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Dynamic effects of mandatory activation of welfare participants

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Abstract

Previous literature shows that activation requirements for welfare participants decrease welfare participation. However, the dynamics have not been examined, and often only exit effects are analyzed. In this paper, we look more closely at the transition rates into and out of welfare. Using register data on the entire population of Stockholm, we are able to capture how both entry and exit rates were affected when activation requirements were introduced at different times in Stockholm's city districts. The results indicate that the main reduction in welfare participation is due to a small increase in exit rates. The part of the population that is at risk of entering into welfare, though, experiences a reduction in entry rates due to the reform. There are also heterogeneous effects, namely, large effects on entry rates for young individuals. In addition, there are larger effects on exit rates for unmarried individuals without children compared to the population as a whole.

Keywords: Welfare reform, mandatory activation program, welfare entry, welfare exit
JEL classification: I38, H31

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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 because receiving benefits generally conflicts with retaining work incentives. Throughout history, it has been common to require poor individuals to provide some service to society to prove themselves to be "worthy" of support. This view was particularly prominent in England during the 19th century, where the poor were required to move to workhouses to receive financial aid. This was also common in Sweden for some time, as was requiring the poor to take low-paying so-called "emergency jobs" (nödarbeten). During the postwar expansion of the welfare state in Sweden, these policies began to be used less frequently. Nevertheless, 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], both in Sweden and other industrialized countries.

Mandatory activation requirements may imply very different things. In a strong ver-sion known as "workfare" programs, the welfare recipient is required to work in some publicly provided job to retain assistance. Weaker versions may merely mandate participation in a job preparation or job search program. There are also optional activation programs in which noncompliance does not always lead to sanctions.

Most theoretical work on activation requirements for welfare recipients focuses on incentive effects in an optimal taxation framework[2]. It is generally explicitly or implicitly assumed that required work is not productive (or at least is less productive than market work) and does not improve human capital; it only provides incentives. 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 if they have some possibility of supporting themselves because there is an implicit cost associated with welfare use. Furthermore, in the longer run, people may make choices that reduce the risk of becoming welfare dependent in the future, for example, by completing more education, because welfare is a less attractive alternative. Hence, mandatory activation programs affect both welfare participants and nonparticipants through exit and entry effects, respectively. However, most

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

[2] See for example Chambers (1989), Brett (1998) and Cuff (2000).

previous research has focused solely on their effects on welfare participants and thus has captured only their effect on welfare exits. Grogger, Haider, and Klerman (2003) show, using both observational data and simulations, that welfare exits accounted for around half of the large reduction in US welfare use during the 1990s, while the other half is explained by reduced entry rates. It is thus clear that by not studying the possible welfare entrants, a large share of the dynamics is lost. The importance of entry rates is also established in Hansen and Lofstrom (2006)), where it is shown that entry rates explain a large part of the difference in welfare participation between native Swedes and immigrants.

Although they have been shown to be important, the effects of activation policies on entry rates have not been thoroughly studied. To our knowledge, the only study where an attempt to study entry into welfare is made is that by Dahlberg, Johansson, and Mörk (2008). Their study uses the differences in the timing of implementation of activation requirements in the different city districts of Stockholm to identify their effect on welfare caseloads. Their study captures both the entry and exit effects because the data consist of information on the whole population rather than only welfare recipients. However, they are not able to distinguish between the two, and thus the dynamics are still largely unknown.

In this study, we aim to examine these dynamics more closely and study the effects of mandatory activation on welfare entry and welfare exit separately. The identifying variation that we use, which is the same as that used by Dahlberg, Johansson, and Mörk (2008), is the time differences in implementation of mandatory activation for unemployed welfare participants in different city districts of Stockholm. By using individual-level panel data, we can use this variation to compare entry and exit rates across city districts with different requirements at different time periods. The advantage of looking only at the city districts of Stockholm is that they have the same political composition 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, one may worry that relocation of welfare-prone individuals might invalidate the exogenous variation. Several recent studies confirm the hypothesis that regions with generous welfare systems attract welfare participants; that is, welfare-prone individuals relocate to places where social assistance is higher (Meyer 2000, Gelbach 2004, McKinnish 2007 and Fiva 2009). However, in most cases, welfare migration is small in magnitude. A study by Edmark (2009) shows no indication that the activation requirements implemented in Stockholm affected migration patterns. Thus, we do

not think that migration of welfare-prone individuals will bias the results of this study.

In general, the Swedish experience of welfare reforms makes a valuable contribution to the existing literature because Sweden is considered to have a relatively generous welfare system. For example, in Sweden, all individuals can be eligible to receive welfare benefits, whereas in the US, support is primarily aimed at single mothers. The Swedish system makes it possible to look at heterogeneous treatment effects across dif-ferent demographic groups. Also, the Swedish reform provides credible identifying variation because it has not been combined with other instruments such as time limits and tax subsidies, as is often the case in the US. In this study, we find that mandatory activation has no effect on the entry rates when studying the whole population, but exit rates increase by 0.9 percentage points. When we restrict our sample to estimate the effect on individuals that can be assumed to have a larger risk of entering welfare dependency, we find that the activation programs lead to a significant reduction in entry rates for this group. In addition, for young individuals (aged 18-25), there are large effects on entry rates, and there are larger effects on exit rates for unmarried individuals without children compared to the whole population.

The structure of the paper is as follows. In section 2 we discuss previous literature. Section 3 presents institutional settings in Sweden and the data used in this study. The empirical strategy is described in section 4. We begin section 5 by presenting the effects on the welfare caseload before we present the results from both our main specification (5.2) and also from placebo estimations (5.3). We then discuss the results of estimations in which we allow for time-varying effects (5.4) and, at last, results for different sub-groups (5.5) to see if there are heterogeneous effects. Finally, in section 6, we conclude and discuss our results.

2 Previous literature

The major change in the American welfare system during the 1990s created a huge body of economic research evaluating the effects of these reforms. Even before the major reform in 1996, when the US changed from Aid to Families with Dependent Children (AFDC) to Temporary Assistance to Needy Families (TANF), many states had waivers from the AFDC rules. The proposed waiver programs had to be evaluated by the state after implementation. Because many of these waivers allowed states to enforce work requirements for welfare recipients, there are many studies in which

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

Since the changes in the US welfare system, the caseload has decreased substantially. However, this cannot be attributed only to the welfare reforms because the US also experienced strong economic growth during the same time period. Klerman and Haider (2004) show that it is important to look at how entry and exit rates are affected by welfare programs together with economic conditions because both determine the total caseload. They find that 50 percent of the decline in caseload in California can be attributed to the decline in the unemployment rate. It is therefore very important to take changes in economic conditions into consideration when estimating the effects of welfare programs.

