



Incentivizing last-resort social assistance clients: Evidence from a Finnish policy experiment

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Abstract

In 2002, the Finnish government introduced an earnings disregard experiment aimed at improving the incentives of low-income individuals who receive last-resort social assistance. The aim of the experiment was to reduce 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 experiment as an event study using coarsened exact matching (CEM) and difference-in-differences. On average, the results show no employment effects, but there is some evidence of positive employment effects on women.

Keywords Difference-in-differences · Making work pay · Earnings disregard · Welfare

JEL Classification C93 · H53 · I38 · J68

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. They imply that benefits are withdrawn less than in a one-for-one ratio when a recipient starts to earn income.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 in Finland allowing for a monthly earned income disregard up to €100 for social assistance recipients. 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,

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consequently leading to an increase in the effective wage rate. Standard economics theory would predict a higher labour supply for low-income individuals as a result of the reform.

The purpose of this paper is to evaluate the Finnish earnings disregard experiment using a quasi-experimental design. This evaluation is based on high-quality individual-level register data that cover 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. The earnings disregard is evaluated as an event study. A difference-in-differences model is combined with coarsened exact matching and individual fixed effects. The control group is formed of means-tested labour market subsidy recipients. Labour market subsidy is meant for unemployed persons who enter the labour market for the first time or who have not worked long enough so that they are not entitled to earnings-related unemployment insurance. On average, the results show no employment effects, but there is some evidence of women's positive employment response to the earnings disregard.

This study is motivated by several factors. First, the maximum €150 monthly income increase can be significant for individuals living under the poverty line. Second, last resort-social assistance recipients are a substantial and policy-relevant group for their 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 related to making work pay policies is limited in Nordic countries¹.

This paper is organized in the following way. The next section introduces related literature and contributions. The third section describes the social security system in Finland and provides details on the experiment. The fourth section describes the empirical strategy and the data. The fifth section provides the results and discusses the sensitivity of the estimations, and the last section concludes.

2 Related literature

Internationally 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 USA and its close counterpart in the UK. The EITC is a refundable tax credit for low-income families with qualifying children. Several studies have found the EITC increased the labour supply at the extensive margin but not at the intensive margin (Eissa & Liebman, 1996; Eissa & Hoynes, 2004; Hotz & Scholz, 2006). For

¹ To our knowledge the effects of in-work benefits or earnings disregards on social assistance clients have not been studied in a Nordic country before. Edmark et al. (2016) evaluated the Swedish earned income tax credit, but they conclude that the reform cannot be evaluated using a quasi-experimental design. Related to the same EITC scheme in Sweden, Laun (2017) utilised a larger EITC for older workers, above the age of 65. There are also some studies on supplementary UI benefits. Kyyrä et al. (2013) studied a supplementary UI benefit in Denmark, and Kyyrä (2010) studied a similar scheme in Finland.

example, Nichols and Rothstein (2015) review the literature on the EITC. Recently, Kleven (2019) has questioned the consensus related to the effectiveness of the EITC reform. His estimations imply that the earlier results were driven by confounding effects of welfare reform and a booming macroeconomy. 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) found a large seven percentage point increase in single mothers' employment rate. Blundell et al. (2005) found that the WFTC and related reforms increased single parents' employment by around 3.6% 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. (2005) found 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 et al., 2005). Lemieux and Milligan (2008) provided labour supply evidence from a substantial incentive change in social assistance. In Quebec, social assistance recipients under the age of 30 without children received benefits 60% lower than the recipients older than 30. Using a regression discontinuity design, the authors found 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) studied 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 per month without having it deducted from their welfare benefits. Using a triple difference-in-differences approach, they found a positive employment effect for immigrants but a small effect for native single mothers. Matsudaira and Blank (2014) evaluated changes in earnings disregards for US welfare recipients following a welfare reform in 1996. Although some states introduced large earnings disregards, they found little evidence on increased labour supply because only few women used the earning disregards. These results imply that the labour supply effect may be 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. The 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. Nordic countries tend to have a high rate of social spending but a higher rate of unemployment linked to a different institutional setting.

