# Finnish Basic Income Experiment: Pre-analysis plan

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Abstract

The Finnish Basic Income experiment is a large scale randomized field experiment organized by the Social Insurance Institution of Finland. The experiment began in January 2017 and it includes 2,000 unemployed workers receiving basic income. The basic income is 560 euros per month and it is paid unconditionally for two years. The experiment increased work incentives, removed most bureaucracy and allowed for an opt-out from active labor market policies. We will evaluate the effect of this basic income model using register-based data. Our main objective is to estimate the total effect and the effect of each mechanism on labor supply. Our secondary outcomes include earnings, income, participation in labor market programs, and health indicators.

## 1 Introduction

This plan outlines the hypotheses and empirical specifications to be used in the analysis of the Finnish basic income experiment. The foundations for the experiment were laid in the May 2015 government program. The newly selected government passed the organization of experiment to the Ministry of Health and Welfare. The ministry opened an open tender in August 2015 in which research groups were asked to draft a proposal for a basic income experiment with the main objective of studying its employment effects.

The Ministry of Health and Welfare chose the research group led by professor Olli Kangas to carry out the project. The authors of this plan participated in the work of the research group. Our research group published a mid-term report in April 2016 in which we proposed a randomized field experiment targeted to all low income individuals (see Kela 2016). We also recommended that the experiment includes several treatment groups with varying levels of flat taxes and basic income. Although our report was well received, the government insisted on starting a smaller scale experiment on a very tight schedule.

The scientific recommendations played a minor role in the final planning of the experiment. The ministry decided to pursue a very simplified version of basic income model that was targeted to unemployed workers only with no changes in the income tax schedule. In absence of tax changes, the experiment was also expensive to implement resulting in a substantially smaller sample size than recommended. The final report by Kangas et al. (2016) included critical remarks on the on the actual law proposal. Our main concern was the lack of power in the experiment. Despite the criticism, the law proposal was passed on to the parliament practically unchanged, and it was accepted on December 29$^{th}$ 2016.

The pre-analysis plan is outlined as follows. The next section presents the main aspects of the intervention. Because the implementation of the experiment is governed by law, all the details are described in the paragraphs of the law (No. 1528/2106).

In Section 3, we first define our primary outcome and main hypothesis. Then we list several secondary outcomes that we use to study different alternative mechanisms through which the experiment may affect individuals. As the power of the experiment is likely to be too low for multiple hypothesis corrections, the secondary outcome analysis will not provide confirmatory evidence.

The design of the sampling is described in Section 4. We also provide simple power calculations that indicate the minimum detectable effect to be around 11% increase in employment, and show the first balancing tests. Section 5 presents the econometric specification for the intention to treat analysis. The objective of this analysis is to conduct a straightforward evaluation of the experiment. The next two sections focus on data collection and study timeline. The last section discusses the 2018 reform in the unemployment benefit system that may have implications for the evaluation of basic income experiment. Preliminary plans for unravelling labor supply estimates are discussed in Appendix.

# 2 Intervention

The basic income experiment is a two-year experiment where participants receive an unconditional 560 euros monthly allowance paid by the Social Insurance Institution of Finland (Kela). The income tax schedule remains unaffected which means that monetary incentives for unemployed persons to find a job increase dramatically through increased net wages. However, an increase in net income might attenuate some of this effect through the income effect. The experiment also removes most of bureaucracy related to unemployment benefit applications, and allows the participants to opt out from any activation measures provided by labor administration.

The participants receive the basic income on the 2$^{nd}$ banking day of every month. The 560 euros basic income is non-taxable but it is deduced from other social benefits so that the income level remains roughly the same while being out of work. Social benefits based on household income are adjusted only if households' income changes during the experiment. Among household income based benefits, effective marginal tax rates are mainly affected by housing allowance which means that the employment incentives have regional variation depending on housing costs.

Basic income has a varying effect on employment incentives also through the presence of children in a family. The Ministry decided to exclude child supplements from basic income which means that participants with children under the age of 18 lose 90–170 euros monthly, depending on the number of children. To compensate this loss the participants may apply for unemployment benefits on top of basic income in which case they also face regular benefit rules.

As some social benefits depend on household income the changes in monetary incentives for employment are best analyzed at the household level. Below we present two such illustrations based on microsimulation as they were reported in the mid-term report by Kangas et al. (2016). The first example reports a case where the experiment results in a large improvement in incentives across the relevant earnings

distribution, while the second example shows a person whose incentives are only modestly affected by the experiment.

