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How to stimulate single mothers on welfare to find a job: evidence from a policy experiment

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Abstract We present the results from a policy experiment in which single mothers on welfare were stimulated to enter the labor market and increase their work experience. The aim of the policy was not per se for single mothers to leave welfare completely but to encourage them to find a job if only a part-time job. Two policy instruments were introduced: an earnings disregard and job creation. The experiment was performed at the municipality level in the Netherlands, a country with relatively high benefits and low incentives for single mothers to leave welfare for work. In our analysis, we make a distinction between native and immigrant welfare recipients. For immigrant single mothers, we find a positive employment effect of an earnings disregard. Job creation in addition to the earnings disregard increased working hours for

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some groups of single mothers. Although the outflow from welfare was not affected, welfare expenditures were reduced.

Keywords Welfare · Single mothers · Policy experiment · Earnings disregard · Job creation

JEL Classification C41 · I38 · J64

1 Introduction

In the past decades, political and public attitudes toward welfare recipients have changed. Welfare recipients including single mothers with young children are encouraged to find a job. It is, however, particularly difficult to increase the labor force participation of single mothers as they have to combine work and childcare and often for them the financial incentives to go to work are not very strong.¹ Usually, benefits are reduced one-for-one when a welfare recipient starts earning. Making work pay policies have been introduced to strengthen financial incentives to employment, through in-work benefits, earnings disregards or reemployment bonuses. In-work benefits are provided conditional on the recipient having a job. Earnings disregards imply that welfare benefits are withdrawn less than one-for-one with earnings. Reemployment bonuses are provided to unemployed individuals who find a job.²

In-work benefits are popular. In 2007, half of the OECD countries had some form of in-work benefit policy (Immervoll and Pearson 2009). In the USA, the Earned Income Tax Credit (EITC) is a financial incentive program to encourage work among low-income families with children by providing refundable tax credits or earnings subsidies. Several studies find that the increased generosity of the EITC as from 1987 had large effects on employment rates (Eissa and Liebman 1996; Ellwood 2000; Meyer and Rosenbaum 2001; Hotz et al. 2001).³ Financial incentives, however, worked almost exclusively through the participation margin, while single mothers were rather unresponsive on the intensive margin (Meyer 2002). Not all previous US studies find positive effects of financial incentives on single mothers labor supply. Following a welfare reform in 1996, Matsudaira and Blank (2014) analyze the impact of changes in earnings disregards (in addition to the EITC, that operates through the tax system). They exploit state differences in the level of the earnings disregards and

¹ A large empirical literature has studied labor supply responses to taxes, transfers and active labor market policies. See, for example, Blundell and MaCurdy (1999) and Heckman et al. (1999) for an overview.

² Alternatively, programs of direct job creation and wage subsidies may have a positive effect on employment at the lower end of the labor market. However, participation in subsidized jobs may stigmatize and reduce chances of finding a regular job (Boeri 1997). Martin and Grubb (2001) provide an overview of evaluation findings in OECD countries and conclude that subsidized jobs do not help the unemployed to get permanent jobs. Kluve (2010) presents a meta-analysis of 137 ALMP evaluation studies in Europe. He finds that direct employment programs in the public sector are rarely effective and frequently detrimental for the employment prospects of participants.

³ See Ingles (2001) for a survey of US findings.

find that the changes had little effect on labor supply or income because only few women used these earnings disregards.

An important ‘making work pay’ policy in the UK is the Working Families Tax Credit (WFTC). This program, which was introduced in 1999, provides support for low wage working families. An important difference with the EITC is that the WFTC has a minimum weekly hours eligibility condition.⁴ At the introduction of the less generous predecessor of the WFTC (the Family Credit), the minimum hours cut-off was set at 24 h. In 1992, this was reduced to 16 h, and this encouraged a significant fraction of inactive single mothers into work (Blundell and Hoynes 2004). At the introduction of the WFTC in 1999, financial incentives increased, particularly for single mothers with young children. Among others, Brewer et al. (2007) find that the WFTC induced single mothers to increase their participation in the labor market by 5 % points.

For the Netherlands, Bettendorf et al. (2014) studied the extension of an earned income tax credit for mothers with their youngest child between 12 and 15 years. They find a negligible effect on the labor force participation and argue that this is probably because they use as the control group single mothers with children younger than 12 or older than 15 years of age. When they use as control group single women without children as is done in most related studies, they also find a positive effect of the financial incentive. In the case of Bettendorf et al. (2014), single women without children are an invalid control group, which is in line with the critique of Blundell and Macurdy (1999) and Meghir and Philips (2010). In our paper, we will therefore also use single mothers as a control group, instead of single women without children.

Financial incentives were also investigated in Canada and Australia. In Canada, the self-sufficiency project experimented with a generous time-limited earnings supplement available to single parents who had been on welfare for at least a year, and who subsequently left welfare and found full-time work (defined as at least 30 h a week). Results show positive effects in the short run (Michalopoulos et al. 2005), but temporary wage subsidies did not have a long run effect on wages and welfare participation (Card and Hyslop 2005). In Australia, policy shifts implemented in 1987, 2004, and 2006–2007 increased employment of single mothers (Doiron 2004; Gong and Breunig 2014). Working hours were not always increased.

Reemployment bonuses create direct financial incentives to find and accept a job. On the basis of outcomes from four US cash bonus experiments and six job search experiments, Meyer (1995) concludes that economic incentives affect the speed by which unemployed workers leave the unemployment insurance rolls. Nevertheless, empirical studies on reemployment bonuses usually show modest effects on the behavior of the unemployed workers who could benefit from such bonuses (see Woodbury and Spiegelman 1987; Anderson 1992; Decker and O’Leary 1995; O’Leary et al. 2005; Van der Klaauw and van Ours 2013).

Several US studies report that welfare reforms had a greater effect on black single mother’s employment than on white single mother’s employment (among others, McKernan et al. 2000; O’Neill and Hill 2001; Noonan et al. 2007). This differential

⁴A description and comparison of the EITC and WFTC program can be found in Brewer (2001).

impact is attributed especially to differences in educational attainment among black and white immigrants and to different familial circumstances. Black single mothers are more often never married. This increases their economic vulnerability, which may make them more open to work requirements. However, even after controlling for these differences, Noonan et al. (2007) found that changes in welfare policies and labor market conditions affected black single mothers' employment more than white single mothers' employment.

Our paper investigates policies to increase the labor force participation of single mothers on welfare. We present the results of a policy experiment for single mother welfare recipients in the Netherlands. Previous studies on financial incentives for single mothers' welfare recipients have been focusing on the USA and the UK, which are both countries with relatively low benefits. The Dutch labor market is interesting to study as it is a representative of countries with high benefits and low incentives for single mothers to make a transition from welfare to work. Until 2009, Dutch single mothers on welfare had no financial incentives to work in small part-time jobs. When a single mother started to earn money, her welfare benefits were reduced one for one. Therefore, she was financially tempted to enter the labor market only if she could earn at least the amount of the welfare benefit. However, single mothers on welfare are often low skilled and therefore have a low earnings capacity. A minimum wage single mother can only escape welfare when she works at least about 30 h per week. This is a difficult task in combination with childcare responsibilities and is often not in accordance with the assumptions of these mothers about motherhood. The Dutch government intended to make work financially more attractive, decrease welfare expenditures, and possibly reduce poverty among single mothers. The policy did not aim for single mothers to leave welfare completely, a part-time job would suffice. A policy experiment was set-up to explore the potential of two policy measures; an earnings disregard and direct job creation. As we will describe in more detail below, the earnings disregard was introduced to persuade single mothers to accept part-time jobs, while remaining on welfare. The experiment took place in 2009 and 2010 and was focused on single mothers on welfare with at least one child younger than 12 years. In the experiment, 14 Dutch municipalities implemented the earnings disregard while 6 of them also implemented direct job creation. Our paper evaluates the instruments using a difference-in-difference-in-differences approach in which we simultaneously compare (1) the period before and during the experiment, (2) the municipalities with and without the experimental policy instruments, and (3) single mothers with at least one child younger than 12 years and single mothers with all children 12 years and older. Furthermore, because of the difference in labor market situation, we make a distinction between native and immigrant single mothers.

There have been quite a few papers on welfare and the labor supply of single mothers with young children. We have a threefold contribution to this literature. First, unlike many studies who use single women without children as control group, we have as control group single mothers with older children. Second, we are able to study for the same group of individuals the separate effects of two policy instruments, a financial incentive through the earnings disregard and a job opportunity through job creation. Third, we study the policy measures on welfare recipients in a country with a relatively generous benefit system. Note that the Netherlands is not representative

for other countries when considering the widespread use of part-time work.⁵ When single mothers in the Netherlands have a larger flexibility in choosing the number of working hours, the estimates in this study may provide an upper bound for the potential effects in other countries.

Our empirical analysis proceeds in three steps. First, we investigate the total effect of the policy experiments. Then, we proceed by investigating the distinct effects of the earnings disregard and of job creation on top of the earnings disregard. Finally, as in Blundell et al. (2009), we make a distinction between two types of treated single mothers depending on the age of their youngest child. Since the compulsory education requirement for children starts at the age of 5, we distinguish single mothers with the youngest child in the age category 0–4 years from those with the youngest child 5–11 years. Our main finding is that for immigrant single mothers, there was a positive employment effect of an earnings disregard. Job creation increased working hours for immigrant single mothers with the youngest child between the age of 5 and 11 and for native single mothers. Although the outflow from welfare is not affected, welfare expenditures go down. For native single mothers, we find smaller treatment effects than for immigrant single mothers.

The set-up of our paper is as follows. Section 2 shortly explains the Dutch welfare system and describes the policy experiment. Section 3 describes the data and Section 4 presents some exploratory analysis. The empirical analysis is presented in Section 5. Section 6 concludes.

2 Policy experiment

2.1 Welfare benefits in the Netherlands

In the Netherlands, welfare benefits guarantee a minimum income for all unemployed or part-time employed workers. Welfare benefits are means-tested and a single mother can only qualify if she does not have sufficient earnings, alimony, or other benefits, like unemployment insurance benefits or disability insurance benefits. Welfare recipients have to comply with job search requirements. Furthermore, eligibility for welfare depends on wealth. Single renters may own at most 5.5 thousand euro and homeowners only qualify for welfare when they own not more than 46 thousand euro. Welfare benefit levels are set by the central government in January and July of each year. During the experiment, the welfare benefit level net of taxes was about €900 for single mothers between the age of 21 and 65 and €480 for single mothers younger than 21. In addition, people may receive allowances up to some maximum amounts. Welfare benefits are provided by municipalities.

At the start of the experiment, about 300,000 individuals in the Netherlands received welfare benefits (about 2.7 % of all persons between the age of 15 and 65). Of these, a substantial 26 % were lone parents, predominantly single mothers. About

⁵The average annual hours actually worked per worker in the Netherlands was 1384 in 2009, compared to an OECD average of 1766.

10.6 % of all single parents depend on welfare (Statistics Netherlands 2011). This makes single mothers an important group to investigate.

