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BUSINESS START-UP SUBSIDY AS A WAY OUT OF UNEMPLOYMENT: ESTONIAN CASE

Master's Thesis

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Name and signature of supervisor ………………………………….

Allowed for defense on ……………………………………………….

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I have written this master's thesis independently. All viewpoints of other authors, literary sources, and data from elsewhere used for writing this paper have been referenced.

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Abstract

This thesis analyses the effectiveness of Estonian labor market program which offers business subsidy for the registered unemployed. The outcomes of interest are gross income and gross income being positive; the maximum examined period after subsidy is 60 months, 2014 to 2019. The data on prior employment, unemployment and demographic indicators is used in the propensity score matching procedure and the outcome regression models. The results show positive effect of the subsidy on the probability to receive positive income in the first year after treatment and insignificant or negative effect afterwards. The effect on income is strictly negative almost immediately; however, there exist important data limitations which may have influenced the results. Sensitivity analysis has shown that the estimates are rather sensitive to hidden bias and the survival rate proxy indicates that most of the participants stayed employed at high-up positions in companies throughout the whole analysed period after treatment.

Keywords: *treatment effects, policy evaluation, business subsidy, self-employment, propensity score matching*

Contents

1. Introduction

Addressing unemployment is a universal issue for governments in both developing and developed countries, with a number of programs and approaches to programs' design being used, reviewed, started and gotten rid of each year. With the main goals of programs for unemployed including entering employment, increasing employability and help in finding suitable employment (as stated e.g. in OECD's ALMP objectives), the most important question for the authorities is effectiveness of the measures in use. Governments spend considerable sums of money on these measures: in 2017 only, OECD countries have spent 0.52% of GDP on active labor market policies; in Estonia this indicator equalled 0.42%. Passive measures such as benefits and compensations comprised another 0.68% of GDP in OECD, and 0.32% in Estonia.

Both active and passive labor market policies are present in the arsenal of 21st century governments, with the latter referring to assistance in unemployment phase rather than help in jumping back into the labor market. In contrast, the active measures typically aim to increase human capital of jobseekers (training), boost the effectiveness of this process (job search assistance), incentivize employment or directly create it. Incentivising and training are two policies of particular interest – while they may have sizable negative returns in the short run, their effects are, at least in theory, meant to persist, creating better-educated and more experienced workforce.

The volume of comprehensive quantitative studies on ALMP has exploded in the recent decades (see e.g. LaLonde 2003 and Card et al. 2017 for overviews), which allowed to generalize the impact of most of the programs in use. This is especially true for the "classic" programs aimed at the unemployed who want to enter or re-enter dependent employment. Card et al. (2017) summary of 200 papers concludes that the programs of job search assistance have similar effects on employment both in the short (<1 year after treatment) and medium-to-long term, while private sector employment and training are significantly effective in medium and long run but often have insignificant or low significant treatment effects in the short run. Direct public sector employment is largely ineffective or even has a negative impact on the jobseekers across all intervals. Moreover, treatment effects differ across groups of participants, with females, average-aged participants and those who have been unemployed for a long time benefiting relatively more.

The focus of this study is business start-up subsidy program for unemployed in Estonia. This program provides funding for the unemployed who meet program criteria in a form of one-time payment. Upon receiving the lump-sum money, the participant is obliged to start a company or running business as self-employed within the next six months and maintain business activities for at least a year; if these requirements are not met, the recipient must return the subsidy in full. In contrast to other labor market programs, business subsidy provides an opportunity for the unemployed to re-enter labor market in a non-dependent form of employment, thus distinguishing the participants from non-participants in a number of dimensions.

Business subsidy falls into the "work first" category in Card et al. (2017) terms, i.e. the program goal is to enforce employment right away, in contrast to the raising of human capital. In this regard one would expect rather modest positive results, if any, in the short run and possible negative or statistically insignificant results in the medium-to-long run. However, most of existing articles on start-up subsidy policies conclude with significant positive effect of the program in terms of labor market prospects, business survival and, to some extent, income. At the same time, the success of business and entrepreneurial gains has shown to be less promising. The empirics consistently shows that the grant receivers differ greatly from their counterparts and, though to a lesser extent, from one another.

In contrast to other labor market policies, research on business subsidy impact remains rather scarce, with an exception of Germany where the issue has been analyzed from several angles. In other countries, however, there exist only singular studies, if any. The paper on Estonian start-up grant was written based on a rather small data sample (roughly 1000 program participants) and lacks supplementary analysis such as robustness check and testing alternative model specifications.

Current Thesis contributes both to the overall literature on business subsidy ALMP and to the research on Estonia in several ways. First, it adds to the study by Estonian Unemployment Insurance Fund (EUIF) from 2014 (Vilsaar et al., 2014) by analyzing a later cohort of program participants. Second, the data is available for a quite long time span of a minimum of 24 months before and up to 60 months after subsidy. Finally, various matching variations are performed, including the one used in 2014 paper, and the model sensitivity is checked in contrast to the abovementioned paper. This allows to compare the results and extend the previous research on Estonia.

Importantly, there exist substantial data limitations discussed in the sections below. Apart from the lack of some data on covariates which EUIF study has used, the outcome data is restricted to the income from tax declarations (thus not including dividends and the income of individuals registered as self-employed persons), and the data linking the program participants to the information on the firms they created is lacking.