Figure 1 presents household disposable income for a single person who pays a low rent. For these types of participants, basic income increases the disposable income at earnings levels exceeding 300 euros per month. The relative growth peaks at around 35% with earnings of around 2,000 euros, after which the relative impact starts to decline owing to the flat change of 560 euros per month in disposable income. For these types of persons the experiment creates a very large improvement in employment incentives. Furthermore, the relative change peaks at earnings level that is accessible considering the target population's employment prospects.

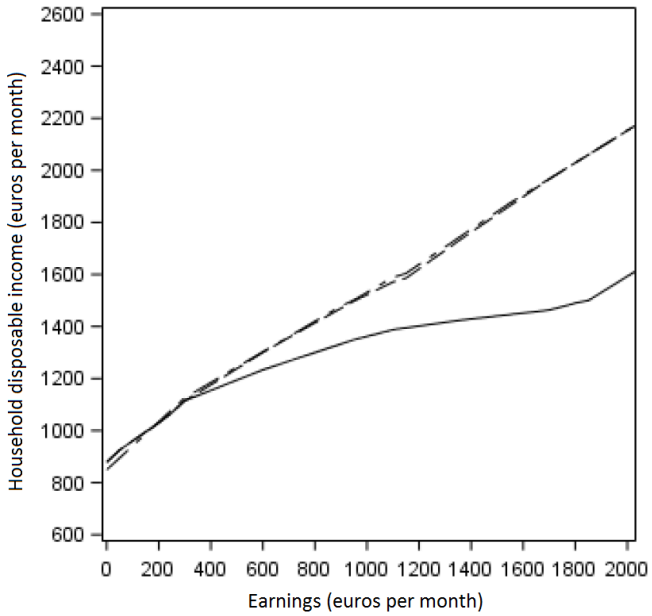


Figure 1. Household disposable income by earnings for a single person household (solid line is regular benefits, dotted line is basic income with UI benefits, dashed line is basic income without UI benefits, source: Kangas et al. 2016).

Figure 2 reports changes in disposable income at different levels of earnings for a single-parent household with two children. The family's housing costs are assumed to be similar as in the suburban region surrounding the capital city of Helsinki. For this person the regular unemployment benefits exceed the level of basic income at low income levels due to missing child supplements. When monthly earnings are less than 800 euros, the family has monetary incentives to apply for unemployment benefits in which case they are subject to the same benefit rules as the control group. The relative increase in disposable remains below 10% for earnings between 800 and 3,000 euros because single-parents remain eligible for housing allowance and adjusted unemployment benefits up to median earnings.

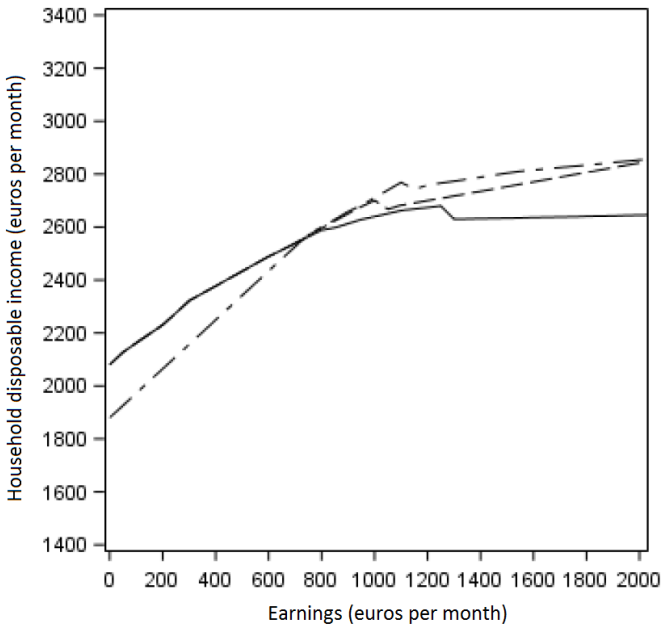


Figure 2. Household disposable income by earnings for a single-parent household with two children (solid line is regular benefits, dotted line is basic income with UI benefits, dashed line is basic income without UI benefits, source: Kangas et al. 2016).