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 basis. Social assistance is meant to be temporary and secondary in the sense that it comes on 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 at 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 a 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 the average length of social assistance reached six months in 2010 (Kauppinen et al., 2013, p. 40). In an effort to reduce 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, Internet, basic health and small costs for hobbies and leisure. In 2021, this minimum level of basic income was €504.06 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 benefits, labour market subsidy and housing allowance. The labour market subsidy and housing

allowance are means-tested. 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 became a permanent policy in 2014. It allowed for social assistance clients to keep at least 20% of their earned income up to €100 (€150 as of 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 experiment can be summarized as a three-stage model (Hiilamo et al., 2004, p. 68):

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

Figure 1 shows a stylized budget constraint without the earnings 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 2005.² The social assistance amount depends on household size and individual conditions, and the budget constraints differ accordingly. The vertical axis shows disposable income as a function of earned income. The BC line indicates disposable income before the earnings disregard experiment.³ For a social assistance client with a low earning potential, it is not optimal to accept irregular or temporary work. The BDA and BEFA lines present the budget constraints after the earnings disregard experiment 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 BDA and BEFA present the budget constraints at these extremes. When 20% of the earned income is disregarded, the maximum monthly benefit from

² 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.

³ A negligible amount of earnings and gifts was already disregarded before and after the experiment. This amount was generally €50, but the practice varied across municipalities.

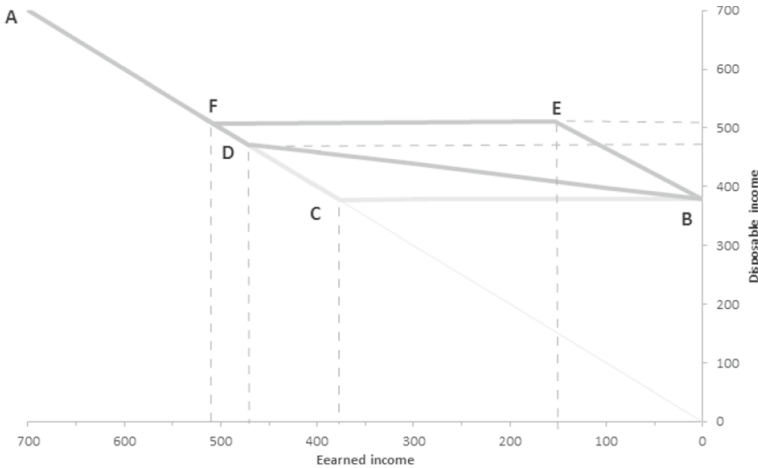


Fig. 1 Budget constraints for single persons before and after the earnings disregard experiment. *Note:* the lines *BCA* show the old policy. The lines *BDA* show the budget constraint when 20% is disregarded. The lines *BEFA* show the budget constraint with €150 disregarded

the experiment 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 may reduce their labour supply after the experiment, but this case seems quite trivial. The line *BEFA* 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%.

At the time of the experiment, there was no uniform policy on how the earnings disregard 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.⁴ Because it is not known how much was disregarded, this is an intention-to-treat research setting.

⁴ Karjalainen et al. (2013, pp. 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 per cent of the social workers disregarded between these extremes.

4 Empirical strategy

4.1 Difference-in-differences with coarsened exact matching

The aim of this paper is to causally evaluate the employment effects of the earnings disregard experiment. Observable differing characteristics between the treatment and control groups are balanced using coarsened exact matching (Iacus et al., 2012). CEM coarsens the selected variables into strata and performs exact matching on the coarsened data. The CEM algorithm allows for decreasing imbalance in any variable without increasing imbalance in any other variables. This monotonic imbalance property reduces model dependence accounting for interactions and nonlinearities.

An individual fixed effects model is estimated with the following specification:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{\substack{s = -7 \\ s \neq -1}}^5 \beta \times \mathbb{1}[t = s] \times D_i + \epsilon_{it}, \quad (2)$$

where α_i and γ are individual and year fixed effects, respectively, and i denotes an individual. The main identifying assumption is the parallel trends assumption, that is the treatment and control groups would have had parallel trends in the absence of treatment. The main outcome variable Y_{it} is yearly earnings. This is the most accurate employment measure in the data, and it covers all years between 1995–2007. The model is also estimated using work months as an outcome. This variable includes the years 1997–2007 and does not contain entrepreneurs. Yearly earnings also contain entrepreneurial income. The treatment variable D is defined solely on a pretreatment period, that is, in 2001 (-1). This is because the earnings disregard increased eligibility for social assistance, and there is likely some inflow to social assistance in the post-treatment period. Period-1 is used as a reference category. The regressions are estimated with CEM-weights, and the standard errors are clustered at the individual level.

