to offer childcare to children who need a new place it is easy for parents to increase childcare time when having a placement.

Since the municipalities are responsible for providing subsidized childcare, and also do it to a very large extent, there are few alternative childcare services in Sweden. The implication is that for those families where publicly provided childcare was not available before the reform there existed basically no other alternatives; if the parent became unemployed, the child had to leave childcare.

In table 2, descriptive statistics (means) for the different municipality groups are shown for the year 2000. As can be seen, the unemployment rate is higher in the treatment municipalities, and these municipalities also have smaller populations on average. The cohort sizes of children aged 2-6 years and the shares of women are similar for all three groups, while the share of immigrants is slightly smaller in those municipalities that, before the reform, only offered childcare to unemployed parents for a limited number of months.

The reform had a positive effect on the rate of participation in childcare among children of unemployed parents. The share of children of unemployed parents in childcare increased from 65 percent in 1999 to 82 percent in 2002, when the reform was implemented (Swedish National Agency for Education, 2003). As the unemployment rate decreased during the same period of time, the total number of children of unemployed parents in childcare was unchanged, but the changes are heterogeneous across the groups of municipalities.

Unfortunately, the childcare reform was not the only reform imple-

Table 2: Descriptive statistics (means) of municipality groups, 2000

	Treatment	Limited	Control
Unemployment (%)	4.952	3.994	4.144
Population	18,566	20,145	35,607
Children age 2-6	0.054	0.054	0.055
Immigrants	0.106	0.087	0.107
Women	0.500	0.498	0.500
N	14	43	151

mented on July 1, 2001, that may have had an effect on unemployed parents' probability of entering the workforce. On the same date, the first part of an unemployment insurance reform that introduced a new two-tiered benefit structure for some individuals and raised the benefit level was implemented. Bennmarker, Carling, and Holmlund (2007) used this reform to evaluate whether the higher benefits increased the unemployment duration. They found, consistent with theory, that unemployment durations increased for men, but for women, the unemployment duration decreased. They mentioned the Swedish childcare reform as a plausible explanation for the difference between men and women. This UI reform affected those with higher earlier earnings more, and although I am not able to control for earlier earnings, heterogeneous effects over education could be expected if this reform had a differential effect on individuals with higher earlier wages. Education level is included as a control variable in the estimations, but I also divide the sample according to education to search for heterogeneous effects; see section 5.3.

3 Econometric method

3.1 Difference-in-differences

In difference-in-differences (DD), the identifying assumption is that there are parallel trends between the treatment group and the control group. If this assumption is fulfilled, the estimation gives the treatment effect of the treated. In this case, the treatment group consists of those parents with children aged between two and six years living in municipalities that did not offer any childcare to unemployed parents before the reform. It is then possible to use two different control groups. The first consists of parents with children of the same age living in municipalities where childcare was available for unemployed parents before the reform, that is, the control municipalities. The second consists of unemployed parents whose youngest child is aged between six[7] and ten years old living in the same municipalities as the treatment group. These parents with older youngest children were not affected by the childcare reform because their children attend school every day, giving them time to search for jobs. In summary, the treatment group consists of target parents living in treatment municipalities, the first control group consists of target parents living in control municipalities, and the second control group consists of non-target parents living in treatment municipalities.

In the first DD estimation with control municipalities the linear specification that will be used to find the change in the probability of leaving unemployment is then given by[8]:

$$\mathbf{x}'\boldsymbol{\alpha} = \alpha_1 Z^m + \alpha_2 Z^t + \alpha_3 Z^m Z^t \tag{1}$$

where Z^mequals one if the municipality did not offer childcare to unemployed parents before the reform and Z^t equals one after the reform date. α_3 is the DD parameter estimating the effect of childcare availability on the probability for the target parents in the treatment municipalities to start working. In the second DD estimation, using non-target parents as the control group, the linear specification is given by:

$$\mathbf{x}'\boldsymbol{\lambda} = \lambda_1 Z^a + \lambda_2 Z^t + \lambda_3 Z^a Z^t \tag{2}$$

where Z^a equals one if the parent belongs to the target group (that is, if the parent's youngest child is between two and six years old) and λ_3 is the DD parameter.

To obtain an unbiased estimator in equation (1), the assumption is that the trends are equal for unemployed parents with young children in the different municipalities. For equation (2) to give an unbiased estimator, the trend has to be equal for parents with children of different ages within the municipalities. Estimations are performed with both control groups, both with and without additional covariates, to control for differences in the groups and thereby increase the efficiency of the estimation.

In ordinary DD estimation, the control group is untreated, but in this case, the control group is treated all the time (as with parents in the control municipalities, equation (1)) or can be seen as treated all the time (as

[7]If the child is six years old, he or she will be in childcare during the spring and begin preschool class in August. Unemployed parents with six-year-old children will then be in the target group until July, and from August onward they will be in the non-target group.

[8]All linear specifications will be put in a proportional hazard model, see section 3.3.

with parents with older youngest children, equation (2)). Instead of the interaction term, I will therefore use a dummy, $CC_{m(a)t}$, that equals one if childcare (or school for parents with older children) is available for the unemployed parent. In the first DD estimation, in which the control group consists of target parents in the control municipalities, even those parents living in municipalities that only offered childcare for a limited number of months will be included. Because the covariates are allowed to vary in the hazard model, a parent who was unemployed for more months than childcare was available before the reform will first have childcare for as many months it is available for unemployed ($CC_{mt} = 1$) and then lose it ($CC_{mt} = 0$). To control for any difference between these municipalities and the others, an additional dummy variable for municipality, Z^{m2}, that equals one if the parent was living in one of the municipalities only offering childcare for a limited number of months before the reform is included. In the estimation with additional covariates, the equation then becomes[9]:

$$\mathbf{x}'\boldsymbol{\alpha} = \alpha_1 Z^{m1} + \alpha_2 Z^{m2} + \alpha_3 Z^{t} + \alpha_4 CC_{mt} + \alpha_5 u_{mt} + \boldsymbol{\gamma}\mathbf{S}(\mathbf{t}) + \boldsymbol{\delta}\mathbf{W}(\mathbf{i}) \qquad (3)$$

where u_{mt} is local unemployment, $\mathbf{S}(\mathbf{t})$ captures seasonal effects and $\mathbf{W}(\mathbf{i})$ controls for individual characteristics. In the DD estimation when parents with older youngest children are in the control group, only municipalities that did not offer childcare before the reform are included, and the estimation with additional covariates is:

$$\mathbf{x}'\boldsymbol{\lambda} = \lambda_1 Z^{a} + \lambda_2 Z^{t} + \lambda_3 CC_{at} + \lambda_4 u_{mt} + \boldsymbol{\gamma}\mathbf{S}(\mathbf{t}) + \boldsymbol{\delta}\mathbf{W}(\mathbf{i}) \qquad (4)$$

Because there is one additional dimension to compare over, it is possible to run placebo estimations. The placebo estimation for equation 3 will be run with only non-target parents with older youngest children. The parents living in treatment municipalities where childcare was not available before the reform will, in this placebo estimation, have $CC_{mt} = 0$ before the reform date and $CC_{mt} = 1$ after. The placebo estimation for equation 4 uses the control municipalities where childcare was available for unemployed parents before the reform, but parents with younger children will have $CC_{at} = 0$ before the reform date.

If α_4 and λ_3 are close to zero in the placebo regressions this indicates that the assumptions are realistic. If this is not the case in any of the estimations, there may be both municipality trends and trends within groups of parents with the youngest child of different ages. To control for both of these trends,

[9] $Z^{m1} = 1$ for those municipalities that did not offer any childcare before the reform, and $Z^{m2} = 1$ for those municipalities offering childcare for a limited number of months. Both are otherwise equal to zero.

Table 3: Difference-in-difference-in-differences

Treatment municipality $Z^m = 1$	After $Z^t = 1$	Before $Z^t = 0$	Difference
Target $Z^a = 1$	$\beta_1+\beta_2+\beta_3+\beta_4$ $+\beta_5+\beta_6+\beta_7$	$\beta_1+\beta_2+\beta_4$	$\beta_3+\beta_5$ $+\beta_6+\beta_7$
Non target $Z^a = 0$	$\beta_1+\beta_3+\beta_5+\beta_7$	$\beta_1+\beta_7$	$\beta_3+\beta_5$
DD_T			$\beta_6+\beta_7$
Control municipality $Z^m = 0$	After $Z^t = 1$	Before $Z^t = 0$	Difference
Target $Z^a = 1$	$\beta_2+\beta_3+\beta_6+\beta_7$	$\beta_2+\beta_7$	$\beta_3+\beta_6$
Non target $Z^a = 0$	$\beta_3+\beta_7$	β_7	β_3
DD_C			β_6
$\text{DDD}=\text{DD}_T-\text{DD}_C$			β_7

difference-in-difference-in-differences (DDD) estimation can be used.

3.2 Difference-in-difference-in-differences

In a basic DDD-estimation, the linear specification is given by:

$$\mathbf{x}'\boldsymbol{\beta} = \beta_1 Z^m + \beta_2 Z^a + \beta_3 Z^t + \beta_4 Z^m Z^a + \beta_5 Z^m Z^t + \beta_6 Z^a Z^t + \beta_7 Z^m Z^a Z^t \quad (5)$$

where Z^m indicates if a municipality did not offer childcare before the reform, Z^a indicates if the parent belongs to the target group with the youngest child between two and six years old and Z^t indicates time after the reform. β_7 gives the effect of childcare because $Z^m Z^a Z^t$ measures the difference in availability of childcare for the target group in the treatment municipalities. As in the DD estimation, I use CC_{mat}, which equals one if childcare (or school) is available instead of $Z^m Z^a Z^t$. The difference for the target group in the treatment municipalities when childcare is available will still be measured by β_7, as can be seen in table 3.

As in the first DD estimation, apart from the change of the DDD variable to CC_{mat} in equation 5, I include an additional variable for those municipalities offering childcare for a limited number of months before the reform, Z^{m2}, in addition to interactions of this variable with Z^a and Z^t. I also include u_{mt}, $\mathbf{S(t)}$ and $\mathbf{W(i)}$ to control for local unemployment, seasonal effects and individual characteristics.

The full model to be estimated will be:

$$\begin{aligned}\mathbf{x}'\boldsymbol{\beta} =& \beta_1 Z^{m1} + \beta_2 Z^{m2} + \beta_3 Z^a + \beta_4 Z^t + \beta_5 Z^{m1} Z^a + \beta_6 Z^{m2} Z^a + \beta_7 Z^{m1} Z^t \\ &+ \beta_8 Z^{m2} Z^t + \beta_9 Z^a Z^t + \beta_{10} CC_{mat} + \beta_{11} u_{mt} + \boldsymbol{\gamma}\mathbf{S(t)} + \boldsymbol{\delta}\mathbf{W(i)}\end{aligned} \tag{6}$$

In both the DD and the DDD estimations, the standard errors are clustered on municipalities.

Because childcare has traditionally been performed by mothers, the availability of childcare to unemployed parents may affect mothers and fathers differently. Therefore, the estimations will be done separately for men and women.

3.3 Proportional hazard model

To estimate how the availability of childcare affects the probability of becoming employed for unemployed parents with younger children, the DD and DDD estimations are applied to a proportional hazard model (Cox, 1972). In this model, the conditional hazard rate (the probability of leaving unemployment), $\lambda(t|\mathbf{x}, \boldsymbol{\beta})$, is factored into separate functions according to:

$$\lambda(t|\mathbf{x}, \boldsymbol{\beta}) = \lambda_0(t)\phi(\mathbf{x}, \boldsymbol{\beta}) \tag{7}$$

where $\lambda_0(t)$ is the baseline hazard capturing any state dependence and $\phi(\mathbf{x}, \boldsymbol{\beta})$ is a function of $\mathbf{x}(t)$. Only the current value of $\mathbf{x}(t)$ matters, not the entire history of $\mathbf{x}(t)$, but $\mathbf{x}(t)$ is allowed to vary over the unemployment spell. The model is semiparametric, where the baseline hazard is unspecified and the functional form of $\phi(\mathbf{x}, \boldsymbol{\beta})$ is fully specified as:

$$\phi(\mathbf{x}, \boldsymbol{\beta}) = \exp(\mathbf{x}'\boldsymbol{\beta}) \tag{8}$$

The $\boldsymbol{\beta}$-vector is found by partial likelihood estimation, and the baseline hazard drops out in the estimation but may be estimated in a second step. This second step is not done in this study because the interest here is the effect of availability of childcare and not whether there is any state dependence. The results will be interpreted as hazard ratios, $\exp(\beta_i)$. If x_i changes by one unit, the probability of leaving unemployment will change by $\exp(\beta_i) - 1$.

The strength of this model is that it is possible to include time-varying covariates and handle right-censored data; i.e., a parent whose unemployment spell ends in an outcome other than employment can still be included in the analysis.[10]

4 Data

The data set used in this study includes register data of all individuals in Sweden together with all unemployment spells registered at the Public employment service office in Sweden. The propensity to register at the labor market office is very high among unemployed individuals because registration is required to receive unemployment benefits.

My sample consists of unemployed parents, with their youngest child being between two and ten years old, who registered at the labor market office between July 2000 and June 2002 and lived in one of the 208 municipalities where it is possible to classify the availability of childcare before the reform[11]. As mentioned in section 2.2, municipalities could offer childcare to unemployed parents differently according to whether or not they already had a childcare placement for their child. To minimize the risk of parents being miscategorized as having childcare in the control municipalities when they did not, only those parents with unemployment insurance are included because an unemployed individual must have some employment history to be eligible for unemployment insurance. Of the mothers in the sample, 83 percent have UI, and for fathers, the number is 88 percent. All spells are censored at the reform date, July 1st, 2001 (or, for spells beginning after the reform date, one year later) because it is very unlikely that the municipalities that did not offer childcare before the reform would be able to provide childcare to all unemployed parents immediately at the reform date.

The time span was chosen to be as close as possible in time to the reform date, but still long enough to control for seasonal effects. Because parents are entitled to paid parental leave for more than a year (450 days at the time of the reform), parents whose youngest child is older than two years old are used to minimize cases in which an unemployed parent has a spouse on parental leave taking care of the child. Because it is unclear whether childcare is available for unemployed parents participating in labor market programs, these parents' unemployment spells are censored when they participate in any form of program. Also, if a parent's employment is subsidized by the government, the spell is censored. If a parent has

[10]The problem is whether there is unobserved heterogeneity, which causes a selection problem. If this is the case, β is probably underestimated, but the asymptotic bias is towards zero; see Van den Berg (2001).

[11]see table 1, section 2.2

temporary work for ten days or less, this is included in the unemployment spell.

To control for individual heterogeneity, I use a large number of covariates, including 5 dummies for education level, 20 dummies for regions, age and age squared, and dummies for being an immigrant, a disabled worker and being married. Seasonal effects are captured by 11 time-varying dummies for month. As all spells are censored on July 1st, a control variable for entering month will also be included to control for the fact that the unemployment spells are allowed to be of different lengths depending on when the parent became unemployed. Local labor market conditions are captured by time-varying municipal unemployment rates. Sample characteristics and the reasons for ending the unemployment spells are shown in tables 4 and 6 for target parents and tables 5 and 7 for non-target parents. In these tables, parents are divided into groups according to the municipalities in which they live and whether their unemployment spell began before or after the reform date. In the estimation, only ordinary work implies leaving unemployment; the remaining destinations are censored.

Table 4 shows that, for target mothers in the treatment municipalities, the mean unemployment duration is approximately the same after the reform compared with before, while in the other municipality groups, the mean duration increased. There are also more target mothers in the treatment municipalities whose unemployment spells ended in work after the reform. As seen in table 5, this is not the case for the non-target mothers, where no particular change can be seen in spells that end in work. The mean duration increases for the non-target mothers living in the treatment municipalities, but even more for the non-target mothers in the control municipalities. For the non-target mothers living in municipalities only offering childcare for a limited number of months, the mean duration decreases. For the target fathers in the treatment municipalities, the mean unemployment duration decreased (table 6), but it decreased even more for the non-target fathers in the same municipalities. It is also notable in these tables (4-7) that, even though the total shares of immigrants were similar in the treatment and control municipalities (see table 2), the share of unemployed immigrant parents was lower in the treatment municipalities compared with the other municipalities.

For a graphical presentation showing the Kaplan-Meier estimates of survival in unemployment, see Appendix A.

