IFAU
Institute for Evaluation of Labour Market and Education Policy

Activation programs, benefit take-up, and labor market attachment

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DISSERTATION SERIES 2013:3

Presented at the Department of Economics, Uppsala University

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This doctoral dissertation was defended for the degree of Doctor in Philosophy at the Department of Economics, Uppsala University, October 15, 2013. Essay 1 is a revised version of IFAU Working paper 2010:6 and Essay 3 is a revised version of IFAU Working paper 2011:17.

ISSN 1651-4149

Doctoral dissertation presented to the Faculty of Social Sciences 2013

Abstract

Dissertation at Uppsala University to be publicly examined in Hörsal 2, Ekonomikum, Tuesday October 15, 2013 at 10:15 for the Degree of Doctor of Philosophy. The examination will be conducted in English. PERSSON, Anna. 2013. Activation Programs, Benefit Take-up, and Labor Market Attachment. Department of Economics. Economic Studies 138. 143 pp. Uppsala. ISBN 978-91-85519-45-3. ISSN 0283-7668. urn:nbn:se:uu:diva-205490 (http://urn.kb.se/resolve?urn=urn:nbn:se:uu:diva-205490)

This thesis consists of four self-contained essays.

Essay 1 (with Ulrika Vikman): Previous literature show that activation requirements for welfare participants reduce welfare participation, but the dynamics behind these results have not been fully examined. In this paper we use a rich set of register data covering the entire working age 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 number of welfare participants is mainly due to an increase in welfare exits. The effect is larger for unmarried individuals without children and for young individuals. Among the young we also find a reduction in welfare entries. It thus seems that individuals with fewer family responsibilities are more responsive to the reform.

Essay 2: We study the impact of a set of labor market programs directed to unemployed welfare participants on criminal behavior. To isolate the causal effect we exploit the sequential implementation of activation programs in municipalities and city districts in Stockholm county. We find that criminal activity increased as a result of the programs. The size and significance levels of the estimates should be interpreted with caution, but we can conclude that the reform did at least not have a mitigating effect on crime. We find no evidence that the effect is larger for financially motivated crime.

Essay 3: Given the trend towards more active policies on reducing the take-up of welfare benefits, the consequences of leaving welfare constitutes an important issue. This paper studies disposable income and poverty among welfare leavers in Sweden during 19 years (1990-2008). Using a rich set of register data we can accurately measure disposable income and labor market outcomes. We find that there are large significant differences in post-welfare financial situation among those working full time and those who work only a little or not

at all. Leavers neither working nor receiving benefits from social insurance are likely to be financially dependent on family members, and are more likely than others to be in poverty. We conclude that leaving welfare is not always associated with becoming financially better off, post-welfare well-being depend heavily on labor market outcomes.

Essay 4 (with Matz Dahlberg and Linna Martén): In 1999, the Swedish government announced one of the largest reforms of the national defense in modern times, which led to closure and significant downsizing of several military bases, as well as large reductions in the workforce. Previous studies have found that workers that have been displaced from their previous employers experience significant earnings declines, even in the long run. In this paper we study the labor market effects of the involuntary job losses following the drastic changes of the Swedish security policy. Using population wide register data we estimate how labor income and unemployment benefit take-up changed among those employed at the affected military bases, relative to workers at unaffected military units. We find a decrease in labor earnings, primarily among civil servants. We find no effect on neither unemployment nor employment, indicating that the drop in earnings is likely to be driven by lower re-employment wages.

Acknowledgements

It is amazing to think about how many people have contributed to, or supported my own motivation to complete, this dissertation. There are countless people to thank, I will only mention a few.

First of all, I am thankful to my advisors, Matz Dahlberg and Eva Mörk. Without the encouragement from Matz I would never have considered starting the PhD program, and without the support and help from both of you I would definitely not have been able to finish.

I am thankful to Jon Fiva and Karin Edmark, the discussants at my licentiate and final seminars, for their comments and suggestions on my essays, as well as Björn Öckert for his careful reading. I have really appreciated the excellent support from the administrative staff at the Department of Economics, especially Katarina Grönvall. I am also thankful to Bob Shoeni and Frank Stafford who gave me the opportunity to spend a great semester at the University of Michigan in the fall of 2010.

I started the PhD program together with some wonderful people, thank you all for making the (often stressful) first year a great experience! Thank you Arizo - your support has been invaluable. Thank you Martin and Mattias for many great discussions and all your encouragement, and Susanne for all your optimism. Thank you Adrian, Erik, Kajsa, Karolina, Mattias, Oscar, Tove, all other colleagues as well as the founding members of FENSU for your enthusiasm, encouragement and helpful advice. A special thank you to Ulrika Vikman and Linna Martén for terrific co-authorship on two of the essays in this thesis.

Finally, I owe great gratitude to my friends and family. Thank you Johan for giving me confidence and inspiration when I need it most. Thank you Emma, my lovely sister, and my parents - your support means everything to me.

Uppsala, August 2013
Anna Persson

Contents

Introduction

The organization and functioning of the labor market is fundamental to the rest of society. Among other things, the socio-economic structure of any modern society is closely related to the labor market, and when labor market inequalities increases, economic inequalities are likely to grow. This is true both in the relationship between employers and employees, but also for the differences between those that have versus do not have a strong position on the labor market. Working is also an essential way of participating in society. Paid work, which implies contributing to society in the form of taxes, is possibly one of the most important ways that individuals experience cohesion and self-control. To this end, both the financial aspects, that is, being financially self-sufficient and not having to rely on support from society, and the social relations with co-workers can be equally important. It has been shown that an individuals perceived notion of social cohesion decreases with the time in unemployment, and at the same time health and well-being also deteriorates (Machin and Manning, 1999).

This thesis consists of four self contained essays, all relating in different ways to labor market attachment and consequences of not having a secure position on the labor market. More specifically, the thesis is centered around two themes, long-term labor market outcomes among individuals who are or risk becoming unemployed and the effects of labor market programs aimed at increasing self-sufficiency among individuals with a very weak labor market attachment. The two themes will be discussed below, starting with a general discussion of the ways that financial support is provided for unemployed individuals.

Income during unemployment

In most developed societies there is an ambition to somehow provide financially for individuals with an unstable position on the labor market or health problems that prevent them from working. This can be implemented in various ways, for example as a social insurance scheme where compensation is based on previous income, cash welfare benefits, or in kind, such as through food stamps or subsidized housing. Often a country runs several parallel systems, targeting different parts of the population.

The various types of programs share an important feature common to all income replacement and financial support programs, namely the conflict between providing support while incentivising individuals to work. Using the same, very

stylized, terminology as Lindbeck, Nyberg, and Weibull (1999), we can think of an individual as having two choices in her economic decision, either to work full time or to rely on public benefits. This decision will probably be heavily influenced by social norms since there is, in most societies, a strong normative pressure to earn one's own income from work. However, even though there might be a social cost of receiving benefits, there can also be large financial gains. Policy makers need to assert that the costs of participation balances the gains, and thus make sure that the income support programs are not attractive for individuals who are able to provide for themselves and are thus not truly in need of public support. If the social norms against benefits dependency are not strong enough, there are two common ways to discourage benefit take up and encourage work. One alternative is to set the benefit level low enough to incentivise individuals who are able to provide for themselves not to apply for benefits, since it would always imply a substantially lower standard of living compared to working. The other alternative is to create a system where benefit take up is associated with implicit costs (beyond the social stigma). Such costs could be a requirement to provide some effort, for example performing unpaid work or provide evidence of job search, in exchange for benefits. It is also possible to limit benefit take up through other regulations, such as restricting the total amount of time an individual can receive benefits during her lifetime.

In the Swedish welfare state the primary income replacement system for unemployed individuals is the unemployment insurance. To be eligible for these benefits, an individual needs to have had a sufficiently high income during the preceding year, and be a member of an unemployment fund. Unemployed individuals that do not qualify for unemployment insurance and can not rely on income from a spouse nor have any private savings will typically have to rely on social assistance. Social assistance is administered and financed by the municipalities and is designed to provide temporary support for individuals who lack other income. Compared to unemployment insurance and other income replacement programs, such as sickness insurance, available to those who are working, where eligibility is based on an individual's characteristics such as unemployment or an established illness, social assistance is strictly means tested and only covers the most basic household needs. This highlights an important feature of the Swedish welfare state, sometimes referred to as the welfare paradox. This notion is due to the fact that working individuals can benefit from relatively generous support if they become unemployed or sick, while those who are not working are only guaranteed a minimum standard of living.

Unemployment insurance benefits are often conditioned on taking part in a labor market program or searching for jobs actively. Benefit eligibility can also be terminated if an individual denies a job offer, or declines to participate in the assigned activities. Over all, there is a very strong focus on the return to employment. In the social assistance system, however, there has historically been

very little emphasis on the labor market situation of the participants, probably because many of them would typically have very low prospects of becoming self-sufficient. In many municipalities this changed during the financial crisis that hit Sweden in the early 1990s. The number of unemployed individuals increased dramatically, especially among individuals with a weak position on the labor market, such as immigrants and young individuals, who were not eligible for unemployment insurance. Thus, the number of unemployed individuals applying for social assistance increased dramatically (Dahlberg et al., 2008).

Labor market programs for uninsured individuals

Since unemployed individuals receiving social assistance were typically not enrolled in any of the labor market programs organized by the unemployment agency (although they were often registered as job-seekers), many municipalities started to provide locally administered programs. The aim of these initiatives was both to improve the employment prospects of the social assistance participants, make benefit take up less attractive, and reduce the costs of social welfare. In 1998, the Social Services Act, which states the legal framework of social welfare provided by the municipalities, was changed to make it possible for municipalities to condition social assistance for individuals who are able to work on participation in local labor market programs, often called activation programs. This marked a systemic shift in Swedish welfare policy, implying that social assistance, which previously provided unconditioned support to households in financial distress, was no longer an unconditional right, but rather a service that required applicants to gain eligibility by providing some effort in return (Dahlberg et al., 2008).

In some areas the activation programs were very ambitious and tried to mimic the services provided by the national unemployment agency. Initially, participation in the programs were more or less voluntary, but after the legislative change in 1998 more and more municipalities have started to condition eligibility for social assistance on participation in activation programs. According to the legal text this is possible either if the individual is below the age of 25, or otherwise if there is reason to believe that the activation program might help the individual to become self-sufficient. Generally, this has been interpreted as that receiving social assistance because of unemployment is a sufficient condition to require participation in an activation program, regardless of the individual's age. If the individual is assigned to a program and declines to participate, future benefits can be canceled or reduced. Since social assistance is only available for individuals who lack all other sources of income, the threat of being denied benefits is likely to be very severe. Municipalities differ substantially in how often this threat is realized. In some areas applications are immediately canceled if the individual fail to live up to the requirements, while in others it is common that the applicant were given several warnings before benefits are reduced. The vast ma-

jority of municipalities typically make exceptions for households with children, who can usually get enough benefits to cover expenses for food and housing regardless of whether they take part in the activation program or not (Salonen and Ulmestig, 2004). The extent to which municipalities use activation programs, and have chosen to make participation mandatory in order to qualify for benefits, varies greatly but has increased continually over time. Since the programs are not coordinated nationally, there is only very scarce information on the content and administration of the programs outside each respective municipality. Thus, it is hard for both policy makers and researchers to understand if the programs are important or not, and what effects they might have.

Assessing the effects of activation programs

This thesis contains two essays where we attempt to assess the effects of mandatory activation on individuals that are exposed to it. In these two papers we study activation programs in the municipality of Stockholm, where the different city districts initiated the programs at different points in time between 1998 and 2004. The information on these activation programs was gathered in a survey conducted by Karin Edmark and Kajsa Hanspers in 2004. In the survey municipality representatives from each city district were asked questions about whether or not they used activation programs, when the programs had become mandatory, and how many hours per week the participants were required to attend the activation center. This information allows us to determine when mandatory activation programs was implemented in each of the districts. Thus, we can use variation across both area and time and compare the change over time among individuals in districts where the programs had been implemented to individuals in areas where the program had not been implemented that far. This allows us to identify the causal effect of the introduction of mandatory activation on the outcomes we are interested in. Since the survey does not provide any information on the content of the programs, we have almost no information on how they were operated. Through a case study on the activation programs in the district of Skärholmen (Thorén, 2005), we know that activities were sometimes adapted to the client's needs, for example by providing Swedish courses to immigrants. However, the programs often consisted of supervised job search or unpaid work which did not necessarily improve the clients' employment prospects, but was rather a way to control the participants' willingness to work. Our data also lacks information on whether an individual participated in an activation program or not, and whether she applied for assistance for labor market reasons (and would thus be targeted by the programs). Instead we rely on information on where the individual lived and when each city district implemented mandatory activation programs. These data limitations implies that we cannot interpret our results as an effect of participating in any specific program. Rather, we estimate the effect of moving to a situation where benefit

eligibility is conditioned on being active, and where benefits can be denied due to non-activity, relative to a situation where there are no such threats.

One of the essays (co-authored with Ulrika Vikman) studies how the programs affected the flows of individuals into and out of social assistance receipt. Previous research (Dahlberg, Johansson, and Mörk, 2008) have shown that the programs contributed to a decline in the number of social assistance participants, but the mechanism behind this result has not been fully understood. Previous studies, primarily on data from the US, illustrates the importance of a reduced number of new social assistance applicants due to the implementation of stricter eligibility criteria (Grogger, Haider, and Klerman, 2003; Moffitt, 1996; Moffitt, 2003). This indicates that the programs works not only by affecting the program participants, but that they also have some effect on non-participants. Thus, some individuals are discouraged from applying for social assistance when benefits are conditioned on participation in an activation program. Such effects have also been established in other labor market programs, and has been referred to as a "threat-effect" (Black et al., 2003). Our result indicates that among young individuals there is a significantly negative effect on the number of new social assistance applicants in districts that have implemented an activation program, relative to other districts. This indicates that among the young the programs are not perceived as positive, and make individuals either find other sources of financial support (for example working harder to find employment or rely on help from friends and family) or accept a lower standard of living rather than taking part in the activation and collect social assistance benefits. We also find that the activation requirements increased the rate at which social assistance participants left benefit take-up.

The other essay on the topic relates the requirement to participate in activation programs to the risk of engaging in illegal activities. Results from previous research (Grogger, 1998; Machin and Meghir, 2004; Foley, 2011) indicates that a relationship between labor market factors and crime might arise through several different channels and can be either positive or negative. For example, it has been shown that crime is positively associated with financial hardship, which could imply that when it is harder to qualify for social assistance some individuals might be more inclined to engage in crime (Zhang, 1997). On the other hand, individuals who participate in the programs might improve their employment prospects and thus the implicit cost of engaging in crime increases. Being in the program also implies that there will be fewer opportunities to engage in crime as it reduces the amount of leisure (Lochner, 2004; Machin, Marie, and Vujić, 2011). I find weak evidence that conditioning social assistance on participation in activation programs contributed to an increase in the number of crimes committed, especially among young males. This effect could arise through a number of different channels. For example, it is possible that young males are discouraged from applying for benefits when activation re-

quirements are implemented and instead spend more time in illegal activities. It could also be the case that the activation programs bring together unemployed, socially excluded young males, who inspire each other to commit crimes.

Labor market attachment and unemployment in the long run

There is a substantial literature on how an individual's previous labor market experience might affect future outcomes. For example, it has been shown that even short spells of unemployment at the start of one's career causes significantly lower labor income several years later (Gregg, 2001) and that past spells of unemployment tends to bring future unemployment. It has also been shown that individuals who are displaced from an employer experience a lower trajectory for future earnings, even after they have been re-employed (Jacobson, LaLonde, and Sullivan, 1993). In an extreme form, these two notions combine in a low pay/no pay cycle, where repeated spells of unemployment are interrupted by employment in low paid and unstable jobs. Thus, unemployment does not only bring the immediate loss of income during the period of joblessness, but can also cause a more permanent "scar" through a higher incidence of future unemployment and lower earnings. Scarring can occur for several reasons, for example, during unemployment human capital can be lost both as new experiences are not gained and old skills deteriorate. Job loss in itself may also lead to a loss of firm or industry specific human capital acquired at the previous job. Another explanation is that unemployment is taken as a signal of low productivity or ability by future employers, which implies that the re-employment wage will be lower than for an otherwise similar worker who have not been unemployed (Arulampalam, Gregg, and Gregory, 2001). It is also possible that there are psychological factors associated with unemployment, making the transition back to employment difficult. For example, unemployment is associated with a lower self-esteem and damages the individual's perception of self-worth (Goldsmith, Veum, and William, 1996). It is also possible that after some time in unemployment, the individual becomes used to the situation and unemployment becomes the norm. This is often referred to as habituation and can reduce the individual's motivation to change her labor market status.

In this thesis there are two chapters concerning outcomes of individuals experiencing, or at risk of experiencing, unemployment in different ways. The first of the two relates to the issues discussed above, looking at transitions out of social assistance and long-term outcomes among individuals who have left social assistance participation. The second of the two chapters is a study of labor market effects among individuals affected by the large cutbacks in the military sector in 1999, where the closure of several military bases implied the destruction of thousands of jobs. I will discuss both these topics below.

Labor market attachment among social assistance participants

It is well documented that individuals relying on social assistance are often further from the labor market and are more isolated compared to other individuals, and that social exclusion increases with the length of the benefit spell (Bergmark and Bäckman, 2007). Although social assistance is intended to provide temporary support during short periods of financial difficulties benefit spells can often be long and extend over several years. Also, shorter benefit spells are often repeated, and it seems to be difficult for a previously assistance reliant individual to leave benefit take-up for good. This issue is important when trying to understanding the potential effect of the labor market programs for unemployed social assistance participants discussed above, since it is likely to depend heavily on how far from the labor market the targeted individuals are.

This study uses population wide register data covering a time period of 19 years, which allows me to follow individuals for a long time and look at both short- and long-term outcomes. I study both the probability of leaving social assistance for different labor market states, representing different degrees of labor market attachment, and how this labor market attachment is associated with disposable income, the risk of poverty and returning to benefit dependency. It is clear that very few individuals have a strong attachment to the labor market and are able to support themselves completely through work after leaving social assistance. It is more common that individuals who stop collecting benefits work part time, have temporary jobs, or remain completely out of the labor force. These post-benefit outcomes are strongly associated with long-term income and poverty. Social assistance leavers who have a strong labor market attachment experience relatively low risk of being in poverty, or needing to return to benefit take-up, while the opposite is true for those who have no or little attachment to the labor market. These results indicate that most social assistance participants are quite far from the labor market and that most individuals who stop collecting benefits have rather low prospects of becoming completely self-sufficient in the long run (at least without support from society). When discussing ways to reduce the reliance on social assistance, it is thus important to consider what the alternatives are, since otherwise many benefit reliant individuals risk ending up having to rely on family members.

Labor market effects of military base closures

The final paper (co-authored with Matz Dahlberg and Linna Martén) studies labor market attachment from a different perspective, namely what happens when individuals with a relatively strong position on the labor market, with high wages and a low risk of experiencing unemployment, either becomes unemployed or faces a reduced employment security. Studying the effects of unemployment at an individual level is problematic since individuals do not

become unemployed in a random manner. Instead, employers are more likely to dismiss less productive workers, for example workers with lower skills. In order to obtain estimates of the causal effect of becoming unemployed on any outcome of interest it is thus necessary to find some exogenous source of unemployment. In the literature it has been common to look at unemployment that arises due to a firm closure or mass lay-offs, that is, looking only at unemployment spells that occur after a very large number of individuals are displaced from the same firm (Couch and Placzek, 2010; Hijzen, Upward, and Wright, 2010). Thus, there is argued to be less selection of which workers to keep in the company and which workers should be dismissed. Using these methods the previous literature has found a number of long-term effects of unemployment. The effects are both economic, such as the lower re-employment wages discussed above, and social and medical, such as an increased risk of being admitted to the hospital and going through a divorce (Eliason and Storrie, 2009; Eliason, 2012). Clearly, unemployment can have large negative implications even in cases where the unemployment spell is not very long or causes large direct income losses.

We study the effect of being exposed to the large military base closures that occurred in Sweden in 1999, when it was decided that the Swedish defence, which had previously focused on the threat of invasion, should now focus on interventions in conflicts abroad. This implied a significant downsizing of the whole military sector, and a large number of individuals had to look for employment opportunities elsewhere. The cutbacks were limited to military bases in a few municipalities, while units in other areas where left unaffected. Thus, we study how the labor market outcomes of military employees in the affected areas differ from similar individuals employed by military units in unaffected municipalities. Relative to other groups of workers, individuals employed in the military sector tends to have higher earnings and a higher education. Thus, one would assume that their position on the labor market is quite strong. However, it is also important to consider that military employees, especially those who have had all their training within the military sector and are employed as military personnel rather than as civilians (such as administrative or health care personnel), have a very specific form of human capital. These individuals typically have technical or strategic expertise that are not fully applicable in other parts of the labor market, and although they are highly educated they might experience some difficulties in the labor market outside the military sector. Another important factor is the role of occupational identity. It is possible that military employees identify more strongly with their occupational role than the average worker, since the military have a very specific role in society. Thus, it might be more difficult for this group to cope with threats to this occupational identity, and the transition into another part of the labor market might be more difficult than it would be for other workers.

We find that, relative to the unaffected group, individuals that were affected by the military base closures in 1999 were not more likely to be unemployed five years later. However, although they were employed at the same rate as the control group, average labor income was persistently lower among those who were exposed to the closures, especially among civil servants. Thus, our results indicate that although the difficulties discussed above might be important, they do not hinder military employees from finding new jobs outside the military sector, although they might have to accept a lower wage or working fewer hours relative to when working in the military.

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Essay 1

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 (that is, the overall fraction of individuals receiving social assistance), and a positive effect on employment (Dahlberg, Johansson, and Mörk, 2008). In this study we decompose these previous results into effects on entry and 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 the other half was explained by increased exit rates. 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 of activation requirements on welfare entry is 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 prepa-

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

ration or job search program. There are also optional activation programs in which non-compliance 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 (see for example Chambers (1989) and Brett (1998)). 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 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 fact 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.[2] This has previously been explicitly studied by Edmark (2009) for the same reform and most of the years that are used in our study. She shows that the implementation of activation requirements did not increase out migration of welfare-prone individuals and thus we conclude that

[2]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).

migration is very unlikely to bias the results of this study.[3]

In this study we find that mandatory activation had no effect on the overall probability of entering welfare but the probability of exiting welfare increased with 0.9 percentage points. However, for young individuals the activation requirements had a rather large effect on the entry rate into welfare, a reduction of 0.6 percentage points, and the increase in the exit rate was also somewhat higher than for the average for the whole population of welfare recipients. We also find larger exit effects for unmarried individuals without children, 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 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 (for example Gueron and Pauly, 1991; Friedlander and Burtless, 1995), but there are 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 has therefore by construction 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

[3] 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. 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.

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 effects of the reforms are 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 non financial factors, such as work requirements. He uses survey data from only post reform years in three American cities where single-mothers both on and off welfare were surveyed. Recipients where asked questions about work and other requirements and sanctions. To capture effects on entry rates the survey includes questions to TANF applicants about different diversion programs, where the applicant is required to work or demonstrate job search prior to application. Moffitt finds that work requirements increase exit rates, but have no significant effect on the entry rate of the applicants. 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 of the policies 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.[4] 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.

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

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 elderly 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 the distribution of 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 benefit 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 sick leave benefits, 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 have decreased somewhat since then. However, the cost of benefits per household has increased substantially throughout the whole time period. In 1983, the average yearly amount of benefits received among those on social assistance was around SEK 9,000 (USD 1,125)[5] per year and household. In 2008, this figure was almost SEK 44,000 (SEK 5,500). 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 the number of 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

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

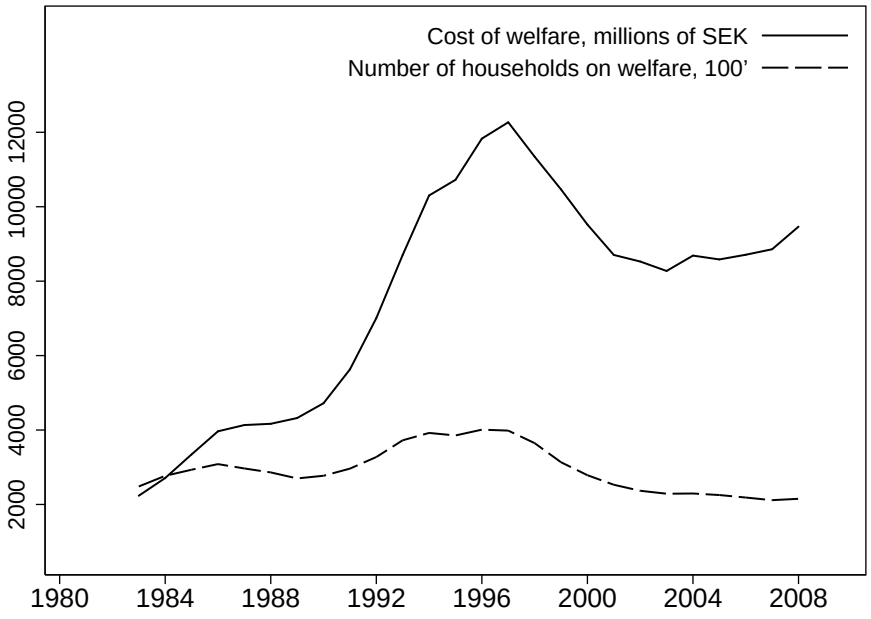

Figure 1: Cost of welfare (millions of SEK) and number of welfare households (100's) 1983-2008. Source: Statistics Sweden.

participants to take part in activation programs to retain their eligibility.[6] 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 district councils. During the time period relevant to this study, there were 18 city districts within the municipality. City districts 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.

Table 1 shows some characteristics (as of 1993) of the city districts used in this study. The second column represents the mean social assistance received, including all individuals in the districts, that is, even those who do not receive social assistance. Clearly, this varies substantially, from around SEK 1,000 for Bromma to SEK 5,800 for Rinkeby. However, for those actually receiving social assistance, the mean only varies between SEK 15,400 and SEK 19,100

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

(shown in the 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, the highest social assistance entry rates and the lowest exit rates.

For around three quarters of the social assistance recipients in Stockholm in 2005, unemployment is the main 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.

The starting year of the activation programs in each district is determined using results from questionnaires and interviews conducted by Karin Edmark and Kajsa Hanspers. 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 program participation. It was possible to determine a starting year for 12 of the 18 city districts. In the five most centrally located districts as well as in 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.[7] 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 social assistance 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 imple-

[7]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 Skarpnäck is close to the average

Table 1: City district characteristics 1993

	Share of welfare recipients	Average welfare benefits[a]	Average disposable income[b]	Share born in non-Western countries	Average benefits per recipient[c]	Population	Entry rate	Exit rate	Activation year
Rinkeby	0.308	5785	96,052	0.463	18,771	5737	0.115	0.229	1998
Skärholmen	0.111	1713	124,328	0.124	15,387	15,124	0.048	0.319	1999
Farsta	0.115	2181	128,714	0.048	18,918	21,758	0.047	0.302	2001
Kista	0.171	3189	126,035	0.226	18,602	14,439	0.073	0.279	2001
Älvsjö	0.067	885	145,118	0.032	13,175	10,184	0.033	0.340	2002
Hägersten	0.072	1380	134,266	0.032	19,080	14,437	0.032	0.349	2003
Liljeholmen	0.095	1744	126,067	0.039	18,303	14,815	0.042	0.325	2003
Spånga-Tensta	0.149	2555	131,017	0.214	17,131	15,795	0.058	0.289	2003
Bromma	0.058	998	154,035	0.025	17,217	28,318	0.026	0.352	2004
Enskede-Årsta	0.075	1318	133,375	0.043	17,686	21,682	0.030	0.363	2004
Hässelby-Vällingby	0.071	1140	141,590	0.048	16,043	30,094	0.032	0.342	2004
Vantör	0.122	2219	124,368	0.067	18,152	16,943	0.048	0.298	2004
Total	0.102	1798	133,960	0.085	17,594	209,326	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.

ment 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 job centers that social assistance recipients are required to attend for at least a few hours each week, varying 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 labor market programs for unemployed individuals relying on social assistance. Unemployed recipients were directed to the unemployment office, but there were no sanctions if they did not participate in any activities or searched for actively. 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 job center. In order to retain eligibility for welfare, the applicant must visit the job center 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 job center 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 job center 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 applies for benefits, that is, when an unemployed individual applies for social assistance he or she is sent to the job center 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.[8] 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[9] that the individual has received during the past year as well as ben-

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

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

efits 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 used classification. What we refer to as social assistance is thus the individual's share of the household's total received benefit.

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 from our sample during their first three years in Sweden to avoid capturing the dynamics that arises due to this sort of support. Table 2 shows descriptive statistics for our sampled population. The mean amount of welfare benefit received by an individual is slightly above SEK 2,000 (USD 250) 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 SEK 23,600 (USD 2,950) per year.

