Dynamic effects of mandatory activation of welfare participants*

Anna Persson† and Ulrika Vikman‡

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Abstract

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

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

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†Department of Economics, Uppsala University and Uppsala Center for Labor Studies (UCLS); Anna.Persson@nek.uu.se

‡Institute for Labor Market Policy Evaluation (IFAU), Department of Economics, Uppsala University, UCLS, and Uppsala Center for Fiscal Studies (UCFS); Ulrika.Vikman@ifau.uu.se

1 Introduction

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

In this paper we study the effect of such work requirements on the flows into and out of welfare participation in a Swedish municipality. The identifying variation that we use arises due to a sequential implementation of activation programs in different city districts of Stockholm. This reform has been shown to have a negative effect on the overall caseload, and a positive effect on employment (Dahlberg, Johansson, and Mörk 2008). In this study we decompose these previous results into effects on entry and exit rates. The importance of performing this decomposition is established by Grogger, Haider, and Klerman (2003), who show that a reduction in welfare entry accounted for around half of the decline in US welfare caseloads during the 1990s, while increased exit rates explained the other half. Also,

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

Grogger (2004) shows that entry and exit are not symmetrically affected by the economy and welfare reform. Thus, by not including effects on both flows in the analysis, a lot of information will be lost. However, studying the full dynamics of welfare participation requires more data than what is commonly available. Most previous literature thus focus on welfare exits, since one then only needs data on welfare participants or welfare leavers. The studies that do analyze welfare entry find ambiguous results (Klawitter, Plotnick, and Edwards (2000), Gittleman (2001) and, Acs, Phillips, and Nelson (2005)). A priori, the effect on activation requirements on welfare entry are ambiguous. As discussed by Moffitt (1996) the effect will depend on weather the activity is viewed as a burden or something that might favour future employment probabilities. Also, the program might affect welfare stigma and thus the implicit social cost of welfare participation.

There are many variations of activation programs, and participating in activation may imply very different things. In a strong version known as "workfare" the welfare recipient is required to work in a publicly provided job to retain assistance. Weaker versions may merely mandate participation in a job preparation or job search program. There are also optional activation programs in which noncompliance does not lead to sanctions. Moreover, programs differ in how much focus they put on increasing human captial by providing relevant skills relative to testing the participants willingness to work. In most theoretical work on activation requirements for welfare recipients it is assumed that the activation does not improve human capital, they only change individuals' incentives[2]. Besley and Coate

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

(1992) show that the incentive effects of mandatory activation are twofold. In the short run, it will induce individuals to refrain from applying for welfare or to exit welfare faster because there is an implicit cost associated with welfare use. In the longer run people might make choices that reduce the risk of becoming welfare dependent in the future, for example by completing more education, when welfare becomes a less attractive alternative. Hence, mandatory activation programs affect both welfare participants and non-participants through exit and entry effects, respectively.

Our study makes valuable contributions to the existing literature in several ways. First, while welfare reform in the US often implied the implementation of a bundle of reforms with a combination of work requirements, time limits and financial incentives such as the EITC, reforms in Sweden have been restricted to activation. By looking at Swedish data we can thus more credibly isolate the effect of activation requirements from that of other interventions. Second, since we have access to data for the whole population and are not restricted to labor force or welfare participants, we are able to capture the full effect on the probability of non-participants to enter welfare. Third, the feature that all individuals permanently residing in Sweden are potentially eligible to receive welfare benefits, whereas in the US support is primarily aimed at single mothers, makes it possible to look at heterogeneous treatment effects across different demographic groups. And fourth, there is also additional advantages of looking only at the city districts of Stockholm, namely that the districts have the same political representation and, most importantly, belong to the same labor market region. It is thus possible to control for (unobserved)

common macroeconomic shocks.

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

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

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

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

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

Also, it seems that effects are larger for groups that can be assumed to be more mobile and have fewer family responsibilities.

