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On mandatory activation of welfare receivers

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WORKING PAPER 2008:24

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On mandatory activation of welfare receivers*

by

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Abstract

This paper investigates whether mandatory activation programs for welfare receivers have effects on welfare participation, employment and disposable income. In contrast to earlier studies we are able to capture both entry and exit effects. The empirical analysis makes use of a Swedish welfare reform in which the city districts in Stockholm gradually implemented mandatory activation programs for individuals on welfare. The reform is well suited for investigating effects of such programs for several reasons. First, the reform was not combined with any other policy instruments, like time limits or tax credits, making sure that we will capture effects of mandatory activation policies and nothing else. Second, the reform was initiated at different points of time in different city districts, which ease identification. Third, using data from city districts within a single local labor market we can control for confounding macro economic shocks. Overall, we find that mandatory activation of welfare receivers reduce overall welfare participation and increases employment. We also find that mandatory activation programs appear to work best for young people and for people born in non-Western countries. For disposable income, we do not find a statistically significant effect.

Keywords: Welfare reform, Mandatory activation programs, Welfare participation, Employment, Difference-in-differences
JEL-codes: I38, H31

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1 Introduction

A basic problem facing a welfare state is how to provide help towards the poor without ruining the incentives to work for those who are able to support themselves. The more generous the welfare benefits are, the more likely it is that people that would otherwise have worked are attracted by welfare. On the other hand, if no help is provided to the truly needy, some individuals will perish. The agency problem origins in the incapability of the principal (the government) to observe the agents' true need for welfare. One suggested, and commonly used, solution to this problem is to condition welfare on requirements to work or to engage in work-related activities, such as education, training or job search.[1] Activation requirements hence work like a screening device, ideally separating the truly needy from those who are not.

The idea that conditioning welfare on some type of activation is an efficient tool in poverty-alleviation programs has a long history in societal program design dating back to, e.g., the English Poor Law's, according to which "no able-bodied person was to receive money or other help from the Poor Law authorities except in a workhouse".[2] Besley and Coate (1992) formalize the mechanism behind activation requirements, and show that activation may have both short run and long run effects on welfare take-out. First, requirements on activation can make some individuals who actually can be self-supporting refrain from seeking welfare benefits, thereby reducing caseloads in the short run.[3] Second, these requirements may also decrease welfare in the longer run. If individuals need welfare because of choices made earlier on in life, activation requirements may affect these choices, since welfare has become a less attractive alternative. For example, individuals may choose to get more education, or another type of education, in order to increase the probability of future employment. Thereby, the likelihood that individuals will ever need welfare benefits is reduced.

[1] Work requirements are for example one of the instruments used in the major U.S. welfare reform in 1996. For good overviews of this reform, see Blank (2002), Grogger & Karoly (2005), and Moffitt (2007). Also, mandatory activation is also one important component in active labor market programs.

[2] One main difference between the 19th century's workhouses and modern activation programs is of course that the latter also have the intention of helping the participants in improving their job-search skills and/or their human capital.

[3] Grogger and Karoly (2005) also present an economic model describing how mandatory activation reduces welfare use as well as welfare payments.

While the theoretical model predicts that mandatory activation programs affect program participants as well as non-participants, earlier studies, mostly based on randomized experiments (see, e.g. Hamilton 2002), only address effects on program participants. Not being able to capture entry effects is an important shortcoming, which is illustrated by, e.g., the U.S. welfare reform, where, among others Grogger et al. (2003) and Moffitt (2007) argue that much of the decline in welfare use and caseloads following the reform were due to decreased entry rather than to increased exit.

In this paper, we will use quasi-experimental data from a Swedish welfare reform in order to empirically investigate to what extent conditioning welfare on participation in work related activities reduces the number of people on welfare. As opposed to earlier studies we are able to catch both entry and exit effects, though not being able to separate between the two. Through the reform, mandatory activation programs were implemented gradually in the city districts in Stockholm over the period 1998 to 2004. We will use this gradual implementation in a difference-in-differences setup. Using data from city districts within a single local labor market have large advantages, since it makes it possible to control for macro economic shocks, something that is hard when using, e.g., data on U.S. states. Also, the reform was "clean" in the sense that the activation programs on welfare receivers were implemented in isolation, hence not accompanied by e.g. financial incentives, like the EITC, or time limits. Finally, having access to very rich individual-level register data (on all individuals living in Stockholm over the period 1993–2003) we can also investigate whether the effects are heterogeneous with respect to, e.g., age and country of birth, as well as investigating the effects on a number of important outcomes, such as employment and disposable income.

Overall, we find that the activation programs decrease welfare participation and increase employment. However, the effects are different across groups; in particular, mandatory activation has especially strong positive effects for immigrants and young people. We do however not find any significant effects on disposable income.

The remainder of the paper is organized as follows: the next section gives an overview of earlier studies, whereas section 3 describes the Swedish welfare system and the activation programs in Stockholm. In section 4 the data used is described, and in

section 5 we present the empirical strategy that is employed. The main results are presented in section 6, whereas section 7 investigates the dynamics of the effects. Section 8 examines whether there are heterogeneous effects, and section 9, finally, summarizes the paper and concludes.

2 Earlier studies

Studies investigating effects of activation on both program participants and non-participants are absent. There do however exist studies investigating effects for program participants (i.e. focusing on effects on exit, but ignoring possible entry effects); in particular from a number of randomized experiments in the U.S. and Canada, as well as some non-randomized studies from Sweden and Norway.[4]

In the years preceding the major U.S. welfare reform in the 1990s, a number of states, through state waivers, implemented different types of mandatory welfare-to-work programs. Bloom and Michalopoulus (2001) present an overview of the results from 29 welfare reform initiatives in the U.S. and Canada whereas Hamilton (2002) focuses on the 11 projects that were implemented under the National Evaluation of Welfare-to-Work Strategies (NEWWS) Program. These programs typically involved mandatory employment services, earnings supplements and/or time limits. Among the programs that involved mandatory activation, there existed two types of programs, those with an employment-focused approach and those with an education-focused approach. In addition, there were also some programs that applied mixes of the two approaches. The evidence from this research indicates that programs increased employment and decreased welfare benefits among participants, but had, on net, no effect on the participants' economic well-being. Also, programs that emphasized short-term job search assistance and encouraged participants to find jobs quickly had positive effects on employment already after year one, whereas programs that emphasized longer-term skill-building activities took some time to have effects. After five years, the second type

[4] It can also be mentioned that in a related literature on unemployment insurance (UI), there exists two studies that both find that workfare, or the threat of workfare, decreases the length that participants remain on UI, see Benus and Johnson (1997) and Black et al. (2003).

of programs had however caught up with the job-first programs (see Hamilton, 2002). Most successful were the programs that combined the two approaches.

There exist two Swedish studies analyzing the effect of activation programs, Milton and Bergström (1998), and Giertz (2004). Milton and Bergström analyze a program that existed in one of the districts in Uppsala in the early 1990's. The program, which was labeled "Uppsalamodellen" ("The Uppsala Model"), demanded unemployed welfare receivers to actively seek for job full-time. They were also to meet with the case worker on a regular basis, presenting a list with the jobs they had applied for and the employers they had contacted. If the caseworker was not satisfied with the recipient's achievement, (s)he could deny further welfare benefits. Examining the effects of the program, comparing 251 individuals from the district that implemented the program with 244 individuals from a part of Uppsala that did not have the program, Milton and Bergström find in a cross-sectional analysis that the program had no effect on the time that a person was on welfare or on the probability of getting employed. Giertz (2004) studies 8 projects with 600 participants implemented in the city of Malmö in the south of Sweden. The programs differed somewhat with respect to content, but a common factor was that they built heavily on individual counseling and stimulations of job search activities. Also, the social workers tried to persuade, rather than force, recipients to participate, but there existed some cases where sanctions had been imposed. Comparing contemporary participants with earlier participants, Giertz finds no effects on welfare costs.