Table 2: Summary statistics

	Mean	Std. dev
Social assistance (SEK 100')	20.667	99.936
Share with social assistance	0.087	0.283
Income[a]	166,323	268,045
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	2,986,175	

[a] 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 only 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 define this group using individual characteristics; 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. What we can do 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 individuals 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 the population are probably harder to notice. Those leaving welfare due to the reform have a 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 individuals, immigrants born in non-Western countries, single parents and individuals with few years of education. Because these groups have a higher probability of receiving benefits than others, we attempt to create a more well defined entry sample by estimating effects on entry rates using only a sub population consisting of individuals with any of these characteristics. Thus, we reduce the problem of es-

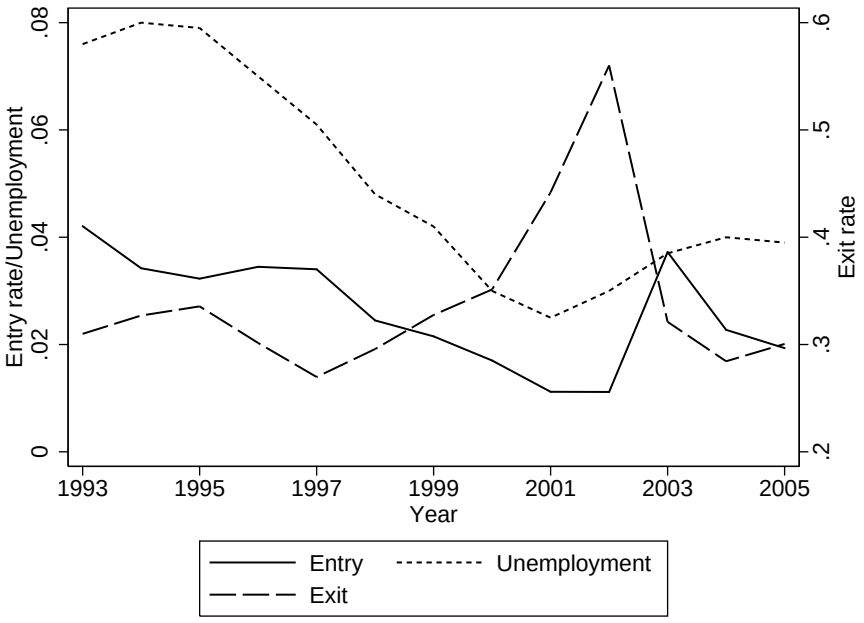

Figure 2: Unemployment rate, raw entry and exit rates, by year in Stockholm

timating an effect for individuals who have close to zero probability of ever participating in welfare (for example, individuals with high education and income are unlikely to change their behavior in response to a reform that will probably never affect them). It would also be possible to define the population at risk of social assistance based on income, since 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 a bias because poverty is likely to be higher in an area with low benefit levels (or stricter eligibility criteria) and vice versa, which might affect welfare participation as well as entry and exit. Thus, we prefer constructing the risk group based on individual characteristics.

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 single individuals without children, who are also very mobile (Fiva, 2009). This is a group with low prob-

ability 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 sub populations 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 estimate the treatment effect of mandatory activation we use a difference-in-differences approach over multiple events and time periods in a linear probability model. 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 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, we sample all individuals receiving social assistance at *t-1*, and the event of interest is whether or not receive social assistance at *t*, $P(W_{it} = 0|W_{it-1} = 1)$.

Since all city districts implemented mandatory activation, they will all be in both the control group and the treated group. In a certain year the districts who have changed policy are treated while the rest of them are in the control group. The identifying assumption is thereby that the city districts would have developed similarly if no treatment had occurred. Thus, implementation of activation requirements cannot be related to (unobserved) city district-specific conditions.

In the linear model 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

$$Y_{ijt} = \alpha_{\mathbf{j}} + \tau_{\mathbf{t}} + \beta D_{jt} + \gamma_{\mathbf{t}}\mathbf{X_{ijt}} + trend_j + \eta_{ijt} \quad (1)$$

where α_j and τ_t are city district and year dummies, respectively. β then gives us the effect, average across all city districts and years, of mandatory activation on the probability of entry and exit. To control for individual heterogeneity that varies over time, $\mathbf{X_{ijt}}$ is included.[10] 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).

Since treatment only occur at the district level the observations may not be independent within districts, which can cause a downward bias of our standard errors. As our data only covers 12 twelve city districts we are not able to use the common solution and cluster at the level of treatment. Instead we follow Wooldridge (2003) to check if there is any correlation between observations in the same district. If the null hypothesis of no unobserved city district correlations are rejected we will use the method proposed by Donald and Lang (2007) (hereafter D-L procedure) and do the estimations for the group averages instead. 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 statistic is rejected at the five percent level.[11]

5 Results

In the following section 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 estimating a placebo regression, and in section 5.4, we determine

[10] 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).

[11] If there is no correlation in standard errors within clusters, the D-L procedure reduces the amount of available information more than necessary which is why we only use this procedure if the test proposed by Wooldridge (2003) is rejected.

whether the treatment effects varies over time. Finally, in section 5.5, we study if the effects are heterogeneous across groups.

5.1 Effects on caseloads

According to Dahlberg, Johansson, and Mörk (2008), the welfare caseload 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 sub populations using equation (1). 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 null hypothesis in 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.

Table 4: Estimation results: Caseload

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

Year dummies, city district dummies, city district linear trends and time varying controls are included in all estimations. Standard errors in parentheses. D-L standard errors in square brackets.
* p<0.1, ** p<0.05, *** p<0.01

In our estimation, we find a smaller reduction in welfare participation due to the reform, 0.3 percent, compared to Dahlberg, Johansson, and Mörk (2008), and 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 the 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 which might explain this discrepancy. In our sample we include Rinkeby, we have two additional years of data and we define immigrants from non-Western countries in a slightly different way. Furthermore, in our sample, immigrants are not included during their first three years in Sweden, compared to two years 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 not 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 sub samples even though we find it when we include all individuals, since different groups of individuals can be more or less homogeneous across districts. Thus, we might find heterogeneous shocks when studying the whole population, but not when restricting the sample to subgroups where the individuals are possibly more similar to each other.

5.2 Baseline estimation

Table 5 shows 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 becomes insignificant. We conclude that we are not able to identify any effects of activation on welfare entry. The reform may, however, still have had an effect at different times after implementation and for different sub populations, especially for populations at greater risk of entering welfare (see section 5.4 and section 5.5).

Table 5: Estimation results: Entry and exit

	Entry	Exit
Mandatory activation implemented	-0.001 (0.000)** [0.002]	0.009 (0.004)**
Wooldridge-test; SSR(df)[p-value]	134.4 (107) [0.037]	30.6 (107)[1.000]
N	2,698,222	287,953

Year dummies, city district dummies, city district linear trends and time varying controls are included in all estimations. Standard errors in parentheses. D-L standard errors in square brackets.
* p<0.1, ** p<0.05, *** p<0.01

The point estimates for the exit rate 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, such as diverging trends in the treatment and control groups, 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.[12] 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.

Table 6: Results from placebo estimations: Entry and exit

	Entry	Exit
Mandatory activation implemented	0.001 (0.001)** [0.001]	0.003 (0.004]
Wooldridge-test; SSR(df)[p-value]	14.453 (48) [0.998]	7.2 (48)[1.000]
N	1,530,957	188,904

Year dummies, city district dummies, city district linear trends and time varying controls are included in all estimations. Standard errors in parentheses. D-L standard errors in square brackets.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$

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 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 the activation programs.

5.4 Time-changing treatment effects

Even if we are not able to assess any effect on the overall entry rates following 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 1 slightly and estimate separate treatment effects for the year of implementation,

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

the first year after implementation and two or more years after implementation. The results are given in Table 7 .

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]	
Wooldridge-test; SSR(df)[p-value]	131.018	29.291
	(105) [0.044]	(105) [1.000]
N	2,698,222	287,953

Year dummies, city district dummies, city district linear trends and time varying controls are included in all estimations. Standard errors in parentheses. D-L standard errors in square brackets. * p<0.1, ** p<0.05, *** p<0.

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.

5.5 Heterogeneous effects

Population at risk

As mentioned in section 3.3, certain groups of individuals[13] 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, we find no effect on the entry rate.

Effects on sub populations

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 activa-

[13] 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

Mandatory activation implemented	–0.001 (0.001) [0.003]
Wooldridge-test; SSR(df)[p-value]	44.935 (96) [1.000]
N	877,762

Year dummies, city district dummies, city district linear trends and time varying controls are included in all estimations. Standard errors in parentheses. D-L standard errors in square brackets. * p<0.1, ** p<0.05, *** p<0.

tion on young individuals and individuals born in a non-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. We find that 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, 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 both a larger reduction in entry rates for this group, and an increase in the exit rate. Also, young individuals might be more likely to move back to live with their parents to avoid the activation programs.

Table 9: Estimation results:Age< 26

	Entry	Exit
Mandatory activation implemented	–0.006 (0.002)***	0.014 (0.009)*
Wooldridge-test; SSR(df)[p-value]	0.211 (107) [1.000]	0.096 (107) [1.000]
N	312,850	59,475

Year dummies, city district dummies, city district linear trends and time varying controls are included in all estimations. Standard errors in parentheses. D-L standard errors in square brackets. * p<0.1, ** p<0.05, *** p<0.

The results for immigrants born in a non-Western country are presented in Table 10. We find positive effects for both entry and exit, but neither is statistically significant. Since the activation that immigrants participate in is likely to consist mainly of language training, and thus differ from that offered to other clients, it is not surprising that we get unexpected results.

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

	Entry	Exit
Mandatory activation implemented	0.001	0.000
	(0.002)	(0.005)
Wooldridge-test; SSR(df)[p-value]	14.361	17.290
	(107) [1.000]	(107) [1.000]
N	260,084	112,833

Year dummies, city district dummies, city district linear trends and time varying controls are included in all estimations. Standard errors in parentheses. D-L standard errors in square brackets. * p<0.1, ** p<0.05, *** p<0.

We also present results from separate estimations for unmarried individuals without children as this group could be expected to be relatively 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 the 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
Mandatory activation implemented	–0.001	0.020
	(0.001)	(0.006)***
Wooldridge-test; SSR(df)[p-value]	45.775	16.134
	(107) [1.000]	(107) [1.000]
N	1,249,097	146,898

Year dummies, city district dummies, city district linear trends and time varying controls are included in all estimations. Standard errors in parentheses. D-L standard errors in square brackets. * p<0.1, ** p<0.05, *** p<0.

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 activation programs are implemented, but in most cases, the researchers have only included individuals who are already receiving welfare benefits and have therefore 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 theoretical predictions, 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.

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 setup over multiple events and time periods.

Our results indicate that entry rates decrease as a result of mandatory activation, but these results are not robust to 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. For young individuals the entry rates were reduced 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 relatively large, positive effects on the exit rates.

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. Young individuals, who become less likely to start collecting benefits when participation in the program becomes mandatory, might be 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 in-

dividuals 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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Essay 2

The Effect of Welfare-to-Work Programs on Crime: Evidence From a Natural Experiment

1 Introduction

Economic hardship is associated with a number of social problems, of which one important component is crime. Individuals experiencing poverty and social exclusion are over represented in criminal registers, both as offenders and as victims (Socialstyrelsen, 2010; Brottsförebyggande rådet, 2008). Thus, the question of how crime is affected by policy interventions targeted at this group is of great economic and political significance. For example, it has been debated whether income replacement programs, such as welfare benefits, create an environment of marginalization and social stigma that encourages criminal behavior. Thus, labor market programs that promote employment and self-sufficiency among disadvantaged individuals can also have a mitigating effect on illegal activities. On the other hand, lowering welfare benefits has been argued to cause crime levels to rise, because of increased financial distress or frustration among welfare reliant individuals. In this paper, we investigate some of these mechanisms further by exploiting the variation caused by the sequential implementation of mandatory activation programs aimed at unemployed welfare participants in Stockholm. We have access to individual-level data on all crimes that have been tried in all Swedish district courts and we can link this information to place of residence, various demographic characteristics (own and parental), and information on income sources. This unique data allows us to identify a causal effect of workfare programs on an individual's propensity to be involved in criminal activities.

The approach in this paper differs from that of most earlier studies in that we study the effect of mandatory activation programs, rather than changes in benefit levels. We use the fact that social assistance for unemployed individuals became contingent on participating in assigned activities at different points in time in different municipalities in Stockholm county and different districts within Stockholm municipality. Thus, the prevalence of activation requirements varies both over time and across regions. The municipalities in Stockholm county are all part of the same labor market region, and thus we can implicitly control for common labor market shocks. Within Stockholm municipality there are 18 districts with significant freedom in administering, but not financing, the welfare system. Thus, when studying only the districts within the municipality of Stockholm, we can hold the benefit level and political representation in the district councils constant.

There are several aspects of the Swedish welfare system that motivate this study. First, Sweden has a very general program for income support and in a given year a relatively large fraction of the population receives financial support in some form. Thus, reforming this system might have important effects, not only on financial but also on social outcomes. Secondly, the social assistance benefits are not targeted at families, as is often the case in other countries. In-

stead, there is a large fraction of young males in the welfare caseload, a group that is also heavily over-represented in criminal registers. Thus, unlike in for example the US where the welfare caseload consists mainly of mothers with dependent children, there may be a strong and direct link between the social assistance benefit system and criminal activities.

The advantages of using individual-level data is that we can identify effects for precisely defined groups. Thus, in this paper, we can separate the effect of the activation programs between those that are targeted by the programs, that is, the more welfare prone, and others, such as individuals that are more likely to engage in crime. As far as we are aware, this type of data has very rarely been used to study the causal link between welfare usage, labor market programs, and crime. Having a good understanding of this relationship is important in order to fully capture the social and financial benefits of such programs.

Our main findings indicate that the activation programs caused an increase in the average number of convictions per individual. Since different inference methods yield somewhat different results, the size and significance levels of the estimates should be interpreted cautiously. However, we can conclude that the activation programs do not seem to have had a negative effect on the crime rate. The estimated effect is larger for young men, representing an increase in the average number of conviction of almost six percent due to the reform. We are not able to isolate the mechanism behind this result, one possible explanation is that some individuals (young men in particular) lose eligibility for benefits or choose not to apply for them when they are conditioned on program participation (this result is shown in Persson and Vikman (2010)). Thus, they probably experience a reduction in income which could also be associated with more frustration and social exclusion, something that could lead to more risky behavior. We find no evidence that the effect is larger for financially motivated crime, such as theft and shoplifting. Given the structure of the data and how treatment varies there are some concerns about how to achieve valid inference. The direction and magnitude of the results are robust to a number of specification checks, but the significance levels should be interpreted with caution.

In the following section, we present some related literature, followed by a background on crime in Sweden, as well as a description of the institutional setting and the reform that gave rise to the variation we will exploit. Thereafter, we describe the data and present descriptive statistics. In section 5, we present the main estimation strategy, discuss potential identification issues, and alternative econometric approaches. Next, we present the results, robustness checks, and heterogeneity analysis. In section 7, we discuss the results and conclude the paper.

2 Related literature

This paper relates to several strands of the earlier literature. The relationship between economic thinking and crime is a classic issue, dating back to G. Becker (1968) who formalized the economic rationale for engaging in crime. The relationship between public policy, primarily labor market and education policies, and crime has also received attention, but has been empirically understudied due to a lack of reliable data.

There are a number of papers relating welfare receipt to crime. These papers typically focus on the income effect of benefits, arguing that a higher benefit level weakens the financial incentives to engage in crime. This relationship is supported by both theoretical and empirical evidence. For example, Zhang (1997) develops a theoretical model that illustrates how a lower benefit level leads to more crime. The primary mechanism behind this is that with higher welfare benefits there is less need for recipients to engage in criminal activity to earn (illegal) income. He also tests this relationship using data from the US, finding a strong negative correlation (although no causal relation is established). This result is also in line with findings in cross-national comparisons, such as Savage, Bennett, and Danner (2008), who find a positive association between higher spending on social welfare and low crime rates.

Since the reform of the welfare system in the US, starting in the mid-1990s, theoretical and empirical work on the various aspects of the reforms and waivers have generated a large literature. Most closely related to this paper are the evaluations of work requirements and improved financial incentives to find work. It has been found that conditioning welfare eligibility on job search has reduced welfare participation. However, it has proven to be more effective in terms of labor market outcomes to combine employment services with education and earnings supplements for low-wage earners (Bloom and Michalopoulos, 2001). Another related topic is if and how welfare reform have affected the children of welfare participants. The evidence on this relationship is inconclusive, but there are indications that some aspects of the reforms led to a significantly higher risk of being in trouble with the police among children above age 10 (Grogger and Karoly, 2005). The negative effects of welfare reform on adolescents are also discussed in Clark-Kauffman, Duncan, and Morris (2003) and Waldfogel (2007).

This study also relates to studies on the relationship between unemployment, or labor market opportunities in general, and crime. A number of papers have studied this connection from various perspectives. For example, Grogger (1998), Gould, Weinberg, and Mustard (2002), and Machin and Meghir (2004) find that individuals who engage in crime are responsive to financial incentives and argue that the higher crime rates among young males can largely be explained by their lower average wages. Raphael and Winter-Ebmer (2001) find a pos-

itive effect of the state unemployment rate on most types of crime (but the effect is negative for rape and murder). However, very little attention has been given to the effect of labor market programs on crime. One of the few papers that attempts to study this relation is Schochet, Burghardt, and McConnell (2008), finding that the Job Corps program in the US, a labor market program targeting low-educated youth, reduced the probability of being arrested. The effect was larger among older program participants and for less serious crime. Fallesen et al. (2012) study a small-scale welfare-to-work program in a Danish municipality, finding that, compared to unemployed, uninsured individuals receiving non-pecuniary benefits, individuals who were required to take part in training programs reduced their criminal activity.

Using Swedish county level data, Edmark (2005) finds a positive effect of aggregate unemployment on crime. This result is confirmed by Öster and Agell (2007) in another study of the effect of labor market policy on crime. However, they find no effect of spending on labor market programs on crime among youth. Using individual-level register data, Grönqvist (2011) finds that unemployment leads to a large and statistically significant increase in the risk of conviction of several types of crime.

An effect of labor market programs on crime might arise through a number of different mechanisms. The channels can be both financial, if the programs make welfare less and crime relatively more attractive, as well as social, since participation in the programs might imply being put in a new network of peers (which may have both a positive and negative effect on criminal behavior, depending on the characteristics of the peer group). If the programs implied that individuals more prone to engage in crime were brought together at the activation center this might have a positive effect on crime, for example through reduced stigma (Glaeser, Sacerdote, and Scheinkman, 1996) or information sharing (Calvó-Armengol and Zenou, 2004). It is also possible that the programs impose some structure on the everyday life of the participants, who therefore will have less time to engage in crime. To some extent, the activation programs can also be seen as an investment in human capital, which would increase expected future earnings and thus reduce the relative returns to crime.[1] It is also important to note that the effect might differ between different categories of crime. For example, expressive crime, such as violence, vandalism and robbery, might be affected differently than instrumental crimes with a larger financial component, such as theft.

Earlier studies of the effect of the activation programs in Stockholm indicate that the reform led to a decrease in the number of young individuals applying for welfare (Dahlberg, Johansson, and Mörk, 2008; Persson and Vikman,

[1] This relates to the literature on the relationship between human capital and crime, for example Moretti, Henderson, and Thisse (2004), Lochner (2004), Lochner and Moretti (2004) and Machin, Marie, and Vujić (2011).

2010). This could imply that there was also an increase in the number of young individuals experiencing financial difficulties and needing to rely financially on their parents. This could lead to frustration and a feeling of insufficiency, something that, in the sociology and criminology literature, is often associated with deviant behavior. For example, according to Merton (1938), one way to handle such difficulties is to give up on both success goals and honest means and "drop out" of society. Hirschi (1969) argues that social ties are a crucial determinant of who engages in crime. Individuals who experience less involvement and are subject to less social control have less social capital to lose when deviating from societal norms and are thus more likely to take part in crime. Another theory of deviance has stressed the importance of labeling and stigma (H. Becker, 1963; Goffman, 2002). These theories argue that an individual who is in some way deviating from the norm is labeled in the public mind as delinquent. Such labels can affect the individuals adversely, those that are discredited are more likely to develop a deviant behavior. Thus, if the activation programs lead to a feeling of social exclusion among some individuals, we might suspect that this group may become more likely to engage in crime and other activities associated with deviant behavior.

3 Background

3.1 Crime and criminal justice in Sweden

As in most western countries, the number of reported crimes have increased in Sweden since the mid-1900's. To a great extent this can be traced back to the dramatic increase in the number of acquisitive crime, which is in turn closely associated with economic growth and higher average income. However, at the beginning of the 1990s, the crime rate stabilized and has since only increased slightly (Brottsförebyggande rådet, 2008). One of the crime types that has continued to increase is violent crime, which might be a result of a lower tolerance for violence and thus an increase in the propensity to report these types of crimes. This illustrates the role of public opinion when constructing and interpreting statistics of crime. For an illegal action to be registered as a criminal act it must be observed by someone, recognized as a crime, and reported to the police. Hence, if the public becomes less tolerant of violence, more violent crimes will be reported. Another potentially important factor that may have contributed to the increase in the number of violent crimes is the steady increase in the consumption of alcohol.[2]

Young individuals are clearly over-represented in most types of crime, theft in particular, while there is no clear age profile for violent crime. Crime is also heavily concentrated among individuals with low income and in areas with high

[2]The effect of alcohol consumption on crime is shown in Grönqvist and Niknami (2011).

unemployment and welfare dependence.

Sweden has two parallel types of courts: the civil courts and courts handling conflicts between the citizens and the public authorities. Civil courts handle both criminal and civil cases and have three instances- district courts, courts of appeal, and the Supreme Court. The district court is the court of first instance and there are 48 such courts in Sweden. A criminal case is usually instituted by an application from a public prosecutor to the court about initiating prosecution against a suspect. A crime is determined to be solved either if a perpetrator can be linked to the crime and is found guilty, or if the offense is determined not to be a crime, when there is no suspect or when the suspect is younger than 15 years old. In the data for this study, we observe all cases where a perpetrator has been identified and found guilty. However, not all of these cases lead to a prosecution. For example, if the offender is very young the court can decide to absent from prosecution (*åtalsunderlåtelse*). Also, for less severe crimes the police or prosecutor can issue a summary punishment by fine (*strafföreläggande*), rather than initiate a prosecution. However, for both these types of sanctions the offender is registered in criminal records, which may have consequences if he/she commits further crimes in the future as well as when applying for jobs. 36 percent of all solved crimes lead to prosecution and a legal sentence. Around a fourth of these sentences involve imprisonment.

3.2 The social assistance system

The Swedish social assistance system is designed to be a final safety net in society and to serve as temporary support for individuals with no or very limited financial resources. The benefits are means-tested and households are only eligible for social assistance benefits if they are determined by the caseworker to have sufficiently low income and no valuable assets.

Unlike social insurance, such as unemployment and sickness insurance, social assistance is administered and financed by the municipalities rather than the national government. Thus, unemployed individuals not eligible for unemployment benefits are typically not enrolled in the labor market programs that are organized by the unemployment agency to the same extent as those who receive benefits from an unemployment fund. Until the late 1990s, most welfare participants did not take part in any activation or education programs and there were no systematic attempts to move them into the labor force. This became increasingly problematic during the financial crisis in Sweden during the early 1990s. The recession led the number of unemployed who were not entitled to unemployment insurance to increase to historically high levels. As a result, there was a large number of unemployed, the majority were young individuals with low education, who had to rely on social assistance and received unconditional benefits. This put financial pressure on the municipalities, and to reduce the cost of social assistance some started to introduce locally admin-

istered programs to increase self-sufficiency among benefit recipients. In 1998, the Social Services Act, the legal framework that states the main principles for financial support, was changed to explicitly allow municipalities to condition social assistance on participation in activation programs. The general knowledge about the prevalence and contents of these local labor market programs is scarce, and little is known about their efficiency. In 2002, around 12 percent of all social assistance participants of working age took part in some kind of activation program (Salonen and Ulmestig, 2004). This number has increased over time, but more precise recent information is lacking. In this study, we use information only on programs in Stockholm county where we have reliable data on which programs were operating during the relevant time period. For around 40 percent of all social assistance participants unemployment is their main reason for benefit eligibility, and no or insufficient unemployment compensation is the most common reason for benefit receipt among all demographic groups (Socialstyrelsen, 2011).

3.3 Activation programs in Stockholm municipality and Stockholm county

Stockholm county has 26 municipalities, the biggest of which (at least in terms of population) is Stockholm municipality, which is also the Swedish capital. The administrative responsibility for public welfare services, among which social assistance is an important part, in Stockholm municipality is decentralized to city district councils. During the time period we study in this paper, there were 18 city districts, in later years some of these have been merged (in 2010, there were 14 districts).

The district councils are led by politicians, but there are no elections at the city district level, the councils have the same political representation as the municipal council. Also, their responsibilities are primarily administrative and the districts are not responsible for financing the services they provide. Taxes are collected by the central municipal council and resources are divided among the districts according to their estimated needs (rather than actual costs). Their financial independence is thus limited, and in the case of social assistance, guidelines for the benefit amount are set at the municipal level and are thus common for all districts. However, during the time period relevant for this study the districts had substantial freedom in how to practically organize the work with social assistance participants (this freedom has since then been reduced and decision making is now more centralized). Thus, when the Social Services Act was changed to allow for municipalities to condition social assistance benefits on the recipient taking part in activation programs, these policies were not implemented uniformly throughout Stockholm municipality. Rather, the districts sequentially implemented different types of mandatory programs for the unemployed welfare participants, independent of budgetary and political

motives, starting with Rinkeby in 1998. Before the programs were launched, welfare participants were mostly in contact only with the social worker at the welfare office, and there was no systematic cooperation with the employment services. Unemployed welfare applicants were encouraged to register at the unemployment office and to look for work, but there were no sanctions if the individual did not follow these directions. With the implementation of the activation programs, the system changed so that when an unemployed individual applied for social assistance she or he was immediately sent to the activity center. The activities at these centers varied between districts, and the number of hours per week that the individual was required to attend the center varied between 4 and 15 hours (Edmark, 2009). The more exact content of most of the programs is unknown, but through a case study by Katarina Thorén (Thorén, 2005), we do have some more insight into the program that was operated in the district of Skärholmen. This program received much attention and has been said to have influenced the programs that were later implemented in other districts. The program in Skärholmen became mandatory for all social assistance participants in 1999. It was one of the most ambitious programs in terms of required attendance, participants had to attend the center for at least three hours per day, following a rotating schedule. The activities mostly consisted of supervised job search where the participants got access to computers, telephone and mailing services free of charge. Thorén (2005) notes that the activity center was often crowded and that resources were insufficient. Thus, the activities often served as a test of the individuals' willingness to work rather than providing any meaningful training.

The programs in Stockholm have been evaluated in a few recent papers. Dahlberg, Johansson, and Mörk (2008) find that the programs reduced the number of welfare participants and increased employment. The effect is stronger for young individuals and immigrants born outside the OECD, that is, groups that are more likely to be on welfare. Persson and Vikman (2010) find that the reduction in welfare participation can to some extent be explained by that the programs reduced the number of welfare entrants, especially among young individuals. Thus, young individuals who would otherwise start taking up welfare benefits refrain from doing so when benefits are conditioned on participating in activation schemes. Overall, the estimated effects on welfare participation are small in economic terms. However, it might contribute substantially to an increase in the crime rate since the group that is primarily affected is the group that is most likely to engage in crime.

The same type of programs as in Stockholm municipality were also in operation in some neighboring municipalities, and in nine cases we have sufficient information on if and when the programs started and who they targeted. Thus, we add individuals living in these municipalities to the analysis. Doing this, we lose the implicit control of political representation, since the other munic-

ipalities will have different political leadership. We are also unable to control for the norms around the determination of the benefit level. However, as our results show, this does not have much impact on the results.

In order to draw correct inference, we must assert that there are no other factors affecting the crime rate that changes discontinuously at the same time as the activation requirements are implemented. One might, for example, be worried that police resources and priorities change when new social policies are adopted. However, police districts do not follow the same administrative borders as the city districts, and it is unlikely that there is any coordination between the systems. Another potential problem is the possibility that welfare-prone individuals chose to move between districts or leave the municipality in order to avoid having to take part in the activation programs. Edmark (2009) explicitly studies how the activation programs affected the migration patterns of welfare-prone individuals. She finds that the programs did not have an effect on the probability of moving, neither between districts within the municipality nor from the municipality.

4 Description of the data

4.1 Data registers

In the analysis, we use data from several different registers administered by Statistic Sweden. These contain information on individual characteristics such as gender, year of birth, place of residence, et cetera, as well as information on income and all income sources. This data has been linked to conviction data gathered and administered by the National Council for Crime Prevention (Brottsförebyggande rådet, BRÅ). We have information on all criminal cases in Swedish district courts, the lowest level of judicial instance, including information on crime type and the sentence ruled by the court. This data is essentially daily, but since the population registers provide only yearly information we are not able to exploit the high frequency of the conviction data. The data includes all convictions, but also cases where the prosecutor, as opposed to the court, have decided on a summary punishment (typically a fine) instead of a prosecution and cases where the prosecutor have decided not to initiate a prosecution, usually because the suspect is very young. In the following we refer to all these outcomes as convictions. There is one observation per criminal case, but there may be multiple crimes recorded in the same case. However, in each case there is one crime that is indicated to be the main (the most severe) offense on which the punishment is determined. In cases where there are more than one offense we will base the analysis on the main crime.

For around 60 percent of our sample, we can identify at least one parent and thus we can link parental information on education and criminal history to

these individuals. For individuals born outside Sweden and for older individuals, we have no information on parents. Thus, controlling for parental characteristics in the regression will yield a selected sample of relatively young Swedish-born individuals. However, since there is a strong inter generational transmission of the risk of engaging in crime (Hjalmarsson and Lindquist, 2011). controlling for family background could help increase the precision of our estimates.

4.2 Data on program implementation

Data on starting year for the program in each jurisdiction has been gathered by Karin Edmark and Kajsa Hanspers through questionnaires and interviews with local officials. A program is determined to be mandatory only if (1) it has daily or almost daily scheduled activities; (2) it requires participation of all welfare participants that are unemployed (that is, who are seeking financial support primarily for labor market reasons); and (3) participants' attendance is registered, and a failure to attend leads to a risk of being subject to sanctions in the form of reduced or withdrawn benefit payments.

In total, we have information on the existence and starting year of activation programs in 21 districts. Twelve of these are city districts within Stockholm municipality and the remaining 9 are other municipalities in Stockholm county. Thus, we lack data on programs in six out of the 18 districts in Stockholm municipality and most of the other municipalities in the county. The high non-response rate is probably due to the fact that the survey is based on recall, and in some cases it is hard for the responsible officials to provide information that is detailed enough to determine whether the programs can be regarded as mandatory and what year they were implemented. We will use the full sample of 21 districts, in the baseline analysis. As a robustness check, we will limit the sample to the 12 districts within Stockholm municipality, since with the smaller sample we are able to implicitly control for all factors that varies between but not within municipalities.