2.2 Set-up of the experiment

The goal of the experiment was to increase the labor force participation of single mothers on welfare and reduce welfare expenditures. Since for this group it is particularly difficult to leave welfare, the experiment was focused on encouraging part-time work while still receiving welfare benefits. Two policy instruments were introduced, an earnings disregard of €4 for each hour worked, up to €120 net per month and direct job creation in the public and private sector. These instruments were not designed to achieve outflow from welfare in the short run but to increase work experience which might make it easier for single mothers to find a full-time job later on in life. Therefore, a part-time job would suffice.

The first policy instrument, the earnings disregard, encouraged single mothers to participate in paid employment by allowing them to earn income while they were receiving financial assistance. In the experiment, €4 per hour worked were exempted as income when determining the monthly welfare benefits. So, for every hour that a single mother worked, she received €4, up to a maximum of €120 per month.⁶ Without this instrument, welfare benefits would have been reduced by the full amount of the earnings.⁷ Single mothers on welfare who were already participating in the labor market before the start of the experiment were also eligible for the earnings disregard during the treatment period. Therefore, there was no reason to postpone labor force participation and we do not have to worry about anticipation effects. The aim of the earnings disregard was to increase labor force participation if only through a part-time job. By entering the labor market on a part-time basis, the women might find it easier to expand their working hours later on in life.

Figure 1 illustrates how in a labor supply framework, the earnings disregard affects financial incentives in case a single mother could earn the hourly minimum wage of (approximately) €7.50 and receives welfare benefits of €225 per week. In this situation, a minimum wage job of 30 h per week would generate the same income as welfare benefits would. The budget constraint for a welfare recipient is BCA and maximum utility would be derived at point B, where the welfare recipient does not participate in the labor market. If an earnings disregard is introduced, the new budget constraint is BDECA.⁸ The maximum earnings disregard on a weekly basis would be

⁶As a point of reference, in January 2011, the gross legal minimum wage was €1424 per month, for employees as from the age of 23 who worked full-time (36, 38, or 40 hours per week, depending on the sector of employment). This means a net wage of about €1150 and a net hourly wage of about €7.50. The experiment could increase the income of single mothers on welfare by about 13 % (120/900).

⁷Due to decentralization, municipalities may apply different policies to bring people on welfare back to work. An earnings disregard in this form, however, had no legal basis. This was only allowed for the treatment group in the treatment municipalities during the treatment period.

⁸Figure 1 does not take into account the possible costs of formal child care. However, when single mothers on welfare use formal child care to be able to work, they receive a generous subsidy from the tax authority and the municipality.

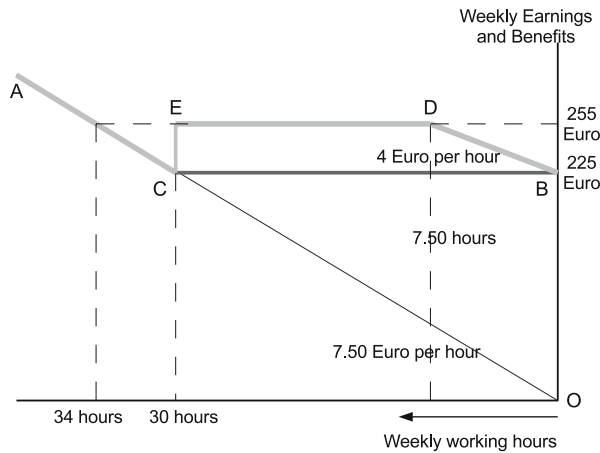


Fig. 1 Budget constraint with and without earnings disregard

after 7.5 h at point D. Figure 1 indicates that a welfare recipient might reach a higher utility in point D or perhaps slightly right of point D. It does not seem very likely that the welfare recipient would find a full-time job more attractive because of the earnings disregard. However, this was not the main aim of the experiment. In the example of a minimum wage earner, there is even a disincentive to work between 30 and 34 h per week because in that case the single mother loses her welfare benefits and her income drops. So, for some single mothers, the outflow from welfare may diminish because of the design of the experiment. Because the earnings disregard makes it more attractive to be on welfare, the experiment may encourage single mothers with young children to enter welfare. In the analysis below, we explore whether such an entitlement effect exists.

The second policy instrument is direct job creation in the public and private sector through a so called ‘labor pool.’ Municipalities act as ‘clearing houses’ for employers and job-seekers and create new jobs for at least 12 h a week for regular employers or for the municipality itself. To hire a single mother with a low earnings capacity, municipalities may provide a subsidy to an employer. Those who obtained a job through the job creation scheme were also eligible for the earnings disregard. When analyzing direct job creation, we analyze an intention to treat, since the results are based on the initial treatment assignment and not on the treatment effect for people who actually took the treatment and participated in the labor pool. The intention-to-treat effect does not suffer from a nonrandom assignment of jobs to single mothers eligible for direct job creation. Also, using an intent-to-treat analysis, we analyze how direct job creation works in a realistic context, which explicitly acknowledges the success or failure that municipalities experience in finding jobs for single mothers on welfare. A disadvantage of the intent-to-treat design is that we may underestimate the potential effectiveness of job creation, for example, in case municipalities do not succeed in finding jobs for single mothers on welfare.

Fourteen Dutch municipalities participated in the experiment (the treatment municipalities).⁹ Municipalities were not forced to participate in the experiment. Instead, municipalities got the opportunity to apply for the experiment and if they participated they had to implement the instruments, for which they received a financial compensation. The advantage of voluntary participation is that the participating municipalities were capable to implement the instruments and deliver data that are needed to evaluate the experiment. A possible drawback of voluntary participation is that a selection bias is introduced if in particular motivated municipalities participate, who already bring single mother welfare recipients back to work quickly. It could also be that participating municipalities have relatively underprivileged single mothers. By using a triple difference approach, we correct for *a priori* differences between treatment and control municipalities. We will see later that observed characteristics of single mothers in the treatment and control municipalities are about the same and that the growth rates of the inflow into welfare are similar in treatment and control municipalities. Furthermore, we find that before the experiment, job finding rates and the outflow from welfare were similar in treatment and control municipalities. Replacing the treatment municipalities by placebo treatment municipalities lead on average to insignificant results.

All of the treatment municipalities applied the earnings disregard to the treatment group, i.e., all single mothers on welfare with a least one child below the age of 12. Six of these 14 municipalities also applied direct job creation. We exploit this variation to identify the effect of direct job creation on top of the earnings disregard.¹⁰ The control group consists of single mothers with children between the age of 12 and 18 while all other Dutch municipalities form the control municipalities.¹¹ In the robustness checks, we will narrow the sample around the age eligibility threshold of 12. The earnings disregard was announced and implemented at the start of the year

⁹These are Almere, Bodegraven, Breda, Echt-Susteren, Enschede, Groningen, Heerenveen, Nijmegen, Reiderland, Schiedam, Scheemda, Vlaarding, Winschoten, and Zwolle.

¹⁰During the period of analysis, also reemployment bonuses were introduced in the treatment municipalities. Single mothers on welfare could receive a reemployment bonus of €500 when they left welfare for at least 6 months because of working enough hours in the labor market to be financially independent. In some municipalities, reemployment bonuses were already implemented before the experiment and part of the municipalities that did not participate in the policy experiment also provided reemployment bonuses. We have no information about which control municipalities provided reemployment bonuses and therefore we cannot identify the effect of the reemployment bonus using the difference-in-differences approach. In general, when reemployment bonuses are part of the municipal policy, they hold for all individuals in welfare, regardless of having a (young) child. Our triple differences analysis takes the effects of these bonuses into account by analyzing differences between the treatment and control group in treatment municipalities and correcting for overall differences between treatment and control municipalities. Therefore, we are not worried that the reemployment bonuses affect our results seriously. In the estimation, we correct for the fact that in some treatment municipalities single mothers on welfare (including the control group) were entitled to a reemployment bonus and that in some treatment municipalities single mothers on welfare (including the treatment group) were already entitled to a reemployment bonus in the control period.

¹¹On January 1, 2009, the Netherlands had 441 municipalities. The results hardly change when we leave out the four largest municipalities (Amsterdam, Rotterdam, The Hague, and Utrecht).

2009 in all treatment municipalities. No announcements were made before the start of the experiment. The implementation of direct job creation took somewhat longer and started in January 2010 for most of the municipalities (in the estimation, we take into account timing differences across municipalities). The experiment ended at the end of December 2010. We have also data of the years 2005–2008 available, which form the control period.

The treatment municipalities are of different size and are geographically spread through the Netherlands. It is possible that single mothers selected themselves into treatment municipalities. However, this is not very likely since single mothers did not know in advance which municipalities would participate in the experiment. And, we observe no movements of welfare recipients to treatment municipalities.

During the control and the treatment period, other policy changes were implemented that affected single mothers with young children. Childcare subsidies were increased for parents with a youngest child up to 12 years old (see Fig. 2 in Bettendorf et al. 2015). However, since the subsidy rates remained rather constant for low-income parents, this reform seems not relevant for mothers on welfare. The earned income tax credit (EITC) for single parents was also increased between 2005 and 2009 (see Fig. 3 in Bettendorf et al. 2015). The change in the credits was mostly targeted at middle- and high-income earners, like the change in childcare subsidies. As far as changes in the EITC and childcare subsidies did have an effect on mothers on welfare, our triple differences analysis takes the effects of these reforms into account by correcting for differences between the treatment and control group in control municipalities.

A final concern is that increased labor market participation of the treatment group would crowd out participation of single mothers in the control group. Then, we would overestimate the effect of the instruments. However, since single mothers are only a small group in the total population of job seekers, we do not consider this to be problematic.¹²

3 Data

Our data are from the 2005–2010 statistics on welfare benefits (BUS, *Bijstandsuitkeringenstatistiek*, CBS 2010a) gathered by Statistics Netherlands. The BUS is an administrative dataset that contains all welfare recipients in the Netherlands and provides information about gender, age, the cause of the welfare benefits, whether welfare recipients receive income from other resources (like labor income or alimony), the amount of welfare benefits that they receive, and whether or not they are a homeowner. We also have information about the number of children, age of the

¹²As mentioned before, single mothers (treatment + control group) comprise about 25 % of all welfare recipients.

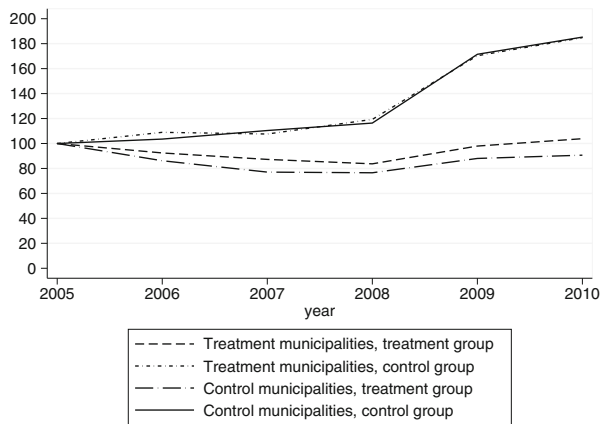


Fig. 2 Yearly inflow of single mothers in welfare (start of new spells); 2005 is the base year. Treatment municipality = municipality that implemented the instruments; control municipality = all other Dutch municipalities; treatment group = single mothers with children younger than 12 years; control group = single mothers with all children 12 years or older

children, marital status, and the ethnicity for all welfare recipients between 2005 and 2010 based on the population register.