All variables are measured at the individual level. Although social assistance is means-tested at the family level, it is granted to the applicant. The earnings disregard was household-specific so that one household was entitled to a maximum €150 earnings disregard. In the means-testing, the social assistance applicant's earnings and spouse's earnings were disregarded up to €150. Because social assistance is

means-tested at the household level and the experiment changed spouses' incentives, the results are also estimated without social assistance clients' spouses who belong to the control group.

The labour market subsidy recipients form a similar group to social assistance recipients. The labour market subsidy is a means-tested benefit provided by the government. It is meant for unemployed persons who enter the labour market for the first time or who have not worked long enough so that they are not entitled to an earnings related unemployment insurance. The labour market subsidy is paid on weekdays only, and the paid amount was €33.78 a day in 2021. There is no duration limit in the labour market subsidy. As a robustness check, the results are also estimated using only home-owning labour market subsidy recipients as a control group. This group does not receive social assistance for predominately exogenous reasons. Home-owners are entitled to social assistance, but their earnings are higher on average, and they have more often a spouse to support their economic well-being.

4.2 Data and sample selection

This evaluation uses rich individual-based panel data collected by Statistics Finland. The register-based data cover 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 observations form a representative sample of approximately 10% of the Finnish population. Ages 18–64 are included to reflect primary working age. All variables are measured on a yearly level. Register-based data sets from the experiment time do not contain monthly earnings that could be linked to monthly social assistance. Using yearly measures likely adds some inaccuracy to the results. Since social assistance is granted monthly, monthly data would capture the experiment effect more precisely. Monthly data would capture a treatment effect on those social assistance clients who did not receive it in 2001, but they received it for some month(s) before the experiment in 2002. The benefit of yearly measures is that the control group is to a larger extent formed of individuals and families that were not eligible or did not apply for social assistance.

Table 1 shows how many months social assistance and labour market subsidy were received in 2001. The reported values are for ages 18–64. Social assistance recipients are more often short-term recipients. Table 1 shows that 21.2% of social assistance clients received it for only a month, and 13.5% of labour market subsidy recipients received it for one month. The empirical model does not capture positive treatment effects on those social assistance clients who received social assistance in 2001 but not afterwards. Of all social assistance clients who received social assistance in 2001, 64.9% received it in 2002, 55.6% in 2003, 47.1% in 2004 and 33.6% in 2007.⁵ Twenty-two per cent (21.6%) of social assistance clients did not receive social assistance on any consecutive year between 2002 and 2007.

⁵ The frequencies are author's calculations from the data. The frequencies were calculated for social assistance clients aged 18–64 in 2001.

Table 1 Social assistance and labour market subsidy dependency in 2001

Variable	Social assistance	Labour market subsidy
Months received	5.33 (3.95)	6.64 (4.05)
Cumulative month distribution		
1	21.17	13.51
2	35.13	22.02
3	44.99	30.61
4	52.0	37.49
5	58.39	44.23
6	63.73	51.37
7	68.55	56.83
8	73.04	62.13
9	77.51	67.18
10	82.20	71.97
11	88.01	77.16
12	100	100
Observations	17 375	21 238

(1) The reported values are averages expect for the cumulative distribution function. Standard deviations are shown in parentheses. (2) Labour market subsidy is granted for weekdays only. Labour market subsidy months are calculated assuming that there are 21.5 weekdays in a month. Days below 10.75 are rounded upwards to one month

Table 2 Descriptive statistics on labour market status in 2001

Variable	Social assistance	Labour market subsidy
Earnings (€)	4679.0 (7135.1)	3256.8 (4617.5)
Months employed	3.63 (4.44)	3.12 (3.85)
Hourly wage (€)	9.09 (16.5)	8.30 (3.60)
Main activity, %		
Employee	30.14	27.22
Unemployed	34.72	51.72
Student	13.09	9.77
Retiree	8.89	0.45
Disability retiree	0.30	0.16
Military or civilian servant	0.64	1.22
Outside the labour force	12.22	9.46
<i>N</i>	17 375	21 169

(1) The reported values are averages for earnings, work months and hourly wages (aged 18–64). Standard deviations are shown in parentheses. (2) Main activity refers to activity in the last week of the year

