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Removing Welfare Traps: Employment Responses in the Finnish Basic Income Experiment*

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Abstract

This paper provides evidence that replacing minimum unemployment benefits with a basic income of equal size has minor employment effects at best. We examine an experiment in Finland in which 2,000 benefit recipients were randomized to receive a monthly basic income. The experiment lowered participation tax rates by 23pp for full-time employment. Despite the considerable increase in work incentives, days in employment remained statistically unchanged in the first year of the experiment. Moreover, even though all job search requirements were waived, participation in reemployment services remained high.

Keywords: Employment, Field experiment, Social insurance, Unemployment benefits
JEL: C93, H55, I38, J65

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1 Introduction

It has been widely hypothesized that universal basic income might alleviate many problems in contemporary labor markets, examples being job polarization and inequality. Another common argument is that social security in its present form creates welfare traps and is unduly complicated and inefficient. According to its proponents, basic income could tackle these issues by providing a guaranteed income in a simple system that also rewards work effort.[1]

This study analyzes the first nationwide randomized experiment on basic income in an advanced economy. The experiment was carried out in Finland in 2017–2018, with 2,000 persons whose minimum unemployment benefit was replaced by a monthly guaranteed income of €560 ($631). Participation in the experiment was mandatory, and it removed all obligations for job search set by the public employment services. The experiment had no impact on disposable income if one was out of work. When employed, persons in the treatment group continued to receive the basic income, whereas regular unemployment benefits are reduced as the recipient's labor earnings increase. As the tax schedule remained unchanged in the experiment, there was no phase-out point and the effective marginal tax rates of the treatment group decreased substantially. As an illustration, this resulted in a reduction in the participation tax rate from 66% to 43% at a monthly wage level of €2,000 ($2,255).

We use detailed administrative data to study how the bundle of a new social benefit, reduced administrative barriers, and lower marginal tax rates affected employment. In the first year of the experiment, we find no statistically significant effect on days in employment, the main outcome defined in a pre-analysis plan. The point estimate for the treatment effect is 1.5 days (95% CI -2.3–5.4), which can be contrasted with the average of 49 days in employment per year in the control group. The treatment group participated in reemployment services at a high rate, despite not being required to do so. This suggests that the possibility to avoid job search requirements had only a limited negative impact on employment in the experiment.

The second-year employment effect turns out to be somewhat higher (6.6 days, 95% CI 1.3–11.9). The interpretation of this result is unclear owing to the unemployment benefit reform that was implemented on 1 January 2018, which tightened the eligibility

[1] See Hoynes and Rothstein (2019) for a broader discussion on arguments for a basic income.

criteria for unemployment benefits asymmetrically in the control and treatment groups. Although simultaneous reforms do not invalidate our randomized research setting, the second-year result has to be interpreted as arising from the combined effects of the basic income experiment and the 2018 reform.

Several experiments on cash transfers have been conducted in developing countries in recent years, but only few of these meet the criteria of universality and the lack of conditionality on receiving benefits (Banerjee et al. 2019). In addition, the findings are not directly applicable to advanced countries, as the mechanisms through which such programs work in developing countries are quite different. For instance, Haushofer and Shapiro (2016) studied the unconditional cash transfer experiment conducted by the NGO GiveDirectly in Kenya and found an increase in food security and subjective well-being. The context is quite different in a Nordic welfare state, where safety nets are already in place to ensure a subsistence minimum.

One model for the Finnish experiment was the series of negative income tax experiments carried out in the U.S. in the 1970s. These were used to test negative income tax schedules provided through a guaranteed income that was taxed away as earnings increased. The present experiment differs from these in several respects. In the U.S. experiments, the treatment augmented the current system, whereas in the Finnish experiment the new benefit type replaced existing unemployment benefits. In addition, the target population in the U.S. experiments consisted of people who were employed, and the research setting had several treatment arms that allowed estimation of income and substitution effects. Furthermore, the guaranteed income resulted in the participants reducing their work effort, with women being affected more than men (Ashenfelter 1978; Burtless 1986; Ashenfelter and Plant 1990). Our findings supplement these results by providing evidence on a guaranteed income offered to unemployed persons.

Our results also add to the literature on the impacts of in-work benefits. Although conceptually different, the Finnish experiment lowered the effective marginal tax rates in a manner that bears similarities to the Earned Income Tax Credit (EITC) in the U.S. and the Working Families Tax Credit (WFTC) in the U.K. (see e.g., Blundell and Hoynes 2004). Even more closely comparable to the Finnish experiment is the randomized Self-Sufficiency Project (SSP) conducted in Canada between 1992 and 1999 (Card and Robins 1998; Michalopoulos et al. 2000). Most studies on in-work benefits have concluded that labor supply is responsive at the extensive margin. In this regard,

the comparatively minor effects found in the Finnish experiment provide an interesting contrast. Potential explanations for the difference relate to the target population and the profile of earnings supplements. The Finnish experiment included the entire population of those receiving the minimum unemployment benefit. This is a group among whom long-term unemployment is common, one-third of whom only have a basic education, and one-fourth of whom have an immigrant background. The smaller employment effects could thus be attributed to the basic income recipients having a more serious lack of skills than, for example, the SSP participants. The Finnish experiment also provided the largest improvements in employment incentives at higher wage levels as compared to the tax credit programs and the SSP. These wage levels might have been otherwise unattainable for many basic income recipients.

In addition, this study contributes to the literature examining active labor market policy. It is well documented that the exit rate from unemployment increases before an active labor market program starts (Black et al. 2003; Graversen and Van Ours 2008; for a survey, see Filges and Hansen 2017). An explanation for this finding is that the value of being unemployed declines during participation in an active labor market program. As a benefit that had no constraints on receiving it when the recipient was unemployed, the basic income provided a possibility to avoid all such programs, as well as other obligations built into unemployment benefits, without any cost. Yet, our results show that the treated made very little use of this possibility; we attribute this finding mainly to the unemployment benefit supplements that participants in active labor market programs receive during participation. This suggests that providing monetary incentives for unemployed persons to participate in such programs may negate the positive ex-ante effects of reemployment policies.

The rest of the paper is structured as follows. In section 2, we present the institutions involved and the experiment in more detail. Section 3 goes on to discuss the empirical strategy and sections 4 and 5 present and discuss the results. Section 6 concludes.

2 The experiment

2.1 Institutions

Social benefits during unemployment

Finland has a three-tier unemployment benefit system in which the type of benefit depends on the recipient's employment history and unemployment fund membership. Type 1 (earnings-related) is paid to fund members who have worked at least 6 months during the previous 28 months. The maximum duration of this benefit is 400 weekdays. Other jobseekers receive benefits paid by the Social Insurance Institution (SII) of Finland. Those who meet the employment criterion but are not members of an unemployment fund receive benefit type 2. It is a flat-rate benefit with no means testing that can be paid up to 400 weekdays. Those who do not meet the employment criterion, or who have exhausted benefits type 1 or 2, receive benefit type 3 (means-tested), which is a flat-rate benefit with wealth testing and paid indefinitely. In 2017, both flat-rate minimum benefits were €32.4 per day, or €697 per month, while the average type 1 earnings-related benefit was €1,371 per month. The unemployment benefit is increased for those with children under 18 years of age. This child supplement varies from €5.28 per day for one child up to €10 per day for three or more children. All unemployment benefits are taxable.

The Finnish tax-benefit system produces high effective marginal tax rates at the extensive margin owing to income taxation and benefit tapering. Unemployment benefits are adjusted when paid to jobseekers with part-time or temporary employment. Monthly earnings below €300 do not affect unemployment benefits. Above €300, all unemployment benefits are reduced at a 50% marginal rate. A large proportion of the recipients of unemployment benefits also receive housing benefits. Housing benefits are paid to low-income households and cover up to 80% of housing costs; the costs accepted by the SII depend on the household type and region. Rents typically exceed the maximum accepted costs, especially in the capital region, where a single person household can receive a maximum monthly housing benefit of €413. Household earnings reduce housing benefits at a marginal rate of 34% after a €300 monthly deductible per earner. Finally, social assistance is paid as a last resort to unemployed persons whose income and assets do not cover their essential daily expenses, such as

food, clothing, and minor medical expenses. As these expenses are mainly covered by other benefits, the impact of social assistance on effective tax rates is restricted to persons with very low levels of labor income. An illustration of the Finnish tax-benefit system is provided in the next section, where we look more closely at the changes that the experiment caused in work incentives.

Public employment services

To receive unemployment benefits, jobseekers need to register with the public employment services. After registration, a jobseeker is interviewed within two weeks, with interviews held at three-month intervals thereafter. A typical interview is carried out on the telephone and lasts between 10 and 20 minutes. Only 18% of interviews happen face to face with a counselor. During an interview, the unemployed person and counselor agree on a set of targets that the jobseeker has to meet before the next interview. This individually tailored and mutually agreed contract is called an employment plan. Depending on the jobseeker, the plan may include different goals related to activities such as carrying out a job search, preparing a resume, arranging a health check-up or applying for active labor market programs (ALMPs).

ALMPs in Finland consist of measures similar to those analyzed in Card et al. (2018), that is, labor market training, subsidized jobs, work practice, and rehabilitative work. Excluding subsidized jobs based on job contracts, participants receive their regular unemployment benefits during program participation. Unemployed persons are further encouraged to participate in ALMPs through benefit supplements. Recipients of unemployment benefits may receive a €4.74 daily supplement for 200 weekdays when participating in a program. Participants are also entitled to €9 in nontaxable compensation for daily expenses. In 2017, almost 40% of all unemployment benefit payments by the SII were paid to participants in different ALMPs.

The fulfillment of the employment plan is monitored in subsequent periodic interviews, and non-compliance triggers sanctions that result in the withdrawal of unemployment benefits. Unlike the SII, the employment services do not require periodic reports, as their job search registration is open-ended. It is particularly beneficial for our purposes that no information on the treatment status was given to the employment services nor did receipt of the basic income automatically affect a person's jobseeker status. It is reasonable to assume that the employment services served all target group

members similarly and thus that any differences between the two groups can be attributed to the experiment.