The effects are estimated by regressing the income outcomes on a list of available covariates and the treatment factor. The subsidy recipients are matched to the potentially comparable non-recipients among the unemployed using propensity score matching and the nearest-neighbor algorithm. The proxy for survival rates is the treated individuals being registered as managers, board members, self-employed persons or on other high-ranking positions in companies.

2. Previous Studies

2.1. Business Subsidy for the Unemployed

Start-up subsidy as a way out of unemployment is present in the list of active labor market policies (ALMP) of developed countries continuously since the 1980-s. Comprehensive research on its effectiveness, however, emerged rather recently (O'Leary, 1999; Pfeiffer & Reize, 1998) and by now the area remains understudied compared to the other LMP. The most studied is German policy, with a series of papers such as (Baumgartner & Caliendo, 2008) and its further extensions, alongside individual papers on Finland (Tokila, 2009), Sweden (Behrenz et al., 2016), Romania (Rodrıguez-Planas and Jacob, 2010), Poland and Hungary (O'Leary, 1999) and some other. Among these the two distinct directions are comparison of subsidized start-ups with regular ones and contrasting participants of business subsidy program with other unemployed individuals. The observation longitude in these papers varies from 1.5 to 5 years, with the only exception of Tokila (2009) whose dataset allows to track the performance of businesses for a maximum of 14 years after start-up.

Both the empirical literature and common reasoning allow to difference the unemployed individuals who apply for business subsidy both from other unemployed and from other entrepreneurs. For instance, one goal of start-up subsidy for the unemployed is to correct for possible disadvantages faced by subpopulation. Apart from credit constraints (see e.g. Schäfer et al., 2011), formerly unemployed businessmen are also characterized by less prior experience of self-employment than non-subsidized entrepreneurs, having a higher level of education in the firm-specific field and a lower general one. Moreover, industry-specific knowledge and experience, assets and managerial experience were found to be strongly associated with business survival for subsidy receivers when, in contrast, regular start-ups' survival correlated greatly with human and social capital and organizational ecology (Tokila 2009).

Majority of authors emphasize the significance of intergenerational transmission, i.e. the ALMP participant's parents being self-employed or running a firm at some point of their life. Other common distinctions include gender, region, industry of firm operation, experience and education.

Disadvantages faced by the target group may put a barrier for their employment prospects and potential business success; for instance, higher-educated unemployed with considerable amount of experience may struggle less to get back into the labor market. In addition, females were shown to have higher preference towards flexible working schedule and responsiveness to childcare policies (see e.g. Lefebvre and Merrigan, 2008); historical and socioeconomic differences within a country contribute to diversification as well (e.g. Caliendo and Künn, 2014, on East versus West Germany). On the other hand, some individual factors are not necessary to include since they are already captured in formal variables. As Caliendo, Künn and Weißenberger (2016) have shown, adding variables for the Big Five personality traits, risk aversion and locus of control does not contribute to the explanatory power of the end model.

The survival rates of subsidized start-ups are rather high in general and in contrast to their non-subsidized counterparts, especially during the first 1 to 2 years (Duhautois et al., 2015, Vilsaar et al., 2014, Caliendo et al., 2015), with some studies emphasizing the subsidized firms' better survival for all analyzed intervals (Tokila, 2009). However, the estimations of other business success dimensions for ALMP participants are, predictably, more conservative. Caliendo, Hogenacker, Künn and Wießner (2015) show that German subsidized start-ups lag behind the regular ones in business development and innovation.

However, the first and foremost ALMP agenda is to facilitate re-entering employment. Compared to other unemployed, the participants in start-up subsidy program consistently show better results in terms of employment prospects. With an exception of study on Estonia partly reporting statistically insignificant results[1], the empirics concludes unambiguously that the treated have way higher probability of being in non-subsidized employment. Meanwhile, the program impact on income is more modest, with most papers concluding with slight positive statistically significant results in contrast to other unemployed[2], and the firm studies showing lower income of the treated group.

There remains one more problem to evaluating the effect of business subsidy on formerly unemployed. While participants differ from other population groups substantially, heterogeneity within the group also naturally occurs, causing some to benefit from treatment more than others. As mentioned earlier, business subsidy LMP may be more beneficial for disadvantaged individuals who allegedly find it harder to find a place in the labor market or to start a business. This assumption is consistent with segmented labor markets theory, though human capital theory predicts experienced and skilled workers to be the prime benefactors. Segmented labor markets theory gives a two-sectoral perspective on labor markets. Here, in contrast to a primary labor market with highly productive jobs and corresponding benefits, secondary sector is a place where less productive, more ‘traditional’ labor activities take place. The problem of unemployment thus may be related to the structure of employment, where relative prevalence of low-productive jobs displaces some individuals. Self-employment thus may be seen as a necessity for the people who would otherwise rather work a regular salary job. Human capital theory, on the other hand, suggests that better educated businessmen with prior experience have more chances of survival and maintained (self-)employment.