From the descriptive statistics it appears that there are effects on the probability of finding work for mothers, but not for fathers, when childcare is available for unemployed parents. Because there are similar effects for parents with older youngest children (non-target group), it is very

Table 4: Descriptive statistics - target women

	Before reform			After reform		
Municipality	Treatment	Limited	Control	Treatment	Limited	Control
Duration (days)	55.0	57.2	56.6	55.1	60.8	61.4
Age	32.5	32.5	32.8	32.4	32.8	33.1
Immigrants	0.142	0.255	0.247	0.159	0.258	0.254
Married	0.437	0.464	0.458	0.439	0.468	0.469
Elementary school < 9 years	0.015	0.024	0.019	0.013	0.024	0.019
Elementary school	0.115	0.114	0.125	0.126	0.119	0.121
High school ≤ 2 years	0.371	0.366	0.354	0.355	0.332	0.327
High school ≤ 3 years	0.279	0.281	0.257	0.278	0.289	0.269
University < 3 years	0.150	0.146	0.146	0.167	0.149	0.154
University ≥ 3 years	0.070	0.067	0.095	0.059	0.084	0.108
Number of spells	1378	3800	21242	1277	3650	21241
Percent of spells ending in:						
Work	32.37	32.74	33.54	36.81	33.89	33.76
Subsidized work	1.45	0.87	0.93	1.10	0.82	1.07
Labor market program	13.43	12.16	10.11	14.41	11.70	11.52
Other destination	5.37	5.37	6.81	8.46	7.07	8.23
Studies	7.62	7.84	8.05	6.50	6.82	7.24
Censored due to time	39.77	41.03	40.55	32.73	39.70	38.18

Table 5: Descriptive statistics - non-target women

	Before reform			After reform		
Municipality	Treatment	Limited	Control	Treatment	Limited	Control
Duration (days)	60.4	68.0	60.4	61.1	63.4	67.1
Age	36.7	37.1	37.3	37.1	37.1	37.6
Immigrants	0.157	0.284	0.251	0.196	0.297	0.268
Married	0.485	0.509	0.460	0.468	0.476	0.462
Elementary school $<$ 9 years	0.017	0.023	0.026	0.019	0.027	0.026
Elementary school	0.111	0.135	0.119	0.113	0.114	0.117
High school $\leq$ 2 years	0.409	0.364	0.384	0.392	0.359	0.361
High school $\leq$ 3 years	0.208	0.236	0.202	0.222	0.240	0.211
University $<$ 3 years	0.151	0.151	0.158	0.152	0.150	0.160
University $\geq$ 3 years	0.104	0.089	0.108	0.096	0.107	0.122
Number of spells	952	2444	12876	893	2370	13027
Percent of spells ending in:						
Work	37.18	35.43	35.43	37.40	35.27	35.81
Subsidized work	1.89	1.35	1.12	1.79	1.60	1.42
Labor market program	15.97	11.62	11.70	14.89	13.12	12.24
Other destination	4.52	6.18	6.11	8.96	6.92	7.58
Studies	6.62	6.34	7.95	5.15	8.23	7.37
Censored due to time	33.82	39.08	37.69	31.80	34.85	35.59

Table 6: Descriptive statistics - target men

	Before reform			After reform		
Municipality	Treatment	Limited	Control	Treatment	Limited	Control
Duration (days)	72.5	68.6	68.5	70.5	72.5	74.7
Age	35.0	36.3	36.1	35.4	36.4	36.3
Immigrants	0.168	0.316	0.295	0.164	0.328	0.294
Married	0.455	0.582	0.551	0.481	0.552	0.565
Elementary school < 9 years	0.016	0.030	0.024	0.018	0.028	0.024
Elementary school	0.102	0.141	0.134	0.099	0.132	0.131
High school ≤ 2 years	0.629	0.480	0.465	0.586	0.460	0.429
High school ≤ 3 years	0.156	0.162	0.155	0.161	0.169	0.155
University < 3 years	0.060	0.104	0.120	0.079	0.118	0.136
University ≥ 3 years	0.034	0.080	0.094	0.057	0.091	0.121
Number of spells	685	1950	9758	669	1862	10200
Percent of spells ending in:						
Work	50.95	41.23	44.73	46.79	40.76	42.53
Subsidized work	2.34	2.00	2.38	1.94	2.47	2.18
Labor market program	13.87	13.23	11.24	16.44	13.27	12.35
Other destination	6.42	5.28	6.89	7.62	6.93	7.63
Studies	3.21	3.28	3.87	2.84	3.60	3.28
Censored due to time	23.21	34.97	30.89	24.36	32.98	32.03

Table 7: Descriptive statistics - non-target men

	Before reform			After reform		
Municipality	Treatment	Limited	Control	Treatment	Limited	Control
Duration (days)	78.4	73.9	75.6	72.7	79.1	82.9
Age	40.4	40.7	40.9	40.5	40.9	41.0
Immigrants	0.163	0.254	0.235	0.167	0.271	0.253
Married	0.604	0.621	0.620	0.620	0.640	0.606
Elementary school < 9 years	0.024	0.034	0.035	0.019	0.041	0.034
Elementary school	0.165	0.158	0.156	0.154	0.155	0.139
High school ≤ 2 years	0.578	0.515	0.495	0.582	0.474	0.481
High school ≤ 3 years	0.124	0.121	0.125	0.103	0.155	0.129
University < 3 years	0.075	0.094	0.095	0.099	0.099	0.108
University ≥ 3 years	0.034	0.074	0.088	0.044	0.073	0.106
Number of spells	533	1334	6655	526	1271	6904
Percent of spells ending in:						
Work	53.28	45.95	48.82	53.99	46.50	46.28
Subsidized work	2.06	2.92	2.67	1.90	2.52	3.20
Labor market program	12.76	13.19	11.93	16.54	11.72	12.07
Other destination	6.38	6.52	6.31	7.60	8.34	7.01
Studies	3.19	3.15	3.01	1.14	2.52	2.72
Censored due to time	22.33	28.26	27.26	18.82	28.40	28.72

important to control for municipality characteristics, which is done by including the monthly local unemployment rates, population and share of children aged 2-6 years.

5 Results

5.1 Difference-in-differences

Estimation results of the effects of childcare availability from the first DD estimation, with parents living in municipalities where childcare was available before the reform as the control group, are given in the first row of table 8. Standard errors are given in parentheses and p-values in brackets. For both mothers and fathers, the estimates are greater than zero in all estimations, but it is only for mothers that the estimates are significantly different from zero. Because the estimates increase when additional covariates are included in the full model and therefore some unobserved heterogeneity is taken away, more unobserved heterogeneity will probably increase the estimates even more. The estimated effect is large, with an increased probability of finding work of 20 percent for mothers if childcare is available when the parent is unemployed (given by taking $\exp(\beta_i) - 1$, see section 3.3).

Table 8: Estimation results for childcare, DD over municipalities

	Mothers		Fathers	
	Basic	Full	Basic	Full
Target Group	0.153	0.181	0.0312	0.0675
	(0.0759)	(0.0677)	(0.0720)	(0.0635)
	[0.044]	[0.008]	[0.664]	[0.288]
N	52588	52588	25124	25124
Placebo Estimation				
Non Target group	0.0772	0.137	0.114	0.147
	(0.0836)	(0.0768)	(0.0904)	(0.0724)
	[0.356]	[0.074]	[0.206]	[0.042]
N	32562	32562	17223	17223

Standard errors clustered on municipality in parentheses, P-values in brackets.

The problem is that there are probably reasons other than childcare that are captured by the childcare variable because there are estimates greater than zero even in the placebo estimations (second part of table 8), where parents with older children are compared, and no effect would be found if the assumption for this DD estimation were fulfilled. In the basic estimation with no additional covariates, the effect is insignificant for both mothers and fathers. For mothers, the effect is also smaller than for the target group. This also gives an expectation that the estimates in the DDD estimation will be smaller for mothers than they are in this estimation. However, for fathers, the point estimate is bigger and significant in the full model with additional covariates when estimated for parents with older youngest children. Estimation results and standard errors for more covariates are shown in table B 1 for the target group and in table B 2 for the placebo estimations in Appendix B.

In this first DD estimation parents living in municipalities offering childcare to unemployed parents for a limited number of months before the reform are also included. This means that the estimates are determined also by parents losing their childcare placement. The effect may be asymmetric between getting and loosing childcare and I have therefore also done estimations without the municipalities offering childcare a limited number of months. This gives similar estimates for mothers but the precision decreases. For fathers the estimates differs more but for the target group the point estimates are still insignificant.

The results from the second DD estimation within treatment municipalities with parents with older youngest children as the control group are shown in table 9. As there were only 14 municipalities where childcare was not available before the reform, the sample size is much smaller. None of the estimates are significant at any sufficient level, but the estimates are greater than zero for mothers and smaller than zero for fathers.

In the placebo estimations, none of the estimates are significantly different from zero and all are close to zero, which indicates that the assumption for this DD is fulfilled. Estimation results and standard errors for more covariates from these DD estimations are shown in table B 3 for the treatment estimation and in table B 4 for the control estimation in Appendix B.

Table 9: Estimation results for childcare, DD over age of youngest child

	Mothers		Fathers	
	Basic	Full	Basic	Full
Treatment Municipalities	0.150	0.131	-0.115	-0.0939
	(0.103)	(0.118)	(0.107)	(0.119)
	[0.143]	[0.270]	[0.283]	[0.430]
N	4500	4500	2413	2413
Placebo Estimation				
Control Municipalities	0.0123	0.0228	0.0166	0.0112
	(0.0247)	(0.0254)	(0.0358)	(0.0365)
	[0.617]	[0.370]	[0.644]	[0.759]
N	68386	68386	33517	33517

Standard errors clustered on municipality in parentheses, P-values in brackets.

5.2 DDD-estimation

Estimation results from the DDD estimations are shown in table 10 (for more results, see table B 5 in Appendix B.) The first and third columns show estimates from the basic DDD model with no individual or seasonal covariates. The estimates are insignificant but greater than zero for mothers and smaller than zero for fathers.

Table 10: Estimation results for childcare from the DDD estimation

	Mothers		Fathers	
	Basic	Full	Basic	Full
Childcare	0.0877	0.149	-0.106	-0.0308
	(0.0789)	(0.0767)	(0.0929)	(0.0828)
	[0.266]	[0.052]	[0.256]	[0.710]
N	85150	85150	42347	42347

Standard errors clustered on municipality in parentheses, P-values in brackets.

In column 2 and 4 of table 10 (and table B 5), all covariates are included. The probability of leaving unemployment increases for both mothers and fathers compared with the basic model without any covariates, but it is only for mothers in the full model that the estimate is significantly different from zero. For mothers, the probability of leaving unemployment for work increases by 16 percent when childcare is available for unemployed parents.

When excluding parents living in municipalities offering childcare a limited number of months before the reform the precision decreases, due to the decreased variation, giving insignificant results, but the point estimate for mothers still gives an increased probability of finding work of 10 percent.

As was expected from the first DD estimation, when target parents in other municipalities were the control group, the estimates were smaller for mothers in the DDD estimation compared with the first DD estimation. It is though somewhat surprising that the estimates is still so large. What would be expected is an estimate that is approximately the difference between the DD for the target and the non-target groups. Since the proportional hazard model isn't linear the difference do not need to be exact and there could be expected to be small differences since parents with children starting pre-school class are censored in the DD for the target parents. In the basic estimations the differences between the DD estimations and the estimate in the DDD estimations are similar but for every added covariate the differences increases. In the full DDD estimation with all covariates the estimate for mothers are more than 0.1 larger than the differences between the DD estimations. If only using the exact same population and censoring in both the DD and the DDD estimation the point estimates in the DDD estimation is similar to the difference between the point estimates in the DD estimation, even when additional covariates are included, but the precision decreases giving insignificant results.

5.3 Heterogeneous effects

For mothers, a large effect is found, but there may be heterogeneous effects; therefore, the sample of women is divided by level of education, number of children, age group, immigrant status and marital status, respectively, to see if there are heterogeneous effects over any of these dimensions. Estimation results and standard errors from the different estimations are shown in table 11. The estimates from the DDD with all unemployed mothers are shown in the first row of table table 11. (The same is done for fathers, but the results are not presented since no heterogeneous effects are found.)

Table 11: Estimation results for childcare from DDD estimations, different subsamples

	Mothers		
Population	Basic	Full	N
All	0.0877	0.149*	85150
	(0.0789)	(0.0767)	
One child	-0.120	-0.129	27919
	(0.119)	(0.111)	
Two children	0.206*	0.281**	40013
	(0.117)	(0.113)	
> 2 children	0.145	0.259*	17218
	(0.154)	(0.137)	
Compulsory school or less	0.256	0.329**	12110
	(0.160)	(0.146)	
High school ≤ 2 years	-0.0997	-0.0260	30164
	(0.119)	(0.117)	
High school 3 years	0.189	0.252**	20932
	(0.142)	(0.123)	
More than high school	0.216	0.0348	8666
	(0.332)	(0.292)	
Age ≤ 30 years	0.0486	0.0958	21426
	(0.142)	(0.122)	
Age 31-35 years	0.109	0.188	27511
	(0.130)	(0.129)	
Age > 35 years	0.0477	0.128	36213
	(0.103)	(0.113)	
Swedish born	0.115	0.152*	63754
	(0.0778)	(0.0780)	
Immigrants	0.0104	0.135	21396
	(0.168)	(0.169)	
Married	0.179	0.262**	39521
	(0.121)	(0.126)	
Not married	0.0157	0.0676	45629
	(0.0846)	(0.0838)	

Standard errors clustered on municipality in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

When the sample was divided according to number of children, no effect could be seen for those mothers with only one child. The estimates are less than zero but insignificant. The greatest effect seems to be for those women with two children, for whom the hazard ratio (when controlling for individual characteristics) indicates that the probability of leaving unemployment for work increases by 32 percent when childcare is available. Even for mothers with more than two children, the estimate is larger than for the whole population, but it is only significant at any sufficient level when all additional covariates are included.

When the sample is divided over education level, all groups, except for those mothers with two or fewer years of high school education, have higher estimates than when all mothers are included. For those mothers with no more than two years of high school, no effect at all could be seen. The greatest effect seems to be for those mothers with very low education and secondly with university education. If the UI reform implemented at the same time (see section 2.2) had affected the unemployed mothers, those mothers with the highest earlier wages, and therefore probably those with the highest education, should have had a lower probability of finding work than other mothers. As such is not the case, the effects of the UI reform are probably similar in the control groups and therefore captured by the estimation strategy.

No heterogeneous effects over the mothers' age or immigrant status were found. Because most of the mothers were born in Sweden, the hazard ratio for Swedish-born mothers is similar to that for all mothers. Finally, married unemployed mothers seem to be more affected by availability of childcare than unmarried unemployed mothers.

6 Conclusions

In this paper, I have evaluated the effects of availability of childcare during unemployment on parents' probability of finding work using a reform implemented in Sweden in July 2001. The reform made it mandatory for Swedish municipalities to offer childcare to unemployed parents for at least 15 hours each week. Before the reform, the majority of municipalities already did this, but those that did not can be used as a treatment group in a difference-in-differences (DD) framework. In the DD estimations, two different control groups were used: parents with young children living in municipalities already offering childcare to unemployed parents before the reform and parents living in treatment municipalities whose youngest child was older than childcare age. Both of these control groups were then used in a difference-in-difference-in-differences (DDD) estimation. In both the DD and DDD estimations, a basic model, with only time and group dummies and their interactions, and a full model, with individual characteristics and seasonal effects also included, were estimated.

In the first DD estimation, with parents in other municipalities as the control group, the point estimate in the full model gave an increased probability of 20 percent of finding work for mothers when childcare is available. Unfortunately, when doing placebo estimation using parents with older youngest children, a positive effect of 15 percent was found, but the standard error is larger. This result indicates that there are probably factors other than the childcare reform increasing the probability of finding work in the treatment municipalities. In the second DD estimation, with parents with older youngest children as the control group, the point estimate was positive but insignificant. This is probably because the variation is only over 14 municipalities, and therefore, the sample size decreased substantially.

From the DD estimations, especially the first, it seems important to control for trends both within groups of parents and within municipalities, which is done in the DDD estimation. Here, the full model gives that the probability of finding work increased by 16 percent for mothers when childcare was available.

Unfortunately the precision disappears when mothers living in municipalities only offering childcare a limited amount of months before the reform are removed from the analysis. The point estimate is still large but reduced which indicate that the effect of childcare availability when unemployed may be smaller than estimated with the full sample.

For fathers, no effect could be found in any of the estimations. In the full DDD estimation, the estimate was close to zero, but the standard error was large.

When dividing the sample of mothers into different sub-populations, there was no effect of childcare availability for those mothers with two or fewer years of high school education, but large effects for both mothers with only compulsory school or less and mothers with a university education. The probability of finding work for mothers with two children increased with the availability of childcare, while no effect was found for mothers with only one child.

As was mentioned in the introduction, the expected effect is ambiguous because the availability of childcare may both decrease and increase the unemployment duration, depending on how its availability affects the parents' search intensity and reservation wage. For most mothers, the possibility of increasing search intensity seems to dominate. It is somewhat surprising that no effect was found for fathers when such large effects were found for mothers, but this may be because the responsibility for caring for children still rests mainly with mothers (Statistic Sweden, 2003).

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Appendix A: Graphical presentation

Figure A 1 show Kaplan-Meier survival estimates of survival in unemployment. Only work is seen as leaving unemployment, other spell ends are censored. The top figures show survival estimates for target mothers before and after the reform, while the bottom figures are for non-target mothers with older children. The solid line represent those municipalities where no childcare was available before the reform (treatment municipalities), and the dashed line represents those municipalities where childcare was available without any time restriction before the reform (control municipalities). Mothers living in one of the municipalities only offering childcare for a limited number of months before the reform are not included in the figures.

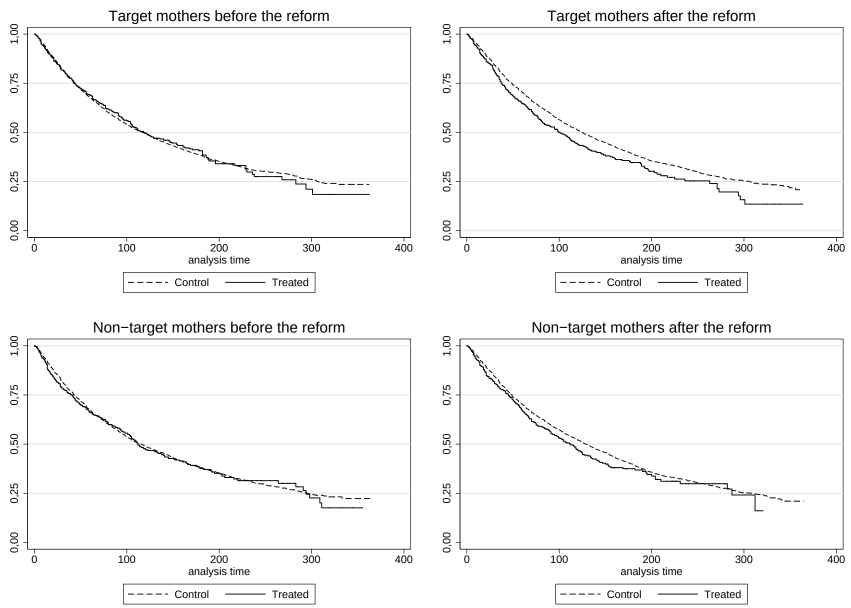

Figure A 1: Kaplan-Meier Survival estimates, Mothers.

Beginning with the target mothers it is clear that mothers in the treatment municipalities leave unemployment to a larger extend after the reform compared to mothers in the control municipalities. Studying the survival rates for the non-target mothers this could maybe to some extend be explained by the situation in the municipalities since also non-target mothers in the treatment municipalities leave unemployment earlier than mothers in the control municipalities, at least before unemployment day 200.

The same figures for fathers are shown in figure A 2. In line with the descriptive statistics in tables 6 and 7, there is a clearer increase in leaving unemployment for the non-target fathers in the treatment municipalities after the reform than for the target fathers.

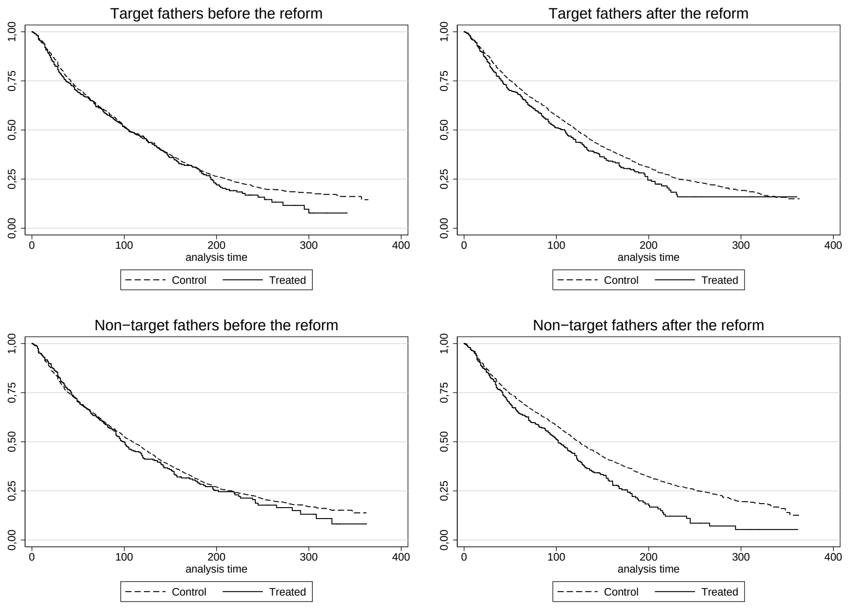

Figure A 2: Kaplan-Meier Survival estimates, fathers.