2.2 Basic income

The basic income experiment was first mentioned in the strategic program that the newly elected government submitted to the Parliament on 29 May 2015. The experiment was scheduled to start at the beginning of 2017 with a budget of 20 million euros. The initial objective was to study the effect of a basic income model on a target population including employed persons as well. This design had to be scaled down mainly due to the tax authority, which stated that the timetable was too tight for making the required changes in tax parameters. The government did not want to delay the experiment for political reasons and decided to go forward with an experiment focusing on employment incentives for those receiving minimum unemployment benefits (types 2 and 3).

As the basic income experiment interfered with the Finnish social security system, implementation required a legislative basis. The government introduced the bill on the experiment in Parliament on 20 October 2016. After debates in the Constitutional Law and Social Affairs and Health Committees, Parliament passed the Act on the Basic Income Experiment on 13 December; it came into force on 1 January 2017. It was decided that the experiment would last for two years, during which time 2,000 randomly selected individuals would be paid a guaranteed and nontaxable basic income of €560 per month. Those assigned to the treatment group were randomized from the pool of unemployed people who received unemployment benefits from the SII in November 2016. For a person out of work, the basic income corresponded roughly to the after-tax unemployment benefits without supplements paid to the control group. The experiment removed most of the bureaucracy related to unemployment benefit applications and allowed the treatment group to opt out of any monitoring or reemployment services.

The persons randomized to the treatment group were eligible to apply for other social benefits, but the basic income was deducted from the net value of such benefits. Social benefits based on household income were adjusted only if households' income changed during the experiment. The experiment did not affect the income tax schedule and thus the incentives to find a job improved among basic income recipients. Figure

1 illustrates these changes in work incentives by reporting disposable income for four stylized households at different monthly wage levels.[2] The four panels show that the experiment had no impact on employment incentives at low earnings levels. Disposable income of the treated exceeds that of the controls after the monthly wage level of €200. For a single parent this occurs when the monthly wage level exceeds €500. Another noteworthy observation is that the change in work incentives increases in earnings. The wage level required for the maximum increase of €560 in monthly disposable income ranges between €1,700 and €2,500 per month depending on the number of children in a household. As the median wage of a full-time worker is roughly €3,200 per month, these wage levels are located at the low end of the wage distribution.

The illustrations in Figure 1 mask substantial variation in the incentives, because it is impossible to present all combinations of family types and their different benefit levels with stylized households. A more detailed analysis of work incentives can be gained by utilizing individual records. Table 1 presents the participation tax rates (PTR) calculated for each person in our data set using a microsimulation model that takes into account the entire tax and transfer system. The main difference between the columns arises as all unemployment benefits are reduced at a 50% marginal tax rate for the controls, whereas only child supplements to unemployment benefits are reduced for the treated persons with children. The basic income payment remains the same over the earnings distribution. The figures are simulated for two earnings levels based on the observed characteristics of the target population at the end of 2016.[3] We separate

[2]We took into account the changes that an increase in labor income induces in income taxation, unemployment benefits, housing allowance and social assistance by using the tax-benefit microsimulation model SISU (https://www.stat.fi/tup/mikrosimulointi/index_en.html). The four stylized households are based on hypothetical data. The single person lives in a rental dwelling in a small town where the contract rent equals the amount of maximum accepted housing costs. The married couples consist of two initially unemployed persons who otherwise have the same background characteristics as the single person. The families with children receive the child benefit for one child. The single parent also receives child support.

[3]The PTR calculations are based on a tax-benefit microsimulation (the SISU model) by the SII using data on the observed benefit eligibility at the end of 2016 for the target population. For the two earnings level, the income taxes are simulated under an assumption that monthly earnings remain constant over the year and that both controls and treated apply for benefits for which they are eligible. In particular, the recipients of basic income with children are assumed to apply for unemployment benefits to receive child supplements to unemployment benefits. The €2,000 monthly earnings represent a median monthly wage for full-time cleaning work, hairdressers and related occupations (SOC 37-2010 and 39-5000, Statistics Finland). In addition, the first deciles of service workers and basic construction and warehouse workers are paid around €2,000 per month.

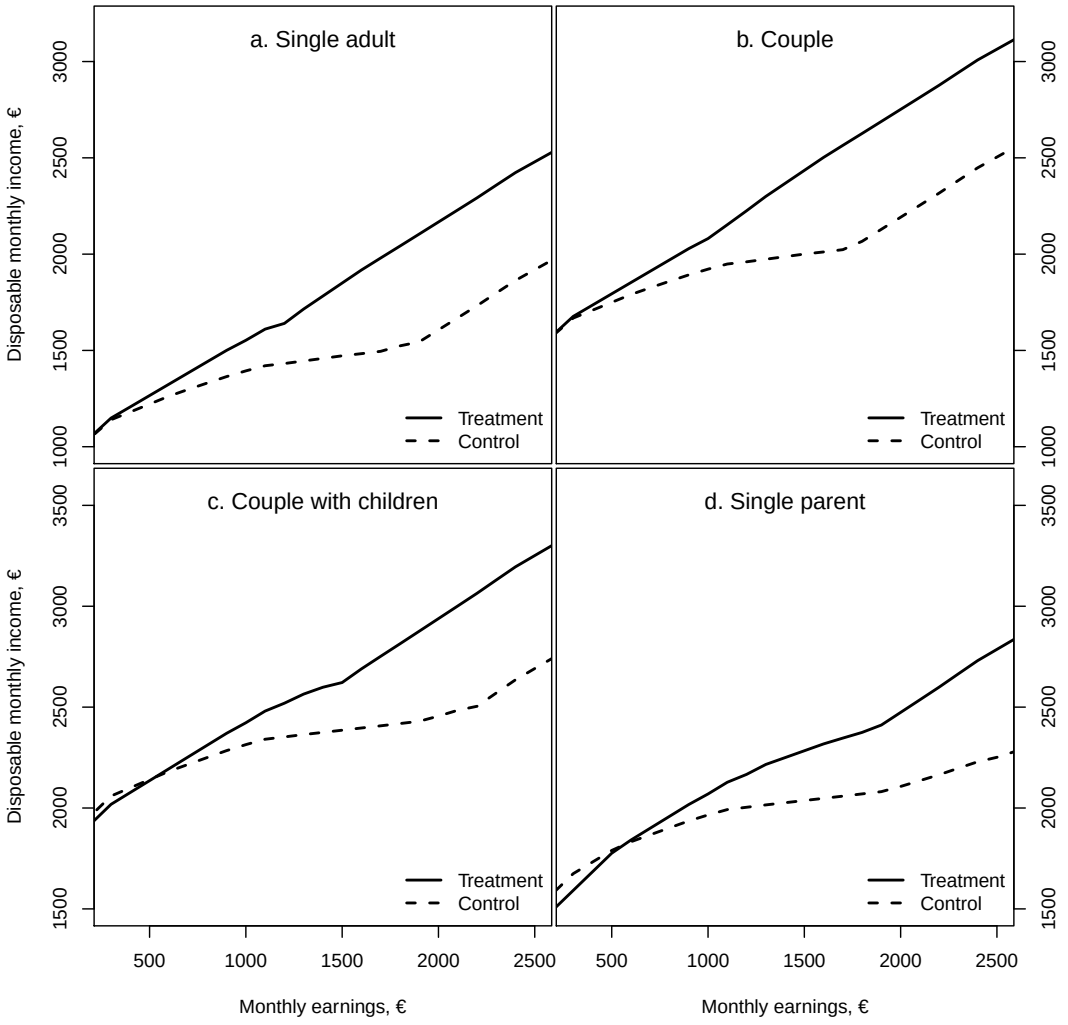

Figure 1: Changes in the work incentives for stylized households. Each is in an urban area outside the capital region in a rental dwelling with the maximum accepted housing costs. The spouses in the married or cohabiting couples receive minimum unemployment benefits. The two households with children receive the child benefit for one child. The single parent also receives child support.

Table 1: Changes in participation tax rates. The average participation tax rates for monthly labor earnings of €1,000 and €2,000 by eligibility for housing benefits and social assistance at the end of 2016.

Housing benefits	Social assistance	N	Earnings €1,000/mo. Treated (%)	Earnings €1,000/mo. Controls (%)	Earnings €2,000/mo. Treated (%)	Earnings €2,000/mo. Controls (%)
No	No	45757	24.0	40.6	28.3	53.6
	Yes	1346	54.0	68.5	45.8	70.2
Yes	No	53638	42.1	52.1	44.8	67.2
	Yes	28000	67.6	74.8	61.4	81.4
All		128741	41.3	53.1	42.5	65.5

Note: Tax rates are simulated for the target population using Statistics Finland's SISU microsimulation model based on the observed benefit eligibility at the end of 2016. The baseline is calculated for recipients of full-time unemployment benefits without labor earnings in November. Source: Hämäläinen et al. (2019).

the results by use of housing benefits and social assistance, which are the key benefits affecting incentives. The last row in Table 1 shows that the experiment reduced the average PTR by 23pp from 65.5% to 42.5% for a low-paid full-time job offering monthly earnings of €2,000, an amount corresponding to 62% of the median wage. For a part-time job with monthly earnings of €1,000, the average PTR decreased by 11.8pp from 53.1% to 41.3%. Interestingly, despite a large variation in the actual levels of PTRs, the differences between the two groups remain fairly similar across benefit categories.

Above we have shown that the experiment had a varying impact on work incentives depending on household type and benefit eligibility. Further differences arise from the removal of eligibility requirements for receiving benefits while being unemployed. As the basic income included no child supplement, the unemployment benefit for a control person with dependent children was some 16% to 31% higher than the basic income of a corresponding treated person if out of work. To compensate for this loss, the treated were allowed to apply for the difference. They were then paid the basic income payment as well as the difference between the after-tax regular benefits and the basic income payment. Receipt of this latter payment required them to comply with the unemployment benefit rules, and they had to be registered as jobseekers at the employment services. This created incentives for persons who were eligible for child supplements on unemployment benefit to stick with the eligibility criteria for unemployment benefits in the experiment.

2.3 The 2018 unemployment benefit reform

A reform came into force on 1 January 2018 that impacted the control and treatment groups in the basic income experiment asymmetrically. The reform tightened the eligibility criteria for unemployment benefits. According to the new rules, benefits were cut by 4.65% for the next three-month period if an unemployed person had not participated in ALMPs for five days or worked at least for 18 hours during a three-month period. Those receiving basic income were also affected if they decided to apply for unemployment benefits, but for them the benefit reduction applied only to any supplementary unemployed benefits paid in excess of the basic income.