Indeed, results point to either direction depending on the start-up subsidy program (as in Germany and Romania, where two separate programs coexisted), outcome and heterogeneity source considered. Within the same business subsidy program, low education level is associated with larger effect on the labor market outcome; at the same time, the effect on income is higher for the better educated (Caliendo and Künn, 2011; Behrenz et al., 2016). German nationals rather surprizingly benefit (also from both German LMP) more than non-Germans (Caliendo and Künn, 2011). Depending on the program, results differ for age groups, education and skill level, and also for regions with different economy characteristics such as unemployment rate and GDP per capita (Rodrıguez-Planas and Jacob, 2010; Caliendo and Künn, 2014; 2015).

Finally, a few studies highlight the possibility of dead weight presence, though its identification is a nontrivial task. Deadweight effect in terms of economic policy means that the outcome would be the same without treatment and is considered in O’Leary (1999) and Caliendo et al. (2015). The former claims that selection mechanism of control group and including substantial number of covariates eliminates deadweight loss and thus does not test the dead weight assumption directly. The latter set two outcomes to test: labor market status and business success, asking participants whether the program had any effect on these during the follow-up interview. Almost half of respondents stated they would start a business anyway, with some 23 per cent admitting they registered as unemployed specifically to receive the start-up subsidy. At the same time, though, roughly half of these individuals said that the subsidy was highly important for the firm survival during the period of its reception, which means that deadweight effect is in fact lower.

Somewhat related is cost-benefit analysis in Caliendo and Steiner (2007) which concludes that the program effect is positive at least from the point of its direct costs and direct benefits for one of the two German programs. Vilsaar et al. (2014) included some indirect effects in their analysis as well, namely – management and employees’ wages and value added, and found that the benefits exceed the costs at the third year after start-up. Still, this perspective on efficiency is rather understudied, even though it highlights some significant flaws potentially existing in the business

[1] Their statistically significant results were positive for overall employment.
[2] Vilsaar et al. (2014) find treatment effect on income to be negative on the second year after receiving the subsidy.

subsidy policy. The question is not directly addressed in this Thesis, but it should be considered in further research on the topic.

2.2. Estonia

Business subsidy for the unemployed individuals in Estonia is granted for founding a new company or starting business activity as a self-employed person. A few requirements must be met in order to receive the grant. First, the applicant must have completed a business training or have either vocational education in economic field or prior experience of running a business. Second, the unemployed individual or the non-working job seeker of retirement age must present a business plan in the application. The maximum aid is 4,474 euros, and the sum may be withdrawn if economic activity is not started within six months upon receiving it.

Since 2009, from 1.6 (2013) and up to 2 million euros (2017) was spent each year on this ALMP in Estonia. At least four hundred Estonians participated each given year, with females comprising more than a half of these cases in each of the latter years.[3]

A 2014 study of Estonian Unemployment Insurance Fund (Vilsaar et al., 2014) covers 2009-2011 subsidy applicants, with slightly over 1,000 recipients and 10,427 control individuals. The results of treatment effect estimation are ambiguous: some are positive or statistically insignificant; some are even negative. The latter is true, for example, for paid employment after roughly 1.5 years after treatment. However, this measure excludes the members of company management or supervisory board who receive remuneration for their work; after including those in estimation the results after 1.5 years since treatment become statistically insignificant. This may be a sign of data limitations: in each case, treated are matched with the controls who registered in the same month, so the number of observations diminishes over periods.

Interestingly, the effect of ALMP on income does not show statistically significant positive results at all in EUIF study, in contrast to other papers on the topic reporting slight but positive and significant effect of treatment. Even though business survival rates are high among Estonian participants, their earnings were found to be either no different or even lower than those of non-participants.

Finally, there is a data limitation in EUIF article which somewhat reflects current Thesis issue: that is, the variables available on the treated and control individuals may not be enough to construct an adequate control group given the conditional independence assumption.

[3] 2011 to 2018.

3. Methodology

Given that the individuals in both groups have the same chance to be selected into treatment, differences in their outcomes (or lack thereof) provide useful insights from economic policy perspectives. Randomization, however, is rarely a property of real-life experiments, with selection of individuals with particular characteristics into treatment happening due to treatment specifics or restrictions on entering. The issue has been addressed by researchers systematically at least since the 1970's, with growing complexity of methodology for making participants and non-participants comparable. This section will discuss a few common-use methods and the tools which recently became available for causal inference and related analyses and talk through the research methods of this thesis.

3.1. Causal Inference

Despite the existence of a number of other approaches, such as including covariates directly in regression, using instrumental variables or synthetic control, matching individuals by their propensity scores is arguably the most common in economic literature. The method is based on gathering the information about the differences between treated and controls into a single indicator of probability to be treated, which is later used in estimating treatment effect (Imbens, 2000). The basic idea is to make it possible to compare the outcomes of treated individuals with the outcomes of the non-treated who are the most akin to them. As argued in e.g. Nihms (2010), this way of balancing is not only complementary to other approaches, but also has an advantage of highlighting the insufficient overlap between the compared groups. Moreover, performance diagnostics is easier for methods involving matching, in contrast to selection and regression models.

