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## EVALUATION OF THE FINNISH INCOME DISREGARD REFORM

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# Evaluation of the Finnish Income Disregard Reform\*

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## Abstract

In 2002, the Finnish government introduced an earnings disregard reform aimed at improving the incentives of low-income individuals who receive last-resort social assistance. The aim of the reform was to decrease unemployment by providing social assistance clients better incentives to receive at least temporary or part-time work. This paper evaluates the employment effects of the reform using a quasi-experimental design. After a behavioral adjustment period, there are positive results for females, single-person households and individuals with earnings. No effects on the extensive margin imply that a behavioural response requires some attachment to the labour market. No transition from social assistance to longer-term employment is observed.

JEL: C93, H53, I38, J68

Keywords: Difference-in-differences matching, making work pay, earnings disregard, welfare

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## Tiivistelmä

Tutkimuksessa tarkastellaan kvasikokeellisin menetelmin vuonna 2002 voimaan tulleen toimeentulotuen etuoikeutetun tulon työllisyysvaikutuksia. Etuoikeutettuna tulona jätetään huomiotta vähintään 20 prosenttia ansiotuloista. Kotitalouskohtainen maksimäärä oli aluksi 100 euroa kuukaudessa, joka korotettiin 150 euroon vuonna 2005. Tarkasteltava ajanjakso kattaa vuodet 2002-2005. Tutkimuksen aineistona käytetään Tilastokeskuksen rekisteriainestoa.

Tutkimuksessa havaitaan tilastollisesti merkitseviä vaikutuksia naisille, yksinasuville ja työtuloja saaville toimeentulotukiasiakkaille. Vuosiansiossa reformin vaikutus tuloihin oli noin 200 euroa, 250 euroa ja 300 euroa vastaavasti ryhmittäin. Lyhytaikaista toimeentulotuen saantia ei ole pääsääntöisesti huomioitu. Tulosten mukaan etuoikeutetulla tulolla ei ollut juurikaan vaikutuksia työmarkkina-aseman muutoksiin. Pääasialliset vaikutukset olivat jo työmarkkinoilla olevien ansiotuloihin. Reformi ei kasvattanut työmarkkinoille osallistuvien määrää eikä sillä ollut vaikutusta siirtymiin toimeentulotuesta pidempiaikaiseen työllistymiseen.

Tutkimusasetelma ei mahdollista niiden toimeentulotukiasikkaiden havaitsemista, jotka tosiasissa saivat etuoikeutettua tuloa. Keskimääräiset tulokset toimeentulotukiasiakkaille ovat kuitenkin politiikkarelevantteja, sillä etuoikeutettua tuloa ei voida kohdentaa tietyille ryhmille. Riippuen toimeentulotukiasiakkaan olosuhteista, uudistuksen positiiviset vaikutukset voivat kuitenkin olla estimoituja suuremmat.

Asiasanat: Toimeentulotuki, etuoikeutettu tulo, tulonsiirtojärjestelmä, kannustavuus

# 1 Introduction

Making work pay policies have been introduced to improve financial incentives to accept work and alleviate poverty. These aims are vital within social transfer systems that impose high marginal taxes on low-income individuals. While in-work benefits are often implemented through tax credits, earnings disregards function within a social transfer system<sup>1</sup>. They imply that benefits are withdrawn less than in a one-for-one ratio when a recipient starts to earn income.

In 2002, a three-year experiment – nowadays a permanent policy – was introduced allowing a monthly earned income disregard up to €100. In 2005, the maximum amount was increased to €150. Before the reform, social assistance was reduced one-for-one when a recipient started to earn income. The reform is effectively equivalent to reduced tax rates, consequently leading to an increase in the effective wage rate. Standard economics theory would predict a higher labour supply for low-income individuals.

The purpose of this paper is to evaluate the Finnish earnings disregard reform using a quasi-experimental design. This evaluation is based on high-quality individual-level administrative data that covers the years 1995-2007. While many of the in-work benefit programs are targeted at specific demographic groups such as working families or single mothers, the Finnish experiment was targeted at all social assistance clients without additional eligibility conditions. Since everyone receiving social assistance was eligible, it is feasible to compare labour supply responses across many demographic groups. The empirical strategy employs a difference-in-differences propensity score matching approach. Means-tested unemployment benefit recipients form a valid control group to evaluate the employments effects in a credible way.

This study is motivated by several factors. First, the maximum €150 monthly income increase can be highly significant for individuals living under the poverty line. Second, last resort-social assistance recipients are a substantial and policy relevant group for its high rate of unemployment and social exclusion. For example in 2002, when the policy was introduced, 8.3% of the population received last resort social assistance. Last, quasi-experimental labour supply evidence from tax or benefit reforms in particular related to making work pay policies is very limited in Nordic countries<sup>2</sup>.

Results estimated using both matching and regression techniques indicate the earnings disregard reform had positive effects on earnings. It allowed social assistance clients to earn a small temporary income to supplement their disposable income. The results are group dependent. After a behavioural adjustment period, there are statistically significant results for females, single-person households and individuals with earned income.

This paper is organized in the following way. The next section introduces related literature and contributions. The third section explores the social security system in Finland and provides details on the reform. The fourth section describes the empirical

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<sup>1</sup>A substantial body of literature has studied labour supply effects of in-work benefits and related policies. See for example Immervoll and Pearson (2009) and Card et al. (2010)

<sup>2</sup>Edmark et al. (2012) evaluate the Swedish earned income tax credit, but they conclude that the reform cannot be evaluated using quasi-experimental design. Bastani et al. (2016) estimate participation responses using a housing allowance reform in Sweden.

strategy and the data. The fifth section provides the results and discusses the validity of the estimations, and the last section concludes.

## 2 Related Literature

International in-work benefits are widely used and researched. More than half of the OECD countries have implemented an in-work benefit (Immervoll & Pearson, 2009). Most of the research has focused on the earned income tax credit (EITC) in the United States and its close counterpart in the United Kingdom. The EITC is a refundable tax credit for low-income families with qualifying children. A broad consensus is that the EITC has increased the labour supply at the extensive margin but not at the intensive margin (Eissa & Liebman, 1996; Eissa & Hoynes, 2004; Hotz & Scholz, 2006). For example, Nichols and Rothstein (2015) review the literature on the EITC.

The British Working Families' Tax Credit (WFTC) was introduced in 1999. In contrast to the EITC, the British tax credit has a minimum 16 hours of work a week condition and no phase-in region. Francesconi and Van der Klaauw (2007) find a large seven percentage point increase in single mothers' employment rate. Blundell, Brewer, and Shephard (2005) find that the WFTC and related reforms increased single parents' employment by around 3.6 percentage points. Since other reforms were introduced at the same time as the WFTC, several other studies use a structural model. For example, Brewer et al. (2007) find that the reform increased the labour supply of single mothers by around 5.1 percentage points.