The welfare system in Norway resembles the Swedish one in many aspects. Dahl (2003) compares welfare recipients at local offices that have required activation with recipients at local offices without such requirements. He finds no effects of activation programs on the likelihood the welfare recipients leave welfare for work.

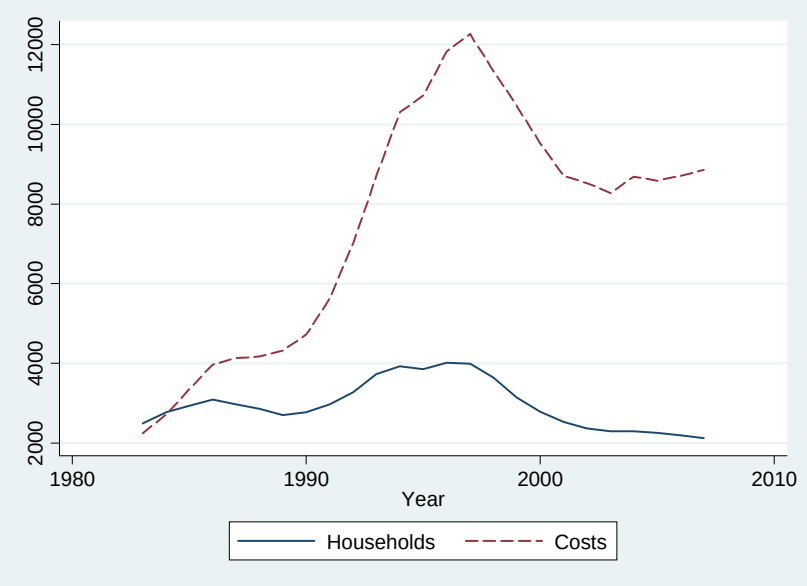
Common for both the Swedish and the Norwegian studies is that they only study effects on participants. Just as the U.S. studies, they therefore miss potential entry effects.

3 Welfare in Sweden

The Swedish social security system is often considered as one of the most extensive and generous systems in Western welfare states. The responsibility for supplying welfare benefits (the Swedish term is social assistance) lays on the local governments, even though The Social Services Act constitutes the framework for welfare benefits. It is constructed as a frame law, which means that the interpretation and enactment of the law is delegated to each municipality. Since 1982 the law ensures all Swedish and foreign citizens living in Sweden the right to obtain welfare benefits in the absence of other means of economic support. As opposed to the situation in many other countries (e.g. U.S. and U.K.), receiving welfare is not dependent on having children. However, in order to be eligible for welfare benefits all other means, including savings and valuable assets, must be exhausted. The benefit level should ensure a reasonable standard of living, but it is up to the municipalities to decide the exact level. However, until 1998, there existed recommendations from the National Board of Health and Welfare, and since 1998 these recommendations have been replaced by a minimum level.

In 2006, 392,500 individuals (or about 4.3 percent of the population) received welfare benefits (some of the receivers were newly arrived immigrants). About 30 percent of these received welfare more than 10 months during a year, and are therefore defined as long term receivers. Figur 1 describes the development of the number of welfare receivers as well as the costs for welfare benefits since the mid-eighties up to 2006. As can be seen from the figure, starting in the end of the nineties, both the number of individuals receiving welfare and the costs for welfare benefits have dropped. However, the costs per recipient (not shown in the figure) have increased, indicating that the individuals that are still on welfare are so for a longer time. In 1999, the Swedish government declared an ambition to cut costs for welfare benefits in half, but this objective has proven hard to accomplish; even though welfare costs have decreased over time it has not decreased with 50 percent. Also, since 2003 the decrease seems to have ended.

Figur 1 Welfare households (100's) and costs (m. of SEK) for welfare benefits 1983–2007.



Source: Statistics Sweden.

Welfare receivers are not evenly spread across different groups in society. The probability of receiving welfare is largest among unemployed youths without eligibility for unemployment benefits, single mothers and individuals born outside Western countries.

During the 1980's the right to welfare was not tied to any specific requirements on the receiver of welfare benefits other than having exhausted all other means of financing and being available for work. "Being available for work" was in the beginning of the 1980's defined by The National Board for Health and Welfare as searching for jobs and not turning down any "suitable offers". A "suitable offer" was perceived as a job matching the skills and qualifications of the individual and in line with collective agreements. However, as the 1990's recession led to difficulties in financing the social welfare system, the right to welfare became subject to stricter means-testing and the requirement of being available for work was extended to also include participation in

internships and labor market projects.[5] At the same time, the generosity of welfare benefits was reduced in many municipalities.

The right to require participation in activation programs by the welfare receivers was formally introduced by a change in the Social Services Act in 1998.[6] The new law made it possible for municipalities and city districts to demand participation in work related activities, such as internships and supervised job search, in return for welfare benefits.

In this paper, we will focus on the city districts in the city of Stockholm. The city of Stockholm is by far Sweden's largest municipality, with approximately 780,000 inhabitants in 2006. It makes up the central part of a much larger labor market area. Next, we will turn to a description of the programs in place in Stockholm.

4 Empirical setting

During the period studied (1993–2003), the municipality of Stockholm was divided into 18 city districts (see Map in Appendix A).[7] The city districts are responsible for the majority of the municipality's services within their geographical areas.[8] However, the municipality sets taxes[9] and allocates funds between the city districts. In addition, it, through guidelines, defines overall goals. The political composition in the District Councils is equivalent to that of the Municipal Council, which is elected every fourth year. Hence, there are no elections at the city district level and the political majority is the same all over Stockholm.

The earliest examples of activation programs in Stockholm are from 1998 and 1999 when Rinkeby and Skärholmen introduced programs intended to enroll all unemployed

[5] For a discussion of the welfare system during the 1990s, see Johansson (2000, 2001) and Bergmark (2000).
[6] Many of the changes prescribed by the 1998-law reflected trends that had been in practice earlier; Salonen and Ulmestig (2001) show that many municipalities seem to have applied rules similar to the new policy even before 1998. Also, the rule has been used in a wider sense, for example been extended to apply to other groups than youths.
[7] Since January 1, 2007, the number of city districts has decreased to 14.
[8] The districts' responsibilities include refugee reception services, recreational programs for children and youth, pre-school, income support, budgetary counseling and debt restructuring, consumer advisory services, local business and labor market initiatives, local urban environment issues, maintenance of parks, services and care for the disabled, social services, care and treatment, family law, and elderly services.
[9] In Sweden, municipalities have the right to collect revenues from a local, proportional, income tax. They are also allowed to charge user fees for some of the services they provide.

welfare recipients in job searching activities.[10] They were followed by Kista and Farsta in 2001, and since then by many other city districts. In fact, since 2004 there are mandatory activation programs in force in all city districts.

These programs have been known under the name "activation programs" and typically require a number of hours' attendance each week. According to the official descriptions, the aims of the programs are to facilitate job search for the unemployed and "coaching" the participants to become self-supporting. However, in a case study by Thorén (2005) it is concluded that "municipal activation policy in its practical form will not necessarily improve client's prospects to find employment since its primary function rather is as a method to control clients' entitlement to social assistance". The organization of the programs makes it possible for the welfare administration to monitor the willingness to work.