Bloom and Michalopoulos (2001) argue that activation programs in the US can be divided into three groups. The first consists of programs founded on the belief that the individual's employability is improved mostly by working, even at low-paying jobs. Thus, these programs focus on employment, and if the individual is not successful in finding a job, he or she may be offered some training. The second group of programs focuses on education, and most applicants first take part in classroom education to increase their skill level before beginning to apply for jobs. In the third group, the initiatives are mixed: some individuals are assigned to training and others to job search ac-tivities. These mixed initiatives programs have proven to be the most effective of the three types.

A related literature investigates the relation between the duration of unemployment and the generosity of the unemployment insurance system. For example, Black, Smith, Berger, and Noel (2003) show that the threat of required training (rather than the pro-gram as such) significantly reduces the duration of unemployment spells. This implies that activation policies have a stronger effect on the individuals' incentives rather than their employability. If the activation programs for social assistance recipients in Stockholm are a greater deterrent to those who can find other means of support and provide less help to those who want to leave welfare dependence, we will find larger effects on the entry rates than on the exit rates.

Dahl (2003) evaluates a workfare program in Norway in 1995. He finds

that welfare recipients who were allocated to the program were farther from the labor market than a comparison group, but he does not find any treatment effect of the program.

There are only a few studies that use Swedish data to investigate the effect of activation requirements on welfare participation. In a study by Milton and Bergström (1998), an activation program in the municipality of Uppsala is studied. In this program, some welfare participants were required to apply for jobs full time and report the number of applications to a caseworker. The authors find no effect of this program on the number of people on welfare or on the probability of becoming employed. Largely similar results are found in Giertz (2004), which analyzes an activation program in Malmö. However, it is uncertain if there were any sanctions for those recipients in Malmö who did not attend the activation program. Jönsson (2007) finds that activation requirements in Swedish municipalities significantly reduce participation rates. However, she does not find a significant effect on the cost of welfare. 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.

Hansen and Lofstrom have also used Swedish data to look at welfare participation. Their main focus has been on differences between native-born Swedes and immigrants. They find that the differences between natives and immigrants are mainly due to differences in entry rates (Hansen and Lofstrom 2003) and that immigrants' participation in welfare decreases with time spent in Sweden, even though immigrants have higher par-ticipation rates than natives even after 20 years in Sweden (Hansen and Lofstrom 2006).

3 Institutional setting and data

3.1 Social assistance in Sweden

Sweden is divided into 290 municipalities, which are responsible for the majority of the publicly provided welfare services, such as childcare, education and elder care. The local governments have historically also been responsible for relief for the poor, whereas labor market policies have been administered by the central government. Although social assistance is largely a local responsibility, there is national legislation establishing the main principles for benefits. The legal framework is stated in the Social

Services Act passed in 1982. This law ensures all Swedish citizens and foreign citizens living in Sweden financial support to maintain a "reasonable" standard of living in default of other means of support. A minimum 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 health insurance, is not available or is insufficient. Unlike the social insurances, social assistance is not income based. However, eligibility is universal in the sense that it is not dependent on, for example, having children, as is the case in some other countries (for example, the US and the UK).

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

In response to the financial difficulties and increase in unemployed social assistance beneficiaries during the recession, many local governments started to develop municipal activation programs to try to move social assistance recipients from welfare to self-sufficiency. In 1998, the Social Services Act was changed to explicitly allow municipalities to require welfare participants to take part in activation programs to retain their eligibility[3] . The activation programs in the Swedish municipalities consist of job-search programs and education as well as practice at job sites. In some cases, rehabilitation programs are also offered (Salonen and Ulmestig 2004).

3.2 The city districts of Stockholm

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

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

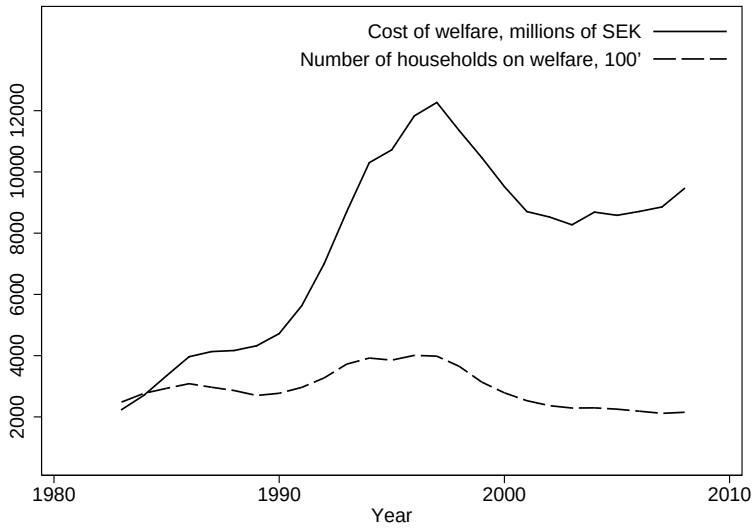

Figure 1: Cost of welfare (millions of SEK) and number of welfare households (100's) 1983-2008. Source: Statistics Sweden.

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

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

Of the social assistance recipients in Stockholm in 2005, around three quarters were unemployed. A large fraction of these, 77 percent, are unemployed and 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).

Table 1: City district characteristics 1993

	(1) Share of welfare recipients	(2) Average welfare benefits[a]	(3) Average disposable income[b]	(4) Share born in non-Western countries	(5) Average benefits per recipient[c]	(6) Entry rate	(7) Exit rate	(8) Activation year
Rinkeby	0.308	5,785	96,052	0.463	18,771	0.115	0.229	1998
Skärholmen	0.111	1,713	124,328	0.124	15,387	0.048	0.319	1999
Farsta	0.115	2,181	128,714	0.048	18,918	0.047	0.302	2001
Kista	0.171	3,189	126,035	0.226	18,602	0.073	0.279	2001
Älvsjö	0.067	885	145,118	0.032	13,175	0.033	0.340	2002
Hägersten	0.072	1,380	134,266	0.032	19,080	0.032	0.349	2003
Liljeholmen	0.095	1,744	126,067	0.039	18,303	0.042	0.325	2003
Spånga-Tensta	0.149	2,555	131,017	0.214	17,131	0.058	0.289	2003
Bromma	0.058	998	154,035	0.025	17,217	0.026	0.352	2004
Enskede-Årsta	0.075	1,318	133,375	0.043	17,686	0.030	0.363	2004
Hässelby-Vällingby	0.071	1,140	141,590	0.048	16,043	0.032	0.342	2004
Vantör	0.122	2,219	124,368	0.067	18,152	0.048	0.298	2004
Total	0.102	1,798	133,960	0.085	17,594	0.042	0.310	

[a] Average welfare benefits in city district including entire population.