The direct impact of the 2018 reform on employment remains unclear, as it may intensify job search as well as participation in ALMPs. One might expect the demand for ALMPs to rise primarily among the control group of the basic income experiment owing to the larger threat of benefit reduction it entails. In addition, after parliamentary hearings of the reform started in the late 2017, it drew considerable media attention to job search and marginal employment. If unemployed persons responded to this type of public debate on employment incentives, the impact was probably larger among the basic income receivers, whose incentives were improved dramatically in the experiment. Therefore, we interpret the second-year results of the basic income experiment as arising from a combination of the experiment and the 2018 reform, the latter potentially having different impacts on the control and treatment groups.

3 Empirical strategy

3.1 Randomization procedure

The target population of the experiment consisted of individuals who received minimum unemployment benefits (types 2 and 3) paid by the Social Insurance Institution (SII) in November 2016. The requirement for inclusion in the experiment was that a person had to be between 25 and 58 years of age on 1 December 2016, whereby young persons and those close to retirement age were excluded. The Act on the Basic Income Experiment also listed some specific conditions that excluded benefit recipients from the experiment, examples being persons applying for subsidies for childcare at home, beginning to take a pension, or moving abroad. These restrictions excluded only a small

number of people from the experiment, and the basic income payments were discontinued for only 45 and 92 persons at the end of the first and second year, respectively. The resulting target population consisted of 175,222 individuals.

The Act authorized the SII to implement a simple random assignment of 2,000 eligible individuals to the treatment group. As the experiment was implemented by law, participation was mandatory, leaving aside any problems with non-compliance. The randomization was carried out on 15 December 2016. At that point no one was informed about the randomization result, and no one had an opportunity to manipulate the assignment. Information letters were sent to the treatment group members on 28 December, and the first basic income payments were made on 9 January 2017. Thereafter, they were paid on the second banking day of every month.

3.2 Data

The data for this study come from official registers collected primarily for administrative purposes. The SII and Ministry of Economic Affairs and Employment collect information on all official dealings with, and services provided to, unemployed persons. Information on all employment contracts is entered into the register maintained by Finnish Centre for Pensions. Other public administrative bodies whose registers have been utilized in the analysis include the Tax Administration and the Population Register Centre. All information has been merged using unique personal identifiers.

For each person in the target population, we observe the exact starting and ending dates for employment contracts, jobseeker registrations and active labor market program (ALMP) participation. We also observe the exact dates when the basic income or social benefit payments were made by the SII, detailed information on amounts, and exact entitlement periods. The data also contain information useful for constructing control variables.

We use employment spells together with yearly earnings of employment contracts to form our primary outcome, that is, employment days in nonsubsidized labor markets. We exclude publicly subsidized jobs from the primary outcome and include them in days in ALMP. Our secondary outcomes include taxable income, the amounts of social benefits paid, and the use of public employment services.

3.3 Descriptive statistics and balancing tests

Table 2 presents average background characteristics for the treatment and control groups. A comparison across groups reveals that the mean values do not differ significantly from each other, with the exception of the share receiving type 3 means-tested unemployment benefits. A joint test for the background characteristics and past outcomes predicting treatment status is insignificant (p = 0.23). Therefore, we conclude that the randomization was successful.

The recipients of minimum unemployment benefits form a heterogeneous group in which many characteristics typically associated with poor labor market prospects are overrepresented. The target population tends to be skewed towards lower education levels. The share of persons with only basic education is twice as high as that in the entire population of comparable age (32% vs. 15%). A target group member was also more likely to have a native language other than one of the official languages in Finland (25% vs. 9%) or to live in a single household (40% vs. 29%). In addition, the share of those with weaker employment prospects due to a medical condition, as defined by the employment services' disability indicator, was high (16%). In contrast, the target group included many young and educated persons, whose labor market prospects should not have been particularly poor at the time. Any generalizations of the results to the entire population have to be made with caution, however.

3.4 Estimation

To explore the causal effect of basic income on primary and secondary outcomes, we estimate the following linear model separately for both analysis years:

$$Y_i = \alpha + X_i'\beta + \delta Tr_i + \varepsilon_i,$$

where Y_i is the given outcome variable, Tr_i is the treatment group indicator, X_i is a vector of observed characteristics measured before the experiment started, and ε_i summarizes the unobserved factors. Adjusting for the covariates is not required for consistency as randomization makes the treatment status exogenous. However, such an adjustment can increase the precision of the estimated treatment effect by reducing its standard error. We use heteroscedasticity-robust standard errors. The causal effect of

Table 2: Balance statistics. Means for the past outcomes and background characteristics for the treatment and control groups.

	Treated	Controls	Difference	p-value
Past outcomes in 2016				
Days in employment	23.79	23.93	-0.14	0.93
	(70.17)	(70.68)		
Days on unemployment benefit	286.0	285.6	0.42	0.86
	(106.3)	(106.6)		
Earnings (€)	1864	1896	-31.27	0.75
	(4275)	(4324)		
Background characteristics (%)				
Receives type 3 unemployment benefit	87.15	84.63	2.52	<0.01
Women	47.75	47.48	0.27	0.83
Age				0.29
25–34	33.50	35.12	-1.62	
35–44	27.45	27.14	0.31	
45–59	39.05	37.74	1.31	
Education				0.10
Basic	31.70	33.77	-2.07	
Secondary	46.65	45.99	0.66	
Tertiary	21.65	20.24	1.41	
Family type				0.72
Single	39.55	39.51	0.04	
Couple	18.25	17.98	0.27	
Couple with children	26.60	26.01	0.59	
Adult with children	15.60	16.50	-0.90	
Foreign language	24.55	25.36	-0.81	0.42
Disability	16.20	16.48	-0.28	0.76
Region type				0.99
Helsinki	13.85	13.74	0.11	
Surrounding capital region	9.55	9.47	0.08	
Other urban areas	44.30	44.69	-0.39	
Rural areas	32.30	32.10	0.20	
Joint F-test				0.23
N	2 000	173 222		

Note: The t-test is used for continuous variables, the χ^2-test for categorical variables. The joint F-test is done for all 11 variables predicting treatment status. Standard deviations are in parentheses. The type 3 benefit refers to the means-tested minimum unemployment benefit. The category "adult with children" includes single parents and adults with dependent children. "Disability" indicates those with an employment services' diagnosis code. Region type follows the cost-of-living categories in housing benefits.

basic income is captured by the estimated coefficient δ.

The vector of covariates, X_i, is selected to include variables that are predictive of the main outcome. These are determined using the Akaike information criterion (AIC). As the experiment was based on randomization, we do not expect to find any serious correlation between the treatment status and observed characteristics. As noted above, this might not be the case where type of minimum unemployment benefit is concerned, and thus there is a clear case for controlling it in regressions. Other control variables are age, gender, family type, education level and field, disability indicator, foreign language, region of residence, pre-experiment unemployment days, employment days, earnings, and housing benefit.

4 Results

4.1 Graphical evidence

We begin the analysis by providing an overview of the dynamics and main effects of the experiment. Figure 2 shows the monthly shares of treated and controls with days in employment and the difference between the two groups. As the population analyzed consisted of recipients of minimum unemployment benefits (types 2 and 3), employment was at a very low level in November 2016. During the first year of the experiment, the employment share in the control group rose from 8% to 18%. Employment in the treatment group followed this trend very closely, and none of the monthly estimates measuring the difference between the groups differs significantly from zero. Based on the graphical evidence, the experiment had negligible effects on the probability of employment during its first 12 months.

During the second year of the experiment, the employment shares continue to grow in both groups. However, the growth is slightly slower in the control group, and the monthly employment share is fairly consistently around two pp higher in the treatment group. This divergence in the employment trends coincides with the introduction of the 2018 unemployment benefit reform and, accordingly, the emergence of differences between the analysis groups cannot be attributed to the basic income experiment alone.

The lack of employment responses during the first year of the experiment raises the question of how the participants reacted to the experiment. Figure 3 presents the

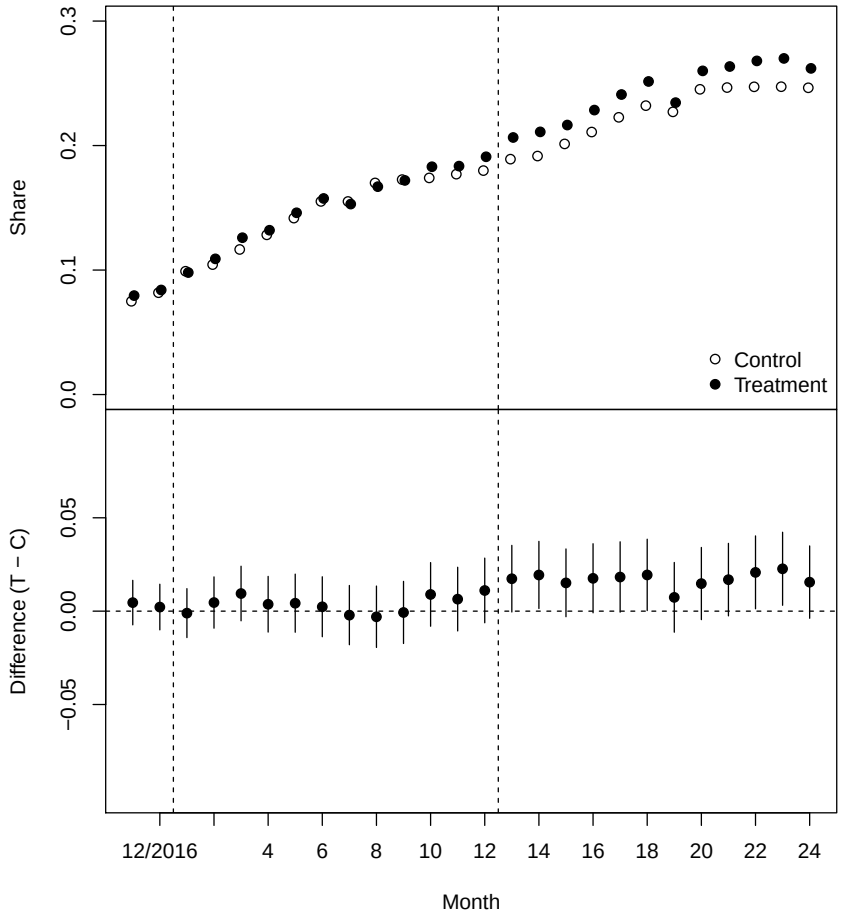

Figure 2: Employment in the analysis groups. The upper panel shows the share of persons with days in employment. The lower panel presents the difference between the treatment and control groups (vertical bars denote the 95% CI).