4.3 Summary statistics

In this section, we present summary statistics of the population covered by our data. We present numbers only for the baseline sample, that is, individuals in the municipalities where we have information on activation programs.

Table 1 shows the distribution of various types of crimes, summarized over the whole time period (1993-2005). The crimes are grouped according to what legal text they refer to. Most serious crimes are gathered in the penal code, while there are separate laws regarding crimes that are typically thought of as less serious, for example crimes related to traffic, waste management, and surveillance. There is also a separate law for crimes related to the selling or posses-

sion of narcotic substances. Among the crimes in the penal code, robbery is by far the most common offense, but there are also many cases of assault, violence to public servant, fraud, and mischief. It should be noted that the numbers reported here are those that appear in the criminal records and relative to actual crimes these are most likely skewed by differences in the propensity of detecting, reporting, and solving different types of crimes.

Table 1: Crimes in Stockholm county, 1993-2005

		Number	Percent of total
Crimes in the penal code		121,566	45.542
of which	Murder	251	0.094
	Involuntary manslaughter	612	0.229
	Assault	18,033	6.756
	Criminal negligence	90	0.034
	Unlawful threat	7658	2.869
	Robbery	52,491	19.664
	Theft, shoplifting	2194	0.822
	Arbitrary conduct	4163	1.560
	Fraud	9261	3.469
	Embezzlement	5188	1.944
	Crime against a creditor	3942	1.477
	Criminal mischief	7281	2.728
	Violence to public servant	10,358	3.880
	Crime against the safety of the Realm	44	0.016
Crime against the penal law on narcotics		20,547	7.697
Any other crime		124,821	46.761
Total		266,934	100.000

In the analysis we present results for all crimes, all crimes in the penal code, and property crime. Property crime includes theft and shoplifting, that is, crimes that are likely to be driven by financial motives rather than aggression. Table 2 below shows the characteristics of individuals found guilty of these different types of crimes, which can be compared to each group's share of the total population (the rightmost column). The share of males is comparatively high in all categories, but less so for property crime. Looking at expressive and violent crimes, the share of male offenders is even higher, above 90 percent in some cases (numbers not in the table). It is also clear that individuals convicted of a crime tends to be younger and less educated than the average. They are also more likely to be born in a non-OECD country.

Since the risk of being welfare dependent and/or being involved in criminal activities is not uniformly distributed across the population, we perform the analysis separately for groups that are more likely to be welfare participants and/or engage in crime. To capture the effect on the group that is targeted by the reforms, we define a group of welfare-prone individuals. Since being on

Table 2: Demographic composition of convicted criminals, 1993-2005

	Any crime	Any crime (penal code)	Property crime	Total population
Male	0.820	0.817	0.630	0.501
Age in years	33.622	33.843	32.793	39.242
Age<26	0.354	0.348	0.396	0.201
Compulsory schooling or less	0.419	0.412	0.489	0.208
Some secondary schooling	0.182	0.189	0.172	0.367
Born outside the OECD	0.187	0.187	0.233	0.104

welfare is endogenous with respect to the reform (as shown by Dahlberg, Johansson, and Mörk (2008) and Persson and Vikman (2010)) and there is likely a strong selection of non-criminals into the programs, we define this group using background characteristics rather than benefit take-up. The group of welfare-prone is defined as single parents, young individuals, those with compulsory schooling or less, and immigrants born outside the OECD. These characteristics are associated with higher welfare participation rates and higher benefit receipt (results not shown, available on request). By focusing on this group rather than welfare participants we are able to capture the effect on the behavior of those individuals that choose to leave benefit take-up when it becomes necessary to take part in activation. We also perform the analysis separately for young males since they are the group with the highest over-representation in criminal registers. Also, previous evidence (Persson and Vikman, 2010), suggests that young individuals respond differently to the reform compared to the overall population in the sense that the reduction in social assistance participation in response to the reform is largely driven by a reduction in the entry rate. That is, the programs make younger individuals less likely to start collecting benefits, rather than helping already benefit-reliant individuals leave welfare. Thus, there is reason to believe that younger social assistance participants might be affected differently by the activation programs also when we are studying criminal behavior. Table 3 shows the average number of convictions per year (in the pre-treatment period) of the types of crimes and demographic groups that we focus on in the analysis. There are clear differences in the incidence of crime across the population. For example, the overall number of convictions is three times higher among young men compared to the average population. The group that we refer to as at risk of receiving benefits also have a higher incidence of all types of crime. The table also reports the share of social assistance participants in each of the groups. This shows that, compared to the average, both the population defined to be at risk of benefit dependence and young males experience a substantially higher probability of receiving social assistance.

Table 4 shows basic summary statistics for the population in the city districts and municipalities that we use in our baseline estimations. It is clear that there

Table 3: Average number of convictions per year and social assistance receipt

	All	At risk of SA	Young males
All crime	0.022	0.035	0.065
	(0.198)	(0.253)	(0.327)
All crime in the penal code	0.020	0.032	0.059
	(0.185)	(0.234)	(0.303)
Property crime	0.004	0.008	0.011
	(0.077)	(0.100)	(0.118)
SA participation	0.071	0.131	0.113
	(0.257)	(0.337)	(0.317)

Note: Standard deviations in parentheses.

are large differences between the districts, both with respect to demographic composition, especially regarding the share of immigrants, and disposable income. The column farthest to the right shows in what year each district implemented the activation programs, that is, in what year treatment starts. For the districts where no year is given we know that no programs were in operation during the relevant time period and thus we use those groups as controls. As a robustness check, we limit the sample to only Stockholm municipality, that is, the top 12 groups in the table.

5 Identification

To estimate the effect of the reform on the individual's propensity to be convicted we use a difference-in-difference approach, exploiting variation within districts over time. The model is specified as a fixed-effect poisson model, to account for the fact that the dependent variables are all concentrated around a few discrete values, with a high proportion of zeros. We prefer this approach over a model where we would construct the dependent variable as a binary indicator for having had any conviction, since using the count data allows us to use more variation in the dependent variable. The number of crimes registered for each individual is assumed to follow a stochastic process where

$$Pr[Y = y] = \frac{e^{-\mu}\mu^{y}}{y!} \qquad y = 0, 1, 2... \tag{1}$$

In the model, the dependent variable *y* varies across individuals (*i*), city districts (*j*), and time (*t*). The distribution depends on the parameter μ_{ijt} according to the log linear function

$$\ln \mu_{ijt} = \lambda_t + \lambda_j + \beta D_{jt} + \gamma \mathbf{x}_{ijt} + trend_j \tag{2}$$

where λ_t and λ_j are fixed effects represented by district and year dummies. $\mathbf{x}$ is a vector of covariates including age in years, dummy variables indicating gen-

Table 4: District characteristics, 1993-2005

	Female	Age	Disposable income	SA	Compulsory schooling or less	Post-secondary schooling	Immigrant born outside the OECD	Immigrant born in the OECD	Parent	Single parent	Start year
Stockholm municipality											
Rinkeby	0.483	35.738	108,289	0.374	0.382	0.293	0.552	0.228	0.479	0.078	1998
Skärholmen	0.516	39.091	145,545	0.154	0.297	0.276	0.275	0.154	0.444	0.078	1999
Farsta	0.502	39.452	162,621	0.117	0.240	0.296	0.110	0.103	0.358	0.065	2001
Kista	0.484	38.103	146,862	0.183	0.246	0.351	0.380	0.147	0.405	0.071	2001
Älvsjö	0.499	39.840	190,228	0.057	0.207	0.370	0.072	0.092	0.468	0.050	2002
Hägersten	0.502	38.722	176,971	0.065	0.187	0.382	0.072	0.105	0.335	0.052	2003
Liljeholmen	0.490	37.445	168,580	0.077	0.171	0.421	0.080	0.119	0.261	0.051	2003
Spånga-Tensta	0.495	38.531	157,422	0.196	0.281	0.336	0.320	0.144	0.486	0.061	2003
Bromma	0.508	38.872	219,073	0.045	0.136	0.503	0.054	0.112	0.382	0.041	2004
Enskede-Årsta	0.505	37.667	175,709	0.070	0.182	0.396	0.089	0.108	0.330	0.057	2004
Hässelby-Vällingby	0.499	39.761	179,889	0.081	0.214	0.341	0.118	0.095	0.430	0.056	2004
Vantör	0.492	38.085	153,972	0.147	0.251	0.281	0.165	0.127	0.354	0.071	2004
Stockholm county											
Sundbyberg	0.496	38.116	171,686	0.068	0.208	0.340	0.119	0.114	0.308	0.059	1997
Botkyrka	0.496	39.085	149,329	0.101	0.305	0.246	0.228	0.153	0.439	0.054	1999
Södertälje	0.488	39.153	159,763	0.100	0.291	0.249	0.128	0.144	0.402	0.051	2000
Upplands-Väsby	0.501	39.364	174,289	0.068	0.243	0.266	0.090	0.125	0.413	0.055	no program
Solna	0.499	38.548	176,347	0.056	0.172	0.424	0.119	0.135	0.286	0.048	no program
Norrtälje	0.487	40.916	162,730	0.054	0.281	0.198	0.024	0.069	0.419	0.048	no program
Salem	0.505	40.025	178,281	0.058	0.229	0.293	0.068	0.097	0.485	0.054	no program
Lidingö	0.512	40.672	243,954	0.026	0.157	0.490	0.057	0.121	0.434	0.042	no program
Värmdö	0.488	40.648	188,808	0.044	0.237	0.275	0.026	0.101	0.475	0.051	no program

der, educational level, and being a parent, as well as indicators for country of birth (outside the OECD and inside the OECD, excluding Sweden). D_{jt} indicates treatment and is constructed as a dummy variable that is equal to one if a mandatory activation program has been implemented in district *j* at time *t*. To account for different time trends in each district, we also control for district-specific linear trends.

The coefficients estimated from equation 2 can be recalculated to represent marginal effects, and since the treatment variable D is binary we write this as

$$ME_D = E(y|\mathbf{x}^*, D = 1) - E(y|\mathbf{x}^*, D = 0) \tag{3}$$

where $\mathbf{x}^*$ indicates all covariates other than the treatment variable D. In the tables, we will present both coefficients and the average marginal effects (AME). The marginal effects are calculated using the margins command in STATA, applying the delta method.

This specification does not account for the fact that the variation occurs at the district level, while the data is at the individual level. Thus, if there are city-district specific characteristics that vary over time, observations might not be independent within districts, which could lead to a bias of the standard errors associated with specification 2. We account for this by using cluster-robust standard errors and thereby allowing the standard errors to be arbitrarily correlated among units within each group.[3].

6 Results

In this section, we present results from the baseline estimation as well as various robustness checks and the heterogeneity analysis. Initially, we base the analysis on the districts in Stockholm municipality and other municipalities in Stockholm county for which we have information on the prevalence and starting year of activation programs. This gives us 21 groups of which six are untreated

[3] This is potentially important, but since we have a limited number of clusters (21 in the baseline sample and 12 in the restricted sample) one should be careful in interpreting the significance levels, at least in the smaller sample, due to the poor small-sample properties of clustered standard errors. We also estimate the model using block-bootstrap, which shares the small sample properties of the clustered standard errors (Cameron, Gelbach, and Miller, 2008). Not surprisingly, the confidence intervals from the block bootstrap are very similar to the ones where we cluster the standard errors. Methods commonly suggested to account for non-independent observations when the number of clusters is small, such as the wild bootstrap approach (Cameron, Gelbach, and Miller, 2008) or the two-step method (Donald and Lang, 2007) are only valid for linear models, and thus not applicable in this setting. As a robustness check, we have also estimate a linear model, which yields qualitatively similar results, but the standard errors are larger both when calculated using conventional methods and the wild bootstrap approach. As we show in the results section, neither the results nor the significance levels change much when reducing the number of groups, which indicates that the relatively small number of clusters is not a major concern.

throughout the whole time period, while in the remaining 15 treatment is implemented at some point in time between 1997 and 2004. Later we limit the sample to only the 12 treated districts in Stockholm municipality where the information on the various reforms is more detailed, see section 6.2.

6.1 Baseline results

Tables 5, 6, and 7 show results for the full sample, that is, all district and municipalities where we have information on the prevalence of activation programs. We present results for the aggregate population, the fraction of the population for which we can identify the parents (note that this is a selection of mostly younger, Swedish born individuals), those with characteristics that makes them more likely to be benefit dependent (*at risk of SA*, defined as belonging to one or more of the following groups: individuals younger than 26 years old, single parents, individuals with compulsory schooling or less, immigrants born outside the OECD), and males younger than 26 years old. The tables are separated by type of crime, the dependent variable is the total number of crimes, total number of crimes according to the penal code, and total number of property crimes, respectively.

Looking at the results for the probability of being found guilty of any crime, Table 5, the implementation of mandatory activation programs is estimated to have a positive effect on crime. That is, all else equal, individuals living in a district where social assistance is conditioned on taking part in labor market programs commit more crimes than those living in districts where there are no such requirements. The estimate for the aggregate population (column 1) is not sensitive to adding controls for parent's criminal history (pre-reform convictions, column 2).[4] The effect also remains, although the point estimate is slightly smaller, when studying only the group that is at risk of depending on social assistance. The effect is also stronger for the sample of young males compared to aggregate population. This is not surprising since younger men are generally involved in crime to a greater extent than the average population. The precision of the estimates becomes somewhat weaker when allowing the standard errors to be arbitrarily correlated among individuals within the same districts. However, with clustered standard errors the estimates are still statistically significant at conventional levels.

To give an indication of the size of the effect and the economic significance we calculate average marginal effects, presented at the bottom of the table (*AME*). The effect is small in economic terms; for the aggregate population the average number of crimes increases by 0.001, that is from around 0.022 to 0.023, almost

[4] Some, but not all, of the difference that arises when controlling for parental crime is due to the selected sample rather than an effect of adding the control variable. We also estimate this model controlling for parental education rather than crime, which yields similar results.

a five percent increase (the reference levels are presented in Table 3). For the group of young males the (nominal) effect is four times as large, but increases from a higher level and in real terms the effect does not differ much between the groups. The effects are small also compared to other papers studying determinants of crime. For example, as estimated by Hjalmarsson and Lindquist (2011), having a criminal father increases the risk of having a criminal conviction by 32 percent for sons and 53 percent for daughters[5].

Table 5: Total number of crimes

	All	All	At risk of SA	Young males
Treatment	0.037	0.064	0.029	0.058
	(0.011)***	(0.012)***	(0.013)**	(0.022)***
	[0.021]*	[0.024]***	[0.015]*	[0.031]*
Controls	Yes	Yes	Yes	Yes
Controls (parental)	No	Yes	No	No
AME	0.001	0.002	0.001	0.005
	(0.000)***	(0.000)***	(0.001)**	(0.002)***
	[0.001]*	[0.001]**	[0.001]**	[0.002]*
N	6,803,606	5,270,327	3,077,843	620,120

Note: All estimations include year dummies, city district dummies, and city district linear trends. Standard errors in parenthesis. Clustered standard errors in brackets.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$

The results are very similar when restricting the dependent variable to only a count of the number of crimes stated in the penal code, thus excluding most minor crime. These results, presented in Table 6, indicates that the results in the previous table are not driven by petty crimes with small economic significance, and that the reform may actually have an impact on more severe crimes and thus also have important implications for the cost-benefit analysis of the programs. However, the precision is somewhat weaker in these estimations, probably due to a lower incidence of these types of crimes, resulting in less variation in the dependent variable.

When limiting the sample further to only look at how the activation programs affected property crime, we find no significant effect. Thus, there is no evidence that crimes with a clear financial motivation are affected more than other types of crimes, although theoretical predictions often state the hypothesis that illegal income is used as a supplement to welfare income, see for example Foley (2011). The point estimate is largest for young males, although imprecisely estimated, which is what we would expect since young individuals

[5]The estimates in this paper are also much smaller than the estimated effect on crime of liberalized alcohol sales (Grönqvist and Niknami, 2011) and unemployment (Grönqvist, 2011). These factors have been estimated to increase the risk of being convicted by 18.7 percent (alcohol sales) and 22 percent (long-term unemployment).

Table 6: Number of crimes in the penal code

	All	All	At risk of SA	Young males
Treatment	0.039	0.065	0.028	0.060
	(0.012)***	(0.014)***	(0.014)**	(0.023)***
	[0.023]	[0.028]**	[0.019]*	[0.037]
Controls	Yes	Yes	Yes	Yes
Controls (parental)	No	Yes	No	No
AME	0.001	0.002	0.001	0.004
	(0.001)***	(0.001)***	(0.001)**	(0.002)**
	[0.001]	[0.001]**	[0.001]	[0.003]
N	6,803,606	5,270,327	3,077,843	620,120

Note: All estimations include year dummies, city district dummies, and city district linear trends. Standard errors in parenthesis. Clustered standard errors in brackets.
* p<0.1, ** p<0.05, *** p<0.01

experience a higher risk of disqualifying for benefits (or chose not to apply) after the introduction of the programs, and therefore also be more in need of an alternative source of income. Since there are other crimes that can also have a large financial component, one could argue that the definition of property crime is too narrow (including only theft and shoplifting). To account for the potential economic motives behind robbery, which is usually regarded as violent crime rather than property crime, we also perform the analysis on robbery and theft together. The estimates are qualitatively similar and none of them are statistically significant.

Table 7: Number of property crimes

	All	All	At risk of SA	Young males
Treatment	−0.017	0.018	−0.021	0.043
	(0.025)	(0.030)	(0.029)	(0.056)
	[0.051]	[0.050]	[0.039]	[0.060]
Controls	Yes	Yes	Yes	Yes
Controls (parental)	No	Yes	No	No
AME	0.000	0.000	−0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.001)
	[0.000]	[0.000]	[0.000]	[0.001]
N	6,803,606	5,270,327	1,283,019	620,120

Note: All estimations include year dummies, city district dummies, and city district linear trends. Standard errors in parenthesis. Clustered standard errors in brackets.
* p<0.1, ** p<0.05, *** p<0.01

We also test the effect on violent crime, criminal mischief, and crime against

the penal law on narcotics without finding significant effects. It is likely that the precision is reduced due to too little variation in the dependent variable when the groups become too narrowly defined. However, we do find a positive effect on the number of prison sentences (results not presented), which confirms that the estimated effect is not driven only by the least serious crimes.

We have also separated the dependent variables into the number of crimes committed on weekdays (Monday through Friday) and weekends (Saturday and Sunday), respectively. The results, using the total number of crimes as the dependent variable, are presented in Table 8 and Table 9. The estimates are similar, indicating that the reform does not affect behavior on weekdays differently compared to weekends. These results indicate that the estimated increase in crime is driven by those that did not take part in the activation. For this group the reform had no effect on the amount of leisure (that is, time available for criminal activities). Rather, the reform would lead to a decrease in disposable income, and if income (or frustration due to a lack of income) is what motivates crime we would expect the effect to be constant throughout the week.

Table 8: Total number of crimes committed on weekdays

	All	All	At risk of SA	Young males
Treatment	0.035	0.068	0.027	0.058
	(0.013)***	(0.015)***	(0.016)*	(0.026)**
	[0.022]	[0.026]***	[0.018]	[0.036]
Controls	Yes	Yes	Yes	Yes
Controls (parental)	No	Yes	No	No
AME	0.001	0.001	0.001	0.003
	(0.000)***	(0.000)***	(0.001)*	(0.001)**
	[0.000]	[0.001]**	[0.001]	[0.002]
N	6,803,606	5,270,327	3,077,843	620,120

Note: All estimations include year dummies, city district dummies, and city district linear trends. Standard errors in parenthesis. Clustered standard errors in brackets.
* p<0.1, ** p<0.05, *** p<0.01

6.2 Robustness checks

To establish that the estimates presented above are really a causal effect and not a measure of some spurious correlation, we test the robustness of the results in a number of ways. To test the parallel trend assumption that we need to justify the difference-in-difference approach we first create a placebo experiment where we move the year that treatment starts three years back in time and only use data for the years that no district had really implemented the programs. The results of this pseudo treatment are presented in Table 10, where the dependent

Table 9: Total number of crimes committed on weekends

	All	All	At risk of SA	Young males
Treatment	0.050	0.055	0.040	0.068
	(0.022)**	(0.024)**	(0.026)	(0.039)*
	[0.026]*	[0.027]**	[0.027]	[0.053]
Controls	Yes	Yes	Yes	Yes
Controls (parental)	No	Yes	No	No
AME	0.000	0.000	0.000	0.002
	(0.000)**	(0.000)**	(0.000)	(0.001)*
	[0.000]*	[0.000]**	[0.000]	[0.001]
N	6,803,606	5,270,327	3,077,843	620,120

Note: All estimations include year dummies, city district dummies, and city district linear trends. Standard errors in parenthesis. Clustered standard errors in brackets.
* p<0.1, ** p<0.05, *** p<0.01

variable is the total number of crimes per individual. None of the estimates are statistically significant and they are all smaller than the main estimates (some changes sign). This shows that there are no pre-reform (non-linear) trends that differ between districts that implemented the programs earlier compared to those that implemented them later, which strengthens the argument that the estimates above can be interpreted as a causal effect. The same analysis is done for the two other outcomes, with similar results (available on request).

Table 10: Total number of crimes, placebo treatment

	All	All	At risk of SA	Young males
Treatment	−0.001	−0.002	−0.018	0.023
	(0.022)	(0.025)	(0.025)	(0.041)
	[0.020]	[0.038]	[0.031]	[0.058]
Controls	Yes	Yes	Yes	Yes
Controls (parental)	No	Yes	No	No
AME	0.000	0.000	−0.001	0.002
	(0.001)	(0.001)	(0.001)	(0.003)
	[0.001]	[0.001]	[0.001]	[0.005]
N	2,836,595	2,126,814	1,328,569	266,184

Note: All estimations include year dummies, city district dummies, and city district linear trends. Standard errors in parenthesis. Clustered standard errors in brackets.
* p<0.1, ** p<0.05, *** p<0.01

To be able to control for factors that vary at the municipality level, such as the social assistance benefit level and taxes, we limit the sample to only the districts within Stockholm municipality. This also has the advantage that we have a somewhat better understanding of the content and labor market effect of these programs (Dahlberg, Johansson, and Mörk, 2008; Persson and Vikman, 2010).

Except for the group that we refer to as at risk of receiving social assistance, all results from the main analysis carry over to this restricted sample.

Table 11: Total number of crimes, Stockholm municipality

	All	All	At risk of SA	Young males
Treatment	0.043	0.069	0.019	0.073
	(0.015)***	(0.017)***	(0.018)	(0.029)**
	[0.019]**	[0.030]**	[0.018]	[0.045]*
Controls	Yes	Yes	Yes	Yes
Controls (parental)	No	Yes	No	No
AME	0.001	0.002	0.001	0.006
	(0.000)***	(0.001)***	(0.001)	(0.002)**
	[0.000]**	[0.001]**	[0.001]	[0.004]
N	3,374,556	2,563,037	1,538,587	304,104

Note: All estimations include year dummies, city district dummies, and city district linear trends. Standard errors in parenthesis. Clustered standard errors in brackets.
* p<0.1, ** p<0.05, *** p<0.01

Table 12: Number of crimes in the penal code, Stockholm municipality

	All	All	At risk of SA	Young males
Treatment	0.044	0.072	0.018	0.084
	(0.016)***	(0.019)***	(0.019)	(0.031)***
	[0.022]**	[0.034]**	[0.020]	[0.049]*
Controls	Yes	Yes	Yes	Yes
Controls (parental)	No	Yes	No	No
AME	0.001	0.002	0.001	0.006
	(0.000)***	(0.000)***	(0.001)	(0.002)***
	[0.001]**	[0.001]**	[0.001]	[0.004]*
N	3,374,556	2,563,037	1,538,587	304,104

Note: All estimations include year dummies, city district dummies, and city district linear trends. Standard errors in parenthesis. Clustered standard errors in brackets.
* p<0.1, ** p<0.05, *** p<0.01

To further test the robustness of our baseline results, we also perform the analysis on a sample of only males as well as only males younger than 40. Since females and older individuals are substantially less likely to be involved in most types of crimes including these groups in the analysis probably attenuate the results. When excluding these individuals the point estimates becomes slightly larger and the precision increases marginally. Since "young males" is a rather broadly defined group, and crime is concentrated among young males with low education, we also estimate the model on a sample of only young males with only compulsory schooling. The results are qualitatively similar to the ones for the whole group of young males, but the standard errors are larger and none

Table 13: Number of property crimes, Stockholm municipality

	All	All	At risk of SA	Young males
Treatment	−0.001	0.040	−0.043	0.082
	(0.033)	(0.040)	(0.038)	(0.074)
	[0.033]	[0.054]	[0.039]	[0.076]
Controls	Yes	Yes	Yes	Yes
Controls (parental)	No	Yes	No	No
AME	0.000	0.000	0.000	0.001
	(0.000)	(0.000)	(0.000)	(0.001)
	[0.000]	[0.001]	[0.001]	[0.001]
N	3,374,556	2,563,037	1,538,587	304,104

Note: All estimations include year dummies, city district dummies, and city district linear trends. Standard errors in parenthesis. Clustered standard errors in brackets.
* p<0.1, ** p<0.05, *** p<0.01

of the results are statistically significant (probably because the sample size is reduced considerably).

All results are also robust to changing the specification in equation 2 and estimating a model without linear trends. The placebo estimation shows that overall the district follow a similar trend with respect to the outcome variable, the only exception is violent crime which increases more rapidly in some districts than in others.

7 Conclusions

In this paper, we analyze the effect of implementing mandatory activation programs for unemployed social assistance participants on the individual probability of being found guilty of a crime. This issue is of great interest since financially disadvantaged individuals are heavily over represented in criminal registers. Thus, programs that target these individuals may have an effect not only on labor market outcomes and income, but also on criminal behavior. Since there are a number of mechanisms that can give rise to this relationship, the effect may be either positive or negative. If the effect arises due to that the amount of leisure decreases when unemployed social assistance participants are required to attend the job center, the number of crimes should be reduced. However, it is not obvious that all individuals that are targeted by the reform actually take part in the activation program. In fact, a significant fraction of individuals, especially among the young, that would have been eligible for support fail to meet the requirements or choose not to apply for benefits when they are conditioned on activation. Instead, these individuals end up with less income and the same amount of leisure as when the benefits were unconditioned.

Our results indicate that, on average, the number of convictions per individual increases as a result of the reform, but we have not been able to isolate the mechanism behind this result. We find some evidence (although not statistically significant) that the effect is larger for young males. It is thus possible that the effect is driven by an increase in the number of young individuals that were discouraged from applying for social assistance when benefits became conditioned on activation. Hence, these individuals probably have very poorer financial resources and perhaps experience more frustration than what would have been the case if there were no activation requirements. The fact that the effect does not differ for crimes committed on weekdays relative to weekends also indicates that the increase is driven by individuals who are not enrolled in the programs, and whose leisure time during the week is not constrained by the schedule at the activation center.

To test the stability of our results, we also limit the geographical region of the study and look at effects only of the programs that were implemented in the city districts within Stockholm municipality. By doing this we can implicitly control for factors related to the social assistance system that varies at the municipality level, such as political composition, financing, and the benefit level. The results from this smaller sample are only marginally different from the baseline estimates, which strengthens the results of the main analysis.

The size and significance of the results should be interpreted with caution since different inference methods yields somewhat different conclusions. Nevertheless, it is reassuring that the sign of the estimates are consistent across specifications and sub-populations.

We conclude that the introduction of mandatory activation programs for unemployed social assistance participants in Stockholm county did not have a mitigating effect on crime, but seems to have caused a small increase in criminal activity. It is important to take this into account when evaluating the costs and benefits of these and similar programs, since it indicates that the programs may have unintended effects on individual behavior as well as unpredicted costs for interventions by the police and the judicial system. However, these effects are probably to a great extent determined by the content of the programs and how they are perceived by the participants, which might explain why the results in this study differ from evaluations of other, similar programs. If the programs are attractive enough, and perceived as an investment in human capital, they may have less adverse effects on criminal behavior compared to the case where the programs appear mostly as a means to test the participants' willingness to work.

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Essay 3

Earnings, Income, and Poverty Among Welfare Leavers in Sweden

1 Introduction

The trend towards more active policies on reducing the take-up of welfare benefits has raised the issue of how individuals are faring, financially and with respect to health outcomes, after leaving welfare. Some researchers have expressed concern that those that end welfare participation without finding employment might suffer a reduction in disposable income and remain in poverty. Also, it has been shown that a high fraction of welfare leavers return to benefit receipt within a few years. Thus, it is not obvious that reducing welfare participation reduces poverty and increases well-being, neither in the short nor in the long run. In this paper, we study the income of welfare leavers, and how this is influenced by the type of labor market outcome the individual moves to. We also look at the risk of returning to welfare, and how this risk varies with post-welfare outcomes.

In most Western countries, financial support to poor individuals and families, often through cash benefits, has been the dominant strategy to fight (post-transfer) poverty. During the past two decades however, increasing costs and a shift in political preferences has led to more focus being put on encouraging employment and reducing dependence on social benefits, such as welfare benefits and food stamps in the US, and social assistance in Sweden[1]. To this end, various reforms have been implemented to strengthen the incentives for welfare participants to move from welfare to work, and to make this transition easier. The main goal of such policies is twofold and involves both reducing public expenditures and, perhaps most importantly, increasing the well-being of those taking part in the system. Throughout Europe and the United States different methods are used to social assistance less attractive. This can be done either by setting the benefit level at a low enough level so that it is always below the lowest wage in the economy, or through social control by requiring the recipient to work for benefits. As shown in Lødemel and Trickey (2001), states tend to combine these two schemes. The perceived need for social control seems to be stronger when the social assistance system is generous, than when benefits are low. Over all, there has been a trend towards extending the compulsory work requirements of welfare participants and away from the view of poverty relief as a right for all citizens.