The obtained propensity score may be further used in several ways for treatment effect estimation. Direct matching (such as nearest neighbor or radius matching) compares each treated directly with one or several close control individuals, averaging the result afterwards. A potential drawback of such methods is that part of data may remain unused, even if the discarded observations are quite close in their propensity scores to the treated. Subclassification and weighing, in contrast, use all the available data either by implementing propensity scores in weights or forming groups of similar individuals. Weighting may be done by directly using inverse propensity scores in treatment effect estimation (inverse probability of treatment weighting) or averaging over a number of controls for each treated, with weights depending on the distance (Kernel matching). Weighting approaches, however, are highly sensitive to extreme weights (very close to 0 or 1) and model (mis-)specification, since large weights increase variance of an estimate, which is only fine if the weights are correct. None of the available techniques is thus a priori superior to the others; even different specifications only allow a tradeoff between bias and variance in a model.

This thesis uses several specifications of nearest-neighbor matching, including the one used in Vilsaar et al. (2014) (1:2 matching with replacement) and caliper matching. The choice of nearest-neighbor method as the main propensity score-based one is driven by its comparability with the former EUIF study.

3.2. Supplementary Analyses

In the base potential outcome model (Roy 1951, Rubin 1974) treatment effect for binary treatment is formalized as follows: $ATE = E(Y_1|W = 1) - E(Y_0|W = 0)$, where ATE is the average treatment effect, W is equal to 1 for treated individual and 0 otherwise, and Y is the outcome variable. The techniques that use matching rely on the assumption that after controlling for the confounding variables (i.e. the factors that influenced both assignment into treatment and outcome) the outcomes are comparable. This premise, also called unconfoundedness or conditional independence assumption (CIA), is highly important for the result to be valid.

CIA is a rather strong assumption: the ability to include all the background variables is limited when it comes to empirical matching, and direct testing for justification of the assumption is rather hard. Missing the relevant covariates is costly: while including unimportant variables in the propensity score (PS) estimation only leads to them having little weight or increases variance of estimators, not including relevant variables increases bias in research results. Since it is not always possible to account for all the factors of assignment into treatment and outcome, it is argued that the preference has to lie towards the variables affecting outcome (Brookhart et al. 2006). Including the unobserved data indicators in the PS model is one more way to deal with the issue. Another way is to use generalized boosted models (decision trees) which a priori do not require covariates to be fully observed.

Supplementary analysis is essential for quantitative policy evaluation, providing justification for model choice and conclusions. One way to check robustness is to examine different model specifications before settling on the final model. Specifications may include modelling on different sets of variables, changing functional form and parameters, excluding certain observations or comparing the found outcome with an outcome of alternative estimators. Furthermore, Athey and Imbens (2015) suggest using subsampling the dataset for different model specifications and settling on an estimate for causal effect variable which is weighted average of the obtained estimates.

Another direction of sensitivity analysis is obtaining ranges of estimates relying on assumptions of different weakness levels. Rosenbaum and Rubin (1983) offer to start from the most restrictive specification and gradually relax the main identifying assumptions such as CIA, analyzing the changes in estimates. One example is bounding approach (Rosenbaum, 2002) frequently used in policy evaluation papers; the key idea is to find the level of unobserved heterogeneity at which the results would no longer be robust. Imbens (2003) proposes a data-driven technique for finding correlations between treatment and outcome and the unobserved covariates.

To sum up, supplementary analysis is a must for treatment effect estimation due to diversity of approaches and their effectiveness, and implications of Roy-Rubin model assumptions for matching procedures. Unfortunately, not all studies perform such analysis or report it – as e.g. in Rodrıguez-Planas and Jacob (2010) on Poland and Hungary ALMP and, more importantly, in Vilsaar et al. (2014) on Estonia.

In this study, several specifications were used for each type of matching. In addition to matching the treated to a different number of controls, using different sets of covariates, and trying variations with and without replacement, an issue of treatment period was addressed from several angles. Treatment month and treatment quarter were used in the regressions on probability of treatment as well as the variables for exact matching. I also check the mean standardized differences after matching and test the results with a Rosenbaum bounds test.

The reported treatment effects were estimated by running linear and general linear regressions and calculating robust standard errors for the "treated" variable estimate in the case of continuous outcome and marginal effects in the case of binary outcome.

3.3. Treatment Effect Heterogeneity

Non-random variability in treatment effect magnitude and direction is what we will call treatment effect heterogeneity. It seems reasonable to assume both theoretically and based on previous research that certain subgroups of start-up subsidy participants show results that are consistently different from the other.

The existing papers which addressed heterogeneity in business subsidy LMP effect used subgroup analysis based on pre-selected variables hypothesized to be heterogeneity drivers. Caliendo & Künn (2011, 2014), for instance, perform Kernel matching for subsamples formed for different levels of education, qualifications, nationality and age.

In our case, unfortunately, the size of the dataset does not allow to get reliable results after splitting the data by a given variable. Thus, heterogeneity hypothesis was tested by adding interaction terms of treatment variable and the potential heterogeneity drivers such as gender, education level, mother tongue, region and previous job and unemployment experience to the outcome regression. The coefficient being statistically significant was regarded as an indicator of possible heterogeneity presence.

4. **Data**

This Thesis focuses on comparing different groups of unemployed individuals in Estonia – those who used the business start-up subsidy and the ones who chose another labor market policy or none. The data is available for grant receivers in 2014 to 2018 and their counterparts, and includes background, previous and further employment and unemployment information. The income data is available for 2012-2019, which allows to take into account the information both before and after treatment.