The EITC and its British counterpart work through the tax system. Some studies have found substantial behavioural effects on labour supply for welfare recipients. The Canadian Self-Sufficiency Project was designed to provide evidence of the effects of a generous financial incentive on long-term welfare recipients. One third of the single-parent welfare recipients began to work full-time (at least 30 hours a week), but the temporary program did not have a lasting effect on wages or receiving welfare (Michalopoulos, Robins, & Card, 2005). Lemieux and Milligan (2008) provide labour supply evidence from a negative incentive effect. In Quebec, social assistance recipients under the age of 30 without children received benefits 60% lower than the recipients older than 30. Using regression discontinuity, the authors find that the employment rate dropped from three to five percentage points after the increase in social assistance payments.

Others have studied income disregard policies implemented through the social transfer system. Knoef and Van Ours (2016) study an earnings disregard experiment for single mothers in Holland. In the Dutch experiment, single mothers were allowed to earn €4 per hour up to €120 in a month without having it deducted from their welfare benefits. They find a positive employment effect for immigrants but a small effect for native single mothers. Matsudaira and Blank (2014) evaluate changes in earnings disregards for U.S welfare recipients following a welfare reform in 1996. Although some states introduced large earnings disregards, they find little evidence on increased labour supply because only few women used the earning disregards. These results imply that the labour supply effect may be a different depending on whether the in-work benefit is

implemented through the tax or social transfer system.

This paper contributes to the earlier literature in two ways. First, since everyone receiving social assistance was eligible, labour supply responses can be compared across many demographic groups. Previous literature has typically focused on narrow demographic groups, such as single mothers or families with dependent children. Second, the literature is mainly focused on the USA and the UK with relatively low benefits and high incentives. Finland is representative of a Nordic country with low incentives and high benefits.

## 3 Background

### 3.1 Social Assistance in Finland

According to the constitution of Finland, everyone is entitled to basic income and care necessary for a dignified life. Social assistance is meant to provide this last-resort minimum level of income. It is means-tested at the family level and generally granted on a monthly bases. Social assistance is meant to be temporary and secondary in the sense that it comes on the top of other primary benefits such as housing allowance and labour market subsidy. However, primary benefits have become increasingly insufficient to cover individuals' and families' living expenses causing overlap with last-resort social assistance.

A deep recession in the beginning of the 1990s increased the number of social assistance clients. The share of individuals receiving social assistance nearly doubled from 6.3% to 11.9% between 1990 and 1996. Both poverty, at wide range of measures, and inequality rose after the recession (Riihelä, 2009). After 1996 the share of individuals receiving social assistance started to decline until the financial crisis in 2008. However, long-term dependency on social assistance has increased and average length of social assistance reached six months in 2010 (Kuivalainen, 2013, 40). In an effort to decrease the number of people receiving social assistance and long-term unemployment, activation policy emphasizing individual responsibility has become the guiding policy - the earnings disregard reform being one example.

### 3.2 Eligibility for Social Assistance

All individuals living in Finland are entitled to receive social assistance. Eligibility and entitlement amounts can be described by a simple formula:

$$SA = \max[0; (B + A + H) - (Y)], \quad (1)$$

where  $B$  describes the basic part of social assistance. The basic part is meant to cover food, clothing, phone, transportation, TV, internet, basic health and small costs for hobbies and leisure. In 2017, this minimum level of basic income was €487.89 a month for an individual who lives alone. The basic part is a function of household composition.  $H$  describes necessary housing expenses and covers, for example, acceptable rent, electricity and heating.  $A$  describes discretionary expenses that can be covered with supplementary

and preventive social assistance. They are meant to support social assistance clients' independent living. Supplementary social assistance covers extraordinary expenses, such as sudden housing costs or expenses related to parenting. Preventive social assistance can be granted to ease sudden adverse changes in finances.

$Y$  describes family members' summed earned income and primary benefits.  $Y$  includes earned income and assets that are easily liquidated and not necessary for basic living or work.  $Y$  also includes primary benefits, such as child benefit, labour market subsidy and housing allowance. The labour market subsidy and the housing allowance are means-tested<sup>3</sup>. Because multiple benefits are means-tested and extra benefits can be collected back at a later stage, it is often difficult to know how extra earnings affect disposable income creating income uncertainty. If the family members' summed income in equation 1 is smaller than acceptable expenses, an applicant is entitled to social assistance.

### 3.3 Set up of the Experiment

The earnings disregard experiment became effective in April 2002. It started as a three-year experiment but later became a permanent policy. It allows social assistance clients to keep at least 20% of their earned income up to €100 (€150 in 2005) a month without having it deducted from their social assistance payments. The experiment was household specific so that one household was entitled to only one maximum €150 amount disregarded irrespective of the number of earners in a household. This creates relatively a larger incentive effect for small households. The aim of the experiment was to decrease unemployment by providing social assistance clients incentives to take at least temporary or part-time work. Ideally, the goal of the reform can be summarized as a three-stage model (Hiilamo, Karjalainen, Kautto, & Parpo, 2004, 68):

- In the first stage, a social assistance client has no earned income or very little.



- In the second stage, the reform provides incentives for extra income. The new income stays at a level at which the social assistance client is entitled to the disregarded earnings amount but does not lose his or her social assistance.



- In the third stage, the social assistance recipient is attached to the labour market due to higher incentives and has no need or little need for social assistance.

The panel structure of the data allows to study the reform effects both at the second and third stage. Figure 1 shows a stylized budget constraint without the earnings

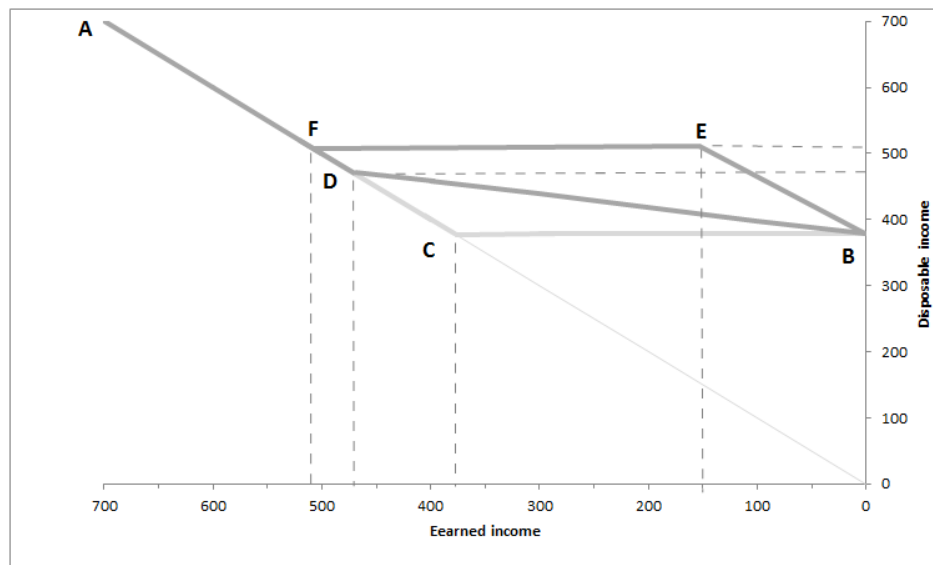
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<sup>3</sup>At the time of the reform, 50% of the earned income was disregarded from labour market subsidy. The housing allowance equals  $0,8 * (\min(\text{max acceptable rent}) - \text{basic deductible})$  where the basic deductible is a function summed family income and household size.