In order to determine when the different city districts launched mandatory activation, we have conducted a questionnaire addressed to the heads of the welfare administration in each city districts.[11] The questionnaire was complemented with telephone interviews whenever it was difficult to categorize a program based on the information given in the questionnaire. Based on the information from the questionnaire and the interviews, we can determine which year a mandatory program was launched in each city district. In order to be labeled as "a mandatory program" it has to be directed towards all unemployed individuals receiving social assistance and to require attendance for some hours per week. The programs all use a common reporting system in which the attendances of the participants are registered daily. Most importantly, the register is open to social workers, which means that absence is immediately detected, and will in many cases lead to reduced benefits. Some of the programs are extensions of previous

[10] Noteworthy is that, as opposed to in some other Swedish municipalities, there did not exist any large scale activation programs in any of Stockholm city districts before 1998 when the Social Service Act was changed.
[11] The questionnaire is given in Appendix B.

programs, but the ambitions of the current programs are much higher.[12] Table 1 shows when the activation programs subject to this study were implemented.[13]

A valid question is of course whether we can trust the answers given by the welfare administrators. Do the programs really include *all* individuals receiving welfare and are they as harsh as the administrator claims? Without conducting thorough implementation studies we can of course never be 100 percent certain.[14] However, as far as we can think of, there are no reasons for the administration not to tell the truth. Also, it is worth noting that if the programs de facto are not as compulsory and "tough" as stated by the heads of the welfare administration, we would get estimates that, if anything, are biased towards zero. Hence, the effect that we find in the paper should be seen as a lower bound of the effects of general activation programs.

Table 1 Starting years for activation programs in Stockholm city districts

District	Year
Skärholmen	1999
Farsta	2001
Kista	2001
Älvsjö	2002
Hägersten	2003
Liljeholmen	2003
Spånga-Tensta	2003
Bromma	2004
Enskede-Årsta	2004
Hässelby-Vällingby	2004
Vantör	2004

[12] In the earlier years, job seeking activities were often limited to occasional contacts with an employment counselor whose role mostly consisted of discussing the client's situation and possibly arranging labor market training. The cooperation between social administration and consultants was scarce and a common view is that the follow-up was insufficient.

[13] Since our data ends in 2003, the programs started in 2004 are not used in the identification of the program effect. Also, in one districts it is impossible to establish when the "ambitious" program begun (Skarpnäck) and Skarpnäck is therefore excluded. In addition, the most central city districts are excluded from the sample altogether as the share of receivers of welfare benefits is very low in this part of the city and as their methods are difficult to categorize. Finally, Rinkeby is excluded from the analysis since it is an outlier in several respects, not the least in terms of welfare participation and share of inhabitants born outside Sweden. We have also estimated the model when excluding other city district, one at the time, and it turns out that Rinkeby seems to be different. It is important to remember that excluding Rinkeby implies that we cannot draw inference from our results to city districts like Rinkeby.

[14] We would like to stress that the questionnaire has been complemented with several telephone conversations where we have tried to get more detailed information when needed. In addition, in the interviews we ask about programs that actually have been in place a number of years, making it likely that it is the actual program, not just the ambitions of the program that we capture.

In order to give a better understanding of the programs, we will describe the program in Skärholmen in more detail. The program in Skärholmen is one of the most documented programs (see Ekström, 2005, and Thorén, 2005, for a more detailed description) and is to a large extent comparable to other, less documented programs in other parts of the city.[15] For example, three other city districts (Hägersten, Liljeholmen and Älvsjö) have joined the project and during our study period the four districts shared the facilities in Skärholmen.

In 1998 the city district of Skärholmen began to apply a method that has later become known as "the Skärholmen model". During the first year the activities were only directed to students who were unemployed during the summer, but in 1999 the program was extended to include all unemployed receivers of welfare benefits. When welfare applicants enter the welfare services, those whose main motivation for applying for welfare is categorized as “unemployment” are immediately sent to “The Jobcentre” (the local employment agency that administers the job seeking activities for welfare receivers). Usually, the applicants have to meet Jobcentre personnel before their application is processed. Sometimes the applicant is given suggestions on jobs to seek or other activities already on their first visit at the Jobcentre. As long as a person has not found a job or an activity to participate in, the program requires three hours of daily attendance at the Jobcentre, either in the morning or in the afternoon. Every second week the schedule rotates in order to prevent black market work. The central component in the model is job-seeking activities. These are facilitated by providing job seekers with an individual labor market coach and material which may alleviate job search – such as computers, telephones and stationery. In addition to job-seeking activities, the program involves participation in internships, shorter education such as computer courses and other activities arranged by the city district, such as gardening or cleaning in the community. As noted by Thorén (2005), much of the activities aim at testing the participants’ willingness to work. There is also a large amount of cooperation between

[15] Blomberg et al. (2006) study the activation programs implemented in six city districts (Vantör, Skärholmen, Kista, Hässelby-Vällingby, Rinkeby and Spånga-Tensta) and conclude that the programs are similar in many respects. For example, all districts have reception offices from which the welfare applicants are directed to activation centers. At these centers, a mix of the following activities takes place: own job-search, assisted job-search, internships, work practice, and job-guidance.

the welfare office and the coaches at the Jobcentre. Not participating actively at the centre will be reported to the welfare administrator who can decline the recipients their welfare benefit.

The data from the questionnaire is combined with individual register data from Statistics Sweden. The register data contains yearly information on all individuals aged 18–64 living in the municipality of Stockholm, over the years 1993 through 2003. Table 2 reports summary statistics on the variables used in this paper. In order to measure the effects on welfare participation we use a dummy (*Welfare receiver)* that indicates whether the individual lives in a household that received welfare during the year.[16] We see from Table 2 that this is true for approximately 9 percent of all individuals in our sample. A potential problem with this measure of welfare participation is that it is quite crude in the sense that an individual is considered as being a welfare participant if he or she has received some welfare benefits at some point during a year. The amount received differs however substantially between individuals and it is therefore also interesting to investigate the effect on the amount of welfare money received during a year (*Welfare benefits*).[17] The average amount received is approximately 2,000 SEK per year. This might seem like a low figure, but note that the zeros are included. For those individuals that did receive some welfare, the average amount received is approximately 22,300 SEK.

Since we are interested in what happens to individuals that potentially leave welfare or refrain from entering into welfare, we will also investigate the effects on employment. We use four different measures of employment: A dummy indicating whether the individual worked as least 1 hour in November (*Employed in November*), a dummy indicating whether the individual was employed all 12 months (*Employed all year*), a variable that measures how many months the individual was employed in the year (*Months employed*), and income earned from employment (*Income from employment*). In the variables *Employed all year* and *Months Employed* an individual

[16] Welfare benefits are directed to households, not individuals. For simplicity, we will in the rest of the paper write it as if it was the individual that received welfare. What we mean is however whether the individual lived in a household that received welfare.
[17] The variable "Welfare benefits" is the individual's share of the household's welfare benefits.

was defined as employed if the work performed that month generated an income larger than 25 percent of the minimum wage of workers in the hotel and restaurant sector. Summary statistics for the different employment measures are reported in Table 2. Approximately 74 percent of the population is employed according to the first definition.

Finally, we will investigate what happens with the economic well-being of individuals by investigating effects on disposable income. As we can see from the table below disposable income varies substantially between individuals.