In this study, we select all Dutch municipalities except Amsterdam and Langedijk. Amsterdam and Langedijk participated in the experiment, but the implementation of the experimental instruments failed in these municipalities, such that they did not provide reliable information about the effects of the instruments. Furthermore, we select all single mothers that flowed into welfare as from January 1, 2005. Figure 2 shows the yearly inflow of single mothers in welfare for the treatment group and the control group in the treatment and control municipalities. Clearly, there are differences between the treatment and control group, but within these groups, the differences between treatment and control municipalities are small. For the control group relatively, a lot of welfare spells started in 2009 and 2010, probably as a result of the financial and economic crisis. The number of new spells in the treatment group declined a bit between 2005 and 2008, but increased thereafter. Note that the growth rate in 2009 and 2010 is about the same for the treatment and control municipalities. This indicates that the experiment has not attracted more single mothers with young children into welfare.¹³

¹³When we regress the growth rates of the treatment group for the control and treatment municipalities on year dummies, an indicator for the treatment municipalities and interaction terms of the indicator and the year dummies we find no significant difference in the growth rates between treatment and control municipalities.

Table 1 Number of welfare spells per single mother

# Spells	Treatment municipality				Control municipality				Total	
	Treatment group		Control group		Treatment group		Control group			
	Freq.	%	Freq.	%	Freq.	%	Freq.	%	Freq.	%
1	5584	86.3	854	88.4	41,288	85.6	6650	90.3	54,376	86.2
2	767	11.9	96	9.9	6206	12.9	650	8.8	7719	12.2
3	107	1.7	13	1.4	680	1.4	61	0.8	861	1.4
4	12	0.2	3	0.3	72	0.2	6	0.1	93	0.1
5	3	0.1	0	0	5	0.0	1	0.0	9	0.0
Total mothers	6473	100	966	100	48,251	100	7368	100	63,058	100
Total spells	7502		1097		56,053		8162		72,814	

Treatment municipality = municipality that implemented the instruments; control municipality = all other Dutch municipalities; treatment group = single mothers with children younger than 12 years; control group = single mothers with all children 12 years or older. Single mothers are classified into the category of their first welfare spell

Table 1 shows the number of spells we observe for each single mother in the treatment and control group. In total, we observe 72,814 welfare spells for 63,058 single mothers. About 12 % of the welfare spells are from a treatment municipality and the treatment group is about six times larger than the control group. For most of the single mothers (86 %), we just observe one spell, for 12 % of the single mothers we observe two spells, and for the remaining 2 % we observe three spells or more.

The characteristics of the single mothers on welfare are presented in Table 2, where we compare the treatment and the control group, for the treatment and control municipalities, and for the treatment and control period. The descriptives show that the average age of the single mothers is 34 in the treatment group and 41 in the control group (most single mothers are aged between 20 and 53 years). On average, single mothers in the treatment group have 1.9 children and in the control group 1.7. The youngest child is on average 4.4 years old in the treatment group, and 13.9 years old in the control group. The treatment group consists of a lot of unmarried single mothers, while the control group has relatively more divorced mothers. Between 2005–2008 and 2009–2010, the number of divorced single mothers increased, while the number of married mothers decreased. However, all mothers that we study are living alone. A relative large share of 32 % of the single mothers is first generation non-western immigrant. Labor force participation is more than 20 % and is somewhat lower in the treatment municipalities than in the control municipalities. Alimonies are received by 16 % of the single mothers and the share of homeowners is low (3.3 %).

Table 2 Characteristics of the sample

	Treatment municipalities				Control municipalities			
	Control period		Treatment period		Control period		Treatment period	
	Treatment group	Control group	Treatment group	Control group	Treatment group	Control group	Treatment group	Control group
Average age single mother	32.5	38.3	33.4	41.7	33.2	39.3	34.2	42.9
Average age youngest child	4.0	13.8	4.5	13.9	4.2	13.8	4.7	13.9
Average number of children	1.9	1.7	1.9	1.6	1.9	1.8	2.0	1.7
% Unmarried	50.9	40.2	53.9	36.6	43.7	33.3	46.7	26.5
% Divorced	26.1	41.1	30.3	47.8	30.1	45.0	34.6	57.2
% Married	22.5	17.4	15.2	15.0	25.4	20.1	18.0	15.2
% Widowed	0.4	1.2	0.4	0.6	0.5	1.3	0.5	1.1
% Cohabiting	0.1	0.0	0.2	0.1	0.3	0.2	0.2	0.1
% First generation non-western immigrants	34.9	25.7	36.7	33.3	31.0	24.1	33.3	30.3
% Second generation non-western immigrants	6.9	4.4	8.8	3.4	5.0	3.3	6.4	2.4
% Immigrants from Western countries	10.4	13.7	10.2	13.6	10.5	12.3	10.7	12.4
% Receiving labor income	18.9	22.7	20.2	22.1	20.9	26.6	22.3	26.2
% Receiving alimony	10.7	12.5	11.8	15.7	14.8	15.6	16.0	18.6
% Home owner	3.7	3.9	3.7	3.7	3.6	3.4	2.9	3.3
# Spells	4793	632	2662	515	36384	4755	19131	3942

Treatment municipality = municipality that implemented the instruments; control municipality = all other municipalities; treatment period = 2009–2010; control period = 2005–2008; treatment group = single mothers with children younger than 12 years; control group = single mothers with all children 12 years or older

4 Exploratory analysis

To establish the effectiveness of the policy instruments, we observe five variables of interest. Two of them relate to durations, i.e., the duration from the start of the welfare spell to the acceptance of a part-time job while still remaining in welfare and the duration from the start of the welfare spell until outflow from welfare occurs. The other three variables of interest are financial variables, i.e., earnings received by the single mothers, welfare benefits paid by the municipalities, and the sum of earnings and welfare benefits. The variables of interest are only defined for welfare recipients. We do not have data about all single mothers in the Netherlands and this would be a problem when the decision to take up welfare is a function of benefit generosity. However, as discussed before, Fig. 2 strongly suggests that the experiment did not attract more single mothers with young children into welfare. In the remainder of this section, we explore the variables of interest, where we separately consider immigrant single mothers which we define as non-Western immigrants and native single mothers which we define as native Dutch or Western immigrants.¹⁴ Immigrant single mothers may have language difficulties and may be influenced by their home-country culture regarding the upbringing of children (Fernandez 2007; Kok et al. 2011). This may influence their labor force participation and their response to the policy instruments. Furthermore, compared to natives, immigrant single mothers relatively often bring up their children alone as from birth, i.e., without the help of a father, a grandmother, or a grandfather. By way of exploratory analysis, we consider the total treatment effect by comparing the treatment group to the control group, treatment municipalities to control municipalities, and the treatment period to the control period. In the exploratory analysis, we investigate the total effect of the earnings disregard and direct job creation and we ignore the potential heterogeneity of the treatment group.

4.1 Duration indicators

The earnings disregard and direct job creation may increase the inflow of single mothers to a part-time job. The solid lines in Fig. 3a, b present the inflow to part-time work for immigrant single mothers in the treatment and the control group. At $t = 0$, a welfare spell starts and the graphs show the share of immigrant single mothers that started a part-time job over the duration of their welfare benefit spell.

The two thin solid lines in Fig. 3a show that in the control period, the inflow into part-time work was about the same in treatment and control municipalities. This suggests that the treatment and control municipalities were comparable before the start of the experiment. Within 730 days after the start of a welfare spell, almost 20 % of the single mothers started part-time work. The thin and thick grey solid lines in Fig. 3a show that in the control municipalities the inflow to part-time work is lower in the treatment period than in the control period (probably related to the slowdown in economic activity due to the Great Recession). This decline is represented by the

¹⁴83 % of the group defined as 'native' is born in the Netherlands.

first difference (grey dashed line), which lies below zero because of the negative difference between the treatment and control period. Meanwhile, despite the economic crisis, the inflow into part-time jobs increased for the treatment municipalities (the thin and thick black solid lines). This can also be seen by the positive first difference (the black dashed line). Since we have no reason to assume that single mothers in the treatment municipalities are not influenced by the crisis, this indicates a positive effect of the experiment on the inflow to part-time work.

Figure 3b shows the same graph for the control group. As expected, because of the economic crisis, we find that the inflow to part-time work declined in both

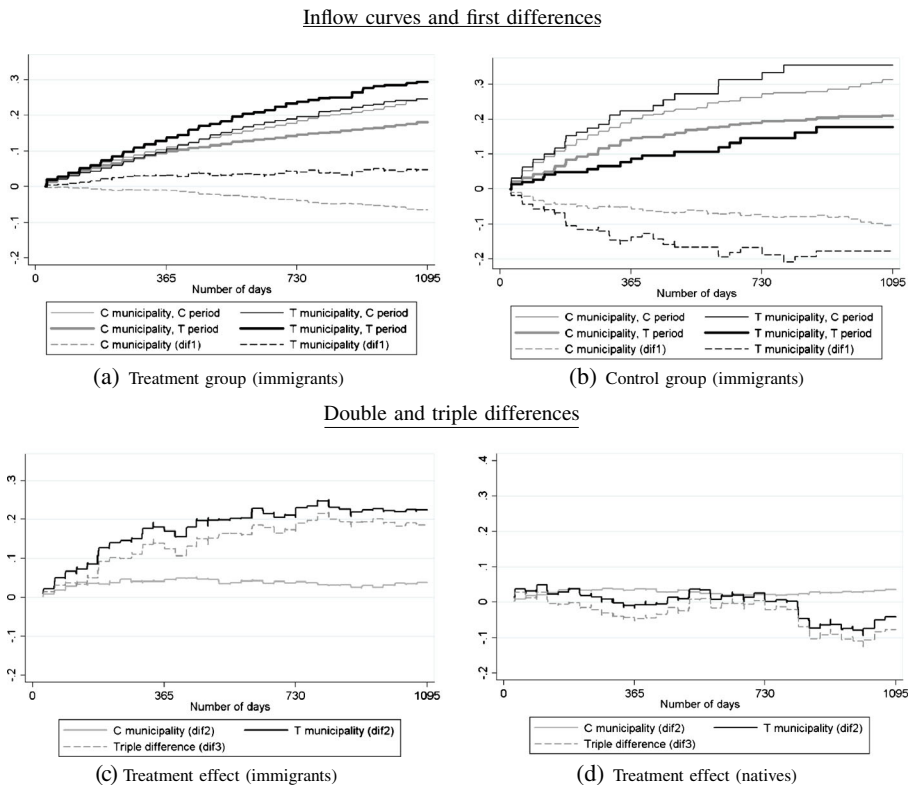


Fig. 3 Share of single mothers that start working in combination with welfare. Panels **a**, **b** show graphs for the inflow into part-time work while receiving welfare. At $t = 0$, a welfare spell starts and the graphs show the share of single mothers that started working over the duration of their welfare benefit spell. In addition, **a**, **b** show first differences, that present the increase or decrease in the share of single mothers that started working between the treatment and control period, for the treatment group and the control group in the treatment municipalities and control municipalities. Panels **c**, **d** present double and triple differences. The double differences show us to what degree the increase in the inflow to work was higher in the treatment than the the control group. Finally, we correct the double difference estimates of the treatment municipalities for the double differences found in the control municipalities. This leads to triple difference estimates of the treatment effects

treatment and control municipalities. This results in negative first differences (the dashed lines are below zero). In Fig. 3c, we plot the double differences—between the treatment period and the control period and between the treatment group and the control group—for the control municipalities and for the treatment municipalities. For example, the black solid line in Fig. 3c, that represents the double difference (dif2) of the treatment municipalities, shows the difference between the black dashed lines in Fig. 3a, b. This line illustrates that in the treatment municipalities, the increase in the inflow to part-time work within 2 years after the start of the welfare benefit was 23 % higher in the treatment than in the control group. The line that represents the double difference of the control municipalities is almost zero (the solid grey line), indicating that there are no large differences between the treatment and the control group in the control municipalities. The triple differences, computed by taking the difference between the two double differences, is clearly positive indicating a positive treatment effect. Using the triple difference approach, we take into account that the business cycle may have had a different effect on the treatment and the control group, and we exclude the possibility that the results are the consequences of municipality-specific developments (e.g., that treatment municipalities are less affected by the crisis than control municipalities).