[b] For the year 1995, since only available for the years 1995-2005.

[c] Average welfare benefits among welfare recipients.

Dahlberg, Johansson, and Mörk (2008), using results from questionnaires and interviews conducted by Karin Edmark and Kajsa Hanspers, determine when activation requirements were implemented in the different city districts. For an activation program to be classified as mandatory, the activity must be directed to all unemployed welfare participants and require the individuals to attend the activity center for at least a few hours every week. It was possible to determine a starting year for 12 of the 18 city districts. In the five most centrally located districts and Skarpnäck, it was not possible to determine when activation programs were implemented. For the central districts, this is mainly due to the fact that there are very few welfare participants in this area. 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.

The activation programs created new Jobcentres that social assistance recipients are required to attend for at least a few hours each week. Previously, welfare recipients were only in contact with the local social worker, and there were no mandatory programs for all social assistance recipients. Unemployed recipients were directed to the unemployment office, but there were no sanctions if they did not participate in any activities. The activation program in Skärholmen is the most renowned program, usually referred to as "the Skärholmen model". It started as a measure to reduce welfare participation among students who were unemployed during the summer. In 1999, the program was widened to include all unemployed welfare participants. The main feature of the program is that unemployed welfare applicants are sent to the Jobcentre. In order to retain eligibility for welfare, the applicant must visit the Jobcentre for three hours every day, following a rotating schedule to prevent black market work, until he or she finds a job. The required activity consists mostly of individual job searching. The Jobcentre provides computers with internet access and assistance from staff when necessary. As noted by Thorén (2005), the resources are often limited; for example, clients can rarely use the computers for more than 15 minutes each day. There is daily registration of participants' attendance, and because there is close cooperation between social workers and Jobcentre staff, absence is easily detected and can (and often does) lead to a reduction in benefits. This possibility of imposing sanctions is common

to programs in all city districts. 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[4]. It also contains the share of the household's social assistance that the individual has received during the past year as well as benefits collected from other parts of the social security system. Social assistance is directed at households rather than individuals, and we define an individual as a welfare participant if he or she is living in a household that received social assistance sometime during a given year. This is a very rough but commonly used classification. What we refer to as social assistance is thus the individual's share of the household's total received benefit. Because all newly arrived immigrants are eligible for social assistance during their first 18 months in Sweden (introduktionsbidrag) under different eligibility criteria than other welfare participants, these individuals are excluded for three years to avoid capturing their dynamics due to this sort of support. Table 2 shows descriptive statistics for the population. The mean amount of welfare benefit received by an individual is slightly above 2,000 SEK per year. However, it should be noted that all zeros are included here and that the mean amount of benefits among those who actually receive any benefits at all is around 23,600 SEK per year.

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

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

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

Table 2: Summary statistics

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

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

tory activation has been implemented. It is important to note that the exit population will change over time due to the reform because individuals closest to the labor market may never enter the population of social assistance recipients due to the introduction of mandatory work requirements. This may call the assumption for difference-in-differences into question (see section 4)[5]. What can be done is to see if there are different effects of the reform between the year in which activation was implemented and the following year. It can be expected that the exit effects decrease over time because the individual closest to the labor market never enters, and therefore, the remaining population of individuals on social assistance have a harder time finding other means of support.

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

[5]This is also a problem, albeit probably a smaller one, in the entry sample

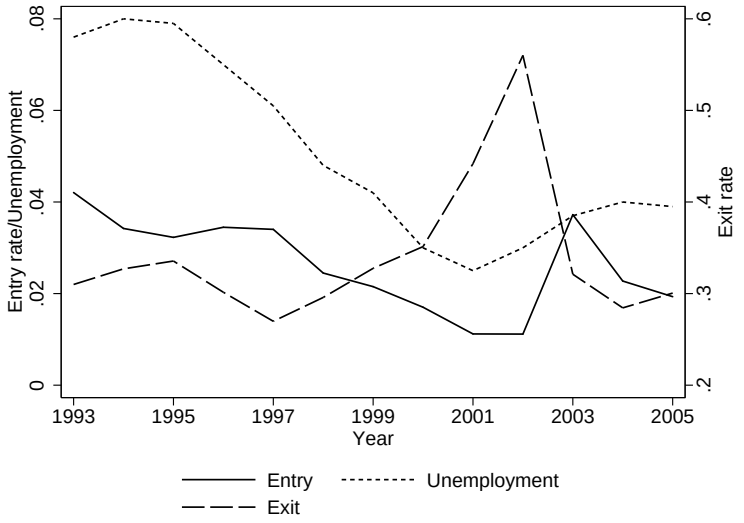

Figure 2: Unemployment rate, raw entry and exit rates, by year in Stockholm

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 people with little education. Because these groups have a higher probability of receiving benefits than others, we attempt to create a better-defined entry sample by estimating effects on entry rates using only a subpopula-tion consisting of individuals with any of these characteristics. Thus, we reduce the problem of estimating an effect for individuals that have close to zero probability of ever participating in welfare (for example, individuals with high education and income are unlikely to change their behavior in response to a reform that will probably never affect them). We prefer to define the population at risk of entering into welfare using demographic characteristics rather than income. It is likely that individuals with low income are more likely to receive welfare benefits than others. However, Meyer (2000) argues that restricting the sample to include only

low-income individuals might create bias because poverty is likely to be higher in an area with low benefit levels and vice versa, which might affect welfare participation as well as entry and exit.

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

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 mother	0.070	0.277
Single without children	0.028	0.352

4 Empirical strategy

To determine the treatment effect on the treated (TT) when mandatory activation is introduced, we use a difference-in-differences (DD) approach in a linear probability model (LPM). When estimating the effects on entry and exit rates, there will be different events of interest. In the entry case,

the population used is those individuals who did not receive any social assistance at *t-1*, and the event of interest will be if they then receive social assistance at t. Let $W_{it} = 1$ indicate that the individual received welfare at time t; then, the probability of entry is given by $P(W_{it} = 1|W_{it-1} = 0)$. When we estimate the effect on exit rates, the population is comprised of those individuals receiving social assistance at *t-1*, and the event of interest is if they do not receive social assistance at t, $P(W_{it} = 0|W_{it-1} = 1)$.

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

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

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

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

$$Y_{ijt} = \alpha_{\mathbf{j}} + \tau_{\mathbf{t}} + \beta D_{jt} + \gamma_{\mathbf{t}}\mathbf{W_{ijt}} + trend_j + \eta_{ijt} \tag{2}$$

where α_j and τ_t are city district and year dummies, respectively. β measures the effect of mandatory activation on the probability of entry and exit. To control for individual heterogeneity that varies over time, $\mathbf{W_{ijt}}$ is included[6]. $trend_j$ are linear city district-specific time trends, and η_{ijt} is an error term.