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

2 Previous literature

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

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

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

Moffitt (2003) analyzes effects on both entry and exit rates of nonfinancial factors, where work requirements is one factor. He uses survey data from only post reform years in three American cities where single-mothers both on and off welfare were asked questions. Recipients where asked questions about work and other requirements and sanctions. To capture effects on entry rates, questions to TANF applicants about different diversion programs are used. One diversion program require the applicant to work or demonstrate job search activity prior to application. Moffitt finds that work requirements increase exit rates, but no effect is found on applicants entry rates. The diversion practices gives mixed results for effects on

entry rates, possibly due to selectivity on unobservables. Since the survey only captures TANF applicants the study may not capture the whole effect since some single-mothers may choose to never apply due to the work requirements.

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

3 Institutional setting and data

3.1 Social assistance in Sweden

Sweden is divided into 290 municipalities, which are responsible for the majority of the publicly provided welfare services, such as childcare, education and elder care. The local governments have historically also been responsible for relief for the poor, whereas labor market policies have been

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

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 Swedish recession and financial crisis in the 1990s, the social assistance caseload grew, and many municipalities faced difficulties in financing the social assistance system. As shown in Figure 1, both the cost of welfare benefits and the number of households receiving welfare increased until the mid-1990s, but they have since decreased. However, the cost of benefits per household has increased substantially. In 1983, the average benefit received among those on social assistance was around 9,000 SEK (1,125 USD)[7] year and household. In 2008, this figure was

[7]Between 1983 and 2008 the exchange rate varies between 9 USD per 100 SEK and

almost 44,000 SEK (5,500 USD). This implies that individuals who were on welfare in 2008 received benefits for more months during a year and/or larger amounts of benefits than was the case in 1983.

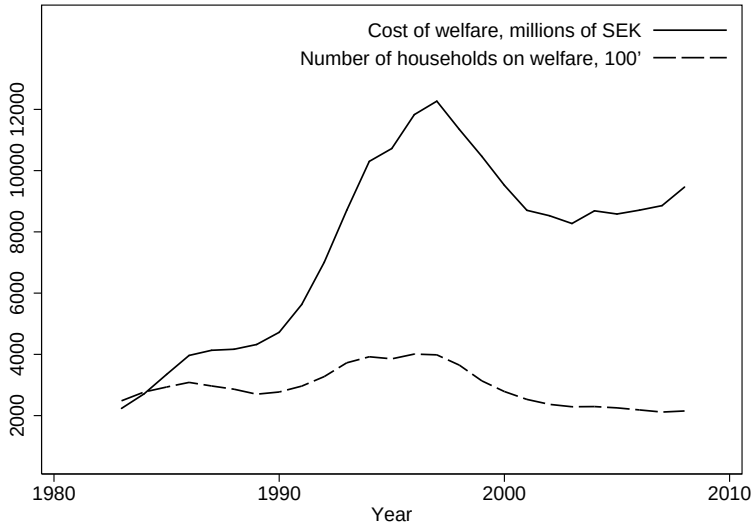

Figure 1: Cost of welfare (millions of SEK) and number of welfare households (100's) 1983-2008. Source: Statistics Sweden.

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

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

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

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

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

For around three quarters of the social assistance recipients in Stockholm in 2005, unemployment is the reason for needing social assistance. A large fraction of these, 77 percent, do not meet the eligibility criteria for un-

Table 1: City district characteristics 1993

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

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

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

[c] Average welfare benefits among welfare recipients.

employment insurance; that is, they do not have labor market experience and/or are not members of an unemployment benefit fund. However, they are registered at the employment office and are looking for and willing to accept a job (USK 2007). These are the individuals targeted by the reforms that we study.

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

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

Table 1 shows the launching year for activation requirements in each city district. It is important to note that when applying for social assistance the individual must contact the office of the district in which he or she lives (or is registered), it is not possible to choose which district to apply within and thereby avoid the programs or take part of activities in other districts.

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

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

ployed during the summer. In 1999, the program was widened to include all unemployed welfare participants. The main feature of the program is that unemployed welfare applicants are sent to the Jobcentre. In order to retain eligibility for welfare, the applicant must visit the Jobcentre for three hours every day, following a rotating schedule to prevent black market work, until he or she finds a job. The required activity consists mostly of individual job searching. The Jobcentre provides computers with internet access and assistance from staff when necessary. As noted by Thorén (2005), the resources are often limited; for example, clients can rarely use the computers for more than 15 minutes each day. There is daily registration of participants' attendance, and because there is close cooperation between social workers and Jobcentre staff, absence is easily detected and can (and often does) lead to a reduction in benefits. This possibility of imposing sanctions is common to programs in all city districts. Activation starts when the individual apply for benefits, that is when an unemployed individual applies for social assistance he or she is sent to the Jobcentre immediately. The main goal of the activation programs is to improve individuals' chances of becoming self-supportive. However, Thorén (2005) concludes that many of the activities primarily aim at testing the client's willingness to work.