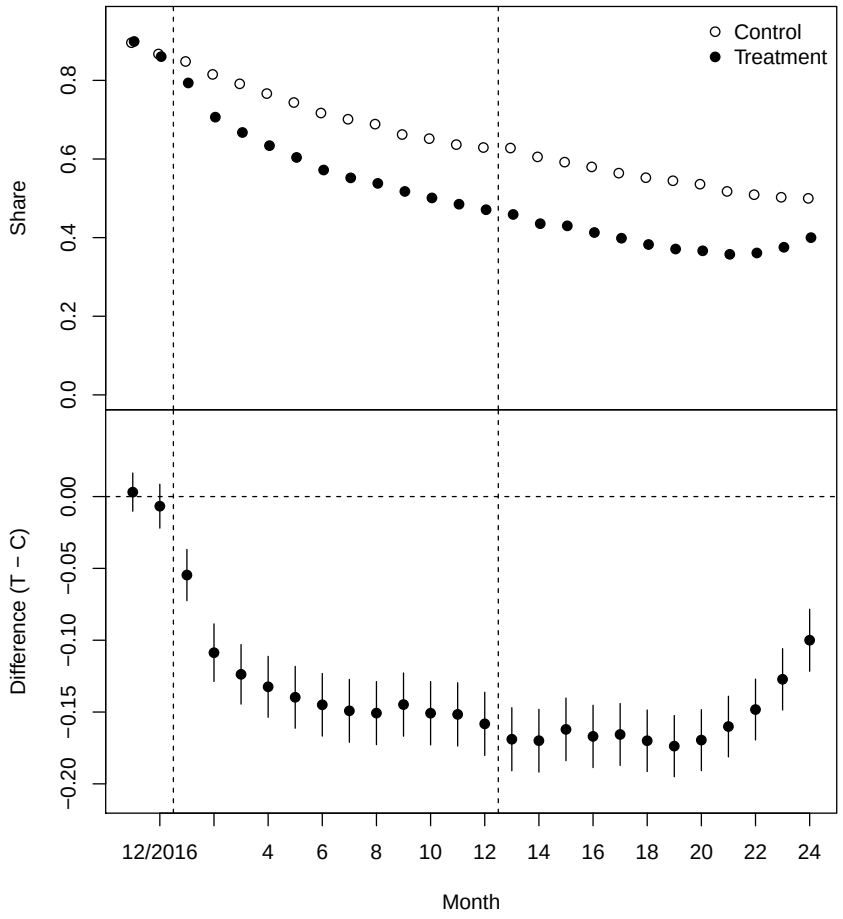

Figure 3: Use of unemployment benefits in the analysis groups. The upper panel shows the share of persons using minimum unemployment benefits. The lower panel presents the difference between the treatment and control groups (vertical bars denote the 95% CI).

use of unemployment benefits by the analysis groups. Benefit use among the controls declines steadily from 87% to 50% during the experiment. In December 2018, 46% of those who have stopped claiming unemployment benefits were employed. The other exit routes are largely unidentifiable in our data.[4] In the treatment group, the take-up of unemployment benefits drops rapidly during the first few months of the experiment. After that, benefit take-up among the treated declines more gradually, the difference between the two groups remaining at 15–17pp for most of the experiment time. A few months before the end of the experiment the gap begins to narrow as the treatment group starts to apply for unemployment benefits following the instructions of the Social Insurance Institution (SII). All in all, the take-up rate of unemployment benefits remains at a high level among the treated, even though they received basic income payments that equaled the level of regular unemployment benefits paid when out of work.

4.2 Estimation results

Table 3 reports the OLS estimates for various outcomes. The first row gives our main result, in which the primary outcome is defined as cumulative days in employment for both years of the experiment. We find an insignificant increase in employment of 1.5 days in the first year, with the 95% confidence interval ranging from -2.3 to 5.4 days. In the second year, the employment effect becomes larger and statistically significant. The estimate is 6.6 days (95% CI 1.3–11.9), which corresponds to an 8.6% increase in employment relative to the control group. These estimates confirm the results from the graphical analysis, according to which the experiment had a negligible impact on average employment before the introduction of the 2018 unemployment benefit reform. As the reform affected both analysis groups, the second-year estimates should be interpreted as a joint effect of the basic income and the reform.

Our measure of employment days is based on the national pension register, which records all employment spells regardless of wage level. In keeping with our pre-analysis plan, we apply a minimum wage threshold to exclude zero-hour contracts with

[4] We observe that 3% participated in subsidized employment and 2% was on sickness benefits. For the remaining 49% of the individuals our data is not very informative. Jobseeker records provide information on the reason for ending job search, but there is no requirement to contact the employment office if a jobseeker stops claiming for benefits. Consequently, 78% of the uncertain cases haven't provided any reason for leaving the register. Among those who have provided information, the far most common reason for leaving the register is the exit from the labor force.

Table 3: Treatment effects of basic income. Primary analysis of employment and secondary analysis of earnings, benefits, and the use of public employment services.

	2017				2018			
	Control mean	Estimate	S. E.	*p*-value	Control mean	Estimate	S. E.	*p*-value
Primary outcome								
Days in employment	49.06	1.54	1.98	0.44	77.34	6.63	2.71	0.01
Earnings and benefits (€ per year)								
Taxable income with basic income	11965	1362	141.3	<0.01	13469	1873	208.0	<0.01
Earnings	4251	9.10	145.6	0.95	6715	295.3	210.3	0.16
Sickness benefits	216.4	-100.1	15.46	<0.01	308.0	-150.5	22.27	<0.01
Housing benefits	2509	-31.92	31.95	0.32	2298	-138.3	39.48	<0.01
Social assistance	1344	-429.6	37.25	<0.01	1419	-468.7	40.84	<0.01
Use of employment services								
Days in active labor market programs	109.5	-12.53	2.75	<0.01	96.38	-15.88	2.69	<0.01
Prepared employment plan	0.70	-0.050	0.010	<0.01	0.65	-0.058	0.011	<0.01
Received sanction statement	0.088	0.020	0.007	<0.01	0.085	0.004	0.006	0.49

Note: Basic income is added to taxable income. Labor earnings and sickness benefits are included in taxable income. Housing benefits and social assistance are nontaxable benefits. The control variables are unemployment benefit type, gender, age, language, family type, region type, province (NUTS 2), level and field of education, disability indicator, employment in 2016, earnings in 2016, unemployment benefits in 2016, housing benefits in 2016. Standard errors are heteroskedasticity-robust.

low earnings from our employment measure.[5] By setting this wage criterion, we may have missed some employment dynamics in very low-paying jobs. To examine the robustness of our main result, we removed the wage threshold altogether and include all employment spells with positive annual earnings. For the first year of the experiment, this increases the average employment in the control group from 49 to 77 days in a year, and the point estimate of the treatment effect to 3.3 days (see Appendix Table A.1). Although the estimate remains insignificant, it indicates that occasional work may have become slightly more common in the treatment group. The removal of the wage threshold has a similar impact for the second-year results. Moreover, controlling for different sets of background variables has only a minor impact on the results in the main regression (see Appendix Table A.1).

The small average employment effect of the basic income experiment in the first year may mask interesting variation in employment responses. This variation could arise from different demographic groups facing different changes in their effective marginal tax rates or from particular characteristics of jobseekers leading to heterogeneity in labor market outcomes. To explore these effects, we conducted a subgroup analysis with respect to key background characteristics and examined the effect heterogeneity with respect to changes in the participation tax rates (see Appendix Tables B.1 and B.2). None of the resulting estimates turned out to be statistically significant before the introduction of the 2018 unemployment benefit reform. However, it should be noted that the point estimates for different family types follow an unexpected pattern whereby employment responses decrease as work incentives improve. For the second year, the effect heterogeneity remains qualitatively similar. These findings have to be considered tentative owing to power issues arising from the small sample sizes and the exploratory nature of the subgroup analysis.

Next, we turn to secondary outcomes. The subsequent rows in Table 3 present the results for earnings and various social benefits. In the first year, the pattern mirrors the negligible employment effects, as the average earnings remain intact. After the introduction of the reform in 2018, the earnings estimate becomes larger but remains insignificant. Relative to the control group, the point estimate corresponds to a 4.4%

[5] Our pre-analysis plan defines the primary outcome using data from both years of the experiment. Here we report separate results for both years as the 2018 unemployment benefit reform intervened with the second year of the basic income experiment.

increase in earnings, which is around half of the increase in employment. To explore whether the employment effects differed across the earnings distribution, we estimated the probability of being in a particular earnings category. These changes as well turned out to be small and insignificant (see Appendix Table A.2).

In the first year, total annual income increases by €1,362, which is explained by two factors. First, nearly all treated persons received an extra benefit payment in January 2017, when they were paid both the first basic income payment and unemployment benefits owed from December. Second, the employed persons in the treatment group received the basic income on top of their wage income. The effect on income grows to €1,873 in 2018, the second year of the experiment. This is to be expected, as the number of treated who used the basic income as an in-work benefit increased during the second year of the experiment.

The higher income had a varying impact on social benefits depending on how the benefits are recalculated. The change is most pronounced in the case of social assistance, which is recalculated on a monthly basis. The impact is far smaller where the housing benefit is concerned: it is recalculated only if a change in income lasts over 3 months, and temporary changes are taken into account as average income over the next 12 months. This means that the amount of housing benefits changes only if income increases by more than €400 per month, or more than €4,800 at the annual level. The recalculation rule explains why housing benefits are adjusted significantly only in the second year.

We also find a statistically significant decrease in sickness benefits. However, this result needs to be interpreted with care. The level of sickness benefits corresponds to that of unemployment benefits, and thus the target population had limited monetary incentives to file an application for sickness benefits in the first place. Nonetheless, the sickness benefits exempt recipients from obligations built into unemployment benefits, and one year on sickness benefits is required before a person can apply for a disability benefit. The rejection rate for disability pension applications is 35% in the population under the age of 59, and probably even higher among unemployed persons. It is thus plausible that some basic income recipients decided to postpone the application process during the experiment.