Appendix B: Estimation results

Table B 1: Estimation results from the DD over municipalities, target parents

	Mothers		Fathers	
	(1)	(2)	(3)	(4)
Childcare	0.153**	0.181***	0.0312	0.0675
	(0.0759)	(0.0677)	(0.0720)	(0.0635)
Z^{m1}	0.172*	0.177**	0.133*	0.129
	(0.0939)	(0.0747)	(0.0781)	(0.0811)
Z^{m2}	0.00163	0.0874*	-0.0351	0.0574
	(0.153)	(0.0528)	(0.259)	(0.0915)
Z^{t}	-0.0616***	-0.0926***	-0.120***	-0.132***
	(0.0217)	(0.0224)	(0.0249)	(0.0296)
Entering month		-0.309***		-0.455***
		(0.0202)		(0.0232)
Elementary school		-0.0804		0.143*
		(0.0624)		(0.0856)
High school ≤ 2 years		0.162***		0.240***
		(0.0582)		(0.0867)
High school ≤ 3 years		0.225***		0.0755
		(0.0597)		(0.0852)
University < 3 years		0.181***		-0.0702
		(0.0593)		(0.0879)
University ≥ 3 years		0.183***		0.0108
		(0.0700)		(0.0699)
Age		-0.0490***		0.0298*
		(0.0161)		(0.0159)
Age squared		0.000518**		-0.000551**
		(0.000239)		(0.000215)
Immigrant		-0.261***		-0.456***
		(0.0265)		(0.0374)
Disable		-1.200***		-1.510***
		(0.0553)		(0.0956)
Married		-0.0201		-0.0125
		(0.0176)		(0.0203)
Municipality unemployment		-4.314***		-0.565
		(1.026)		(1.352)
Population		-0.000613***		-0.000792***
		(0.000119)		(0.000222)
Share of children age 2-6		-9.388***		-16.20***
		(3.550)		(4.838)
N	52588	52588	25124	25124
LOG L	-172394.4	-171014.4	-98539.5	-96830.8

Standard errors clustered on municipality in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B 2: Estimation results from the DD over municipalities, non-target parents

	Mothers		Fathers	
	(1)	(2)	(3)	(4)
Childcare	0.0772	0.137*	0.114	0.147**
	(0.0836)	(0.0768)	(0.0904)	(0.0724)
Z^{m1}	0.128	0.182**	0.221**	0.221**
	(0.104)	(0.0920)	(0.0879)	(0.0900)
Z^{m2}	-0.0270	0.0616	0.0143	0.0587
	(0.148)	(0.0635)	(0.191)	(0.0746)
Z^{t}	-0.0671***	-0.106***	-0.126***	-0.130***
	(0.0209)	(0.0251)	(0.0279)	(0.0277)
Entering month		-0.336***		-0.467***
		(0.0224)		(0.0277)
Elementary school		0.119*		-0.0363
		(0.0650)		(0.0620)
High school ≤ 2 years		0.235***		0.0609
		(0.0650)		(0.0612)
High school ≤ 3 years		0.332***		-0.180***
		(0.0651)		(0.0685)
University < 3 years		0.220***		-0.242***
		(0.0688)		(0.0742)
University ≥ 3 years		0.276***		-0.289***
		(0.0769)		(0.0899)
Age		0.0603***		0.0523**
		(0.0227)		(0.0234)
Age squared		-0.000840***		-0.000741***
		(0.000302)		(0.000278)
Immigrant		-0.205***		-0.457***
		(0.0304)		(0.0422)
Disable		-1.336***		-1.541***
		(0.0569)		(0.0974)
Married		0.0553***		0.0305
		(0.0212)		(0.0251)
Municipality unemployment		-3.255***		0.571
		(1.234)		(1.266)
Population		-0.000556***		-0.000879***
		(0.000148)		(0.000246)
Share of children age 2-6		-9.945**		-17.67***
		(4.220)		(4.879)
N	32562	32562	17223	17223
LOG L	-110180.1	-109042.2	-72995.1	-71451.0

Standard errors clustered on municipality in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B 3: Estimation results from the DD over age of youngest child, treatment municipalities

	Mothers		Fathers	
	(1)	(2)	(3)	(4)
Childcare	0.150	0.131	-0.115	-0.0939
	(0.103)	(0.118)	(0.107)	(0.119)
Z^a	0.0769	0.0165	-0.0987	-0.181**
	(0.0557)	(0.0661)	(0.0874)	(0.0745)
Z^t	-0.0207	-0.0405	0.0768	0.137*
	(0.0884)	(0.0968)	(0.0594)	(0.0801)
Entering month		-0.378***		-0.440***
		(0.0419)		(0.0613)
Elementary school		-0.0363		0.128
		(0.213)		(0.235)
High school ≤ 2 years		0.410*		0.240
		(0.221)		(0.253)
High school ≤ 3 years		0.577***		0.0412
		(0.200)		(0.221)
University < 3 years		0.387*		-0.0402
		(0.209)		(0.306)
University ≥ 3 years		0.527***		0.303
		(0.173)		(0.285)
Age		-0.0680		0.0132
		(0.0527)		(0.0355)
Age squared		0.000979		-0.000310
		(0.000725)		(0.000436)
Immigrant		-0.125		-0.219***
		(0.116)		(0.0653)
Disable		-1.264***		-1.530***
		(0.131)		(0.164)
Married		0.0707		0.0259
		(0.0452)		(0.0582)
Municipality unemployment		-5.404*		5.867
		(2.877)		(4.034)
Population		-0.00527		-0.0106*
		(0.00457)		(0.00541)
Share of children age 2-6		-0.330		-1.569
		(14.03)		(19.59)
N	4500	4500	2413	2413
LOG L	-12025.3	-11833.3	-8423.8	-8212.0

Standard errors clustered on municipality in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B 4: Estimation results from the DD over age of youngest child, control municipalities

	Mothers		Fathers	
	(1)	(2)	(3)	(4)
Childcare	0.0123	0.0228	0.0166	0.0112
	(0.0247)	(0.0254)	(0.0358)	(0.0365)
Z^a	-0.00349	-0.0695***	0.00191	-0.0297
	(0.0220)	(0.0209)	(0.0307)	(0.0280)
Z^t	-0.0833***	-0.107***	-0.150***	-0.137***
	(0.0211)	(0.0254)	(0.0264)	(0.0285)
Entering month		-0.319***		-0.468***
		(0.0154)		(0.0222)
Elementary school		0.0231		0.0944
		(0.0483)		(0.0642)
High school ≤ 2 years		0.201***		0.183***
		(0.0455)		(0.0643)
High school ≤ 3 years		0.268***		-0.0176
		(0.0459)		(0.0604)
University < 3 years		0.213***		-0.113*
		(0.0481)		(0.0616)
University ≥ 3 years		0.225***		-0.105*
		(0.0602)		(0.0573)
Age		-0.0427***		0.0263*
		(0.0126)		(0.0145)
Age squared		0.000455***		-0.000473***
		(0.000176)		(0.000178)
Immigrant		-0.223***		-0.437***
		(0.0207)		(0.0334)
Disable		-1.322***		-1.494***
		(0.0469)		(0.0757)
Married		0.00970		0.0123
		(0.0149)		(0.0166)
Municipality unemployment		-2.278**		1.001
		(1.063)		(1.317)
Population		-0.000466***		-0.000573***
		(0.0000903)		(0.000115)
Share of children age 2-6		-5.211		-12.35***
		(3.311)		(3.910)
N	68386	68386	33517	33517
LOG L	-240432.5	-238468.3	-144269.5	-141799.0

Standard errors clustered on municipality in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B 5: Estimation results from the DDD estimation

	Mothers		Fathers	
	(1)	(2)	(3)	(4)
Childcare	0.0877	0.149*	-0.106	-0.0308
	(0.0789)	(0.0767)	(0.0929)	(0.0828)
Z^{m1}	0.0366	0.0697	0.0512	0.0715
	(0.107)	(0.0964)	(0.0930)	(0.0946)
Z^{m2}	-0.0666	0.0248	-0.0490	0.00308
	(0.155)	(0.0609)	(0.204)	(0.0772)
Z^{a}	-0.0131	-0.0826***	-0.0149	-0.0342
	(0.0189)	(0.0175)	(0.0221)	(0.0230)
Z^{t}	-0.0780***	-0.110***	-0.147***	-0.145***
	(0.0206)	(0.0251)	(0.0246)	(0.0288)
$Z^{m1} * Z^{a}$	0.0588	0.0780	-0.0869	-0.0707
	(0.0609)	(0.0518)	(0.0815)	(0.0834)
$Z^{m2} * Z^{a}$	0.0270	0.0228	-0.0382	0.00503
	(0.0291)	(0.0306)	(0.0881)	(0.0614)
$Z^{m1} * Z^{t}$	0.0876	0.0464	0.204***	0.145**
	(0.0876)	(0.0820)	(0.0604)	(0.0612)
$Z^{m2} * Z^{t}$	0.0766**	0.0748**	0.0863	0.0747
	(0.0363)	(0.0355)	(0.0561)	(0.0496)
$Z^{t} * Z^{a}$	0.00479	0.00614	0.0138	0.00406
	(0.0223)	(0.0228)	(0.0310)	(0.0317)
Entering month		-0.318***		-0.461***
		(0.0136)		(0.0193)
Elementary school		0.00519		0.0507
		(0.0426)		(0.0541)
High school ≤ 2 years		0.195***		0.148***
		(0.0407)		(0.0539)
High school ≤ 3 years		0.276***		-0.0471
		(0.0406)		(0.0516)
University < 3 years		0.196***		-0.165***
		(0.0438)		(0.0536)
University ≥ 3 years		0.226***		-0.128**
		(0.0523)		(0.0513)
Age		-0.0325***		0.0245**
		(0.0114)		(0.0118)
Age squared		0.000325**		-0.000445***
		(0.000159)		(0.000145)

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	Mothers		Fathers	
Continued from last page	(1)	(2)	(3)	(4)
Immigrant		-0.238***		-0.459***
		(0.0223)		(0.0339)
Disable		-1.279***		-1.517***
		(0.0417)		(0.0644)
Married		0.00981		0.00764
		(0.0132)		(0.0147)
Municipality unemployment		-3.861***		-0.0703
		(1.041)		(1.208)
Population		-0.000602***		-0.000824***
		(0.000127)		(0.000226)
Share of children age 2-6		-9.598***		-16.98***
		(3.528)		(4.298)
N	85150	85150	42347	42347
LOG L	-306111.5	-303583.7	-186957.9	-183688.6

Standard errors clustered on municipality in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Essay III

Workfare for the Old and Long-Term Unemployed*

1 Introduction

The burden of unemployment is in many countries to a disproportional extent placed on workers in both ends of the age distribution. The anatomy of unemployment spells does however differ dramatically between the young and the old; where the young on average have high inflow rates into unemployment but short unemployment durations, the opposite pattern holds for older workers. Few old workers become unemployed, but those who do tend to stay unemployed for very long periods of time. In terms of policy, the two groups are also treated remarkably different; while youth unemployment often is tackled by specially provided mandatory activation or workfare programs, old unemployed instead tend to be granted extended periods of passive benefit receipt (see e.g. de Georgi, 2005 and Tatsiramos, 2010). One possible rationale for this difference is that the old and jobless are considered unemployable, regardless of which policy measures are used, and that passive financial insurance therefore is to be preferred over workfare policies. This paper contributes to the stock of policy relevant knowledge by studying how the job finding rate is affected by a policy shift from very long passive benefits towards (earlier) workfare policies for older long-term unemployed workers.

There is an existing, quite extensive, literature on how the design of unemployment insurance systems affects unemployment duration. Numerous studies have found that the probability of leaving unemployment increases when job-seekers approach the time of benefit exhaustion (e.g.

*Co-authored with Helge Bennmarker and Oskar Nordström Skans

Addison and Portugal, 2004; Card, Chetty, and Weber, 2007; Carling, Edin, Harkman, and Holmlund, 1996; Ham and Rea 1987; Katz and Meyer, 1990; Meyer, 1990; Røed and Zhang, 2003) and that the unemployment duration increases when maximum benefit duration is extended (e.g. Card and Levine, 2000; Hunt, 1995; Katz and Meyer, 1990; Lalive, Van Ours, and Zweimller, 2006; Meyer, 1990; van Ours and Vodopivec, 2006).

The interpretation of benefit exhaustions is, however, likely to vary depending on the institutional context (see e.g. Røed and Westlie, 2012 or Carling, Edin, Harkman, and Holmlund, 1996). One reason is that other public transfers may be available when UI-benefits expire. Our data are drawn from Sweden in the 1990s, where expired passive UI-benefits implied that the unemployed got access to other, equally generous, benefits if they agreed to participate in an active labor market program which was universally offered. Thus, benefit exhaustion did not change the financial incentives to search for jobs, but it made program participation an additional requirement for continued benefits. Importantly, programs were available as an option already before benefits expired, but became mandatory (i.e. a condition for continued benefits) at the time of UI-exhaustion.

There are a number of previous studies on the effects of mandatory programs, in particular for youths. Examples include Carling and Larsson (2005), Forslund and Skans (2006) and de Georgi (2005) who use age discontinuities to study the impact of mandatory programs on youths in Sweden and the UK, all finding evidence of positive short run pre-program (threat) effects. Dolton and O'Neill (1996, 2002) evaluate the restart program for long-term unemployed in the UK and find positive long run effects, at least for males. Hägglund (2011) analyses a set of randomized experiments in Sweden where job seekers were called to participate in mandatory job-search programs with a few weeks' notice and found evidence of increased job finding before the programs started. These types of pre-program effects are also found by Black, Smith, Berger, and Noel (2003), Geerdsen (2006) and Geerdsen and Holm (2007). In a particularly relevant study, Graversen and van Ours (2008) document the effects of mandatory programs in Denmark using data from an experiment where unemployed workers were randomly assigned into a mandatory program. They find evidence of positive average effects (mainly stemming from increased monitoring), which appear to be particularly large for older unemployed.

Whereas activation has been extensively used (and hence studied) for youths, the policies aimed at older unemployed instead tend to focus on extended durations of passive benefits (see e.g. Tatsiramos, 2010). The theoretical underpinnings for this policy route appear weak, however.

Michelacci and Ruffo (2011) argue that the distortions from UI benefits are larger, and that the insurance motive is smaller, among older workers, suggesting that UI-benefits in fact should be less generous for the older unemployed. In general, the probability for unemployed workers to find employment also tends to decrease when approaching retirement age (Hairault, Langot, and Sopraseuth, 2011). Interestingly, this seems to be a margin that can be affected by policy. Lalive (2008) analyze very clear age and regional discontinuities in an Austrian setting and the results show that extended benefits for older workers leads to longer unemployment spells, but also that the effect becomes enormously large if the UI-periods become sufficiently long to bridge into the retirement system. On the other hand, cutting older workers off benefits altogether may be a politically infeasible policy option, suggesting a potential role for workfare policies. A rationale for making programs mandatory is as a means to verify that the worker is available for work and at the same time reduce the value of unemployment without inducing poverty among those who cannot find employment (see e.g. Andersen and Svarer, 2007).

This paper studies a Swedish policy reform in 1998 which raised an age threshold in the UI system and thereby effectively reduced the maximum duration of passive UI-benefits from 90 to 60 weeks for workers aged 55 or 56. Program slots (mostly work practice) were offered to all unemployed who lost their UI-benefits and benefit levels during program participation was exactly at par with the UI system. A new spell of passive benefits was granted after six months of program participation. As expected, the inflow into active labor market programs increased massively around 60 weeks after registration for the covered group.

We analyze the changes in job finding probabilities due to the reform using a control group consisting of a mix of slightly older and slightly younger workers. Our results show that the reform increased transitions to jobs among the covered workers. The effects appear around the time of inflow into the programs, suggesting that the effects are due to the confiscation of leisure, rather than arising from human capital accumulation due to program participation. We further show that the reform caused average monthly earnings among the workers who returned to employment to increase. Assuming that the reform did not improve the average unobserved earnings potential among those finding jobs, this result suggests that the main effects are driven by increased search intensity rather than by reduced reservation wages, and that match quality did not deteriorate.

Overall, the results suggest that earlier, mandatory, program participation may induce older workers to find jobs earlier, that the extensive use of long periods of passive benefits for older workers in many European countries may contribute to long-term unemployment among this group, and that the older workers' unemployment periods can be reduced without inducing poverty among those who are unable to find jobs.

The paper is structured as follows: In Section 2 we discuss the overall institutions and describe the relevant labor market conditions. In Section 3 we describe the data and the empirical methods. Section 4 presents the empirical results. Section 5 provides a discussion.

2 Institutions and labor market conditions

2.1 UI and ALMPs in the 1990s

Our empirical analysis uses a reform in the Swedish UI system in 1998. Here we therefore briefly describe the relevant Swedish institutions in the mid to late 1990s[1]. During this period, an unemployed worker was entitled to UI benefits if he or she

i had been a member (voluntary) of a UI-fund for 12 months,

ii had been employed for six months before becoming unemployed, and

iii was registered (and complied with search requirements) at the Public Employment Service (PES).

The PES is responsible for monitoring job search, administrating sanctions, providing job search assistance, and administrating active labor market programs. Program participants receive an alternative transfer, "Activity Support" (AS), with identical benefit levels as in the UI system. Workers who participated in programs (and therefore received AS) did not consume UI-days during the duration of the program.

A particular feature (see e.g. Sianesi, 2008) was that UI-recipients who participated in an active labor market program lasting for at least six months re-qualified for a new period of passive UI-benefits. Thus, it was possible to remain on benefits for an indefinite time period by "cycling" between UI and AS.

New UI spells started with a five day uncompensated waiting period after which, for most workers, UI lasted for approximately 60 weeks (300 work days). Workers aged 55 or older after 60 weeks and who became unemployed before January 1, 1998 were however given 30 additional weeks (150 work days) of passive UI benefits. The age threshold was

[1]For a more detailed description, see Carling, Edin, Harkman, and Holmlund (1996).

raised from 55 to 57 for unemployment spells beginning after January 1, 1998.