# 3 Outcomes and hypotheses

## 3.1 Primary outcome

Our primary outcome is employment, on which the basic income has an ambiguous impact, a priori. Employment may increase through two transmission mechanisms (Table 1). First, the basic income improves incentives to accept job offers by reducing effective marginal tax rates. However, some of the effect of the higher wages might attenuate through the income effect. Second, any unconditional benefit removes bureaucratic traps that may increase willingness to accept temporary job offers. The unconditional basic income may equally well have a reverse impact on employment. The level of basic income equals previous unemployment benefits for many treated persons, so they do not have to register as a job seeker to receive social benefits. This also means that these individuals will not be exposed to any activation measures, monitoring or screening by employment agencies; neither will they face any threat of benefit sanctions for noncompliance. If these policies speed up job finding the removal of them is likely to reduce employment.

Table 1. Hypothesized effects of the treatment.

| Mechanism | Hypothesized effect on labor supply |
|---|---|
| Reduced effective marginal tax rates | Ambiguous (income effect vs. substitution effect) |
| Removal of bureaucratic traps | Increases labor supply |
| Removal of activation measures, sanctions, monitoring and screening | Reduces labor supply |

To explore the employment effects of basic income, our primary outcome is defined as days spent in non-subsidized employment offering minimum daily salary of €24.84 over the period of Nov 2017–Oct 2018. Registers do not contain actual working hours, so we will use a lower limit on earnings instead. This effectively removes from our primary outcome e.g. zero-hour contracts with no working hours. The salary requirement derives from the employment condition for claiming earnings-related unemployment benefits. Employment spells with the minimum of 18 weekly hours and the monthly wage of €1 187 accumulate the employment condition. This corresponds to a minimum hourly compensation of €6.9, and the minimum working hours of 3.6 per day. Employment days are counted for a period of one year that excludes the last two months of the experiment when the treated may anticipate the ending of the experiment. This also gives time for the treated to adapt to the idea of receiving basic income at the beginning of the experiment, and change their behavior accordingly.

Data for our primary outcome will be collected from two official registers. Finnish Centre for Pensions maintains the register that covers all registered employment spells in Finland. This information includes the starting and the ending dates of employment spells together with received salaries. To separate publicly sponsored jobs from non-subsidized employment we will employ the register maintained by the Ministry of economic affairs and employment that includes the starting and ending dates of all subsidized jobs that employment agencies provide for unemployed persons. Data is of very high quality as people's statutory pensions, wage subsidies paid for firms, and unemployment benefits are based on these registers.

The primary outcome is further analyzed by exploring employment dynamics over time. This might be relevant as previous studies on negative income tax experiments report that the impacts of the experiment tend to change over time (Burtless, 1986).

## 3.2 Secondary outcomes

Our secondary outcomes capture different mechanisms through which basic income may affect individuals' well-being. Taxable income has to be included among the secondary outcomes as it supplements our primary outcome. One hypothesis on the functioning of basic income is that it increases self-employment by enabling to start-up new businesses. Although our primary outcome includes self-employment, it may not capture all aspects of it. The pension contributions of self-employed persons are regulated by the Self-Employed Person's Pensions Act. The insurance is obligatory for a self-employed person if business operations have been ongoing for at least four months, and the estimated earnings from work amount to at least €7,557.18 per year (2016). To increase the coverage of pension contributions among self-employed persons, the pension contribution is set to be completely tax-deductible. Regardless of this, it is likely that

some self-employed persons, who do not have to pay obligatory pension contributions, choose not to pay them on voluntary bases. Income received as a self-employed person is, however, taxable and thus it will show up in the income register maintained by the tax authority.

Other secondary outcomes can be divided into two groups. The first set of outcomes consists of labor earnings, benefit take-up rate, participation in active measures and entrepreneurship that are closely related to our primary outcome. The second set of outcomes explores different choices that individuals may make after receiving basic income, and potential health effects that basic income may have. The latter is motivated by the idea that the knowledge of unconditional and secure income may ease economic stress that may also show up in the usage of drugs.