The last three rows in Table 3 show the effects of the experiment on the use of employment services. The removal of eligibility requirements for benefits while out

of work did not cause large-scale avoidance of reemployment services. The treatment group spent almost 100 days in different active labor market programs (ALMPs) in 2017 and some 80 days in 2018. For the first year, this is only 11% fewer days than in the control group, and some of this difference can likely be explained by the basic income recipients acquiring similar services from providers other than the employment services. The results on employment plans and sanction statements confirm a willingness to adhere to the obligations of the labor administration. We find a 7% decline in the share of persons who received an employment plan in 2017. This means that a vast majority of the treated complied with a renewal of their employment plan and whatever requirements for participation in reemployment services were included in the plan. A similar trend emerges when examining sanctions, which were imposed mostly in cases where recipients refused to participate in an ALMP. Given that sanctions had no effect on the basic income payments, one would expect to see a much larger difference between the two groups than the observed 23% in 2017 and 5% in 2018 if people actually disliked ALMPs.

5 Discussion

The aim of the randomized basic income experiment was to test the employment effects of a simple benefit that improved incentives to find a job and removed any job search requirements for benefit eligibility. The implementation of a new benefit in the existing social security system turned out to be a complex task. For 42% of the treated, the level of pre-experiment unemployment benefits exceeded the basic income payment due to child supplements. They had the opportunity to claim for this difference, which created an incentive to remain a registered jobseeker with the public employment services. As they were required to follow the same obligations built into unemployment benefits as controls, the experiment improved their work incentives without relaxing their job search requirements. Accordingly, the experiment is best thought of as a bundle of treatments that varied across subgroups. Our research design allows us to produce causal evidence on the joint impact of different components of the experiment. We cannot identify the impacts of different mechanisms directly but, owing to the richness of register data available, we are able to shed some light on the potential magnitudes of opposing effects.

Our main finding is that the Finnish experiment had no detectable employment effect in the first year of the experiment despite a large increase in employment incentives. The graphical analysis in Figure 2 shows that the monthly employment effect, measured as the share of individuals with days in employment, is consistently close to zero during the first year of the experiment. Regressions based on our primary outcome, which measure cumulative employment by a sum of yearly employment days, verify this finding, as the estimated employment effect of 1.5 days is far from being statistically significant. Taking into account the low level of employment among the controls and the precision of the estimate, the 95% confidence interval imply a fairly wide range from -5% to 11% for the relative change in employment. In the second year, the estimated employment effect of 6.6 days turns out to be statistically significant with the 95% confidence interval ranging from 2% to 15% for the relative change. Unfortunately, this improvement in the employment effect coincides with the introduction of the 2018 unemployment benefit reform, which is why it cannot be attributed to the basic income experiment alone. For that reason, we focus primarily on the first-year result, which are not confounded by the 2018 reform.

There is limited experimental evidence on participation elasticities that can be contrasted to our results. The closest comparison point is the Canadian Self-Sufficiency Project (SSP) experiment, which tested a generous and temporary earnings subsidy for full-time employment. During the eligibility period, the participation elasticity in the SSP was estimated to be 0.38 (Card and Hyslop 2005; Chetty et al. 2013). Based on the average participation tax rates reported in Table 1, the Finnish experiment resulted in a 67% increase in the net-of-tax rate for full-time employment. If the basic income receivers had reacted similarly to the SSP participants, this would have resulted in an increase of some 25% in the relative employment rate of the treated. A change of this magnitude is well outside the confidence intervals for the Finnish experiment, suggesting that the responsiveness of employment to financial incentives was lower in the Finnish experiment than in the Canadian. In fact, the upper bound for the employment effect in the first year of the Finnish experiment suggests that we can rule out any participation elasticities exceeding 0.16, assuming that the removal of active labor market programs (ALMP) in the treatment group had no negative employment effect.

The labor supply effects of intertemporal incentive changes have also been analyzed in some quasi-experimental studies. Several studies using variation on tax holidays in

Iceland and Switzerland report participation elasticities of a magnitude similar to that observed here (Martínez et al. 2021; Sigurdsson 2019; Stefansson 2019). The evidence is not conclusive, however, as Bianchi et al. (2001) found a participation elasticity of 0.42 when analyzing the tax-free year in Iceland. For permanent earnings subsidies, the evidence is more abundant. This brand of literature has used variation in the incentive structure provided by tax credit programs such as the EITC. In general, these studies have reported higher participation elasticities than the ones suggested above, although some studies also report lower elasticities that are in line with our findings (see for example Meyer and Rosenbaum 2001; Eissa and Hoynes 2006; Bastian 2020; Chetty et al. 2013 for survey; Kleven 2020 for a critical view).

There are several potential explanations why our results differ from those of the earlier studies. One lies in the differences in target groups. The SSP predominantly targeted single mothers who were long-term welfare recipients, while the EITC targeted large families as well as single mothers. These are the groups for which the changes in incentives were weakest in the Finnish experiment. Another plausible explanation relates to the phaseout regions of the subsidy. In the SSP experiment, the earnings subsidy required full-time employment and decreased with labor earnings; it thus provided the strongest incentives to accept low-paid full-time jobs. The required wage for the largest earnings subsidy in the Finnish experiment was above €1,700 per month, which is considerably higher than that in the case of the SSP or EITC. This might have been unattainable for many basic income recipients. In addition, our primary outcome, cumulative days in employment, includes both extensive and intensive margin responses in labor supply decisions. However, the changes in the intensive margin are likely to be negligible in our case, as the average changes in the monthly employment rates reported in Figure 2 are actually very close to the yearly changes in employment days (2.5% in 2017 and 7.6% in 2018). There are also other possible explanations, for example, differences in institutions, but the bottom line is that the labor supply of long-term beneficiaries in the Finnish experiment turned out to be less responsive than could be expected based on the majority of previous studies on employment subsidies or tax credit programs targeting poor families.

A novel feature of the experiment for a Nordic welfare state was that the treatment group received basic income payments regardless of their labor market status or job search efforts. Our results show that such freedom had only a modest impact on indi-

viduals' behavior. One year after the start of the experiment, nearly half of the persons receiving basic income still applied for unemployment benefits. In other words, they willingly participated in all the reemployment services, monitoring and reporting that the public employment services arrange for and requires of jobseekers. During the first year, two-thirds of the treated prepared an employment plan in a joint meeting with a caseworker. In doing so, they agreed to follow the sequence of targets set up in the plan aimed at increasing their job search efforts. In the end, the participation in ALMPs, accounted for 40% of unemployment benefit usage in the treatment group. For clarity, it should be pointed out that participation in an ALMP had no impact on either the treatment status or basic income payments.

The relevant question here is why so many people in the treatment group chose to remain clients of the public employment services. This finding stands in sharp contrast to the literature reporting an apparent dislike of mandatory reemployment services (Graversen and Van Ours 2008), low take-up rates of voluntary active measures (Behaghel et al. 2014), and modest employment effects of many ALMPs (Card et al. 2018). It is unlikely that unemployed persons value ALMPs more in Finland than they do in other countries. We believe that a more plausible explanation for the favorable attitude towards reemployment services lies in the benefit supplements that are paid during program participation. An average ALMP participant in the target group spent 145 working days in a program during the first year of the experiment, receiving a non-taxable expense compensation for 73 days and benefit supplements for 34 days (see Appendix Table C.1). Overall, these provided a 13.7% increase in the unemployment benefits. This may well create a large enough monetary incentive to outweigh the costs of leisure time lost during program participation. This would be the case at least for those who do not need the automatically granted compensation to cover travel costs.[6]

The Finnish experiment combined a large improvement in employment incentives with a possibility to avoid all reemployment services. Based on the evidence discussed above, both of these components may have economically significant opposing effects on employment and could cancel each other out in our evaluation results. However, the willingness to participate in reemployment services suggests that this is unlikely to

[6]The decision to participate in an ALMP is endogenously determined depending on the preferences of the jobseeker and counselor. A more detailed analysis on the role of ALMPs in remaining unemployed would require exogenous variation in the monetary incentives to participate that is not available in our setting.

be the case. If waiving the requirements for receiving unemployment benefits had no major impact on individuals' behavior, the improvement in monetary incentives could not have a considerable employment effect in the experiment either.

Similar results could have emerged if the treated had been unaware that they were receiving the basic income. However, there are several reasons why a widespread ignorance of the experiment seems improbable. After randomization, all treated persons were sent a letter clearly stating that the basic income would not be affected by work income. This resulted in an increase of over 10pp in contacts with the the Social Insurance Institution during the first months of the experiment (see Appendix Figure D.1). By the end of March, almost 40% of the treated had contacted the SII to receive guidance and make inquiries. The treated also received an additional benefit transfer when the first basic income payment was made on 9 January 2017. This differed from the day when regular unemployment benefits from December were due. Given the low income level of the target group, it is doubtful that these changes went totally unnoticed. Finally, the start of the experiment generated media interest, which was difficult to avoid (see Appendix Figure D.2).

6 Conclusions

This paper presents evidence based on a randomized experiment that provision of a guaranteed basic income has only a limited impact on employment prospects of long-term unemployed persons in an advanced country. The Finnish experiment sought to remove potential welfare traps that unemployed persons face. It pursued this aim by diminishing administrative barriers through a monthly basic income that was combined with a considerable improvement in the monetary incentives for employment. The new benefit type was fitted to the existing tax-benefit system, which resulted in a partial basic income model with an unaltered tax schedule. The impacts of this new benefit were studied by comparing the randomized treatment group to the control group, which continued to receive traditional social benefits.

Our results show that the experiment had minor employment effects at best. The 95% confidence interval of our first-year primary outcome estimate, measured in annual employment days, ranges from -2.3 to 5.4. We believe that this estimate is precise enough to rule out any economically meaningful employment effects that should

be achieved by the 23pp average decrease in the participation tax rates in the experiment. This is further supported by the fact that the labor earnings did not meaningfully change. Our findings also reveal that the treated were reluctant to leave the usual reemployment services and bureaucracy of unemployment benefits despite their receiving a benefit regardless of job search efforts. We attribute these findings to the interactions that the experimental basic income had with the existing benefit systems and employment services. In particular, our results suggest that Finnish labor market policy has perhaps become so generous that it actually locks in unemployed persons, encouraging them to wait for another active labor market program.

