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. We will evaluate the effect of this basic income model using register based data. Our main objective is to estimate the effect 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. To understand the experimental setting, it is first useful to discuss the planning process briefly. After inclusion of a basic income experiment in the May 2015 government program, the organization of the experiment was passed on to the Ministry of Health and Welfare. The ministry ordered a research project on the topic through an open tender in August 2015.

The aim of the research project was to draft a proposal for a basic income experiment. The objective of the experiment was clearly specified as to study the employment effects of basic income model. The Ministry of Health and Welfare chose a 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). The proposition included 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 in extremely tight schedule.

The scientific recommendations played a minor role in the final planning of the experiment. The ministry decided to pursue onward with 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 which led to a substantially smaller sample size. The final report by Kangas et al. (2016) included critical remarks on the on the 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 29th 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. Because we consider the power of the experiment 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. We also show the first balancing tests that were provided to us after randomization.

Section 5 presents the econometric specification we plan to use in the intention to treat analysis. The objective of this analysis is to conduct a simple evaluation of the experiment. In Section 6 we discuss plans to conduct additional analysis to explore the mechanisms in more detail. In particular, we specify an econometric specification to estimate labor supply elasticities. The last sections focuse on data collection and study timeline.

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 employment increase dramatically. In addition, the experiment removes most of the bureaucracy related to unemployment benefit applications and it allows opting out from active labor market programs.

The participants receive the basic income every 2nd banking day of the month. The 560 euros is non-taxable but it is deduced from other social benefits the participants receive. However, the housing allowance is paid based on household income and, therefore, it is affected only if households' income change during the experiment. The housing allowance has a substantial impact on the effective marginal tax rates which means that the employment incentives have regional variation depending on housing costs.

The level of the basic income was set to correspond to the level of net minimum unemployment benefits. For a typical participant, the basic income removes the need to apply for unemployment benefits. However, the basic income lacks child supplements that are included in unemployment benefits. Participants with children under the age of 18 lose 90–170 euros monthly, depending on the number of children, unless they apply for unemployment benefits on top of the basic income. If participants decide to apply for unemployment benefits, the regular benefit rules are applied.

The changes in monetary incentives for employment need to be analyzed at the household level because the housing allowance is an important benefit on low income levels. The analysis of household disposable income is best done using a microsimulation model. Next we illustrate the incentive changes using two simulated examples from a report by Kangas et al. (2016). The first example is a case where the experiment improves incentives strongly across the relevant earnings distribution, while in the second example the experiment improves incentives only modestly.

Figure 1 presents household disposable income for a single person household with a low rent. For these types of participants, the disposable income increases with earning over 300 euros per month. The relative increase peaks at around 35% with earnings of around 2,000 euros, after which the relative impact declines since the change in disposable income remains flat at 560 euros per month. Thus, in this case the experiment creates very large incentives for employment and the relative change is the largest for an earnings level that is relevant considering the target population's employment prospects.

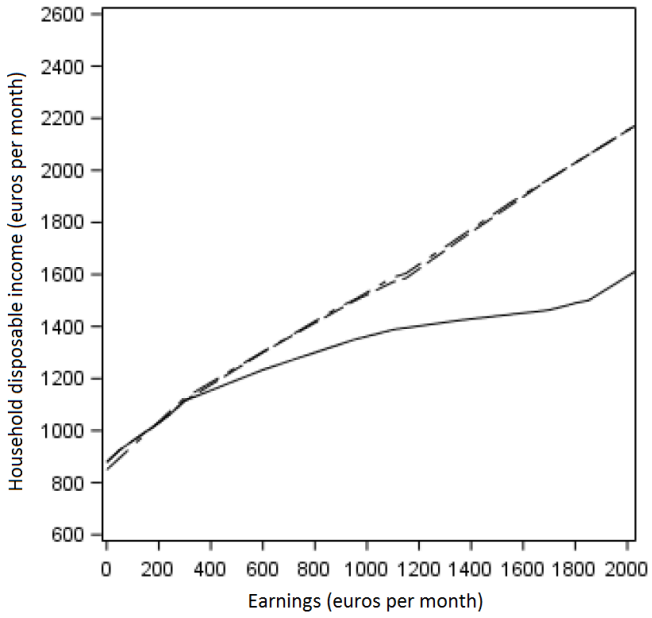

Figure 1. Household disposable income by earnings for a single person household (solid line is regular benefits, dotted line is basic income with UI benefits, dashed line is basic income without UI benefits, source: Kangas et al. 2016).

Figure 2 presents the change in disposable income 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 city of Helsinki. In this case, the regular unemployment benefits are higher than the basic income 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.

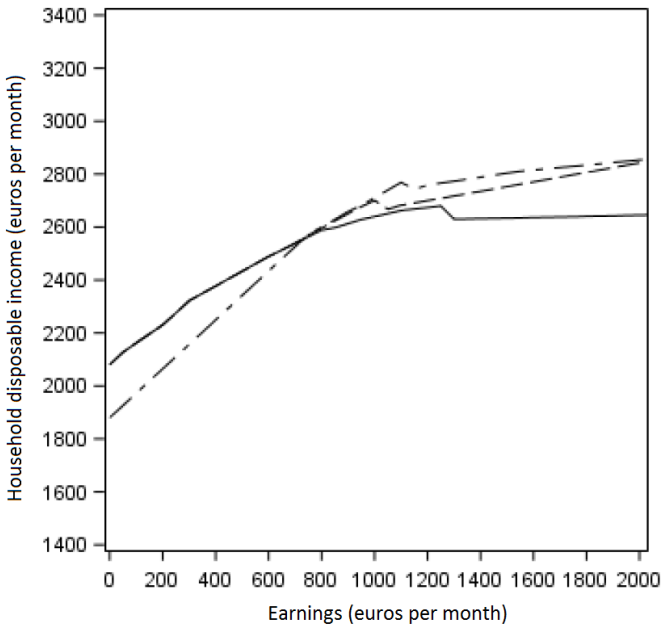

Figure 2. Household disposable income by earnings for a single-parent household with two children (solid line is regular benefits, dotted line is basic income with UI benefits, dashed line is basic income without UI benefits, source: Kangas et al. 2016).