Figure 3d presents the triple differences for natives and shows that there is hardly any treatment effect.¹⁵ When interpreting the graphs in Fig. 3, we have to take into account that not all single mothers received the treatment as from the start of their welfare spell. In addition, the sample becomes more and more selective over the welfare duration with regard to observed and unobserved characteristics. In the empirical analysis below, we will take this into account.

Panel a of Table 3 is set-up in the same way as Fig. 3. It collects the results of the graphs in Fig. 3 after a welfare spell of 2 years and shows how the triple difference estimates of the treatment effect after a welfare spell of 2 years can be calculated. The left part of panel a is about immigrant single mothers. In the control municipalities, there is almost no difference between the evolution of the treatment group and control group in the share of single mothers finding a part-time job within 2 years. The double difference is equal to 0.04. For the treatment municipalities, there is a substantial difference. Whereas the control group faces a decline of 0.19, the treatment group faces an increase of 0.04, leading to a double difference of 0.23. In combination with the double difference for the control municipalities of 0.04, this leads to a triple difference of 0.19, i.e., a triple difference of 19 percentage-points (this is also what we see in Fig. 3c after 2 years). The right-hand side part of Table 3a shows that for native single mothers the triple difference estimates of the treatment effects are small. Here, the triple difference is only -0.01 .

The second duration indicator from which the effectiveness of the treatment may be derived is the duration of the welfare spell. In the long run, the policy instruments may strengthen mother's labor force attachment and increase their probability

¹⁵ Inflow curves and first differences for native single mothers can be found in Fig. 6 of Appendix A.

Table 3 Treatment effects—exploratory analysis

		Immigrants			Natives		
		Treatment group	Control group	$\Delta\Delta$	Treatment group	Control group	$\Delta\Delta$
a. Part-time work after 2 years in welfare							
Treatment municipality	Treatment period	0.23	0.14		0.28	0.29	
	Control period	<i>0.19</i>	<i>0.33</i>		<i>0.34</i>	<i>0.36</i>	
	Δ	0.04	−0.19	0.23	−0.06	−0.07	0.01
Control municipality	Treatment period	0.14	0.19		0.28	0.28	
	Control period	<i>0.18</i>	<i>0.27</i>		<i>0.34</i>	<i>0.36</i>	
	Δ	−0.04	−0.08	0.04	−0.06	−0.08	0.02
$\Delta\Delta\Delta$				0.19			−0.01
b. Outflow from welfare after 2 years in welfare							
Treatment municipality	Treatment period	0.41	0.40		0.49	0.49	
	Control period	<i>0.54</i>	<i>0.69</i>		<i>0.60</i>	<i>0.75</i>	
	Δ	−0.13	−0.29	0.16	−0.11	−0.26	0.15
Control municipality	Treatment period	0.41	0.42		0.48	0.49	
	Control period	<i>0.50</i>	<i>0.65</i>		<i>0.61</i>	<i>0.72</i>	
	Δ	−0.09	−0.23	0.14	−0.13	−0.23	0.10
$\Delta\Delta\Delta$				0.02			0.05
c. Earnings							
Treatment municipality	Treatment period	197.1	212.3		307.1	364.0	
	Control period	<i>204.1</i>	<i>286.0</i>		<i>340.9</i>	<i>420.1</i>	
	Δ	−7.0	−73.7	66.7	−33.8	−56.1	22.3
Control municipality	Treatment period	185.1	240.5		343.8	390.8	
	Control period	<i>212.9</i>	<i>293.9</i>		<i>361.8</i>	<i>422.8</i>	
	Δ	−27.8	−53.4	25.6	−18.0	−32.0	14.0
$\Delta\Delta\Delta$				41.1			8.3
d. Welfare benefits							
Treatment municipality	Treatment period	945.4	933.3		881.1	814.6	
	Control period	<i>915.8</i>	<i>834.9</i>		<i>848.8</i>	<i>743.6</i>	
	Δ	29.6	98.4	−68.8	32.3	71.0	−38.7
Control municipality	Treatment period	953.3	912.5		840.4	790.9	
	Control period	<i>912.9</i>	<i>833.8</i>		<i>792.5</i>	<i>713.3</i>	
	Δ	40.4	78.7	−38.3	47.9	77.6	−29.7
$\Delta\Delta\Delta$				−30.5			−9.0
e. Earnings + Welfare Benefits							
Treatment municipality	Treatment period	1142.5	1145.6		1188.2	1178.7	
	Control period	<i>1119.8</i>	<i>1120.9</i>		<i>1189.6</i>	<i>1163.7</i>	
	Δ	22.7	24.7	−2.0	−1.4	15.0	−16.4

Table 3 (continued)

		Immigrants			Natives		
		Treatment group	Control group	$\Delta\Delta$	Treatment group	Control group	$\Delta\Delta$
Control municipality	Treatment period	1138.4	1153.0		1184.2	1181.7	
	Control period	1125.8	1127.7		1154.3	1136.1	
	Δ	12.6	25.3	-12.7	29.9	45.6	-15.7
	$\Delta\Delta\Delta$			10.7			-0.7

This table provides an indication of the treatment effect using a triple difference approach; Δ = first difference; $\Delta\Delta$ = Difference in differences; $\Delta\Delta\Delta$ = Triple difference. Treatment municipality = municipality that implemented the new policy; control municipality = all other Dutch municipalities; treatment group = single mothers with children younger than 12 years; control group = single mothers with all children 12 years or older. Earnings and welfare benefits in € per month

to flow out of welfare. However, in the short run, direct job creation may have a lock-in effect and this may reduce the probability to flow out of welfare. The earnings disregard may also be so attractive that outflow from welfare becomes less attractive. To investigate this, we test whether the total outflow from welfare is reduced by the experiment.¹⁶ Figure 4a shows that during the control period, 31 % of the single mothers in the treatment group left welfare within 1 year after the start of a welfare spell. After 2 years, this was 54 %, and after 3 years, this was about 63 %. Just as in Fig. 3, we compute first, double, and triple differences. Figure 4c shows that the triple differences are small indicating a small treatment effect. Figure 4d shows that by and large the triple difference results for natives are very much the same. Outflow curves and first differences for natives can again be found in Appendix A.

Panel b of Table 3 collects the results of the graphs in Fig. 4 and shows us triple difference estimates of the treatment effects after 2 years in welfare. The treatment effect is 2 % points for immigrants and 5 % points for natives. The treatment effects are small or perhaps even absent. Apparently, the total outflow from welfare is not reduced by the experiment.

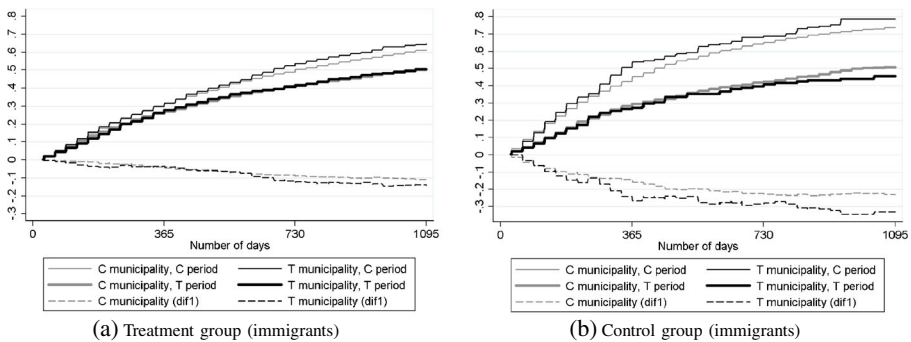
4.2 Financial indicators

The remaining three indicators of the success of the treatment relate to financial variables. By investigating labor earnings, we examine whether the instruments increased the inflow to part-time work and/or whether they increased the number of working hours. Furthermore, we evaluate whether the instruments reduced the welfare benefits paid by the municipalities and whether the instruments reduced poverty among single mothers and their children.

Figure 5 shows the distribution of income from sources other than welfare during the experiment. Mostly, these are earnings, alimonies from ex-partners, and tax

¹⁶Outflow may take place because of finding a job or because of other reasons, such as finding a partner.

Outflow curves and first differences



Double and triple differences

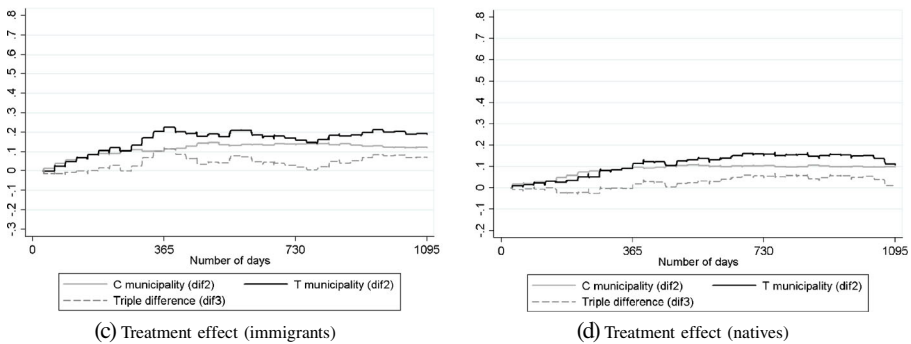


Fig. 4 Outflow from welfare. Panels **a**, **b** show graphs for the outflow from welfare. At $t = 0$, a welfare spell starts and the graphs show the share of single mothers that left welfare over the duration of their welfare benefit spell. See note of Fig. 3 for an explanation of first, double and triple differences

reductions. Unfortunately, we cannot distinguish earnings from other income that single mothers receive in addition to welfare. However, using the difference-in-differences approach, we can reasonably assume that other income, like alimonies, are not influenced by the experiment and in the remainder of this paper we will therefore define income from other sources as earnings. When income from other resources than welfare exceed the welfare benefit level, people have to leave welfare. Extreme income levels in Fig. 5 are probably related to post-payments.¹⁷

Panel c of Table 3 indicates that earnings increased as a result of the policy instruments, using the same triple difference set-up as before. For immigrant single mothers, the triple difference estimate of the treatment effect on earnings is €41 per

¹⁷For example, a single mother did not receive her alimony some months and now she receives a back pay of the alimonies from the last months at once.