The information about the starting year of activation programs is combined with individual-level register data from the LOUISE database administered by Statistics Sweden. This database includes information on various individual characteristics such as age, country of birth, number of children, education, etcetera for all individuals aged 16-64 living in Swe-

den[10]. This means that we have data for the whole population, regardless of labor market attachment and welfare participation. The data also contains the share of the household's social assistance[11] that the individual has received during the past year as well as benefits collected from other parts of the social security system. Social assistance is directed at households rather than individuals, and we define an individual as a welfare participant if he or she is living in a household that received social assistance sometime during a given year. This is a very rough but commonly used classification. What we refer to as social assistance is thus the individual's share of the household's total received benefit. All unemployed individuals living in a household receiving social assistance are directed to the jobcenter to fulfil activation requirements. Since all newly arrived immigrants are eligible for social assistance during their first 18 months in Sweden (introduktionsbidrag) under different eligibility criteria than other welfare participants, these individuals are excluded for three years to avoid capturing their dynamics due to this sort of support. Table 2 shows descriptive statistics for the population. The mean amount of welfare benefit received by an individual is slightly above 2,000 SEK (250 USD) per year. However, it should be noted that all zeros are included here and that the mean amount of benefits among those who actually receive any benefits at all is around 23,600 SEK (2,950 USD) per year.

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

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

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

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.

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

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

It is important to note that both the entry and the exit populations

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

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

A strength of our econometric analysis is that individuals in our data are part of the same labor market region and therefore meet the same economic conditions, but live in areas where mandatory activation was

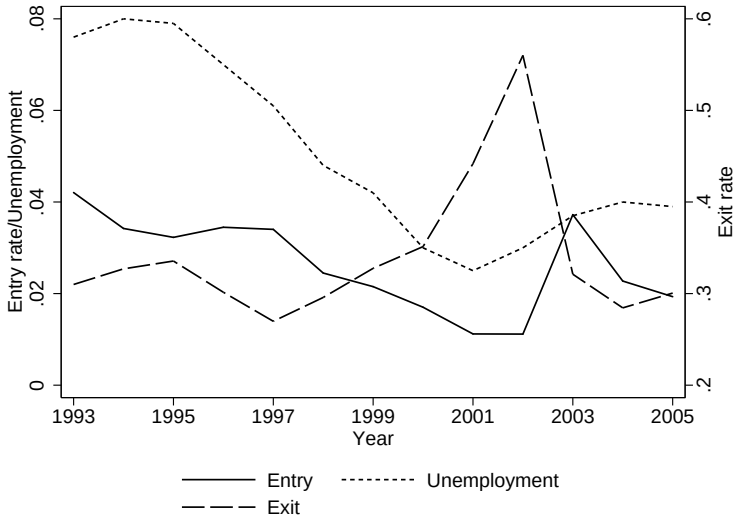

Figure 2: Unemployment rate, raw entry and exit rates, by year in Stockholm

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 few years of education. Because these groups have a higher probability of receiving benefits than others, we attempt to create a better-defined entry sample by estimating effects on entry rates using only a subpopulation consisting of individuals with any of these characteristics. Thus, we reduce the problem of estimating an effect for individuals that

have close to zero probability of ever participating in welfare (for example, individuals with high education and income are unlikely to change their behaviour in response to a reform that will probably never affect them). We prefer to define the population at risk of entering into welfare using demographic characteristics rather than income. It is likely that individuals with low income are more likely to receive welfare benefits than others. However, Meyer (2000) argues that restricting the sample to include only low-income individuals might create bias because poverty is likely to be higher in an area with low benefit levels and vice versa, which might affect welfare participation as well as entry and exit.