To see if the results are very dependent on specification, we perform the estimations in three steps, where the first step is with constant parameter

[6]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).

estimates (γ) for the individual characteristics. We then add time trends in the second estimation, and in the third estimation we allow for even more flexibility using time trends and time-interacted covariates (giving γ_t). This last step is done because individual characteristics may differently influence the probabilities of moving into and out of social assistance depending on the business cycle. Because there may be different effects of the reform between the year in which man-datory 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).

4.1 Standard error corrections

The model proposed by equation 2 does not consider the possibility of city district-specific shocks. If such shocks exist and are correlated with the timing of the reform, the effect of the shock will be captured in our estimates of the reform effect. An additional problem with these kinds of shocks is that they may cause the standard errors to be cor-related among individuals living in the same city district. If this is the case, the standard errors may be dramatically downward biased, and thus the inference is not valid. However, because this study only concerns a relatively small geographical region with one common labor market, we argue that we are able to capture such shocks with our most flexible specification.

The standard errors may, though, still be correlated within groups because observations are not necessarily independent within districts. One way to control for this correlation is to estimate treatment effects on group averages. This would be a simple solution, but it implies that we cannot make use of the microcovariates and would cause a large loss of information. Instead, we test for whether observations are correlated following Wooldridge (2003). He proposes a two-stage procedure where an efficient minimum distance (MD) estimator is obtained in the first step by estimating

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

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

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

with weighted least squares where the weights are given by $1/\hat{\sigma}_{jt}$. Under the null of no unobserved city district specific shocks we have that (in the second stage estimation) $SSR \overset{a}{\sim} \chi^2(S-K)$ where S is equal to $J \times T$ and K

is equal to the number of parameters estimated in equation 4. If the null hypothesis is rejected city district specific shocks exists and Wooldridge (2003) argues that a consistent estimator can be found using the two stage procedure proposed by Donald and Lang (2007). They argue that it is possible to correct for group and time specific shocks as well as controlling for serial correlation in the error term by estimating group averages, while still using the information in the microcovariates. In practise, this is equivalent to estimating 3 and 4 but using the group size, that is the share of the total sample population living in each specific city district every year, as weights in equation 4 instead of the variance of $\hat{q}_{jt}$ from equation (3). This between estimator gives the correct standard errors and t-statistics, and thus provides a valid inference. However, if there is no correlation in standard errors within clusters, this approach reduces the amount of availible information more than necessary. Therefore, we only use group averages when the Wooldridge test rejects the null of independent observations.

5 Results

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

5.1 Effects on caseloads

According to Dahlberg, Johansson, and Mörk (2008), the caseload (share of welfare re-cipients) was reduced by 0.5 percentage points in Stockholm due to mandatory activa-tion 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 using equation (2) and for different subpopulations. The caseload results are shown in table 4.

In our estimation, we find a smaller reduction in welfare participation due to the reform, 0.3 percent, than Dahlberg, Johansson, and Mörk (2008) found. There are, however, heterogeneous effects, and the effect is much

Table 4: Estimation results: Caseload

	(1)	(2)	(3)	(4)
	All	Age < 26	Born in non-Western country	Unmarried w/o children
Mandatory activation implemented	-0.003***	-0.012***	0.006*	-0.006***
	(0.001)	(0.002)	(0.003)	(0.001)
Time-interacted controls	Yes	Yes	Yes	Yes
Linear trend	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
City district dummies	Yes	Yes	Yes	Yes
N	2,986,175	372,325	372,917	1,395,995

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

larger for both young individuals and unmarried individuals without children (1.2 and 0.6 percent, respectively).

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

5.2 Baseline estimation

The baseline estimations show the results from estimations of equation 2 and the test statistics from the two-step Wooldridge approach given by equations 3 and 4. In the tables, column 1 shows results from the least flexible specification, where we neither include trends nor let the impact of covariates vary over time. In column 2, we add linear, city district-specific trends, and in column 3, we also add time-interacted cova-riates. In column 4, we present the Donald and Lang estimates, if the null from the Wooldridge test is rejected at the 5 percent level in column 3, using the most flexible approach (that is, including trends and time-interacted covariates). Table 5 and Table 6 show the results for the estimates of the probability

of entry and exit, respectively. Looking at the Wooldridge test statistics, we see that for the entry estimates, we reject the null of no correlation in standard errors for all specifications (the null is rejected at the 5 percent level if the test statistic is greater than 132 with 107 degrees of freedom). For the exit rates, we do not reject the null in the last specification, and thus we do not present a Donald-Lang estimate in this case.

Table 5: Estimation results: Entry

	(1)	(2)	(3)	(4)
Mandatory activation implemented	-0.005***	-0.002***	-0.001*	-0.001
	(0.000)	(0.000)	(0.000)	(0.002)
Time-interacted controls	No	No	Yes	Yes
Linear trend	No	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
City district dummies	Yes	Yes	Yes	Yes
Wooldridge-test; SSR(df)	2792.433(130)	2068.807(119)	134.418(107)	
N	2,698,222	2,698,222	2,698,222	156

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The estimates for the effect on entry vary from a reduction by 0.5 percentage points in the least flexible estimation to an insignificant reduction by 0.1 percentage points in the Donald and Lang estimation. To be able to capture true reform effects, it therefore seems important to control for time trends in the city districts and to allow the effects of the covariates to vary over time. The conclusion is that we do not see any effect on the entry rates for the whole population when mandatory activation is implemented. The reform may, however, still have had an effect on the entry rates at different times since implementation and for different subpopulations, especially for populations at greater risk of entering welfare (see section 5.4 and section 5.5).

Also, the point estimates for the exit rates vary by specification and become smaller with increasing flexibility of the strategy used. The probability of exit is increased due to the implementation of activation requirements, but these estimates are relatively small in the last specification (the exit rates are around 33.5 percent on average - see Figure 2 - which implies that the number of exits increases by around 200 individuals each year as a result of the reform). Table 19, Table 20 and Table 21 in the Appendix present further estimation results, including parameter estimates

Table 6: Estimation results: Exit

	(1)	(2)	(3)
Mandatory activation implemented	0.021***	0.011**	0.009*
	(0.004)	(0.004)	(0.004)
Time-interacted controls	No	No	Yes
Linear trend	No	Yes	Yes
Year dummies	Yes	Yes	Yes
City district dummies	Yes	Yes	Yes
Wooldridge-test; SSR(df)	912.000(130)	750.405(119)	36.600(107)
N	287,953	287,953	287,953

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

for covariates[7] as well as district and year dummies.