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. 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 they will 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 a law (No. 1528/2016). The law was proposed by the government on October 20th and accepted 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. However, the target population in the law differed from the recommendations of the research group substantially by narrowing it down to include only those receiving minimum unemployment benefits.

The law restricted the experiment to include those who had received unemployment benefits paid by the Social Insurance Institution of Finland (Kela) in November 2016. Youth and those close to retirement were also excluded by including only those between 25 and 58 years of age on December 1st 2016. In addition, the law lists recipients of 11 different benefit types of which recipients are excluded 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). Based on the preliminary statistics we received from Kela (see Table 1), the size of the target population is

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 law drafting process, mainly to persuade the law makers to increase the power of the experiment. Here, we present the planning phase power calculations and update them for our primary outcome variable.

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

In this analysis plan, we chose the number of days in employment between November 1st 2017 and October 31st 2018 as our primary outcome. We use past data on unemployment (from Kela) and employment (from Finnish Centre for Pensions) spells to estimate the variance of this outcome variable. Using most recent available microdata from November 2011 to define target group of the reform and data on 2012–2013 employment spells, the mean days in employment is 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 would imply around 11% increase in employment days in the treatment group. However, the employment level estimate should be considered suggestive only because 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.

4.3 Balance and attrition

We obtained preliminary statistics from Kela after randomization. These statistics in Table 2 show group balance with respect to three key background variables. The shares of the treatment group were published but because frequencies of the control group were not, balance checking was not possible using published data only.

Chi-squared tests show no significant difference between groups with respect to gender or age distribution (p-values 0.83, and 0.25, respectively). However, the distribution of unemployment benefit type is significantly different with p-value of 0.0021. The labor market support recipients are overrepresented in the treatment group while the unemployment allowance recipients are underrepresented.

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

Because the labor market support recipients have, on average, worse labor market characteristics than the unemployment allowance recipients, it is possible there are also other significant differences in the background characteristics. More detailed data on background characteristics are not yet available to us. We plan to conduct similar balancing checks on other relevant demographic characteristics and labor market history variables, especially on those variables that are related to the benefit type. Finally, it should be noted that based on current information, and after double checking with Kela, we believe that randomization was conducted correctly and the benefit type distribution is imbalanced simply by chance.

Attrition is not likely to be a major problem in this experiment. The follow-up is carried out mainly using administrative register data, which removes the possibility of nonresponse. In addition, the 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 ε_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 will be included among covariates as a robustness check. 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 α, β and δ. 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.

6 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.

6.1 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 ΔI is the counterfactual effect the treatment would have had on net incomes given hw, the pre-treatment earnings, η_w^c is the compensated wage elasticity and η_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 α 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 Δt 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.

6.2 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 will have to be that $\hat{Y}_i$ does not have a direct effect on C_i.

7 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 groups in December 2016. 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 are not aware that Kela or other parties have filed any data requests by the end of May 2017. Our plan is to begin the data collection around the end of 2019 when the experiment ends and the administrative data become available.

8 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. In the summer and fall of 2016 a law was prepared independent of the research team work. The experiment will be carried out during 2017–2018. Data collection and analysis will take place during 2019.

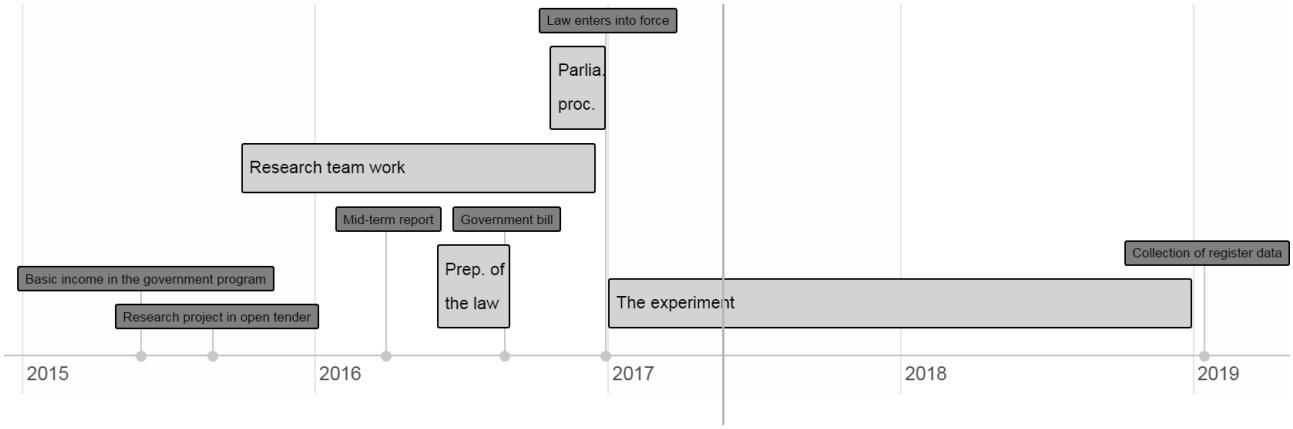

Figure 3. Study timeline.

Appendix

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;
```

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