When passive UI benefits expired after 60 or 90 weeks, unemployed workers were offered to participate in a labor market program, and hence receive unchanged transfers through the AS-system. The contents of the programs could vary, but the primary focus was on work practice schemes.

The benefit levels were determined by the previous wage with a 75 percent replacement rate until 31th of August 1997 and thereafter with an 80 percent replacement rate. There was a floor compensation of 230 SEK (29 USD)[2] and a cap of 564 SEK (71 USD) per day until end of December 1997 when they were changed to 240 SEK (30 USD) and 580 SEK (73 USD) respectively. All of these numbers refer to both the UI and AS systems. A major reform in February 2001 changed many features of the system, including an abolishment of the age threshold for UI duration[3].

In Appendix A we illustrate the evolution of transfers and disposable income over the course of unemployment spells for older long-term unemployed workers. The data show that the net level of insurance (accounting for all taxes and transfers) was high, disposable income never fell below 74 percent of the pre-unemployment disposable income. The data are also fully in line with the institutional description above; UI payments fell in the second full year after job loss, but other transfers (AS) fully compensated.

2.2 Age and unemployment in Sweden

The period under study, with the reform taking effect at the beginning of 1998, was one of recovery from a very deep recession, as shown in Figure 1. The figure also shows that, as in most countries, unemployment is higher among the youths than among prime aged and older workers in Sweden. Unemployment among 55 to 64 year olds on the other hand has remained around the unemployment rates of prime-aged workers.

Although the unemployment rates among older workers do not stand out as particularly troubling in the Swedish context, this is entirely driven by low rates of unemployment inflow. The time it takes to find a job increases monotonically over the age distribution (see Figure 2).

[2] According to the exchange rate of about 8 SEK/USD in January 1998.

[3] Since 2001 workers are transferred into a never-ending (but low-intensive) program when passive UI-benefits expire. During 2001 to 2006 case workers at PES were given discretion over whom to offer extended benefits (an additional 60 weeks) and whom to place in mandatory programs after 60 weeks. Currently the 60 week limit for mandatory programs is binding for all except parents with children (90 weeks), and for youths (90 days). Participants receive AS when UI-benefits have expired also in the current system, but benefit levels can be lower.

Figure 1: Unemployment rates by age in Sweden, according to OECD

Note: We truncate the data series in 2004 due to a major break in the data series in 2005.

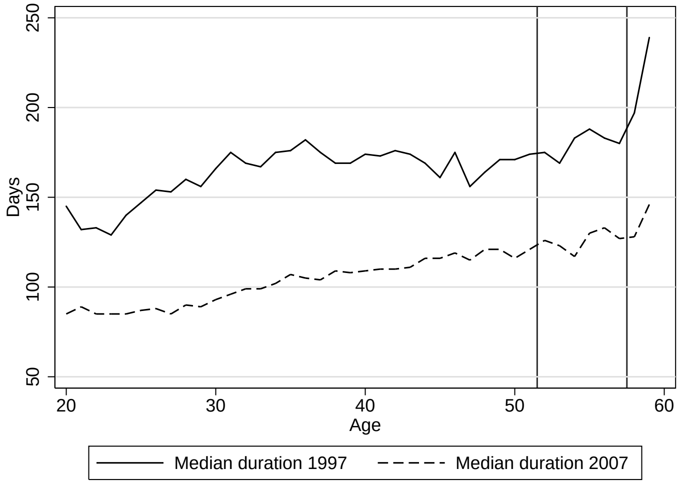

Figure 2: Median unemployment duration (in days) to jobs, by age at inflow (PES data) in 1997 and 2007

Note: Based on own calculations. The two vertical lines indicate the age span used in this paper.

3 Data, description and methods

3.1 Data

The data used in this paper are all drawn from population wide registers in the IFAU-database. We combine data originating from Statistics Sweden, the PES, and the UI-funds. More specifically, we use the PES registers on unemployment spells (Händel), the UI-payment register (AKSTAT), an income and population register (Louise) and tax records of employer-employee transfers (Rams).

The PES register is an event database which records start and end dates of registered unemployment spells (registration is a prerequisite for UI and AS benefits) as well as reasons for exits, together with start and end dates of various forms of "search categories" such as ("open") unemployed job search, on-the-job search (employees who are registered at PES), and participation in active labor market programs (ALMPs).

We construct our study population from the inflow into PES-registered unemployment among workers who are eligible for UI-benefits. In order to exclude workers with ongoing UI-spells we restrict the sample to workers who had a maximum of 10 days of unemployment during the year before registration. For the same reason, we also exclude workers who participated in some form of subsidized employment during the preceding year. We further require that they receive payments from the UI-fund. In order to increase precision we also exclude very short unemployment spells (lasting 5 weeks or less) under the assumption that the outflow this early is unaffected by changes in UI-rules which cover those that remain unemployed for at least 60 weeks.

Our empirical models (see below) analyses hazards from unemployment to jobs. We use a definition of unemployment which as closely as possible mimics official definitions of unemployment, letting PES-registration proxy for active job search. We therefore consider workers as unemployed if they are registered at the PES as long as they are not being registered as searching on the job. On-the-job search include those in temporary, part-time and partly subsidised employment. Participants in active ALMPs such as work practice or training are considered as unemployed. Unemployed who exit to a job according to the PES registers, or who spend more than 10 days registered as searching on the job, are considered as having found a job. Other exits are censored in our main specification. We also censor all spells after 120 weeks, as well as spells reaching into February 2001 when the entire UI system was reformed.

As an alternative source of information regarding outcomes we use a register of the total annual earnings received by each worker from each employer, as well as the first and last remunerated month during that year and employer-employee combination. We use these data for two purposes. The first is to analyse the impact of earnings conditional of job-finding. The second is as an alternative measure of transitions into employment. In order to quantify transitions, we code workers as employed in the first month where earnings (from all jobs) exceed one third of a minimum wage (corresponding to the 10 day limit).[4] These data are however somewhat imprecise in terms of the exact measurement of when employment takes place since the month indicators refer to the time of payment (not the time of work) and since missing month indicators are indistinguishable from spells lasting from January to December in our raw data. We therefore focus our main analysis on the more precise PES data.

Our key explanatory variables are age and calendar year. The combination of these defines the duration of passive UI-benefits. Consistent with the UI-rules, we define *year* as the year of inflow, and *age* in years 60 weeks after the start of the UI-spell.

As controls we include a very rich set of variables capturing seasonality, socioeconomic status, and previous labor market experience. The variables include gender, immigration status, three indicators for level of education, and a marital status indicator from population registers. From the PES register we code calendar month of entry, days unemployed previous four years, indicators for previous (two to four years back)[5] unemployment, a disability indicator[6], and ten indicators for type of municipality[7]. Finally, we include ten indicators for previous occupations[8] as well as the wage underlying the UI-benefit level, all drawn from the UI-records.

[4]The minimum wage is calculated as the 10th percentile in the overall wage data using data from the Structure of Earnings Statistics.

[5]One year back all individuals are working due to the restrictions we put on our sample.

[6]Recorded disabilities are heavily affected by unemployment duration (see Johansson and Skedinger, 2008) and are, in our data, recorded at the end of each spell. We therefore only use information from disabilities in previous spells.

[7]Grouped according to classification by Swedish Association of Local Authorities and Regions (SKL): Metropolitan centre, metropolitan suburban, larger centre, larger suburban municipality, commuting municipality, smaller tourism oriented municipality, smaller goods producing municipality, rural municipality, municipality in densely populated area, municipality in mainly unpopulated area.

[8]Based on UI-fund information, indicators for each of the 8 largest funds and the rest aggregated into one residual white collar category and one residual blue collar category.

3.2 Description

The unemployed

We start by describing our sample of workers. In Table 1 we show descriptive statistics for pre-determined characteristics. We provide four descriptive columns, before (1996-97) and after (1998-99) the change in age threshold for the "control group" (i.e. workers aged 53-54 and those aged 57-58) and for the treatment group (workers aged 55-56). In the final column we show the differences in differences between these. We use the composite control group (i.e. a mix of slightly older and younger workers) since using any of the two parts (older or younger) separately gives us a very unbalanced experiment in terms of background characteristics due to age and cohort differences.

Starting with the overall characteristics of the sample, we see that the average age is close to 56 as expected. Slightly less than half are female and nearly two thirds are married. A large fraction of the sample is low educated, which is natural due to the cohorts involved (born in 1938-46). Almost 90 percent are Swedish born and half of the immigrants are from one of the neighboring Nordic countries. The average worker had approximately 100 days of registered unemployment during the four years preceding the analyzed spells, and the proportion of individuals being unemployed at all during two, three or four years before the current spell is between 15 and 22 percent. Between one and two percent of workers had a recorded disability from a previous unemployment spell. Pre-unemployment wages are slightly higher than the wage corresponding to the UI cap[9], and about half (somewhat increasing over time) received the maximum (cap) UI benefit level.

The final column shows DD estimates of the effect of the reform on the characteristics of the unemployed. Most of the covariates are extremely well balanced. In particular this holds for all variables capturing previous labor market performance. We do however find a very marginal significant effect on the fraction of east European immigrants, as well as significant effects on the fraction married and on the average age. Although the latter effect is quite small, and do not seem to be important enough to affect any of the other covariates (except for the marriage rate), we will analyze its importance for the results in the robustness section.

[9] Ranging from about 15,000 SEK to 16,000 SEK during the period, depending on the replacement rate and the level of the cap.

Table 1: Descriptive statistics for treatment and controls before and after the reform.

	1996-1997		1998-1999		
	Control mean	Treatment mean	Control mean	Treatment mean	DD
Age after 60 weeks	55.75	55.98	55.90	55.99	-0.136***
Female	0.444	0.447	0.436	0.432	-0.007
Married	0.656	0.640	0.630	0.640	0.027**
Schooling					
Compulsory or less	0.412	0.416	0.388	0.388	-0.003
Upper secondary	0.433	0.419	0.450	0.446	0.010
Tertiary	0.155	0.166	0.162	0.166	-0.006
Immigration status					
Born in Sweden	0.891	0.896	0.892	0.908	0.011
Other Nordic country	0.054	0.049	0.051	0.043	-0.003
Western Europe	0.020	0.018	0.015	0.016	0.002
Eastern Europe	0.019	0.022	0.022	0.019	-0.006*
Born outside Europe	0.015	0.015	0.019	0.014	-0.004
Labor market history					
Days in unemployment during previous 4 years	112	106	118	115	3.4
Unemployed:					
2 years earlier	0.169	0.159	0.156	0.159	0.013
3 years earlier	0.223	0.208	0.204	0.200	0.011
4 years earlier	0.209	0.198	0.225	0.221	0.007
Disability (previous spell)	0.016	0.018	0.018	0.016	-0.003
Previous wage (1000 SEK)	16.03	16.10	17.18	17.25	-0.003
Receiving max UI	0.422	0.424	0.570	0.567	-0.006
Observations	8,717	4,132	8,107	4,190	25,148

Note: Description of the used data set. DD estimates are from regressions with treatment and year interval dummies, and the estimates show the impact of the interaction of these. Standard errors are available upon request. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Finally, Table 2 shows reasons for exiting ongoing unemployment spells within the sample. About two thirds of the subjects exit to jobs, half of which exit to regular full time employment and the other half to jobs including temporary jobs and part time jobs. Some caution is however warranted in interpreting the relative magnitudes since the distinction between different forms of job exits in the data somewhat arbitrarily depends on how individual case workers choose to record the events. In our empirical analysis we treat all job exits as positive outcomes and perform a number of robustness checks regarding how to treat other exits.

Table 2: Reasons for exits from unemployment

	1996-1997		1998-1999	
	Control	Treatment	Control	Treatment
Regular employment	34.17	32.79	33.64	34.32
Other jobs	31.90	35.31	29.91	31.12
Studies	2.88	1.48	3.21	2.60
Lost contact	2.93	3.24	3.00	3.27
Other	9.44	8.42	10.92	10.29
Censored due to time	18.68	18.76	19.33	18.40
Observations	8,717	4,132	8,107	4,190

Note: Spells are censored due to time after 120 weeks and in February 2001. ”Other jobs” include exits to part time jobs, temporary jobs, partly subsidized self-employment and partly subsidized regular employment. ”Other exits” include transfers to other authority.

Benefit duration and transitions to programs

Figure 3 describes the flows into programs over the duration of an unemployment spell from the registration at PES. The first panel is for the period before the reform which shortened UI-duration for workers aged 55 to 56 and the second panel is for the period after. The vertical lines at 60 and 90 weeks indicate the two thresholds for when benefits on uninterrupted UI will expire depending on age and period. There are three lines in each panel, one for the younger part of control group which always had 60 weeks of UI (aged 53-54), one for the treatment group where the duration changed (aged 55-56) and one for older part of the control group which remained at 90 weeks throughout (aged 57-58).

Firstly, we see that there is a very clear peak at benefit expiration for each of the three groups. The fact that the peaks are bell shaped rather than perfectly aligned with the 60/90 week thresholds is expected since some workers may choose to enter ALMPs earlier, whereas other workers may postpone the expiration of benefits by taking shorter breaks in the UI-sequence. In particular, we do not treat short low-intensive job search programs whose participants also are financed through AS as ALMPs in this context. This ”fuzziness” implies that our analysis should

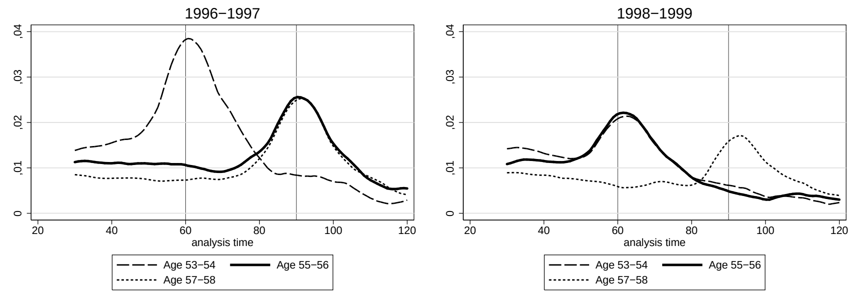

Figure 3: Smoothed hazard estimates to program by age

be interpreted in a reduced-form sense; we estimate the effects of changing expectations regarding the maximum duration of passive benefit weeks for workers who remain in unemployment.

Secondly, the figure shows that the reform moved the treatment group from exactly mimicking the older part of the control group to mimicking the younger part of the control group equally precise. We interpret this as showing that the reform affected program placement around benefit exhaustion as intended.

Since the focus of this paper is on the effects of changes in the duration of passive UI-benefits, it is useful to briefly describe the kinds of programs that were offered jointly with the AS-benefits when UI-benefits expired. In Table 3 we show the composition of programs for those who enter active labor market programs in the 30-week interval surrounding the expiration of passive benefits. The table shows that although there was a shift over time towards training (driven by computer training courses) the main thrust of the programs were offered as various forms of work practice programs.

3.3 Empirical specifications

The purpose of the empirical exercise is to pin down how the duration until workfare affects the job finding rate among older unemployed. In order to do so we rely on the change of age threshold in the UI-system. As shown in the descriptive section above, our data include workers aged 53-58, where the two intermediate cohorts (55-56) are affected by a reduction in passive UI-duration. The dual control group consists of both older and younger workers to ensure that potential exogenous changes in age related job-finding rates over time should be less of a concern. We use the unemployment inflow in 1996-97 (pre-reform) and 1998-99 (post-reform).

Table 3: Programs around end of UI

	1996-1997		1998-1999	
	Control	Treatment	Control	Treatment
Work practice	71.99	71.18	54.24	50.57
Labor market training	21.41	21.30	36.30	34.85
Other programs	6.60	7.52	9.46	14.58
Observations	1,303	399	719	528

Note: Data cover transitions into programs during the weeks 45-75 after the start of the unemployment spell for those with 60 UI weeks and during weeks 75-105 for those with 90 UI weeks. "Work practice" includes relief work, work experience schemes, workplace introduction and resource jobs within the public sector. "Labor market training" also includes computer training courses (Datortek); "Other programs" include start-up grants and the employability rehabilitation program.

As our main model we use a standard stratified Cox-proportional hazards (CPH) model where other exits than jobs are censored, but where program participation is treated as continued unemployment.

Our covariate of interest is the duration of passive benefits. We model this as a dummy variable taking the value one for workers with 60 weeks of passive benefit duration (D^{60}) and zero for those with 90 weeks duration. We further let the impact of the dummy vary over the duration of the spell in pre-specified bins (τ) which is a function of analysis time t[10].

The stratified CPH model conditions out the baseline hazard for each stratum. We stratify the model on *age*. Allowing for age-specific baseline hazards implies that each age group is allowed to have a specific relationship between job-finding hazards and unemployment duration. This is necessary for the identification of the effect of interest since UI-duration vary with age within the control group (the younger part has 60 weeks of passive benefits, and the older has 90 weeks). The stratification accounts for these differences and thereby forces the identification of the effect of interest (D^{60}) to come from changes in UI-duration over time within an age group (i.e. from the reform).[11]

Our model further includes a set of individual characteristics capturing socioeconomic status and the pre-unemployment labor market history (X)

[10]This is necessary since the effect is likely to change sign over the duration of the spell making the average effect uninformative (see below).

[11]In effect, specifying D^{60} as the interaction between time and being aged 55 to 56 therefore gives numerically identical results.

as well as year-of-in-flow dummies (*Year*). Formally the log hazard is given by:

$$\log h_i(t) = \log \lambda_0^{age}(t) + \sum_{\tau=1} \gamma_\tau D_i^{60} + Year_i \beta^y + X_i \beta^X \tag{1}$$

This model is varied in various ways in the empirical section in order to assess the robustness of the results. In particular we vary sample restrictions, the functional form of the age and time controls, the censoring, and the underlying data source. In addition, we also analyze the impact of the reform on the monthly earnings of those who find employment using a traditional (linear) difference-in-difference model.

It is important to note that the two policy regimes we compare only differ in the timing dimension. The short passive UI-duration is compared to longer UI-duration within the same overall framework. Our design is therefore well-suited to analyze effects on transitions relatively early on in the spells, but less well-suited to analyze the impact on transitions later on in the spells. This point is shown in a very stylized example presented in Table 4 below. The table denotes the period-specific baseline hazard by h and assumes that expiring UI (i.e. forced transitions into programs) has a deterrence effect (γ) in the period when UI expires and a post-program effect (ϕ) in the period thereafter.[12]

Table 4: A stylized model of shorter passive UI and hazards to jobs

Hazard to jobs, by period and duration of passive benefits				
	Period 1	Period 2	Period 3	Period 4
Passive benefit duration:				
Two-period UI	*h1*	*h2*+γ	*h3*+ϕ	Censored
Three-period UI	*h1*	*h2*	*h3*+γ	Censored
Effects of shorter UI	0	γ	$\phi - \gamma$	–

Note: The four periods approximates the structure of the empirical analysis. *h1*, *h2* and *h3* denote period-specific baseline hazards, γ denotes a potential deterrence effect and ϕ denotes a post-program effect.