Also for the exit rates the point estimates vary over specification and gets smaller the more flexible strategy that we use. The probability of exit is increased due to the implementation of activation requirements, but these estimates are relatively small in the last specification (the exit rates are around 33.5 percent on average, see Figure 2, which implies that the number of exits increases by around 200 individuals each year as a result of the reform).

5.3 Placebo estimations

In order to verify that the effects estimated above are truly reform effects, we performed 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. 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 columns in Table 7 and Table 8 shows equivalent estimations as in the previous tables: column 1 shows the results from the least flexible specification, linear trends are added in column 2, and in column 3, we also time-interact the control variables. The Wooldridge test statistic does not reject the null of no correlation in the standard errors in the third

[7] Due to limited space, we only show parameter estimates from the second specification, where the covariates are not interacted with time. More detailed results are available upon request.

specification, but we still present the Donald and Lang estimates in column 4 of Table 7 for comparison because we use the Donald and Lang estimator in the baseline estimation of effects on entry rates.

Table 7: Results from placebo estimations: Entry

	(1)	(2)	(3)	(4)
Mandatory activation implemented	0.001	0.002**	0.001*	0.002
	(0.001)	(0.001)	(0.001)	(0.002)
Time-interacted controls	No	No	Yes	Yes
Linear trend	No	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
City district dummies	Yes	Yes	Yes	Yes
Wooldridge-test; SSR(df)	311.088(70)	113.200(59)	14.453(48)	
N	1,530,957	1,530,957	1,530,957	91

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In the placebo estimations for entry, the results are significantly different from zero in two of the specifications. 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 real estimations.

Table 8: Results from placebo estimations: Exit

	(1)	(2)	(3)
Mandatory activation implemented	0.001	0.001	0.003
	(0.004)	(0.004)	(0.004)
Time-interacted controls	No	No	Yes
Linear trend	No	Yes	Yes
Year dummies	Yes	Yes	Yes
City district dummies	Yes	Yes	Yes
Wooldridge-test; SSR(df)	103.820(70)	62.698(59)	7.222(48)
N	188,904	188,904	188,904

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In the estimations of how the "pseudo"-reform affected exit, none of the results are significantly different from zero, which strengthens the argument that the results from the baseline estimations are real effects of the implementation of mandatory activation.

5.4 Time-changing treatment effects

Even if we do not find any effects on entry rates due to the reform or any large effects on exit rates, there may be varying effects over time. To see whether the estimated treatment effects are increasing or decreasing with time elapsed since the reform, we change the specification given by equation 2 slightly and estimate separate treatment effects for the year of implementation, the first year after implementation and two or more years after implementation. The results are given in Table 9 and Table 10 (for more estimation results, see Table 22 and Table 23 in the Appendix).

Table 9: Results from estimations with time-specific treatment: Entry

	(1)	(2)	(3)	(4)
Year of implementation	-0.003***	-0.002**	-0.000	-0.001
	(0.000)	(0.001)	(0.001)	(0.002)
One year after	-0.006***	-0.003***	-0.002**	-0.002
	(0.001)	(0.001)	(0.001)	(0.002)
Two years after or more	-0.009***	-0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.003)
Time-interacted controls	No	No	Yes	Yes
Linear trend	No	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
City district dummies	Yes	Yes	Yes	Yes
Wooldridge-test; SSR(df)	2720.628(128)	2039.531(117)	131.018(105)	
N	2,698,222	2,698,222	2,698,222	156

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In the entry estimation, the effects are still insignificant in the Donald-Lang 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. However, the estimates for the different time periods are not statistically different from each other in the third column. Overall, there seems to be a phase-in of the reform during the reform year but no large variation in treatment effects after the first year.

As mentioned earlier, the assumption for difference-in-differences can be called into question, especially for the exit sample, if entry rates are affected by the reform. Because there does not seem to be any effect of mandatory activation on entry rates in the year of implementation, we estimate exit rates but restrict the sample to include each city district only in the year that activation was introduced and the year after. The results obtained using this sample are shown in Table 11.

As can be seen, the point estimates in the last specification are somewhat

Table 10: Results from estimations with time-specific treatment: Exit

	(1)	(2)	(3)
Year of implementation	0.014**	0.012**	0.010*
	(0.004)	(0.004)	(0.004)
One year after	0.018***	0.014**	0.009
	(0.005)	(0.005)	(0.005)
Two years after or more	0.054***	0.040***	0.019**
	(0.005)	(0.007)	(0.007)
Time-interacted controls	No	No	No
Linear trend	No	Yes	Yes
Year dummies	Yes	Yes	Yes
City district dummies	Yes	Yes	Yes
Wooldridge-test; SSR(df)	857.842(128)	728.684(117)	29.291(105)
N	287,953	287,953	287,953

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 11: Results from estimations up to one year after reform has been implemented: Exit

	(1)	(2)	(3)
Mandatory activation implemented	0.016***	0.017**	0.010
	(0.004)	(0.005)	(0.005)
Time-interacted controls	No	No	Yes
Linear trend	No	Yes	Yes
Year dummies	Yes	Yes	Yes
City district dummies	Yes	Yes	Yes
Wooldridge-test; SSR(df)	637.477(108)	496.893(97)	15.815(86)
N	253,366	253,366	253,366

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

higher, but because the standard error also increases, the estimate is no longer significant at the five percent level.

5.5 Heterogeneous effects

5.5.1 Population at risk

As mentioned in section 3.3, certain groups of individuals[8] 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 12. Here, the Wooldridge test is not rejected in the most flexible specification. The results indicate a reduction in entry rates of 0.3 percentage points for this population. The program therefore seems to have had an effect on entry rates for those individuals at greater risk of entering welfare.

Table 12: Results for population at risk: Entry

	(1)	(2)	(3)
Mandatory activation implemented	-0.006***	-0.004***	-0.003**
	(0.001)	(0.001)	(0.001)
Controls	Yes	Yes	No
Time varying controls	No	No	Yes
Linear trend	No	Yes	Yes
Year dummies	Yes	Yes	Yes
City district dummies	Yes	Yes	Yes
Wooldridge-test; SSR(df)	297.370(119)	227.830(108)	15.826(96)
N	839,078	839,078	839,078

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

5.5.2 Effects on subpopulations

To study whether activation requirements affect subgroups of the population differently, we performed separate estimations for some of these groups[9]. Because Dahlberg, Johansson, and Mörk (2008) find large effects

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

[9]We also performed estimations for single mothers, who constitute another vulnerable group, but no effect of the implementation of mandatory activation could be found on either entry or exit rates for this group.

of mandatory activation 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 13 and Table 14. The effect on the probability of entry is reduced by 0.6 percentage points in the last specification. 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 exit effect is insignificant.