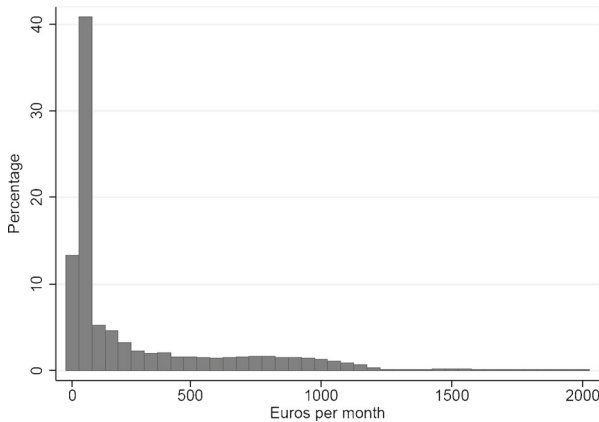


Fig. 5 Distribution of 'income from other sources'

month, while for native single mothers it is €8 per month. Panel d of Table 3 shows that for immigrant single mothers, welfare benefits on average went down with about €30 per month because of the experiment. For native single mothers, this was only €9 per month. Finally, panel e shows that the net benefits for the single mothers are low; for immigrant mothers, it is about €10 per month, for native mothers it is almost zero.

5 Empirical analysis

5.1 Model

In addition to the exploratory analysis, we present model estimates that take into account background characteristics and unobserved heterogeneity. In this way, we allow for changing compositions of the treated and non-treated single mothers that may affect the outcome variables. To identify the effect of the experiment, we use three dimensions, (1) we compare the treatment and the control group, (2) we compare treatment and control municipalities, and (3) we compare the treatment period 2009–2010 and the control period 2005–2008. We combine these dimensions using the difference-in-difference-in-differences approach.¹⁸

The advantage of the triple difference approach compared to the double difference approach is twofold. First, when developments are different for the treatment and the control group for other reasons than the policy experiment, we take this into account by comparing the developments in the control municipalities. Second, when developments are different for the treatment and control municipalities because of other reasons than the policy experiment, we take this into account by comparing the

¹⁸This approach is, among others, also used by Gruber (1994), Hamermesh and Trejo (2000), and Campolieti (2004).

control group in the treatment and control municipalities. Using the triple difference approach, we assume that there is no contemporaneous shock that affects the relative outcomes of the treatment group in the treatment municipalities during the treatment period. Furthermore, we assume that developments for the control group and developments for the treatment group in the control municipalities are not influenced by the experiment. Figures 3a and 4a show that before the experiment, inflow into a part-time job and outflow from welfare were very comparable in the treatment and control municipalities. To test the validity of the results, Section 5.5 also discusses the outcomes of tests in which we use placebo treatment years and placebo treatment municipalities.

The determinants of the inflow into part-time work and the outflow from welfare are established using a hazard rate model in which the transition rate of individual i at duration t conditional on observed characteristics X , unobserved characteristics u and treatment variables function $D\gamma$ is specified as follows:

$$\theta(t|X_{it}, D_{it}, u_i) = u_i \lambda(t) \exp(X_{it}\beta + D_{it}\gamma) \quad (1)$$

where β is a parameter vector and $\lambda(t)$ a piecewise constant function representing the pattern of duration dependence. We assume unobserved heterogeneity u to follow a Gamma distribution with variance α .

The vector X contains control variables such as the age of the single mother, the age of the youngest child, the number of children, marital status, the cause of the welfare benefits, whether the single mother receives alimony, and whether the single mother is a homeowner. In addition, X contains year dummies, dummies for the treatment municipalities, and dummy variables to correct for the presence of a reemployment bonus in some treatment municipalities.¹⁹ The vector X also contains time-varying covariates. For example, if during a welfare spell a child is born, the number of children increases and the age of the youngest child goes down.

The treatment variables function $D\gamma$ is specified as follows:

$$D_{it}\gamma = \gamma_1 G_{it} + \gamma_2 T_t + \gamma_3 M_i \cdot G_{it} + \gamma_4 M_i \cdot T_t + \gamma_5 G_{it} \cdot T_t + \delta G_{it} \cdot M_i \cdot T_t \quad (2)$$

where G represent a dummy variable for the treatment group, M a dummy variable for the treatment municipalities, and T a dummy variable for the treatment period (T). Sometimes, treatments started at different moments in time (e.g., direct job creation started at different moments in time across municipalities). We do not include M as a separate variable, because all treatment municipality dummies are already included in X . Also note that G is a time-varying variable as a single mother may

¹⁹Unfortunately, we do not have information about reemployment bonuses in control municipalities. However, for all treatment municipalities, we know whether and in which years they provided reemployment bonuses to the control group and the treatment group. To correct for this, we include interactions of year and the provision of a reemployment bonus (yes = 1, no = 0). In addition, we allow these effects to be different for the treatment and the control group, and later on within the treatment group for single mothers with the youngest child younger or older than 5.

switch from control group to treatment groups due to the introduction of the new policy or may switch from treatment group to control group because of the fact that her youngest child turns 12 years of age.²⁰ The main parameter of interest in Eq. 2 is δ , which represents the treatment effect based on the triple-difference approach.

The first parameter, γ_1 , captures the differences between the treatment and the control group (identification comes from the control period and the control municipalities). The second parameter, γ_2 , captures the difference between the treatment period and the control period (identification comes from the control group and the control municipalities). Parameter γ_3 captures the possible differences between the treatment groups in the municipalities that did and did not implement the instruments (identification comes from the control period). The period effect may be different for the treatment and control municipalities, which is captured by the fourth parameter (γ_4). Parameter γ_5 represents the interaction between the treatment group and the treatment period.

The determinants of earnings y_{it} that single mother i receives in time period t in addition to her welfare benefit is established using a linear regression model of the form

$$y_{it} = X_{it}\rho + D_{it}\kappa + \varepsilon_{it} \quad (3)$$

where X_{it} and D_{it} are specified before. In the same way, welfare benefits per recipient, paid by the municipalities, and income are modeled using linear regression models.

By computing clustered standard errors at the municipality level, we take into account error correlation within municipalities. Clustering standard errors at the municipality level is important, especially because the variable of interest is measured at the level of the municipality (e.g., Moulton 1986). Furthermore, among others, Bertrand et al. (2004) suggest that it is crucial to allow for the presence of serial correlation. In this paper, we allow for the presence of any correlation pattern within municipalities by using clustered standard errors. Clustered standard errors at the municipality level are used in Eqs. 1 and 3. To compute clustered standard errors in Eq. 1, we use a bootstrap procedure.²¹

5.2 Parameter estimates

We present the results of our empirical analysis in three steps. In this section, we present the overall effectiveness of the policy instruments. In Section 5.3, we investigate the effect of the earnings disregard and job creation separately. In Section 5.4, we investigate whether the treatment effects differ within the treatment group, making a distinction between single mothers with very young children aged 0 to 4 and single mothers with older children aged 5 to 11.

²⁰Nevertheless, results hardly change when we exclude the 177 untreated single mothers who are untreated because their children passed the age of 12 during the treatment period.

²¹In this procedure, we draw municipalities with replacement. For each bootstrap repetition, we store the estimation results and once all repetitions have been computed we calculate the standard errors of the stored model estimates.

Table 4 shows the estimated total treatment effects. Panel I of Table 4 shows the parameters estimates of the treatment. The full parameter estimates are presented in appendix Tables 9 and 10.²² To improve the interpretation of the treatment effects, panel II of Table 4 presents average adjusted predictions and average marginal effects. The average adjusted predictions show the average probability of part-time employment and outflow within 2 years²³ after the start of a welfare spell, in case all treated single mothers who entered welfare during the experiment would (or would not) have received the treatment. The difference gives us the average marginal effect of the treatment. For the standard errors in Ia, Ib, and II, we used 250 bootstrap replications.²⁴

As shown in panel Ia and IIa of Table 4, the treatment increased the inflow rate to a part-time job for immigrant single mothers significantly. The inflow to part-time work within 2 years after the start of a welfare spell increased by 18.7 %-points, from 4.6 to 23.4 %. For native single mothers, there is no significant treatment effect. From panel Ib and IIb of Table 4, it appears that outflow from welfare is not significantly affected by the treatment. Panel Ic shows that for both immigrant single mothers and native single mothers, the policies increased earnings; €95 per month for immigrant single mothers and €62 per month for native single mothers. This means that immigrant single mothers increased their participation on average with about 3 h per week and native single mothers with about 2 h per week.²⁵ Welfare benefits go down for both groups albeit the effect is only significant for immigrant single mothers. It is reassuring that the reduction in welfare benefits because of the experiment is lower than the earnings growth, since only part of the earnings (€4 per hour) are exempted from welfare benefits.

Finally, a potential effect of the experiment is poverty reduction among single mothers on welfare. This can also indirectly improve other aspects of life, such as health and self-esteem of the single mother. Panel Ie shows that single mother welfare recipients experience a net increase in their income, although this is only significant for immigrant single mothers. When we compare the results in Table 4 with the triple difference estimates in Table 3, we find that taking into account unobserved heterogeneity and observed differences between single mothers does not change

²²With regard to the control variables, Appendix Table 9 shows that the inflow into part-time employment increases with the age of the children and decreases with the number of children in the household. Lone single mothers who receive alimony start working part-time sooner than single mothers without any alimony and immigrants, especially first generation immigrants, are less likely to start a part-time job in addition to their welfare benefits. The longer people are in welfare without a part-time job, the less likely they are to start a part-time job (state dependence). Related to this, we find that the unobserved heterogeneity term is significant (the variance of the unobserved heterogeneity term, α , is significant).

²³We choose a period of 2 years, such that all treatment effects in the table are based on the same treatment of 2 years (the effects on earnings, welfare benefits, and income are also based on a treatment period of 2 years).

²⁴Results are basically the same when using 200 replications.

²⁵ $95/(\text{€}8 \times 4 \text{ weeks}) \approx 3$ and $62/(\text{€}8 \times 4 \text{ weeks}) \approx 2$.