This trend has been particularly strong in the US, where a series of reforms transformed the welfare system during the 1990s. Evaluations of the situation of single mothers leaving welfare in the US after these reforms suggest that they are faring quite well, assuming that they receive the tax deductions and allowances they are entitled to while working (Blank and Haskins, 2001). Danziger et al. (2002) find that moving from welfare to work after the US wel-

[1]Throughout this paper, welfare benefits and social assistance (American and Swedish terms, respectively) will be used interchangeably.

fare reform was associated with increased income and reduced material hardships. However, welfare reform might have contributed to an increase in the number of families leaving welfare without starting to work, so called "disconnected leavers". According to Blank and Kovac (2008), these families are very poor, and tend to cycle between benefit participation and "disconnectedness" without entering the labor market. Harris (1996) finds high rates of re-entry into welfare among single mothers in the US. She also shows that exits to work are not more likely to permanently end welfare dependence than are other exit routes, for example marriage or the aging of children. Moreover, she finds that, for those who exit welfare to work, and remain off welfare, wage rates do not increase during the following three years and welfare leavers often remain poor. Danziger et al. (2000) shows that while the fraction of welfare participants fell by 22.6 percent in the US between 1995 and 1997, the number of female headed households whose pre-transfer income was below the poverty line declined by only 5.4 percent. Thus, it is clear that reducing welfare participation is not equivalent to increasing income and reducing poverty. Some studies have also found that the advantages of work over welfare are not uniformly distributed over the population, and that single-parent households benefit less from moving from welfare to work than do other types of households (Bauman, 2002; Scott et al., 2004).

Looking at welfare leavers in Sweden is interesting in several ways, not least because of the special features of the welfare state, and the unusually rich register data on incomes and transfers. Most previous studies have been done in Anglo-Saxon countries whose welfare systems differ substantially from those in the Nordic countries. For example, Swedish minimum wages are high and the problem of working poor, often discussed in the US setting, is not large in Sweden (SOU 2004:5). We would thus expect that if welfare leavers are able to find employment, they would not remain (or become) poor. The problem is rather, as we will demonstrate, that only very few welfare leavers are able to find full-time work when ending benefit take-up.

In comparison to most countries outside of Scandinavia, the social insurance system in Sweden is generous, with high replacement rates (for long durations) and a high take-up rate. However, this generosity does not extend to the part of the population without labor market experience, that is, those who do not have enough previous labor income to qualify for benefits. Instead, this group must rely on means-tested benefits from the local welfare office. This distinction gives rise to what has been referred to as the *welfare paradox*, where a high standard of living is guaranteed for some (the working) individuals while others are guaranteed only a minimum income. In this respect, the social assistance system resembles the old poor relief, which might contribute to the high stigma that is associated with benefit take up.

The paradox of the welfare system has become more pronounced over time,

as the fraction of unemployed individuals who do not qualify for the social insurance schemes has increased substantially since the early 1990s. For example, the increasing number of unemployed immigrants and young individuals who have no insured income that would entitle them to unemployment insurance have contributed to an increase in the fraction of unemployed individuals who have to rely on social assistance. At the same time, it has also become harder to gain eligibility for other financial support programs, such as housing allowances (Turner and Whitehead, 2002). Moreover, since the mid-1990s it has also become more and more common to condition social assistance eligibility on participation in workfare programs, often referred to as activation programs in the Swedish setting, which typically consists of supervised job searching or low-paid public employment. This development can be seen as a fundamental shift in Swedish welfare policy as it implies that social assistance, which has been the ultimate financial safety net in society, is no longer unconditionally available to all individuals in need of financial support. It is also the case that while the social insurance system has been thoroughly evaluated, little is still known about the social assistance system and it's participants.

Additionally, using Swedish data gives us access to a rich set of register information on the whole population over a long time period, 19 years. Thus, we do not have a problem with high non-response rates and difficulties with tracking individuals, a problem that often arises when relying on survey data. We are able to follow individuals over time and capture long-term changes in financial variables as well as study return to welfare participation and multiple exits from benefit take-up. Previous research on welfare dependence and poverty has often focused on single transitions, and re-entry has often been ignored. As pointed out by Stevens (1999) and Hansen and Wahlberg (2004) this may cause a significant underestimation of the number of years in poverty. Stevens (1999) show that within four years half of the individuals who leave poverty have returned to it. The estimated re-entry rate is lower for Sweden, Socialstyrelsen (2010) shows that half of the poverty-exiting population in 1991 had returned within eight years.

Swedish data provides rich information on incomes and transfers. Our main interest is in the receipt of social assistance benefits and disposable income. Disposable income serves as a measure of post-transfer income and consumption possibilities, and we thus measure financial well-being rather than looking at health or consumption. In our data, household disposable income is calculated by Statistics Sweden using register data on the full set of household income. Individual disposable income is calculated using an equivalence scale set by the National Board of Health and Welfare. We can thus avoid the measurement errors that commonly arise when using self-reported information. Following sociological literature, we define an indicator variable of labor market attachment measuring how closely linked the individual is to the labor market and

to what extent he or she can support him- or herself through labor work. This variable indicates the main source of support and labor market participation using the categories *strong* and *weak labor market attachment*, *recipient of sickness insurance benefits* or *unemployment insurance benefits*, *student*, or *other/unknown*.[2] We use these categories to study to what extent the outcome state determines the financial situation of leavers. We also have data on employment and labor earnings, to further asses labor market attachment. Financial well-being is often discussed in terms of poverty, and thus we also look at the poverty rate, defined as having an (equivalized) disposable income less than 60 percent of the median.

We find that leaving welfare with a strong labor market attachment is the only alternative that yields an economically significant increase in income and provides some insurance against repeated dependence on welfare benefits. Other outcomes are associated with lower income and higher poverty rates. Also, the risk of returning to welfare is higher, particularly among disconnected leavers. The association between a strong attachment to the labor market and financial well-being is similar for individuals leaving welfare during different states of the business cycle, as well as for different groups within the population.

In the following chapters, we will first review the institutional setting and the structure of the Swedish social assistance system. Thereafter, we describe the data and variable definitions before discussing the methods used for estimation. We then present the results, starting with an overview of welfare leavers at different states of the business cycle. We then move on to analyzing where in the labor market welfare leavers are more likely to end up, and how these initial labor market outcomes affect disposable income, poverty, and repeated welfare dependence. We then look specifically at what we refer to as disconnected leavers, who leave welfare without any attachment to either the labor market or the social insurance system. Lastly, we discuss the results and conclude the paper.

2 Institutional setting

The Swedish welfare state entails an extensive system of social services, including child care, elderly care, and support for disabled individuals. A great part of public expenditure, however, is devoted to income replacement programs such as unemployment and sick leave insurance. Social insurance is not means tested but relies on actual individual characteristics such as unemployment or established illness. To qualify for these benefits, one has to have previous work experience and sufficient previous income (*sjukpenninggrundande inkomst*). Also,

[2] These categories were originally defined in Bergmark and Bäckman (2007) and have (with slight modification) been used in various reports from the National Board of Health and Welfare, see for example Socialstyrelsen (2006).

to be eligible for unemployment insurance one has to be a member of an unemployment benefit fund, and for sickness insurance it is necessary to have a medical note certifying that one is unable to work. Individuals who do not qualify for social insurance might be eligible for social assistance benefits, a financial support system initially intended to provide temporary financial support for households in need.

Unlike the income-related social insurance benefit system, which is administered at the national level, social assistance in Sweden is a local responsibility, administered and financed at the lowest level of government, the municipalities. Thus, there is a large degree of municipal discretion, although national legislation constitutes the main principles of the social assistance system and sets a minimum benefit level. This law states that all individuals permanently residing in Sweden should have a "reasonable" standard of living. Eligibility for social assistance is universal in the sense that all individuals may become eligible for benefits, unlike in the US where welfare benefits are only available for families with children, primarily single mothers. Nevertheless, benefit eligibility is subject to strict means testing, and an individual can only become eligible when all other means of support and all other parts of the social security system have been exhausted. Moreover, eligibility for social assistance is determined at the household level, which means that if there are two adults in the household, they must both exhaust all other alternative support before qualifying for benefits. Due to this, social assistance is commonly referred to as the last piece of the social safety net. A large fraction of the welfare participants in Sweden are unemployed individuals that do not meet the eligibility criteria for unemployment insurance. This group has been growing over time and constitutes around 40 percent of the total welfare caseload in 2010 (SCB, 2010). The increase started during the recession in the early 1990s when unemployment, especially among young individuals without labor market experience, increased dramatically and thus the number of unemployed in need of welfare benefits also rose. This caused a large increase in municipal expenditures on social assistance, and many local governments faced difficulties in financing the system.

In response to the heavier financial burden, some municipalities started to require unemployed welfare participants to take part in mandatory activation programs in order to retain eligibility for benefits. In 1998, the Social Services Act was changed to explicitly allow for these kinds of work requirements, and since then the number of municipalities implementing mandatory work-related activities has increased. There is now a large diversity of programs at the local level, and the degree of activation differs substantially between municipalities. On average, about 12 percent of all social assistance participants of working age took part in an activation program during 2002 (Salonen and Ulmestig, 2004), but this number varies greatly across municipalities and has grown substantially

over time. This trend towards more active labor market policies for welfare participants as well as other unemployed individuals is not unique to Sweden, but has been shared among most Western states, led by the US and the UK. In all these countries the political consensus on the benefits from so-called "welfare-to-work" programs has grown during the past decades and these policies are now common throughout Europe and the US.

There is only limited knowledge about the structure and efficiency of these municipal activation programs. One exception is the municipality of Stockholm, where activation requirements for unemployed social assistance participants were implemented sequentially between 1998 and 2004. These programs are relatively well-documented, which has made it possible to study them in greater detail, see Dahlberg, Johansson, and Mörk (2008), Persson and Vikman (2010) and Persson (2013).

3 Data

3.1 Data registers and variable definitions

The data used in this analysis is gathered from the IFAU database and contains variables from the income and employment registers, which are administered by Statistics Sweden. The data covers all individuals of working age permanently living in Sweden. Data is collected annually and contains information on individual characteristics such as year of birth, marital status, and number of children in the household. Also, there is register information about income sources, income levels, earnings, and employment.[3] The advantage of using registers rather than survey data is that it minimizes the problem of measurement errors that often arise when individuals self-assess income and benefit levels. Disposable income and social assistance benefits are defined at the household level and are individualized using an algorithm determined by the National Board of Health and Welfare. This takes into account the number of adults and children in the household, assuming all household members share the same living standard, and hence these measures are equivalent for individuals and households. All income variables are in real terms, with 1997 being the base year.

Disposable income will be used to measure individual and household post-transfer income. One important aspect is that if social assistance (and income transfer programs in general) is successful in fighting poverty there should be a large difference in pre- and post-transfer income. Moreover, if social assistance constitutes an important part of disposable income, a reduction in benefit take-up might reduce disposable income even though labor income increases.

[3] Employment is measured by a dummy variable that takes the value one if the individual was employed in November the given year.

Poverty is defined as having a disposable income below 60 percent of the median disposable income. This is a conventional threshold when measuring relative poverty in the European Union, since low income is taken as an indication of being at risk of social exclusion and inability to participate in society.

In the data, "families" or "households" are constructed by joining together individuals who are registered as living at the same address, starting with the oldest person, and consisting of not more than two generations. Unmarried adults living together is only identified as a family if they have common children, if not they will be identified as two single individuals. These definitions are set by Statistics Sweden, and constitutes a limitation in the data since it is quite common for couples without children to live together without being married, and thus the number of actual "families" is likely to be underestimated. This might be problematic since we cannot observe all couples with an obligation to financially support one another according to the Social Services Act. Using a household measure of disposable income, or performing the analysis on a household head representing the whole family would not solve this issue since the household variables are also constructed based on the limited definition of a household. Also, we cannot accurately observe the number of family members, and thus it is not possible to weight the analysis by household size.[4]

We exclude immigrants during their first two years after arriving in Sweden, since they might be eligible for "introduction benefits" during the first 18 months, while participating in introductory language courses or some other labor market introduction. These benefits are very similar to social assistance benefits, but the application process is typically simpler. We also exclude individuals younger than 18 years old, and older than 64, since we are primarily interested in those individuals that participate in the labor force.

Labor market attachment and social assistance participation

Following various works by Bergmark and co-authors (for example Bergmark and Bäckman, 2007) status of support is defined according to main source of income, and income stability. A slightly different version of this model has been used in studies by the National Board of Health and Welfare, analyzing connections to the labor market and crowding-out effects. The definitions given by the model generates seven different categories, presented in Table 1, that indicate how strong the individual's attachment to the labor market is. The categories are constructed to be mutually exclusive within a given year. The basic amount or, more accurately, the "price basic amount" (*prisbasbelopp*), is calculated by Statistic Sweden based on changes in the general price level. In 1992,

[4]However, the analysis have also been carried out on a sample of household heads, where we sample only one individual per household (the one with the highest earnings). All results are qualitatively similar to those from the individual level analysis.

the basic amount was SEK 33,700 (around USD 4,800) and SEK 36,400 in 2005. The definitions are constructed at the individual level, and will be used to determine individual, rather than household, labor market status. Having a strong labor market attachment can imply full-time work all year with low pay, or working some months with a higher pay. The threshold of earning 3.5 times the basic amount of labor income is chosen to represent the amount of labor income that is necessary for financial self-sufficiency during a year. The "other" category will include those who combine different types of income in a way that does not fit in any of the other categories. We will consider this in greater detail when we look at those leaving welfare for unknown outcomes. It is also important to note that the unemployed and those on sick leave are defined based on income, so that individuals that are unemployed or sick without receiving monetary compensation from unemployment and sick leave insurance, respectively, are not included in these categories. Being a social assistance recipient is defined as receiving more than half of the basic amount of welfare benefits in a given year. This definition differs from ones made in earlier works, where it is defined as receiving welfare during at least six months during a year. Since we do not have access to monthly information, we cannot use this definition.

Table 1: Labor market attachment

Category	Description
Strong labor market att	Labor earnings of 3.5 times the basic amount, less than 1/4 of total earnings from sickness benefits, less than 1 basic amount in pension benefits, and no unemployment benefits
Weak labor market att	Labor earnings of at least 1/2 but less than 3.5 times the basic amount, otherwise as core labor force
Sick leave benefit and disability pension	Sickness benefit of at least 1/4 of labor income, or disability pension benefit of at least 1 basic amount
Unemployed	Income from unemployment insurance
Student	Income from study grant/loan of at least 1 basic amount, less than 1/2 the basic amount in labor income
SA recipient	More than 1/2 the basic amount in social assistance benefits
Other	Belonging to none of the above categories and having less than 1/2 the basic amount in labor income

The definition of welfare participation is important in determining what population that is included in the analysis. As shown by Dahl and Lorentzen (2003) the study design, what sample is chosen and the definition of welfare spells to a large extent determines the results and what inference we can draw from them.

To show the heterogeneity with respect to type of spell, Figure 1 shows the Kaplan-Meier survival estimates for spells of participation and non-participation in social assistance using different definitions. The duration a non-welfare spell corresponds to one minus the risk of returning to welfare. Definition 1 represents the case in which an individual is defined as being on welfare if receiving some non-zero amount of benefits during a year and being off welfare if not receiving any benefits. This is a very broad definition and it will capture a very heterogeneous population. Definition 2 represents the threshold presented in Table 1. According to definition 3 an individual is on welfare if social assistance makes up at least 50 percent of disposable income, and off welfare if it is less than 10 percent. Here, the definitions are set to be mutually exclusive, so that definition 1 is any welfare benefit larger than zero, but less than one basic amount and 50 percent of disposable income, and similarly for the other definitions. Around 75 percent of welfare spells thus belong to definition 1, while the remaining spells are divided equally between the remaining categories.

As indicated by the figure, the hazard of leaving a spell of welfare participation is higher for those who receive only small amounts of benefits, more than half of these leave welfare within a year. After ten years, around five percent are still on welfare, according to definition 1 while 10 and 15 percent remain according to definition 2 and 3, respectively. Individuals receiving low amounts of benefits also experience a higher risk of returning to receiving benefits (the solid line). That is, the duration of a non-welfare spell is lower, around 45 percent have not returned to welfare return within 10 years. For definition two and three, the non-return rate after ten years is around 60 percent (they differ very little). This indicates that spells with small amounts of benefits tend to be short, but repeated. Excluding those who receive very small amounts of benefits, by using definition 2 or 3, makes a big difference for the estimated duration of benefit receipt, especially for the risk of returning to welfare. However, there is only a small difference between the last two definitions.

This paper will use the first two of these definitions. We use the first category since it includes all welfare participants and can be seen as representing the average welfare participant. The majority of welfare participants in this group receive only small amounts of benefits, or only for one or two months in a given year. When using this definition, we modify the categories in Table 1 to be mutually exclusive, so that each indicator takes the value zero for all years when the individuals receive any benefits. Clearly, this group is very diverse and to obtain results for the smaller group that are somewhat dependent on welfare we also use definition 2, in which welfare participation is defined as receiving more than half a basic amount of benefits during the year. This is an arbitrary definition, but it has the advantage of including a relatively heterogeneous population, while excluding those who receive very small amounts and who are unlikely to *depend* on social assistance for their livelihoods. Also, it will

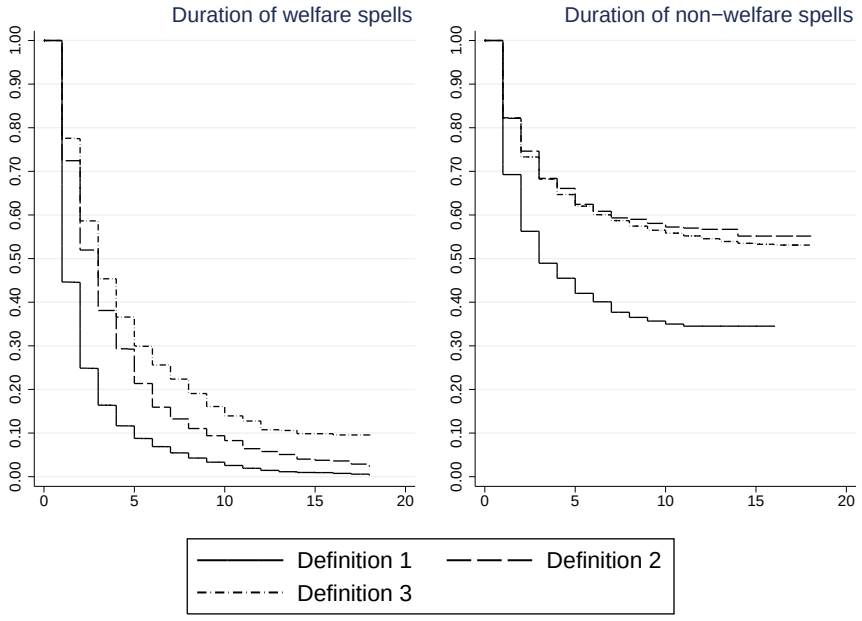

Figure 1: Duration on and off welfare by type of spell. Definition 1 includes all leavers, while definition 2 and 3 include only those with previous benefits amounting to at least one half of the basic amount (def. 2) or half of disposable income (def. 3).

not include most of those eligible for other social insurance benefits, but have to rely on welfare while waiting for these benefits. We sample all individuals classified as welfare participants according to at least one of the definitions in at least one year between 1990 and 2008.

Throughout the analysis, we have the potential complication that eligibility for social assistance is determined at the household level while we are observing individual outcomes. This is problematic since we might observe individuals leaving welfare without experiencing an increase in any income when the individual's partner starts to work or receive other income causing the whole household to lose benefit eligibility. If this is the case, those that are supported by a spouse will be identified as being disconnected, although this is not true at the household level. Still, looking at individual outcomes is relevant in the sense that it determines the individual's ability to be self sufficient. However, for individuals in the "other" category, we also study changes at the household level, and if there are two adults in the household we look at labor income of the spouse.

3.2 Descriptive statistics

To illustrate the implications of using a multiple spell framework, Table 2 shows the number of years with social assistance, divided into single and multiple spells. Note that in both categories, left- and right-censored spells are included. The length of a spell is defined as the number of consecutive years during which the individual receives any non-zero amount of social assistance. As the table indicates, of all single spells, 60 percent last only one year, 17 percent last for two years, et cetera. For those with multiple spells, one fifth experience two years with welfare, that is, two spells of one year each. Another 16 percent experience three years on welfare, either in two or three different spells. This indicates that the majority of the spells on social assistance are short, especially among those who experience only one spell. It is also clear that individuals who experience many years with social assistance often do so in several spells, rather than during one uninterrupted spell. Thus, it is important to take the return to welfare into account when looking at welfare dynamics, rather than studying single spells.

Table 3 shows summary statistics for the sampled population averaged over the time period 1990 to 2008. The left column represents an eight percent random sample of the whole population[5], while the columns to the right repre-

[5] That is, the population of working age (18-64 years old), excluding newly arrived immigrants. The reason for using an eight percent sample in this case is the fact that using the whole population would give us an intractable amount of data. Since the sample, is random it is representative of the population as a whole. This sample is used only to calculate descriptives for the whole population, when constructing the sample of welfare participants the entire population is used.

Table 2: Number of years with social assistance, by number of spells 1990-2008

	Single spell	Multiple spells	Total
1	60.606	0.000	40.070
2	17.155	20.874	18.415
3	8.366	16.764	11.211
4	4.270	13.338	7.343
5	2.845	11.084	5.637
6	1.389	8.939	3.947
7	1.189	6.567	3.011
8	0.575	5.144	2.123
9	0.619	3.858	1.717
10	0.446	3.331	1.424
11	0.424	2.767	1.218
12	0.341	1.547	0.749
13	0.339	1.607	0.769
14	0.159	1.186	0.507
15	0.342	1.157	0.618
16	0.298	0.699	0.434
17	0.220	0.868	0.440
18	0.166	0.272	0.202
19	0.251	0.000	0.166
Total	100.000	100.000	100.000

sent the fraction receiving any benefits and at least one half of the basic amount of social assistance benefits at some point between 1990 and 2008. These are the populations the following analysis will focus on, as shown at the bottom of the left column these populations constitutes around 10 percent and 2 percent of the whole population, respectively. This gives us a panel of 19 years containing every individual who received social assistance during at least one year between 1990 and 2008. From this data, we also extract the population of those who were at some point welfare participants according to the stricter definition presented in Table 1 above, that is, those who received more than one half of the basic amount during at least one year. "SA" indicates the average amount of yearly social assistance in the whole sample while "SA among participants" indicates the average amount during the years when receiving some non-zero amount. Being employed is represented by a dummy variable that takes the value one if the individual was employed in November of the given year and is thus a very imprecise measure, but might give some indication of labor market participation. "Immigrant" indicates being born outside of Sweden, and "born outside the OECD" indicates being born in a country outside Europe, North America, Australia, and New Zealand.

Clearly, welfare participants receive more benefits, both on average and during the year(s) they receive benefits. They also have lower income and are less likely to be employed. Immigrants, single parents, and individuals with only

compulsory schooling are more likely to be on welfare than other groups. Among those receiving relatively high benefits, there is an over-representation of single parents and individuals with low education. They also tend to be older than the average welfare participant.

Table 3: Summary statistics, individuals 1990-2008

	Total[a]	SA participants, all[b]	SA participants, def 2[c]
SA	1054	4233	12,672
SA among participants	17,505	17,501	27,725
Disp income	129,549	104,363	91,564
Employed	0.750	0.596	0.390
Age	40.724	37.758	38.369
Age<26	0.161	0.197	0.094
Female	0.493	0.504	0.416
Immigrant	0.069	0.213	0.204
Born outside the OECD	0.023	0.110	0.107
Parent	0.384	0.422	0.348
Single parent	0.046	0.102	0.152
Compulsory schooling or less	0.234	0.297	0.366
Post-secondary schooling	0.276	0.178	0.108
Share w SA, all	0.103	.	.
Share w SA, def. 2	0.020	.	.
N (over 19 years)	5,024,939	25,034,164	239,548

[a] Based on an 8 percent sample of the total population.
[b] Defined as receiving any benefits during at least one year.
[c] Defined as receiving more than one half of the basic amount of benefits during at least one year.

4 Estimation methods

The probability of leaving welfare will be modeled using a multivariate discrete choice model, where there are six different outcomes: strong and weak labor market attachment, sick leave, unemployment, studies, and other. The last category essentially captures all cases where we cannot determine the main source of income. The "other" category is used as the base category, and thus the resulting parameter estimates can be interpreted as a series of binary logit models with comparison being done to the base category. Thus, the probability of observing alternative *j* given that either alternative *j* or the base category is chosen is

$$Pr(y_i = j | y_i = j \text{ or } 1) = \frac{Pr(y_i = j)}{Pr(y_i = j) + Pr(y_i = 1)} = \frac{exp(\mathbf{x}_i' \beta_j)}{1 + exp(\mathbf{x}_i' \beta_j)} \quad (1)$$

where y_{ij} indicates outcome *j* for individual *i*.

Using this model, we estimate the probability of various demographic groups to leave welfare for the different labor market states defined in Table 1. That is, we study where in the labor market welfare leavers are more likely to end up.

To study how the probability of experiencing an increase in income and being in poverty varies between the different post-welfare outcomes, we estimate standard logit models. For all non-linear, models we present the average marginal (partial) effects of the regressors, rather than their coefficients, that is:

$$ME_j = \frac{\partial E(y|\mathbf{x} = \mathbf{x}*)}{\partial x_j} \tag{2}$$

for continuous **x** and

$$ME_j = E[y_i|\mathbf{z}_i = \mathbf{z}_i^*, d_i = 1] - E[y_i|\mathbf{z}_i = \mathbf{z}_i^*, d_i = 0] \tag{3}$$

for discrete regressors, where $\mathbf{x} = (\mathbf{z}, d)$ and $\mathbf{z}$ denotes all regressors but the binary regressor d. Odds ratios for all non-linear regressions are available on request. We study how the initial labor market outcome correlates with disposable income and poverty, both in the short and in the longer run. We also look at the probability of returning to welfare. This will tell us how former welfare participants are faring, and how their situation differs depending on labor market outcome.

The covariates in the **x**-vector includes dummy variables for being female, being younger than 26 years old, having only compulsory schooling, having immigrated to Sweden from a non-OECD country, and being a single parent. To control for the (presumably large) impact of the business cycle and unobserved differences between municipalities, fixed effects for ending year of the spell and municipality of residence at the end of the spell are included.

As a heterogeneity analysis, we perform the analysis separately for a period of financial downturn (1992-1994) and a period when the economy was stronger (1998-2000). This analysis illustrates if and how labor market conditions affect the post-welfare situation of welfare leavers. We also perform separate estimations for young individuals and individuals who have immigrated to Sweden from a country outside the OECD. These groups are chosen because they are commonly thought of as being particularly disadvantaged, and because they have unusually high rates of welfare participation. Also, it is often argued that these groups are more affected by business cycle fluctuations than are other groups in the population.

Finally, we also study the disconnected leavers, that is, those who leave welfare to the "other" category defined in Table 1, in more detail. Primarily, we try to determine the importance of income earned by other family members, since, given the structure of the data, we might suspect that some of those that become disconnected are in fact living with someone who are determined by the welfare office to have sufficient income to support the whole household.

5 Results

This section first presents a graphical analysis of how income changes after leaving welfare. To illustrate this, we sample all leavers in 1991 and 1996, that is, everyone who received benefits in 1990 but not in 1991, and in 1995 but not in 1996, respectively. These years are somewhat arbitrarily chosen, but will represent different states of the overall economy, with 1991 at the beginning of a financial crisis and recession that started to come to an end in the middle of the 1990s. Thus, leavers in 1991 and 1996 will face very different labor market conditions. The exit rates from welfare only differs slightly between the two years. However, if welfare participation is defined receiving any benefits during a year, 41 percent of all welfare participants in 1990 had left welfare the following year, whereas the exit rate was 43 percent for the welfare caseload in 1995. Defining welfare participation as having more than one half of a basic amount of benefits during a year (definition 2) the exit rates were 34 percent and 36 percent, respectively. In this initial analysis, we focus on three labor market outcomes, strong labor market attachment (which is what we are most interested in), weak labor market attachment, and the category referred to as "other", the so called disconnected leavers, which are outcomes where most of the welfare leavers who have received higher benefit amounts end up. We will then move on to the econometric analysis of the probability of leaving welfare for different labor market states, the subsequent change in income, and the risk of poverty. We also look at heterogeneous effects across demographic groups and business cycles. Finally, we study the "disconnected" leavers in more detail.

5.1 Characterizing leavers and post welfare outcomes

Figure 2 shows the development of individual disposable income for welfare leavers in 1991 (to the left) and 1996 (to the right), separated by some demographic characteristics. We present both the more generous definition (welfare participation defined as receiving any benefit, top panel) and the definition presented in Table 1 (receiving at least one half of the basic amount of welfare during a year). This figure shows a clear difference between the two definitions of welfare. The average leavers (definition 1) start out at a higher income level and this difference remains throughout the whole time period. Leavers who were previously receiving higher benefit levels do worse, particularly those who left during the weaker economy in the early 1990s. Throughout, immigrants and single parents have a lower income than the average, but overall, and in particular for those who have received high benefits, the differences between the demographic groups are small. Naturally, the increase that is observed in these graphs needs to be related to the income development of those who remain on welfare. Plotting the same income paths of welfare participants in 1991 and 1996, respectively, who remain on welfare shows that these individuals experi-

ence only very small growth in disposable income, which reaches levels above SEK 90,000 for only a few individuals during the last years of the data (graph not shown, available on request).