disregard and with the disregard excluding other benefits. The budget constraints are calculated for an individual who lives alone using the basic social assistance amount (€378.54) in 2005<sup>4</sup>. The vertical axis shows disposable income as a function of earned income. The BC line indicates disposable income before the earnings disregard reform. For a social assistance client with a low earning potential, it is not optimal to accept irregular or temporary work. The BD and BEF lines present the budget constraints after the earnings disregard reform is introduced.

The social assistance law allowed municipalities and social workers to decide the disregard percentage between 20% and 100% they applied to earned income (at most €150). The lines BD and BEF present the budget constraints at these extremes. When 20% of the earned income is disregarded, the maximum monthly benefit from the reform is €100, and after earning €478.54, an individual is no longer eligible for social assistance. This indicates a small incentive effect at a very low income. On the CD line there may be some individuals who decrease they work after the reform, but this case seems quite trivial. The line BEF shows the budget constraint at the other extreme when 100% of the earned income is disregarded. Here, it is optimal to work until point E - that is to earn €150. For more than €150 in earnings, the marginal tax rate is 100%. An optimal disregard percentage is found between these two extremes.

Figure 1: Budget constraints before and after the earnings disregard reform



At the time of the experiment, there was no uniform policy on how the earnings disre-

<sup>4</sup>The budget constraints do not take into account interactions from other social transfers and benefits. The social assistance is dependent on the household type and housing costs which generally increase the amount of social assistance.



gard policy was implemented across different municipalities. Likely because of the high volume of social assistance applications and due to cost reasons, in most municipalities the computing systems were set to automatically disregard the minimum 20%. Still, social workers used discretion in applying the disregard percentage<sup>5</sup>. Because it is not known how much was disregarded, this is an intention-to-treat research setting.

## 4 Identification Strategy

### 4.1 Matching Combined with Difference-in-Differences

The goal of this paper is to causally evaluate the average effect of the earnings disregard reform. Let  $D_{it}$  be a binary variable that defines the treatment eligibility status at year  $t$ .  $D = 1$  if social assistance has been received in period  $t + s$ , where  $s \in \{1, 2, 3, \dots, T\}$  indicates the treatment period in years. The outcome variable  $Y$  takes the form of earned income or months worked.

In Rubin’s potential outcome framework, let  $Y_{it+s}^1$  denote earned income or months worked after the earnings disregard is introduced. Also, let  $Y_{it+s}^0$  denote the unobserved counterfactual outcome if person  $i$  had not been assigned to the treatment. The causal effect of the earnings disregard for person  $i$  can be now defined as:

$$Y_{it+s}^1 - Y_{it+s}^0, \quad (2)$$

where the counterfactual term  $Y_{it+s}^0$  is unobservable. Because neither the treated individuals are observable, the estimated average treatment effect is based on the eligibility condition  $D$ . The average causal effect of the income disregard reform is defined as:

$$E\{Y_{t+s}|D_{it} = 1\} - E\{Y_{t+s}|D_{it} = 0\}, \quad (3)$$

which provides a policy relevant outcome because a policy maker cannot assign the treatment to a specific target group. The causal inference hinges on a credible estimation of the counterfactual, which is the term  $E\{Y_{it+s}|D_{it} = 0\}$  in equation 3. This is estimated using labour market subsidy recipients who are a sufficiently similar group in terms of characteristics to social assistance clients. The labour market subsidy is a means-tested unemployment benefit provided by the government. More details on the selection of the treatment group and its estimated counterfactual are provided in the next subsection.

Matching reduces the selection bias generated by the non-random experimental design by using individuals in the treatment and control groups who are sufficiently similar in

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<sup>5</sup>Kuivalainen et al. (2013, 193-195) interviewed 142 social workers in nine municipalities and asked how they applied the earnings disregard. Based on the social workers’ interviews in 2012, 47% usually disregarded 20% of the earned income, and 43% disregarded the maximum amount €150. Ten percent disregarded between these extremes.

their observable characteristics,  $X$ . Rosenbaum and Rubin (1983) proposed how the matching problem can be reduced into one-dimensional propensity score  $p(X)$ . In this study, the propensity score is defined as the conditional probability of receiving social assistance. Compared to conventional regression methods, propensity score matching avoids both extrapolating outside common support and specifying a functional form.

Matching can be combined with a difference-in-differences estimator in both cross-sectional and panel contexts. The panel context tends to yield a different selection because receiving last-resort social assistance over many years implies more serious social marginalization and likely unresponsiveness to the reform. Thus, the focus is on cross-sectional estimation except when exiting social assistance is modelled. As the cross-sectional context is used, a logistic model is used to estimate the propensity score for each year:

$$Pr(SA_{it} = 1) = F(X_{it}), \quad (4)$$

where the vector,  $X$ , contains covariates such as education, household composition, the type of housing and regional characteristics.

Difference-in-differences controls for time-invariant unobservable heterogeneity. Difference-in-differences matching estimator is defined as:

$$\widehat{ATE} = \frac{1}{N_{T_A}} \sum_{i \in T_A \cap S_p} \left[ (Y_{it}^A - Y_{it}^B) - \left( \sum_{j \in C_A \cap S_p} W_{ij} Y_{jt}^A - \sum_{j \in C_B \cap S_p} W_{ij} Y_{jt}^B \right) \right] \quad (5)$$

where the subscripts  $i$  and  $j$  denote an individual belonging to the treatment and control groups respectively. The superscripts  $A$  and  $B$  denote time after and before the reform.  $N_{T_A}$  is the sub-set of individuals in the treatment group after the reform who belong to region of common support,  $S_p$ . Also, let  $T$  and  $C$  denote the set of individuals who belong to the treatment and control groups respectively. The weight  $W_{ij}$  is used to match individual  $j$  to individual  $i$  in the treatment group.