Table 3 Descriptive statistics for the selected sample

	Treatment		Control	
	Mean(Sd.)	Freq.	Mean(Sd.)	Freq.
Earned income (€)	5592.5 (7486.3)	7137	4072.0 (4940.2)	6795
Months employed	4.35 (4.55)	6734	3.91 (4.07)	6385
Months received	5.24 (3.89)	7134	6.30 (3.99)	6790
Age	33.49 (10.43)	7137	35.81 (11.80)	6795
Female	0.46 (0.50)	3308	0.61 (0.49)	4136
Spouse	0.21 (0.41)	1517	0.39 (0.49)	2647
High education	0.10 (0.30)	689	0.14 (0.35)	962
Middle education	0.51 (0.50)	3618	0.59 (0.49)	3990
Low education	0.40 (0.49)	2820	0.27 (0.44)	1837
Couple without children	0.09 (0.29)	671	0.18 (0.39)	1255
Couple with children	0.28 (0.45)	2003	0.54 (0.50)	3691
Single parent	0.25 (0.43)	1794	0.12 (0.32)	792
Single person	0.28 (0.45)	2014	0.10 (0.31)	707
Tenant	0.70 (0.46)	5 048	0.37 (0.48)	2512
Observations				
Treatment:	7137			
Control:	6795			

Table 2 shows descriptive statistics on labour market status and employment in 2001 (aged 18–64). Social assistance clients have approximately €1400 higher yearly earnings, and the earnings have considerably more variance. Receiving social assistance can be very temporary, or it can become a long-term dependency. Short-term recipients are more often students, they may be between jobs, or for various reasons they have experienced a sudden but temporary loss of income. Table 2 also shows that hourly wages have more variation for social assistance recipients. The higher share of short-term recipients and higher variance in earnings implies that some of the social assistance clients have a reasonably good labour market position. It is likely that these short-term recipients became employed for reasons unrelated to the earnings disregard.

Table 2 also shows the main activity in the last week of the year. The shares of activity appear quite similar. For example, the share of employees and individuals outside the labour force is similar between the social assistance and labour market subsidy recipients. A distinctive characteristic is a larger share of retirees among the social assistance recipients. Because retirees form a less relevant group for the studied scheme, they are dropped from the sample.

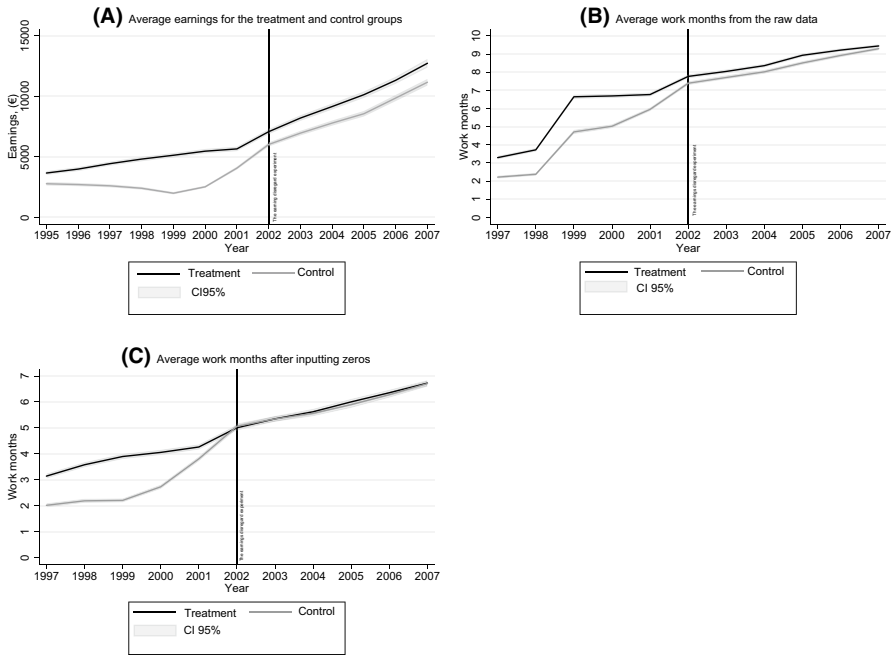


Fig. 2 Employment trends for the treatment and control groups

The treatment and control groups are defined so that individuals in the treatment group have received a positive amount of social assistance, and the control group does not receive any social assistance in 2001. Table 3 shows summary statistics for the treatment and control groups. In a balanced panel, there are totally 7137 individuals in the treatment group and 6795 individuals in the control group. Both the treatment and control groups have higher earnings than in Table 2. The treatment group has higher earnings because retirees were dropped and the control group because individuals who receive social assistance are excluded. In 2001, 33% of labour market subsidy recipients were also social assistance recipients, which is an indicator of a weaker social position as they receive multiple benefits. Table 3 also shows that the average time to receive social assistance was 5.2 months and 6.3 months for labour market subsidy recipients. Social assistance recipients were more often tenants, without a partner, low educated and single parents.