An important insight from this table is that the set-up allows us to identify the deterrence effects fairly well (period 2), but also that these deterrence effects will generate a wave-like pattern with negative effects in period 3 even in the absence of post-program effects (i.e. if $\phi = 0$). Thus, it is difficult to identify post-program effects if the deterrence effects are

[12]The period-sequence is chosen to approximate the results presented below.

substantial.[13]

4 Results

4.1 Main results

Given that our identification of the causal effects relies on a differences-in-differences type of identification, we first plot the evolution of the job finding hazard for the different age groups in Figure 3. The left panel shows, for each two-year age group, the log cumulative hazard up to week 75 by year of inflow. The picture shows that the trends in hazards are converging, rather than being parallel, before the reform. This time pattern, which is likely to be driven by a relationship between age and the impact of improving business cycle conditions, motivates our use of a composite control group mixing slightly older and slightly younger workers. The right hand side panel of the same figure shows a comparison between the full control group and the treatment group. Our interpretation of this panel is that the trends for the treatment and the full control group respectively were parallel before the reform and then diverge. This suggests that the identification strategy is valid, and indicate that the reform did increase the hazard to jobs in the treated group relative to the control group.

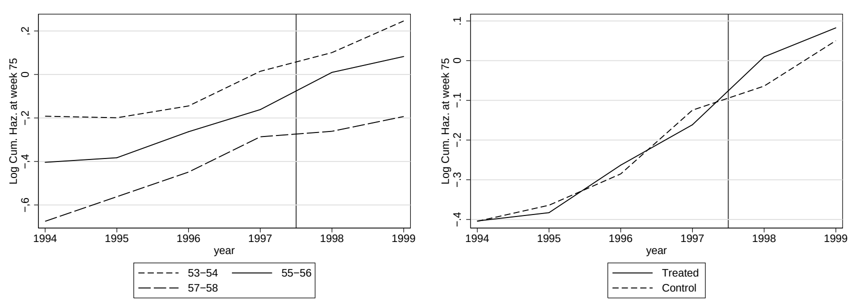

Figure 4: Log cumulative hazard at week 75, among those still unemployed at week 6.

We proceed by presenting results from the Cox proportional hazards model outlined in equation (1), where all exits except those to jobs are censored. The estimates are presented in Table 5, with and without our

[13]This adds to the dynamic selection bias which may plague estimated effects periods following impacts earlier on in the spells. The combination of the problems makes us reluctant to identify post program effects by imposing structure in the form of a constant deterrence effect over the two periods.

set of X-variables which capture seasonality, marital status, education, disabilities, region, occupation, immigrant status, unemployment history and previous wage.

Table 5: Impact of 60 rather than 90 weeks of passive benefits on job finding

Job finding interval:	Without covariates		With Covariates	
-15 weeks	0.056		0.040	
	(0.055)	-30 w	(0.055)	-30 w
16-30 weeks	0.068	0.062	0.051	0.045
	(0.058)	(0.042)	(0.058)	(0.042)
31-45 weeks	0.146**		0.116	
	(0.073)	31-75 w	(0.073)	31-75 w
46-60 weeks	0.091	0.133***	0.063	0.109**
	(0.087)	(0.051)	(0.087)	(0.051)
61-75 weeks	0.165*		0.153	
	(0.096)		(0.097)	
76-90 weeks	-0.210*	76-120 w	-0.223*	76-120 w
	(0.116)	-0.280***	(0.117)	-0.291***
91-105 weeks	-0.488***	(0.082)	-0.504***	(0.083)
	(0.145)		(0.146)	
106-120 weeks	-0.113		-0.110	
	(0.175)		(0.175)	
N (subjects)	25,146	25,146	25,146	25,146
Controls:				
Age at 60 weeks	Yes	Yes	Yes	Yes
Time of inflow	Yes	Yes	Yes	Yes
X-covariates	No	No	Yes	Yes

Note: Non-job exits are censored. Stratified on Age and Year of inflow is controlled for by dummies. X-covariates are dummies for registration month (season), female, married, education, disabled, 10 municipality groups, occupation (eight largest UI funds, blue and white collar workers), immigrant background, if unemployed 2-4 years before registration date, days unemployed previous 4 years and wage before unemployment. Standard errors clustered on individual are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The main message from the table is that the reduction of time until workfare from 90 to 60 weeks caused an increase in the hazard to jobs by about 10 percent in the period 30 to 75 weeks from registration.[14] Letting

[14]The baseline job finding rate is about 1 percent per week in this interval.

the effect vary within even finer interval gives a very similar pattern although the precision is substantially reduced as expected. The table also shows that the impact of the very rich set of covariates on the estimate of interest is relatively minor.

Our results do not suggest any significant effects during the first 30 weeks. On the other hand, we see a fairly dramatic negative effect after week 75. As explained in Section 3.3 above this estimated effect does however contain a mixture of (the negative of) the deterrence effect and potential post-program effects. Furthermore, the estimate may also be negatively biased through dynamic selection effects if the positive effect in earlier periods made the better equipped (in an unobserved sense) of the unemployed to leave for jobs. For these reasons, we do not wish to give the estimate effects for weeks after 75 any economic interpretation, but continue to report them for completeness.

A classical profile of effects for labor market programs might include three phases: pre-treatment deterrence, a non-positive locking-in-effect while participating, followed by a potentially positive effect of increased human capital after the program. Although these three effects in theory should follow in exactly this sequence, it is easy to imagine that they may sometimes overlap. Deterrence might for example act also in the beginning of the program (for those who do not appreciate the program) and human capital effects might take force before the program has come to an end. However, thinking of treatment effects in these terms, it seems straightforward to interpret our estimates as deterrence effects since they appear during the phase when program entry increased dramatically (see Figure 3 above) due to the shortening of passive UI benefits. Still, it might be illuminating to explicitly allow for different kinds of program effects in the analyses. In Appendix B we therefore perform a formal analysis where controls for program participation are included to try to separate deterrence effects from the other mechanisms through which programs might work. This analysis does not change the conclusions. The analysis also shows very large negative effects of being in a program, and small, insignificant effects of lagged participation.

4.2 Effects on different exit margins

The results presented above are all based on specifications where exits other than jobs are censored. However, it is evident that workers with a high value of leisure may choose to leave the labor force due to the workfare requirements. This process is interesting in itself, e.g. Card, Chetty, and Weber (2007) find that the increased spike at benefit exhaustion gets much smaller when looking at time to first job instead of time spent in the unemployment system. But exits from the registers may also lead us to overestimate the job finding effects if the composition in the group at risk is altered. To verify the importance of these concerns, Table 6 shows the effects on different outcomes and under varying censoring schemes. The first column replicates the job finding estimate of Table 5. In the second column we display the estimated effect on all exits, including transitions out of the labor force. This effect is larger than the effect on job finding, suggesting that workfare also generates exits from the labor force. On the one hand, this is an expected effect. Conditioning benefits on program participation should reduce the value of continued unemployment and therefore lead to higher exits out of the labor force. On the other hand, it should be noted that previous studies have found that half of the unemployed who leave the PES registers due to lost contact actually have found jobs (see e.g. Forslund, Johansson, and Lindqvist, 2004). Thus, some of these exits may in fact be misclassified job exits.

In the final column we choose a rather extreme specification where we treat all who leave the PES for other reasons than jobs as continuously unemployed. Here we still find positive effects on the job finding hazard of around eight percent.[15] In this model we only censor due to time making the results robust to deviations from the competing risk assumption required for censoring other exits in a CPH-model.

[15] Workers are treated as finding jobs if they reenter the PES and leave for jobs later (using the later date).

Table 6: Impact of 60 rather than 90 weeks of passive benefits on different outcomes

	Outcomes		
	(1) Job	(2) All exits	(3) Job, w/o cens
-30 weeks	0.045	0.024	0.093**
	(0.042)	(0.039)	(0.042)
31-75 weeks	0.109**	0.164***	0.084*
	(0.051)	(0.045)	(0.050)
76-120 weeks	-0.291***	-0.188***	-0.384***
	(0.083)	(0.068)	(0.067)
N (subjects)	25,146	25,146	25,146
Censored	Other exits and time	Time	Time
Controls:			
Age at 60 weeks	Yes	Yes	Yes
Time of inflow	Yes	Yes	Yes
X-covariates	Yes	Yes	Yes

Note: Stratified on Age, Year of inflow is controlled for by dummies. X-covariates are dummies for registration month (season), female, married, education, disabled, 10 municipality groups, occupation (eight largest UI funds, blue and white collar workers), immigrant background, if unemployed 2-4 years before registration date, days unemployed previous 4 years and wage before unemployment. Standard errors clustered on individual are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.3 Robustness and heterogeneity

In this section we discuss a number of model variations which should be interpreted as robustness checks of the baseline specification. These variations all imply that we relax some of the initial assumptions, and precision will therefore in general be slightly poorer than in the baseline specification. For this reason, and in order to conserve space, we focus on the model which only allows the effect to vary in three broad segments of analysis time. The first estimate in Table 7 is, again, the baseline specification. Specification (2) only uses data from 1997 and 1998, finding similar point estimates but with much poorer precision. Specification (3) narrows down the age span in order to address the potential concern left from the descriptive analysis where it was found that the age composition in the inflow had tilted somewhat during the sample period. Reassuringly, the estimates using a narrower age span is similar to, albeit somewhat larger than, those based on the full sample.

Next, specification (4) relies on the alternative, earnings based, indicator of job finding. Here we measure the time until monthly earnings exceed one third of the minimum wage (the 10th percentile in the overall wage distribution) for the first time since becoming unemployed. Again, we find results that are in line with, but somewhat larger than, the baseline specification. A problem with these data is that monthly earnings are distributed evenly across the months of employment within each employment spell (worker-firm pair) and calendar year.[16] We are therefore more confident in the timing derived from the PES data. Nevertheless, we find it reassuring that estimated effects of interest on time until the workers pass a monthly earnings threshold seem to agree with the pattern derived from the PES data.

In the last specification (5), we include controls for calendar time (months) which are allowed to change over the unemployment spell. The baseline specification relies on inflow year and seasonal dummies for month of inflow, but these may fail to properly account for seasonal changes in the labor market for long-term unemployed. However, results presented in specification (5) show that the results are insensitive to modelling dummies for calendar time in months.

We have also conducted a number of unreported specification tests. We have excluded subjects that are on the verge of passing the age threshold when UI expires. Borderline workers can, in principle, manipulate their spell to get a longer UI duration by making sure that they do not use all their UI days before they pass the age threshold. Excluding workers who have less than 0.2 years to their 55th or 57th birthday does however not change the results at all. We have included spells shorter than 5 weeks and the estimates change very little although precision is reduced (main estimate is significant at 10 percent level). We have estimated a model relying on age dummies instead of the age stratification while forcing the variable of interest to be identified from the reform by defining it as an interaction of age and time, again finding very similar results.

We have also re-estimated the model relying only on workers who enter week 30 without having found a job. By doing so, we make sure that we estimate the full model in the interval of the estimate of interest. We also exclude those that enter a program before week 30 and who therefore are unaffected by workfare around week 60 (remember, early program participants also re-qualify for new benefit periods). This creates a sample where the time to benefit exhaustion is binding. A drawback is that the

[16]This may explain the somewhat puzzling positive estimates in the 75+ week interval.

Table 7: Robustness checks of the effects on job finding

	Specification				
	(1) Baseline	(2) Closer in time	(3) Closer in age	(4) Using earnings data	(5) Time dummies
-30 weeks	0.045	0.083	0.080*	0.018	0.011
	(0.042)	(0.059)	(0.047)	(0.030)	(0.042)
31-75 weeks	0.109**	0.084	0.144***	0.158**	0.109**
	(0.051)	(0.070)	(0.055)	(0.062)	(0.051)
76-120 weeks	-0.291***	-0.041	-0.254***	0.259***	-0.244***
	(0.083)	(0.101)	(0.085)	(0.085)	(0.083)
N (subjects)	25,146	12,866	16,619	25,146	25,146
Censored:	Other exits and time	Other exits and time	Other exits and time	Time	Other exits and time
Controls					
Age at 60 weeks	Yes	Yes	Yes	Yes	Yes
Time of inflow	Yes	Yes	Yes	Yes	No
X-covariates	Yes	Yes	Yes	Yes	Yes

Note: Non-job exits are censored. Stratified on Age and Year of inflow is controlled for by dummies. X-covariates are dummies for registration month (season), female, married, education, disabled, 10 municipality groups, occupation (eight largest UI funds, blue and white collar workers), immigrant background, if unemployed 2-4 years before registration date, days unemployed previous 4 years and wage before unemployment. Standard errors clustered on individual are in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

analyzed sample may be affected by selection effects. The assumption for a causal interpretation is that the included covariates handle any potential selection effects induced by the sample restriction, an assumption which should be fulfilled if there is no effect on transitions during the first 30 weeks (as indicated by most of our results). The estimates are displayed in Table 8. Here we also show the estimates using the different censoring assumptions discussed in Table 6. Columns (1) and (2) focus on the effects on job finding with and without covariates, as in Table 5 above. As expected by the fact that we force identification to be driven by observations which should be more directly affected by benefit exhaustion at 60 weeks, we find substantially larger effects for this sample. Estimates in Columns (3) and (4) where we look at all exits and exits to jobs without censoring are again of approximately the same magnitude.

Table 8: Estimates for sample entering week 30 w/o previous programs

	Outcomes			
	(1)	(2)	(3)	(4)
	Job, w/o cov	Job	All exits	Job, w/o cens
31-75 weeks	0.248***	0.217***	0.214***	0.250***
	(0.066)	(0.066)	(0.057)	(0.065)
76-120 weeks	-0.263***	-0.249**	-0.171**	-0.383***
	(0.099)	(0.099)	(0.080)	(0.083)
N (subjects)	11,661	11,661	11,661	11,661
Censored	Other exits and time	Other exits and time	Time	Time
Controls:				
Age at 60 weeks	Yes	Yes	Yes	Yes
Time of inflow	Yes	Yes	Yes	Yes
X-covariates	No	Yes	Yes	Yes

Note: : Non-job exits are censored. Stratified on Age and Year of inflow is controlled for by dummies. X-covariates are dummies for registration month (season), female, married, education, disabled, 10 municipality groups, occupation (eight largest UI funds, blue and white collar workers), immigrant background, if unemployed 2-4 years before registration date, days unemployed previous 4 years and wage before unemployment. Standard errors clustered on individual are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Finally, we have estimated the model on various sub-samples. The precision is however in general too poor for the results to be informative.

However, relying on the model presented in Table 8 to gain precision, we find that men and women are equally affected and that the impact is shared equally across very low skilled (9 years or less) workers and high school educated, whereas we find a zero effect for workers with a tertiary education.

4.4 Search intensity or reservation wages?

There are two possible mechanisms which could explain the patterns we find, even if we take it as given that the positive effects, as argued, should be interpreted as deterrence effects: Either the unemployed search harder for work when being forced to participate in a program earlier, or workers lower their reservation wages for the same reason. The welfare consequences (even ignoring distributional aspects) differ between these two mechanisms if a lower reservation wage translates into lower average match quality and hence a lower average productivity. In order to shed some light on this issue we have estimated a straightforward difference-in-difference model with log of monthly earnings as the dependent variable and a dummy for 60 weeks duration until workfare as the variable of interest alongside dummies for age and time as well as all the covariates from the hazard models.

The regressions can only include those that do find jobs and it is therefore likely that the estimates will be negatively biased if those that do find jobs as a consequence of the reform are less qualified (in an unobserved sense) than those that would have found jobs even without the reform.

Figure 5 shows the estimated effects from regressions where the sample consists of those who have employment in each of the months (respectively) since inflow into unemployment (we have rescaled the figure into weeks). The point estimates suggest that monthly earnings are largely unchanged during the first 30 weeks (7 months) since the start of the spell. In the 30 to 90 weeks interval, the point estimates suggest that wages have been increased somewhat (five to ten percent) as a consequence of the reform. Clearly, however, the precision of the estimates is fairly poor, and only a few of the individual estimates are therefore significant. When estimating an overall effect pooling observations in the 30 to 75 week interval, we do find a significant effect however. In particular since it seems reasonable to assume that these estimates are negatively biased through selection, these results thus speak against falling reservation wages as an important mechanism behind the main results.

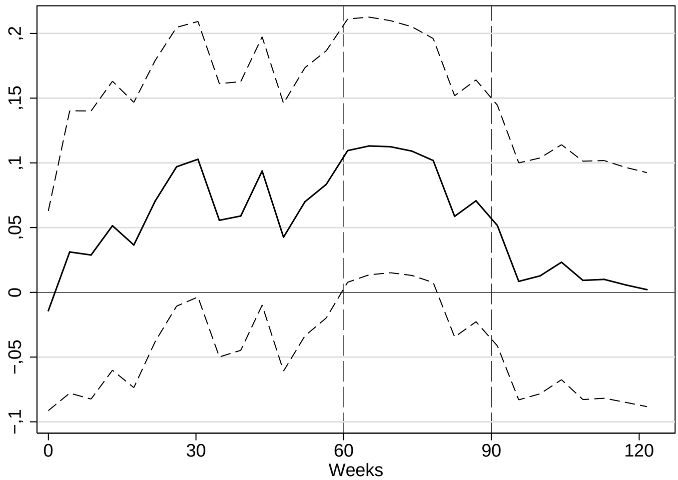

Figure 5: Estimated wage effects among those who find employment

5 Discussion

We have studied the effects of reduced UI-duration on the employment probability of older workers. We argue that UI-exhaustion in the Swedish context is irrelevant as a financial incentive since other transfers form a perfect substitute. Since the main alternative transfer is conditional on program participation, and work practice programs were offered to all with expired benefits, we interpret the effects as being driven by changes in the time until programs become a pre-requisite for benefits (workfare).

We find substantial effects. The outflow to work increased by 10 percent in the 30 to 75 week interval. Since this effect appears before and during the program-entry phase it is most likely due to the fact that mandatory programs confiscate leisure. Our analysis of wages among those that do find employment does not suggest that earlier workfare reduces reservation wages or match quality, which points to increased search intensity as the most likely driving force.