Two different matching methods are used that differ in the weights,  $W_{ij}$ , they use to match individuals in the control group. The nearest neighbour method involves the risk of bad matches if the closest neighbour is far away. Radius caliper matching sets a tolerance level for the propensity score distance and uses all comparison units within the caliper<sup>6</sup>. The kernel-weighted estimator gives the highest weight to the individual with propensity scores closest to the treated individual. Conventional regression methods are also used to supplement the analyses. Different specifications are presented in the results section.

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<sup>6</sup>Literature suggests (e.g. Austin, 2011) using a caliper width of 0.20 or 0.25 of the standard deviation of the propensity score, but estimations here suggest that a smaller caliper yields a better covariance balance. A Caliper width 0.01 is used in all estimations.

## 4.2 Data and Sample Selection

This evaluation uses rich individual-based panel data collected by Statistics of Finland. The data set covers the years from 1995 to 2007, each year containing more than 500 000 observations with a variety of income as well as socio-demographic and regional characteristics. The labour market outcomes are measured as one month worked and earned income containing both wages and entrepreneurial income. At least 15 working days counts for one worked month<sup>7</sup>. Due to data restrictions, months worked don't include entrepreneurs. Only ages 17-64 are included in the analysis to reflect primary working age and individuals who are entitled to labour market subsidy (individuals younger than 17 are not eligible). All variables are measured at the yearly level.

Social assistance recipients are a heterogeneous group. Receiving social assistance can be temporary, or it can become a long-term dependency. Because receiving social assistance can be very temporary, individuals who have received social assistance above the median are used as the treatment group. Similarly, individuals who have received labour market subsidy above the median are used as the control group. Labour market subsidy is a means-tested unemployment benefit paid to job seekers who are not entitled to an earnings-related unemployment insurance. Furthermore, since the reform was meant to improve the incentives of the unemployed and since there is a relatively large share of individuals with substantial income, yearly income earners above €10 000 are not selected. Using pooled data, individuals below the percentile corresponding to €10 000 are selected in the treatment and control groups. Wage indices are added to €10 000 using the year 2001 as the base year. To supplement the analyses the results are also estimated for a higher income group. This group includes individuals with earnings up to €20 000 plus wage indices. Table 1 shows summary statistics for the treatment and control groups.

Based on table 1, there are clear observable differences between the treatment and control groups. Social assistance clients are on average four to five years younger. The treatment group has more single parents and individuals living in a rented apartment. They tend to have lower education but higher earnings. A likely reason for the higher earnings is the median condition. Individuals and households may receive social assistance on temporary basis, but they receive it above the median because of a large household size, for example. Individuals receiving labour market subsidy above median are by definition longer-term unemployed. These raw differences imply that it is important to control for observable characteristics. Still, the composition of the treatment and control groups is fairly stable over time. For example, the proportion of females does not change before and after the reform. The household and education composition stays largely the same as well. Overall, there are no large changes in composition over time. This increases confidence that the reform effect can be identified in a credible way.

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<sup>7</sup>After year 2005, at least 16 working days count as one worked month. A day is counted a work day irrespective of how many hours a person has worked.

Table 1: Descriptive statistics

	Pre-Reform			Post-Reform		
	Treatment	Control	$\Delta$	Treatment	Control	$\Delta$
Earned income	1495	903	592	1446	883	563
Months unemployed	7.3	9.0	-1.7	6.8	8.8	-2.0
Age	35.4	39.7	-4.3	35.0	40.9	-5.5
Female	0.43	0.57	-0.14	0.43	0.57	-0.14
High Education	0.08	0.12	-0.04	0.07	0.13	-0.06
Middle Education	0.41	0.50	-0.09	0.40	0.49	-0.09
Low education	0.52	0.38	0.14	0.53	0.38	0.15
Married	0.25	0.42	-0.17	0.23	0.43	-0.20
Couple without children	0.10	0.20	-0.10	0.10	0.20	-0.10
Couple with children	0.33	0.52	-0.19	0.31	0.52	-0.21
Single parent	0.23	0.13	0.10	0.23	0.13	0.10
Lives alone	0.22	0.08	0.14	0.25	0.09	0.16
Rented apartment	0.76	0.41	0.35	0.80	0.44	0.36
Composition	0.58	0.42	0.16	0.59	0.41	0.18
No. of observations	15 753	11 242		14 395	10 099	

Note: Pre-reform refers to the years 2000 and 2001. Post-reform refers to the years 2003 and 2004. The reported values are averages.

## 5 Results

### 5.1 Results for the Whole Sample

This section begins by presenting the results for all social assistance clients within the selected sample and after that for two subgroups, females and individuals with earned income. The first three columns present the results when the number of months employed are used as the outcome variable, and the last three columns in tables show the results when yearly earnings are used as an outcome variable. In addition to radius and kernel estimations, the tables show the results from OLS estimations with the following specification:

$$Y_{igt} = \alpha_g + \gamma_t + \sum_{\tau=1}^4 \delta_{\tau} D_{g\tau} + \beta X_{igt} + \epsilon_{igt}, \quad (6)$$

where  $\alpha_g$  is a group fixed effect and  $\gamma_t$  is a year fixed effect.  $D_{g\tau}$  is the treatment eligibility variable, and  $X_{igt}$  is a vector of observable individual characteristics. The specification in equation 6 allows the treatment effect to vary across different years after the reform.

Table 2 shows the results of the logit model that is used to estimate the propensity score for the whole sample. Exactly the same specification is used in all estimations. The dependent variable gets the value of one if social assistance has been received in a given year. The covariance balance after radius calliper matching is presented in figure 2. To further improve the matching quality, five percent trimming has been used in all matching estimations.

Table 3 shows the cross-sectional results for all social assistance clients within the selected sample. The OLS estimations indicate a growing but not statistically- significant effect on earned income after the reform was introduced. Neither matching nor OLS estimations show statistically significant effects on the outcome variables. Among the social assistance clients, there are those who are severely socially marginalized with a weak or nonexisting labour market attachment because of long-term health issues, for example. Thus, intent-to-treat estimations are likely to be smaller than the incentive effects on those who were actually treated. It is expected that the results are higher for specific sub-groups, such as individuals with an existing labour market attachment.