The complete list of secondary outcomes and their information source is the following:

1. Annual earnings (registers maintained by tax authority)
2. Annual income (register maintained by tax authority)
3. Benefit take-up rate (register maintained by the Social Security Institution of Finland)
4. Participation in active labor market programs (register maintained by the Ministry of Economic Affairs and Employment)
5. Entrepreneurship (register maintained by Finnish Centre for Pensions)
6. Enrolment in an educational institution (Register on students maintained by Statistics Finland)
7. Being out of labor force (various register information from the above sources)
8. Usage of antidepressant drugs and other drugs (the Drug Prescription Register maintained by the Social Insurance Institution of Finland)
9. Internal and external migration (Register maintained by Statistics Finland)
10. Having a child / marital status

# 4 Experimental design

## 4.1 Sampling frame

The target population of the experiment was specified by the Act on Basic Income (No. 1528/2016). The Act was proposed by the government on October 20th and accepted by the parliament on December 29th 2016. The proposition was loosely based on the recommendations of the research group (Kela 2016). The key adopted elements were randomization and mandatory participation. Otherwise, the experiment differed substantially from the recommendations of the research group.

The Act on Basic Income restricted the experiment by including only persons who had received unemployment benefits paid by the Social Insurance Institution of Finland (Kela) in November 2016. Youth and persons near the retirement age were also excluded, so the target population consisted of individuals between 25 and 58 years of age on December 1st 2016. In addition, the Act lists altogether 11 different types of benefits that result in the exclusion from the experiment, but these restrictions have only a minor impact on the size of the target population.

The size of the treatment group was set to 2,000 individuals. Randomization was carried out by Kela on December 15th. The SAS code used in the randomization was published by Kela (included in Appendix 1). Based on preliminary statistics we received from Kela (see Table 1), the size of the target population was 175,222 individuals. After sampling 2,000 individuals into treatment group, this leaves 173,222 individuals to be potentially used as a control group.

## 4.2 Power calculations

The design of the experiment was primarily based on administrative discretion. However, power calculations were conducted during the planning process, mainly to persuade lawmakers to increase the power of the experiment. Below we present the power calculations as they were carried out during the planning phase, and update them for the primary outcome variable set up in this plan.

The original power calculations were published in the research report by Kangas et al. (2016). The calculations were based on the employment rate of 30% in the target population after the follow-up period of two years. Given the treatment group of 2,000 individuals and a control group of 20,000 individuals, the minimum detectable effect with 80% power is 2.9 percentage points in the employment rate.

In this analysis plan, we propose days in employment between November 1st 2017 and October 31st 2018 as our primary outcome. To update power calculations, we utilized past data on unemployment (from Kela) and employment (from Finnish Centre for Pensions) spells to estimate the variance of our primary outcome variable. Using available microdata from November 2011 to define the target group, and data on 2012–2013 for calculating days in employment, the average days in employment turned out to be 78 days with the standard deviation of 131.

The minimum detectable effect for the primary outcome with 80% power and 5% significance level is 8.6 days. This corresponds to an 11% increase in employment days in the treatment group. These estimates should, however, be considered as tentative since employment prospects and the composition of long-term unemployed individuals vary over time. The standard deviation of days in employment is likely to be more stable. Also, adding controls e.g. for work history could lower the minimum detectable effect.

## 4.3 Balance and attrition

To gain the first insight on group balance we asked Kela for information on three background variables after randomization (Table 2). Chi-squared tests show no significant difference between the treatment and the control group with respect to gender or age distribution (p-values 0.83, and 0.25, respectively). However, the distribution of unemployment benefit type differs significantly between the two groups with the p-value of 0.0021. The labor market support recipients are overrepresented in the treatment group while the unemployment allowance recipients are underrepresented. This finding is somewhat worrying, but based

on current information, and double checking with Kela, we believe that randomization was conducted correctly and the benefit type distribution is imbalanced simply by chance. At the minimum, the observed difference calls for controlling for the unemployment benefit type in all analyses.

Table 2. Preliminary statistics (source: Kela).

| | Treatment | | Controls | | Target population | |
|---|---|---|---|---|---|---|
| | N | % | N | % | N | % |
| LM support | 1,743 | 87.15 | 146,599 | 84.63 | 148,342 | 84.66 |
| UE allowance | 257 | 12.85 | 26,623 | 15.37 | 26,880 | 15.34 |
| Women | 955 | 47.75 | 82,250 | 47.48 | 83,205 | 47.49 |
| Men | 1,045 | 52.25 | 90,972 | 52.52 | 92,017 | 52.51 |
| 25-34 years | 604 | 30.20 | 55,675 | 32.14 | 56,279 | 32.12 |
| 35-44 years | 570 | 28.50 | 48,248 | 27.85 | 48,818 | 27.86 |
| 45-58 years | 826 | 41.30 | 69,299 | 40.01 | 70,125 | 40.02 |
| Sum | 2,000 | 100.00 | 173,222 | 100.00 | 175,222 | 100.00 |

The receivers of labor market support recipients have typically worse labor market prospects than the unemployment allowance recipients, so the possibility remains that there are other significant differences also in other background characteristics. As we do not have access on actual research data, we cannot examine group balance any further at this stage. After receiving data, we will conduct similar balancing checks on other relevant demographic characteristics and labor market history variables, especially on those variables that are likely to be related to the benefit type.