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

and higher exit rates, which indicates mobility. Immigrants, especially those born in a non-Western country, have high entry rates and low exit rates. The high entry rates are in line with Hansen and Lofstrom (2006). The same pattern observed for immigrants can be observed among single parents.

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

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

4 Empirical strategy

To determine the treatment effect on the treated (TT) when mandatory activation is introduced, we use a difference-in-differences (DD) approach in a linear probability model (LPM). When estimating the effects on entry and exit rates, there will be different events of interest. In the entry case, the population used 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 control group if no treatment had occurred. Thus, implementation of activation requirements cannot be related to (unobserved) city district-specific conditions. As mentioned earlier, this assumption can be questionable because the composition of the samples is affected by the reform if individuals leave welfare and fewer individuals enter welfare due to the reform.

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

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

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, $\mathbf{X_{ijt}}$ is included[12]. All individual covariates are time-interacted (giving γ_t) to allow these individual characteristics to influence the probabilities differently over the business cycle. $trend_j$ are linear city district-specific time trends, and η_{ijt} is an error term.

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

4.1 Standard error corrections

A problem with difference-in-differences when the treatment is at the group level instead of at the individual level is that if observations are not independent within groups and we are not able to control for common group/time errors the estimated standard errors are biased downward[13]. This issue is normally solved by clustering standard errors on the level of randomization. In this case, however, clustering standard errors is likely to introduce another source of bias due to too few groups. Instead, a consistent estimator can be found using the two stage procedure proposed by

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

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

Donald and Lang (2007). They argue that it is possible to correct for group and time specific shocks by estimating group averages, while still using the information in the microcovariates. However, if there is no correlation in standard errors within clusters, this approach reduces the amount of available information more than necessary. We will therefore use a method proposed by Wooldridge (2003) to test for if these correlations exists. He proposes a two-stage procedure where an efficient minimum distance (MD) estimator is obtained in the first step by estimating

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

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

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

with weighted least squares where the weights are given by $1/\hat{\sigma}_{jt}$. Under the null of no unobserved city district specific shocks we have that (in the second stage estimation) $SSR \overset{a}{\sim} \chi^2(S-K)$ where S is equal to $J \times T$ and K is equal to the number of parameters estimated in equation 4. If the null hypothesis is rejected city district specific shocks exists and we will use the Donald and Lang (2007) procedure (hereafter D-L procedure). In practise, this is equivalent to estimating 3 and 4 but using the group size, that is the share of the total sample population living in each specific city

district every year, as weights in equation 4 instead of the variance of $\hat{q}_{jt}$ from equation (3). This between estimator gives the correct standard errors and t-statistics, and thus provides a valid inference. We will show the test statistic from the Wooldrige test together with p-values in all our result tables and present the standard errors from the D-L procedure if the test statistic is rejected at the five percent level.

5 Results

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

5.1 Effects on caseloads

According to Dahlberg, Johansson, and Mörk (2008), the caseload (share of welfare recipients) was reduced by 0.5 percentage points in Stockholm due to mandatory activation requirements. However, their study uses a different sample as they do not include Rinkeby and use data only up to the year 2003. Therefore, for comparison of our main entry and exit results, we run estimations of caseloads with our complete sample and for different subpopulations using equation (2). The caseload results are shown in table 4, where we include both ordinary standard errors and the

standard errors from the D-L procedure if the Wooldrige test is rejected. The test statistics from the Wooldridge tests are also shown in the table together with number of degrees of freedom and p-values.

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)***
	[0.003]			
Time varying controls	Yes	Yes	Yes	Yes
Linear trend	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
City district dummies	Yes	Yes	Yes	Yes
Wooldridge-test; SSR(df)[p-value]	194.871	0.252	20.726	42.881
	(107) [0.000]	(107) [1.000]	(107) [1.000]	(107) [1.000]
N	2,986,175	372,325	372,917	1,395,995

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

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

Surprisingly, we find a significant increase in caseload due to the reform for immigrants from non-Western countries, whereas Dahlberg, Johansson, and Mörk (2008) found large negative effects. There are four differences between our sample and theirs. We include Rinkeby, have two additional years of data and define immigrants from non-Western countries in a slightly different way - they do not include immigrants from Eastern Eu-

rope 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.