We are unable to make firm conclusions regarding the effects on job hazards *after* the programs for two reasons. The first is the fact that we study changes in the duration until workfare. This means that the counterfactual is one where workfare starts to bind 30 weeks later. The comparison later on in the spells therefore becomes one between post-program effects among those in the "early workfare group" and pre-program effects among those in the "late workfare group". The second reason is the risk of dynamic selection. The large effects we find earlier in the spells suggest that the composition of workers may have changed substantially after the 75th week.

In order to provide an account of how large the effects are, we calculated the expected number of days in unemployment with and without the treatment.[17] Results suggest that the behavioral response to the threat of treatment, on average, reduced the time until employment by 12 days among those still unemployed after 30 weeks. Allowing for other exits, raises the estimate to 14 days, thus suggesting that the main impact was through increased transitions to jobs. The reduction in unemployment corresponds to 20 days per worker who actually remain in unemployment until workfare starts, which is a more useful statistic in relation to the costs of the program. Our interpretation is that this suggest that pre-treatment deterrence effects entail a sizable benefit from mandatory workfare programs for the elderly.[18]

Interestingly, our pre-program results are of a similar magnitude (in terms of hazards), and show a similar time pattern, as in the Forslund and Skans (2006) study of mandatory programs for short-term unemployed youths in Sweden. This suggests that the pre-program effects of mandatory programs vary very little between these two groups, despite very different work histories and average job-finding rates. This suggests that the one-sided approach taken in many countries (e.g. the UK and Sweden) where early mandatory programs are focused solely on youths may be difficult to motivate, in particular since the workfare effects seems to be of a short-run nature also for the youths.

Our analysis also suggests that the fact that alternative transfers often are poorly documented in studies of how benefit durations affect job finding is a shortcoming in the existing literature. Although the Swedish example is extreme, there are many other examples of benefits that may take effect after UI expires, such as social assistance, pensions, sickness insurance, disability insurance, and student grants. Without knowing the extent of alternatives, and the conditions under which these are granted, it is very difficult to provide a clear interpretation of the effects of UI-benefit duration. Since the effects of economic incentives provide a keystone in many calibration studies of optimal unemployment insurance, it is important to filter out the exact magnitude of the economic incentives by

[17] Based on the estimated effects for the interval 30 to 75 weeks displayed in table 8 and the empirical post-treatment hazard, we compute and compare actual and counterfactual cumulated job-finding hazards.

[18] We do not provide a cost-benefit assessment of the program since we are unable to measure post-program effects, but it is noteworthy that the included programs are low-cost ones. In particular, the dominating work-practice programs only have administrative costs (employers are not compensated).

providing proper measurement of alternative transfers.

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Appendix A: Economic consequences of UI expiration

In the paper we show that program participation was indeed affected by UI-duration as expected from our institutional description. We also argue that the institutions are such that workers who remain in unemployment should receive unchanged total transfers when UI expires. Ideally, we would like to document this in the data as well. However, in order to do this, we must rely on less precise data since other transfers than UI-benefits are recorded at an annual frequency in our data.

It is difficult to map the effects of the reform into a good measure of annual transfers since the inflow is dispersed over different parts of the year, and we are therefore not able to analyze the impact of the reforms on the transfers. Instead, we try to illustrate the general picture: what happens to transfers for workers who are entitled to 60 weeks of benefits but who remain unemployed longer than that? In order to allocate the different parts of the unemployment spells into something which is meaningful to analyse at the annual frequency, we sampled all UI-entitled workers aged 45 to 54 who became unemployed in June to October 1997 and who remained unemployed until the end of 1999. These workers should have UI-benefits during most of 1998, but lose their benefits towards the end of that year, and thus be out of benefits during 1999. We put no restrictions on outcomes after the end of 1999.

Figure A1 shows how different forms of annual earnings evolve over time for this sample before (1995-96) and after (1997-) job loss and after UI-exhaustion (1999-). To recap, note that the unemployed can enter programs earlier than in the 60th week, that program participants should receive AS at par with UI during participation, and that receiving AS during six months re-qualifies for an additional UI-benefit period.

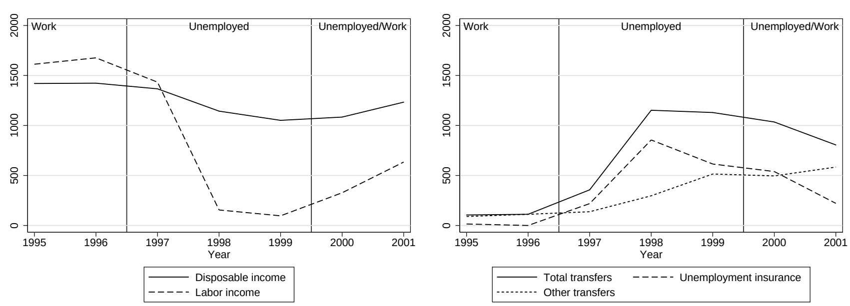

Figure A 1: Annual earnings evolvement over time for long-term unemployed

The main message from the picture is twofold. First, the level of total insurance is very high. Disposable income (which accounts for all transfers and taxes, and hence is lower than labor earnings before job loss) is on average 142,000 SEK before unemployment and never fall below 105,000 SEK, despite the fact that the sample remain unemployed during two full years. Second, disposable income is unaffected by UI-exhaustion. UI-transfers are much lower in 1999 (as expected), but other transfers (mostly AS) fully compensate for this.

Although the sample is restrictive, the message is fully in line with the institutions at the time: When UI-benefits expire, workers are offered programs and receive AS at an unchanged compensation level. Thus, we will interpret UI-duration as the expected time until program participation becomes a requirement for continued benefits.

Appendix B: Modeling program participation

Our main model has been a reduced form Cox-proportional hazard model with job-finding as the outcome. Other exits, such as out-of labor force, have been censored, while program participation has not been separated from open unemployment. The models presented in the paper therefore identify the effects of changes in the anticipation of program participation. Here we also incorporate actual program participation in the model. Participation will, by necessity, vary over spells in a non-predetermined manner.

The reason for modeling program participation is that programs may affect job-finding through other mechanisms then deterrence. The main purpose is thus to separate the different mechanisms through which programs act, in order to rule out other explanations for what we in the main paper regard as deterrence. The mechanisms in mind are locking-in effects during participation and post-program human capital effects. The first mechanism is likely to result in a decrease in job-finding while in program. There may of course be several economical reasons for locking-in effects, which we however leave aside. To us it is merely a way of excluding in-program effects from pre-program deterrence. The second mechanism is the influence of increased human capital, due to participation, on job-finding. This is likely to take effect mostly after the program has come to an end. Since increased human capital and/or improved labor market contacts are probably the very reason for running programs, one would at first expect this to unambiguously improve labor market opportunities. However, since increased human capital also may affect reservation wages, the sign of the total effect is not obvious (Calmfors, Forslund, and Hemström, 2004).

We will thus include variables for current and lagged program participation. Current participation (*CP*) in week t, is defined as participating in a program in week t-1 (since programs by definition end when jobs start). Lagged participation (*LP*) is defined as participating in a program in the time span (t-8, t-2). Formally the log hazard is given by:

$$\log h_i(t) = \log \lambda_0^{age}(t) + \alpha_1 CP_i(t) + \alpha_2 LP_i(t) + \sum_{\tau=1} \gamma_\tau D_i^{60} + Year_i \beta^y + X_i \beta^X \quad (2)$$

The estimated models are presented in Table B 1. We find very large negative effects of being in a program and insignificant effects of lagged participation. The table also shows estimates of the reform, γ_τ in three bins, analogous to what we do in the main paper. The effects of being close to benefit exhaustion, the week 31-60 coefficient, remain very close to that of the main specification, presented in Table 5. It should however be noted the interpretation differs somewhat because of the inclusion of program participation variables. These estimates should now be interpreted as the effect of being close to benefit exhaustion, while not yet participating in a program.

Table B 1: Impact of 60 rather than 90 weeks of passive benefits controlling for program participation

	(1) Without covariates	(2) With covariates
-30 weeks	0.072*	0.055
	(0.042)	(0.042)
31-60weeks	0.135***	0.110**
	(0.051)	(0.051)
61-75 weeks	-0.284***	-0.293***
	(0.082)	(0.082)
Current program	-0.173***	-0.178***
	(0.024)	(0.024)
Lagged program	0.054	0.048
	(0.040)	(0.040)
N (subjects)	25,146	25,146
Controls:		
Age at 60 weeks	Yes, strata	Yes, strata
Time of inflow	Yes	Yes
X-covariates	No	Yes
Program participation	Yes	Yes

Note: : Non-job exits are censored. All analyses are stratified on Age. Year of inflow is controlled for by dummies. X-covariates are dummies for registration month (season), female, married, education, disabled, 10 municipality groups, occupation (eight largest UI funds, blue and white collar workers), immigrant background, if unemployed 2-4 years before registration date, days unemployed previous 4 years and wage before unemployment.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Although this model relies on the assumption that selection into programs is handled by the covariates; the results support the notion that the effects of shorter benefits are driven by deterrence effects and not by human capital effects from program participation. Regarding the significantly negative effect of shorter benefits in the week 61-75 bin, the previously stated precautions still hold: it is both affected by selection in the previous period (week 31-60) and by the fact that the reference group (those entitled to 90 weeks of UI) might now be affected by deterrence, since they approaching benefit exhaustion. It should also be noted that the issue on selection into programs implies that estimated effects of current and lagged program participation should be interpreted with caution.

Another aspect of programs foreseen in the presented analyses is that programs can postpone benefit exhaustion, since UI-days are not consumed while participating in programs. Time until exhaustion would hence be measured with better precision if aggregated participation was subtracted. We have done this and estimated the effect of being close to exhaustion. In doing this, spells were measured along the ordinary time axis, from spell start, in the usual manner. This was done both with and without controls for present and previous programs, in the fashion described above. In these analyses we also censored spells when programs had lasted long enough for the jobseeker to get entitled to another 60 (or 90) weeks of UI-benefits. We think we ended up with a somewhat cleaner setup, although losing some of the experimental features of our original design. The results of these analyses were however very similar to those presented above.

Essay IV

Dynamic Effects of Mandatory Activation of Welfare Participants*

1 Introduction

There is a broad consensus that the welfare state has the responsibility of providing economic support to poor individuals. However, the form that poverty alleviation should take is a much-debated issue since receiving benefits generally conflicts with retaining work incentives. Throughout history, the poor were often required to provide some service to society to prove themselves to be "worthy" of support. It was thus common to require welfare participants to take on publicly provided low-paying jobs or move to workhouses to retain eligibility for benefits. In the last twenty years, work requirements and activation programs have again been discussed as ways of creating "the correct incentives" for recipients of social assistance[1].

In this paper we study the effect of such work requirements on the flows into and out of welfare participation in a Swedish municipality. The identifying variation that we use arises due to a sequential implementation of activation programs in different city districts of Stockholm. This reform has been shown to have a negative effect on the overall caseload, and a positive effect on employment (Dahlberg, Johansson, and Mörk, 2008). In this study we decompose these previous results into effects on entry and

*Co-authored with Anna Persson

[1]We will use the words welfare and social assistance (American and Swedish terms, respectively) as equivalents.

exit rates. The importance of performing this decomposition is established by Grogger, Haider, and Klerman (2003), who show that a reduction in welfare entry accounted for around half of the decline in US welfare caseloads during the 1990s, while increased exit rates explained the other half. Also, Grogger (2004) shows that entry and exit are not symmetrically affected by the economy and welfare reform. Thus, by not including effects on both flows in the analysis, a lot of information will be lost. However, studying the full dynamics of welfare participation requires more data than what is commonly available. Most previous literature thus focus on welfare exits, since one then only needs data on welfare participants or welfare leavers. The studies that do analyze welfare entry find ambiguous results (Klawitter, Plotnick, and Edwards, 2000; Gittleman, 2001; Acs, Phillips, and Nelson, 2005). A priori, the effect on activation requirements on welfare entry are ambiguous. As discussed by Moffitt (1996) the effect will depend on weather the activity is viewed as a burden or something that might favor future employment probabilities. Also, the program might affect welfare stigma and thus the implicit social cost of welfare participation.

There are many variations of activation programs, and participating in activation may imply very different things. In a strong version known as "workfare" the welfare recipient is required to work in a publicly provided job to retain assistance. Weaker versions may merely mandate participation in a job preparation or job search program. There are also optional activation programs in which noncompliance does not lead to sanctions. Moreover, programs differ in how much focus they put on increasing human capital by providing relevant skills relative to testing the participants willingness to work. In most theoretical work on activation requirements for welfare recipients it is assumed that the activation does not improve human capital, they only change individuals' incentives[2]. Besley and Coate (1992) show that the incentive effects of mandatory activation are twofold. In the short run, it will induce individuals to refrain from applying for welfare or to exit welfare faster because there is an implicit cost associated with welfare use. In the longer run people might make choices that reduce the risk of becoming welfare dependent in the future, for example by completing more education, when welfare becomes a less attractive alternative. Hence, mandatory activation programs affect both welfare participants and non-participants through exit and entry effects, respectively.

Our study makes valuable contributions to the existing literature in several ways. First, while welfare reform in the US often implied the implementation of a bundle of reforms with a combination of work requirements, time limits and financial incentives such as the EITC, reforms

[2]See for example Chambers (1989) and Brett (1998).

in Sweden have been restricted to activation. By looking at Swedish data we can thus more credibly isolate the effect of activation requirements from that of other interventions. Second, since we have access to data for the whole population and are not restricted to labor force or welfare participants, we are able to capture the full effect on the probability of non-participants to enter welfare. Third, the feature that all individuals permanently residing in Sweden are potentially eligible to receive welfare benefits, whereas in the US support is primarily aimed at single mothers, makes it possible to look at heterogeneous treatment effects across different demographic groups. And fourth, there is also additional advantages of looking only at the city districts of Stockholm, namely that the districts have the same political representation and, most importantly, belong to the same labor market region. It is thus possible to control for (unobserved) common macroeconomic shocks.

When studying the effect of mandatory activation on entry and exit rates, a common concern is that relocation of welfare-prone individuals might invalidate the exogenous variation[3]. This has previously been explicitly studied by Edmark (2009) for the same reform and most of the years that is used in our study. She shows that the implementation of activation requirements did not increase outmigration of welfare-prone individuals[4] and thus we conclude that migration is very unlikely to bias the results of this study[5].

In this study we find that mandatory activation has had no effect on the overall probability of entering welfare but the probability of exiting welfare increased with 0.9 percentage points. For young individuals the activation requirements had a rather large effect on entry rates into welfare, a reduction of 0.6 percentage points, and the increase in exit rate was also somewhat higher than for the average for the whole population of welfare recipients (1.4 percentage points). For one group, unmarried individuals without children, we also find larger effects on exit rates, a 2 percentage points increase. These heterogeneous effects might be explained by the fact that the programs consists of different activities depending on the needs of the participant, and that the various activities might have different effects. Also, it seems that effects are larger for groups that can be assumed to be

[3]The hypothesis that regions with generous welfare systems attract welfare participants, that is, welfare-prone individuals relocate to places where social assistance is higher, is confirmed in several recent studies; Gelbach (2004), McKinnish (2007) and Fiva (2009).

[4]If it was the case that individuals fictitiously changed address to avoid the activation, this would also be captured in this study since it uses information on where the individuals is registered to live, not self-reported information

[5]We do not find any migration due to the reform in our sample either and have run the estimations both with and without movers but the result does not change.

more mobile and have fewer family responsibilities.

The paper proceeds as follows: In section 2 we summarize the relevant literature, then we describe the Swedish institutions and the data in section 3. In section 4 and section 5 the empirical setting and the results are presented before we conclude in section 6.

2 Previous literature

There is a number of studies in which the effects of activation requirements on welfare participants are investigated (see, for example, Gueron and Pauly, 1991 and Friedlander and Burtless, 1995). There is few studies in which the effects of such changes on both welfare participants and non-participants are analyzed. Instead, most previous work has consisted of experimental studies or leavers' studies and therefore by construction has focused on exit effects and duration of welfare participation. The results reported by these studies are mixed (see, for example Blank, 2002, for an overview).

Klerman and Haider (2004) show the importance of looking at how entry and exit rates are affected by welfare programs together with economic conditions because they both determine the total caseload. However, economic factors does not seem to affect entry and exit rates symmetrically. As shown by Grogger (2004) improvements in the economy are important in reducing the entry rate, while welfare reform and the unemployment rate are more important in determining the exit rate.

Previous studies on what factors determine entry into welfare provide mixed results. Klawitter, Plotnick, and Edwards (2000) show that for young women in the US welfare entry is strongly correlated with the birth of their first child. The probability of welfare participation and the timing of entry is also associated with low education, previous poverty and poor academic achievement. Using SIPP data up to 1996, Gittleman (2001) finds that state waivers before the launching of TANF increased both entry and exit rates. On the other hand Acs, Phillips, and Nelson (2005) find that welfare reform significantly reduced entry rates. These contradictory findings might be explained by the fact that both studies have access to data on only a few post-reform years and that the effect of the reforms is thus not fully captured. There is also some concern that the results should not be given a causal interpretation since, for example, the treatment of applicants or attitudes towards welfare may have changed during the reforms, and that the reform serves as a proxy for other state-level changes.

Moffitt (2003) analyzes effects on both entry and exit rates of nonfinancial factors, where work requirements is one factor. He uses survey data from only post reform years in three American cities where single-mothers both on and off welfare were asked questions. Recipients where asked questions about work and other requirements and sanctions. To capture effects on entry rates, questions to TANF applicants about different diversion programs are used. One diversion program require the applicant to work or demonstrate job search activity prior to application. Moffitt finds that work requirements increase exit rates, but no effect is found on applicants entry rates. The diversion practices gives mixed results for effects on entry rates, possibly due to selectivity on unobservables. Since the survey only captures TANF applicants the study may not capture the whole effect since some single-mothers may choose to never apply due to the work requirements.

Moreover, the flows into and out of welfare are different for different groups and might explain differences in overall participation rates between groups. For example, Hansen and Lofstrom (2006), show that entry and exit rates explain part of the difference in welfare participation between immigrants and natives in Sweden. Most Scandinavian studies have found small or insignificant effects of activation on participation rates and costs for welfare[6]. The previously mentioned study by Dahlberg, Johansson, and Mörk (2008) finds that the activation requirements in Stockholm reduce welfare participation, especially among young people and immigrants from non-Western countries. They also find a positive effect of activation requirements on employment.