I use the data collected from several sources (Estonian Unemployment Insurance Fund, Tax and Customs Board, Commercial Register) by Statistics Estonia, which, unfortunately, imposes limitations on the access to certain variables and hence may have influenced the quality of the analysis. For instance, the data on participation in a particular (incl. business) training by EUIF and on areas of education received by unemployed is not automatically present in Statistics Estonia database, and its inquiry from external sources is not feasible in the current project's timelines. Ideally, filtering by several criteria would allow to select only eligible non-participants prior to the matching procedure, i.e. those who either have participated in business training; or have received vocational or higher education in economics; or had experience in managing a company or being self-employed. It is possible, however, to extend the analysis further with less time restrictions. In either case, larger number of observations than in the previous study (Vilsaar et al., 2014) together with a diversified methodological setup allow to assume that rather reliable conclusions can be drawn. Moreover, matching on the covariates helps overcome the mentioned limitation at least partly.

Another drawback is that the available income data does not account for all the possible sources of income such as dividends. This limitation is addressed by using a dummy version of income in addition to the continuous one, since at least a minimum wage should be received in order to have health insurance in Estonia. In addition, the data on income of self-employed individuals is stored separately from the data on tax declarations which was available for the current research. However, only 47 out of 2138 treated registered as self-employed in the first months after subsidy. Still, separate analysis was conducted without self-employed individuals; the results are available in Appendix D.

The outcome variables of interest are income and income being positive, the latter being a proxy for employment. Information on both is extracted from tax data, and the outcomes are estimated up to 60 months after treatment. The full list of covariates used for the matching and outcome regressions specifications include gender, education, region, number of previous unemployment spells, occupation at the last job, business experience, self-employment experience, age and age squared, duration of the last employment, average income 1 year before treatment and treatment period. For program participants, treatment month or quarter were simply the month or quarter of receiving the subsidy. The non-participants were filtered out for whom unemployment has started before given month and ended after or has not ended. In addition, I dropped the controls whose unemployment started before the earliest unemployment start of the treated.

The overall number of business subsidy recipients in the dataset is 2,148; these individuals are on average 38 years old, ranging from 20 to 69 at the time of start-up. Interestingly, more than half of them are female overall (57.7%) and in each individual month. 27.5% of grant receivers have already had some experience in business management, with 2% having run a business themselves or worked as self-employed.[4]

The total number of potential controls in the pre-matching sample is 216,218. Each treatment month there are from 19 to 57 treated and from 1,864 to 5,152 potential controls, except for

[4] See Table 1 for more details.

January 2014. Due to the filtering choice, the duplicated observations were omitted so the first treatment period has more control observations. While this may be considered a drawback, such setting affects only a small portion of overall results both because of the large number of periods analyzed and the restrictions posed by nearest neighbor algorithm combined with exact matching on the treatment period.

The pre-matching sample is rather unbalanced. The grant receivers are more often female (57.7 compared to 50.8% in the control group), Estonian speakers (86.8 compared to 60%), having had business experience in general (27.5 compared to 15.4%) or having been self-employed (2 compared to 1.1%) before registry in EUIF and more often registering as unemployed for the first time. In addition, there are relatively fewer program participants from North-East region and Harjumaa and relatively more from South-East and West. Much more (42.6 compared to 18.2%) treated individuals have received higher education; there are relatively more controls who have received only general secondary education (40 compared to 25%). In terms of propensity scores, a large portion of potential controls had a close to zero probability to be treated; however, there was enough overlap for the two groups to proceed with comparing the outcomes (See Figure 1 and Table 1).