In the empirical analysis we will also control for a number of individual specific characteristics; summary statistics for those variables are also provided in Table 2.[18]

Table 2 Summary statistics

Variable	Mean	Std. Dev.	Min	Max
Outcome variables				
The probability of receiving welfare	0.089	0.285	0	1
Welfare benefits	2,004	9,571	0	510,800
Employed in November	0.737	0.440	0	1
Employed all year	0.650	0.477	0	1
Months employed	8.542	5.136	0	12
Income from employment	164,234	170,712	0	25,977,500
Disposable income*	158,138	266,384	-1,551,500	223,910,800
Control variables				
Woman	0.499	0.500	0	1
Age 18–25	0.151	0.358	0	1
Age 26–35	0.262	0.440	0	1
Age 36–45	0.357	0.479	0	1
Age 46–64	0.231	0.421	0	1
With young children (<7 years)	0.184	0.387	0	1
Born in Sweden	0.776	0.417	0	1
Born in Nordic country	0.047	0.211	0	1
Born in Western country	0.025	0.156	0	1
Born in East European country	0.036	0.186	0	1
Born in other country	0.120	0.325	0	1
Elementary school< 9 years	0.204	0.403	0	1
Elementary school 9 years	0.259	0.438	0	1
High school	0.197	0.398	0	1
College/University<2 years	0.165	0.371	0	1
College/University>2 years	0.166	0.372	0	1
Ph D	0.009	0.095	0	1
Immigration 2–4 years ago	0.017	0.131	0	1
Immigration 5–9 years ago	0.050	0.217	0	1
Immigration 10–14 years ago	0.045	0.207	0	1
Immigration>15 years ago or not at all	0.888	0.315	0	1
1 child	0.201	0.401	0	1
More than 1 child	0.203	0.402	0	1

* Only available for the years 1995–2003.

The city districts are rather heterogeneous with respect to demographic composition and outcome variables, which is illustrated by Table 3 that presents summary statistics from 1993 on some of the outcome variables as well as the share foreign born.

Table 3 City district characteristics in 1993

	Share welfare receivers	Average welfare benefits	Share employed (November)	Average disposable income*	Share of foreign born individuals
Bromma	0.06	1,087	0.76	149,045	0.12
Enskede-Årsta	0.08	1,525	0.73	129,633	0.16
Farsta	0.13	2,431	0.70	124,991	0.17
Hägersten	0.08	1,449	0.73	130,481	0.15
Hässelby-Vällingby	0.08	1,288	0.74	137,476	0.15
Kista	0.19	3,847	0.67	120,446	0.42
Liljeholmen	0.10	1,922	0.71	122,920	0.16
Skärholmen	0.13	2,092	0.66	119,657	0.32
Vantör	0.14	2,606	0.68	120,665	0.20
Spånga-Tensta	0.17	3,209	0.64	124,431	0.42
Älvsjo	0.07	1,050	0.76	140,942	0.14

* Only available for the years 1995–2003.

Comparing the figures in Table 3 with the year of program implementation shown in Table 1 it is worth noting that it is the city districts with the highest welfare participation that seem to have implemented the policy first. In the next section we will discuss how this is taken into account in the empirical analysis.

5 Econometric strategy

When investigating the effect of a specific policy on individual behavior, the econometric challenge is to separate effects of the policy from other factors that also may affect individual behavior. If one only compares the behavior of an individual before and after a policy change, there is a major risk that one also captures differences in the behavior that depends on factors other than the policy. One way to isolate the effect of the policy from all other things that may affect individual behavior is to compare the changes in behavior of individuals residing in a city district that has implemented the policy with changes in the behavior of individuals residing in a city

[18] Exact definitions of all variables as well as data sources are given in Appendix C.

district that has *not* implemented the policy, thereby netting out other factors that may affect individual behavior. We will use this difference-in-differences approach in the paper.

The identifying assumption for this model is that if the policy had not been implemented, welfare caseloads in the city district that implemented the policy would have changed in the same way as in the city districts that did not implement the policy. As mentioned above, the city districts implemented the policy in different time periods. The labor market in these years (1998–2003) was somewhat turbulent, with decreasing unemployment rates until 2001 followed by a small increase. Barth et al. (2004, 2006) have shown that that labor market conditions matter differently for different groups, i.e., the weaker the group is with respect to labor market attachment, the more sensitive is the group to fluctuations in labor market conditions. Given that the city district with the potentially weakest groups were those that implemented mandatory activation first, one might worry that not taking this into account would put the identifying assumption at risk. In order to avoid this potential problem, we will control for a number of individual specific characteristics and also allow the coefficients for these characteristics to have different effects over time. In doing this, we control for the fact that a specific demographic structure in the early years may affect welfare caseloads differently than having the same demographic structure in the later years, when the labor market conditions differ. [19]

Even after controlling demographics in the flexible way described above, there might be different time trends in the different city-districts. We will therefore also allow for linear, city-district specific time-trends. The equation that forms the basis for our empirical analysis is given by

$$Y_{ijt} = \alpha_j + \tau_t + \beta program_{jt} + \theta_t X_{ijt} + trend_j + \varepsilon_{ijt}. \qquad (1)$$

[19] If welfare prone individuals move between city districts depending on whether the districts have implemented strict mandatory activation programs or not we might be worried that equation (3) captures these effects rather than effects on welfare participation. However, Edmark (2007) does not find that the moving patterns of welfare prone individuals differ from the moving patterns of non-welfare prone individuals.

where Y_{ijt} is the outcome of interest for individual *i* in city district *j* in time-period (year) *t*, α_j are city district-specific fixed effects, τ_t are time-specific fixed effects that are common for all city districts, and $program_{jt}$ is an indicator variable that takes the value 1 if the policy is implemented in city district *j* in year *t* (and all years thereafter), X_{ijt} is a vector of demographic covariates, $trend_j$ are city district specific time trends, and ε_{ijt} are error terms.

One thing that equation (1) does not control for is unobserved city-district specific shocks that might vary over time. If such shocks exist, they might cause two different kinds of problems. First, if the shocks are correlated with the timing of the reform, β might capture these shocks rather than true program effects. Second, such shocks might imply that the standard errors of individuals within the same city district will be correlated, making the estimated standard errors biased and, thereby, invalidating inference.

Since we focus on city districts within a close geographical distance which also make up the centre of a much larger labor market region, we believe that we are likely to capture any such shocks with the common time effect together with the time-varying coefficient on the control variables. However, to examine whether there still exists any correlation within the residuals that make inference problematic, we will conduct the test suggested by Wooldridge (2003). He suggests to initially restrict the unobserved city-district specific shocks to zero and then solve for β using the minimum distance (MD) estimator. The efficient MD-estimator is obtained by estimating the following model;

$$Y_{ijt} = q_{jt} + \theta_t X_{ijt} + \eta_{ijt} \qquad (2)$$

and then, using the predicted $\hat{q}_{jt}$ from equation (2), estimate equation (3) using weighted least squares;

$$\hat{q}_{jt} = \alpha_j + \tau_t + \beta program_{jt} + trend_j + \mu_{jt} \qquad (3)$$

where the weights are given by $1/\hat{\sigma}_{jt}^2$, $\hat{\sigma}_{jt}$ being the estimated standard errors for q_{jt} from the estimation of equation (2), and where η_{ijt} and μ_{jt} are error terms. Under the null of no unobserved city specific time shocks, $SSR_w \overset{a}{\sim} \chi^2(S-K)$, where S is given by $J \times T$ and K is the number of estimated parameters in (3). If H_0 is rejected, then Wooldridge proposes to instead use the two-step estimator suggested by Donald and Lang (2007). This two-step estimator is conducted by estimating (2) and (3), but where the weights for (3) are given by the population share of the different city districts.

As a further sensitivity check, we will also conduct a placebo-experiment where we pretend that the programs took place five years before their actual implementation, and then estimate the effects of these placebo-programs using data from the pre-reform period, i.e. before any city district had implemented any program. Furthermore, we will investigate whether there exist any pre-program effects, in which case we might suspect that the treatment is not exogenous conditioning on controls. If we find an effect of the true timing of the reform, but no effect for the placebo reform or pre-program effects, we will be more confident that we have in fact captured relevant differences in the city-districts with our model specification, and thus finding the true program effect.