Table 4 Treatment effects—empirical models

	Immigrants		Natives	
	Coeff.	S.E.	Coeff.	S.E.
I. Parameter estimates total treatment effect				
a. Finding (part-time) employment	1.778***	0.640	−0.130	0.243
b. Outflow from welfare	0.237	0.402	−0.120	0.135
c. Earnings	0.095***	0.027	0.062***	0.022
d. Welfare benefits	−0.051***	0.010	−0.035	0.028
e. Income (earnings + welfare benefits)	0.044*	0.025	0.027	0.020
II. Average adjusted predictions and average marginal effects				
a. Part-time employment within two years in welfare				
1) No treatment	0.046	0.062	0.341***	0.064
2) Treatment	0.234***	0.048	0.307***	0.036
2-1) Treatment effect	0.187**	0.078	−0.034	0.064
b. Outflow from welfare within two years in welfare				
1) No treatment	0.220***	0.059	0.505***	0.050
2) Treatment	0.269***	0.023	0.464***	0.020
2-1) Treatment effect	0.049	0.053	−0.040	0.045

Earnings, welfare benefits, and income in €1000 per month. Panel I contains parameter estimates of the treatment effects. Panel II contains average adjusted predictions and average marginal effects for the inflow to part-time work and the outflow from welfare. The average adjusted predictions show the average probability of part-time employment and outflow within 2 years after the start of a welfare spell, in case all treated single mothers who entered welfare during the experiment would (or would not) have received the treatment. The difference gives us the average marginal effect of the treatment. In the predictions, unobserved heterogeneity is ignored, i.e., the unobserved individual specific effect (u_i) is assumed to be one. For the bootstrapped standard errors in Ia, Ib, and II, we used 250 replications. For the standard error with regard to the outflow of immigrants, we excluded one bootstrap replication with an impossible high coefficient caused by an unfortunate combination of municipalities

the estimated treatment effects on the time to employment and outflow from welfare,²⁶ but increases the estimated treatment effects on earnings, welfare benefits, and income.

5.3 Sensitivity analysis—heterogeneous treatment effects

As indicated before, municipalities applied different types of policies to persuade single mothers to find (part-time) jobs. All treatment municipalities allowed

²⁶The average adjusted predictions with treatment in Table 4 can to some extent be compared with the employment and outflow rates of treated parents in Table 3. However, the selection and underlying methods are different. The rates in Table 3 are collected from Figs. 3 and 4 and are based on all individuals that participated in the experiment (also those who started their welfare spell before the experiment). Panel II of Table 4, on the other hand, shows the employment and outflow rates only for individuals that entered welfare during the experiment. They received the treatment as from the start of their welfare spell and the employment and outflow rates are calculated using the model results.

Table 5 Heterogeneous treatment effects

	Immigrants		Natives	
	Coeff.	S.E.	Coeff.	S.E.
I. Parameter estimates treatment effects				
a. Finding (part-time) employment				
Earnings disregard	1.592***	0.552	−0.247	0.239
Job creation	0.486	0.639	0.562*	0.299
b. Outflow from welfare				
Earnings disregard	0.369	0.560	−0.143	0.148
Job creation	−0.544	0.499	0.086	0.328
c. Earnings				
Earnings disregard	0.083***	0.030	0.049**	0.023
Job creation	0.065***	0.021	0.078***	0.026
d. Welfare benefits				
Earnings disregard	−0.038***	0.011	−0.025	0.031
Job creation	−0.055***	0.018	−0.057**	0.027
e. Income (earnings + welfare benefits)				
Earnings disregard	0.045	0.027	0.025	0.021
Job creation	0.010	0.016	0.020	0.015
II. Average adjusted predictions and average marginal effects				
a. Part-time employment within 2 years				
1) No treatment	0.042	0.072	0.305***	0.051
2) Earnings disregard	0.181***	0.056	0.249***	0.024
3) Earnings disregard + job creation	0.270***	0.076	0.390***	0.071
2-1) Effect earnings disregard	0.139**	0.066	−0.056	0.057
3-2) Effect job creation	0.089	0.112	0.141*	0.081
3-1) Total treatment effect	0.228**	0.111	0.085	0.074
b. Outflow from welfare within 2 years				
1) No treatment	0.252***	0.076	0.497***	0.057
2) Earnings disregard	0.341***	0.077	0.450***	0.047
3) Earnings disregard + job creation	0.217***	0.051	0.478***	0.065
2-1) Effect earnings disregard	0.089	0.100	−0.047	0.048
3-2) Effect job creation	−0.124	0.108	0.028	0.101
3-1) Total treatment effect	−0.035	0.093	−0.019	0.093

Earnings, welfare benefits, and income in €1000 per month. Panel I shows the estimated treatment effect of the earnings disregard and the estimated treatment effect of job creation on top of the earnings disregard. Panel II contains average adjusted predictions and average marginal effects for the inflow to part-time work and the outflow from welfare. See also note Table 4. For the standard error with regard to finding (part-time) employment for immigrants, we excluded six bootstrap replications with an impossible high coefficient caused by an unfortunate combination of municipalities

single mothers in the treatment group an earnings disregard. In addition to that, some municipalities stimulated direct job creation in the public and private sector. By way of sensitivity analysis, Table 5 shows to what extent different policy measures had different treatment effects.²⁷ Panel Ia shows that for immigrant single mothers the earnings disregard had a significant positive effect on the inflow to a part-time job but job creation did not have a significant additional positive effect. The average marginal effect in panel IIa shows that the earnings disregard increased part-time employment within 2 years on average with 13.9 %-points, from 4.2 to 18.1 %. For native single mothers, job creation (in addition to the earnings disregard) increased part-time employment with 14.1 %-points (weakly significant). The implied value of the elasticity of the participation rate is 0.3, which is well within the range presented by Bettendorf et al. (2014).²⁸ For the outflow from welfare, we find no significant effect of either policy instrument.

As shown in panel Ic of Table 5, the earnings disregard increased earnings of immigrant single mothers significantly with €83 per month, which implies an increased participation of about 2.5 h per week ($€83/€8 \times 4$ weeks) on average (as suggested by Fig. 1, the growth in the number of hours worked per week is small). For native single mothers, the effect of the earnings disregard on earnings was also significantly positive (€49, about 1.5 h per week). For both types of mothers, job creation had an additional significant positive effects on earnings. Single mothers probably started jobs for more hours per week than they would have done without direct job creation.

One of the aims of the experiment was to decrease welfare expenditures. When the experimental instruments increase labor force participation and/or the number of hours worked, and only part of labor income is exempted from the welfare benefit, this has a positive effect on the welfare benefits paid by the municipality. On the other hand, single mothers that already worked part-time before the experiment was implemented now receive an earnings disregard of €4 per hour, whereas they received nothing before the experiment. This has a negative effect on the amount of welfare benefits per welfare recipient paid by the municipality. Panel Id of Table 5 shows that the positive effect dominates, but that the effect of the earnings disregard is not significant for native single mothers. Job creation, additional to the earnings disregard, significantly reduced welfare benefits for immigrant and native single mothers by €55 and €57, respectively. The sum of welfare benefits and earnings is not significantly affected.

²⁷For this sensitivity analysis, we included a treatment variables function (like Eq. 2) for the six municipalities that implemented direct job creation, the period in which job creation took place, and the treatment group.

²⁸The employment rate of single mothers with young children in welfare is 20.7 % (Table 2). In our estimates, we find that the earnings disregard increased the labor force participation (through part-time work) by 13.9 %-points for immigrants and by -5.6 %-points for natives. This means a weighted average of $13.9 \times 1/3 - 5.6 \times 2/3 = 0.9$ %-points (about one third of the single mothers with young children on welfare is immigrant). This implies an increase in the employment rate of $0.9/20.7 = 4.4$ %. The maximum earnings disregard is 30 euro/week. The welfare benefits are approximately 225 euro/week; so the treatment is $30/225 = 13.3$ %. Combining these numbers, we find an implicit elasticity of the participation rate with respect to the net income gain when moving from welfare to work of $4.4/13.3 = 0.3$.

Table 6 Treatment effects: differentiated by age of the youngest child

Age youngest child	Immigrants				Natives			
	0–4		5–11		0–4		5–11	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
I. Parameter estimates treatment effects								
a. Finding (part-time) employment								
Earnings disregard	1.444***	0.361	1.801***	0.526	−0.262	0.162	−0.228	0.205
Job creation	0.655*	0.388	0.310	0.709	0.544	0.540	0.595*	0.306
b. Outflow from welfare								
Earnings disregard	0.316	0.655	0.442	0.542	−0.157	0.156	−0.122	0.200
Job creation	−0.529	0.461	−0.579	0.475	−0.056	0.357	0.240	0.347
c. Earnings								
Earnings disregard	0.096***	0.034	0.076***	0.028	0.043**	0.020	0.061**	0.030
Job creation	0.049	0.031	0.079***	0.015	0.094***	0.031	0.061**	0.028
d. Welfare benefits								
Earnings disregard	−0.055***	0.014	−0.023*	0.013	−0.006	0.034	−0.047	0.030
Job creation	−0.033	0.022	−0.076***	0.021	−0.057**	0.025	−0.057*	0.031
e. Income (earnings + welfare benefits)								
Earnings disregard	0.041	0.030	0.053**	0.025	0.037*	0.019	0.014	0.030
Job creation	0.016	0.017	0.003	0.020	0.037	0.023	0.004	0.019
II. Average adjusted predictions and average marginal effects								
a. Part-time employment within 2 years								
1) No treatment	0.039	0.056	0.044	0.079	0.289***	0.058	0.324***	0.059
2) ED	0.202***	0.051	0.229***	0.076	0.239***	0.041	0.269***	0.038
3) ED + JC	0.261***	0.094	0.293***	0.084	0.386***	0.091	0.428***	0.072
2-1) Effect ED	0.164***	0.061	0.184***	0.068	−0.050	0.047	−0.055	0.052
3-2) Effect JC	0.058	0.113	0.064	0.133	0.148*	0.084	0.159*	0.087
3-1) Total effect	0.222*	0.116	0.248**	0.124	0.097	0.079	0.104	0.083
b. Outflow from welfare within 2 years								
1) No treatment	0.298***	0.087	0.196***	0.075	0.553***	0.059	0.440***	0.067
2) ED	0.419***	0.129	0.286***	0.075	0.511***	0.076	0.402***	0.066
3) ED + JC	0.266***	0.068	0.173***	0.040	0.595***	0.096	0.478***	0.061
2-1) Effect ED	0.121	0.124	0.090	0.095	−0.042	0.065	−0.037	0.060
3-2) Effect JC	−0.153	0.114	−0.112	0.090	0.084	0.112	0.076	0.104
3-1) Total effect	−0.032	0.099	−0.022	0.085	0.042	0.100	0.038	0.094

Earnings, welfare benefits, and income in €1000 per month. Panel I shows the estimated treatment effect of the earnings disregard and the estimated treatment effect of job creation on top of the earnings disregard. Panel II contains average adjusted predictions and average marginal effects for the inflow to part-time work and the outflow from welfare. *ED* earnings disregard, *JC* job creation. See also note Table 4. For the standard error with regard to finding (part-time) employment for immigrants, we excluded three bootstrap replications with an impossible high coefficient caused by an unfortunate combination of municipalities

5.4 Sensitivity analysis—heterogeneous treatment groups

Table 6 presents the parameter estimates of the heterogeneous treatment effect on the various outcome measures if we distinguish two treatment groups, single mothers with the youngest child aged between 0 and 4 years and single mothers with the youngest child aged between 5 and 11 years.²⁹ Panels Ia and IIa of Table 6 show the estimated treatment effects for the inflow to part-time work. Comparing the parameter estimates with those in panels Ia and IIa of Table 5, it is clear that making a distinction between two treatment groups does not add much to the insight in the policy effects. The parameter estimates for the two treatment groups are not significantly different. Estimation results for the outflow from welfare are reported in panels Ib and IIb of Table 6 and are also not significantly different for the two treatment groups.