3 Institutional setting and data

3.1 Social assistance in Sweden

Sweden is divided into 290 municipalities, which are responsible for the majority of the publicly provided welfare services, such as childcare, education and elder care. The local governments have historically also been responsible for relief for the poor, whereas labor market policies have been administered by the central government. Although social assistance is largely a local responsibility, there is national legislation establishing the main principles for benefits. The legal framework is stated in the Social Services Act passed in 1982. This law ensures all Swedish citizens and foreign citizens living in Sweden financial support to maintain a "reasonable" standard of living in default of other means of support. A minimum bene-

[6]See Milton and Bergström (1998) and Giertz (2004) for Sweden, and Dahl (2003) for Norway

fit level is stated in the legal framework, but the exact level of the benefit is decided by each municipality. Social assistance is a means tested benefit, implying that all other financial resources (such as savings and valuable assets) must be exhausted before an individual is eligible for benefits. This benefit is a last resort when social insurance, such as unemployment insurance and health insurance, is not available or is insufficient. Unlike the social insurances, social assistance is not income based. However, eligibility is universal in the sense that it is not dependent on, for example, having children, as is the case in some other countries (for example, the US and the UK).

During the Swedish recession and financial crisis in the 1990s, the social assistance caseload grew, and many municipalities faced difficulties in financing the social assistance system. As shown in Figure 1, both the cost of welfare benefits and the number of households receiving welfare increased until the mid-1990s, but they have since decreased. However, the cost of benefits per household has increased substantially. In 1983, the average benefit received among those on social assistance was around 9,000 SEK (1,125 USD)[7] per year and household. In 2008, this figure was almost 44,000 SEK (5,500 USD). This implies that individuals who were on welfare in 2008 received benefits for more months during a year and/or larger amounts of benefits than was the case in 1983.

In response to the financial difficulties and increase in unemployed social assistance beneficiaries, during the crisis in Sweden, many local governments started to develop municipal activation programs to try to move social assistance recipients from welfare to self-sufficiency. In 1998, the Social Services Act was changed to explicitly allow municipalities to require welfare participants to take part in activation programs to retain their eligibility[8]. The activation programs in the Swedish municipalities consist of job-search programs and education as well as practice at job sites. In some cases, rehabilitation programs are also offered (Salonen and Ulmestig, 2004).

3.2 The city districts of Stockholm

In Stockholm, the responsibility for many municipal services is decentralized to city districts' councils. During the time period relevant to this study, there were 18 city districts within the municipality. City districts

[7]Between 1983 and 2008 the exchange rate varies between 9 USD per 100 SEK and almost 19 USD per 100 SEK. For the years we use in our analysis (1993-2005) the exchange rate varies less and the mean exchange rate is 12,5 USD per 100 SEK which we use for comparison in this paper.

[8]Some municipalities implemented activation programs prior to 1998.

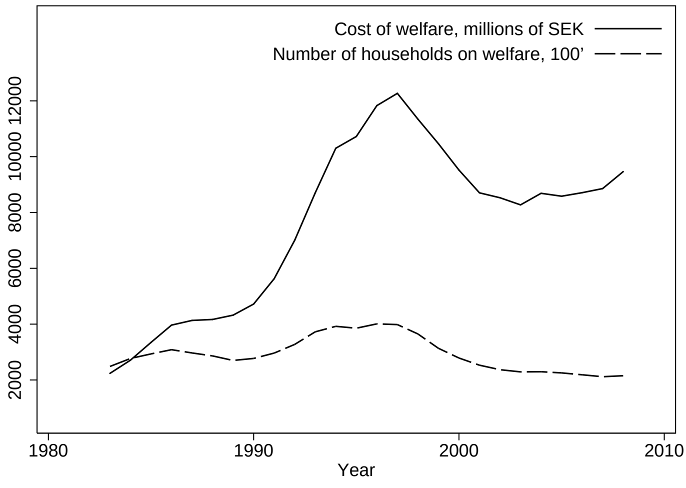

Figure 1: Cost of welfare (millions of SEK) and number of welfare households (100's) 1983-2008. Source: Statistics Sweden.

are not responsible for collecting taxes and in general follow guidelines given by the Municipal Council. There are no elections at the city district level, and hence, the political representation is equivalent at the district and municipal levels.

In Table 1, some characteristics of the city districts used in this study for 1993 are shown. The second column is mean social assistance including all individuals in the districts, that is, even those who do not receive social assistance. As can be seen, this varies between around 1,000 SEK for Bromma and 5,800 SEK for Rinkeby. However, for those actually receiving social assistance, the mean only varies between 15,400 SEK and 19,100 SEK (see fifth column). The city district that is most different from the others is Rinkeby, with the lowest mean disposable income and high shares of social assistance receivers, immigrants and low-educated individuals, highest social assistance entry rates and lowest exit rates.

For around three quarters of the social assistance recipients in Stockholm in 2005, unemployment is the reason for needing social assistance. A large fraction of these, 77 percent, do not meet the eligibility criteria for unemployment insurance; that is, they do not have labor market experience and/or are not members of an unemployment benefit fund. However, they are registered at the employment office and are looking for and willing to accept a job (USK, 2007). These are the individuals targeted by the reforms that we study.

Table 1: City district characteristics 1993

	(1) Share of welfare recipients	(2) Average welfare benefits[a]	(3) Average disposable income[b]	(4) Share born in non-Western countries	(5) Average benefits per recipient[c]	(6) Population	(7) Entry rate	(8) Exit rate	(9) Activation year
Rinkeby	0.308	5785	96052	0.463	18771	5737	0.115	0.229	1998
Skärholmen	0.111	1713	124328	0.124	15387	15124	0.048	0.319	1999
Farsta	0.115	2181	128714	0.048	18918	21758	0.047	0.302	2001
Kista	0.171	3189	126035	0.226	18602	14439	0.073	0.279	2001
Älvsjö	0.067	885	145118	0.032	13175	10184	0.033	0.340	2002
Hägersten	0.072	1380	134266	0.032	19080	14437	0.032	0.349	2003
Liljeholmen	0.095	1744	126067	0.039	18303	14815	0.042	0.325	2003
Spånga-Tensta	0.149	2555	131017	0.214	17131	15795	0.058	0.289	2003
Bromma	0.058	998	154035	0.025	17217	28318	0.026	0.352	2004
Enskede-Årsta	0.075	1318	133375	0.043	17686	21682	0.030	0.363	2004
Hässelby-Vällingby	0.071	1140	141590	0.048	16043	30094	0.032	0.342	2004
Vantör	0.122	2219	124368	0.067	18152	16943	0.048	0.298	2004
Total	0.102	1798	133960	0.085	17594	209326	0.042	0.310	2003

[a] Average welfare benefits in city district including entire population.
[b] For the year 1995, since only available for the years 1995-2005.
[c] Average welfare benefits among welfare recipients.

Dahlberg, Johansson, and Mörk (2008), using results from questionnaires and interviews conducted by Karin Edmark and Kajsa Hanspers, determine when activation requirements were implemented in the different city districts. For an activation program to be classified as mandatory, the activity must be directed to all unemployed welfare participants, require the individuals to attend the activity center daily or almost daily every week and welfare benefits are strictly connected to programme participation. It was possible to determine a starting year for 12 of the 18 city districts. In the five most centrally located districts and Skarpnäck, it was not possible to determine when activation programs were implemented. For the central districts, this is mainly due to the fact that there are very few welfare participants in this area[9] . A shortcoming of the information on the implementation year is that we do not know when during the year the activation program was implemented. According to the classification, the first city districts to implement activation requirements were Rinkeby (in 1998) and Skärholmen (in 1999). Eventually, other city districts followed, and by the end of the studied time period, all districts where classification was possible had implemented mandatory activation. The last column of Table 1 shows the launching year for activation requirements in each city district. It is important to note that when applying for social assistance the individual must contact the office of the district in which he or she lives (or is registered), it is not possible to choose which district to apply within and thereby avoid the programs or take part of activities in other districts.

Since we do not know why the different city districts implemented the programs at different times there is a possibility that the adoption is somewhat endogenous. Looking at the observable characteristics it seems that the first districts to implement the reform had among the highest shares of welfare participants. However, this pattern is not clear cut since both Spånga-Tensta and Vantör, both with very high participation rates, were among the last to implement the programs. To formally examine if there is some endogenous factors driving the implementation, we perform placebo estimations on data for the time period before the programs started, see section 5.3.

The activation programs created new so-called Jobcentres that social assistance recipients are required to attend for at least a few hours each week, which varied between 4 and 15 hours in the city districts (Edmark, 2009). Previously, welfare recipients were only in contact with the local social worker, and there were no mandatory programs for all social assis-

[9]We also study the descriptive statistics in Table 1 for the districts in the non-response group and we find that, as expected, the central districts have low participation rates while Skarpnck is close to the average

tance recipients. Unemployed recipients were directed to the unemployment office, but there were no sanctions if they did not participate in any activities. The activation program in Skärholmen is the most renowned program, usually referred to as "the Skärholmen model". It started as a measure to reduce welfare participation among students who were unemployed during the summer. In 1999, the program was widened to include all unemployed welfare participants. The main feature of the program is that unemployed welfare applicants are sent to the Jobcentre. In order to retain eligibility for welfare, the applicant must visit the Jobcentre for three hours every day, following a rotating schedule to prevent black market work, until he or she finds a job. The required activity consists mostly of individual job searching. The Jobcentre provides computers with internet access and assistance from staff when necessary. As noted by Thorén (2005), the resources are often limited; for example, clients can rarely use the computers for more than 15 minutes each day. There is daily registration of participants' attendance, and because there is close cooperation between social workers and Jobcentre staff, absence is easily detected and can (and often does) lead to a reduction in benefits. This possibility of imposing sanctions is common to programs in all city districts. Activation starts when the individual apply for benefits, that is when an unemployed individual applies for social assistance he or she is sent to the Jobcentre immediately. The main goal of the activation programs is to improve individuals' chances of becoming self-supportive. However, Thorén (2005) concludes that many of the activities primarily aim at testing the client's willingness to work.

The information about the starting year of activation programs is combined with individual-level register data from the Louise database administered by Statistics Sweden. This database includes information on various individual characteristics such as age, country of birth, number of children, education, etcetera for all individuals aged 16-64 living in Sweden[10]. This means that we have data for the whole population, regardless of labor market attachment and welfare participation. The data also contains the share of the household's social assistance[11] that the individual has received during the past year as well as benefits collected from other parts of the social security system. Social assistance is directed at households rather than individuals, and we define an individual as a welfare participant if he or she is living in a household that received social assistance sometime during a given year. This is a very rough but commonly

[10]Individuals aged 16 and 17 are excluded from our sample.

[11]The individual's share of the households benefits is calculated using an equivalence scale determined by the National Bord of Health and Welfare (Socialstyrelsen)

used classification. What we refer to as social assistance is thus the individual's share of the household's total received benefit. All unemployed individuals living in a household receiving social assistance are directed to the jobcenter to fulfil activation requirements. Since all newly arrived immigrants are eligible for social assistance during their first 18 months in Sweden (introduktionsbidrag) under different eligibility criteria than other welfare participants, these individuals are excluded for three years to avoid capturing their dynamics due to this sort of support. Table 2 shows descriptive statistics for the population. The mean amount of welfare benefit received by an individual is slightly above 2,000 SEK (250 USD) per year. However, it should be noted that all zeros are included here and that the mean amount of benefits among those who actually receive any benefits at all is around 23,600 SEK (2,950 USD) per year.

Table 2: Summary statistics

	Mean	Std. dev
Social assistance (100' SEK)	20.667	99.936
Share with social assistance	0.087	0.283
Income (100' SEK)*	1,663.295	2,680.451
Age	40.525	12.151
Age<26	0.125	0.330
Female	0.499	0.500
Immigrant	0.223	0.416
Native	0.702	0.458
Born in Western country	0.098	0.298
Born in non-Western country	0.125	0.331
No of children	0.657	0.995
Parent	0.372	0.483
Single parent	0.063	0.244
Compulsory schooling or less	0.195	0.396
Post secondary schooling	0.350	0.477
N	2,986175	

*The income variable is only available for individuals from the year 1995.

We define entry into welfare as being on welfare in year *t* but not in year *t-1*. The share of welfare entrants is the fraction of the whole population not receiving welfare the previous year that enters into welfare in a given year. If possible, it would be preferred (and more precise) to define the share of entrants as the fraction entering relative to the population *at risk of entering*. However, it is difficult to assess this population because eligibility for social assistance is not based on income (or other variables that we can observe) alone but also on financial assets and various household characteristics. We will, however, make an attempt to do this; see section 3.3.

Welfare exit is defined as receiving welfare support in year *t-1* but not in year *t*. In this case, the studied population is more easily defined and consists of all individuals receiving welfare in year *t-1*. An individual is exposed to treatment if he or she is living in a city district where mandatory activation has been implemented.

It is important to note that both the entry and the exit populations may change over time due to the reform. Individuals closest to the labor market may never enter the population of social assistance recipients or leave it faster due to the introduction of mandatory work requirements. This may call the assumption for difference-in-differences into question (see section 4). What can be done is to see if there are different effects of the reform in the year in which activation was implemented compared to the following year. It can be expected that the exit effects decrease over time because the individual closest to the labor market never enters, and therefore, the remaining population of individuals on social assistance have a harder time finding other means of support. The effect on entry rates from changes in population are probably harder to notice. Those leaving welfare due to the reform have higher probability of re-entering, which may increase entry rates. At the same time it may take some time before those at risk of entering welfare become aware of the program which also delays the expected decrease on entry rates.

Figure 2 presents the average entry and exit rates by year for the studied population together with the unemployment rate in the municipality of Stockholm. We can see that entry and exit rates follow the unemployment rate, with high entry rates and low exit rates during the first half of the time period. Entry rates decreased and exit rates increased with the economic recovery until 2003. This is in line with the development of the welfare caseload as shown in figure 1.

A strength of our econometric analysis is that individuals in our data are part of the same labor market region and therefore meet the same economic conditions, but live in areas where mandatory activation was implemented at different times. Including time dummies will therefore hopefully capture the common economic conditions in Stockholm.

3.3 Social assistance in different groups

It is clear that the probability of becoming dependent on social assistance is not uniformly distributed over different demographic groups and across the income distribution. Among the more welfare-prone groups are young

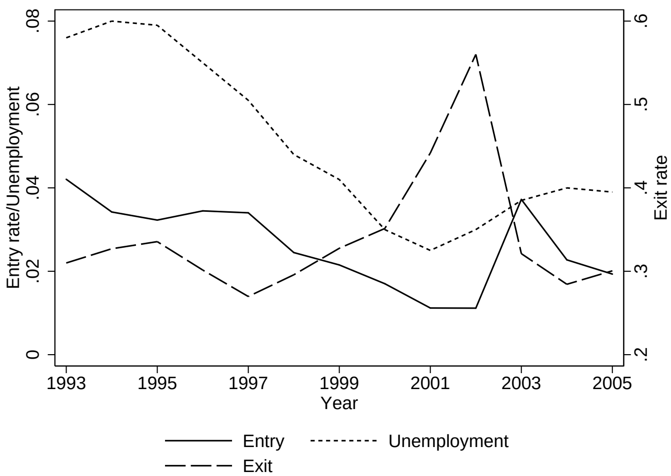

Figure 2: Unemployment rate, raw entry and exit rates, by year in Stockholm

individuals, immigrants born in non-Western countries, single parents and people with few years of education. Because these groups have a higher probability of receiving benefits than others, we attempt to create a better-defined entry sample by estimating effects on entry rates using only a subpopulation consisting of individuals with any of these characteristics. Thus, we reduce the problem of estimating an effect for individuals that have close to zero probability of ever participating in welfare (for example, individuals with high education and income are unlikely to change their behaviour in response to a reform that will probably never affect them). We prefer to define the population at risk of entering into welfare using demographic characteristics rather than income. It is likely that individuals with low income are more likely to receive welfare benefits than others. However, Meyer (2000) argues that restricting the sample to include only low-income individuals might create bias because poverty is likely to be higher in an area with low benefit levels and vice versa, which might affect welfare participation as well as entry and exit.

We are also interested in how activation requirements affect more specific subgroups in the population. As shown by Dahlberg, Johansson, and Mörk (2008), the activation programs that we study have a larger caseload effect for young individuals and immigrants born in non-Western countries. Thus, we look at the entry and exit effects for these groups separately. Young individuals are likely to be more mobile than others, and we therefore expect them to experience larger effects of activation requirements. Young people may also have more opportunities to begin an educational program or receive financial help from their families. Another interesting group is singles individuals without children, who are also very mobile (Fiva, 2009). This is a group with low probability of receiving social assistance but since it is a large group a large fraction of those receiving social assistance comes from this group. Table 3 shows entry and exit rates for different subpopulations in our sample, averaged over the whole time period. This shows that young individuals have both higher entry rates and higher exit rates, which indicates mobility. Immigrants, especially those born in a non-Western country, have high entry rates and low exit rates. The high entry rates are in line with Hansen and Lofstrom (2006). The same pattern observed for immigrants can be observed among single parents.

Table 3: Raw entry and exit rates, by different populations

	Entry	Exit
All	0.026	0.335
Women	0.025	0.337
Men	0.026	0.334
Age<26	0.051	0.351
Immigrant	0.050	0.288
Born in non-western country	0.070	0.275
Single parent	0.065	0.283
Single without children	0.028	0.352

4 Empirical strategy

To determine the treatment effect on the treated (TT) when mandatory activation is introduced, we use a difference-in-differences (DD) approach in a linear probability model (LPM). When estimating the effects on entry and exit rates, there will be different events of interest. In the entry case, the population used consists of those individuals who did not receive any social assistance at *t-1*, and the event of interest will be if they then receive social assistance at t. Let $W_{it} = 1$ indicate that the individual received welfare at time t; then, the probability of entry is given by $P(W_{it} = 1|W_{it-1} = 0)$. When we estimate the effect on exit rates, the population is comprised of those individuals receiving social assistance at *t-1*, and the event of interest is if they do not receive social assistance at t, $P(W_{it} = 0|W_{it-1} = 1)$.

Let $Y_{Dti} = 1$ if the event of interest occurs with treatment D at time t for individual i. If there is mandatory activation, $D = 1$. Also let *t-1* be before activation is implemented in the treatment district and t be after. Then, the identifying assumption for the DD estimator to recover the TT is

$$E[Y_{0ti} - Y_{0t-1i}|X_i, D_i = 1] = E[Y_{0ti} - Y_{0t-1i}|X_i, D_i = 0] \quad (1)$$

That is, we assume that the treatment group would have developed similarly to the control group if no treatment had occurred. Thus, implementation of activation requirements cannot be related to (unobserved) city district-specific conditions. As mentioned earlier, this assumption can be questionable because the composition of the samples is affected by the reform if individuals leave welfare and fewer individuals enter welfare due to the reform.