6 Average effects of mandatory activation

In this section, we will first estimate the baseline DD-estimates of the effects of mandatory activation on welfare, employment and disposable income. Thereafter, we will conduct some placebo-experiments in order to validate that we have indeed estimated treatment-effects.

6.1 Effects on welfare participation

According to the theoretical prediction from the Besley and Coate (1992) model, welfare participation should decrease as a consequence of the introduction of mandatory activation programs. Table 4 presents the effect of mandatory activation on the probability for an individual to receive welfare sometime during a year as well as the amount received (including zeros). We use a linear probability model, controlling for several observed as well as unobserved characteristics of the city districts. In the first

two columns we estimate the model using individual level data, thereby ignoring any city district specific time shocks. Doing this, we find that the probability that the household receives welfare decreases with 0.4 percentage points when mandatory activation is implemented. This corresponds to a 4.5 percent decrease at the mean value. Also, the amount received decrease with almost 80 SEK per year. This corresponds to a decrease with 3.6 percent at the mean value.

Whether or not it is possible to draw correct inference from the estimated standard errors depends on whether there are any city district specific time shocks that we have not controlled for. We test this along the lines suggested by Wooldridge (2003). The resulting test-statistic is given in the third line from the bottom in the table. Since the critical value at the 10-percent significance level is 106.5 we have to reject the null of no city district specific shocks. We therefore turn to the Donald-Lang-estimates presented in columns (3) and (4). They show that mandatory activation decreases welfare participation, but that the effect is only statistically significant (at the ten-percent level) for the probability of receiving welfare. For the benefit level, the effect is statistically significant at the 20 percent level. These results indicate that mandatory activation may reduce welfare participation but no strong conclusion can be drawn due to the large standard errors.

Table 4 Effects on welfare participation

	(1)	(2)	(3)	(4)
	Individual level data		Donald and Lang estimator	
	Prob. of receiving welfare	Welfare benefits, SEK	Prob. of receiving welfare	Welfare benefits, SEK
Treatment effect	-0.004***	-79.5**	-0.005*	-97.0
	(0.001)	(34.2)	(0.003)	(80.82)
Wooldridge test: SSR_w (df)	529.2 (89)	285.8 (89)		
R-squared	0.15	0.10		
No. of obs.	2,535,573	2,535,573	121	121

Notes: Robust standard errors in parentheses. ***, **, and * denote significance at 1, 5 and 10 percent level respectively. Standard errors in columns (1) and (2) are clustered on household level. The estimated models include city district fixed effects, time effects, individual characteristics (gender, education level, immigration year, region of birth, children and age), time varying parameters on covariates and district specific time trends

6.2 Effects on employment

In this section we will examine the effects of activation programs on employment.[20]

We use four different variables in order to capture effects on employment. The first is a dummy taking the value one if the individual was employed in November in a given year, zero otherwise. The second is the number of months that the individual has been employed during a year. The third is a dummy indicating whether an individual has been employed all 12 months of the year, and the fourth is income from employment. The results are presented in Table 5. Regardless of which employment-measure we use, we find that the mandatory activation increases employment. Starting with the November-measure, we find that mandatory activation increases the individual's probability of being employed with 0.4 percentage points, which corresponds to an increase with 0.5 percent. Furthermore, the number of months that the individual is employed increases with 0.04 months (1 percent) and the probability that the individual is employed the full year increases with 0.3 percentage point (0.5 percent). Finally, income from employment increases with 1,283.4 SEK per year, which corresponds to 0.8 percent of the mean value in the sample.

Conducting the Wooldridge test, we cannot reject the null of no city district specific time-shocks. Since all city districts are centered in the middle of the same labor market region, this result is as expected. Hence, we do not need to turn to the Donald and Lang (2007) estimator, but can use individual level data for inference. Doing this, we conclude that all estimates are statistically significant.

[20] The predictions from the Besley and Coate (1992) model are not explicit about other outcomes than welfare participation, but implicitly there is an understanding that mandatory activation should have a positive effect on the employment rate and, possibly, other labor market outcomes.

Table 5 Effects on employment

	(1)	(2)	(3)	(4)
	The probability of employment in November	The number of months employed	The probability of being employed full year	Individual level data Income from employment
Treatment effect	0.004***	0.041***	0.003**	1,283.4***
	(0.001)	(0.015)	(0.001)	(397.1)
Wooldridge test: SSR_w (df)	72.2 (89)	86.8 (89)	97.2 (89)	56.9 (89)
R-squared	0.13	0.14	0.14	0.23
No. of obs.	2,535,573	2,535,573	2,535,573	2,535,573

Notes: Robust standard errors clustered on households in parentheses. ***, **, and * denote significance at 1, 5 and 10 percent level respectively. The estimated models include city district fixed effects, time effects, individual characteristics (gender, education level, immigration year, region of birth, children and age), time varying parameters on covariates and district specific time trends

6.3 Effects on economic well-being

Another interesting question is how well the individuals are doing on net in economic terms. Thanks to reliable, register-based, information on individuals' disposable income[21], we are able to analyze this, something that has not been done in earlier studies on the U.S. welfare reform when relying on observational data.[22] From the results, presented in Table 6, it is clear that, on net, the introduction of mandatory activation leads to a significant increase in disposable income of 1,947 SEK. However, the Wooldridge test rejects the null of no city district specific shocks[23] and the standard errors for the Donald and Lang-estimator are large. Therefore, we must conclude that we cannot find any statistically significant effects of mandatory activation on disposable income.

[21] Disposable income is defined as all income received (from work, social security systems, transfers, etc) minus taxes and other payments (such as study loan payments).
[22] The income data available in the U.S. is self-reported and, as is discussed in Meyer and Sullivan (2003), income therefore tends to be underreported, especially by welfare recipients. Using consumption data instead, Meyer and Sullivan (2004) examine the material conditions of single mothers and their families to assess the net effect of the U.S. welfare reforms on the well-being of these families. They find that the material conditions of single mothers have not declined, either in absolute terms or relative to different comparison groups (such as single childless women).
[23] The critical value at the ten percent level is 114.1.

Table 6 Effects on disposable income

	(1)	(2)
	Individual level data	Donald-Lang estimator
Treatment effect	1,947***	1,929
	(750.6)	(2,197)
Wooldridge test:		
SSR_w (df)	129.0 (69)	
R-squared	0.04	
No. of observations	1,882,630	88

Notes: Robust standard errors in parentheses. ***, **, and * denote significant at 1, 5 and 10 percent level respectively. Standard errors in columns (1) and (2) are clustered on household level. The estimated models include city district fixed effects, time effects, individual characteristics (gender, education level, immigration year, region of birth, children and age), time varying parameters on covariates and district specific time trends.

6.4 Placebo-experiment

In order to investigate whether the estimated effects in the analysis above are indeed program effects we will next conduct a placebo-experiment. If we do not find any effect of this placebo-reform, we will be more confident that the estimated effect is in fact a program effect and not just an unobserved city-district specific shock.

In the placebo-experiment we use data from the period 1993–98, i.e. the period before any mandatory activation program had been put in place in any city district. In order to create placebo-reforms we pretend that the programs were implemented five years before they actually were. Hence, we pretend that Skärholmen implemented the program in 1994 and that Farsta and Kista followed in 1996 etc. We then estimated the same model as in sections 6.3–6.4. Doing this, we get the results presented in .Table 7.