Attrition is not likely to be a major problem in this experiment. The follow-up is carried out using administrative register data, which removes the possibility of nonresponse. It is also beneficial for evaluation that participation is mandatory which prevents dropping out of the experiment. However, the law lists several possibilities in which participants cease to receive the basic income. Because these events can be tracked and many of them are decisions that can be clearly affected by the experiment, we do not consider them as attrition but as outcomes of the experiment. We will report pre-treatment characteristics for the participants who are lost to study any possible imbalance in the sample caused by attrition.

# 5 Econometric specification for ITT analysis

For primary and secondary outcomes, our main specifications will be the following

$$Y_i = \alpha + \delta Tr_i + \varepsilon_i$$

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

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 $\varepsilon_i$ summarizes the unobserved factors.

Adjusting for the covariates is not required for consistency as randomization makes the treatment status exogenous. It can, however, increase the precision of the estimated treatment effect by reducing its standard error.

The vector of covariates, $X_i$, will be selected to include variables that are predictive of the outcome. As the experiment is based on randomization, we do not expect to find any correlation between the treatment status and observed characteristics. As noted above, this might not be the case with the type of benefit so it belongs definitely among the covariates. Other control variables include age, gender, children, marital status, education, labor market history and region of residence, at the minimum. At this stage, we treat the list of covariates as an incomplete one to which other covariates may be included. These decisions can be made only after we actually receive research data, and know the exact content of it.

We will use linear regressions in estimating the parameters $\alpha$, $\beta$ and $\delta$. In cases where the outcome variable is binary, we use heteroscedasticity robust standard errors. In our primary analyses, we will follow the intention-to-treat principle and estimate the effect of basic income as assigned.

It is likely that the sample size is too small to carry out proper examinations of heterogeneous treatment effects. We will, however, try to explore if the impacts of basic income vary among different family types, benefit types, gender, non-mothers vs. mothers and labor market history. We will also study the time-dynamics of the possible effect by studying the evolution of the outcomes at a monthly and quarterly level. We will look at mechanisms as explained in Appendix 2.

# 6 Data collection

The act on basic income experiment obligates the Social Security Institution of Finland (Kela) to implement the experiment. As one part of implementation Kela has stored the social security numbers of all individuals who belonged to the target group. These personal identifiers will be used in combining information from various official registers. Some data is automatically collected by Kela in the benefit register that it maintains. Kela then supplements the benefit register by sending the list of social security numbers of target group individuals, without specifying the treatment or control group status, to other register authorities. Other register authorities will then send required information to Kela where the personal identifiers will be encrypted, and the key stored, before handing over the data set for evaluation.

We asked for preliminary data that would include all target group members during the pre-experiment years 2005-2016 without identifying their treatment status to be handed to us in the mid of August. Our aim was to ensure the data quality and to prepare the variables to be used in the analysis before receiving actual research data. As it happened, this data collection was delayed and the data were not made available for researchers until 4th of November 2018.

The first version of the unblinded research data with the treatment identifier was scheduled to be delivered on 15th of November. The main part of this data delivery is currently agreed with Kela on 20th of November. These data include the employment records from the Finnish Centre for Pensions for the year 2017 but the delivery of the tax data for the year 2017 is delayed further. The research data that include the outcomes for the second year of the experiment are scheduled to be delivered on 15th of November 2019.

In addition to the collection of register data, Kela organized a survey for all the participants of the experiment and a group of individuals of in the control group. The telephone interviews started on 17th of November 2019 and the interviews should be ready on 17th of November 2019. Currently, our research group does not have detailed information about the survey but we hope to utilize the data at a later stage.