Table 13: Estimation results: Entry, age< 26

	(1)	(2)	(3)
Mandatory activation implemented	-0.014***	-0.009***	-0.006**
	(0.002)	(0.002)	(0.002)
Time-interacted controls	No	No	Yes
Linear trend	No	Yes	Yes
Year dummies	Yes	Yes	Yes
City district dummies	Yes	Yes	Yes
Wooldridge-test; SSR(df)	8.027(130)	6.595(119)	0.211(107)
N	312,850	312,850	312,850

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 14: Estimation results: Exit, age< 26

	(1)	(2)	(3)
Mandatory activation implemented	0.023**	0.013	0.014
	(0.008)	(0.009)	(0.009)
Time-interacted controls	No	No	Yes
Linear trend	No	Yes	Yes
Year dummies	Yes	Yes	Yes
City district dummies	Yes	Yes	Yes
Wooldridge-test; SSR(df)	3.400(130)	2.836(119)	0.096(107)
N	59,475	59,475	59,475

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The results for immigrants born in a non-Western country are presented in Table 15 and Table 16.

Neither the entry nor the exit effect is significant. Although Dahlberg, Johansson, and Mörk (2008) found large effects on caseload for this group, the results here are not surprising as we find no effect on caseload for this group in our sample.

Table 15: Estimation results: Entry, immigrants born in non-Western country

	(1)	(2)	(3)
Mandatory activation implemented	-0.002	0.001	0.001
	(0.002)	(0.002)	(0.002)
Time-interacted controls	No	No	Yes
Linear trend	No	Yes	Yes
Year dummies	Yes	Yes	Yes
City district dummies	Yes	Yes	Yes
Wooldridge-test; SSR(df)	70.527(129)	63.102(118)	18.668(100)
N	260,084	260,084	260,084

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 16: Estimation results: Exit, immigrants born in non-Western country

	(1)	(2)	(3)
Mandatory activation implemented	0.002	-0.004	0.000
	(0.005)	(0.005)	(0.005)
Time-interacted controls	No	No	Yes
Linear trend	No	Yes	Yes
Year dummies	Yes	Yes	Yes
City district dummies	Yes	Yes	Yes
Wooldridge-test; SSR(df)	85.674(129)	71.519(118)	26.693(100)
N	112,833	112,833	112,833

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

We also present results from separate estimations for unmarried individuals without children as this group could be expected to be very mobile and is not always eligible for welfare in other countries. As seen in Table 17 and Table 18, mandatory activation policies do not affect the entry rate for this group but lead to a significant increase in exit rate that is larger than the population average (2 percentage points, compared with an average exit rate of 35 percent for this group).

Table 17: Estimation results: Entry, unmarried without children

	(1)	(2)	(3)
Mandatory activation implemented	-0.005***	-0.003***	-0.001
	(0.001)	(0.001)	(0.001)
Time-interacted controls	No	No	Yes
Linear trend	No	Yes	Yes
Year dummies	Yes	Yes	Yes
City district dummies	Yes	Yes	Yes
Wooldridge-test; SSR(df)	490.693(130)	424.396(119)	36.380(107)
N	1,249,097	1,249,097	1,249,097

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 18: Estimation results: Exit, unmarried without children

	(1)	(2)	(3)
Mandatory activation implemented	0.031***	0.024***	0.020**
	(0.005)	(0.006)	(0.006)
Time-interacted controls	No	No	Yes
Linear trend	No	Yes	Yes
Year dummies	Yes	Yes	Yes
City district dummies	Yes	Yes	Yes
Wooldridge-test; SSR(df)	229.139(130)	203.634(119)	13.478(107)
N	146,898	146,898	146,898

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

6 Conclusions

In this paper, we have examined the dynamic effects of introducing mandatory activation of welfare recipients. Earlier literature has found that welfare participation decreases when mandatory activation is implemented, but in most cases, the researchers have only included those individuals

who already are welfare participants and therefore have only captured exit effects. In studies where the effect on the total population has been analyzed, the dynamics are still unclear as the entry and exit effects are not considered separately.

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

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

Our results indicate that entry rates decrease as a result of mandatory activation, but these results are not robust to specification changes when studying the whole population. However, when estimating the effect for a more welfare-prone group[10], the entry rate decreases by 0.3 percentage points. 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, but the effects are small. Thus, our results give some support for the hypothesis of a "threat" effect presented in Black, Smith, Berger, and Noel (2003).

We also perform placebo estimations by moving the activation year five years back and only using data for the time period before activation requirements were implemented in any city district. In the estimation of entry rates, the estimates are small and positive. Changes in exit rates are close to zero and insignificant, which strengthens the results from the main estimations.

Moreover, we check whether the effects arise immediately after the reform or if there is a phase-in period. The results do not give any clear indications of how the effect changes over time. Because the aggregated effects are rather small and unstable, this finding is not surprising. In the exit case, there seems to be a very small effect during the year of

[10]This group consists of young individuals, immigrants born in non-Western countries, single parents and people with little education.

implementation of the reform, but that effect grows over time. This may be explained by the fact that we do not know at what time during the year the reform was implemented. This means that in some districts the reform may have been carried out at the end of the year, and thus, it is likely that the effect of the reform would not be ob-servable until the following year.

To see if the results differ for different subgroups within the population, we also perform estimations for some subpopulations for which the existing literature has found larger effects. The first group consisted of young people under the age of 26. For this group, we find a significant reduction in entry due to the reform, but no exit effects could be found. The second subpopulation is immigrants from non-Western countries, and for this group, we find no effect on either entry or exit rates. The last group consists of unmarried individuals without children. This group has larger exit effects than the whole population, but no effect could be found on entry rates when activation was implemented.

The main conclusion to be drawn from this study is that the improved incentives seem to primarily affect individuals on welfare, while the impact on non-welfare participants is insignificant. However, there is large variation in how the reform has affected different groups. In the population at risk of entering welfare dependency, the programs significantly reduce the entry rate, probably driven by young individuals. These young individuals may pursue further education instead and thus become eligible for study grants. It would be interesting in future research to determine whether this is the case. The exit effect seems to be driven by unmarried individuals without children. Both young individuals and single adults without children can be assumed to be fairly mobile in the labor market, and thus they might have more ability to respond to changes in incentives than other, less mobile groups. When interpreting these results, it is important to consider that the design of the activation program probably has a large impact on its effectiveness, especially for exit effects.