Table 7 Placebo-experiment

	"True reform"	"Placebo-reform"
Prob. of receiving welfare	-0.004***	-0.001
	(0.001)	(0.001)
Welfare benefits, SEK	-79.5**	-1.1
	(34.2)	(32.8)
The probability of employment in November	0.004***	-0.000
	(0.001)	(0.001)
The number of months employed	0.041***	0.008
	(0.015)	(0.016)
The probability of being employed full year	0.003**	0.001
	(0.001)	(0.002)
Income from employment	1,283.4***	77.9
	(397.1)	(300.6)

Notes: Robust standard errors clustered on households in parentheses. ***, **, and * denote significant at 1, 5 and 10 percent level respectively. The estimated models include city district fixed effects, time effects, individual characteristics (gender, education level, immigration year, region of birth, children and age), time varying parameters on covariates and district specific time trends.

Comparing the "true reform"-estimates with the estimates for the placebo-reform, we can conclude that all estimates for the latter are statistically insignificant. Hence, we cannot reject that the effects of the placebo-reforms are zero. Furthermore, all the point-estimates are small and close to zero. These findings strengthen us in the belief that mandatory activation affects employment and possibly also welfare participation.

Another way to investigate whether we have captured true program effects or if the results depend on some trend that we have not adequately controlled for is to – in addition to the treatment indicator in equation (1) – also include dummies for the years preceding the implementation of the programs.

Table 8 shows the results from these estimations allowing for the reform to have some effect already two years before the programs were introduced. For most of the outcome variables (four out of six) we do not find any statistically significant estimates for the two years preceding the programs. Also, the point estimates are all much lower than the point estimate for the program-period. For the probability of receiving welfare and for the number of months employed, we find some statistically significant effects already before the program start. However, the point-estimates are considerably lower than for the actual reform year. We take this as further evidence that we have in fact captured true program effects.

Table 8 Effects the years before program implementation

	Welfare recipient	Welfare benefits	Prob. of employed in November	The number of months employed	Prob. of employed full year	Income from employment
t	-0.006**	-116.0*	0.005*	0.0736***	0.004*	1,095.8
	(0.002)	(53.9)	(0.002)	(0.0248)	(0.002)	(652.7)
t-1	-0.004*	-70.6	0.002	0.0353*	0.001	-211.8
	(0.001)	(43.3)	(0.002)	(0.0197)	(0.002)	(497.2)
t-2	0.001	9.4	0.000	0.0278*	0.001	-148.5
	(0.001)	(33.6)	(0.001)	(0.0155)	(0.001)	(376.2)
R-squared	0.153	0.100	0.133	0.140	0.147	0.231
No. of obs.	2,535,573	2,535,573	2,535,573	2,535,573	2,535,573	2,535,573

Notes: Robust standard errors clustered on households in parentheses. ***, **, and * denote significance at 1, 5 and 10 percent level respectively. The estimated models include city district fixed effects, time effects, individual characteristics (gender, education level, immigration year, region of birth, children and age), time varying parameters on covariates and district specific time trends.

7 Are the effects sluggish?

It could be the case that it takes some time before the programs start to have effects on welfare and employment if, for example, the programs have some start-up-period before they are fully implemented, or if it takes time before inhabitants realize that the social assistance office demands activation. If so, we would expect the effects of mandatory activation to increase over time. In order to investigate this, we have estimated a more dynamic version of the model including two additional indicators, one indicator taking the value one the year after the reform and afterwards and zero otherwise, and the other indicator taking the value one two years after the reform and afterwards. These results are given in Table 9. A statistically significant estimate for t+1 or t+2 should be interpreted as the effect being larger the year after/two years after the reform. As is clear from the table, the full effects kick in already the year of implementation.

Table 9 Are the effects sluggish?

	Welfare recipient	Welfare benefits	Prob. of empl. in November	The number of months employed	The prob. of being employed full year	Income from employment
t	-0.005**	-81.4*	0.004**	0.0457***	0.00433***	1,370.2**
	(0.001)	(32.5)	(0.001)	(0.0151)	(0.00146)	(395.9)
t+1	0.001	6.5	0.001	-0.0154	-0.00356**	-317.8
	(0.001)	(36.6)	(0.002)	(0.0170)	(0.00168)	(414.9)
t+2	-0.001	2.7	0.001	-0.0271	-0.00227	383.7
	(0.002)	(49.8)	(0.002)	(0.0217)	(0.00207)	(511.9)
R-squared	2535573	2535573	2535573	2535573	2535573	2535573
No. of obs.	0.153	0.100	0.133	0.140	0.147	0.231

Notes: Robust standard errors clustered on households in parentheses. ***, **, and * denote significance at 1, 5 and 10 percent level respectively. The estimated models include city district fixed effects, time effects, individual characteristics (gender, education level, immigration year, region of birth, children and age), time varying parameters on covariates and district specific time trends.

8 Does mandatory activation affect vulnerable groups differently?

So far we have estimated average effects. However, as is shown by Table 10, there are certain groups for which welfare participation is especially high, i.e. younger people and those born outside Sweden (in particular for those born in “other countries”, i.e. Asia, Africa and Latin America), and families with children, especially those with a single parent. It is therefore of interest to investigate whether the mandatory activation

programs have different effects for these groups. Also, welfare might be extra harmful for young people or immigrants, due to, e.g., scarring effects, making it especially important to understand how to decrease welfare participation in these groups.[24] In this section we will investigate whether the effects of mandatory activation are heterogeneous with respect to family status, age and country of origin. We do this by extending the baseline model in equation (1) with interaction terms between the variable indicating whether a mandatory activation program had been introduced in a given city district in a given year (i.e., the *program*-variable) and the socio-economic variable of interest (family status, age, or country of origin). In the tables we present the coefficients for the *program*-variable (i.e., the difference-in-differences estimate) and the coefficients for the interaction variables. To save space, we do not report the results for the probability of being employed the full year and given the results in the sensitivity analysis, we refrain from estimating heterogeneous effects for disposable income.

Table 10 Welfare participation among different groups

	Welfare receiver	Welfare benefits	Employment			Income from work
			November	Months	All year	
All	0.089	2,004	0.737	8.542	0.650	164,234
Age						
18–25	0.14	2,494	0.565	6.272	0.377	78,720
Country of birth						
Born in Nordic country	0.095	2,223	0.711	8.318	0.649	146,126
Born in Western country	0.062	1,311	0.598	6.956	0.533	125,532
Born in East Europe	0.157	4,241	0.575	6.615	0.494	109,686
Born in other country	0.294	7,250	0.512	5.877	0.411	84,201
Family status						
Cohabiting parents with small children	0.090	1,413	0.806	9.056	0.702	176,020
Single parent-households with small children	0.319	5,953	0.621	6.841	0.493	90,333

[24] Skans (2004) shows that experiencing unemployment subsequent to graduation from high school has negative effects on both unemployment and earnings at least five years after graduation, whereas Åslund and Rooth (2007) show that exposure to high local unemployment rates affects immigrants for at least ten years after entry to Sweden.

8.1 Family status

We begin by examining whether mandatory activation has different effects on families with children under the age of 7. We have separate indicators for single parents and cohabiting parents. From the results, presented in Table 11, it seems like mandatory activation typically does not have any significantly different effects on single parents with young children. The same goes for cohabiting parents with young children except for the monetary outcomes “welfare benefits” and “income from work”. While mandatory activation reduces welfare benefits for two-parent families with almost 700 SEK, there are no significant effects for single-parent households or households without young children. On the other hand, mandatory activation has a negative effect on income from work for cohabiting parents with young children, while it for the other groups has a significantly positive effect. A possible explanation for the differences in the effect on income might be that those no longer receiving welfare benefits in families with two adults become dependent of the income of their partner instead of turning to paid work.