# 7 Study timeline

The timeline is described in Figure 3. The first official mention of the study is in the government program of the Finnish government, published in May 2015. A research team was commissioned to make suggestions and preliminary analysis for a possible experimental setup. After the research team published the mid-term report in May 2016, the Act was mostly prepared independently from the research team work. The experiment will be carried out during 2017–2018. Data collection and analysis will start at the end of 2018. The evaluation covering both treatment years will take place in 2020.

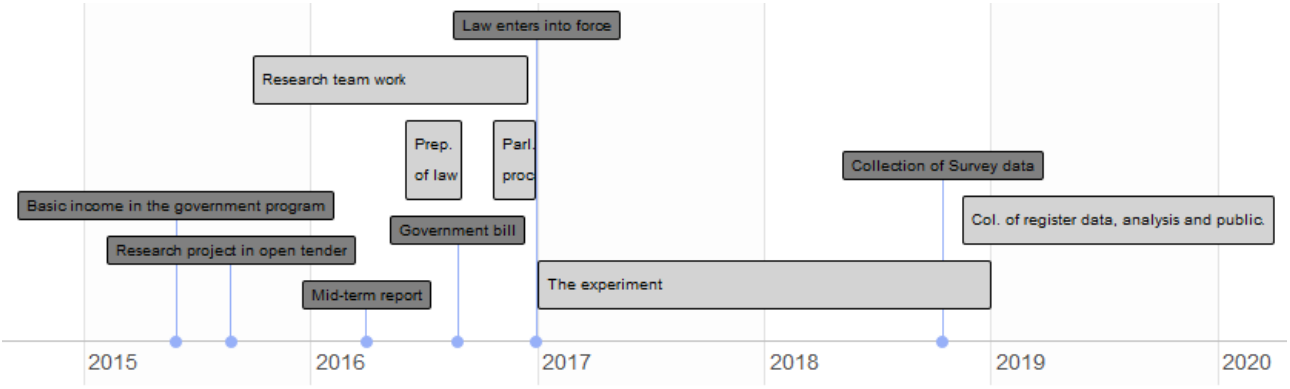


Figure 3. Study timeline.

# 8 Introduction of the active model

In the late 2017, the Finnish government announced a reform in the unemployment benefit system which reduces the benefit level by 4.65% (i.e. value of one day's benefit) for unemployed workers who do not participate in active labor market programs or have earnings from part-time employment. The act on this so called active model was accepted in December 2017 and the monitoring started in January 1st 2018.

The activity of the unemployed person is monitored either by Kela or by an unemployment fund, if the worker receives earnings-related unemployment benefits. The criteria for activity are monitored in periods of 65 days. The required level of activity is participation for at least five days in employment services organized by PES or employment for at least 18 hours in the period.

The adoption of the activity model reduces the value of unemployment benefits and, therefore, is expected to reduce incentives to apply for benefits and possibly enhances exits from unemployment. However, it has

not been documented yet, how strictly the activity is monitored or the sanctions are implemented in practice.

The control group of the basic income experiment is affected by the reform fully for those who continue receiving unemployment benefits in the second year of experiment. The treatment group is affected to the extent the participants are unemployed and have decided to apply for unemployment benefits on top of the basic income. This is expected to be more common among those with children.

# Appendix 1

Sampling code published by the Social Insurance Institution of Finland on December 1st 2016 (http://www.kela.fi/perustulokokeilun-otantakoodi):

```
data _NULL_;
 siemen= int(%sysfunc(TIME())) ;
 call symput('siemen',siemen);
run;
%put &siemen;

proc sort data=perus;
 by henro;
run;

proc surveyselect data=perus method=srs
     n=2000 seed=&siemen
     out=otos;
run;
```

# Appendix 2 Analysis of mechanisms

When analyzing mechanisms in the experiment, we are able to exploit the fact that every individual faces a slightly different set of pre-experiment parameters in their tax and benefit system due to municipal-level differences, family characteristics or individual differences. We can use the Finnish microsimulation model (SISU) to calculate the exact starting values in the parameters. These pre-experiment differences allow us to, in principle, distinguish different mechanism in the experiment.

## Labor supply

Standard labor economics shows that higher unearned income reduces labor supply and higher marginal wages increase labor supply. Ex ante, we expect that a reduced burden of bureaucracy has a non-negative effect on labor supply. We also expect the loss of conditionality of benefits to reduce labor supply. The whole sample was in principle administered the same treatment. Here, we discuss how those four effects could be separated by exploiting heterogeneity in the starting values.