Figure 2 shows the employment trends with earnings and work months as outcomes. The graphs are formed of the panel so that the treatment status is defined on the pre-treatment period, that is in 2001. Graph 3.2A shows that the treatment and control groups follow similar employment paths, but the level difference is not constant in the pre-treatment period. This is adjusted by the CEM matching. Graph 2B shows average work months from the raw data. Statistics Finland changed its work month classification after 1998. In 1997 and 1998, work months were coded zero for all work days between 0 and 15, including individuals who did not have any workdays. After 1998, individuals with no workdays received a NULL value. In graph

2C, the NULL values are replaced with zeros for all individuals who had zero yearly earnings. This makes the earnings and work month outcomes more comparable.

4.3 Matched variables and covariate balance

The following introduces the matched variables. Strictly exogenous variables are chosen. Household types are omitted because these likely affect the treatment. The age of children between 0–2 and 3–8 are included. Young children are less likely to affect the treatment variable, but they may affect the labour supply of their parents. Age is based on quantiles with coarsened bins (24, 34, 44). Earnings are balanced on two pre-treatment periods to avoid endogeneity. The coarsened bins are based on earnings quantiles with coarsened bins in 2000 (0 193 1231 2684 4454 6839 11764). In addition, regional characteristics, education level, sex and students in the pre-treatment period are balanced. Table 4 shows the covariate balance before and after matching.

Table 4 shows that there is a reasonably good covariate balance. The pre-treatment earnings are not fully balanced, but other variables' covariate balance is zero or very close to it. In CEM matching, there is a trade-off between external and internal validity. The more bins, the more precise the results are, but the results may not be externally valid. After matching, there are 5344 individuals in the control group and 4508 individuals in the treatment group. The unmatched individuals are 1451 and 2620 respectively. Thus, the results are not fully externally valid.

Figure 3 shows the employment trends after matching. Figure 3A and B shows that the pre-trends follow similar paths, and the confidence intervals overlap in the pre-treatment period. The employment effects on work months without inputting zeros (Fig. 3B) are not further studied since only positive workdays were counted after 1998. This intensive margin outcome is not further studied as conditioning on positive earnings causes some bias to the results. Figure 3C shows the average work months after inputting zeros if an individual has zero earnings. The pre-trend difference is not constant over time, and these results should be interpreted with caution. It may be more challenging to obtain a good covariate balance with work months as an outcome since the work months is an integer variable with fewer available bins than in earnings.

5 Results

This section begins by showing the regression results for four outcomes. In Table 5, the first column shows the results for earnings and the second column for logged earnings. The third and fourth columns show the results for work months and logged work months, respectively. Work months are estimated for the years 1997–2007 only due to lacking data. Figure 4 plots the treatment effects on yearly earnings and work months.

Figure 4A and Table 4 show no statistically significant treatment effects on earnings. The drawn confidence intervals show that the treatment effects would not be

Table 4 Covariate balance before and after matching

	Treatment		Control		Unmatched diff.	Matched diff.
	Unmatched mean	Matched mean	Unmatched mean	Matched mean		
Earnings $t=2000$	5335.65	3280.11	2490.70	3094.19	2845.95	185.92
Earnings $t=1999$	4971.68	2784.52	1963.57	2661.98	3008.11	122.54
Student $t=2000$	0.18	0.14	0.14	0.16	0.04	-0.02
Age	33.40	33.76	35.81	34.02	-2.41	-0.26
Woman	0.46	0.47	0.61	0.47	-0.15	0.00
Low education	0.40	0.40	0.27	0.40	0.13	0.00
Middle education	0.51	0.54	0.59	0.54	-0.08	0.00
High education	0.10	0.06	0.14	0.06	-0.04	0.00
Children 0-2	0.09	0.03	0.03	0.03	0.06	0.00
Children 3-8	0.16	0.09	0.15	0.09	0.01	0.00
Large cities	0.27	0.22	0.16	0.22	0.11	0.00
Towns and rural areas	0.56	0.63	0.68	0.63	-0.12	0.00
N	7137	4508	6795	5344		