Table 11 Heterogeneous effects with respect to family status

	Welfare receiver	Welfare benefits, SEK	Employed in November	Number of months employed	Income from work, SEK
DD-estimate	-0.004*** (0.001)	31.3 (36.5)	0.004** (0.001)	0.036** (0.017)	2771.2** (466.2)
DD-estimate*Single parent with young children	-0.004 (0.007)	-386.7 (206.8)	-0.011 (0.008)	-0.148* (0.088)	2530.0 (2058.8)
DD-estimate* Cohabiting parents with young children	-0.002 (0.003)	-687.9** (068.0)	0.000 (0.004)	0.057 (0.041)	-10431.9** (1657.8)
R-squared	0.16	0.10	0.13	0.14	0.23
No. of observations	2,535,573	2,535,573	2,535,573	2,535,573	2,535,573

Notes: Robust standard errors clustered on households in parentheses. ***, **, and * denote significance at 1, 5 and 10 percent level respectively. The estimated models include city district fixed effects, time effects, individual characteristics (gender, education level, immigration year, region of birth, children and age), time varying parameters on covariates and district specific time trends.

8.2 Age

Next we examine whether mandatory activation has significantly different effects on young people (aged 18–25). The results are presented in Table 12. While there are no statistically significant differences between the 18–25 years olds and those over 25 years of age when it comes to welfare benefits, there are significant differences between the two groups when it comes to the other outcomes (and the differences are huge when it comes to income from work). Starting with the effects on employment, it seems like mandatory activation has no effects on people aged 26 or older, while it has a positive and significant effect for the younger ones. The increase in the probability of being employed in November for the younger group is 0.9 percentage points, which corresponds to a 1.6 percent increase, and the increase in the number of months employed is 0.14, which corresponds to a 2.2 percent increase. Turning to the income variable, we note that while mandatory activation has a significantly negative effect of

1,813 SEK on income from work for the older age group, it has a significantly positive effect of 19,223 SEK (-1,813+21,036) for the younger age group, which corresponds to an increase with 25 percent. It thus seems like mandatory activation programs work very well for young adults.

Table 12 Heterogeneous effects with respect to age

	Welfare receiver	Welfare benefits	Employed in November	Number of months employed	Income from work
DD-estimate	-0.006***	-81.3*	0.002	0.0199	-1,812.9**
	(0.001)	(35.2)	(0.001)	(0.0161)	(430.0)
DD-estimate	0.011***	12.4	0.009*	0.143***	21,035.9**
*Young (18–25)	(0.002)	(69.8)	(0.004)	(0.0435)	(1,065.8)
R-squared	0.15	0.10	0.13	0.14	0.23
No. of obs.	2,535,573	2,535,573	2,535,573	2,535,573	2,535,573

Notes: Robust standard errors clustered on households in parentheses. ***, **, and * denote significance at 1, 5 and 10 percent level respectively. The estimated models include city district fixed effects, time effects, individual characteristics (gender, education level, immigration year, region of birth, children and age), time varying parameters on covariates and district specific time trends.

8.3 Country of birth

Finally we examine whether mandatory activation has significantly different effects on individuals that are born in outside Sweden. We have separate indicators for whether the individual is born in a Nordic country, in a Western country (apart from the Nordic ones), in an East European country or in some other country (i.e., from Africa, Asia or Latin America). The DD-estimate in Table 13 then captures the effect on native Swedes. It is interesting to note that mandatory activation does not seem to have any significant effects on native Swedes. It seems like mandatory activation works best for the group with highest welfare participation; there is a significant and negative effect on welfare benefits for those born in Africa, Asia and Latin America. The welfare benefits received by this group decreases by 527 SEK, which amounts to 7.3 percent of mean value, while income from work increases by 8,142 SEK (9.7 percent). For those born in a Nordic country on the other hand, mandatory activation seems to be harmful in the sense that they increase welfare benefits, but on the other hand they also increase income from work substantially.

Table 13 Heterogeneous effects with respect to country of birth

	Welfare receiver	Welfare benefits	Employed in November	Number of months employed	Income from work
DD-estimate	-0.002*	-1.8	0.002	0.0203	-966.4
	0.001)	(38.9)	(0.002)	(0.0189)	(526.6)
DD-estimate*Nordic	0.005	442.7*	0.002	0.0116	10,624.9**
	(0.004)	(189.5)	(0.008)	(0.0913)	(2652.3)
DD-estimate*	-0.005	19.1	-0.020	-0.237*	-3,367.0
Western country	(0.004)	(171.8)	(0.011)	(0.129)	(3,552.6)
DD-estimate*	-0.001	-106.6	0.017*	0.145	10,331.1**
East European	(0.005)	(226.5)	(0.008)	(0.0965)	(2,534.4)
DD-estimate*	-0.014***	-527.2**	0.008	0.115**	8,142.1**
Other country	(0.003)	(128.2)	(0.004)	(0.0495)	(1,297.1)
R-squared	0.15	0.13	0.13	0.14	0.23
No. of observations	2,535,573	2,535,573	2,535,573	2,535,573	2,535,573

Notes: Robust standard errors clustered on households in parentheses. ***, **, and * denote significance at 1, 5 and 10 percent level respectively. The estimated models include city district fixed effects, time effects, individual characteristics (gender, education level, immigration year, region of birth, children and age), time varying parameters on covariates and district specific time trends.

9 Conclusions

In this paper we examine whether the introduction of mandatory activation programs have any effects on welfare participation, employment, and disposable income. The theoretical prediction from the Besley and Coate (1992) model is that mandatory activation decreases welfare participation and, implicitly, increase employment. As far as we know, this is the first time that a clear empirical test of the hypothesis that this type of program imply fewer people on welfare has been carried out taking both entry and exit effects into account.

In order to identify causal effects, we make use of a variation in the data that was generated by the gradual implementation of mandatory activation in the city districts in the municipality of Stockholm. The data is very suitable for examining this question several reasons. First, the reform was clean in the sense that no other instruments, like time limits or tax credits, were introduced at the same time, implying that we are able to estimate the direct effects of the programs. Second, the reform was initiated at different points in time in different city districts, which make identification easier. Finally, using data from city districts within a single local labor market we can control for common macro economic shocks

On average, we find a positive effect on employment (the probability that an individual is employed increases with the introduction of the programs). Also, our results indicate that the introduction of mandatory activation programs decrease welfare participation; the introduction of mandatory activation leads to a 0.4 percentage point reduction in the probability of being a welfare participant (an effect that constitutes approximately 5 percent of the average welfare participation rate in the sample). The result support the prediction from the Besley and Coate (1992) model.

We also find that activation requirements seem to work best for young people and for people born in a non-western country. These results are of particular interest given the scarring effects of youth unemployment found in Skans (2004). Hence, it seems like the programs work best for the most welfare prone groups.

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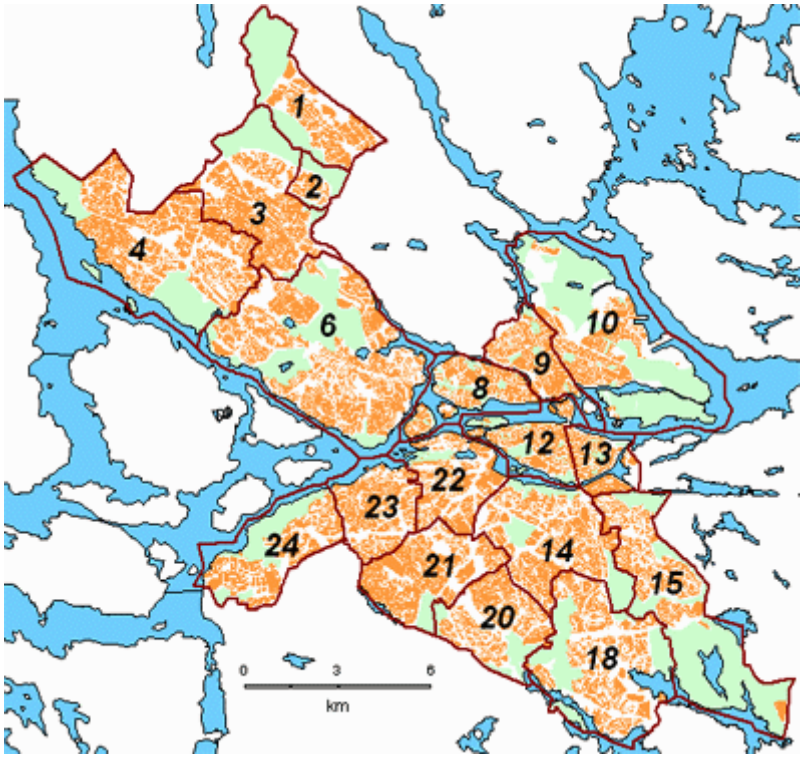
Appendix A: Map – city districts of Stockholm.