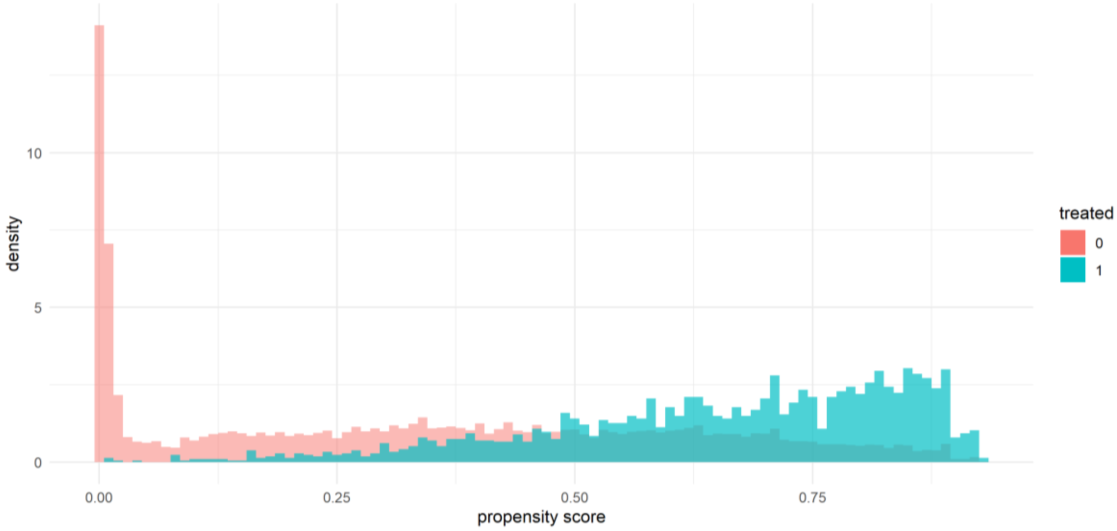
5. Results

5.1. Matching Quality

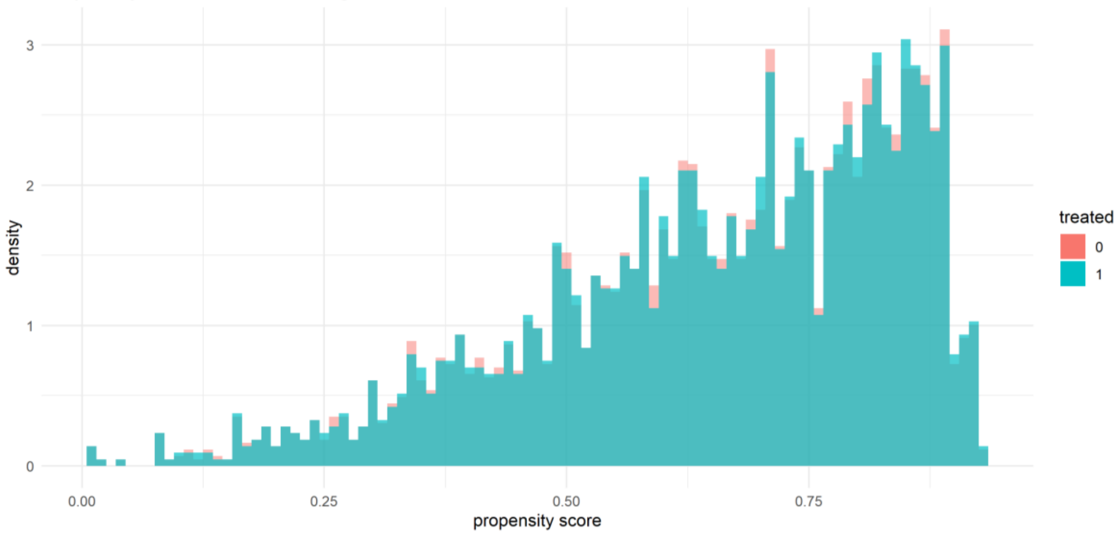
The matching results presented further are those of nearest neighbor matching with replacement, with a maximum of two controls per each treated and exact matching on the month of treatment. The reported results use the version with gender, mother tongue (binary – Estonian or not), business experience, region and education dummies, age, age squared, and average monthly income one year before treatment as predictors of the probability to be treated. Summary of pre- and afterbalance in Table 1 includes also self-employment experience and the first digits of ISCO-08 (International Standard Classification of Occupations) codes from the last job before registry in EUIF. These dummies were used in intermediate analysis but not included in the final models since the data was missing for some of the treated individuals and the information is already captured to a great extent in variables such as business experience and average prior income.[5]

Figure 1. Density of Propensity Scores Before and After Matching

Panel (a). Before matching



Panel (b). After matching



[5] See the descriptions of variables in Appendix A

Note 1: The matching results presented here are those of nearest neighbor 1:2 matching with replacement, with exact matching on treatment month.
Note 2: Since the number of potential controls exceeded the number of treated substantially (99:1), the dataset was reweighted for the logit model which calculated fitted propensity scores.

In contrast to a very unbalanced pre-matching sample, in the matched sample the standardized means did not differ by more than 0.07 for the variables used in the final analysis and did not differ by more than 0.1 for all the observed variables. However, the difference in the average prior income in fact increased after matching; this, in turn, may have influenced the reported treatment effects on the continuous outcome since the matched controls have been slightly higher earners on average than the treated.

Table 1. Summary of Balance for the Main Variables Before and After Matching

Variable	Mean for treated	Mean for controls before matching	Mean for controls after matching	Standard deviation for treated	Standardized mean difference before matching	Standardized mean difference after matching
Male	0.4228	0.4917	0.4270	0.4941	-0.1393	-0.0085
Mother tongue - Estonian	0.8676	0.6008	0.8782	0.3389	0.7871	-0.0311
Number of previous unemployment spells	0.0019	0.3259	0.0023	0.0432	-7.4972	-0.0108
Business experience	0.2746	0.1540	0.2928	0.4463	0.2702	-0.0409
Self-employment experience	0.0206	0.0111	0.0210	0.1420	0.0664	-0.0033
Age	37.280	38.998	37.300	9.8611	-0.1743	-0.0021
Capital	0.3489	0.3745	0.3529	0.4767	-0.0537	-0.0083
West	0.1127	0.0841	0.1258	0.3163	0.0906	-0.0414
Center	0.0907	0.0920	0.0858	0.2873	-0.0043	0.0171
North-East	0.0510	0.1522	0.0507	0.2200	-0.4600	0.0011
South-East	0.3396	0.2322	0.3319	0.4736	0.2268	0.0163
ISCO - 1 (last employment)	0.1618	0.0673	0.1300	0.3692	0.2584	0.0887
ISCO - 2 (last employment)	0.1338	0.0549	0.1085	0.3404	0.2316	0.0742
ISCO -3 (last employment)	0.1370	0.0762	0.1246	0.3439	0.1769	0.0360
ISCO - 4 (last employment)	0.0543	0.0502	0.0447	0.2265	0.0179	0.0423
ISCO - 5 (last employment)	0.1123	0.1302	0.1132	0.3157	-0.0569	-0.0030
ISCO - 6 (last employment)	0.0098	0.0128	0.0115	0.0986	-0.0303	-0.0166
ISCO - 7 (last employment)	0.1146	0.1370	0.0847	0.3186	-0.0702	0.0940
ISCO - 8 (last employment)	0.0332	0.0832	0.0522	0.1792	-0.2791	-0.1057