Panel Ic of Table 6 shows that direct job creation was significantly effective for the earnings of immigrants with children of age 5–11 (an income growth of €79 per month) and natives with the youngest child aged between 0 and 4 years (€94 per month) and 5–11 years (€61 per month), but not for immigrants with the youngest child aged between 0 and 4 years. Regarding job creation, we measure the intention to treat. Immigrant single mothers with the youngest child between the age of 0 and 4 are the most difficult category within the treatment group and are less often approached by municipalities to participate in the labor pool. This may explain the insignificant effect of job creation for this group. Immigrant mothers with the youngest child aged between 5 and 11 years and native mothers with the youngest child aged between 0 and 4 years probably start jobs of more hours per week than they would do without direct job creation (since the time to employment was not reduced by direct job creation, panel a).

The estimated treatment effects for welfare benefits, reported in panel d of Table 6, are significantly different for immigrant single mothers with children between the age of 0–4 and 5–11. The earnings disregard reduced welfare benefits significantly for immigrant mothers with the youngest child between the age of 0 and 4 years, but this is only weakly significant for immigrant single mothers with the youngest child aged between 5 and 11 years. This can be explained by the fact that in the last group (with older children), relatively more single mothers would have worked anyway (also without the earnings disregard). In the experiment, these mothers received an earnings disregard of €4 per hour, whereas they received nothing outside the experiment. This reduced the financial advantage for the municipalities for this group. Job creation as an additional instrument, on the other hand, is more effective for immigrant single mothers with children between 5 and 11 years and for native single mothers with children between 0 and 4 years. As explained above, immigrant single mothers with the youngest child between 0 and 4 are probably less often approached by municipalities. On the other hand, natives with the youngest child aged between 5 and 11 years (the most ‘easy’ category within the treatment group) are approached frequently but probably need less help in finding a job, such that the effectiveness of direct job creation is only weakly significant for them. Finally, panel

²⁹For this sensitivity analysis, we used interactions with a dummy variable indicating whether the youngest child in the household was aged between 5 and 11 years.

e of Table 6 shows the estimated treatment effects on income, which are not significantly different for single mother with young (0–4 years) and old (5–11 years) children.

5.5 Robustness checks

To check the validity of the results, Tables 7 and 8 present the results of two placebo tests, i.e., we replace the actual treatment period by a placebo treatment period and we replace the treatment municipalities by placebo treatment municipalities. Table 7 presents estimated treatment effects when we assume that the treatment already took place in 2007–2008 instead of 2009–2010 (placebo treatment period). Except for income, none of the variables of interest are significant (as it should be). Table 8 presents the results of 250 tests with placebo treatment municipalities. Here, we randomly choose 14 placebo treatment municipalities from all municipalities in the Netherlands, estimate the (placebo) treatment effects and repeat this test 250 times. The first two columns of Table 8 show the parameter estimates (which can also be found Table 4) and z values for the real treatment municipalities; the third and fourth column show the parameter estimates and mean z values for the placebo treatment municipalities. Comparing the first and the third column, it is clear that the average treatment effects for the placebo municipalities are much smaller and often have the opposite sign compared to the treatment effects for the real treatment municipalities. The fourth column shows that on average, the treatment effects for the placebo municipalities are all insignificant. Columns 5 to 9 show the distribution of z values of the placebo tests. For example, the first row shows that the z value of the parameter estimate for the transition rate to (part-time) employment of immigrants in the real treatment municipalities is 3.09, while the 99th percentile of the z values in the placebo tests is 1.71. This means that (almost) none of the placebo treatment effects are as significantly positive as the effect found in the experiment. This also holds for earnings and welfare benefits of immigrants (the third and the fourth row). For earnings of natives, we find that in somewhat more than 1 % of the placebo tests, the z

Table 7 Treatment effects for placebo treatment period

	Immigrants		Natives	
	Coeff.	S.E.	Coeff.	S.E.
I. Parameter estimates total treatment effect with placebo treatment period				
a. Finding (part-time) employment	−0.120	0.422	0.022	0.332
b. Outflow from welfare	0.184	0.377	−0.199	0.175
c. Earnings	0.024	0.048	−0.042	0.033
d. Welfare benefits	−0.083	0.054	−0.001	0.025
e. Income (earnings + welfare benefits)	−0.059*	0.032	−0.043**	0.020

Earnings, welfare benefits, and income in €1000 per month. Ia (natives) and Ib (immigrants and natives) are estimated without unobserved heterogeneity because of convergence difficulties

Table 8 Comparing treatment effects for real treatment municipalities and placebo treatment municipalities

	Real treatment municipalities		Placebo treatment municipalities						
			Mean		Distribution z-values				
	Coeff.	z-value	Coeff.	z-value	p50	p75	p90	p95	p99
Immigrants									
a. Finding (part-time) employment	1.778	3.09	-1.050	-0.26	-0.20	0.24	0.90	1.28	1.71
b. Outflow from welfare	0.237	0.82	-0.031	-0.09	-0.11	0.58	1.15	1.37	1.71
c. Earnings	0.095	3.48	-0.011	-0.29	-0.30	0.58	1.60	2.24	3.10
d. Welfare benefits	-0.051	-5.04	0.009	0.25	0.24	-0.50	-1.20	-2.00	-4.68
e. Income (earnings + welfare benefits)	0.044	1.80	-0.002	0.03	-0.15	0.78	1.70	2.48	4.52
Natives									
a. Finding (part-time) employment	-0.130	-0.42	0.045	0.08	0.01	0.64	1.28	1.73	2.26
b. Outflow from welfare	-0.120	-0.76	-0.002	-0.01	0.01	0.72	1.21	1.52	2.11
c. Earnings	0.062	2.87	-0.005	-0.05	0.00	0.87	1.51	1.95	2.91
d. Welfare benefits	-0.035	-1.27	0.007	0.14	0.12	-0.70	-1.48	-1.91	-3.42
e. Income (earnings + welfare benefits)	0.027	1.39	0.002	0.07	0.19	0.87	1.58	2.05	3.07

Earnings, welfare benefits, and income in €1000 per month. In this placebo test, we randomly choose 14 placebo treatment municipalities. We repeat this test 250 times. From left to right, this table reports the treatment effects of the real treatment municipalities (also found in Table 4) and the accompanying z values, the average treatment effect found for the placebo municipalities with the accompanying mean z value, and the distribution of z values found in the placebo tests. We show the median z value and several percentiles. With regard to welfare benefits (d), we show the 25th, 10th, 5th, and 1st percentile, because we are interested how often a z value below -1.27 appears

value is higher than the z value found in the experiment. In the rows where treatment effects of the experiment are not significant, roughly 5 % of the placebo tests show significant treatment effect. Our main conclusion from Table 8 is that the effects we measure are indeed real treatment effects.

In additional robustness checks not reported, we considered data selections with more narrow ranges around the age limit of 12 years for the youngest child in the household, such as the range from 7 to 16 years. The results hardly change. Only a smaller age range leads to a significant reduction of welfare benefits for natives (on average -88 euro instead of -35 euro found in Table 4). Furthermore, we considered selections of spells based on the cause of the welfare spell (end of job, end of marriage, and end of education). The results are about the same, but less significant, probably because of the smaller sample sizes.

6 Conclusions

Single mothers with young children are often long-term recipients of welfare benefits. Long-term welfare dependency creates a big distance to the labor market. If single mothers on welfare would have a part-time job, they would increase their work experience which might make it easier for them later in life to find a full-time job and leave welfare. However, financial incentives to work part-time while receiving welfare are not very strong. Usually, benefits are reduced one-for-one when single mothers accept a job and start earning money. To explore the possibilities to encourage single mother welfare recipients with young children to at least find a part-time job, the Dutch government funded a policy experiment in 2009 and 2010. In this experiment, an earnings disregard was introduced that was sometimes combined with job creation. The experiment was performed at the level of municipalities in the Netherlands. Our paper presents an analysis about the effectiveness of both policies. We evaluate the effectiveness of the policy experiment by comparing the calendar time periods before and during the experiment, the municipalities with and without the experimental policy instruments and by comparing single mothers with young children (who are treated) and single mothers with older children (who are not treated). In our analysis, we make a distinction between native single mothers and immigrant single mothers as the latter group has a particularly weak labor market position. In addition, we differentiate the treatment effect for single mothers with the youngest child between the age of 0 and 4 and single mothers with the youngest child between the age of 5 and 11, as both have different needs for care. Our study differs from most of the previous research on the labor supply of single mothers on welfare because unlike many studies who use single women without children as control group, we have as control group single mothers with older children. Furthermore, rather than focusing on one policy instrument, we are able to study for the same group of individuals the separate effects of two policy instruments, a financial incentive through the earnings disregard and a job opportunity through job creation. Finally, whereas many studies are done in countries with relatively low benefits, we study policy measures in a country with a relatively generous benefit system.

Our main findings are that the earnings disregard stimulated employment of immigrant single mothers and earnings of all single mothers. In addition, job creation reduced welfare benefits and increased earnings for immigrant single mothers with the youngest child between the age of 5 and 11 and for native single mothers. Regarding job creation, we measure an intention to treat. This means that we do not have to worry about a non-random selection of single mothers into the treatment and that we take into account the success or failure that municipalities experience in finding jobs for single mothers. Nevertheless, we may underestimate the potential effectiveness of job creation. Welfare expenditures are reduced by the experiment. The earnings disregard significantly reduced welfare benefits for immigrant single mothers. That is because labor force participation increased for this group and almost no disregards had to be paid to mothers that would also have worked without the financial incentive. Income increased significantly for immigrant single mothers with the youngest child between 5 and 11 years. This also implies favorable effects on the children in these households. The earnings disregard and job creation do not provide incentives

to flow out of welfare in the short run, but they also did not reduce outflow from welfare (the attractiveness of the instruments may keep single mothers in welfare). The overall effect of earnings disregards is a combination of the effects of financial incentives and the monitoring by case workers as part of the new policy (some treated mothers got an individual appointment with a case worker to inform them about the experiment and review their situation). A surprising finding is that earnings disregards are mostly effective for immigrant single mothers, who are at the low end of the earnings distribution. A possible explanation for this finding is that usually immigrant single mothers were no target group in activation policies. Maybe the effect for native single mothers is lower than for immigrant single mothers because they were monitored already more strictly and were more engaged in activation policies. Furthermore, the lack of effects for native single mothers could have to do with the situation in the labor market. During the period of the experiment, there was little or no job growth in the Netherlands which could have influenced the results. On the other hand, the labor market position of immigrant single mothers on welfare was so weak that it could only improve by providing the right incentives. Maybe, it is not the immigrant status itself that causes a higher receptiveness to the instruments. For example, if immigrant single mothers have a lower education level on average, and the effects of the policy instruments are higher for lower-educated single mothers, this also explains the difference. Furthermore, maybe low wage individuals feel more attracted by the earnings disregard as they benefit the most (relatively) from the earnings disregard (they may keep a larger share of their earnings). If this is the case, this may also explain the difference between immigrant and native single mothers, when wage rates are on average lower for immigrant single mothers than for native single mothers. Finally, native single mothers may receive more informal financial support from their parents than immigrant single mothers whose parents live outside the Netherlands. That would make native single mothers less sensitive to financial incentives than immigrant single mothers.