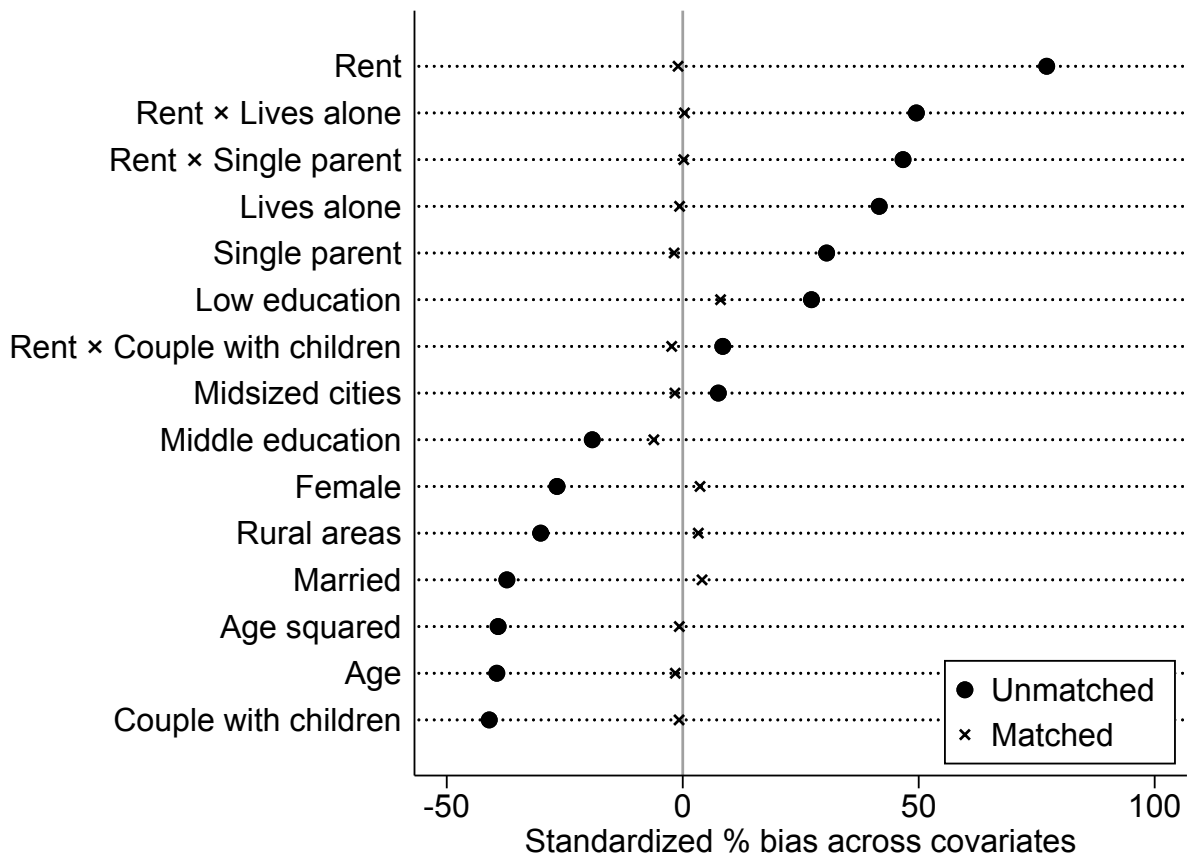
### 5.2 Sub-group Results

Table 4 shows the results for two sub-groups, females and for individuals with positive earnings. Empirical labour supply literature typically finds that women are more responsive to financial incentives than men (e.g. Meghir & Phillips, 2010). The results in table 5 show that there is a growing effect on females' earned income after the reform was introduced. After an adjustment period, kernel and OLS estimations indicate statistically significant, approximately €200 reform effect on earnings for the year 2005. Matching estimations have less power, and radius caliper estimation indicates only a weak statistically significant reform effect. None of the results show effects on months worked.

Table 2: Propensity score for the comparison year 2001

Dependent variable: Probability of receiving social assistance			
Variable	Coefficient	Std.Err.	$P > z$
Age	-0.030	0.012	0.010
Age squared	-0.000	0.000	0.663
Female	-0.898	0.043	0.000
Married	-0.043	0.052	0.399
High education	Ref.		
Middle education	0.088	0.072	0.223
Low education	0.584	0.073	0.000
Couple without children	Ref.		
Couple with children	-0.503	0.075	0.000
Single parent	0.224	0.096	0.000
Lives alone	0.512	0.120	0.000
Rented apartment	0.723	0.078	0.000
Rent×Single parent	1.434	0.130	0.000
Rent×Lives alone	1.079	0.149	0.000
Rent×Couple with children	0.490	0.100	0.000
Regional dummies	Yes		
Number of observations	13 253		
Treatment	7 723		
Control	5 530		
Pseudo R-square	0.197		
LR $\chi^2(15)$	3546.44		

Figure 2: Covariance balance for the comparison year 2001



Note: Mean bias before matching is 34.8 and after matching 2.4.  
 Median bias before matching is 37.3 and after matching 1.7.

Table 3: Cross-sectional difference-in-differences results

Outcome: Method:	Months employed			Earned income		
	Radius	Kernel	OLS	Radius	Kernel	OLS
t=1						
ATE	0.2	0.2	0.0	64	59	42
Standard error	(0.18)	(0.19)	(0.14)	(61.41)	(61.58)	(48.50)
Observations	9 032	9 032	21 735	25 213	25 213	61 948
t=2						
ATE	0.1	0.1	0.0	81	84	59
Standard error	(0.19)	(0.10)	(0.15)	(60.98)	(65.46)	(49.74)
Observations	8 897	8 897	21 735	24 966	24 966	61 948
t=3						
ATE	-0.1	-0.2	-0.2	94	85	72
Standard error	(0.18)	(0.20)	(0.15)	(59.00)	(59.25)	(58.44)
Observations	8 617	8 617	21 735	24 547	24 547	61 948
t=4						
ATE	0.1	0.1	0.1	56	52	79
Standard error	(0.18)	(0.21)	(0.15)	(68.66)	(67.01)	(52.00)
Observations	8 618	8 618	21 735	11 153	11 153	61 948

Note:

- (1) \*Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.
- (2) t refers to the post-reform time in years (t=1 refers to the year 2002).
- (3) Year 2001 is used as a comparison year.
- (4) Standard errors are bootstrapped.
- (5) Age, age squared, female, married low education, middle education, couple with children, single parent, lives alone, rent and interactions with f rent and household types and regional dummies were used as control variables in all estimations.

Individuals with positive earnings are the most likely beneficiaries of the reform. When only these individuals are included, the reform effect is larger than it is in previous estimations. For the year 2005, the estimated earned income effect is €299 from the OLS estimation. The scope of the results is similar in the radius and kernel estimations. Again, the results indicate no effect on employment duration when measured as months employed. Similar to the estimations for females, all estimations indicate a growing effect on earned income in each post-reform year. This can indicate that there was a behavioural adjustment period. Based on social workers' interviews, social assistance clients were not fully informed about the experiment at the beginning (Hiilamo et al. 2004, 142-143). In addition, social workers may not have fully utilized the earnings disregard policy as a social work tool at the beginning of the experiment.