Education secondary general	0.2507	0.4055	0.2493	0.4335	-0.3572	0.0032
Education professional	0.1618	0.1586	0.1492	0.3683	0.0088	0.0343
Education vocational	0.1422	0.2040	0.1562	0.3493	-0.1769	-0.0402
Education higher	0.4261	0.1817	0.4298	0.4946	0.4941	-0.0076
Average income 1 year before treatment	335.81	352.29	379.25	626.04	-0.0263	-0.0694
Propensity score	0.6576	0.3385	0.6576	0.1861	1.7141	0.0001

Note: Standardized mean difference in the difference in means for treated and controls adjusted for the standard deviation for treated, i.e. $\frac{\mu_{treated}-\mu_{controls}}{\sigma_{treated}}$.

In addition, the analysis in highly sensitive to the bias coming from the departure from random assignment into treatment. Table 2 shows the results' sensitivity based on the Rosenbaum test which relies on the assumption that there may exist an unobserved covariate which influenced the treatment assignment and that the treated and controls might thus be the same in terms of observed covariates while their true propensity scores are in fact different. The Rosenbaum (2002) approach seeks to find a threshold of the relationship between an unobserved covariate and the exposure which would make the treatment effect estimate insignificant.

Table 2. Rosenbaum Bounds Test Results for Outcomes 1 Year After Treatment

	Binary outcome (income positive)		Continuous outcome (income)			
	Bounds for McNemar's test statistic		Bounds for Hodges-Lehmann point estimate		Bounds for Wilcoxon signed rank p-value	
Gamma	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
1.0	0	0.00000	-49.99	-49.990000	0.0116	0.0116
1.1	0	0.00044	-50.09	0.010068	0.0001	**0.2485**
1.2	0	0.02567	-50.09	0.010068	0.0000	0.7800
1.3	0	**0.24720**	-50.09	15.010000	0.0000	0.9825
1.4	0	0.68585	-72.49	37.510000	0.0000	0.9996
1.5	0	0.94142	-100.09	56.310000	0.0000	1.0000
1.6	0	0.99499	-125.09	77.010000	0.0000	1.0000
1.7	0	0.99978	-146.79	98.010000	0.0000	1.0000
1.8	0	0.99999	-170.09	119.41000	0.0000	1.0000
1.9	0	1.00000	-189.29	137.81000	0.0000	1.0000
2.0	0	1.00000	-201.59	161.01000	0.0000	1.0000

Notes: Gamma is the odds of differential assignment due to the unobserved factors. McNemar's test statistic is a simple difference between the number of treated and the number of those treated who had outcome 1 with a χ-squared distribution. Hodges-Lehmann point estimate may be interpreted as the median difference in the outcomes of treated and controls. The estimates and p-values in bounds test results may differ from those in the matching analysis.

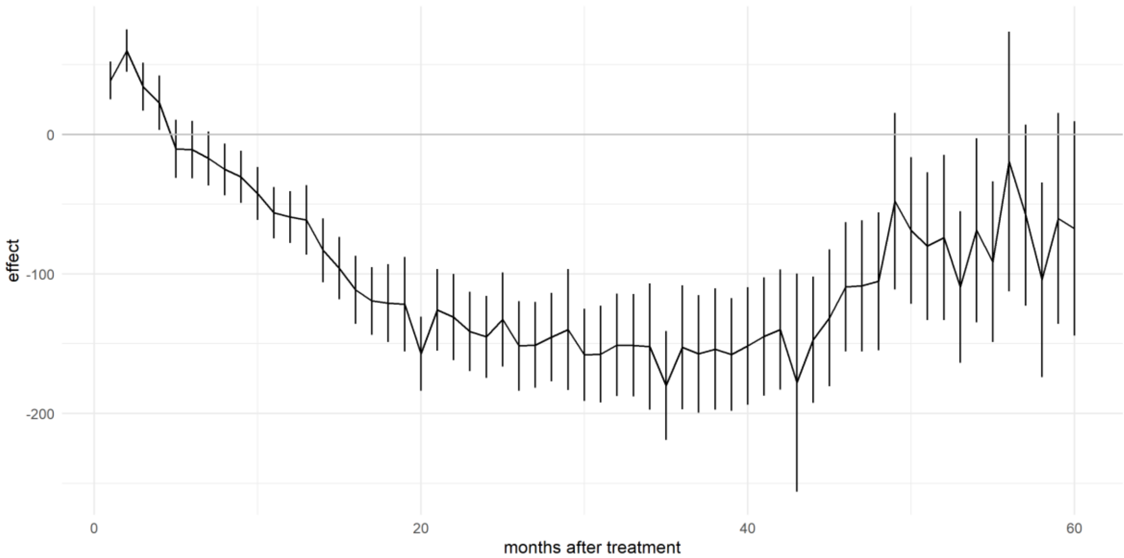
The estimates are sensitive to a bias that would increase the odds of treatment even by 10% in the case of income and by 30% in the case of income dummy. I.e., even rather small variation in the input would change the inference conclusions. Moreover, the outcome data is available for the whole population of treated and controls only up to 12 months after treatment (since the latest available data on income is for December 2019 and the latest available data on business subsidy is for December 2018), and the number of observations reduces after this period. The estimation results must thus be met with cautiousness.