These findings point towards broader lessons. First, they show that in some cases improving monetary incentives for employment can be an ineffective policy tool for hard-to-employ populations, especially if the increase in incentives peaks at relatively high wage levels. These findings are particularly relevant for many European countries, where one sees labor market institutions similar to those in Finland in that they have a high level of long-term unemployment, relatively high minimum wages and extensive social safety nets.

Second, people with the most difficulties in the labor market appear to be surprisingly willing to engage with bureaucracy and reemployment services. Our results suggest that the current practice of filing unemployment benefit claims via the Internet has become so easy that people continue filing them even when they need not do so. If researchers want to find convincing evidence of the existence of bureaucratic traps, a more promising topic could be the benefit complexity that results from the interaction of different types of benefits.

Third, an active labor market policy that attracts participation with monetary supplements appears to counteract any employment-improving threat effects that active labor market programs may have. As job search efforts are inevitably reduced during participation in an active program, and many programs are found to be quite ineffective in enhancing employment prospects, there is a real possibility that what is a well-meaning policy may in fact exacerbate the unemployment problem.

Finally, our results underline the need for realism in the debate on universal basic income in advanced countries. The Finnish experiment failed to produce any sizable short-term employment effects despite offering larger improvements in employment incentives than any realistic nationwide policy could provide. For a basic income model

to be fiscally feasible, it has to be implemented through tax increases, which may have negative employment effects in the employed population. The same is to be expected if the basic income is set to a level at which it replaces all existing social benefits. Obviously, more randomized experiments are needed to gain causal evidence on the urgent question of how to shape the existing benefit system in advanced countries. Interventions that directly target specific problems of long-term unemployed persons might fare better than a basic income, at least in improving the target group's employment prospects.

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Online Appendix for 'Removing Welfare Traps: Employment Responses in the Finnish Basic Income Experiment'

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This appendix presents supplementary material to the paper analyzing the results of the Finnish basic income experiment. It comprises four sections. The first, Section A, studies the robustness of our main result by providing further evidence on the balance of the analysis groups and by showing that our regression estimate is not sensitive to changes in the definition of the outcome variable or to the inclusion of control variables. We also provide the complete OLS output for the main regression. Section B presents a heterogeneity analysis. Section C then analyzes the role of unemployment benefit supplements for active labor market programs and Section D discusses participants' awareness of the experiment.

A Robustness analysis

We start with Figure A.1, which shows trends in employment and the use of minimum unemployment benefits from 2016 to 2018. This figure supplements Figures 2 and 3 in the paper by providing past trends and presenting average days instead of shares of

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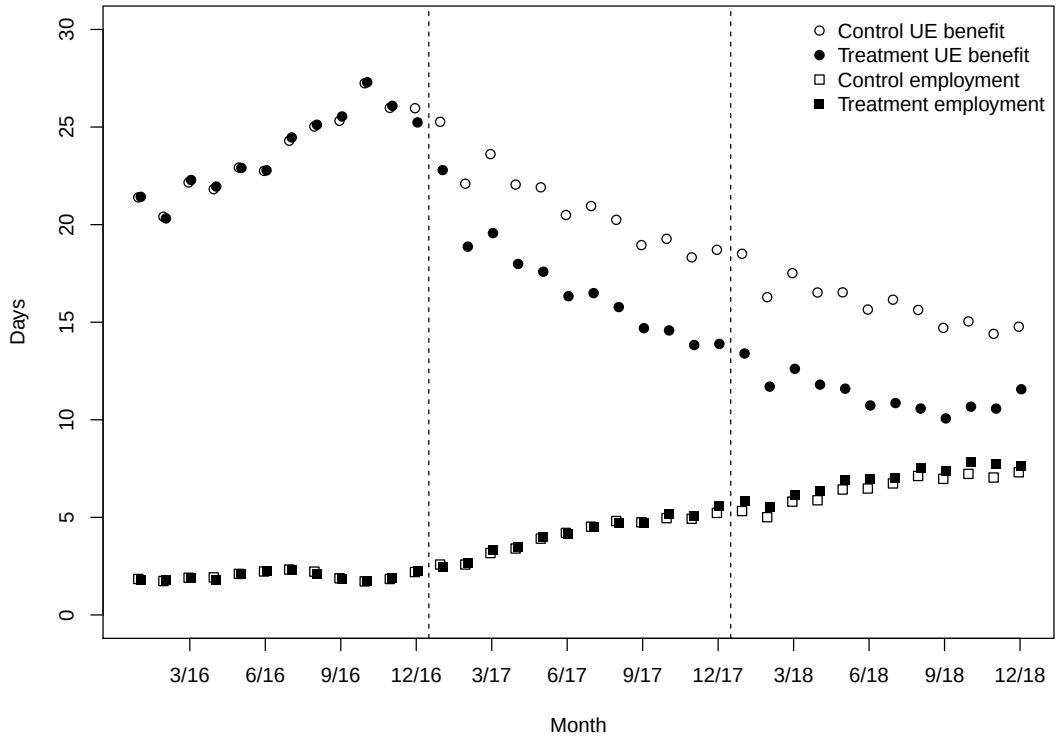

Figure A.1: Monthly trends in employment days and the use of unemployment (UE) benefits in 2016–2018.

those participants with unemployment days. In 2016, trends were almost identical for the treatment and control groups. The only exception to be seen is a small drop in the use of unemployment benefits by those in the treatment group in December, which is explained by the fact that these benefits were claimed in January 2017 and were thus already affected by the experiment. The number of days on unemployment benefits for both groups is high in 2016. Time on benefits increases over the year and peaks at 27 days in October. The target group was defined based on eligibility for benefits in October, with these paid out in November. For time in employment, the average was around 2 days per month throughout the year 2016. The number of employment days starts to increase slowly after randomization and reaches 5 days per month in October 2017. For 2017 and 2018, these trends are similar to those in Figures 2 and 3 in the paper.

Table A.1: Robustness analysis for the number of employment days.

	Control mean	Estim.	*p*-val.	Estim.	*p*-val.	Estim.	*p*-val.	Estim.	*p*-val.
Outcome definition									
2017									
Primary outcome	49.06	0.85	0.72	2.60	0.26	1.92	0.39	1.54	0.44
		(2.35)		(2.29)		(2.23)		(1.98)	
No wage threshold	77.09	1.89	0.51	3.90	0.17	3.01	0.27	3.35	0.16
		(2.88)		(2.82)		(2.74)		(2.38)	
2018									
Primary outcome	77.34	5.61	0.06	7.48	0.01	6.63	0.02	6.63	0.01
		(3.02)		(2.97)		(2.87)		(2.71)	
No wage threshold	104.92	6.37	0.06	8.40	0.01	7.38	0.02	7.86	0.01
		(3.33)		(3.29)		(3.17)		(2.96)	
Control variables									
Benefit type				X		X		X	
Background characteristics						X		X	
Benefit and employment histories for 2016								X	

Note: Background characteristics are gender, age, language, family type, region type, province (NUTS 2), level and field of education, and disability indicator. Benefit and employment histories for 2016 include employment, earnings, unemployment benefits and housing benefits. Robust standard errors are in parentheses.

Table A.1 shows a robustness analysis for days in employment. The results are robust for controlling different background characteristics, which is to be expected if randomization has been carried out successfully. Controlling for unemployment benefit type in November 2016 has the largest impact on the point estimate. This is not surprising as benefit type was the only variable found to be unbalanced after the randomization. Nevertheless, all the changes in the 2017 point estimate are far from being significantly different from zero or from the preferred specification including all controls. In the preferred specification for 2017, the standard errors are reduced by 15% when compared to the specification without any control variables.

Table A.1 also shows the sensitivity of our main result to the change in the definition of employment. For our primary outcome, we use a wage threshold of €23.74

Table A.2: Effect on annual earnings distribution.

	2017				2018			
	Control mean	Estim.	S.E.	*p*-val.	Control mean	Estim.	S.E.	*p*-val.
No earnings	0.5715	-0.0139	0.0094	0.1364	0.5000	-0.0245	0.0102	0.0158
€1–3,000	0.1128	0.0050	0.0069	0.4708	0.1043	-0.0049	0.0067	0.4645
€3,001–8,000	0.1064	0.0062	0.0069	0.3661	0.1014	0.0029	0.0068	0.6707
€8,001–15,000	0.1064	0.0062	0.0069	0.3709	0.1070	0.0133	0.0072	0.0662
$\geq$ €15,001	0.1029	-0.0035	0.0062	0.5740	0.1873	0.0133	0.0084	0.1126

Note: Control variables are unemployment benefit type, gender, age, language, family type, region type, province (NUTS 2), level and field of education, disability indicator, employment in 2016, earnings in 2016, unemployment benefits in 2016, housing benefits in 2016. Standard errors in the linear probability models are robust for heteroscedasticity.

for daily earnings (as specified in our pre-analysis plan). Only employment spells with daily earnings, calculated using the dates of employment contracts and yearly earnings related to the contracts, exceeding the threshold are included in the analysis. In the alternative definition, we relax the wage threshold to the extreme and include all employment spells with positive earnings. This leads to inclusion of zero-hour contracts, which may be of very long duration, but include only a single day with actual work. In the first year, removing the wage threshold increases the average days in employment by 57% in the control group. The treatment effect increases to 3.35 days in the specification including all controls. However, the estimate still remains insignificant.

Table A.2 explores the effect of the experiment on annual earnings distribution, enabling us in turn to study the possible impact of the irregular work in more detail. 57% and 50% of the control group had no labor income in 2017 and 2018, respectively. For others, we define earnings categories based on the earnings deciles in 2017. Using linear probability models, we estimate the effect of being in these earnings categories. In line with the result for the average earnings and days in employment, all of the estimates are small and insignificant for 2017. However, the point estimates reveal an interesting pattern. The probability of having no earnings decreases by 1.39pp and the likelihood of earning from €1 to €15,000 annually increases correspondingly. This indicates that irregular work became slightly more common in the treatment group but the earnings from such employment were low. In 2018, the probability of having no

earnings decreases significantly by 2.45pp and the basic income recipients seem more likely to be in the two highest earnings categories. Here, it should be pointed out that the €15,000 threshold for the highest earnings category is still less than 40% of the median earnings for a full-time employee in Finland in 2017. Overall, our main results are not sensitive to the wage threshold or other changes in definitions of employment spells.