In the difference-in-differences approach in the LPM, we include city districts and year dummies. By doing this rather than only including dummies for treatment and control groups, we are able to control for time-constant unobserved city district-specific effects and systematic changes over time that are common for all city districts. If an individual lives in city district j, where there are mandatory work requirements at time t, the treatment variable $D_{jt} = 1$; otherwise, $Djt = 0$. If the probability for the event of interest (entry or exit) to occur is given by $p(entry/exit) = Y_{ijt}$, then

$$Y_{ijt} = \alpha_{\mathbf{j}} + \tau_{\mathbf{t}} + \beta D_{jt} + \gamma_{\mathbf{t}} \mathbf{X_{ijt}} + trend_j + \eta_{ijt} \quad (2)$$

where α_j and τ_t are city district and year dummies, respectively. β measures the effect of mandatory activation on the probability of entry and exit. To control for individual heterogeneity that varies over time, $\mathbf{X_{ijt}}$ is included[12]. All individual covariates are time-interacted (giving γ_t) to allow these individual characteristics to influence the probabilities differently over the business cycle. $trend_j$ are linear city district-specific time trends, and η_{ijt} is an error term.

Because there may be different effects of the reform between the year in which mandatory activation was introduced and the following year, we will also see if the effects differ at t (when mandatory activation is introduced), $t + 1$ and $\geq t + 2$ (see section 5.4).

[12]The individual characteristics we include in the model are age, age squared, dummy variables for female, parent, single parent, born in a Western country except Sweden, born in a non-Western country, low educated (compulsory schooling or less) and high educated (at least some post-secondary schooling).

4.1 Standard error corrections

A problem with difference-in-differences when the treatment is at the group level instead of at the individual level is that if observations are not independent within groups and we are not able to control for common group/time errors the estimated standard errors are biased downward[13]. This issue is normally solved by clustering standard errors on the level of randomization. In this case, however, clustering standard errors is likely to introduce another source of bias due to too few groups. Instead, a consistent estimator can be found using the two stage procedure proposed by Donald and Lang (2007). They argue that it is possible to correct for group and time specific shocks by estimating group averages, while still using the information in the microcovariates. However, if there is no correlation in standard errors within clusters, this approach reduces the amount of available information more than necessary. We will therefore use a method proposed by Wooldridge (2003) to test for if these correlations exists. He proposes a two-stage procedure where an efficient minimum distance (MD) estimator is obtained in the first step by estimating

$$Y_{ijt} = q_{jt} + \gamma_{\mathbf{t}}\mathbf{X_{ijt}} + \varepsilon_{ijt} \tag{3}$$

where the predicted city district and time specific effects, $\hat{q}_{jt}$, and their estimated standard errors, $\hat{\sigma}_{jt}$, are saved. The predicted $\hat{q}_{jt}$ are then used to estimate the following equation

$$\hat{q}_{jt} = \alpha_{\mathbf{j}} + \tau_{\mathbf{t}} + \beta D_{jt} + trend_j + \mu_{jt} \tag{4}$$

with weighted least squares where the weights are given by $1/\hat{\sigma}_{jt}$. Under the null of no unobserved city district specific shocks we have that (in the second stage estimation) $SSR \overset{a}{\sim} \chi^2(S-K)$ where S is equal to $J \times T$ and K is equal to the number of parameters estimated in equation 4. If the null hypothesis is rejected city district specific shocks exists and we will use the Donald and Lang (2007) procedure (hereafter D-L procedure). In practise, this is equivalent to estimating 3 and 4 but using the group size, that is the share of the total sample population living in each specific city district every year, as weights in equation 4 instead of the variance of $\hat{q}_{jt}$ from equation (3). This between estimator gives the correct standard errors and t-statistics, and thus provides a valid inference. We will show the test statistic from the Wooldrige test together with p-values in all our result tables and present the standard errors from the D-L procedure if the test

[13]Other problems that often arise in difference-in-difference models are discussed in Bertrand, Duflo, and Mullainathan (2004).

statistic is rejected at the five percent level.

5 Results

In the following, we present the results of our estimations. We start by estimating caseload effects for our sample before we evaluate the effects on entry and exit for the whole population. In section 5.3, we conduct some sensitivity analyses by performing a placebo test, and in section 5.4, we determine whether the treatment effects vary over time. Finally, we study if there are heterogeneous effects for different groups in section 5.5.

5.1 Effects on caseloads

According to Dahlberg, Johansson, and Mörk (2008), the caseload (share of welfare recipients) was reduced by 0.5 percentage points in Stockholm due to mandatory activation requirements. However, their study uses a different sample as they do not include Rinkeby and use data only up to the year 2003. Therefore, for comparison of our main entry and exit results, we run estimations of caseloads with our complete sample and for different subpopulations using equation (2). The caseload results are shown in table 4, where we include both ordinary standard errors and the standard errors from the D-L procedure if the Wooldrige test is rejected. The test statistics from the Wooldridge tests are also shown in the table together with number of degrees of freedom and p-values.

In our estimation, we find a smaller reduction in welfare participation due to the reform, 0.3 percent, than Dahlberg, Johansson, and Mörk (2008) found, but the result is insignificant when we use the D-L procedure. There are, however, heterogeneous effects, and the effect is much larger for both young individuals and unmarried individuals without children (1.2 and 0.6 percent, respectively).

Surprisingly, we find a significant increase in caseload due to the reform for immigrants from non-Western countries, whereas Dahlberg, Johansson, and Mörk (2008) found large negative effects. There are four differences between our sample and theirs. We include Rinkeby, have two additional years of data and define immigrants from non-Western countries in a slightly different way - they do not include immigrants from Eastern Europe as we do. Furthermore, in our sample, immigrants are not included during their first three years in Sweden, compared to two in Dahlberg et al.'s study, because we do not want to capture any dynamics due to the social assistance newly arrived immigrants receive. If we exclude Rinkeby, we get a negative point estimate (-0.002), but it is far from

Table 4: Estimation results: Caseload

	(1) All	(2) Age < 26	(3) Born in non-Western country	(4) Unmarried w/o children
Mandatory activation implemented	-0.003	-0.012	0.006	-0.006
	(0.001)***	(0.002)***	(0.003)**	(0.001)***
	[0.003]			
Time varying controls	Yes	Yes	Yes	Yes
Linear trend	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
City district dummies	Yes	Yes	Yes	Yes
Wooldridge-test; SSR	194.871	0.252	20.726	42.881
(df)[p-value]	(107) [0.000]	(107) [1.000]	(107) [1.000]	(107) [1.000]
N	2,986,175	372,325	372,917	1,395,995

Standard errors in parentheses
D-L standard errors in square brackets
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

significantly different from zero.

The null hypotheses of the Wooldrige test is only rejected when we use the full sample. It is not surprising that we do not find city district specific shocks for the subsamples even though we find it when we include all individuals. Shocks affecting the city districts differently are due to the fact that different groups of individuals are affected differently. Since the city districts differ in their composition of individuals, there may be shocks when we include all individuals but when the sample is reduced and we only use for example young individuals, the groups are similar and there are no differences between the city districts anymore.

5.2 Baseline estimation

Table 5 show the results for the estimates of the probability of entry and exit.

The estimates for the effect on entry shows a reduction by 0.1 percentage points. In the Wooldrige test we reject the null of no city district specific shocks and therefore also report the standard errors from the D-L procedure where the result become insignificant. We conclude that we are not able to identify any effects on the entry rates for the whole population when mandatory activation is implemented. The reform may, however, still have had an effect at different times after implementation and for dif-

Table 5: Estimation results: Entry and Exit

	Entry	Exit
Mandatory activation implemented	-0.001	0.009
	(0.000)**	(0.004)**
	[0.002]	
Time-interacted controls	Yes	Yes
Linear trend	Yes	Yes
Year dummies	Yes	Yes
City district dummies	Yes	Yes
Wooldridge-test; SSR	134.4	30.6
(df)[p-value]	(107) [0.037]	(107)[1.000]
N	2,698,222	287,953

Standard errors in parentheses
D-L standard errors in square brackets
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

ferent subpopulations, especially for populations at greater risk of entering welfare (see section 5.4 and section 5.5).

The point estimates for the exit rates is 0.9 percentage points which should be compared to exit rates of 33.5 percent on average (see Table 3) - which implies that the number of exits on average increases by 200 individuals each year as a result of the reform.

5.3 Placebo estimations

In order to verify that the estimates above captures true reform effects, and does not arise due to endogenous factors, we perform a placebo experiment using data from 1993 to 2000. For the years 1998, 1999 and 2000, we exclude Rinkeby, and for 1999 and 2000, we also exclude Skärholmen. Thus, we only use data from before the reform was implemented in any of the city districts. We move the launching year of the actual reform five years back in time[14]. If the estimation of this "pseudo"-reform were to yield significant results, it would indicate the possibility that the estimates above do not represent an effect of the reform but rather of some city district-specific characteristic. The results from these estimations are shown in Table 6.

In the placebo estimation for entry, the results are significantly different from zero. The estimates are positive, however, so if city district characteristics are driving the results in some way, they seem to reduce rather than

[14]We also move the launching year four and three years back in time but this does not change the results.

Table 6: Results from placebo estimations

	Entry	Exit
Mandatory activation implemented	0.001	0.003
	(0.001)**	(0.004]
	[0.001]	
Time-interacted controls	Yes	Yes
Linear trend	Yes	Yes
Year dummies	Yes	Yes
City district dummies	Yes	Yes
Wooldridge-test; SSR	14.453	7.2
(df)[p-value]	(48) [0.998]	(48)[1.000]
N	1,530,957	188,904

Standard errors in parentheses
D-L standard errors in square brackets
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

inflate the estimates in our baseline specification.

In the estimation of how the "pseudo"-reform affected exit, the result is not significantly different from zero, which strengthens the argument that the result from the baseline estimation is a true effect of the implementation of mandatory activation.

5.4 Time-changing treatment effects

Even if we are not able to assess any effect on the overall entry rates following to the reform, there may be effects that vary over time. To see if this is the case, both for entry and exit rates, we change the specification given by equation 2 slightly and estimate separate treatment effects for the year of implementation, the first year after implementation and two or more years after implementation. The results are given in Table 7.

In the entry estimation, the effects are still insignificant with the standard errors from the D-L estimation. The exit estimations do not show a clear pattern of effects over the time periods. If anything, the effect seems to increase over time. An explanation to the lag may be that it took some time for the programs to become effective and then activation really helped people to find work.

Table 7: Results from estimations with time-specific treatment

	Entry	Exit
Year of implementation	-0.000	0.010
	(0.001)	(0.004)**
	[0.002]	
One year after	-0.002	0.009
	(0.001)***	(0.005)*
	[0.002]	
Two years after or more	-0.000	0.019
	(0.001)	(0.007)***
	[0.002]	
Time-interacted controls	Yes	Yes
Linear trend	Yes	Yes
Year dummies	Yes	Yes
City district dummies	Yes	Yes
Wooldridge-test; SSR	131.018	29.291
(df)[p-value]	(105) [0.044]	(105) [1.000]
N	2,698,222	287,953

Standard errors in parentheses
D-L standard errors in square brackets
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5.5 Heterogeneous effects

Population at risk

As mentioned in section 3.3, certain groups of individuals[15] are more likely to be on welfare. Therefore, we estimated the effect of mandatory activation on entry rates separately for this population. We have thus excluded many individuals who are never at risk of entering welfare. The results are shown in Table 8. Even for this group, no effect of mandatory activation is found on entry rates.

Effects on subpopulations

To study whether activation requirements affect subgroups of the population differently, we performed separate estimations for some of these groups. Since Dahlberg, Johansson, and Mörk (2008) find large effects of mandatory activation on young individuals and individuals born in a non-

[15]These groups are young individuals, immigrants born in non-western countries, single parents and individuals with low education.

Table 8: Results for population at risk: Entry

	(1)
Mandatory activation implemented	-0.001
	(0.001)
	[0.003]
Time-interacted controls	Yes
Linear trend	Yes
Year dummies	Yes
City district dummies	Yes
Wooldridge-test; SSR	44.935
(df)[p-value]	(96) [1.000]
N	877,762

Standard errors in parentheses
D-L standard errors in square brackets
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Western country, we begin by estimating entry and exit effects for these groups.

Results for individuals under the age of 26 are presented in Table 9. The effect on the probability of entry is reduced by 0.6 percentage points. This is a rather large effect as the mean entry rate for this group during the studied period was about 5 percent (see Table 3). For young individuals, the estimate for the exit effect is a little bit higher than for the whole population on average, 1.4 percentage points. A possible interpretation is that when facing activation requirement, ordinary education might become a relatively more attractive alternative and since the possibilities of starting an education is larger for younger individuals this would translate into a larger reduction in entry rates for this group. Also, young individuals might be more likely to move back to live with their parents to avoid the activation programs.

The results for immigrants born in a non-Western country are presented in Table 10. Since the caseload effect for this group in our sample is positive we would expect positive entry effect and or negative exit effect. We find a positive entry effect but this is not significantly different from zero. Since the activation that immigrants participate in is likely to consist mainly of language training, and thus differ from that offered to other welfare participants, it might not be surprising that the results are not the ones that we would normally expect.

We also present results from separate estimations for unmarried individuals without children as this group could be expected to be relatively

Table 9: Estimation results:Age< 26

	Entry	Exit
Mandatory activation implemented	-0.006	0.014
	(0.002)***	(0.009)*
Time-interacted controls	Yes	Yes
Linear trend	Yes	Yes
Year dummies	Yes	Yes
City district dummies	Yes	Yes
Wooldridge-test; SSR	0.211	0.096
(df)[p-value]	(107) [1.000]	(107) [1.000]
N	312,850	59,475

Standard errors in parentheses
D-L standard errors in square brackets
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Estimation results:Immigrants born in non-Western country

	Entry	Exit
Mandatory activation implemented	0.001	0.000
	(0.002)	(0.005)
Time-interacted controls	Yes	Yes
Linear trend	Yes	Yes
Year dummies	Yes	Yes
City district dummies	Yes	Yes
Wooldridge-test; SSR	14.361	17.290
(df)[p-value]	(107) [1.000]	(107) [1.000]
N	260,084	112,833

Standard errors in parentheses
D-L standard errors in square brackets
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

mobile and is commonly not eligible for welfare in other countries. As seen in Table 11, mandatory activation policies do not affect the entry rate for this group but lead to a significant increase in exit rate (2 percentage points, compared to an average exit rate of 35 percent for this group). An explanation to this may be that an individual in this group might have lower barriers to employment, since he or she does not have to take the situation of a partner or child into account when accepting a job offer.

Table 11: Estimation results: Unmarried without children

	Entry	Exit
Manadtory activation implemented	-0.001	0.020
	(0.001)	(0.006)***
Time-interacted controls	Yes	Yes
Linear trend	Yes	Yes
Year dummies	Yes	Yes
City district dummies	Yes	Yes
Wooldridge-test; SSR	45.775	16.134
(df)[p-value]	(107) [1.000]	(107) [1.000]
N	1,249,097	146,898

Standard errors in parentheses
D-L standard errors in square brackets
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

6 Conclusions

In this paper, we have examined the dynamic effects of introducing mandatory activation of welfare recipients. Earlier literature has found that welfare participation decreases when mandatory activation is implemented, but in most cases, the researchers have only included those individuals who already are welfare participants and therefore have only captured exit effects. In studies where the effect on the total population has been analyzed, the dynamics are still unclear as the entry and exit effects are not considered separately.

According to the theory, activation requirements will have effects both in the short run, when those who can support themselves by other means will leave welfare, and in the long run, when people will make decisions earlier in life to decrease their probability of ending up on welfare later. In our study, we are not able to distinguish between the short and the long run, but due to the relatively short time period being studied, the effects that we capture are mostly short-run effects.

To analyze the dynamics when mandatory activation is implemented, we use register data on the whole population in the municipality of Stockholm between 1993 and 2005. The municipality of Stockholm is divided into city districts where mandatory activation was implemented at different times between 1998 and 2004. We use this heterogeneity to evaluate the effects of activation requirements on entry and exit rates in a difference-in-differences model.

Our results indicate that entry rates decrease as a result of mandatory activation, but these results are not robust when allowing the standard errors to be correlated within the city districts. The effects on exit rates are positive, indicating that the reform increases the likelihood that current welfare participants will find employment or leave social assistance for some other reason. The effects are rather small, and corresponds to an increase in the number of exits of about 2.7 percent.

We also examine if the treatment effect varies over time, that is, if the impact of the program becomes stronger with time after it was implemented. We find some indications that the effect on exits from welfare increase over time, possibly because it takes some time for the programs to be fully implemented.

To see if the treatment effect is heterogeneous across the population we also perform the analysis separately for subgroups of the population, and we find effects for two groups. For young individuals the entry rates were significantly when mandatory activation was introduced. The probability of entering welfare decreased by 0.6 percentage points for this groups which corresponds to a reduction of 11.7 percent. For unmarried individuals without children we find positive effects on the exit rates. Since the content of the activation programs differ to suit the needs of each participant, it is not surprising that the impact varies between groups. Young individuals and singles without children are probably closer to the labor market than are for example refugee immigrants and single parents. Thus, they are more likely to find an alternative to welfare participation and it might also be easier to construct activities within the program that can help them find employment.

Even if we have data over the whole population covering a long period of years, the data is at very low frequency. We do not know if recipients receive social assistance for few or many months during a year and thereby can't say any thing about the short term dynamics. Mandatory activation requirements may have had an effect that we are not able to capture with our annual data. Individuals may have found temporary work and thereby reduced time dependent on social assistance. For future research higher frequency data would increase our understanding of the effects.

The data is also problematic with respect to the small number of treated groups. The data is at the individual level while treatment only varies over 12 groups. Thus, if observations are not independent within groups we need to take into account the problem of potential correlation among the standard errors. We do this by applying the methods proposed by Wooldridge (2003) and Donald and Lang (2007). However, we are not able to account for possible serial correlation discussed by Bertrand, Duflo, and Mullainathan (2004).

The main conclusion to be drawn from this study is that mandatory activation programs seem to have a rather small effect on the probability that an individual leaves and enters welfare participation. However, there are important differences between groups of individuals. Most importantly, young individuals and single individuals with no children are affected more than other groups. This is probably due to the fact that these groups are more mobile and are more likely to be able to accept a job offer on short notice. Young individuals, who become less likely to start collecting benefits when participation in the program becomes mandatory, are more likely to start pursuing higher education and thus qualify for study grants. For future research it would be interesting to see if it is the case that the activation programs led to more individuals starting higher education rather than relying on welfare. It is also not surprising that individuals with fewer family responsibilities are more responsive to the incentives that the programs create. This is especially true if leaving welfare requires taking short term jobs and if childcare is not easily available. When interpreting these results, it is important to consider that the design of the activation programs probably has a large impact on their effectiveness. For example, activation aimed at young individuals is different from that aimed at immigrants with poor language skills. The programs are thus very likely to affect different groups differently, both in terms of how effective the programs are in providing relevant skills and in what incentives they create.

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