Table 4: Sub-group Results

Outcome: Method:	Months employed			Earned income		
	Radius	Kernel	OLS	Radius	Kernel	OLS
<u>Females</u>						
t=1						
ATE	0.0	-0.1	-0.2	50	65	31
Standard error	(0.28)	(0.28)	(0.20)	(100.47)	(99.03)	(70.02)
Observations	4 603	4 603	11 026	12 435	12 435	30 401
t=2						
ATE	-0.1	-0.1	-0.1	84	85	47
Standard error	(0.26)	(0.25)	(0.21)	(98.13)	(97.84)	(71.47)
Observations	4 599	4 599	11 026	12 431	12 431	30 401
t=3						
ATE	0.1	0.1	0.0	140	159	93
Standard error	(0.33)	(0.32)	(0.21)	(91.63)	(87.63)*	(73.23)
Observations	4 440	4 440	11 026	12 094	12 094	30 401
t=4						
ATE	0.1	0.1	0.3	205	227	179
Standard error	(0.33)	(0.32)	(0.21)	(112.98)*	(106.23)**	(76.50)**
Observations	4 445	4 445	11 026	11 870	11 870	30 401
<u>Earned income&gt;0</u>						
t=1						
ATE	0.1	0.1	0.0	90	96	61
Standard error	(0.18)	(0.17)	(0.14)	(100.87)	(99.93)	(82.88)
Observations	8 731	8 731	21 031	10 489	10 489	24 880
t=2						
ATE	0.2	0.2	0.0	155	166	141
Standard error	(0.20)	(0.20)	(0.14)	(110.41)	(110.43)	(86.25)
Observations	8 614	8 614	21 031	10 265	10 265	24 880
t=3						
ATE	-0.2	-0.2	-0.1	218	219	203
Standard error	(0.20)	(0.19)	(0.15)	(117.06)*	(111.21)*	(89.54)**
Observations	8 321	8 321	21 031	9 912	9 912	24 880
t=4						
ATE	0.0	0.0	0.1	277	266	299
Standard error	(0.17)	(0.18)	(0.14)	(108.66)**	(111.24)**	(92.43)***
Observations	8 321	8 321	21 031	9 730	9 730	24 880

Note:

- (1) \*Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.
- (2) t refers to the post-reform time in years (t=1 refers to the year 2002).
- (3) Year 2001 is used as a comparison year.
- (4) Standard errors are bootstrapped.
- (5) Age, age squared, female, married low education, middle education, couple with children, single parent, lives alone, rent and interactions with rent and household types and regional dummies were used as control variables in all estimations.

Sub-group results for males and different household types are shown in table 5. Since the common trend assumption may not hold for all household types, a linear trend is added to the estimations. In addition, because certain household types, such as single parents are more likely to receive social assistance than labour market subsidy, there are fewer matches available. For these reasons, only the OLS results are presented in table 5. There are no statistically significant results for males or other household types except for single-person households. For couples without children and single parents, standards errors are larger for the reasons mentioned above. The estimated reform effect for single-person households is €250 after an adjustment period for the year 2005. This result is expected as single-person households can benefit more from the earnings disregard reform since it was household specific. In addition, single-person households are better off because extra earnings do not need to be shared within the household.

Table 5: Earned income for males and different household types

Sub-group:	Male	Couple without children	Couple with children	Single parents	Lives alone
t=1					
ATE	30	199	15	-50	81
Standard error	(54.36)	(131.44)	(75.89)	(95.36)	(78.92)
Observations	31 547	8 657	24 586	8 279	10 931
t=2					
ATE	44	275	105	151	105
Standard error	(58.36)	(134.55)**	(79.56)	(113.35)	(88.48)
Observations	31 547	8 657	24 586	8 279	10 931
t=3					
ATE	-23	109	99	75	179
Standard error	(62.54)	(138.87)	(85.26)	(132.46)	(102.18)*
Observations	31 547	8 657	24 586	8 279	10 931
t=4					
ATE	-30	222	159	238	250
Standard error	(68.84)	(148.66)	(91.76)*	(156.66)	(118.53)**
Observations	31 547	8 657	24 586	8 279	10 931

Note:

(1) \*Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

(2) t refers to the post-reform time in years (t=1 refers to the year 2002).

(3) Year 2001 is used as a comparison year.

(4) Age, age squared, female, married, low education, middle education, couple with children, single parent, lives alone, rent and interactions with of rent and household types and regional dummies were used as control variables.

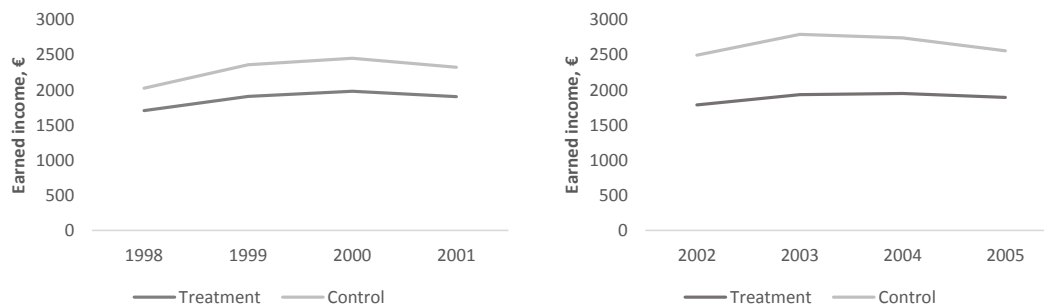
### 5.3 Panel Estimation Results

Previous models did not allow exits from social assistance. It can be hypothesized that at the first stage there is an increased incentive to accept temporary or part-time work. At a later stage, individuals are more attached to the labour market and leave social assistance. To test this hypothesis individuals are followed in two panels before and after the reform. Each individual receives social assistance or labour market subsidy in the first year allowing exits in the following time period. The hypothesis is tested with the following model:

$$\Delta Y_{igt} = \alpha_g + \gamma + \delta D_g + \beta X_{igt} + \epsilon_{igt}, \quad (7)$$

where  $\gamma$  and  $D_g$  are dummy variables for the post-reform period. The median condition is not used here because the common trends assumption appears to hold better with a positive amount of social assistance. A similar condition is used for the control group. As expected, without the median condition the control group has higher earnings now (see figure 3).

Figure 3: Earned income in the panel before and after the reform



The results for all social assistance clients are shown in table 6. The results are largest for the year 2005 but only weakly statistically significant in the matching estimations. The results from the OLS estimation indicates no statistically significant effects. The sub-group results for females and for individuals with earned income tend to be larger, but the results are not statistically significant. These results indicate no transition from social assistance to longer-term employment.