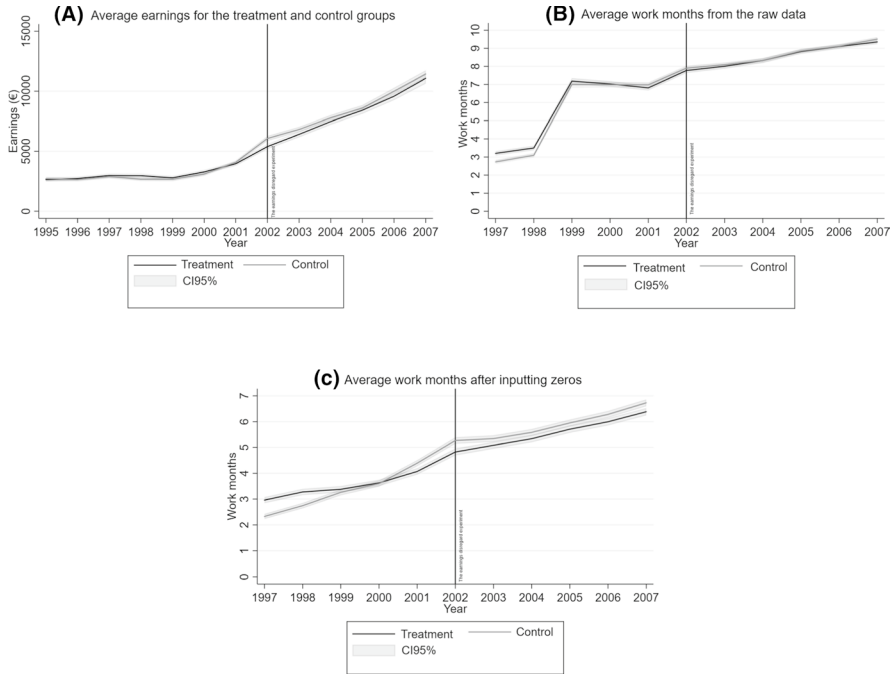


Fig. 3 Employment trends for the treatment and control groups after matching

Table 5 Estimated treatment effects

	(1)		(2)		(3)		(4)	
	Earnings		Logged earnings		Work months		Logged work months	
	β^{DiD}	S.E.	β^{DiD}	S.E.	β^{DiD}	S.E.	β^{DiD}	S.E.
$t = 2002$	-557.4***	180.4	-0.07	0.13	-0.12	0.09	0.01	0.02
$t = 2003$	-278.8	233.1	0.02	0.12	0.06	0.12	0.02	0.02
$t = 2004$	-196.9	279.8	0.09	0.12	0.08	0.13	0.03	0.02
$t = 2005$	-56.3	298.5	-0.07	0.13	0.08	0.13	0.03	0.02
$t = 2006$	-294.5	321.7	-0.09	0.15	0.04	0.13	0.03	0.02
$t = 2007$	-233.7	367.9	0.00	0.16	-0.02	0.14	0.00	0.02
N	128076		128076		91143		91143	

(1) Work months are estimated for the years 1997–2007. Full sample is used for the other outcomes. (2) Standard errors are clustered at the individual level. (3) *Significant at 10%; ** significant at 5%; ***Significant at 1%

sizeable even though the effects were at the upper boundary. Figure 4B also shows that the pre-trend differs statistically significantly from zero in the case of work months. As discussed above, it may be more difficult to flatten the pre-trends for work months because work months is an integer variable with less available bins