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Appendix

Table 19: Estimation results: Entry and Exit

	(1) Entry	(2) Exit
Mandatory activation implemented	-0.002***	0.011**
	(0.000)	(0.004)
Female	-0.002***	0.023***
	(0.000)	(0.002)
Age	-0.003***	0.001
	(0.000)	(0.001)
Age^2	0.000***	-0.000*
	(0.000)	(0.000)
Born in Western country	0.008***	-0.043***
	(0.000)	(0.003)
Born in non-Western country	0.042***	-0.100***
	(0.000)	(0.002)
Parent	-0.003***	-0.019***
	(0.000)	(0.002)
Single parent	0.042***	-0.060***
	(0.000)	(0.003)
Compulsory schooling or less	0.019***	-0.085***
	(0.000)	(0.002)
Post-secondary schooling	-0.011***	0.051***
	(0.000)	(0.003)
Linear trend	Yes	Yes
Year dummies	Yes	Yes
City district dummies	Yes	Yes
N	2,698,222	287,953

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 20: Estimation results: Entry

	(1)	(2)	(3)	(4)
Mandatory activation implemented	-0.005***	-0.002***	-0.001*	-0.001
	(0.000)	(0.000)	(0.000)	(0.002)
Rinkeby	0.025***	0.028***	0.019***	0.019**
	(0.001)	(0.002)	(0.002)	(0.006)
Spånga-Tensta	-0.004***	-0.017***	-0.014***	-0.014**
	(0.001)	(0.001)	(0.001)	(0.004)
Hässelby-Vällingby	-0.012***	-0.030***	-0.023***	-0.023***
	(0.000)	(0.001)	(0.001)	(0.004)
Bromma	-0.014***	-0.032***	-0.025***	-0.025***
	(0.000)	(0.001)	(0.001)	(0.004)
Enskede-Årsta	-0.014***	-0.032***	-0.026***	-0.026***
	(0.000)	(0.001)	(0.001)	(0.004)
Farsta	-0.004***	-0.018***	-0.012***	-0.012**
	(0.000)	(0.001)	(0.001)	(0.004)
Vantör	-0.005***	-0.020***	-0.016***	-0.016***
	(0.001)	(0.001)	(0.001)	(0.004)
Älvsjö	-0.015***	-0.033***	-0.025***	-0.025***
	(0.001)	(0.001)	(0.001)	(0.005)
Liljeholmen	-0.010***	-0.024***	-0.019***	-0.019***
	(0.001)	(0.001)	(0.001)	(0.004)
Hägersten	-0.014***	-0.033***	-0.027***	-0.027***
	(0.001)	(0.001)	(0.001)	(0.004)
Skärholmen	-0.005***	-0.020***	-0.017***	-0.017***
	(0.001)	(0.001)	(0.001)	(0.004)
1994	-0.007***	-0.006***	-0.012*	-0.012***
	(0.001)	(0.000)	(0.006)	(0.002)
1995	-0.009***	-0.006***	-0.018**	-0.018***
	(0.001)	(0.000)	(0.005)	(0.002)
1996	-0.007***	-0.002***	-0.010	-0.010***
	(0.001)	(0.000)	(0.005)	(0.002)
1997	-0.007***	-0.001	-0.002	-0.004
	(0.001)	(0.000)	(0.005)	(0.002)
1998	-0.017***	-0.008***	-0.035***	-0.035***
	(0.001)	(0.000)	(0.005)	(0.002)
1999	-0.020***	-0.010***	-0.054***	-0.054***
	(0.001)	(0.000)	(0.005)	(0.002)
2000	-0.024***	-0.013***	-0.072***	-0.072***
	(0.000)	(0.000)	(0.005)	(0.002)
2001	-0.029***	-0.017***	-0.086***	-0.084***
	(0.001)	(0.000)	(0.005)	(0.002)
2002	-0.030***	-0.016***	-0.074***	-0.072***
	(0.001)	(0.000)	(0.005)	(0.002)
2003	-0.003***	0.012***	0.045***	0.041***
	(0.001)	(0.000)	(0.005)	(0.002)
2004	-0.014***	0.001*	-0.002	-0.003
	(0.001)	(0.000)	(0.005)	(0.002)
2005	-0.017***			
	(0.001)			
Time-interacted controls	No	No	Yes	Yes
Linear trend	No	Yes	Yes	Yes
Wooldridge-test; SSR(df)	2792.433(130)	2068.807(119)	134.418(107)	
N	2,698,222	2,698,222	2,698,222	156

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 21: Estimation results: Exit

	(1)	(2)	(3)
Mandatory activation implemented	0.021***	0.011**	0.009*
	(0.004)	(0.004)	(0.004)
Rinkeby	-0.055***	-0.036***	-0.029***
	(0.004)	(0.008)	(0.008)
Spånga-Tensta	-0.020***	-0.006	0.001
	(0.004)	(0.008)	(0.008)
Hässelby-Vällingby	0.028***	0.096***	0.077***
	(0.004)	(0.008)	(0.008)
Bromma	0.044***	0.087***	0.056***
	(0.004)	(0.009)	(0.009)
Enskede-Årsta	0.045***	0.082***	0.063***
	(0.004)	(0.008)	(0.008)
Farsta	-0.024***	0.024**	-0.000
	(0.004)	(0.008)	(0.008)
Vantör	-0.021***	0.030***	0.012
	(0.004)	(0.008)	(0.008)
Älvsjö	0.035***	0.064***	0.040**
	(0.006)	(0.012)	(0.012)
Liljeholmen	0.037***	0.059***	0.030**
	(0.005)	(0.009)	(0.009)
Hägersten	0.043***	0.078***	0.049***
	(0.005)	(0.010)	(0.010)
Skärholmen	0.033***	0.031***	0.024**
	(0.004)	(0.008)	(0.008)
1994	0.015***	0.017***	0.088
	(0.004)	(0.004)	(0.045)
1995	0.026***	0.030***	0.121**
	(0.004)	(0.004)	(0.043)
1996	-0.004	0.001	0.040
	(0.004)	(0.004)	(0.042)
1997	-0.032***	-0.025***	0.009
	(0.004)	(0.004)	(0.040)
1998	-0.006	0.003	0.045
	(0.004)	(0.004)	(0.039)
1999	0.030***	0.042***	0.121**
	(0.004)	(0.004)	(0.039)
2000	0.051***	0.064***	0.210***
	(0.004)	(0.004)	(0.041)
2001	0.139***	0.156***	0.371***
	(0.005)	(0.004)	(0.044)
2002	0.261***	0.280***	0.207***
	(0.005)	(0.005)	(0.050)
2003	0.001	0.026***	0.050
	(0.006)	(0.005)	(0.059)
2004	-0.042***	-0.013**	-0.049
	(0.006)	(0.005)	(0.050)
2005	-0.032***		
	(0.006)		
Time-interacted controls	No	No	Yes
Linear trend	No	Yes	Yes
Wooldridge-test; SSR(df)	912.000(130)	750.405(119)	36.600(107)
N	287,953	287,953	287,953