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

5.2 Baseline estimation

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

The estimates for the effect on entry shows a reduction by 0.1 percentage points. In the Wooldrige test we reject the null of no city district specific shocks and therefore also report the standard errors from the D-L procedure where the result become insignificant. We conclude that we are not able to identify any effects on the entry rates for the whole population

Table 5: Estimation results: Entry

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

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

when mandatory activation is implemented. The reform may, however, still have had an effect at different times after implementation and for different subpopulations, especially for populations at greater risk of entering welfare (see section 5.4 and section 5.5).

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

5.3 Placebo estimations

In order to verify that the estimates above captures true reform effects, and does not arise due to endogenous factors, we perform a placebo experiment using data from 1993 to 2000. For the years 1998, 1999 and 2000, we exclude Rinkeby, and for 1999 and 2000, we also exclude Skärholmen. Thus, we only use data from before the reform was implemented in any of the city

districts. We move the launching year of the actual reform five years back in time[14]. If the estimation of this "pseudo"-reform were to yield significant results, it would indicate the possibility that the estimates above do not represent an effect of the reform but rather of some city district-specific characteristic. The results from these estimations are shown in Table 6.

Table 6: Results from placebo estimations: Entry

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

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

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

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

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

5.4 Time-changing treatment effects

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

Table 7: Results from estimations with time-specific treatment

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

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

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

people to find work.

5.5 Heterogeneous effects

5.5.1 Population at risk

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

Table 8: Results for population at risk: Entry

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

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

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

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. Since Dahlberg, Johansson, and Mörk (2008) find large effects 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 9. The effect on the probability of entry is reduced by 0.6 percentage points. This is a rather large effect as the mean entry rate for this group during the studied period was about 5 percent (see Table 3). For young individuals, the estimate for the exit effect is a little bit higher than for the whole population on average, 1.4 percentage points. A possible interpretation is that when facing activation requirement, ordinary education might become a relatively more attractive alternative and since the possibilities of starting an education is larger for younger individuals this would translate into a larger reduction in entry rates for this group. Also, young individuals might be more likely to move back to live with their parents to avoid the activation programs.

The results for immigrants born in a non-Western country are presented in Table 10. Since the caseload effect for this group in our sample is positive we would expect positive entry effect and or negative exit effect. We find a positive entry effect but this is not significantly different from zero. Since the activation that immigrants participate in is likely to consist mainly

Table 9: Estimation results:Age< 26

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

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

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

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

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

of language training, and thus differ from that offered to other welfare participants, it might not be surprising that the results are not the ones that we would normally expect.

We also present results from separate estimations for unmarried individuals without children as this group could be expected to be relatively mobile and is commonly not eligible for welfare in other countries. As seen in Table 11, mandatory activation policies do not affect the entry rate for this group but lead to a significant increase in exit rate (2 percentage points, compared to an average exit rate of 35 percent for this group). An explanation to this may be that an individual in this group might have lower barriers to employment, since he or she does not have to take the situation of a partner or child into account when accepting a job offer.

Table 11: Estimation results: Unmarried without children

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

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

6 Conclusions

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

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

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

Our results indicate that entry rates decrease as a result of mandatory

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

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

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

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

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

The main conclusion to be drawn from this study is that mandatory activation programs seem to have a rather small effect on the probability that an individual leaves and enters welfare participation. However, there are important differences between groups of individuals. Most importantly, young individuals and single individuals with no children are affected more than other groups. This is probably due to the fact that these groups are more mobile and are more likely to be able to accept a job offer on short notice. Young individuals, who become less likely to start collecting benefits when participation in the program becomes mandatory, are

more likely to start pursuing higher education and thus qualify for study grants. For future research it would be interesting to see if it is the case that the activation programs led to more individuals starting higher education rather than relying on welfare. It is also not surprising that individuals with fewer family responsibilities are more responsive to the incentives that the programs create. This is especially true if leaving welfare requires taking short term jobs and if child care is not easily available. When interpreting these results, it is important to consider that the design of the activation programs probably has a large impact on their effectiveness. For example, activation aimed at young individuals is different from that aimed at immigrants with poor language skills. The programs are thus very likely to affect different groups differently, both in terms of how effective the programs are in providing relevant skills and in what incentives they create.

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