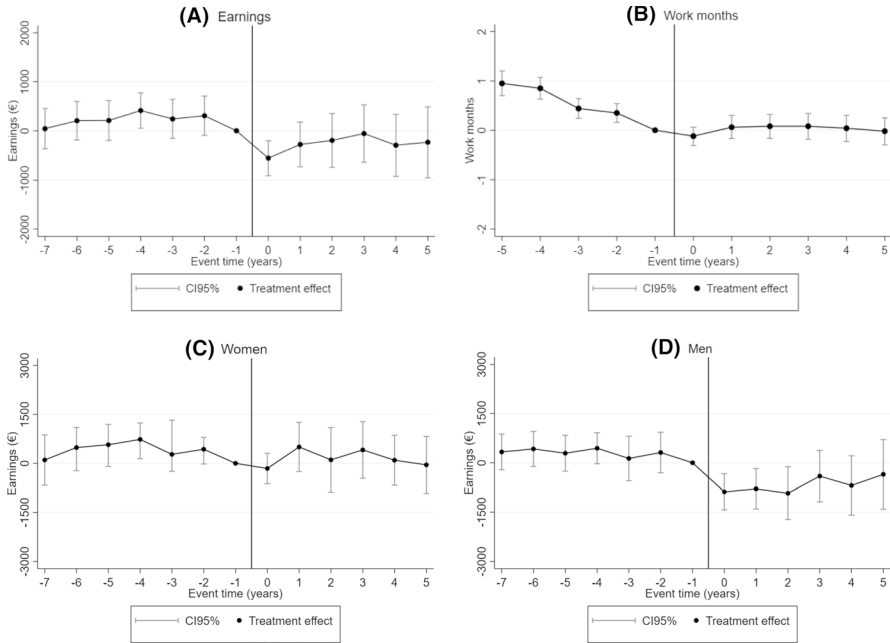


Fig. 4 Estimated treatment effects for the whole sample and women and men

than in earnings. Thus, the results should be interpreted with caution. Table 4 also reports the results after a logarithmic transformation. This gives less weight to high earning individuals. The results are very close to zero.

Figure 4 shows the results for women and men (4C and 4D). The empirical labour supply literature often finds that women are more responsive to financial incentives than men (e.g. Meghir & Phillips, 2010). There are no statistically significant effects on women, but the effects on men are negative, which may be caused by imprecise matching. As a sensitivity analysis, Fig. 5 shows the results after adding household types and unemployment in the pre-treatment period-2 to the set of matched variables. That is, the specification is the same as the main specification introduced in Sect. 4.3 but adds household types and pre-treatment unemployment. The household types include single persons, single parents and couples with children and without children. The additional matched variables increase precision to the estimates (bias-variance trade-off), but the household types are potentially endogenous. Figure 5C shows that there are statistically significant effects on women, but the confidence intervals are wide. Figure 5B also shows that the work month pre-trend is flat with the additional matched variables. The treatment effects remain non-significant using work months as an outcome variable. Figure 5D shows that treatment effects for men are no longer negative.

Table 6 shows three types of robustness checks. The first result column (1) excludes social assistance clients’ spouses who belong to the control group. This is because social assistance is means-tested at the family level, and the earnings disregard was household-specific. This creates a potential downward bias. Table 6 shows

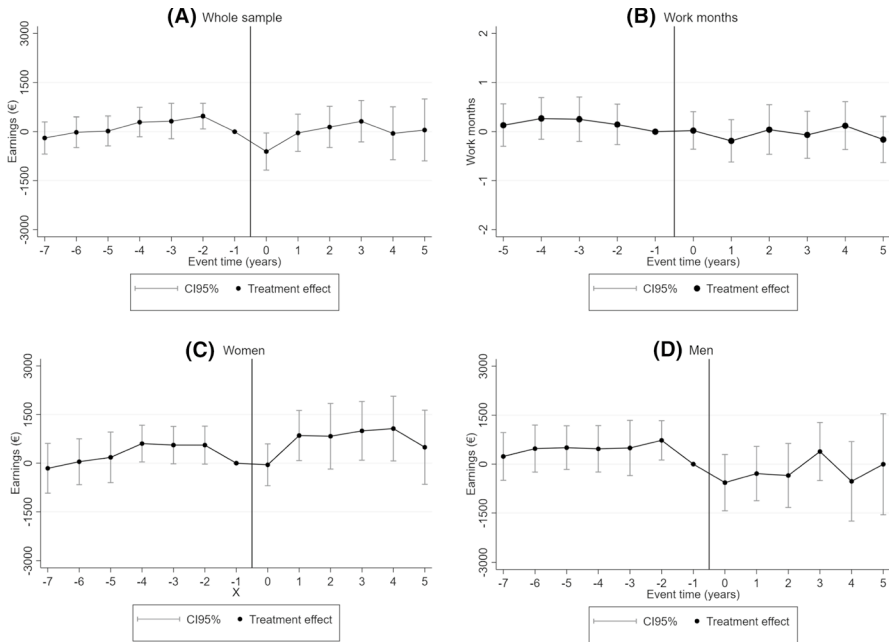


Fig. 5 Estimated treatment effects after adding household types and pre-treatment unemployment in period-2. Household types and pre-treatment unemployment are added to the main specification introduced in Sect. 3. Household types refer to single persons, single parents, couples without children and with children. Pre-treatment unemployment in period-2 refers to unemployment in 2000

that the results are very similar without the spouses. This is partly explained by the yearly data. Because the control group did not receive social assistance for one year, it is to a large extent formed of families and individuals who were not eligible for social assistance or did not apply for it. Approximately eight per cent (8.2%) of the control group members had a spouse who received social assistance in 2001.