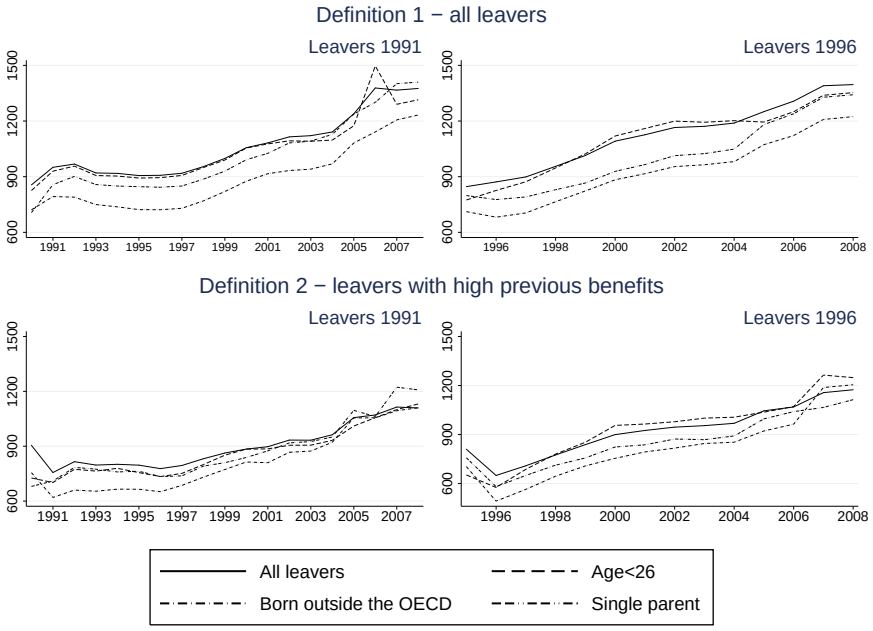

Figure 2: Leavers 1991 and 1996 - disposable income by demographic characteristics (SEK 100'). Definition 1 includes all leavers, while definition 2 includes only those with previous benefits amounting to at least one half basic amount.

Figure 3 shows the same income development as in Figure 2, but separated by some of the initial post-welfare outcomes. Clearly, having a strong labor market attachment is more closely associated with a higher disposable income than any other outcome. In the long, run there is a substantial financial gain in all outcome states, but in the short run some leavers suffer financial losses. This is the case especially for welfare leavers with high previous benefits in the financial downturn in 1991, where all groups lose income at some point within the first few years after leaving welfare. Since the groups are constructed using the initial outcome, that is, labor market position in 1991 and 1996, respectively, the decrease in disposable income can be explained by individuals moving back to welfare or into some of the other categories.

Figure 4 illustrates the returns to welfare, where we plot the (cumulative) fraction of leavers who returned to welfare after having left in 1991 and 1996. This shows that leaving with a strong labor market attachment is associated with a lower risk of return. Among all leavers in 1991, the return rate for those leav-

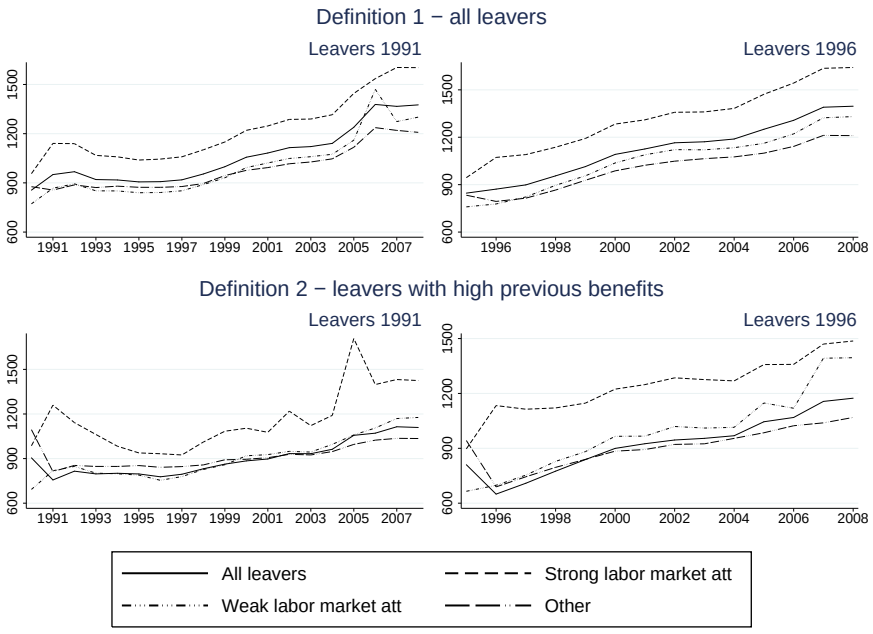

Figure 3: Leavers 1991 and 1996 - disposable income by initial outcome (SEK 100'). Definition 1 includes all leavers, while definition 2 includes only those with previous benefits amounting to at least one half of the basic amount.

ing with a strong labor market attachment is around 50 percent after 10 years, compared to above 60 percent for the other groups. The difference is also large among those leavers who have received higher benefits, the return rate is around 20 percent for those with a strong labor market attachment compared to an average of 40 percent. The differences between the groups are similar for leavers in 1996, but the overall return rate is lower. Especially, welfare participants who had received higher benefits and leave welfare for work in 1996, when the economy is strong, exhibit a risk of repeated welfare dependence of less than 10 percent within the following 12 years.

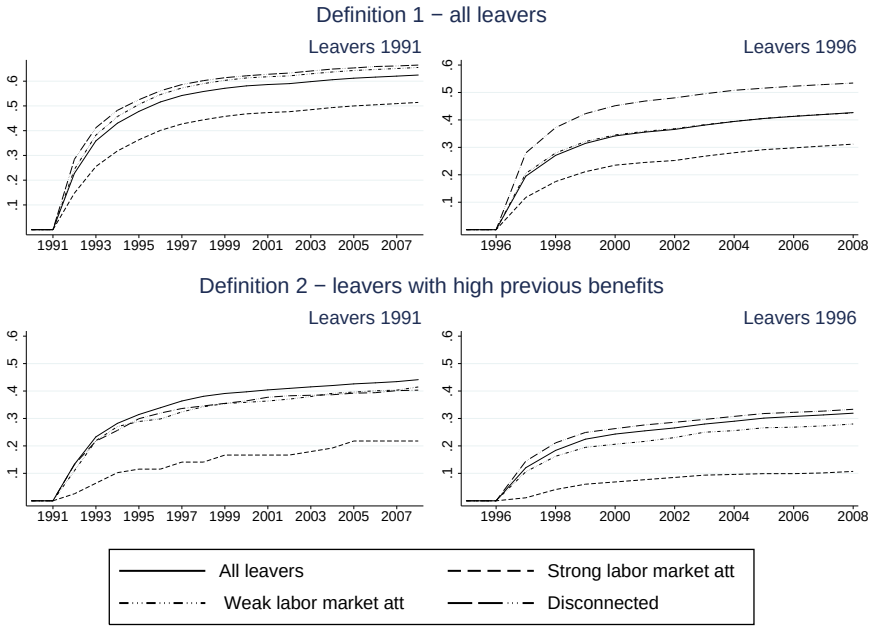

Figure 4: Return to welfare, by post-welfare outcome. Definition 1 includes all leavers, while definition 2 includes only those with previous benefits amounting to at least one half of a basic amount.

It is thus clear from these graphs is that having a strong labor market attachment is associated both with higher post-welfare disposable income and a lower risk of returning to welfare. While the differences between the post-welfare outcomes are substantial, there are only small deviations between demographic groups.

5.2 Labor market outcomes

This section presents estimates of the probabilities of exiting welfare for different states, and how these probabilities vary within the population of leavers. In

these estimations we use the whole time period covered by our data, that is, we include everyone who receive and stop receiving welfare payments at any point between 1990 and 2008. To control for yearly and regional differences in the labor market conditions year and municipality fixed effects are included in the estimations. The estimates are interpreted as average marginal effects (see equations 2 and 3). Tables where we present the exponentiated coefficients (odd-ratios) are available on request.

Table 4 shows the probabilities of the different labor market outcomes for individuals the first year after leaving welfare. The results are obtained in a multinomial logit model where the dependent variable indicates labor market outcome. Panel one presents results where welfare participation is defined as having received any benefits during a given year. In panel two, participation is defined as in Table 1, that is, as having more than a half of a basic amount of benefits in a given year. In each panel, the top line presents the probability of a baseline individual (with all covariates set to zero) to leave welfare for each of the labor market states. Among average welfare leavers, who received any positive amount of benefits (panel 1), the most common post welfare outcome is a strong labor market attachment. Around 45 percent of these leavers have a strong connection to the labor market, and essentially work full-time, in the first year after they leave welfare. This is more uncommon for women, who are 8.8 percentage points less likely to have a strong labor market attachment, as well as younger individuals, immigrants born outside the OECD and those with low education. Some of these groups are instead more likely to have a weak labor market attachment, that is, to work part time during a year. The baseline probability for this outcome is almost 9 percent, and this is especially common for young welfare leavers, who are, on the margin, 11.8 percentage points more likely to have a weak labor market connection. However, immigrants are less likely to have any connection to the labor market.

Having a strong labor market attachment is less common as an initial outcome among those who leave welfare after having received higher amounts of benefits (more than one half of a basic amount during a year). The most common outcome for these leavers is having a weak labor market attachment, this category captures around 33 percent of theses leavers while only 11 percent have a strong labor market attachment. With this definition of welfare participation all groups represented by the covariates have a lower probability of having a strong labor market attachment compared to the baseline, especially females and individuals with low education (they are 3.7 and 4 percentage points less likely to have a strong attachment to the labor market, respectively). Welfare leavers who have received higher amounts of assistance are also more likely to transition to the category referred to as "other", the baseline probability is 24.6 percent and women and individuals with low education are both more than 10 percent more likely to move to this outcome.

Table 4: Multinomial logit analysis of post-welfare outcomes

	Strong labor force att	Weak labor force att	Sick leave benefits	Unemployment benefits	Student	Other
			Panel 1: Definition 1 - all leavers			
Baseline probability	0.449	0.087	0.135	0.179	0.179	0.012
Female	−0.088***	0.044***	−0.012***	−0.002	0.008***	0.050***
	(0.009)	(0.003)	(0.003)	(0.001)	(0.001)	(0.003)
Age<26	−0.121***	0.118***	−0.070***	0.028***	0.033***	0.011**
	(0.008)	(0.001)	(0.001)	(0.005)	(0.001)	(0.004)
Born outside	−0.110***	−0.006***	0.037***	0.073**	0.014***	−0.008
the OECD	(0.007)	(0.002)	(0.004)	(0.004)	(0.001)	(0.004)
Single parent	0.010*	0.017***	−0.012***	0.067***	−0.001	−0.081***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Compulsory	−0.096***	0.003***	0.043***	−0.029***	−0.014***	0.093***
schooling or less	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
N	2,140,674	2,140,674	2,140,674	2,140,674	2,140,674	2,140,674
			Panel 2: Definition 2 - leavers with high previous benefits			
Baseline probability	0.110	0.335	0.117	0.129	0.059	0.246
Female	−0.037***	−0.054***	−0.018***	−0.014***	0.016***	0.108***
	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.003)
Age<26	−0.008***	0.110***	−0.085***	−0.028***	0.056***	−0.043***
	(0.001)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)
Born outside	−0.016***	−0.033***	−0.021***	0.014**	0.023***	0.030***
the OECD	(0.002)	(0.005)	(0.002)	(0.005)	(0.002)	(0.008)
Single parent	−0.008***	0.045***	−0.016***	0.020***	0.028***	−0.069***
	(0.001)	(0.006)	(0.004)	(0.002)	(0.002)	(0.005)
Compulsory	−0.040***	−0.024***	0.012***	−0.050***	−0.018***	0.119***
schooling or less	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.004)
N	530,921	530,921	530,921	530,921	530,921	530,921

Logit estimates, average marginal effects. Year dummies and city district dummies are included in all estimations. Standard errors in parentheses, calculated using the delta method.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Thus, individuals who leave welfare after having received relatively high benefits are less likely than average leavers to become a part of the stable labor force, at least initially. Instead, they are more likely to have unstable work and to move to the category we refer to as "other". Having a strong labor market attachment is more uncommon among women, immigrants, individuals who are young, or have low education. In the following section, we move on to see how these initial outcomes affect short- and long-run post-welfare financial well-being.

5.3 Post-welfare financial situation

Changes in income levels and poverty rates

In this section, we analyze the financial situation of individuals after they leave welfare for different outcomes and follow them forward in time to see how their financial situation changes. Specifically, we look at the probability that welfare leavers in each outcome category will experience an increase in disposable income relative to the income they had during the last year of welfare participation and the risk of being in poverty. Since poverty is defined in terms of disposable income, these two measures will be affected similarly, and if some group remains poor even while their disposable income increases it reflects only the fact that the group had an initially lower income level and higher poverty rate. For this reason, and for reasons of length constraints, we present estimates of changes in income and poverty only for the average population, when studying sub-groups we only present estimates for changes in disposable income. Intuitively, one would expect leaving welfare to be strongly associated with a better financial situation. However, for this to occur, the reduction in benefits must be accompanied by an increase in other income that compensates for the lost welfare income. Thus, there is a possibility that income decreases and the risk of poverty increases when an individual leaves welfare, at least initially. However, one could also argue that, if individuals are well-informed and rational, they would not choose to leave benefits if they were not able to increase for example labor income proportionally, or that an individual would only be denied benefits if the caseworker correctly perceives that the family can get sufficient income from other sources.

As in the multinomial logit specification, the following estimations include fixed effects for year and municipality, in an attempt to distinguish the effect of the transition off welfare from yearly and regional effects. Again, we here present average marginal effects, estimates interpreted as odds-ratios are available on request.

Table 5 shows the results, interpreted as average marginal effects, from a logit estimation of the probability of experiencing an increase in income relative to the last year with social assistance, given being in a specified initial state. The

baseline category is the group we refer to as "other" or "disconnected", so estimations are to be interpreted relative to the outcome for individuals in this group. Panel 1 shows results using the broader definition of welfare participation, while panel 2 shows results for the definition presented in Table 1, that is, those leavers who have received relatively high benefit levels. The top line of each panel shows the baseline probability of the outcome in the reference group, that is, among individuals in the "other" category. These statistics tell us that among those leaving to the baseline category after having received any benefits, around 43 percent experience an increase in income during the first year off welfare (57 percent do not). For those who have received higher benefits (panel 2), the corresponding number is 32.5 percent. After ten years off welfare, 70 percent of all disconnected leavers and 63 percent of those disconnected leavers that were previously receiving relatively high benefits have a higher income than when they were on welfare. Thus, the difference between the groups is persistent over time, after ten years off welfare the group who were more reliant on benefits is still worse off in terms of disposable income, at least among those in the baseline category.

Having a strong labor market attachment have a larger positive contribution for those leavers who were previously more welfare reliant. Compared to the baseline, income is 55 percent more likely to increase for those with a strong labor market attachment after the first year while, for the average leavers, the difference relative to the baseline is 31 percent. For both groups, this positive association is stronger than for any other outcome. However, the difference decreases over time off welfare and after ten years weak and strong labor market attachments are associated with almost the same chances of an increased income, relative to the income during their last year on welfare. Since these two outcomes are relatively similar, it is possible that individuals move between them, which can explain why the groups converge in income.

For both definitions, those leaving welfare to be supported by sick leave benefits are initially worse off than any other group, with only small deviations from the baseline category (the disconnected leavers). Among all leavers (definition 1) those leaving for sick leave insurance are less likely than the baseline to experience an increase in income (1.3 percentage points less likely in the initial year, thereafter the estimates are positive but insignificant).

The results for the probability of being in poverty (defined as having a disposable income below the median) are presented in Table 6. These results are essentially the inverse of those for disposable income, but also taking the relative level of income into account. In the baseline category, that is, among those in the "other" category, the probability of being poor is around 18.7 and 41 percent, respectively for the two definitions, as shown in the top line of each panel (which can be compared to an overall poverty rate of around 14 percent in the economy as a whole). That is, poverty among welfare leavers is much more

Table 5: Probability of increased income

	Year 1	Year 2	Year 5	Year 10
Panel 1: Definition 1 - all leavers				
Baseline probability	0.432	0.494	0.607	0.701
Strong labor market attachment	0.314***	0.245***	0.162***	0.113***
	(0.001)	(0.003)	(0.002)	(0.002)
Weak labor market attachment	0.128***	0.138***	0.107***	0.092***
	(0.002)	(0.001)	(0.002)	(0.003)
Sick leave benefits	−0.013**	0.002	0.009*	0.008
	(0.004)	(0.004)	(0.005)	(0.005)
Unemployment benefits	0.084***	0.077***	0.061***	0.042***
	(0.002)	(0.002)	(0.001)	(0.002)
Student	0.048***	0.029***	0.054***	0.052***
	(0.004)	(0.003)	(0.005)	(0.005)
N	2,140,674	1,692,341	1,113,983	642,497
Panel 2: Definition 2 - leavers with high previous benefits				
Baseline probability	0.325	0.396	0.517	0.630
Strong labor market attachment	0.546***	0.403***	0.249***	0.147***
	(0.008)	(0.007)	(0.012)	(0.010)
Weak labor market attachment	0.346***	0.287***	0.223***	0.171***
	(0.008)	(0.004)	(0.004)	(0.005)
Sick leave benefits	0.031***	0.021**	0.040***	0.033***
	(0.007)	(0.006)	(0.005)	(0.005)
Unemployment benefits	0.106***	0.074***	0.059***	0.033***
	(0.004)	(0.005)	(0.005)	(0.006)
Student	0.266***	0.158***	0.121***	0.104***
	(0.005)	(0.005)	(0.007)	(0.006)
N	530,921	424,318	280,951	150,954

Logit estimates, average marginal effects. Year dummies and city district dummies are included in all estimations. Standard errors in parentheses, calculated using the delta method.
* p<0.1, ** p<0.05, *** p<0.01

common among those that leave welfare after having received higher amounts of benefits. These leavers also depend more on having a strong labor market attachment in order to avoid poverty, and for both definitions of welfare participants essentially no one who have a strong labor market attachment are in poverty after leaving welfare.

For leavers with higher previous benefits, a weak labor market attachment, being unemployed, and being a student are all associated with a reduction of the risk of poverty of around 22 percentage points in the initial year, that is, the poverty rate in these groups is less than half of that experienced by individuals in the baseline category (the disconnected individuals). The differences between the outcomes are smaller and vary more in size in the larger sample of all welfare leavers. However, in both panels we see that the risk of poverty is higher than the baseline for those who leave to be supported by the sick leave insurance, reflecting the fact that this group is also less likely to experience an increase in income, as shown in Table 5. However, this difference is attenuated over time, probably since most individuals do not stay on sick leave benefits for more than a limited amount of time and then move to another category.

These estimates only concern those individuals that stay off welfare, for two, five, and ten years respectively. However, as shown in Figure 4, a substantial fraction of those who leave welfare return to benefit receipt within a few years. The graphical illustration also shows that the return rate differs between individuals in different outcomes. To illustrate this further, Table 7 presents estimates for the probability of return within two, five, and ten years after leaving welfare, and how this differs between post-welfare outcomes.

The baseline probability of return, as shown in the top line of each panel in Table 7, shows that the risk of going back to receiving benefits is around 22 percent for the average welfare participant (panel one), meaning that slightly above one fifth of welfare leavers are self-sufficient for only one year before returning to benefit receipt. After five years, 42 percent have experienced at least one year of repeated welfare receipt, and after ten years, around half of all leavers have returned. For those with a strong labor market attachment in the first year after leaving welfare, the probability of repeated dependency is substantially lower, almost 13 percentage points lower in the first period, and 45 percentage points lower after ten years. The risk of returning to welfare is higher for those on sick leave benefits and for students.

The baseline return rate is lower for welfare leavers who have received higher amounts of benefits (panel 2), 18 percent have returned within one year and 36 and 44 percent after five and ten years, respectively. The pattern observed for the average leavers in panel 1 still holds, with very low risk of return for those with a strong labor market attachment, which is consistent with the illustration in Figure 4. This indicates that cycling into and out of welfare participation is more uncommon for those who receive higher amounts of benefits (and can

Table 6: Probability of poverty

	Year 1	Year 2	Year 5	Year 10
Panel 1: Definition 1 - all leavers				
Baseline probability	0.187	0.148	0.110	0.103
Strong labor market attachment	−0.196***	−0.135***	−0.075***	−0.067***
	(0.002)	(0.001)	(0.001)	(0.001)
Weak labor market attachment	−0.003	−0.003	0.007***	0.001
	(0.002)	(0.002)	(0.001)	(0.001)
Sick leave benefits	0.095***	0.082***	0.064***	0.042
	(0.004)	(0.004)	(0.004)	(0.003)
Unemployment benefits	−0.065***	−0.035***	−0.007***	−0.007***
	(0.002)	(0.002)	(0.001)	(0.001)
Student	−0.008**	0.029***	0.036***	0.019***
	(0.003)	(0.002)	(0.002)	(0.002)
N	2,140,674	1,692,341	1,113,983	642,497
Panel 2: Definition 2 - leavers with high previous benefits				
Baseline probability	0.410	0.335	0.242	0.203
Strong labor market attachment	−0.475***	−0.324***	−0.167***	−0.118***
	(0.004)	(0.004)	(0.003)	(0.004)
Weak labor market attachment	−0.250***	−0.136***	−0.069***	−0.051***
	(0.003)	(0.003)	(0.003)	(0.003)
Sick leave benefits	0.064***	0.067***	0.046***	0.015*
	(0.010)	(0.010)	(0.008)	(0.007)
Unemployment benefits	−0.216***	−0.141***	−0.076***	−0.062***
	(0.003)	(0.002)	(0.004)	(0.003)
Student	−0.214**	−0.086***	−0.027***	−0.029***
	(0.004)	(0.004)	(0.004)	(0.003)
N	530,921	424,318	280,951	150,954

Logit estimates, average marginal effects. Year dummies and city district dummies are included in all estimations. Standard errors in parentheses, calculated using the delta method.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$

thus be said to *depend* on benefits), while it is more common for those who need only small amounts of social assistance to, for example, cover temporary expenditures. Also, it is clear that those who leave welfare for outcomes that are associated with low probabilities of a higher income are also more likely to return to benefit dependence.

Table 7: Probability of return

	Year 2	Year 5	Year 10
Panel 1: Definition 1 - all leavers			
Baseline probability	0.221	0.426	0.515
Strong labor market attachment	−0.129***	−0.355***	−0.455***
	(0.001)	(0.003)	(0.002)
Weak labor market attachment	−0.040***	−0.302***	−0.473***
	(0.001)	(0.003)	(0.001)
Sick leave benefits	0.014***	−0.222***	−0.334**
	(0.002)	(0.002)	(0.002)
Unemployment benefits	−0.035***	−0.266***	−0.456***
	(0.002)	(0.003)	(0.002)
Student	0.048***	−0.202***	−0.444***
	(0.003)	(0.006)	(0.008)
N	2,042,811	1,706,064	1,113,267
Panel 2: Definition 2 - leavers with high previous benefits			
Baseline probability	0.187	0.356	0.437
Strong labor market attachment	−0.291***	−0.336***	−0.358***
	(0.007)	(0.004)	(0.004)
Weak labor market attachment	−0.033***	−0.284***	−0.414***
	(0.004)	(0.003)	(0.002)
Sick leave benefits	0.013**	−0.224***	−0.331**
	(0.004)	(0.002)	(0.003)
Unemployment benefits	−0.094***	−0.318***	−0.459***
	(0.004)	(0.004)	(0.005)
Student	0.044***	−0.192***	−0.406***
	(0.004)	(0.005)	(0.007)
N	502,361	403,169	239,461

Logit estimates, average marginal effects. Year dummies and city district dummies are included in all estimations. Standard errors in parentheses, calculated using the delta method.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$

The results thus far clearly show that working full-time in the first year off welfare implies both doing well financially, in terms of disposable income, and in terms of avoiding repeated dependence. This is true both in the short run and in the long run. This might in part be explained by the fact that leaving welfare for work yields a higher income and promotes self sufficiency, but it

is also the case that those welfare-reliant individuals who transition into the labor market are likely to exhibit some unobserved characteristics that make them more likely to experience a positive outcome. That is, there might be a selection of individuals with higher "ability" out of welfare participation. Also, one should keep in mind that, at least among those leavers who have received relatively high amounts of benefits, having a strong labor market attachment is not very common as an initial outcome. Instead, most of these previously welfare-reliant leavers are more likely to have a weak attachment to the labor market or become disconnected, both of which are outcomes associated with substantially lower probabilities of increased income and higher risk of repeated dependence.

Differences over the business cycle

In this section, we try to determine how the situation of welfare leavers varies with the state of the economy. During the time period covered by our data there were substantial business cycle fluctuations with very high unemployment levels during the beginning of the 1990s, peaking at above eight percent in 1993 and reaching quite low levels (around four percent) towards the end of the decade. To study if and how the situation of welfare leavers differs between those who leave during different states of the economy, we limit the analysis to individuals leaving welfare during a time when unemployment levels were very high (1992-1994) and during a time of low unemployment and a growing economy (1998-2000). In Table 8 and Table 9 we present the estimates of the probability of experiencing an increase in income for leavers at these different points in time. For the latter time period we are not able to follow individuals for as long as ten years after leaving welfare. Hence, for consistency, we choose a five year perspective when isolating leavers at different time periods.

Table 8 shows the estimated probability of an increase in disposable income for individuals who leave benefits during the financial downturn of the early 1990s. The fact that the baseline probability, that is, the probability of an increase in income for those individuals who transition to the "other" category, is lower than the average presented in Table 5 indicates that these leavers do worse in a recession than when the labor market is stronger. Also, the marginal effect of having a strong labor market attachment is lower than those estimated in Table 5. However, the chance of a higher income increases over time and approaches that of the average. This can probably be explained by the fact that the labor market gets stronger and those who do not initially enter the labor force are able to do so when the economy improves.

Individuals leaving welfare during the stronger economy around 1998 to 2000 experience a slightly better initial situation. As shown in Table 9, the probability for the baseline category (those leaving for "other") to have a higher disposable income in the year after leaving welfare is 46 and 34 percent in the

Table 8: Probability of increased income, leavers 1992-1994

	Year 1	Year 2	Year 5
Panel 1: Definition 1 - all leavers			
Baseline probability	0.407	0.442	0.537
Strong labor market attachment	0.268***	0.216***	0.176***
	(0.006)	(0.006)	(0.002)
Weak labor market attachment	0.120***	0.132***	0.118***
	(0.003)	(0.003)	(0.003)
Sick leave benefits	–0.006**	0.010***	0.020***
	(0.003)	(0.003)	(0.005)
Unemployment benefits	0.070***	0.053***	0.047***
	(0.003)	(0.004)	(0.003)
Student	0.057***	0.038***	0.076***
	(0.006)	(0.007)	(0.009)
N	654,980	520,395	353,624
Panel 2: Definition 2 - leavers with high previous benefits			
Baseline probability	0.288	0.320	0.404
Strong labor market attachment	0.524***	0.384***	0.257***
	(0.010)	(0.013)	(0.019)
Weak labor market attachment	0.355***	0.287***	0.239***
	(0.009)	(0.009)	(0.008)
Sick leave benefits	0.072***	0.065**	0.082***
	(0.009)	(0.009)	(0.007)
Unemployment benefits	0.103***	0.051***	0.028**
	(0.006)	(0.010)	(0.009)
Student	0.280***	0.179***	0.132***
	(0.012)	(0.010)	(0.015)
N	104,864	85,015	60,791

Logit estimates, average marginal effects. Year dummies and city district dummies are included in all estimations. Standard errors in parentheses, calculated using the delta method.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$

initial year for each definition. This is higher than what is estimated in Table 5. The relative differences between the different outcome categories are similar to those estimated for the pooled years. Thus, over all, individuals who leave welfare during a stronger economy seem to be doing somewhat better than leavers on average in the sense that a larger fraction of these leavers experience an immediate increase in income.

Table 9: Probability of increased income, leavers 1998-2000

	Year 1	Year 2	Year 5
Panel 1: Definition 1 - all leavers			
Baseline probability	0.464	0.551	0.684
Strong labor market attachment	0.322***	0.238***	0.145***
	(0.005)	(0.003)	(0.002)
Weak labor market attachment	0.109***	0.120***	0.080***
	(0.003)	(0.002)	(0.004)
Sick leave benefits	–0.018**	–0.005	–0.002
	(0.006)	(0.006)	(0.006)
Unemployment benefits	0.092***	0.005***	0.070***
	(0.003)	(0.003)	(0.002)
Student	0.039***	0.023***	0.035***
	(0.006)	(0.005)	(0.006)
N	598,675	503,498	369,608
Panel 2: Definition 2 - leavers with high previous benefits			
Baseline probability	0.338	0.444	0.592
Strong labor market attachment	0.542***	0.392***	0.235***
	(0.007)	(0.008)	(0.008)
Weak labor market attachment	0.343***	0.277***	0.205***
	(0.004)	(0.003)	(0.004)
Sick leave benefits	0.021*	0.001	0.014**
	(0.009)	(0.007)	(0.006)
Unemployment benefits	0.118***	0.087***	0.078**
	(0.005)	(0.006)	(0.006)
Student	0.283***	0.147***	0.099***
	(0.006)	(0.005)	(0.007)
N	169,159	143,806	112,523

Logit estimates, average marginal effects. Year dummies and city district dummies are included in all estimations. Standard errors in parentheses, calculated using the delta method.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$

The differences that are observed between these time periods are possibly explained by the conditions on the labor market, but it might also be the case that different types of individuals choose to leave welfare (or for some reason loses eligibility) at different time periods. Those who leave welfare during a recession

might differ from those who choose to leave when the economy is stronger, both with respect to observed and unobserved characteristics. However, it is not clear how one would expect this to influence the results.

It is also important to note that the fraction of welfare leavers who have a strong labor force attachment during the first year without welfare benefits is lower in the earlier time period, and higher during the latter, compared to the average across time (as shown by the baseline probability in Table 4). It seems that those with higher previous benefits are more sensitive in terms of how the business cycle affects post-welfare outcomes, during the early 1990s the share leaving to the core labor force was four percent, compared to 13 percent during the later time period and 11 percent on average. The corresponding numbers for all leavers (definition 1) are 41 percent during 1992-1994, 48 percent during 1998-2000, and 45 percent on average. The share of leavers having a weak labor market attachment also decreases in the financial downturn, while the group of disconnected leavers grows.