1. Kista
2. Rinkeby
3. Spånga-Tensta
4. Hässelby-Vällingby
6. Bromma
8. Kungsholmen
9. Norrmalm
10. Östermalm
12. Maria-Gamla stan
13. Katarina-Sofia
14. Enskede-Årsta
15. Skarpnäck
18. Farsta
20. Vantör
21. Älvsjö
22. Liljeholmen
23. Hägersten
24. Skärholmen

Appendix B: Survey to the social service unit of the city districts of Stockholm

(Note that the original version is in Swedish, and that this is a translated version.)

The survey refers to information on activities for unemployed individuals, capable of working, that receive welfare benefits.

1. Does your city district currently have any activation/labor market related programs for unemployed individuals, capable of working, that receive welfare benefits?
Yes
No

If no, turn to question 9 of the survey.

If yes, please name the program/programs:

2. From which year do(es) this program/these programs exist(s) in its current form (under the same or a different name)?

3. Do(es) the program/s encompass all individuals, capable of working, that are unemployed and receive welfare benefits?
Yes
No

4. If you have responded "No" to question 3:
- How large share of all individuals, capable of working, that are unemployed and receive welfare benefit are encompassed by the program?
- Which groups of individuals are targeted by the program?

5. Please, specify how and to which extent the following activities are being used in the program/programs:

a. Job-seeking activities

b. Job training activities

c. Other assigned work (for example within the municipal services)

d. Other activities – please specify which:

6. What is the minimum number of hours of weekly attendance that is required in the program/programs?

7. Is absence/non-attendance systematically reported to the social service officials?

Yes

No

Comments:

8. Can absence/non-attendance (without acceptable motives) lead to rejection of the welfare benefit application?

Yes

No

Comments:

In the following part of the survey we ask for information on programs that were targeted to unemployed individuals, capable of working, that receive welfare benefits, before the current program/programs started.

9. Which programs have been in place under the period from 1990 until the start of the current program/programs? Under each number below, please specify the name of the program, or the main activity if you do not know/there was no name for the program (for example "Meeting with job counselor"). Please also specify during which years the program/activity was in place.

Program 1:
Name: _____
Time period:_____

Program 2:
Name: _____
Time period: _____

[..etc..]

Below follows a set of questions about the programs/activities that were in place before the current program/-s. Please, answer the questions about each program under the number that corresponds to the list above.

Program/Activity 1:

1. Which groups were targeted by the program/activity?

2. How large a share of all individuals, capable of working and receiving welfare benefits, were encompassed by the program/activity?

3. Please, specify to which extent the following activities were used in the program/activity:

a. Job-seeking activities

b. Job-training activities

c. Other assigned work (for example within the municipal services)

d. Other activities. Please specify which:

7. Was absence/non-attendance systematically reported to the social service officials?
Yes
No

If yes, in which way:

8. Could absence/non-attendance (without acceptable motives) lead to refusal/rejection on the welfare benefit application?
Yes
No

Comments:

Program/ Activity 2:

[The same questions were repeated for all programs/activities listed]

Appendix C: Register data

The data used in this paper come from three databases (all of them part of the IFAU-database): LOUISE, syss and anst.

- LOUISE: A longitudinal database containing information on education, income and employment for the whole population older than 16 in Sweden. It covers the data for the years 1990 and onwards.
- Syss: Syss is part of RAMS (registered labor market statistics) and contains data on employer, income from employment and employment from 1985 to 2000. For later years, see LOUISE.
- Anst: anst is part of RAMS (registered labor market statistics) and contains information about when the employee started the employment and when the employment was terminated.

Table C.1. Definition of variables

Variable	Database and name	Description
Dependent variables		
Welfare receiver	LOUISE: socbidp1*	Indicator variable which takes value 1 if socbidp1>0.
Welfare benefits	LOUISE: socbidp1	The individual's share of the household's welfare benefits. Includes zeros.
Employed in November	sys: syss*	Indicator variable which takes the value 1 if an individual is employed for at least 1 hour in November.
Employed all year	anst: mantill & manfran	The variable takes the value 1 if an individual has been employed a full year in a position which has generated more than 25 percent of the minimum wage for a worker within the Hotel and restaurant sector.
Months employed	anst: mantill & manfran	The number of months an individual has been employed during the year in a position which has generated more than 25 percent of the minimum wage for a worker within the Hotel and restaurant sector.
Income from employment	LOUISE: loneink	The sum of gross earnings from an employer during the year.
Disposable income	LOUISE: dispink	All income from work and social security systems, transfers minus taxes, study loan payments etc. For details, see SCB (2005, p. 190).
Variables used for heterogeneous effects		
Two parent households with Young children (<7 years)	LOUISE: barn0003 & barn0406, famstf	Indicator variable which takes the value 1 if a household is headed by two adults and has children less than 7 years in the household.
Single-parent household with young children	LOUISE: barn0003 & barn0406, famstf	Indicator variable which takes the value 1 if a household is headed by one adult and has children less than 7 years in the household.
18–25	LOUISE: fodar*	Indicator variable which takes the value 1 if an individual is within the age interval 18–25.
Born in Sweden	sys: fland	Indicator variable for Sweden as country of birth.

Variable	Database and name	Description
Born in Nordic country	sys: fland	Indicator variable for any of the Nordic countries as country of birth.
Born in Western country	sys: fland	Indicator variable for any of the Western countries as country of birth (Western Europe, U.S. and Canada).
Born in Eastern Europe	sys: fland	Indicator variable for any of the Eastern European countries as country of birth.
Born in other country	sys: fland	Indicator variable for any other country of birth.
Other control variables		
Woman	LOUISE: kon	Indicator variable which takes value 1 if an individual is a woman.
Households with young children (<7 years)	LOUISE: barn0003 & barn0406	Indicator variable for the presence of children less than 7 years in the household.
26–35	LOUISE: fodar*	Indicator variable which takes the value 1 if an individual is within the age interval 26–35
36–45	LOUISE: fodar*	Indicator variable which takes the value 1 if an individual is within the age interval 36–45
46–64	LOUISE: fodar*	Indicator variable which takes the value 1 if an individual is within the age interval 45–64
Children=1	LOUISE: barn0003, barn0406, barn0715, barn1617*	Indicator variable for the presence of one child under 18 years in the household.
Children>1	LOUISE: barn0003, barn0406, barn0715, barn1617*	Indicator variable for the presence of more than one child under 18 years in the household.
Elementary school< 9 years	LOUISE: hsun*	Indicator variable which takes the value 1 if the individual's highest education is elementary school< 9 years.
Elementary school 9 years	LOUISE: hsun*	Indicator variable which takes the value 1 if the individual's highest education is elementary school 9 years

Notes: * Variable/s used to generate the variable used.

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