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 22: Results from estimations with time-specific treatment: Entry

	(1)	(2)	(3)	(4)
Year of implementation	-0.003***	-0.001**	-0.000	-0.000
	(0.000)	(0.001)	(0.001)	(0.002)
One year after	-0.006***	-0.003***	-0.002**	-0.002
	(0.001)	(0.001)	(0.001)	(0.002)
Two years after or more	-0.009***	-0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.003)
Rinkeby	0.026***	0.028***	0.019***	0.019**
	(0.001)	(0.002)	(0.002)	(0.006)
Spånga-Tensta	-0.004***	-0.017***	-0.015***	-0.015**
	(0.001)	(0.001)	(0.001)	(0.004)
Hässelby-Vällingby	-0.014***	-0.031***	-0.024***	-0.024***
	(0.000)	(0.001)	(0.001)	(0.004)
Bromma	-0.015***	-0.033***	-0.025***	-0.025***
	(0.000)	(0.001)	(0.001)	(0.004)
Enskede-Årsta	-0.015***	-0.032***	-0.026***	-0.026***
	(0.001)	(0.001)	(0.001)	(0.004)
Farsta	-0.005***	-0.018***	-0.012***	-0.012**
	(0.000)	(0.001)	(0.001)	(0.004)
Vantör	-0.006***	-0.021***	-0.016***	-0.016***
	(0.001)	(0.001)	(0.001)	(0.004)
Älvsjö	-0.015***	-0.033***	-0.025***	-0.025***
	(0.001)	(0.001)	(0.001)	(0.005)
Liljeholmen	-0.011***	-0.024***	-0.019***	-0.019***
	(0.001)	(0.001)	(0.001)	(0.004)
Hägersten	-0.015***	-0.034***	-0.027***	-0.027***
	(0.001)	(0.001)	(0.001)	(0.004)
Skärholmen	-0.004***	-0.020***	-0.017***	-0.017***
	(0.001)	(0.001)	(0.001)	(0.004)
1994	-0.007***	-0.006***	-0.012*	-0.012***
	(0.001)	(0.000)	(0.006)	(0.002)
1995	-0.009***	-0.006***	-0.018**	-0.018***
	(0.001)	(0.000)	(0.005)	(0.002)
1996	-0.007***	-0.002***	-0.010	-0.010***
	(0.001)	(0.000)	(0.005)	(0.002)
1997	-0.007***	-0.000	-0.002	-0.003
	(0.001)	(0.000)	(0.005)	(0.002)
1998	-0.017***	-0.008***	-0.035***	-0.035***
	(0.001)	(0.001)	(0.005)	(0.002)
1999	-0.020***	-0.010***	-0.054***	-0.054***
	(0.001)	(0.001)	(0.005)	(0.002)
2000	-0.024***	-0.013***	-0.072***	-0.072***
	(0.000)	(0.001)	(0.005)	(0.002)
2001	-0.029***	-0.017***	-0.086***	-0.084***
	(0.001)	(0.001)	(0.005)	(0.002)
2002	-0.029***	-0.016***	-0.074***	-0.072***
	(0.001)	(0.001)	(0.005)	(0.002)
2003	-0.002***	0.012***	0.044***	0.040***
	(0.001)	(0.001)	(0.005)	(0.002)
2004	-0.013***	0.001	-0.003	-0.003
	(0.001)	(0.000)	(0.005)	(0.002)
2005	-0.014***			
	(0.001)			
Time-interacted controls	No	No	Yes	Yes
Linear trend	No	Yes	Yes	Yes
Wooldridge-test; SSR(df)	2720.628(128)	2039.531(117)	131.018(105)	
N	2,698,222	2,698,222	2,698,222	156

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 23: Results from estimations with time-specific treatment: Exit

	(1)	(2)	(3)
Year of implementation	0.014**	0.011**	0.010*
	(0.004)	(0.004)	(0.004)
One year after	0.018***	0.014**	0.009
	(0.005)	(0.005)	(0.005)
Two years after or more	0.054***	0.040***	0.019**
	(0.005)	(0.007)	(0.007)
Rinkeby	-0.064***	-0.035***	-0.028***
	(0.004)	(0.008)	(0.008)
Spånga-Tensta	-0.018***	-0.010	-0.001
	(0.004)	(0.008)	(0.008)
Hässelby-Vällingby	0.034***	0.087***	0.074***
	(0.004)	(0.008)	(0.008)
Bromma	0.048***	0.078***	0.053***
	(0.004)	(0.009)	(0.009)
Enskede-Årsta	0.050***	0.074***	0.060***
	(0.004)	(0.009)	(0.009)
Farsta	-0.025***	0.025***	-0.000
	(0.004)	(0.008)	(0.008)
Vantör	-0.016***	0.021**	0.009
	(0.004)	(0.008)	(0.008)
Älvsjö	0.036***	0.063***	0.039**
	(0.006)	(0.012)	(0.012)
Liljeholmen	0.040***	0.055***	0.028**
	(0.005)	(0.009)	(0.009)
Hägersten	0.046***	0.073***	0.047***
	(0.005)	(0.010)	(0.010)
Skärholmen	0.029***	0.034***	0.025**
	(0.004)	(0.008)	(0.008)
1994	0.015***	0.018***	0.088
	(0.004)	(0.004)	(0.045)
1995	0.027***	0.033***	0.122**
	(0.004)	(0.004)	(0.043)
1996	-0.004	0.006	0.042
	(0.004)	(0.004)	(0.042)
1997	-0.032***	-0.019***	0.011
	(0.004)	(0.004)	(0.040)
1998	-0.005	0.011**	0.048
	(0.004)	(0.004)	(0.039)
1999	0.031***	0.051***	0.125**
	(0.004)	(0.004)	(0.040)
2000	0.048***	0.073***	0.212***
	(0.004)	(0.004)	(0.041)
2001	0.134***	0.163***	0.373***
	(0.005)	(0.005)	(0.044)
2002	0.255***	0.288***	0.210***
	(0.005)	(0.005)	(0.050)
2003	-0.010	0.030***	0.051
	(0.006)	(0.006)	(0.059)
2004	-0.054***	-0.009	-0.048
	(0.006)	(0.005)	(0.050)
2005	-0.051***		
	(0.006)		
Time-interacted controls	No	No	Yes
Linear trend	No	Yes	Yes
Wooldridge-test; SSR(df)	857.842(128)	728.684(117)	29.291(105)
N	287,953	287,953	287,953

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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