Hence, the results indicate that post-welfare outcomes differ among individuals who leave welfare in a strong relative to a weak economy. However, the differences diminish over time and those who leave during the recession are not worse off than the average in the long run.

Heterogeneity among groups

In this section, we focus on groups whose welfare participation is commonly regarded as following different patterns than the average population, and are also considered to have a weaker position on the labor market. In particular, we study young individuals (who are younger than 26 years old) and individuals who have immigrated to Sweden from a country outside the OECD. Young individuals typically have higher participation rates in social assistance, but their spells tend to be shorter than the average. Thus, it is interesting to study if their post-welfare situation is different from that experienced by the average welfare participant. Immigrants, on the other hand, tend to have longer spells of benefit receipt and leave welfare at a lower rate than the average.

Tables 10 and 11 shows estimates for individuals younger than 26 years old and immigrants born outside the OECD, respectively. For young individuals, the baseline probability of experiencing an increase in income is similar to that of the whole population (see Table 5), but younger welfare leavers seem to benefit less from having a strong attachment to the labor market, which increases their probability of an increase in income by 29 and 54 percentage points for average young leavers and young leavers with high previous benefits, respectively. Among all young welfare leavers, including those only receiving small benefits, the differences among most of the initial outcomes become small and insignificant after ten years. This implies that for young individuals the initial outcome is not as strongly associated with future income as for other groups, at

least among those who receive smaller amounts of benefits.

For immigrants, the baseline probability of increased income is also similar to that of the average population, at least in the short run. The marginal increase for those having a strong labor market attachment is about the same as for the total population. However, as with younger welfare leavers, the differences between the initial outcomes are attenuated over time, especially for those with lower amounts of previous benefits. Since immigrants on average have a lower level of income, they are still more likely than other welfare leavers to remain or become poor when they stop collecting benefits, both in the short and in the longer run (estimates not shown, available on request).

Table 10: Probability of increased income, age<26

	Year 1	Year 2	Year 5	Year 10
Panel 1: Definition 1 - all leavers				
Baseline probability	0.424	0.476	0.614	0.726
Strong labor market attachment	0.294***	0.202***	0.071***	–0.006
	(0.007)	(0.009)	(0.005)	(0.007)
Weak labor market attachment	0.101***	0.120***	0.071***	0.044***
	(0.005)	(0.005)	(0.005)	(0.003)
Sick leave benefits	–0.060***	–0.042***	–0.043***	–0.051***
	(0.007)	(0.007)	(0.009)	(0.007)
Unemployment benefits	0.071***	0.059***	0.026***	–0.005
	(0.004)	(0.004)	(0.004)	(0.011)
Student	0.042***	0.032***	0.030***	0.011
	(0.010)	(0.008)	(0.009)	(0.005)
N	379,993	302,121	215,601	150,614
Panel 2: Definition 2 - leavers with high previous benefits				
Baseline probability	0.306	0.369	0.498	0.634
Strong labor market attachment	0.541***	0.384***	0.215***	0.103***
	(0.018)	(0.015)	(0.019)	(0.021)
Weak labor market attachment	0.338***	0.272***	0.195***	0.131***
	(0.006)	(0.008)	(0.006)	(0.007)
Sick leave benefits	0.044***	0.021**	0.028***	0.018*
	(0.008)	(0.007)	(0.007)	(0.009)
Unemployment benefits	0.095***	0.050***	0.009	–0.008
	(0.007)	(0.009)	(0.006)	(0.010)
Student	0.285***	0.155***	0.111***	0.094***
	(0.009)	(0.010)	(0.012)	(0.013)
N	75,832	61,172	43,692	29,394

Logit estimates, average marginal effects. Year dummies and city district dummies are included in all estimations. Standard errors in parentheses, calculated using the delta method.
* p<0.1, ** p<0.05, *** p<0.01

Table 11: Probability of increased income, born outside the OECD

	Year 1	Year 2	Year 5	Year 10
Panel 1: Definition 1 - all leavers				
Baseline probability	0.426	0.494	0.614	0.707
Strong labor market attachment	0.309***	0.225***	0.119***	0.081
	(0.005)	(0.004)	(0.005)	(0.007)
Weak labor market attachment	0.111***	0.115***	0.070***	0.058***
	(0.004)	(0.005)	(0.005)	(0.007)
Sick leave benefits	−0.085***	−0.080***	−0.051***	−0.017*
	(0.003)	(0.005)	(0.008)	(0.008)
Unemployment benefits	0.076***	0.057***	0.029***	0.018**
	(0.005)	(0.004)	(0.007)	(0.006)
Student	0.052***	0.026***	0.020*	0.012
	(0.005)	(0.005)	(0.009)	(0.018)
N	236,937	184,446	118,912	54,254
Panel 2: Definition 2 - leavers with high previous benefits				
Baseline probability	0.317	0.391	0.539	0.680
Strong labor market attachment	0.514***	0.386***	0.217***	0.110***
	(0.007)	(0.009)	(0.008)	(0.017)
Weak labor market attachment	0.334***	0.258***	0.164***	0.102***
	(0.009)	(0.006)	(0.008)	(0.009)
Sick leave benefits	−0.049***	−0.074***	−0.046***	−0.041***
	(0.007)	(0.007)	(0.008)	(0.007)
Unemployment benefits	0.092***	0.052***	0.021**	−0.006
	(0.006)	(0.008)	(0.007)	(0.013)
Student	0.280***	0.159***	0.084***	0.058***
	(0.008)	(0.008)	(0.008)	(0.009)
N	112,343	90,167	64,639	35,430

Logit estimates, average marginal effects. Year dummies and city district dummies are included in all estimations. Standard errors in parentheses, calculated using the delta method.
* p<0.1, ** p<0.05, *** p<0.01

5.4 Disconnected leavers - leavers with unknown outcomes

In this section, we look more closely at those leavers who enter the "other" category when ending benefit take-up and who are thus not supported neither by labor work nor some social insurance scheme. Following previous literature, we refer to this group as disconnected leavers. Table 12 shows the average labor income, average disposable income, and the fraction of households below the poverty threshold among disconnected leavers, and among the whole group of leavers, excluding the disconnected. This table does not build on any econometric analysis, but reports unadjusted summary statistics. In this table we

define welfare participants as those who have received more than one half of a basic amount of benefits (definition 2), since it is in this group of leavers that disconnectedness is one of the most common initial outcomes (among those leavers that have collected only small amounts of benefit being disconnected is very uncommon, as shown in Table 4). Clearly, both income from labor (partly by definition of the outcome states) and disposable income is lower for leavers in the "other" category, and the poverty rate in this group is almost twice as high for this group compared to the average for the remaining categories. As shown in Table 4, single parents and young individuals are less likely to become disconnected when leaving welfare, indicating that income pooling with a partner might be important for this group of leavers. This, and the fact that we are studying outcomes at the individual level, while welfare eligibility is determined at the household level, implies that it is important to also study the income and labor market outcomes of a potential partner. It could be the case that an individual whose partner leaves welfare by moving to work is categorized as a disconnected leaver, since she/he is no longer eligible for benefits when the partner is working. To assess if it is the case that most leavers categorized as disconnected are actually supported by a working spouse, we also look at the labor income and post-welfare outcomes of the partners of disconnected leavers. However, since we only have an imperfect measure for cohabitation, this should be seen as an approximation. As shown in Table 12, the partners of disconnected leavers have a higher labor income and are more likely to have a strong labor market attachment relative to partners of individuals in other labor market states. However, the poverty rate is higher than average among the married disconnected leavers, indicating that the income of the working spouse is not sufficient to help the family leave financial hardship.

6 Conclusions

In this paper, we use Swedish register data to look at post-welfare outcomes for individuals leaving social assistance between 1990 and 2008. Having access to a large set of income variables, we can fully characterize the post-welfare income at the individual and household level. Unlike most earlier studies, both in the US and Europe, our data covers a long time period, giving us the opportunity to study repeated welfare dependence as well as how results vary across business cycle fluctuations.

Earlier research and theoretical predictions suggest that it is likely to be difficult for individuals who stop collecting welfare to compensate completely for lost welfare income with labor income. Thus, if the financial incentives are not altered by policy, welfare leavers may suffer a loss in disposable income, and remain or become poor. Moreover, studies from the US indicate that the benefits from leaving welfare are not equally distributed across population groups.

Table 12: Income among disconnected leavers

	Disconnected	Not disconnected
Labor income	2778	49,142
	(9901)	(53,920)
SA (amount)	8141	16,858
	(6929)	(19,165)
Disposable income	66,687	80,169
	(57,242)	(38,448)
Fam disposable income	140,788	135,961
	(170,846)	(92,240)
Frac in poverty	0.410	0.197
	(0.492)	(0.398)
– if married	0.557	0.340
	(0.497)	(0.474)
Partner's labor income	47,924	39,913
	(71,343)	(66,401)
Partners w str labor market att	0.158	0.124
	(0.365)	(0.330)
Returned to SA	0.310	0.262
	(0.462)	(0.440)

Standard deviations in parentheses.

For example, groups that are generally more disadvantaged (in the US primarily single mothers) also have worse post-welfare outcomes. The fact that the social assistance system in Sweden is universal, rather than targeting families with children, implies that we can extend the analysis and study heterogeneous effects for other groups.

We study how income and the risk of falling below the (relative) poverty line changes when leaving welfare, and how this depends on the post-welfare outcome. We define a set of outcome states by identifying the main source of income for each individual. Mainly, we are interested in the group with a strong labor market attachment and the group we refer to as disconnected, for which we cannot identify the main source of financial support. Having a strong labor market attachment is meant to imply that the individuals have a yearly income from labor that is high enough for the individual to be self-sufficient. Someone who is disconnected is not receiving sufficient income from either labor work or social insurance programs, and is thus disconnected both from the labor market and welfare and social insurance programs.

First, post-welfare outcomes depend heavily on the state of the economy and the labor market. Those who left welfare at the start of the financial crisis in the early 1990s do worse, even in the long run, than those who left in the stronger economy a few years later, especially among those who received higher amounts of benefits prior to leaving welfare. This is probably driven by the fact that labor market attachment is important in order to experience an increase in disposable

income, and it is harder to become a part of the stable labor force in a recession. Labor market attachment is particularly important for those who have been more dependent on benefits (that is, who have received higher amounts of welfare) and thus are probably further from the labor market initially. However, since only about one in ten welfare leavers with high previous benefits are able to support him- or herself through work the year after leaving welfare, these leavers on average have lower income than those that have been less welfare reliant.

Among those not having any attachment to the labor market when leaving welfare the majority seems to face difficulties fully compensating for the lost social assistance benefits with other income. This is not surprising since there is, in practice, a tax rate of 100 percent on labor income when receiving social assistance. This means that welfare benefits are discounted by the exact amount of the additional labor income (or any other type of income), so that before earning enough to completely replace welfare income, disposable income is not affected when increasing the number of hours worked.

Our analysis clearly shows that a financially successful exit from welfare, where disposable income increases and the risk of poverty is reduced, is most likely to be realized for those leaving welfare with a strong labor market attachment. Being able to support oneself through labor market work is an efficient insurance against future returns to welfare dependence and most leavers in this outcome are almost certain to experience an increase in disposable income. There is a clear distinction between those who leave welfare with a weaker labor market attachment (who work only part of the year) and those with a strong labor market attachment. Working only part-time does not guarantee an increase in income, and cannot be seen as an insurance against future return to welfare. However, if a person with a weak labor market attachment does not return to welfare, the connection to the labor market might be strengthened and thus lead to stable employment and better financial outcomes in the long run. This is indicated by the fact that the situation of those who initially have a weak and strong attachment to the labor market, respectively, have relatively similar long-run financial outcomes.

The distinction between the initial outcomes is the same regardless of the state of the economy and the labor market situation, each post-welfare outcome is associated with about the same probability of a higher income for individuals leaving welfare in the recession during the early 1990s as for those leaving welfare at the end of the decade, when the economy was stronger. That is, the relative gain in each post-welfare outcome does not depend on the state of the economy, even though the level of post-welfare income might vary over the business cycle.

It is important to note that while a strong labor market attachment leads to an improved financial situation, most welfare leavers do not have full-time

work. Rather, a large number of welfare leavers transition in to a state where they combine incomes in a way that makes it difficult to determine what their main source of support is, we refer to these as disconnected leavers. In this group, poverty is high and relative to the last year with benefit take-up, disposable income decreases when leaving welfare (at least initially). For some of these individuals it is the case that they leave welfare because their partner finds employment and household income thus increases so that eligibility for welfare benefits is lost. Indeed, the labor income of those who have a partner who is disconnected is higher. This indicates that these leavers move from dependence on social assistance to being financially dependent on their spouse. However, being supported by a spouse does not seem to provide long-term insurance from repeated welfare dependence, since the return to welfare participation is high among disconnected leavers. In this group, around 32 percent receive welfare benefits again after five years, while among other leavers the corresponding figure is only around 12 percent. The low return rate for the last group is driven mostly by those who leave welfare with a strong labor market attachment and thus can support themselves through work.

We conclude that having a strong labor market attachment after leaving welfare is the only post-welfare outcome that is associated with a strong financial improvement at the individual level both in the short and long run. Also, it is important to note that post-welfare outcomes differ between those that are more or less dependent on welfare benefits, the heterogeneity of the welfare caseload is reflected in heterogeneous post-welfare outcomes. Average welfare participants, who typically get only small amounts of benefits, are more likely to be completely self sufficient after ending benefit take-up, whereas those who have been eligible for higher benefit levels face more difficulties in the labor market. It is also clear that while the majority of welfare leavers experience an increased disposable income, there is a substantial fraction whose income does not rise and who is still in poverty after leaving welfare.

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Essay 4

The Effect of Military Base Closures on Individual Labor Market Outcomes

1 Introduction

The end of the Cold War denoted the beginning of a new geopolitical landscape. The threat of a border invasion was downgraded in many countries, and focus shifted to the ability to conduct military operations abroad. This led to large cutbacks of the military sector. At the start of the 1990s, the Swedish Armed forces employed more than 45,000 individuals, whereas only 20,000 were employed by 2008. In 1999, the government announced a proposition that mandated one of the largest reforms of the national defense in modern times, resulting in closures and significant downsizing of several military bases. This paper looks at how the closures affected military employees' long run labor market outcomes.

Several studies have found that displaced workers experience costly spells of unemployment and earnings declines, both in the short and in the long run (see for example Stern, 1972; Jacobson, LaLonde, and Sullivan, 1993; Couch and Placzek, 2010; Eliason and Storrie, 2006). To deal with the selection of individuals who experience displacements, most studies focus on (arguably exogenous) events such as mass layoffs or plant closures, where the separation is thought to be independent of a worker's characteristics. These events are typically identified through administrative registers, which can be problematic since there is often only very limited information on how and why the mass layoffs occurred and to what extent they were expected. Furthermore, there could still be problems with selection of the displaced workers or selection between firms, since most studies lack a natural comparison group. By using a political decision as the triggering factor behind a closure, we are able to, at least partially, avoid these problems. Although closing and downsizing of military bases was expected at this point in time, it was not known which places would be affected until the defense proposition was announced in the fall of 1999. Moreover, the decision of where to close down installations was based on factors such as cost efficiency and security policy, implying that it was unlikely to be endogenous to the workers ability. Hence, we argue that the displacements that occurred following the acceptance of the proposition in 2000 were exogenous and unexpected, at least from the perspective of the employees.

During this period, reductions of military personnel and closures of military bases were carried out in many of the salient military powers, such as the UK, France, and Germany. The closures were often expected to have tremendous negative consequences for the affected region, generating concerns and objections from both the public and politicians in the local area (for example Warf, 1997). Thus, most previous studies focus on the local consequences of a base closure, such as economic growth, local employment, and migration. Overall, they find small or insignificant effects (Hooker and Knetter, 2001; Andersson, Lundberg, and Sjostrom, 2007; Paloyo, Vance, and Vorell, 2010). Hence, the general conclusion has been that the negative expectations have not been real-

ized. Although the closures may only have had negligible effects on the local economy, it is unclear if these results also apply to employees that were directly affected. To the best of our knowledge, this is the first paper studying the effect of military base closures using individual-level panel data covering all military employees.

To estimate a causal effect, we employ a difference-in-differences approach where we compare military employees at bases that were affected by the proposition in 1999 to employees at unaffected bases. Using linked employer-employee data, we construct a panel of yearly register data for all individuals that were employed in the military sector during 1998, one year before the proposition was announced. This data allows us to follow individuals over time, making it possible to account for individual heterogeneity, and, furthermore, to separate the treatment effect over time, in order to fully capture its dynamics. By using a relevant control group, consisting of military employees at units that were not affected by the reform, we isolate the causal effect of the closures on unemployment and labor income. Military employees consist of two groups, military and civil servants. Unlike earlier studies in the field, we are able to study how the effect differs between these groups. This distinction is important since the two groups often have different types of employment contracts, but also because the two groups face different labor market opportunities.

We find that workers at the affected bases experience long-term declines in labor income, relative to workers at unaffected bases. The effect is mainly driven by civil servants. This is probably due to the fact that these individuals could be displaced due to redundancy much more easily than individuals employed as military staff. Our results contrast to earlier literature on military base closures, which mostly finds no or small effects on local unemployment and growth.

The remainder of the paper is organized as follows. In the next section, we discuss the previous literature on displaced workers and military base closures. In section 3 we provide a brief background on the defense draw downs in Sweden, section 4 describes the data set. In section 5 we present some summary statistics for our sample, and in section 6 we present the empirical specification. The results are presented in sections 7 and 8, and section 9 concludes.

2 Previous literature

2.1 Literature on displaced workers

There is a substantial literature focusing on displaced workers and the economic difficulties that they face. In the short run, the cost of displacement is ascribed to forgone earnings during unemployment as well as the loss of firm- and industry-specific human capital (Hamermesh, 1987). Besides such mechanisms, the long-term effects, often referred to as unemployment scarring, are explained by

factors such as loss of general human capital and the tendency for an employer to view an individual's labor market history as a signal of productivity (see for example Böheim and Taylor, 2002; Arulampalam, Gregg, and Gregory, 2001).

There are strong reasons to think that involuntary job losses are highly associated with unobserved individual characteristics, such as an individual's knowledge and ability. Hence, most studies in the displacement literature focus on events such as mass layoffs or plant closures, where the separation is thought to be independent of a worker's quality, to deal with the identification of non-voluntary job separations. However, expectations of a forthcoming firm closure might cause a selection of the labor turnover prior to the shutdown.[1] To deal with this problem, most studies define a time window before the closure and define separations during this period as displacements. This method has been criticized for being arbitrary and using ad-hoc definitions, thereby failing to fully capture the selection process (Schwerdt, 2011). Even when focusing on events such as mass layoffs, displaced workers are unlikely to constitute a random sample. Many studies have found that the displaced workers suffer earning losses before the separation occurs (Jacobson, LaLonde, and Sullivan, 1993; Eliason, 2011a). This could be due to the fact that distressed firms cut wages or work hours before a mass layoff, or, perhaps more plausibly, because of selection, that is, the least productive workers are laid off. Eliason (2011a) finds that pre-displacement income from social insurance was higher for the displaced workers compared to their non-displaced co-workers, suggesting that the displaced workers were selected. Using closures rather than mass layoffs is likely to mitigate these selection problems. However, even in the absence of selection within firms, there could be sorting between firms. Abowd, McKinney, and Vilhuber (2009) find that firm closures occur substantially more often in firms that hire a disproportionately high share of worker with low human capital.

Most studies examine the effect of displacements on earnings, and several studies from both the US and Europe have found that displaced workers experience periods with earnings decline. Although the initial drop in income decreases over time, many studies find long-term effects. Looking at studies with a similar empirical design,[2] the long-term earnings decline relative to pre-displacement earnings varies between 13-25 percent in the US (Jacobson, LaLonde,

[1] The firm may choose to lay off its least productive workers first, but, on the other hand, workers with better labor market opportunities may choose to quit before the closure in order to avoid displacement.

[2] These studies use a variant of the model: $y_{it} = \beta X_{it} + \sum_{k \geq m} \delta_k D_{it}^k + \alpha_i + \gamma_t + \varepsilon_{it}$, where y_{it} represents a measure of annual earnings, X_{it} is a vector of time-variant characteristics, D_{it}^k are dummies indicating the k-th period, (before, during, or after the displacement), m denotes the baseline period, γ_t are year fixed effects, α_i is an individual fixed effect, and ε_{it} is the error term.

and Sullivan, 1993; Couch and Placzek, 2010), 12 percent for Germany (Schmieder, Wachter, and Bender, 2010), and 7 percent for Sweden (Eliason, 2011b).[3] However, the magnitude of these estimates depends heavily on factors such as industry, macroeconomic conditions, the institutional setting, and the definition of the control group. Hence, any comparison is problematic since the environment in which the displacements occur will generally differ. An overview of the previous literature can be found in Marsden and Ryxc (2010). Among the displaced workers there is also a great deal of heterogeneity, for example, Carrington (1993) find that those who switch industries following displacement have systematically larger earnings losses.

The decline in earnings could be due to unemployment, to individuals leaving the labor force, or to the fact that there is a decline in the displaced workers re-employment wages. Some studies find that the long-term effect is mainly driven by lower wages (Schmieder, Wachter, and Bender, 2010), while others find that the losses are mainly due to periods of non-employment (Hijzen, Upward, and Wright, 2010). The displaced workers' ability to regain comparable employment is likely to depend on factors such as their education, tenure, industry, and macroeconomic conditions. Given their work specification, military employees might find it particularly challenging to find comparable employment outside of the military sector.

2.2 Literature on military base closure

Military base closures have often generated concerns and objections from both the public and politicians in the local area (for example Warf, 1997). A military base is typically thought to be very important to the local labor market, securing employment opportunities for both military and civil servants, with wages that are primarily financed by the state rather than the region. Consequently, a reduction in military expenditures is expected to have substantial negative effects on the labor market in the affected regions. The closures could cause out-migration as well as higher unemployment rates, which would decrease the average income level. Shutting down a military base may also affect subcontractors, which would cause an additional decrease in the income level. As a result, there is a risk that the local tax base and the local governments' ability to provide local public goods and services will be affected.

There are a few studies that have examined how local communities have been affected by military base closures. Most papers find only small and economically insignificant effects on local growth and unemployment. Andersson, Lundberg, and Sjostrom (2007) study the effect of military base closures in Sweden from 1983 to1998 on municipal growth rate and net migration flows. They

[3] These papers all use administrative register data, and the long term effect is defined as being at least six years after displacement.

find no effect on either of the two outcomes, and argue that one potential explanation is that those previously employed at the military bases have found new employment within the region. Hooker and Knetter (2001) look at military base closures in the US from 1971 to 1994 and find that county-level employment costs were mainly limited to direct closures, with few spill-over effects, whereas average per capita income was little affected by the closures. For Germany, Paloyo, Vance, and Vorell (2010) find that base realignments and closures between 2003 and 2007 has had no significant impact on the economic development of the communities close to the base, measured as household income, regional output, and unemployment. Both Hooker and Knetter (2001) and Paloyo, Vance, and Vorell (2010) argue that one explanation for the absence of negative effects is the fact that the opportunity cost of the military bases, for example land and buildings, has often been overlooked. Many bases have been reused for civilian purposes, which can have positive implications for the community and local businesses and thus contribute to economic growth.

Despite the attention surrounding a base closure, few studies have examined what happens to the displaced workers following a base closure. As far as we know, the only study using individual-level register data to study this issue is Jakobsson (2010), who examines the labor market outcomes for employees in the military sector that were affected by the military base closures in Sweden 1999. Jakobsson (2010) looks at 11 selected municipalities and compares the outcome of military employees to non-military employees in the same municipality. The cross-sectional analysis shows that military employees on average perform better than the control group, regarding unemployment, labor income, and health (measured as sickness allowance and early retirement). However, the results cannot be interpreted as causal since the empirical specification does not account for unobservable heterogeneity, and non-military employees are unlikely to constitute a reliable control group.[4] Some studies have also used small sample surveys to follow individuals affected by a base closure. In the case of Sweden, Eriksson and Hallsten (2003) follow civil servants affected by three selected military base closures in 1997, and find that two-thirds had found new employment after three years, while seven percent were unemployed and 21 percent had retired. They find a lasting depreciation in several health indicators, primarily among males and regarding indicators related to anxiety. Even though their surveys have a panel structure, they lack a reliable counterfactual scenario, since there is no control group.

[4]Jakobsson (2010) uses a logistic regression where the dependent variable is a binary variable for different labor market outcomes (for example income above the average, receiving unemployment benefits) in 2006. The explanatory variable of interest is the binary variable indicating if the individual was employed by the military in 1998. Both demographic controls (age, gender, education, marital status) and municipality fixed effects are included.

3 Background

The objective of Swedish security policy changed drastically after the end of the Cold War. A foreign invasion aiming to occupy Sweden was no longer seen as possible, although attacks at more narrow objects in Sweden could not be dismissed. While the primary focus of the armed forces had previously been the ability to halt a military incursion, more focus was now given to participation in international peace-keeping interventions.[5] Hence, by the end of the 1990s it was decided that the Swedish defense, which had previously focused on the threat of invasion, should now be organized as an interventionistic defense. This implied cut-backs in spending on the national defense as well as closure of a number of military bases (Hedin, 2011).

Individuals working in the military sector are employed as either military or civil servants. Military servants consist of individuals with military education working as career officer, soldiers, or mariners. Civil servants include individuals working with administration, logistics, education, health care, and mechanics. During the time period that we study, the employment contracts of these two groups differed substantially. Civil servants could be dismissed due to a shortage of work, although seniority rules required that workers with shorter tenure were dismissed first. On the other hand, military servants employed prior to 1992 had contracts that guaranteed stronger employment protection (*fullmaktsanställning*), meaning that they could not be dismissed due to redundancy. Although it was possible to dismiss the military staff who did not have these contracts, the ministry of defense was unwilling to do so since it would require dismissing younger military servants, which would probably lead to a hiring freeze.[6] This was particularly unwanted, since the average age of the workforce was already thought to be too high. Rather, it was argued that it was necessary to encourage older military staff to resign voluntarily, and in association with the downsizing a number of rather generous initiatives to promote early retirement and career alternations were launched. For instance, military servants could be offered severance pay of the equivalent of three years of labor earnings (Hedin, 2011). Furthermore, military servants had the option of transferring to another unit or military base, whereas civil servants could not do so without applying for a new position.

The government announced which military bases would be closed in a number of government bills, of which the 1999/2000 bill amounted to one of the

[5] The required number of people doing military service, which was previously mandatory for all young males, also decreased during this period, making the compulsory element less effective. In 2010 the peacetime military draft was abolished.

[6] During 2005-2007 it was possible to dismiss military servants due to shortage of work only if they had declined an offer to move to another military base (Hedin, 2011).

largest reorganizations of the Swedish Armed Forces in modern times.[7] The defense proposition was announced in the fall of 1999, and adopted by the parliament in the spring of 2000. The bill resulted in the closure of 24 battalions, forces, and regiments, as well as a number of headquarters compounds. In some municipalities all military units were closed down, while in other places only a few units were affected.[8] Before deciding which bases to close down, the government had declared that consideration should be taken to military, economic, regional and environmental pros and cons (Proposition 1999/00:30). Due to strategic reasons, there was a clear aim to have military bases geographically scattered over the country. At the same time, the fact that these considerations should be weighed against cost efficiency (such as necessary investments and synergy effects), environmental factors (for example permits and training sites), and regional political considerations, made it difficult to predict which units would be closed down. For instance, the new battle airplane JAS 39 Gripen had just entered service within the Swedish Air Force, which required the air force to adjust. Major investments had recently been made to accommodate the new airplanes at the F10 Wing in Ängelholm. Yet the government decided to close down the F10 Wing, whereas they kept the nearby F17 Wing open, which had not yet been prepared to accommodate the new airplanes. Although the proposition was preceded by much debate and speculation, we argue that there was substantial uncertainty around which units would be affected, and that individuals employed by the military in 1998 could not foresee the upcoming proposition at that time.

The closures generated objections and protests from both the public and politicians in the affected communities, since they were expected to have vast negative consequences. Local politicians attempted to overrule the decision, and at some of the affected places the inhabitants demonstrated against the decision to close their military base.[9] The government started adjustment programs in 1999 involving seven of the affected municipalities as well as two other municipalities (Karlsborg and Karlskoga) also affected by the defense restructuring.[10] Trying to evaluate the short-term effect of the programs, Falkenhall (2004)

[7] The key government bills during this period were: Proposition 1991/92:102, Proposition 1995/96:12, Proposition 1999/00:30, Proposition 2004/05:5, and Proposition 2008/09:140.

[8] All closures were supposed to be implemented by 2000 or 2002, but the actual procedure appears to have been done in a more incremental manner.

[9] These protests received much attention in the media, for example SvD (Nov 11, 1999), TT (Sept 23, 1999), TT (Sept 30, 1999), DN (Nov 1, 1999) and TT (June 27, 1999). Svenska Dagbladet (SvD) and Dagens Nyheter (DN) are two of the largest national daily newspapers in Sweden and the news agency TT (Tidningarnas Telegrambyrå) is the national wire service in Sweden.

[10] Karlsborg had been affected by military base closures in the beginning of the 1990s and the employment level in Karlskoga had decreased due to reductions within industries related to the armed forces. The other seven municipalities were: Boden, Falun, Gotland, Härnösand, Hässleholm, Kiruna, and Sollefteå.

concludes that they appear not to have had any major impact.[11] It is obviously difficult to determine what long term effect the programs had on employment in the affected municipalities, but it is important to note that they were not directed towards the newly displaced workers. Furthermore, to the extent that the programs were successful, our estimations should underestimate the true effect of the closures.