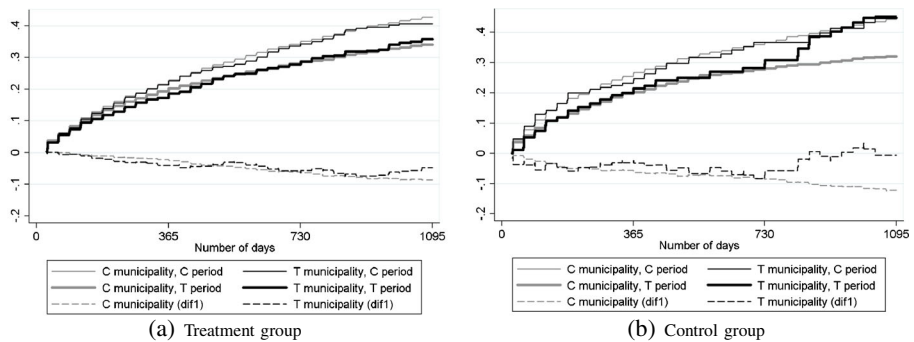
The purpose of the policy experiment was to encourage single mothers on welfare to enter the labor market if only through a part-time job. This would not induce them to leave welfare but it might make it easier for them to find a full-time job later on in life and then leave welfare. The goal of the experiment, to increase part-time work among single mothers on welfare and decrease welfare expenditures, succeeded. However, outflow from welfare did not increase. So, within the window of the data, small part-time jobs did not (yet) function as a stepping stone. From a policy point of view, the earnings disregard is an effective instrument to increase labor force participation and earnings of immigrant single mothers with children younger than 12 years. Nevertheless, as predicted by comparing the budget constraint with and without the experiment, the extra number of hours worked because of the policy experiment is small. If one aims to reduce welfare benefits by using an earnings disregard, it would be wise to lower the age limit of 12 years, to reduce the number of disregards given to single mothers that would have worked anyway. Job creation, in addition to the earnings disregard, also induces lower welfare benefits for single mothers with older children. Of course the efficiency of job creation depends on the costs that municipalities have to make to find jobs for the single mothers. Furthermore, after the implementation of an earnings disregard additional screening may be necessary, to prevent single mothers to enter welfare because of the attractiveness of the earnings disregard.

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Appendix A: Extended figures

Inflow into part-time work



Outflow from welfare

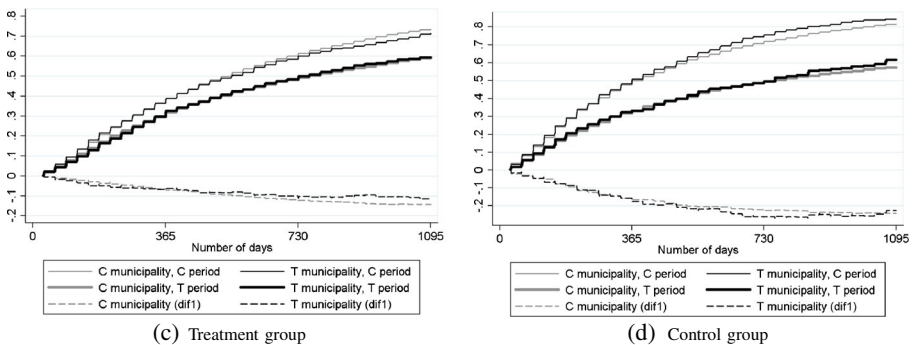


Fig. 6 Inflow, outflow, and first differences for natives

Appendix B: Extended estimation results

Table 9 Finding part-time employment and outflow from welfare

Variable	Finding part-time employment				Outflow from welfare			
	Immigrants		Natives		Immigrants		Natives	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
G	-0.073	0.103	-0.035	0.060	-0.011	0.069	0.053	0.038
M · G	-0.107	0.230	0.007	0.118	-0.129	0.226	-0.020	0.108
T	-0.564***	0.131	-0.429***	0.074	-0.853***	0.082	-0.819***	0.040
M · T	-0.975**	0.393	0.212	0.249	-0.254	0.271	0.179	0.126
G · T	0.190	0.116	0.182***	0.061	0.398***	0.073	0.276***	0.035
G · M · T	1.778***	0.640	-0.130	0.243	0.237	0.402	-0.120	0.135
Welfare 30–60 days	0.046	0.100	-0.025	0.057	0.047	0.104	0.065	0.056
Welfare 61–90 days	-0.359***	0.126	-0.288***	0.069	0.263***	0.098	0.377***	0.054
Welfare 91–180 days	-0.270*	0.147	-0.390***	0.073	0.105	0.090	0.592***	0.053
Welfare 181–365 days	-0.510***	0.140	-0.754***	0.073	-0.071	0.090	0.576***	0.053
Welfare 366–730 days	-0.739***	0.152	-1.018***	0.074	-0.292***	0.090	0.402***	0.053
Welfare > 730 days	-0.998***	0.168	-1.303***	0.091	-0.559***	0.092	0.125**	0.056
Age	0.157***	0.021	0.070***	0.011	-0.043***	0.011	-0.001	0.007
Age ²	-0.002***	0.000	-0.001***	0.000	0.000**	0.000	0.000***	0.000
Age youngest child	0.067***	0.015	0.051***	0.011	-0.079***	0.011	-0.014**	0.006
Age youngest child ²	-0.001	0.001	-0.002**	0.001	0.007***	0.001	0.004***	0.000
# Children	-0.124***	0.022	-0.121***	0.014	-0.033**	0.014	-0.056***	0.008
Unmarried	0.298***	0.044	-0.014	0.023	-0.069**	0.034	-0.251***	0.015
Widow	-0.104	0.196	-0.707**	0.317	-0.004	0.130	0.081	0.104
First gen. immigrant	-0.331***	0.049			-0.153***	0.052		
Western immigrant			-0.143***	0.033			-0.097***	0.019
Alimony	0.521***	0.068	0.387***	0.040	0.243***	0.045	-0.408***	0.029
Homeowner	0.209	0.163	0.083	0.065	0.294***	0.110	0.213***	0.035
Constant	-10.256***	0.375	-7.809***	0.224	-5.754***	0.250	-6.425***	0.141
α (variance)	0.418**	0.176	0.231***	0.083	0.062	0.059	0.065***	0.021
# Individuals	20020		30172		22150		40132	

G target group, M treatment municipality, T treatment period, P treatment municipality with job creation. In this model, we also included year dummies, treatment municipality dummies, dummies indicating the cause of welfare, and correction terms for treatment municipalities with a reemployment bonus. Unfortunately, we do not have information about reemployment bonuses in control municipalities. However, for all treatment municipalities, we know whether and in which years they provided reemployment bonuses to the control group and the treatment group. To correct for this, we include interactions of year and the provision of a reemployment bonus (yes = 1, no = 0). In addition, we allow these effect to be different for the treatment and the control group

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table 10 Earnings and welfare benefits

Variable	Earnings		Welfare benefits				Earnings + Benefits			
	Immigrants		Immigrants	Natives	S.E.	Coeff.	Immigrants	Natives	S.E.	Coeff.
G	-19.15*	9.88	14.26	8.09	8.86	-1.06	-4.89	7.37	6.27	-10.61*
M · G	2.25	31.86	-1.99	24.74	28.23	9.07	0.26	21.33	18.13	0.87
T	-62.73***	10.75	71.29***	7.60	10.00	143.20***	8.57	6.91	7.81	140.62***
M · T	-89.35***	30.80	64.62**	28.23	28.71	21.33	-24.74	26.76	21.44	-54.01**
G · T	31.15***	9.50	-53.56***	7.37	8.58	-40.50***	-22.41***	6.57	5.78	-20.38***
G · M · T	95.22***	32.60	-50.76*	31.14	29.86	-35.09	44.46**	29.40	22.50	27.22
Welfare 30–60 days	-17.45***	5.79	-26.80***	5.03	7.40	-25.41***	-44.24***	5.48	8.49	-36.21***
Welfare 61–90 days	-36.34***	4.33	-134.04***	3.70	5.88	-82.32***	-170.37***	4.23	6.74	-108.39***
Welfare 91–180 days	-85.05***	4.63	-206.98***	3.93	6.08	-180.22***	-292.03***	4.45	7.02	-277.48***
Welfare 181–365 days	-87.89***	4.88	-207.93***	4.18	6.20	-190.94***	-295.82***	4.61	7.16	-296.24***
Welfare 366–730 days	-99.94***	5.12	-184.34***	4.47	6.29	-160.72***	-284.29***	4.80	7.15	-283.19***
Welfare longer than 730 days	-107.72***	5.40	-148.65***	4.79	6.29	-114.55***	-256.37***	4.87	6.85	-248.39***
Age	21.71***	1.71	17.15***	1.69	1.85	17.82***	38.86***	1.71	1.43	44.74***
Age ²	-0.24***	0.02	-0.24***	0.02	0.03	-0.24***	-0.48***	0.02	0.02	-0.56***
Age youngest child	2.31*	1.22	4.73***	1.33	1.14	-7.51***	1.10	1.20	0.83	-2.78***
Age youngest child ²	0.08	0.10	-0.29***	0.10	0.10	0.01	-0.21***	0.09	0.07	-0.13**
# Children	-18.21***	1.51	8.90***	1.91	1.35	16.06***	-9.31***	1.70	0.89	-15.13***
Unmarried	7.95**	3.75	6.27*	4.08	3.27	24.64***	14.22***	3.54	2.08	3.02
Widow	32.20	22.03	-35.01*	35.35	19.80	-37.26	-2.81	31.85	8.67	-2.51
First generation immigrant	-21.48***	4.66	6.73	4.30			-14.74***		3.14	

Table 10 (continued)

Variable	Earnings		Welfare benefits		Earnings + Benefits	
	Immigrants	Natives	Immigrants	Natives	Immigrants	Natives
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
Western immigrant						
Alimony	243.02***	7.15	-177.57***	6.22	65.46***	4.20
Homeowner	42.63**	16.80	-41.45***	14.50	1.19	10.86
Constant	-110.54***	29.94	807.19***	33.34	696.65***	27.54
# Individuals	22177	38614	22177	38614	22177	38614

We only take into account observations for which welfare benefits and earnings are lower than €2000 per month. See also note Table 9

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

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