Table 6: Panel estimation results

Outcome: Method:	$\Delta$ Months employed			$\Delta$ Earned income		
	Radius	Kernel	OLS	Radius	Kernel	OLS
t=1						
ATE	0.1	0.1	0.0	24	37	-57
Standard error	(0.18)	(0.17)	(0.14)	(86.66)	(85.84)	(62.87)
Observations	14 597	14 597	14 917	45 803	45 803	47 007
t=2						
ATE	0.3	0.3	0.1	28	14	-68
Standard error	(0.26)	(0.24)	(0.18)	(92.41)	(89.11)	(69.55)
Observations	10 609	10 609	11 790	44 083	44 083	45 238
t=3						
ATE	0.0	0.0	-0.1	195	179	117
Standard error	(0.22)	(0.21)	(0.16)	(103.60)*	(102.43)*	(73.38)
Observations	17 477	17 477	17 887	42 618	42 618	43 711

Note:

(1) \*Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

(2) t=1 compares the panels 2000-2001 and 2002-2003; t=2 compares the panels 1999-2001 and 2002-2004; t=3 compares the panels 1998-2001 and 2002-2005.

(4) Standard errors are bootstrapped.

(5) Age, age squared, female, marriage, low education, middle education, couple with children, single parent, lives alone, rent and interactions with of rent and household type and regional dummies were used as control variables in all estimations.

## 5.4 Other Specifications

To further study the reform effect two other specifications are modelled – higher income and extensive margin. Table A1 in appendix shows the results for the higher income group. This group includes individuals whose earned yearly income is less than €20 000 plus wage indices using the year 2001 as the base year. The results are smaller and not statistically significant except for individuals who live alone. For females and individuals with positive earnings, there is a growing effect on earned income over time, but the reform effect is non-significant and smaller than in the lower income group.

One reason for this result is that the individuals in the higher income group are more likely to leave social assistance once they start working. The smaller income group is more dependent on social assistance and can potentially benefit more from the reform. Furthermore, micro-simulated budget constraints for example households reveal that the reform did not remove the income trap but moved it to a higher income level implying that the incentive effect is lower at a higher income level (Parpo, 2006). For example, single parents are in an income trap with earned income between 900-1200 euros (Parpo 2006, 49). These income traps are formed because the earnings disregard ends at €150 (with 20% disregarded) and because of interactions effects from other benefits and taxes.

In order to study the effects at the extensive margin, the following linear probability model is estimated:

$$P(Work)_{igt} = \alpha_g + \gamma_t + \sum_{\tau=1}^4 \delta_{\tau} D_{g\tau} + \beta X_{igt} + \epsilon_{igt}, \quad (8)$$

where  $P(Work)$  is defined to take a value of one if an individual has earnings and otherwise 0. The results are shown in table 2A in appendix. None of the sub-groups that were responsive to the reform in previous estimations show significant results at the extensive margin. No effects at the extensive margin imply that some attachment to the labour market is required. Note that this intensive margin result with individuals with positive income does not imply that some individuals were working on the BD-line in figure 2, when the marginal tax rate was 100%. This is because yearly data is used and it is likely that these individuals were not social assistance clients when they worked<sup>8</sup>.

## 5.5 Threats to Identification

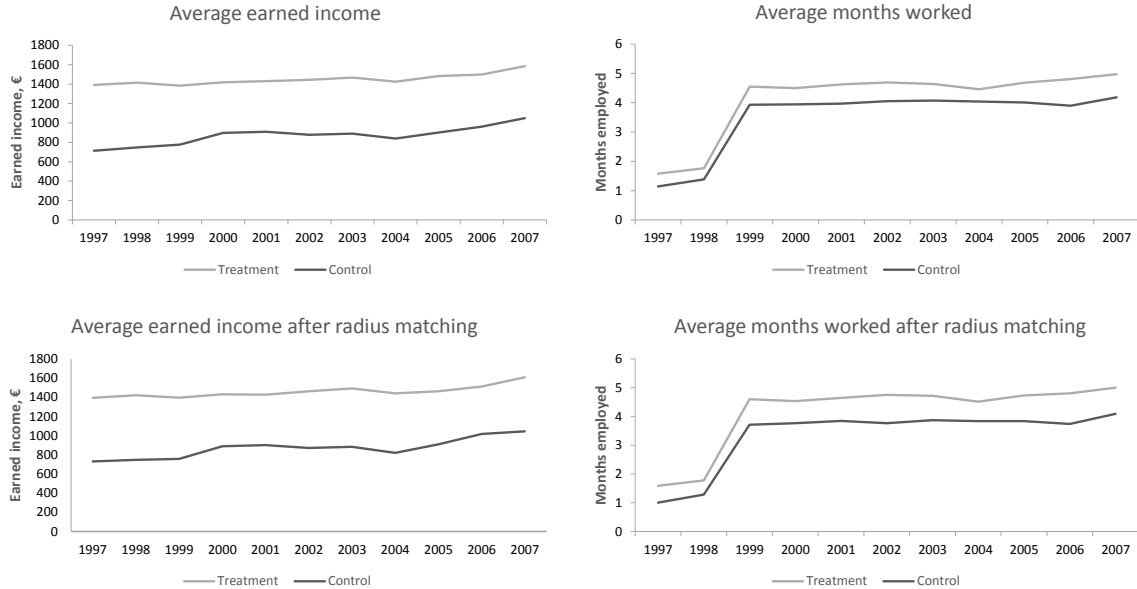
The main identifying assumption is the common trends. Figure 4 show the common trends before selection on observables and after balancing the observables. The graphs indicate that the common trends assumption seems to hold. The treatment and control groups respond to economic shocks in a similar way. In addition, the graphs indicate only minor changes after propensity score matching. This supports the identification as

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<sup>8</sup>In addition, a negligible amount of earnings and gifts was already disregarded before and after the reform. This amount was generally €50, but the practice varied across municipalities.

it implies that observable differences do not significantly affect the results. Both radius caliper and kernel matching yield similar graphs.

Figure 4: Common trends



The stable unit treatment value assumption (SUTVA) implies no spillover effects and stable composition over time. At the individual level, spill over effects cannot be fully ruled out<sup>9</sup>. If a social assistance client becomes employed because of the reform, the spouse receiving labour market subsidy would have fewer financial incentives to find work if income is shared within the household. This tends to increase the reform effect. The reform effect could vary depending on the selection of the control group and its institutional context. Still, the reform effect is fairly modest and focused on temporary work implying that the earnings disregard reform does not significantly affect a spouse's incentives.