Let labor supply in working hours or days per month $h = h(w, y, C, B)$, where the arguments of the labor supply function are wage, unearned income, conditionality of benefits and burden of bureaucracy. All other things equal, the effect of an exogenous change in $\{w, y\}$ can be written as $dh = \frac{\partial h}{\partial w}dw + \frac{\partial h}{\partial y}dy = \eta_w^c \Delta t h + \eta_y \frac{h\Delta I}{y}$, where $\Delta I$ is the counterfactual effect the treatment would have had on net incomes given $hw$, the pre-treatment earnings, $\eta_w^c$ is the compensated wage elasticity and $\eta_y$ is the income elasticity.

Now, let us add the treatments on benefits (*C*) and bureaucracy (*B)*. In this experiment $dC = dB \equiv dTr$. Presented in terms of relative change in labor supply, we propose the following econometric specification:

$$\frac{dh}{h}_i = \alpha + \eta_w^c \Delta t_i + \eta_y \frac{\Delta I}{y}_i + \left(\frac{\partial h}{h\partial C} + \frac{\partial h}{h\partial B}\right) dTr_i + X_i'\gamma + \varepsilon_i,$$

where $\alpha$ captures the time trend and any effect in variation in covariates that is common to the treatment and control groups. $X_i$ is a set of pre-treatment controls.

We use the control group to control for the effect of the time trend, since for them $\Delta t = CE = dTr = 0$. We can also exploit the control group to control for the effect on covariates to the extent that it is not caused by *dTr*. The estimated effect of *dTr* has to be interpreted as the total derivative of *dTr*, since being treated can affect other covariates through some features of the reform or through the Hawthorne effect, i.e., simply by virtue of being treated. The control group's *X*'s are unlikely to be affected by the John Henry effect, i.e., the effect of not being treated, since there was no actual announcement made for this group. However, the estimated effects of $\Delta t$ and $\frac{\Delta I}{y}$ are more likely to be the partial derivatives, since it is less likely that the confounding effects would specifically correlate with these variables.

The econometric specification can also be presented in absolute changes in *h*:

$$dh_i = \alpha + \delta_w^c \Delta \mathrm{t_i} w_i + \delta_y \Delta I_i + (\delta_C + \delta_B) dTr_i + X_i' \gamma + \varepsilon_i.$$

The latter specification might be preferable, since some individuals have an *h* of zero. To differentiate these effects, we try to exploit the fact that pre-treatment these variables had different values. However, with out small sample size, it might turn out to be impossible to find enough variation for such analysis of mechanisms.

## Cost of bureaucracy

Many benefits do not have a 100% takeup. It might improve efficiency to impose restrictions, conditions and costs on benefits in the form of in-kind transfers or even ordeals (Nichols, Smolensky & Tideman, 1971; Nichols & Zeckhauser, 1982; Ravallion, 1991). Imposing external costs might have an effect on allocation through targeting and incentivizing to poverty-reducing investments (Beasley & Coate, 1992). One such cost is the subjective cost of applying for benefits.

In this experiment, many treated individuals can improve their benefits by filling the regular benefit applications. We can measure the monetary cost of not applying, allowing us to estimate the cost of bureaucracy. Low-earning individuals with children face a significant monetary incentive to apply for higher benefits. The more children and the lower the potential earnings level, the higher the incentive. For example, a single parent of two children and no earnings will forego around 200 euros in lost benefits if they do not apply for the standard benefits. This feature of the experiment makes it more difficult to assess the effects on work incentives, but allows us to study the cost of bureaucracy to the participants.

The immediate monetary cost of bureaucracy, *B(C, Y)* is measurable and based on the number of children (*C*) and earnings (*Y*). We can control for *C* and *Y*, but *Y* is endogenous, restricting the inference. We propose to estimate the effect with the following linear probability model specification

$$Takeup_i = \alpha + \delta B_i + \beta_1 C_i + \beta_2 Y_i + X_i' \gamma + \varepsilon_i,$$

where $X_i'$ represents control variables. It is necessary to control a wide set of background variables, since $Y_i$ is not exogenous. However, we propose to instrument $B_i$ by pre-experiment earnings or predicted wage, $\hat{Y}_i$. Our exclusion restriction is that $\hat{Y}_i$ does not have a direct effect on $C_i$.

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