To end this section, we provide the complete OLS output for our main regressions in Table A.3.

B Effect heterogeneity

The basic income was set to correspond to the net level of minimum unemployment benefits without supplements. The exclusion of child supplements implied that the changes in the employment incentives would be heterogeneous with respect to family type. Figure B.1 shows the use of unemployment benefits for single adults and couples with and without children. The graphs reveal that benefit use declined more for those without children, which is in line with the incentive effects. However, the use of unemployment benefits remains very common among different family types. Thus, the child supplements alone do not explain the high benefit take-up in the treatment group.

Table B.1 presents the results of the subgroup analysis for days in employment. Basic demographics, age, gender and education do not show large heterogeneity in 2017. The variation in the point estimates for the subgroups is well within their standard errors. The second year is otherwise similar, but the age group 25–44 years has a larger but insignificant estimate. For family and region type the differences are larger. Both variables are directly linked to the benefit levels. For couples with children, the employment effect is more positive and reaches significance in 2018. This is surprising given that the basic income provided less improvement for their employment incentives compared to childless households. On the other hand, the point estimates for the different region types follow the expected pattern. As housing costs are considerably higher in the capital region than in the rest of country, housing benefits have a stronger impact on effective marginal tax rates. This is realized in the negative point estimate in the capital region, while other regions show positive estimates.

To analyze the effect heterogeneity with respect to employment incentives more

Table A.3: OLS regression coefficients for days in employment (primary outcome).

	2017			2018		
	Estimate	S.E.	*p*-value	Estimate	S.E.	*p*-value
Intercept	34.84	1.05	<0.01	64.54	1.42	<0.01
Treatment group	1.54	1.98	0.44	6.63	2.71	0.01
2016 unemployment benefit type 2	6.16	0.84	<0.01	15.12	1.04	<0.01
Gender woman	0.70	0.46	0.13	0.05	0.62	0.93
Age 35–44	-7.85	0.58	<0.01	-14.48	0.78	<0.01
Age 45–59	-17.13	0.52	<0.01	-34.18	0.70	<0.01
Foreign language	-5.72	0.56	<0.01	-5.25	0.76	<0.01
Family type couple	6.48	0.67	<0.01	11.94	0.89	<0.01
Family type couple with children	12.89	0.63	<0.01	24.70	0.85	<0.01
Family type adult with children	5.55	0.63	<0.01	10.44	0.85	<0.01
Region surrounding capital	1.42	0.91	0.12	4.88	1.22	<0.01
Region other urban	-1.84	0.80	0.02	-1.01	1.06	0.34
Region rural	-2.70	0.86	<0.01	-2.66	1.14	0.02
Province Eastern	-2.54	0.77	<0.01	-2.71	1.03	0.01
Province Lapland	1.34	1.31	0.31	1.95	1.73	0.26
Province Western	2.54	0.59	<0.01	3.91	0.78	<0.01
Province Oulu	-0.23	0.84	0.78	0.15	1.13	0.89
Education level secondary	3.10	0.79	<0.01	8.94	1.08	<0.01
Education level tertiary	8.90	0.98	<0.01	20.85	1.33	<0.01
Education field 1	-0.79	1.00	0.43	-1.45	1.34	0.28
Education field 2	7.91	0.87	<0.01	8.94	1.17	<0.01
Education field 3	6.12	0.87	<0.01	7.97	1.19	<0.01
Disability	-12.86	0.47	<0.01	-25.39	0.64	<0.01
2016 employment days > 0	77.69	1.12	<0.01	65.93	1.30	<0.01
2016 earnings €1–1,500	13.88	0.90	<0.01	27.57	1.19	<0.01
2016 earnings €1,501–4,000	23.61	1.10	<0.01	33.75	1.38	<0.01
2016 earnings €4,001–8,000	34.10	1.28	<0.01	40.86	1.54	<0.01
2016 earnings $\geq$ €8,001	76.86	1.56	<0.01	67.41	1.81	<0.01
2016 unemployment days 241–364	-8.38	0.68	<0.01	-9.89	0.88	<0.01
2016 unemployment days 365–366	-8.92	0.61	<0.01	-7.02	0.85	<0.01
2016 housing benefits €1–2,500	-6.63	0.72	<0.01	-8.23	0.93	<0.01
2016 housing benefits €2,501–4,000	-9.52	0.60	<0.01	-16.77	0.81	<0.01
2016 housing benefits $\geq$ €4,001	-9.95	0.67	<0.01	-17.60	0.91	<0.01

Note: Education field 1 includes the humanities, natural sciences, agriculture and forestry. Education field 2 includes business, social sciences, health and services. Education field 3 includes engineering. Standard errors are heteroskedasticity-robust.

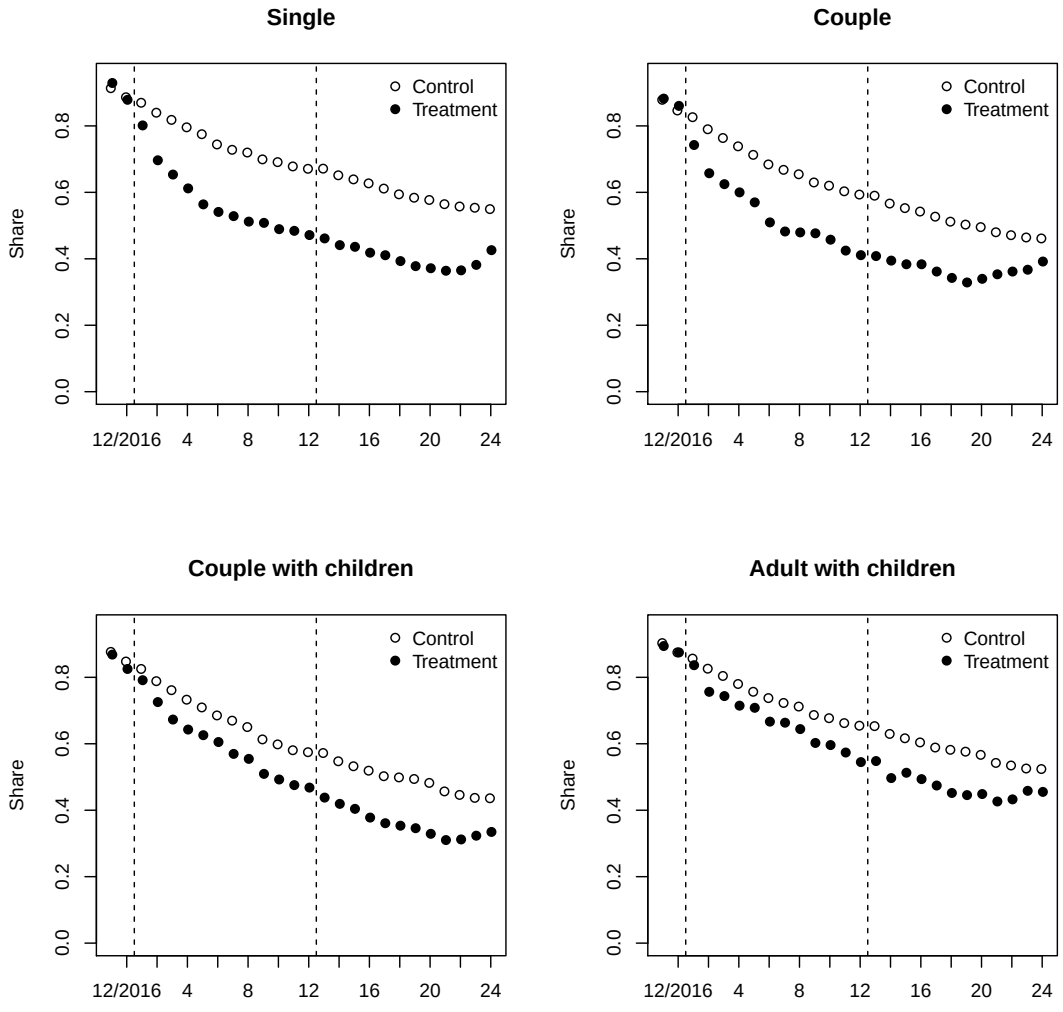

Figure B.1: Use of minimum unemployment benefits by family type between November 2016 and December 2018.

Table B.1: Effect on the number of employment days. Results for the primary outcome and subgroup analysis.

	2017			2018			
	Estim.	S.E.	*p*-val.	Estim.	S.E.	*p*-val.	N treated
Primary analysis							
	1.54	1.98	0.44	6.63	2.71	0.01	2000
Subgroup analysis							
Age							
25–34	1.18	3.66	0.75	5.51	5.01	0.27	670
35–44	2.96	4.15	0.47	9.75	5.64	0.08	549
45–59	0.85	2.71	0.75	5.39	3.73	0.15	781
Gender							
Men	2.31	2.61	0.37	6.46	3.67	0.08	1045
Women	0.69	3.01	0.82	6.81	4.01	0.09	955
Education							
Basic	-0.11	2.86	0.97	5.25	4.27	0.22	634
Secondary or higher	2.31	2.58	0.37	7.26	3.44	0.03	1366
Family type							
Couple with children	8.23	4.58	0.07	13.90	6.04	0.02	532
Single or couple	-0.38	2.39	0.87	2.10	3.31	0.53	1156
Adult with children	-2.74	4.65	0.56	10.98	6.69	0.10	312
Region type							
Capital region	-4.36	4.03	0.28	2.55	5.79	0.66	468
Other urban areas	3.13	2.91	0.28	7.88	4.03	0.05	886
Rural areas	3.63	3.62	0.32	7.85	4.72	0.10	646

Note: Control variables are unemployment benefit type, gender, age, language, family type, region type, province (NUTS 2), level and field of education, disability indicator, employment in 2016, earnings in 2016, unemployment benefits in 2016, housing benefits in 2016. Standard errors are heteroskedasticity-robust.

Table B.2: Effect on the number of employment days. Effect heterogeneity by the change in the participation tax rate (PTR) for monthly labor earnings of €2,000.