Between 1999 and 2010, somewhat more than 19,700 employees resigned, of which only 1550 transferred to old age pension. The vast majority did so with some form of early age pension or due to their own request (Hedin, 2011). Programs that promoted career alternations were not very successful, and Blomsterberg and Kadefors (2009) argue that this can partly be explained by the fact that military servants have a strong professional identity and are thus reluctant to change occupation.

4 Sample

The data used for the analysis stems from the comprehensive database GeoSweden, which is administered by the Institute for Housing and Urban Research at Uppsala University. It contains variables from several different registers (including the income and employment registers) that are collected by Statistics Sweden. The database covers all individuals of working age permanently living in Sweden. It is collected annually and contains information on individual characteristics such as year and country of birth, marital status, the number of children in the household, as well as the individuals' level and type of education. Also, it contains information on pre-tax income from different sources, disposable income, and employment. The data links all individuals to their employers, providing information on which sector the individual is employed in and where the establishment is located.[12] This information makes it possible to identify all individuals employed by the military.

As was clear from the introduction, the military sector has decreased significantly from the second half of the 1990s onward, at least in terms of the size of the workforce. Military employees are likely to differ from the rest of the

[11] The programs were supposed to relocate 1280 government jobs to the affected municipalities and, by grants to private companies, create 1000 private job opportunities. Evaluating the effects of these programs Falkenhall (2004) finds that only 60 percent of the government jobs had been relocated, whereas few private jobs had yet been realized. The grants directed at private companies were only paid out if new hiring occurred. By the end of 2002, the number of new hires only amounted to 62. The affected municipalities were also given general regional policy aid, and most of it was used to finance projects, such as pilot studies. Falkenhall concludes that it was unclear if these would bring about permanent employment when the projects ended or ran out of funds.

[12] The information on employment sector builds on five-digit industry codes (SNI) that are constructed in accordance with EU standards.

population, not least regarding their skill sets. As Table 1 shows, individuals employed by the military are different in terms of a number of observable characteristics. For example, they are more likely to be male, have some post-secondary education, and less likely to be born outside the OECD. Also, they have both higher labor earnings and disposable income, compared to individuals employed in other (non-military) sectors.[13]

Table 1: Summary statistics, by sector (individuals)

	Employed outside military	Employed in military
Male	0.499	0.782
Age	40.424	41.334
Parent	0.396	0.356
Compulsory schooling or less	0.252	0.123
Secondary education	0.494	0.350
Post-secondary education	0.248	0.513
Immigrant, non-OECD	0.079	0.022
Labor income	143,227	237,364
Disposable income	110,688	144,753
Social assistance	1293	160

For the analysis we sample all individuals who were employed by the military in 1998 (one year prior to the announcement of the defense proposition), and construct a panel where we follow these individuals from 1995 to 2008. By starting the sampling period in 1995, we exclude the recession years in the early 1990s, and we increases the probability that most of the younger individuals sampled in 1998 are in the military in the pre-reform period. Since wages often do not reflect young workers' productivity and long-term earnings potential, fixed effects strategies are problematic when analyzing young workers (Wachter and Bender, 2006). Hence, in the analysis, we also limit the sample to individuals who are between 30 and 50 years old in 1998. This restriction ensures that the sampled individuals have a strong attachment to the labor market during the whole sample period.[14] It also reduces the chance that some of the younger individuals are enrolled in school at the start of the sample period, and that the older individuals are able to collect old-age pensions at the end of

[13] The numbers in Table 1 represent averages across time between 1995 and 2008, and include all employed individuals age 18 to 64. Income is given in the 2011 year value.

[14] Including individuals aged 20 and above in 1998, gives us similar results in the post-treatment period, but introduces pre-treatment difference between the treated and untreated individuals.

the period.[15]

We exclude all individuals working in municipalities where the military base was about to close down completely as a result of the earlier defense proposition, announced in 1996. We also exclude a number of military employees registered as belonging to a military establishment in a municipality where there is no official military base, since their treatment status is unclear. These military employees could belong to the home guard or to an official military base, but working in an affiliated unit in another municipality. In line with Jakobsson (2010), we also exclude municipalities that were affected by the proposition in 1999, but only had minor changes. Some municipalities only had administrative changes (Umeå and Örebro), or had their units relocated to a nearby area (Stockholm and Södertälje). Other places only experienced closure of headquarters compounds or very small units (Enköping, Göteborg, Härnösand, Luleå, Haninge, and Ronneby), and these are also excluded. Excluding municipalities with only minor changes does not have much effect on our main estimates, but assures that we are looking at closures of military units similar to previous studies based on aggregate data. The restrictions do, however, have substantial implications for the sample size, which is reduced from almost 33,500 individuals to around 10,000 in the final sample (when pooling all years our baseline estimation sample contains 156,193 observations).

Our sample consists of individuals who were employed by the military in 1998. The treatment group consists of individuals who in 1998 were employed at a military base that was located in a municipality that was affected by the defense proposition in 1999, whereas the control group consists of individuals working in an unaffected municipality in 1998. However, individuals working at an untreated military base in 1998 might transition to a base in a treated municipality in 1999. This is especially common in cases where military servants get their training in one municipality and then transition to work at a military base in another municipality. We argue that these individuals should also be seen as treated, and thus individuals working in a treated municipality in 1998 or 1999 are defined as being in the treatment group.

An implicit assumption is that the municipalities that were not targeted by the proposition were not exposed to the treatment in any indirect way. This could occur, for example, if military staff that transferred from the affected units to military bases in the control group affected the wages in the control group. However, since wages in the military sector are determined primarily according to central agreements, rather than through individual negotiations, we argue that such indirect effects are unlikely. However, it is possible that workers at military bases that were not directly affected still reacted to the proposi-

[15] It should also reduce the number of individuals who are able to start collecting early retirement benefits, since individuals are not able to get early retirement before the age of 55 (Hedin, 2011).

tion by changing their behavior. For example, the fact that colleagues at other regiments lost their jobs might have caused the unaffected workers to perceive their current employment as less secure, although it was not directly threatened. As a consequence, workers at unaffected bases might start searching for jobs in other sectors, and thus leave the military sector at a higher rate than they would otherwise have done. If this is the case, it is likely to attenuate the estimates that we obtain in our analysis, and our estimates should thus be interpreted as a lower bound of the true effect.

We prefer using army employees in unaffected municipalities as our control group rather than individuals who live in the affected municipalities but work in other sectors (such as in Jakobsson, 2010), since individuals in the military often differ from individuals in other occupations. Thus, only looking at differences between occupational groups within municipalities would probably make it difficult to find a valid comparison group, especially for the sub-sample of individuals with military education. A list of the municipalities in the treatment and control groups is presented in Appendix A.

In the baseline sample, we do not exclude individuals who are affected by the defense proposition that was announced in 2004. This means that some of the individuals in our sample are exposed to another wave of military base closures five years after the start of the initial treatment. This primarily affects individuals in the treatment group, since the 2004 proposition primarily mandated further cutbacks in municipalities where reductions had already been made in 1999 (see Appendix A). The fact that there is a second wave of treatment means that we need to be careful in distinguishing short- from long-term effects in our analysis, since any effect that arises after 2004 can be an effect of either the treatment that starts in 1999 (which is the effect we are primarily interested in), or an effect of the treatment that starts in 2004. The control group is largely unaffected by the 2004 proposition, since only three of the control municipalities receive any treatment in that wave.[16] If there is an effect on labor income and employment in these control municipalities it will attenuate the effect that we estimate for the earlier proposition. To obtain alternative estimates of the long-term effects, which are not contaminated by the second wave of base closures, we also estimate our model on a restricted sample where we limit the treatment group to military bases that were closed completely by the proposition in 1999. These bases are located in Kristianstad, Hässleholm, Falun, Sollefteå, Ängelholm, Norrtälje and Kiruna. Naturally, these units could not be affected by future propositions, and thus we can more credibly identify the true long-term effects of the 1999 proposition.

[16] Four municipalities in the treatment group and four in the control group were affected, but the number of units closing down was considerably smaller in the control group.

5 Summary statistics and pre-treatment trends

Figure 1 shows the average number of military employees in the municipalities that are included in our analysis, divided into the treatment and control groups. For this illustration, we define the treatment group as consisting of all municipalities that were affected by the defense proposition in 1999, while the control group consists of municipalities with military units that were unaffected by the proposition.[17] First, it is clear from the graph that there is a sharp decline in military personnel in the treated municipalities from 1999 onwards (going from a municipality average of approximately 850 military employees in 1999 to an average of around 300 in 2008). There does seem to be some relocation of workers, considering that the number of military employees in the untreated municipalities increases in response to the decrease in the treatment group. This is not surprising given that it was possible for military servants at the affected units to transfer to the unaffected military units. However, as discussed above, it is unlikely that this causes a threat to our identification, by for example, affecting the wages of either the control or treatment group. If there is a behavioral effect in the control group, it is likely to attenuate our estimates.

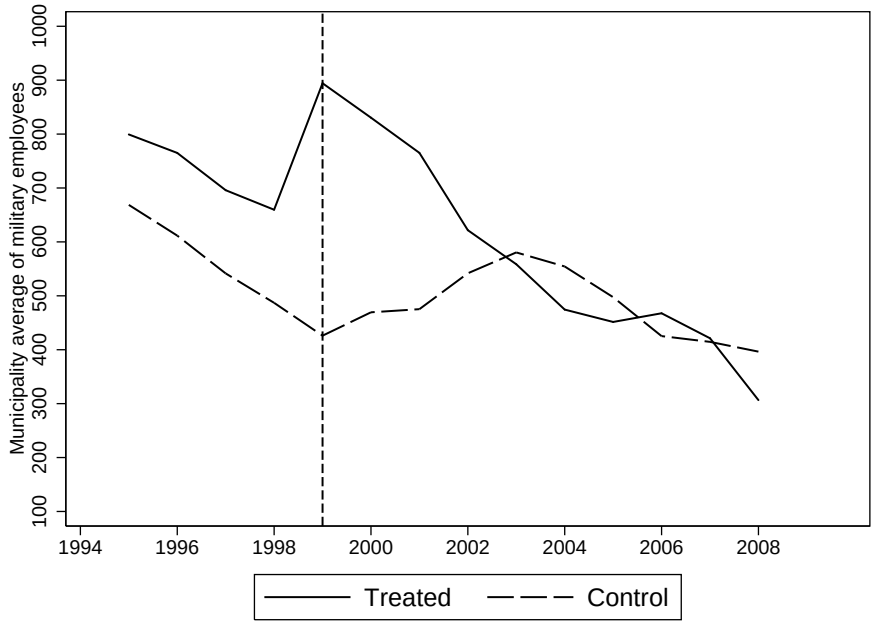

Figure 1: Military employment, by treatment status

[17] Note that this definition differ somewhat from the definition used in the empirical analysis, as defined earlier. The reason is that we want to examine how the number of military personnel develops over time in the two types of municipalities.

Since we are interested in comparing the change in the treated group to that of the control group in a difference-in-differences setting, we need to confirm that the two groups are as similar as possible prior to the treatment. Table 2 shows summary statistics at the municipality level, divided by treatment status, and we also present a t-test for the statistical significance of the differences between the groups. Looking at the t-statistics in this table, there is only a significant difference regarding the share of individuals with low education. This tells us that on an aggregate level, municipalities that eventually become treated are very similar to other municipalities that also host military units, but that will not be treated according to the 1999 defense proposition.

Table 2: Municipality characteristics in 1998, by treatment status

	Mean, control	Mean, treated	Difference	T-statistic
Male	0.521	0.509	0.012	1.193
Age	39.408	39.672	−0.264	−1.048
Compulsory schooling or less	0.171	0.203	−0.031	−2.468
Secondary education	0.504	0.513	−0.009	−0.507
Post secondary education	0.321	0.281	0.040	1.577
Immigrant, non-OECD	0.021	0.017	0.004	0.726
Tax base	124,867	118,383	6483	1.682
Disposable income	116,937	111,889	5048	1.790
Unemployment	0.156	0.165	−0.008	−0.504
Population	32,417	29,962	2454	0.257
Net migration inflow	5.769	25.750	−19.981	−0.236
Share employed by military	0.051	0.045	0.007	0.364
N	13	16	−3	.

Since we use data at the individual level in our analysis, Table 3 presents a similar table based on the individuals used in our econometric analysis. Clearly, there are more differences at this level of aggregation than at the municipality level. Workers in municipalities that are affected by the proposition have a lower labor income, and are more likely to be males, and have a low education. On the other hand, there are no significant differences regarding disposable income or unemployment benefits. If individuals differ in observable characteristics, there could also be differences concerning unobservable features. While our empirical specification controls for all unobservable characteristics that are constant over time, we cannot account for those that varies over time. Hence, if the variations only represent differences in levels, and not in trends, they do not cause a serious threat to our identification strategy.

To examine if the pre-treatment trends of the outcome variables are similar, we plot the raw yearly averages of labor income and unemployment benefits by treatment status in Figure 2. The vertical line in the figure denotes 1999, the year when the defense proposition was announced, and hence we would like to see parallel trends for the treated and the untreated groups up until 1998.

Table 3: Individual characteristics in 1998, by treatment status

	Mean, control	Mean, treated	Difference	T-statistic
Male	0.749	0.817	−0.067	−7.583
Age	39.930	40.105	−0.175	−1.229
Parent	0.589	0.573	0.016	1.445
Compulsory schooling or less	0.073	0.094	−0.020	−3.260
Secondary education	0.325	0.312	0.013	1.266
Post-secondary education	0.602	0.594	0.008	0.761
Immigrant, non-OECD	0.003	0.001	0.002	2.620
Labor income	314,029	298,921	15,107	6.176
Disposable income	123,316	122,143	1173	0.325
Unemployment compensation	1829	1627	201	0.820
N	2859	7103	−4244	.

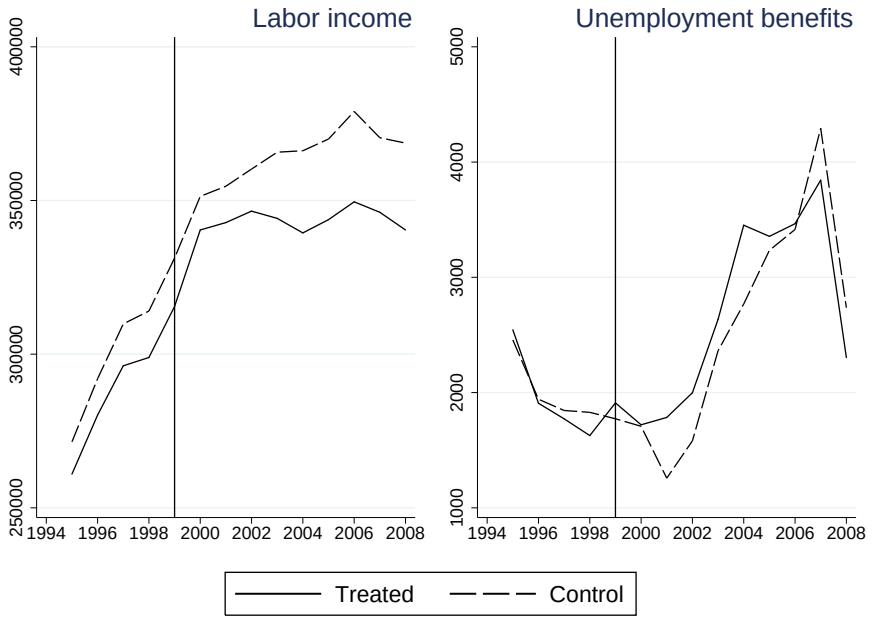

Figure 2: Pre-treatment trends in the outcome variables

For labor income, the series for the two groups follow each other closely and any differential pre-reform trends are hard to detect. From year the 2000, the two series also starts to diverge, with the average labor income in the treated group being lower than the average labor income in the untreated group in all years after 2000. For unemployment benefits, the pattern is not as clear-cut, the two lines start to diverge some years prior to the treatment, and unemployment benefits are slightly lower in the treatment group. However, from 1998 and onwards, the level of unemployment benefits increases in the treated group, and is clearly above that in the control group. Around 2005, unemployment again becomes higher in the control group, which could be explained by the fact that some of the units in the control groups then experiences cut-backs following the 2005 defense proposition. From Figure 2, we hence draw the conclusions that the pre-treatment trends in the outcome variables are quite similar between the treated and untreated groups, and that we see a divergence in the trends of the variables for the two groups (for both labor income and unemployment) in the post-reform period. In the analysis, we will strengthen this conclusion by showing estimates of the pre-treatment differences between the two groups, where we include the full set of controls.

6 Econometric specification

In the econometric analysis, our aim is to isolate the causal effect of military base closures on military personnel's future labor earnings and unemployment probabilities. To accomplish that, our strategy is to use a difference-in-differences approach where we compare the difference in labor market outcomes over time (before and after the defense proposition in 1999) between treated and untreated individuals. The model to be estimated is hence given by:

$$y_{ijt} = \alpha' X_{ijt} + \sum_{k \geq 1996}^{2008} \beta_k D_{iw}^k + \gamma' Z_{jt} + \lambda_i + \lambda_j + \lambda_t + \varepsilon_{ijt} \qquad (1)$$

The outcome of interest, y_{ijt}, represents yearly labor market income or yearly income from the unemployment insurance. We construct a dummy variable that indicating treatment, D_{iw}, where i denotes an individual employed by the military in 1998, w and j denote the municipality in which the individual works and lives, respectively, and t denotes year. D_{iw} is equal to one for all treated individuals, and zero otherwise. We interact the treatment variable with all year dummies, and estimate separate treatment effects, β_k, for all years before, during and after treatment. Thus, we can show that the parallel trend assumption is fulfilled as well as examine all dynamics of the effects of the 1999 defense proposition. To increase precision, we also include a vector of observed individual characteristics, X_{ijt}, including age, and indicators for family com-

position and educational attainment. In order to account for the fact that there might be unobserved differences between treated and untreated employees, we include individual fixed effects, λ_i. This will net out all unobserved factors that vary between individuals but are constant over time, such as differences in innate ability or ambition. To account for local labor market conditions, we include a vector of observed municipality-specific variables, Z_{jt}, to control for observed characteristics that varies over time within each municipality, and a municipality-specific fixed effect, λ_j, to control for unobserved features that are constant over time. These fixed effects are defined based on the municipality of residence, as we believe that it is more important to control for characteristics of the area where the individual lives, than the area where she works. Also, adding dummy variables for the municipality of the workplace is problematic since this variable will be missing for individuals who are not employed. The vector of municipality characteristics includes per capita taxable income, the municipal unemployment rate, and the size of the working age population. Adding these covariates to the model will affect the interpretation of our estimates, since they are all possible intermediaries which can both be affected by the treatment, and have an effect on our outcome variables. When these variables are included in the model, our estimated coefficients will represent the effect of the treatment above and beyond the effect that is transmitted through for example the aggregate unemployment level in the municipality. Excluding the municipality level controls would imply that the estimates should be interpreted as the total effect, including both the direct and indirect impact.[18] Finally, λ_t are year fixed effects that captures aggregate shocks that affect all municipalities and individuals in the same way. Standard errors are clustered at the individual level.[19]

7 Baseline results

In this section, we present results from the baseline specification, as given in equation (1). The results for the parameters of main interest, β_k, will be presented graphically, showing the point estimate for each year over the period 1996-2008 accompanied by a 95 percent confidence interval.[20] Showing the point estimates for the years preceding the 1999 defense proposition is a way to check if our econometric specification picks up any pre-treatment differences between the treated and the control group. The sample consists of all individuals employed in the military sector in 1998 and who were, at the time, between 30 and 50 years old.[21]

[18] Excluding Z_{jt} from the model does not have any qualitative impact on our results, if anything, the effect becomes slightly larger.

[19] Our results are robust to clustering the standard errors on the municipality of residence.

[20] The complete estimation results are presented in Appendix C.

[21] All sample restrictions are described in section 4.

7.1 Results for all personnel employed by the military

Figure 3 shows the effects of the defense proposition on taxable labor income (to the left) and unemployment benefits (to the right), estimated on our sample of military employees. Starting with earnings, there seems to be no immediate effect from the military base closures on labor income. From year 2003, there is however a significant and negative effect on labor income. In 2003, the point estimate indicate that the individuals in the treated group earned almost SEK 10,000 less in yearly labor income compared to those in the control group. In 2004 they earned approximately SEK 15,000 less and in 2008 they earned close to SEK 20,000 less in yearly labor income. These effects are economically significant given that the average labor income in the treated group was SEK 298,921 in 1998, and thus the effect corresponds to almost a five percent reduction of labor income five years after the proposition.[22] The lack of an immediate effect could potentially be explained by the fact that the closures were implemented in an incremental manner, and the fact that military servants who chose to leave their positions voluntarily could get severance payments, which are counted as labor income in the income registers. The fact that the closures were not immediate, makes the distinction between short- and long-term effects more uncertain. However, the implementation period did not last more than three years in most of our treated units.[23] As mentioned earlier, some of the individuals in our treatment and control groups were affected by the next defense proposition in 2004. Thus, the estimated effects from 2005 and onwards might be a mixture of effects from both the 1999 and 2005 propositions. Finally, it is reassuring to note that the estimated β_k in the years preceding the proposition are fairly constant and close to zero, indicating that there are no pre-treatment differences between the treatment and the control group and hence that the parallel trends assumption holds.

Turning to unemployment benefits, there seems to be no effects from the defense proposition. The estimated β_t is mainly insignificant and close to zero, both before and after treatment. These results are robust to instead defining the dependent variable as an indicator that is equal to one if the individual receives any income from the unemployment insurance, or income above one basic amount (which in real terms corresponds to around SEK 44,000 or USD 6700 in 2012).

[22] The results for labor income are robust to using the log of labor income as the outcome variable, instead of the actual labor income.

[23] See figures 8 to 11 in Appendix B.

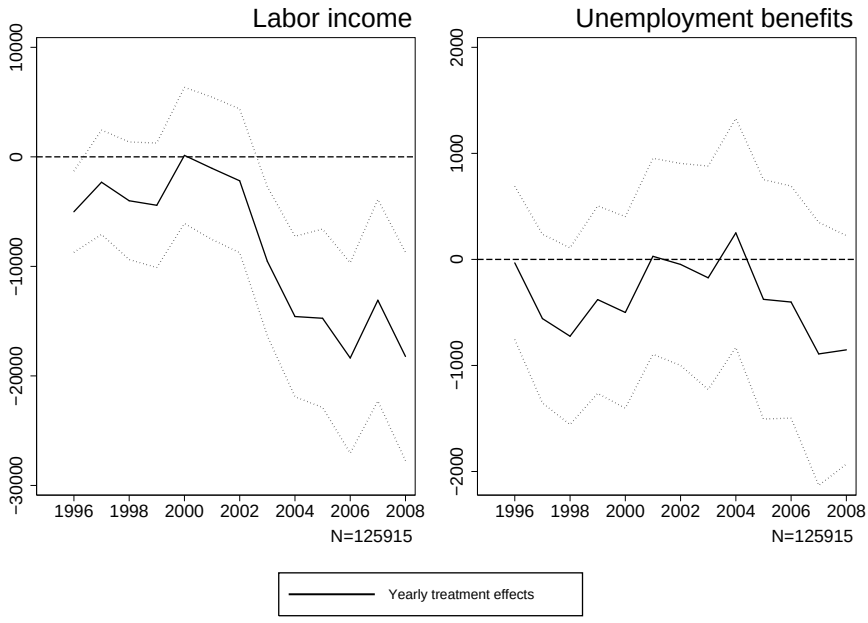

Figure 3: Effects on labor income and unemployment

7.2 Military and civil servants

There are two types of personnel employed by the military, those with a military training (which we label military servants) and those with a position that does not rely on any military training (which we label civil servants). It is reasonable to assume that the closing of a military base might have quite different effects on these two types of personnel. For one thing, they have different types and lengths of education. They also differ in other background characteristics (see below). Finally, there are contractual differences between military and civil servants, whereas military servants could transfer to other units or military bases, civil servants could not do so without applying for a new job. As far as we know, separate effects for these two groups of employees have not been studied before, probably due to lack of good enough data.

Information on each individual's educational track provides information on whether an individual have completed an education program provided by the military sector or not. The data on educational track only tells us an individual's highest degree (available from 2001 and onwards) and which year she or he attained it. We assume that all individuals that have completed any training within the army are also employed as military servants, and that individuals who have not had military training, or have a civil degree that is higher than their military degree are employed as civil servants. This should give us a reasonable

proxy for their employment contract.[24]

To examine if the two groups of personnel differ in other background characteristics than education, we compare treated and untreated military servants and treated and untreated civil servants, respectively. These statistics are shown in Table 4 (military servants) and Table 5 (civil servants). Compared to the civil servants, the military servants earn more, get less unemployment benefits, and are to a larger extent males, parents, and highly educated. There are also clear differences between the treatment and control groups within each occupational group. As in the aggregate analysis, we hope to capture these differences by controlling for individual fixed effects in the analysis.

Table 4: Individual characteristics in 1998 (military servants), by treatment status

	Mean, control	Mean, treated	Difference	T-statistic
Male	0.982	0.985	–0.003	–0.811
Age	39.909	39.871	0.038	0.180
Parent	0.626	0.625	0.002	0.122
Compulsory schooling or less	0.000	0.000	0.000	.
Secondary education	0.032	0.022	0.011	2.037
Post-secondary education	0.968	0.978	–0.011	–2.037
Immigrant, non-OECD	0.001	0.000	0.001	1.746
Labor income	370,904	349,230	21,674	6.818
Disposable income	131,245	126,422	4823	2.682
Unemployment compensation	130	279	–149	–0.927
N	1170	3566	–2396	.

In Figure 4, we show the results from estimating the model separately for military and civil servants. This shows that there are important differences in how the two groups are affected by the proposition and the cut-backs. The significant and negative effect found on labor income is completely driven by the civil servants, where a significant negative effect, that grows larger over time, is visible from 2001 and onwards. In 2004, the year before the next defense proposition was adopted, the point estimate indicates that the 1999 defense proposition lead the treated civil servants to earn around SEK 18,000 less in yearly labor income, compared to the untreated civil servants. This effect corresponds

[24]Using information on what year the individual finished their highest degree, we can track the information backwards for all individuals that got their degrees prior to 1998 (the sampling year). However, for individuals who finished their highest level of military education between 1998 and 2001, we cannot determine what type of education they had in the sampling year. The fact that we only sample individuals who are between the ages of 30 and 50 in 1998 implies that most of them had finished their education by that time. Thus, we find it reasonable to assume that all individuals who had some military training in 2001 had in 1998 as well. However, it is possible that some of them studied to get a civil education between 1998 and 2001. These data limitations imply that we are likely to underestimate the number of military servants.

Table 5: Individual characteristics in 1998 (civil servants), by treatment status

	Mean, control	Mean, treated	Difference	T-statistic
Male	0.588	0.646	−0.058	−4.089
Age	39.945	40.341	−0.396	−2.019
Parent	0.562	0.521	0.042	2.827
Compulsory schooling or less	0.124	0.189	−0.064	−5.830
Secondary education	0.527	0.604	−0.077	−5.264
Post-secondary education	0.349	0.206	0.143	11.235
Immigrant, non-OECD	0.004	0.001	0.003	1.929
Labor income	274,630	248,200	26,429	8.723
Disposable income	117,823	117,829	−6	−0.001
Unemployment compensation	3005	2985	19	0.046
N	1689	3537	−1848	.

to more than seven percent of their pre-treatment income. The point estimates for the military servants hover around zero and are statistically insignificant in most of the years. It is only in 2002 that one can observe a significant and positive effect on their labor income.

The effects on unemployment benefits are insignificant in the absolute majority of the years and the point estimates are very close to zero for both military and civil servants. Hence, for the civil servants we find a negative impact of the defense proposition on their labor income, but no effect on their unemployment benefits. One possible explanation is that they work full-time, but get lower-paid jobs after the 1999 proposition or jobs where they work fewer hours than previously. Another possibility is that they, to a larger extent than the control group, leave the workforce, for instance because they study or get early retirement. However, this does not seem to be the case. We also estimate the effects on employment (defined as having any or more than two basic amounts of labor income) as well as income from student grants, sickness-allowance, and early retirement, and we find no significant effects.[25] This suggests that the treated civil servants are employed to the same extent as the untreated civil servants after the proposition, although they have lower wages.

8 Sub-group analyses

In this section we will conduct two sub-group analyses with the aim of examining what the effects are on individuals that were hit in specific ways by the 1999 defense proposition. First, we will examine what the effects are on the treated individuals that actually left the military sector in the baseline analysis. It is quite likely that these individuals were displaced, and that they are also driving the effects. Second, we will examine what the effects are on those who

[25] These results are available on request.

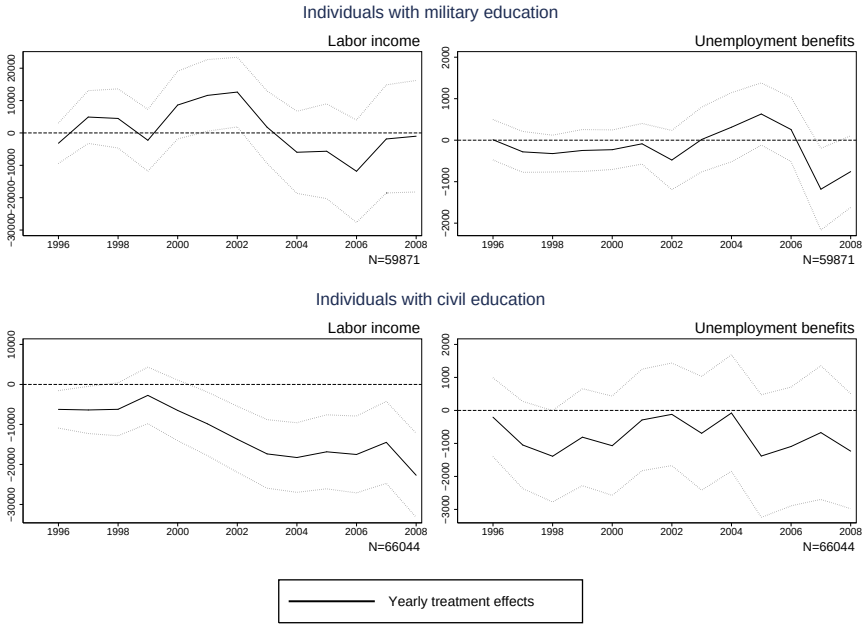

Figure 4: Effects on labor income and unemployment, by profession

were employed by the military in one of the seven municipalities that suffered a complete base closure following the 1999 defense proposition.