Table 6 Robustness checks

	(1)		(2)		(3)	
	Spouses excluded		Pre-trend controlled		Tenants excluded	
	β^{DiD}	S.E.	β^{DiD}	S.E.	β^{DiD}	S.E.
$t = 2002$	-618.2***	183.4	-561.1***	180.4	-543.0***	189.0
$t = 2003$	-345.4	237.6	-286.3	233.1	-418.2*	226.0
$t = 2004$	-250.2	284.5	-208.2	279.8	-579.8**	271.2
$t = 2005$	-85.7	303.0	-71.3	298.5	-349.8	287.6
$t = 2006$	-290.8	325.8	-313.2	321.7	-296.9	326.4
$t = 2007$	-206.6	373.0	-256.2	367.9	-428.2	343.5
N	122,499		128,076		91,143	

(1) Standard errors are clustered at the individual level. (2) *Significant at 10%; **significant at 5%; ***significant at 1%

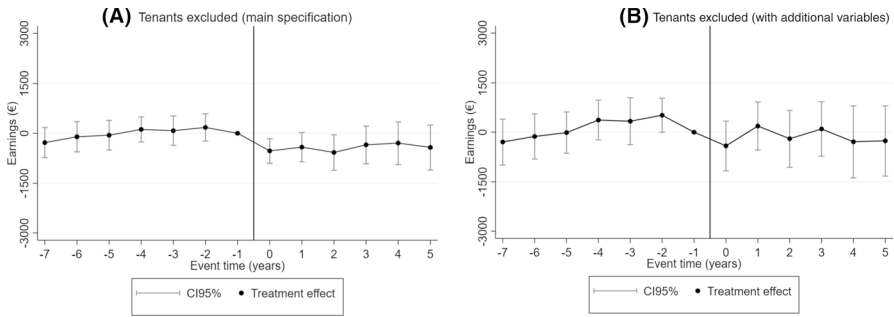


Fig. 6 Estimated treatment effects after excluding tenants from the control group. The right-hand side figure adds household types and unemployment in period-2 to the main specification. The left-hand side figure plots the treatment effects with the main specification introduced in section 4.3. Household types refer to single persons, single parents, couples without children and with children. Pre-treatment unemployment in period-2 refers to unemployment in 2000

Even if there was a treatment effect, the effect would be likely smaller for spouses in the control group. This is because the social assistance was paid to the applicant’s bank account. Although families may share earnings, the fact that the spouse did not receive the social assistance could have diminished the perceived incentive effect for the spouses.

The second model (2) in Table 6 controls for differing pre-existing trends between the treatment and control groups similarly to Kleven et al. (2014). At first, the earnings growth is predicted for post-treatment years from pre-treatment data separately for the treatment and control groups. Then the trends are subtracted from post-treatment data, and these values are used as outcomes. The results remain very similar. The third result column (3) excludes tenants from the control group. Home-owners are less likely to be eligible for social assistance. One way to measure exogeneity and inflow to social assistance is the share of social assistance clients’ spouses in the control group. Only 4.2 % of home owners had a spouse who received social assistance in 2001. The results do not significantly change, but the treatment effects are more negative, which may be caused by imprecise matching. Figure 6 plots the treatment effects with the main specification (6A) and after adding household types and pre-treatment unemployment in period-2 (6B). Figure 6 shows no positive effects after excluding tenants from the control group.

The results show no statistically significant employment effects on average. There is some evidence of women’s employment response to the earnings disregard, but the estimations are potentially endogenous, and the sample size limits the examinations. The results appear robust to the potential inflow from the control group as shown by the estimations using exogenous home owners as a control group and to the changed incentives of social assistance recipients’ spouses.

6 Conclusions

This paper examined the employment effects of the Finnish earnings disregard experiment between 2002–2007. The results show no significant employment effects on average, but the results suggest a treatment effect on women. The empirical approach does not allow for studying the short-term recipients, and monthly data would provide more subtle results. Although the results suggest no clear positive effects, the new policy had positive aspects. The earnings disregard unambiguously improved social assistance clients' situation with limited fiscal implications. Before the experiment social assistance was effectively reduced in one-to-one ratio after a recipient started to earn income. However, from a policy perspective, there are factors that weakened the effectiveness of the experiment. Applying the earnings disregard at the individual level instead of the household level would have likely given 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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