		2017			2018			
		Estim.	S.E.	*p*-val.	Estim.	S.E.	*p*-val.	N treated
Primary analysis								
		1.54	1.97	0.43	6.63	2.71	0.01	2000
PTR change								
Tertile	Decrease (%)							
1st	0–35.5	2.97	3.42	0.39	14.64	3.42	0.00	666
2nd	35.6–43.8	3.47	3.25	0.29	8.39	3.25	0.01	668
3rd	43.9–61.0	-1.82	3.59	0.61	-3.16	3.59	0.38	666

Note: Control variables are unemployment benefit type, gender, age, language, family type, region type, province (NUTS 2), level and field of education, disability indicator, employment in 2016, earnings in 2016, unemployment benefits in 2016, housing benefits in 2016. Standard errors are heteroskedasticity-robust.

directly, we estimate the employment effects by the changes in the participation tax rates (PTR) in Table B.2. The tax rates are simulated using Statistics Finland's SISU microsimulation model at the Social Insurance Institution of Finland.[1] The calculations are based on the observed benefit use of the full-time unemployed target population of the experiment in November 2016. To obtain PTR values for all individuals in the treatment group, the missing 20% of the values are imputed using the cell means of the variables that determine the benefit levels. These variables are gender, family type, number of children, use of social assistance and, most importantly, 20-quantiles of the rent accepted in the housing benefits. This provides 320 cell means that are exact matched for the missing 405 cases in the treatment group.

Table B.2 presents the effect heterogeneity by the PTR change in tertiles for a treated person who accepts a job with €2,000 monthly earnings. The point estimate for the employment effect turns out to be negative in the 3rd tertile in which a decline in PTRs was the largest. The other two tertiles with smaller changes in PTRs show positive employment estimates. This analysis suggests that the effect heterogeneity with

[1]The PTR calculations are based on Hämäläinen et al. (2019) and they are also discussed in Section 2.2 of this paper (Hämäläinen, K., O. Kanninen, M. Simanainen, and J. Verho, 2019. Perustulokokeilun ensimmäinen vuosi. VATT Mimeo 56.).

respect to incentive changes follows an unexpected pattern. In 2017, this heterogeneity is relatively small, and in 2018, it is slightly larger. However, the pattern is consistent with the subgroup analysis where couples with children had larger estimates than other family types. Couples with children typically have relatively high unemployment and housing benefits, which implies smaller decreases in PTR. It has to be noted, however, that these heterogeneity analyses lack power to detect differences between groups and these results should thus be interpreted with caution.

C Benefit supplements for program participants

Table C.1 examines how common different types of benefit supplements for participants in active labor market programs were among the target population. The first and third column include the whole target population of the basic income experiment, that is, individuals who were receiving unemployment benefits from the Social Insurance Institution of Finland (SII) in November 2016. The second and fourth column limit the sample to those who spent at least one working day in an active labor market program either during the year 2017 or 2018, respectively. Subsidized jobs are excluded here, as the participants in these programs are paid a wage instead of unemployment benefits.

The first column of Table C.1 reveals that an average person in the target population spent 78 working days, or 3.5 months, in active labor market programs in 2017. The third column shows that the time spent in programs declines by 9 working days during the second year of the experiment. Columns 2 and 4 show that around half of the target group participated in active programs on both years of the experiment, and that a program lasted over six months on average. As expense compensation is nontaxable, the participants' taxable equivalent supplements during participation periods were €1,017 in 2017 and €706 in 2018, when using the standard SII withholding tax rate of 20%. This is additional to the combined amount of regular unemployment benefit and child supplement payments of €7,600 that the program participants received in 2017, forming a considerable part of the total unemployment benefit.

Table C.1: Mean expense compensations and supplements paid to unemployment benefit recipients.

	2017		2018	
	All	ALMP participants	All	ALMP participants
Share of individuals, %	100	53.97	100	47.50
Days in ALMP	78.10	144.71	69.01	145.29
Days with expense compensation	39.58	73.19	24.49	51.17
Days with benefit supplement	18.47	34.12	11.64	24.22
Expense compensation (€)	369.65	682.79	226.24	472.21
Benefit supplements (€)	88.33	163.20	55.47	115.41

Note: Days refer to working days. Active labor market program (ALMP) participants exclude those in subsidized employment.

D Awareness of the experiment and media attention

One possible explanation for the small changes in benefit take-up, participation in public employment services and, ultimately, employment is that participants were simply not cognizant of the fact that they had been randomized into the treatment group of an experiment. We claim that this was unlikely, at least on a large scale. Figure D.1 shows that contacts asking for guidance and making inquiries increased significantly among the treatment group during the first few months of the experiment. Information letters were sent to the group members on 28 December 2016, which is likely to have prompted a higher contact rate. The contacts increased in December 2016 and the first three months of 2017, after which the difference between the groups becomes insignificant. This implies that many individuals in the treatment group knew about the experiment and asked for more information. In the case of other contact types, there were no significant differences between the two groups.

The basic income experiment was also widely discussed in the media when it was planned and implemented. Figure D.2 shows the Google searches in Finnish for "basic income" or "basic income experiment" versus "disability pension". There are spikes in early 2015, when the new government came into office. The government had written in its program that it would run a basic income experiment and the Google searches show a concurrent spike. The smaller spikes between 2015 and 2017 coincide with

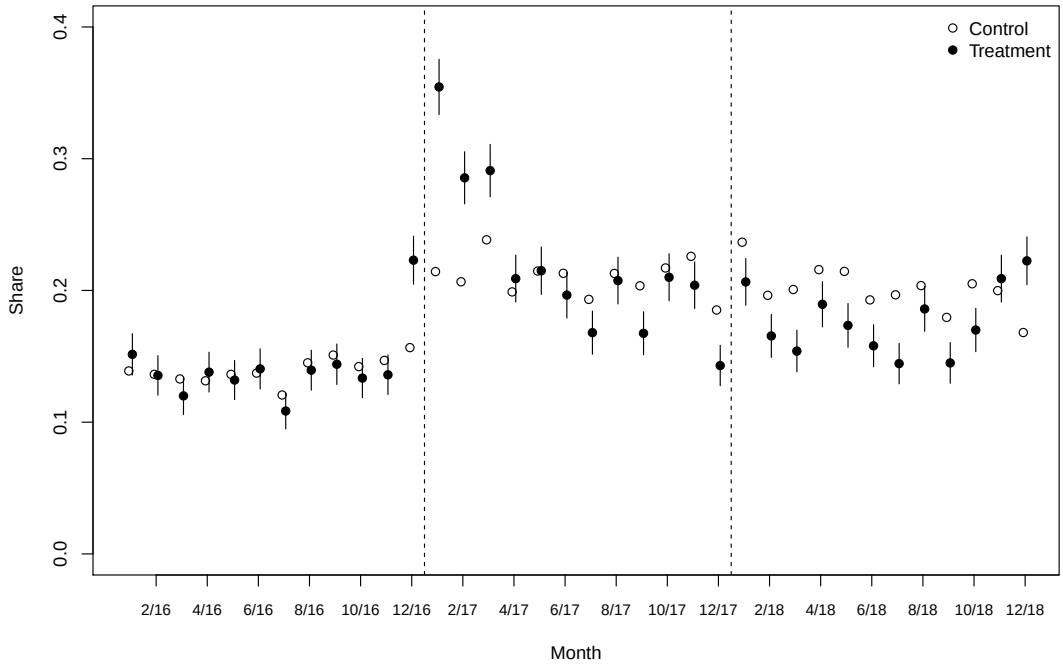

Figure D.1: Contacts with the Social Insurance Institution of Finland regarding social benefits in 2016–2018. The monthly shares of individuals with contacts registered as request for guidance and inquires.

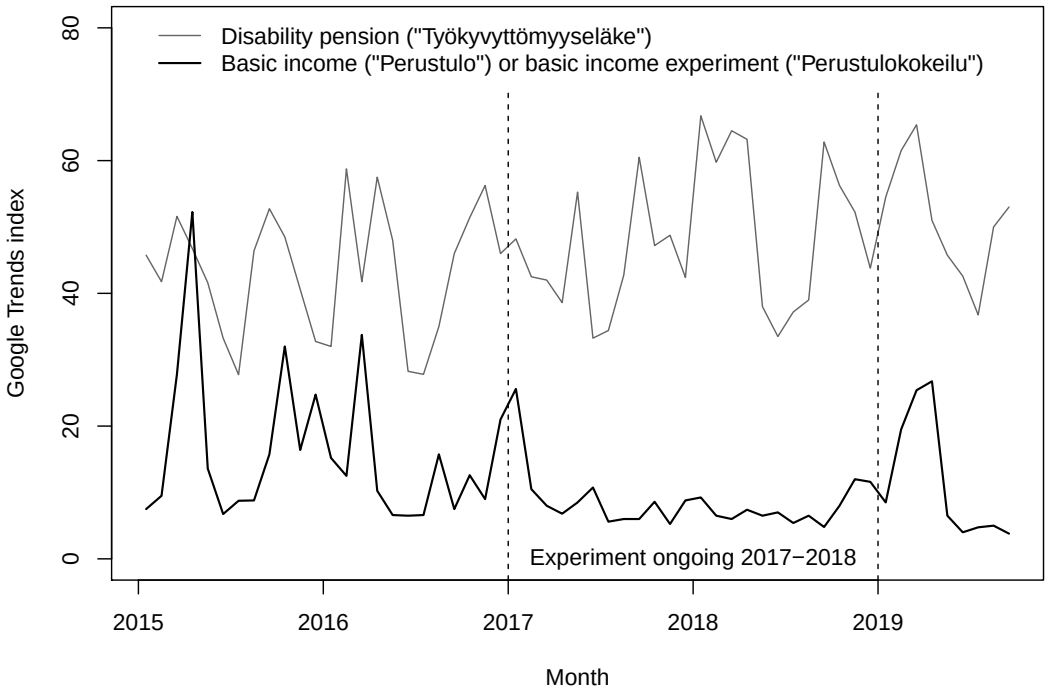

Figure D.2: Popularity of Google search queries for "basic income" [in Finnish]. The monthly Google Trend indices for "basic income" or "basic income experiment" versus "disability pension", included as a reference.

the public discussions about the implementation of the experiment. At the start of the experiment, there are clear spikes in December and January. The search frequency is not particularly high during the experiment, but there is a clear jump again in early 2019, when the preliminary results of the experiment were released. The index for disability pension acts as a baseline for search activity for social benefits. However, the disability pension has 132,000 recipients, which explains the higher level of search activity at all times except when the government program was announced.