8.1 Effects on personnel leaving the military

In the baseline analyses, the treatment effect was estimated using all individuals in the treatment group. However, some of the military personnel that was affected by the 1999 defense proposition, in the sense that they were employed at a base that was considered for downsizing, stayed in the military sector. Either their unit was not affected, or their unit closed down, but they transferred to another one. In fact, 63 percent of the employees at the treated units were still employed by the military sector in 2004.[26] It seems reasonable to assume that the effects from the proposition were quite small for those who stayed in the military, while any effects from the reform are likely to be driven by the individuals who left the military sector. To examine this, we re-estimate the baseline model for the groups of stayers and leavers. "Leavers" are defined as individuals who are not working in the military sector in 2005, while "stayers" are defined as individuals who are working in the military sector in 2005 (re-

[26] Around half of all treated individuals who remain employed in the military in 2004 are working in a different municipality. Those who moved to a base in another municipality were military servants to a much larger extent than civil servants.

gardless of which municipality they are working in). Choosing to leave or stay could of course be endogenous to the individual's outside option, but individuals who worked at the treated bases and then left the military are likely to have been affected by the proposition.

Starting with the military servants (cf. the upper panel in Figure 5), we note, as expected, that there are mainly no significant effects of the defense proposition on the stayers' labor income and their unemployment benefits. For leavers', on the other hand, there seems to be significant effects on labor income following a cyclical pattern over time, starting with an immediate and significantly positive effect between 2000 and 2002, followed by a significant and negative effect between 2004 and 2005. In the latter years, the decreased labor earnings are accompanied with a significant and positive increase in the leavers' unemployment benefits. A plausible explanation of these results is that during the first three years the dismissed military servants were compensated with income, meaning that they could top up their earnings by starting to work outside the military sector, and that the negative effects on labor income and unemployment benefits is visual once this compensation period has ended.

Turning to the civil servants (cf. the lower panel in Figure 5), we note that while both the stayers' and leavers' labor income are affected negatively by the 1999 defense proposition, the effects on the leavers' labor income are stronger and more immediate than the effects on the stayers' labor income. It is quite possible that the downsizing had an impact on the stayers' career opportunities at the bases. The effects on the unemployment benefits also trend in different direction up until 2004, with positive effects on the leavers' and negative effects on the stayers' unemployment benefits.

To understand why re-employment labor income is lower than the income received while working in the military sector, it is of course relevant to look at in what other labor market sectors previous military employees work some time after the base closures. Our data shows that among both military and civil servants who were employed in the military in 1998 but not in 2004, it is most common to be working in the educational sector. However, it is also common to work in public administration or in organizational consulting. Among civil servants it is also common to work in the health care or the construction sector. A crude look at the data tells us that average labor earnings in these sectors are lower than the average earnings among military employees (particularly compared to the military servants).

8.2 Effects on personnel employed at military bases that faced total closures

Some of the military bases were hit harder than others by the 1999 defense proposition, suffering complete closures. To examine if workers at these units were hit harder, we re-estimate our model using only those individuals in the

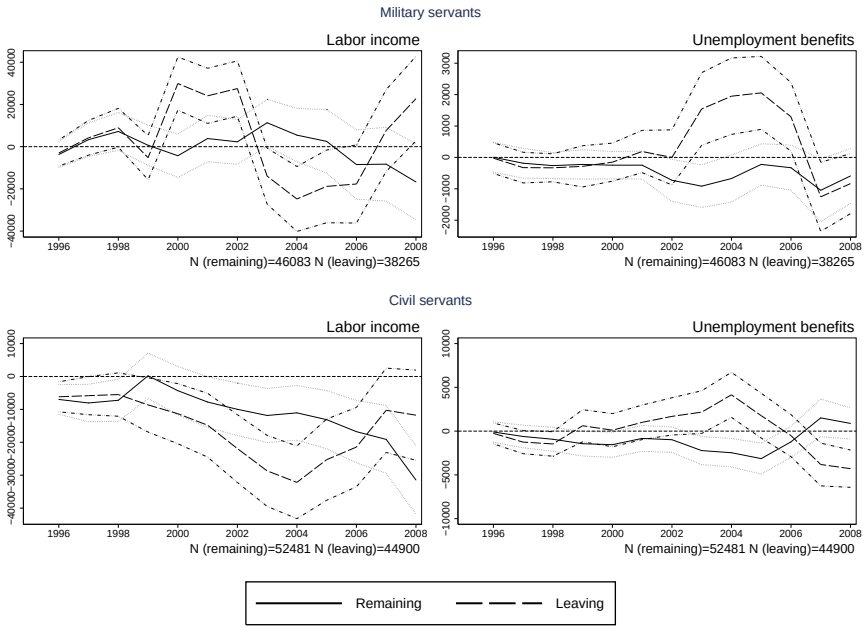

Figure 5: Effects on labor income and unemployment, individuals remaining in the military sector at least until 2005

treatment group that were employed at a base where all units closed down.[27] To get a clean estimate, we exclude individuals from the control group who worked in one of the municipalities that were affected by the 2004 proposition. The yearly changes of the number of military employees in the municipalities that experienced full closures are shown in Figures 8 through 11 in Appendix B. Compared to the baseline analysis, all employees at these bases were directly affected by the proposition. Yet, 50 percent of them were still working within the military in 2004, and civil servants chose to stay to the same extent as military servants.[28]

Figure 6 shows that the time pattern is very similar to that in the baseline analysis (see Figure 4), but there are two main differences. First, for military servants there seems to be an immediate and positive effect on labor income

[27] These municipalities were Ängelholm, Falun, Hässleholm, Kiruna, Kristianstad, Norrtälje, Sollefteå.

[28] This is obviously quite surprising given that civil servants had to apply for a new position, whereas military servants did not. There could be some measurement error regarding who is a military or civil servant, since their position is only based on their highest education. However, the fact that we only observe an effect of severance pay for military servants, just as expected, indicates that the proxy works rather well. It is possible that it was quite difficult for some civil servants, such as mechanics, to find work outside of the military sector.

(in the years 2000-2002), which is followed by a negative effect in the longer run(significantly negative for the years 2004-2008).[29] These results are well in line with the results for the military servants that had left the military sector (cf. Figure 5) in the baseline sample. Second, the magnitudes of the estimated effects are larger for both civil and military servants. The effect in 2004 on earnings for military servants is around SEK 23,000 less in yearly labor income (corresponding to almost seven percent of their pre-displacement income), while civil servants experience an income drop of around SEK 30,000 (more than twelve percent). By 2008, this had changed to almost nine percent for military servants and more than eleven percent for civil servants. It is also noteworthy that the treated civilians' labor income appears to recover up to 2007, and then decrease in 2008, the year of the Financial Crisis. This could indicate that displaced workers are more sensitive to subsequent macroeconomic shocks, in line with Eliason and Storrie (2006).

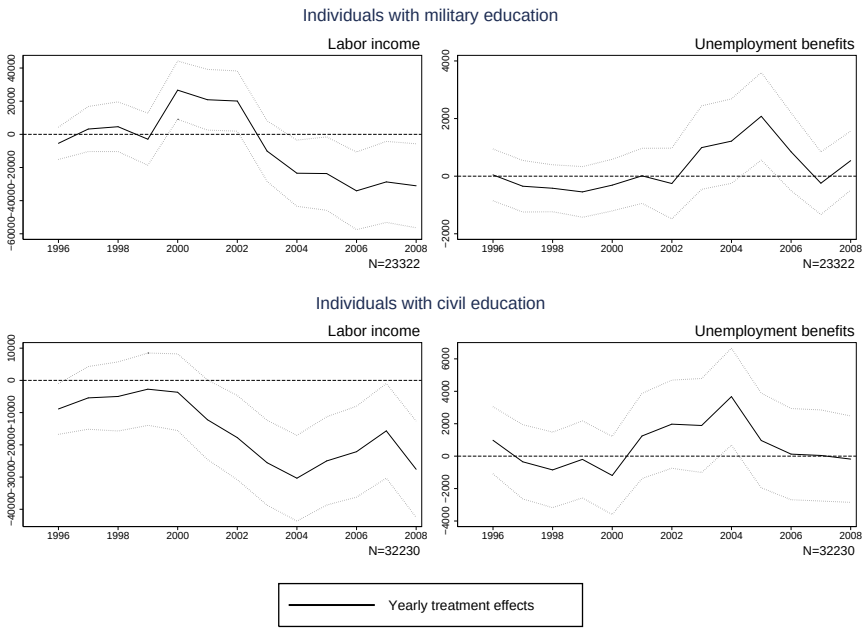

Figure 6: Effects on labor income and unemployment, treatment restricted to bases that are closed following the proposition

Differentiating between the treated individuals who had left or stayed in the military as of 2004, shows that the results in Figure 6 are driven by the leavers. This indicates that individuals who were employed at bases that closed down

[29] When using the sample of total base closures it is more reasonable to talk about long-term effects of the 1999 proposition also after 2004, the reason being that the treatment group is not affected by the 2005 defense proposition.

completely but moved to a base in another municipality did not experience any large reductions in labor income. Hence, the decline that we observe is mainly driven by the employees that leave the military.

8.3 Imposing a three year tenure restriction in 1998

When constructing the sample for the baseline analyses, we imposed no tenure restriction. In previous literature on displacements and mass layoffs (Jacobson, LaLonde, and Sullivan, 1993; Couch and Placzek, 2010), it is common to restrict the sample to individuals who have been employed by the same firm for a number of years. Restricting the sample to workers with tenure guarantees that the individuals have a strong attachment to their workplace and are not newly employed. To see if tenure has any impact on our results, we limit our sample to individuals with at least three years of tenure in the military sector. That is, the military sector was their primary employer during all years between 1995 and 1998. The results, shown in Figure 7, are very similar to the baseline results.

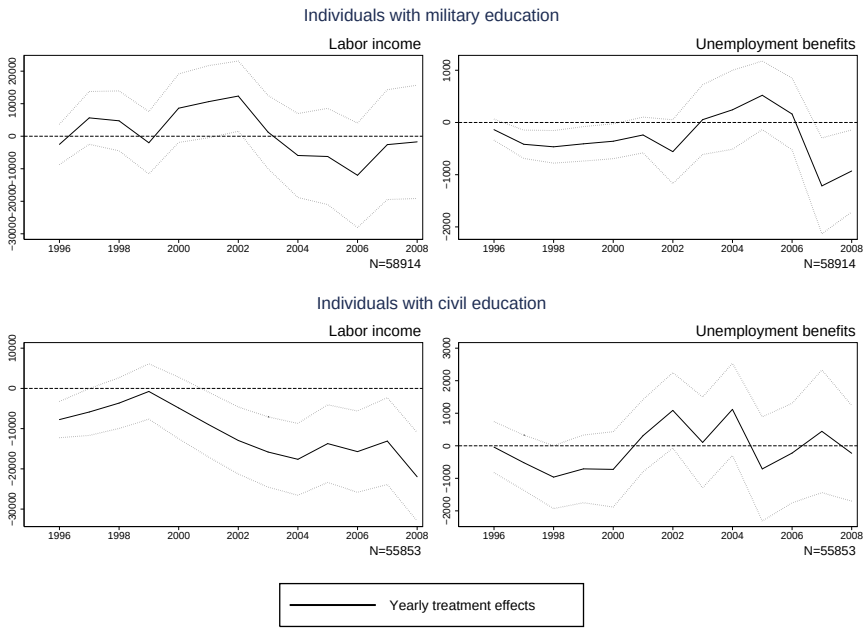

Figure 7: Effects on labor income and unemployment, individuals with at least 3 years tenure

9 Conclusions

In this paper, we study the labor market outcomes for individuals affected by the extensive cutbacks in the Swedish military sector following the defense proposition in 1999. The proposition mandated closure of a large number of military units of different sizes. The information on which municipalities that would be affected was unknown until the proposition was announced in the fall of 1999. Thus, the proposition can be viewed as a substantial shock to employment security for workers in the armed forces in the affected municipalities, which might have substantial effects on the labor market outcomes of the affected individuals, both in the short and long run. Relative to previous literature studying the effects of involuntary job loss, where firm closure is often identified through administrative registers, we are in a better position to determine when and how the military bases were closed (or downsized), since we can refer to the political and legal documents, and we have a relevant control group.

We find that individuals who were employed at the affected military bases experienced a reduction in labor income following the proposition. The effect is driven by workers employed as civil servants, for example administrative staff, and we find no effect among military servants. Relative to pre-treatment earnings, the effect corresponds to a seven percent decrease in labor earnings in the treatment group five years after the announcement of the proposition. We find no effect on unemployment benefits, implying that treated individuals do not become unemployed to a greater extent than individuals in the control group. Thus, it seems either that displaced individuals in the treatment group are able to find new jobs almost immediately, though with lower wages or fewer work hours, or that they partially or completely leave the labor force, for instance, to enroll in education or to retire early. Our analysis shows no effect on either labor market participation, take-up of study grants, sickness allowance, or early retirement benefits. Thus, we conclude that the most likely scenario is that individuals who are displaced from the military find new positions in other sectors, but that those sectors typically offer lower wages.

Since most of the military bases that we define as treated were not completely closed following the proposition, but rather experienced varying degrees of downsizing, a fraction of the individuals that we define as treated will not be directly affected by the propositions. To capture this difference in treatment intensity, we perform separate analyses both for the treatment effect on individuals who leave the military five years after the proposition was announced, and for the individuals working at bases that were completely closed down. Both these samples show that, among those who were directly exposed to the treatment, meaning that they worked at a base that closed down and/or that they left the military, military servants also experience earnings losses. During the first years after the proposition their income increases, probably as a result of

the generous severance payment schemes that were offered to military servants who chose to resign. After the initial increase, unemployment benefit take-up increases and labor income goes down, relative to the control group.

Relative to the research on job displacement, our estimated earnings losses are smaller than most international studies, but similar to some of the studies for Sweden (Eliason, 2011b). However, the differences in sample population, time period, and estimation technique makes the results hard to compare.

Overall, our results indicate that some of the expected negative consequences of the closures were actually realized. Previous studies have found that aggregate outcomes, such as the local growth rate, were not affected by the base closures. It is possible that the magnitudes of our estimates are not large enough to translate into effects on such macro-economic variables, although they are clearly economically significant at the individual level. There could also be general equilibrium effects that we are not able to account for in our model. For example, the local labor market may be positively affected by the large number of buildings and land that were made available to the public, when military bases closed. However, our analysis indicates that such positive effects did not fully compensate for losses at the individual level.

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Appendix A Municipalities in the treatment and control groups

Treated:	**Control:**
Upplands-Bro	Solna
Norrtälje	Vaxholm
Uppsala	Linköping
Strängnäs*	Motala
Eksjö	Lund
Gotland*	Karlsborg
Karlskrona	Lidköping
Kristianstad	Skövde
Hässleholm	Hammarö
Halmstad	Karlstad*
Kristinehamn*	Östersund*
Falun	Umeå
Sollefteå	Arvidsjaur*
Boden*	
Kiruna	
Ängelholm	

* Units in at the military base in the municipality were closed due to the defense proposition in 2004. Bases that only had re-organizations or re-allocations to nearby areas are not marked.

Appendix B Number of military employees in municipalities with (close to) total base closures

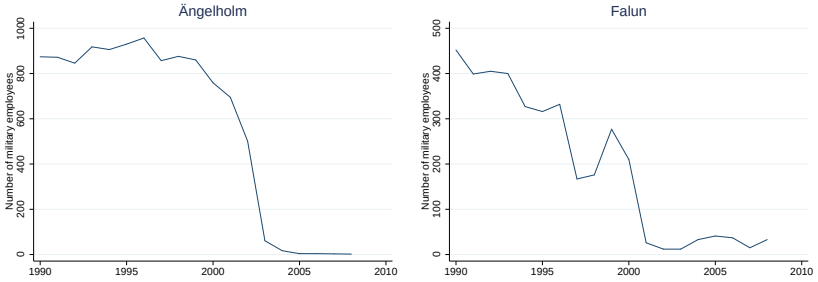

Figure 8: Ängelholm and Falun

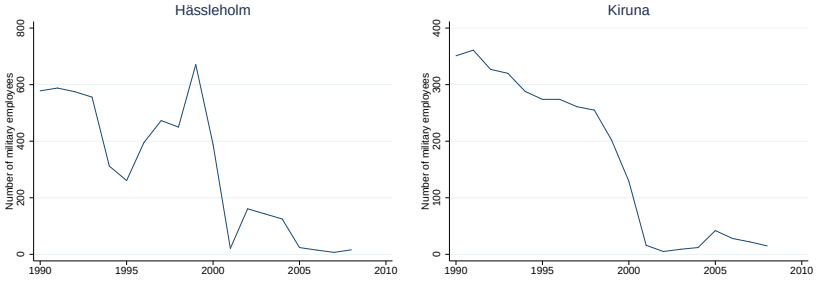

Figure 9: Hässleholm and Kiruna

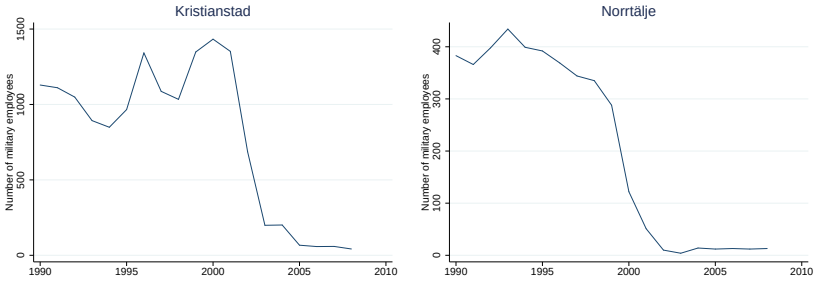

Figure 10: Kristianstad and Norrtälje

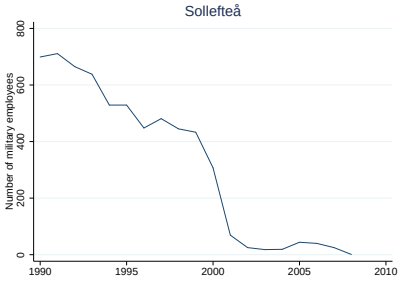

Figure 11: Sollefteå

Appendix C Complete estimation results

Table 6: Effects on labor income and unemployment

	Labor income	Unemployment benefits
1996	-5009.389***	-32.982
	(1893.696)	(368.818)
1997	-2316.968	-557.857
	(2431.781)	(406.019)
1998	-4007.961	-724.217*
	(2737.608)	(425.075)
1999	-4419.477	-378.895
	(2898.888)	(451.121)
2000	134.682	-500.051
	(3171.741)	(460.738)
2001	-1048.977	29.321
	(3316.870)	(471.578)
2002	-2186.802	-47.138
	(3349.938)	(485.980)
2003	-9530.680***	-173.137
	(3473.564)	(537.465)
2004	-14577.275***	250.191
	(3738.890)	(550.791)
2005	-14737.397***	-376.428
	(4154.155)	(575.919)
2006	-18362.825***	-401.765
	(4432.973)	(558.251)
2007	-13088.643***	-891.026
	(4695.809)	(632.825)
2008	-18219.787***	-852.708
	(4851.152)	(549.632)
N	125,915	125,915

Note: All estimations include year dummies, city district dummies, control variables at the individual and municipality level, and individual fixed effects. Standard errors in parenthesis, clustered at the individual level.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 7: Effects on labor income and unemployment, by profession

	Military servants		Civil servants	
	Labor income	Unemployment	Labor income	Unemployment
1996	-3174.375	10.906	-6228.423***	-205.106
	(3187.018)	(248.323)	(2385.841)	(609.618)
1997	4915.273	-281.126	-6380.331**	-1048.423
	(4162.188)	(251.124)	(3001.160)	(673.472)
1998	4483.349	-322.995	-6213.362*	-1387.845**
	(4661.274)	(226.471)	(3376.752)	(707.158)
1999	-2259.294	-247.553	-2726.738	-810.449
	(4868.616)	(257.180)	(3602.717)	(748.924)
2000	8623.636	-228.693	-6490.672*	-1069.057
	(5339.699)	(243.267)	(3866.217)	(767.928)
2001	11607.288**	-86.578	-9846.888**	-288.809
	(5647.366)	(249.703)	(4052.856)	(784.798)
2002	12631.391**	-477.811	-13695.699***	-118.637
	(5491.663)	(364.197)	(4208.228)	(792.533)
2003	1771.835	17.389	-17366.077***	-692.301
	(5706.245)	(398.438)	(4376.878)	(879.948)
2004	-5966.845	312.415	-18268.798***	-78.183
	(6475.480)	(425.142)	(4437.853)	(900.548)
2005	-5645.126	631.698*	-16834.164***	-1382.271
	(7461.621)	(382.109)	(4718.068)	(946.755)
2006	-11832.980	256.731	-17506.440***	-1092.049
	(8072.711)	(394.171)	(4904.428)	(918.756)
2007	-1855.148	-1180.843**	-14475.747***	-671.652
	(8515.818)	(502.674)	(5223.798)	(1034.028)
2008	-1006.873	-756.974*	-22695.517***	-1233.723
	(8793.977)	(440.109)	(5360.481)	(888.817)
N	59,871	59,871	66,044	66,044

Note: All estimations include year dummies, city district dummies, control variables at the individual and municipality level, and individual fixed effects. Standard errors in parenthesis, clustered at the individual level.
* p<0.1, ** p<0.05, *** p<0.01

Table 8: Effects on labor income and unemployment, military servants remaining in the military sector at least until 2005

	Labor income		Unemployment benefits	
	Remaining	Leaving	Remaining	Leaving
1996	-3714.545	-3014.408	5.332	-22.283
	(3130.739)	(3165.245)	(241.336)	(249.227)
1997	3343.891	4219.065	-184.932	-324.798
	(4074.764)	(4192.400)	(241.912)	(249.021)
1998	7223.701	8955.965*	-261.093	-330.902
	(4577.986)	(4704.237)	(211.784)	(227.372)
1999	662.845	-5181.498	-222.666	-284.032
	(4848.836)	(5330.237)	(238.829)	(335.061)
2000	-4297.079	29905.933***	-247.100	-150.382
	(5247.529)	(6425.599)	(222.645)	(308.803)
2001	3849.834	24095.547***	-246.040	188.222
	(5601.664)	(6669.056)	(225.581)	(343.871)
2002	2304.354	27540.836***	-728.050**	1.179
	(5407.170)	(6676.037)	(342.138)	(447.740)
2003	11352.976**	-13952.746**	-915.480***	1543.708***
	(5663.978)	(6761.343)	(345.279)	(590.161)
2004	5477.187	-24782.561***	-671.892*	1950.073***
	(6464.881)	(7847.582)	(380.756)	(621.059)
2005	2513.010	-18810.031**	-221.375	2056.168***
	(7670.609)	(8791.045)	(335.113)	(592.927)
2006	-8453.128	-17687.265*	-327.559	1294.082**
	(8387.161)	(9447.281)	(365.936)	(557.623)
2007	-8279.551	7664.542	-1046.055**	-1255.343**
	(8908.475)	(9890.332)	(511.084)	(562.284)
2008	-16711.847*	22744.527**	-590.936	-833.872*
	(9201.491)	(10227.408)	(444.281)	(487.138)
N	46,083	38,265	46,083	38,265

Note: All estimations include year dummies, city district dummies, control variables at the individual and municipality level, and individual fixed effects. Standard errors in parenthesis, clustered at the individual level.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 9: Effects on labor income and unemployment, civil servants remaining in the military sector at least until 2005

	Labor income		Unemployment benefits	
	Remaining	Leaving	Remaining	Leaving
1996	-6975.057***	-6166.942***	-97.635	-257.401
	(2304.630)	(2324.606)	(596.031)	(606.191)
1997	-8051.914***	-5821.706**	-612.366	-1255.942*
	(2902.379)	(2955.735)	(659.985)	(674.193)
1998	-7230.166**	-5487.304	-929.469	-1464.724**
	(3273.778)	(3362.638)	(697.272)	(714.113)
1999	209.262	-8636.396**	-1441.466**	613.554
	(3510.619)	(4209.796)	(713.984)	(938.575)
2000	-4307.075	-11310.234**	-1546.367**	106.567
	(3755.756)	(4659.659)	(736.364)	(965.268)
2001	-7759.039**	-14802.126***	-832.480	1006.060
	(3926.392)	(4950.973)	(750.350)	(1005.309)
2002	-9973.388**	-21901.153***	-981.559	1685.363
	(4063.308)	(5255.408)	(741.149)	(1086.519)
2003	-11841.675***	-28686.792***	-2224.941***	2171.925*
	(4189.309)	(5508.656)	(816.553)	(1243.764)
2004	-11051.499***	-32174.713***	-2467.086***	4144.459***
	(4236.267)	(5628.125)	(831.293)	(1312.504)
2005	-13116.541***	-25278.556***	-3141.665***	1766.710
	(4505.838)	(6294.680)	(901.493)	(1302.875)
2006	-16767.361***	-21446.097***	-1227.381	-508.806
	(4826.842)	(6150.700)	(905.811)	(1226.012)
2007	-19115.971***	-10271.024	1511.453	-3813.499***
	(5261.365)	(6536.171)	(1099.767)	(1247.560)
2008	-31453.894***	-11754.919*	880.147	-4288.556***
	(5304.208)	(6988.578)	(909.160)	(1087.363)
N	52,481	44,900	52,481	44,900

Note: All estimations include year dummies, city district dummies, control variables at the individual and municipality level, and individual fixed effects. Standard errors in parenthesis, clustered at the individual level.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 10: Effects on labor income and unemployment, treatment restricted to bases that are closed following the proposition

	Military servants		Civil servants	
	Labor income	Unemployment	Labor income	Unemployment
1996	-5407.923	46.053	-8863.814**	980.082
	(4964.685)	(457.993)	(4018.304)	(1055.831)
1997	3206.081	-348.133	-5426.533	-347.803
	(6933.635)	(456.656)	(4964.079)	(1168.797)
1998	4604.486	-418.113	-4981.960	-849.049
	(7643.252)	(416.256)	(5465.249)	(1187.496)
1999	-2934.078	-546.262	-2720.565	-199.456
	(8012.137)	(448.710)	(5723.032)	(1211.781)
2000	26667.222***	-310.622	-3674.253	-1191.038
	(8980.202)	(456.672)	(6076.182)	(1229.552)
2001	20879.245**	12.902	-12204.738*	1245.586
	(9341.123)	(488.347)	(6297.690)	(1340.858)
2002	20091.208**	-254.103	-17758.619***	1974.010
	(9,269.971)	(626.090)	(6,667.295)	(1386.445)
2003	-10099.350	992.041	-25543.031***	1892.596
	(9312.779)	(739.675)	(6720.474)	(1478.281)
2004	-23453.255**	1217.216	-30348.600***	3671.535**
	(10182.932)	(748.826)	(6780.334)	(1524.829)
2005	-23652.866**	2079.130***	-24991.180***	965.863
	(11293.405)	(775.332)	(6987.336)	(1489.200)
2006	-34080.242***	847.728	-22127.672***	125.305
	(11941.053)	(689.125)	(7199.587)	(1435.440)
2007	-28683.799**	-243.390	-15658.589**	43.740
	(12463.636)	(554.532)	(7493.782)	(1434.007)
2008	-31036.646**	540.500	-27581.325***	-181.575
	(12918.826)	(523.015)	(7624.070)	(1359.918)
N	23,322	23,322	32,230	32,230

Note: All estimations include year dummies, city district dummies, control variables at the individual and municipality level, and individual fixed effects. Standard errors in parenthesis, clustered at the individual level.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 11: Effects on labor income and unemployment, individuals with at least 3 years tenure

	Military servants		Civil servants	
	Labor income	Unemployment	Labor income	Unemployment
1996	-2500.366	-137.345	-7741.905***	-40.426
	(3166.039)	(102.869)	(2299.326)	(399.690)
1997	5647.875	-417.080***	-5861.796**	-518.781
	(4136.088)	(138.685)	(2971.547)	(432.264)
1998	4746.354	-465.965***	-3650.446	-962.993*
	(4690.199)	(159.154)	(3222.194)	(493.712)
1999	-2009.456	-408.439**	-759.478	-709.331
	(4893.576)	(168.364)	(3506.082)	(531.186)
2000	8624.755	-358.913**	-4858.645	-725.321
	(5363.809)	(169.895)	(3903.310)	(590.560)
2001	10624.759*	-239.026	-8978.735**	311.004
	(5660.318)	(174.673)	(4114.119)	(565.749)
2002	12346.863**	-558.606*	-12938.267***	1088.304*
	(5507.311)	(312.336)	(4252.049)	(591.805)
2003	1259.600	54.315	-15809.363***	103.797
	(5748.307)	(340.743)	(4457.814)	(711.934)
2004	-5923.792	242.774	-17619.319***	1119.706
	(6576.568)	(386.089)	(4561.018)	(721.975)
2005	-6225.984	521.071	-13714.703***	-711.154
	(7541.001)	(335.228)	(4921.145)	(816.916)
2006	-12003.036	162.001	-15710.766***	-223.846
	(8190.303)	(350.169)	(5156.305)	(780.269)
2007	-2580.567	-1214.121***	-13087.623**	444.914
	(8615.536)	(468.278)	(5499.278)	(961.730)
2008	-1730.859	-928.839**	-21962.634***	-230.347
	(8880.542)	(400.604)	(5611.775)	(749.444)
N	58,914	58,914	55,853	55,853

Note: All estimations include year dummies, city district dummies, control variables at the individual and municipality level, and individual fixed effects. Standard errors in parenthesis, clustered at the individual level.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$