Based on table 1, the composition remains fairly stable across demographic groups and over the treatment and control groups. Anticipation effects may also affect composition, but the effects are likely to be negligible. Bargain, Immervoll, and Viitamäki (2012) studied the non-take-up rate of social assistance benefits using Finnish data and found a substantial non-take-up rate, between 40% and 50%. Given the complexity of the Finnish social security system, it seems unlikely that these individuals are rational calculators who try to benefit the maximum amount from social transfers. Finally, there may be confounding factors from other reforms. No other major reform was introduced after the earnings disregard reform became effective (see table A3 in appendix).

<sup>9</sup>The estimations could be done at household level, but this would significantly reduce the sample size and limit sub-group analyses.

There are also other pieces of evidence that indicate relatively clean identification. First, labour supply literature typically finds that females are more responsive to financial incentives than males. Second, social assistance clients with positive earnings are the most likely beneficiaries of the reform. Similarly, it is expected that single-person households are likely beneficiaries of the reform. The results indicate the largest effects for these groups. Finally, there is timing evidence. The results are the largest and most statistically significant for the year 2005 when the maximum disregarded amount was increased to €150.

## 6 Conclusions

This paper examines the employment effects of the Finnish earnings disregard reform for the years 2002-2005. After a behavioural adjustment period, there are statistically significant results for females, single-person households and individuals with earned income. The implied elasticities are 0.11, 0.14 and 0.16 respectively using the maximum €150 monthly earnings disregard. These elasticities are likely to underestimate the true effect because many municipalities disregarded 20% of the earned income.

The earnings disregard policy allowed social assistance clients to earn small temporal income to supplement their disposable income. Still, there are few changes in the labour market status. No effects on the extensive margin imply that a behavioural response requires some attachment to the labour market. In addition, the panel estimations show no transitions from social assistance to long-term employment. Although the incentive effect is fairly modest, the earnings disregard has many positive aspects.

The earnings disregard reform unambiguously improved social assistance clients' situation with limited fiscal implications. Before the reform social assistance was effectively reduced in one-to-one ratio after a recipient started to earn income. Depending on individual circumstances, the incentive effect on the treated can be larger than the intention-to-treat estimations show. However, from a policy perspective there are factors that weaken the effectiveness of the reform. Applying the earnings disregard at the individual level instead of the household level would likely give a higher incentive effect. Also, the rules for applying the earnings disregard varied across municipalities. Simple rules should be applied to earnings disregards so that it is easy to perceive how taking up work affects disposable income. Interaction effects from other means-tested benefits add complexity to the social security system making it more difficult to perceive how temporary work affects disposable income.

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## Appendix A

Table A1: Difference-in-differences results for the higher income group (€20 000)

Group: Outcome:	Whole sample		Female		Positive earnings		Lives alone	
	Months	Income	Months	Income	Months	Income	Months	Income
t=1								
ATE	0.1	-19	0.1	18	0.1	-52	0.1	170
Standard error	(0.09)	(67.66)	(0.14)	(98.00)	(0.09)	(112.23)	(0.21)	(115.81)
Observations	26 816	67 179	13 361	32 774	26 112	30 111	3 990	11 419
t=2								
ATE	0.0	50	0.1	66	0.04	142	0.0	291
Standard error	(0.10)	(71.04)	(0.17)	(102.02)	(0.10)	(119.24)	(0.26)	(129.49)**
Observations	26 816	67 179	13 361	32 774	26 112	30 111	3 990	11 419
t=3								
ATE	-0.2	11	0.1	98	-0.2	150	-0.2	315
Standard error	(0.12)*	(75.24)	(0.17)	(109.25)	(0.11)	(128.19)	(0.32)	(143.75)**
Observations	26 816	67 179	13 361	32 774	26 112	30 111	3 990	11 419
t=4								
ATE	0.0	-4	0.3	134	0.01	192	0.0	297
Standard error	0.13	(80.33)	0.19)	(113.94)	(0.11)	(138.19)	(0.38)	(163.56)*
Observations	26 816	67 179	13 361	32 774	26 112	30 111	3 990	11 419

Note:

(1) \*Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

(2) t refers to the post-reform time in years (t=1 refers to the year 2002).

(3) Year 2001 is used as a comparison year.

(4) Age, age squared, female, married, low education, middle education, couple with children, single parent, alone living, rent and interactions with rent and household types and regional dummies were as control variables in all estimations.

Table A2: Results for the extensive margin

Group:	Whole sample	Female	Male	Couple without children	Couple with children	Single parent	Lives alone
t=1							
ATE	0.01	0.02	0.00	0.05	0.01	0.01	-0.02
Standard error	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)
Observations	61 948	30 401	31 547	8 657	24 586	11 592	10 931
t=2							
ATE	0.01	0.00	0.01	0.06	0.01	0.00	0.00
Standard error	(0.01)	(0.02)	(0.02)	(0.03)**	(0.02)	(0.03)	(0.04)
Observations	61 948	30 401	31 547	8 657	24 586	11 592	10 931
t=3							
ATE	0.01	0.01	0.01	0.04	0.02	-0.03	0.03
Standard error	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)
Observations	61 948	30 401	31 547	8 657	24 586	11 592	10 931
t=4							
ATE	0.00	0.01	-0.01	0.04	0.02	-0.02	0.01
Standard error	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)
Observations	61 948	30 401	31 547	8 657	24 586	11 592	10 931

Note:

(1) \*Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

(2) t refers to the post-reform time in years (t=1 refers. to the year 2002).

(3) Year 2001 is used as a comparison year.

(4) Age, age squared, female, married, low education, middle education, couple with children, single parent, lives alone, rent and interactions with rent and household types and regional dummies were used as control variables in all estimations.

Table A3: Main reforms between 1995 and 2007

Year	Social assistance	Labour market subsidy
1996	Possibility to cut social assistance 20% for refusing to participate in active labour market programs	Under 20 years old is not eligible without secondary education or applying for it
1996		Under 25 years old is not eligible without secondary education or applying for it
1997		Individual employment plans and personal interviews
1998		
1999	Possibility to cut immigrants' social assistance for refusing to accept an integration plan	
2001	Act on Rehabilitative Work Activity introduced rehabilitating measures and sanctions for the long-term unemployed	Act on Rehabilitative Work Activity introduced rehabilitating measures and sanctions for the long-term unemployed
2001	Applications handled immediately	
2001	Responsibility to register with an unemployment office	
2001	Supplementary and preventive social assistance	
2002	Earned income disregard reform	Reinforcing individual employment plans
2005	Maximum disregarded income increased to €150	
2006	7% own risk in housing expenses removed	Municipalities are responsible for half the benefit costs of the long-term unemployed accompanied by new sanctions
2007		