5.2. Estimation Results

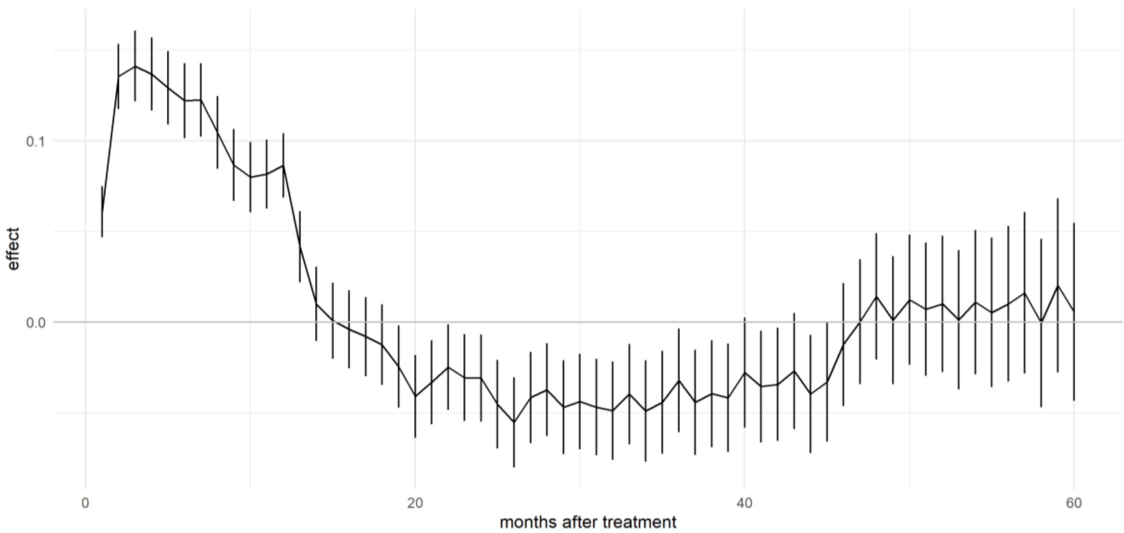
The effect of treatment on earnings has been negative in all matching variations. Picture 2 shows that throughout almost the whole observed time after treatment the average income of the treated was significantly lower than that of the controls, reaching a bit under 200-euro difference. Even in the more ambiguous results from other variations, the number of consequent months with significant negative treatment effect values was over 30 (from ca 5th to 40th month after treatment).

Figure 2. Treatment Effect

Panel (a) Income



Panel (b) Income Being Positive



Notes: Treatment effect on Panel (a) is the coefficient of variable "treated" in the outcome regression. On Panel (b) y-axis is the marginal effect of treatment dummy on the outcome dummy. The range is ±1.96SE, robust errors.

However, as pointed out earlier, the income data does not capture all the possible sources of income. The effect on income being positive has shown to be more ambiguous – positive throughout the first year after subsidy and negative or insignificant afterwards. The positive treatment effect during the first year should not come as a surprise, though, since one of the EUIF demands is to maintain business activities for at least a year after the subsidy has been received.

The results on Figure 2 are the coefficients (in the case of income) and marginal effects (in the case of income dummy) of the treatment variable. The heterogeneity hypothesis was tested by including an interaction term into the joint outcome regressions. Even though several coefficients of the interaction terms were significant for the income, it may be possible that these results were affected by the exclusivity of this outcome variable. Results in Table 3 suggest that the grant program may have been more useful to the individuals with secondary education, those without a degree and those without business experience.

As for the binary outcome, only the treated males had significantly higher chance to have a positive gross wage 24 months after treatment; living in Tallinn, conversely, reduced the effect of treatment. However, as seen in Table 3, unemployed males in general had lower probability to have positive earnings and those residing in capital may have had higher (though not significant statistically) probability to receive positive earnings. More importantly, the marginal effect of treatment on dummy variable 24 months after treatment is not statistically significant (see Appendix B).

Table 3. Summary Statistics for the Outcome Models, 24 months after treatment

	Dependent Variable						
	Income positive logistic			**Income** OLS			
	base	**interactions**		**base**	**interactions**		
Treated	**-0.126**** (0.051)	**-0.230***** (0.068)	**-0.038** (0.063)	**-155.086***** (15.126)	**-93.407***** (14.953)	**-175.89***** (18.473)	**-128.73***** (15.966)
Male	-0.101* (0.057)	-0.263*** (0.074)	-0.097* (0.057)	49.070*** (15.122)	47.082*** (15.041)	49.206*** (15.118)	50.110*** (15.125)
Mother tongue Estonian	0.227** (0.093)	0.222** (0.093)	0.229** (0.093)	73.900*** (23.466)	75.418*** (23.445)	74.262*** (23.425)	74.451*** (23.449)
Number of previous unemployment spells	0.414 (0.486)	0.426 (0.489)	0.415 (0.488)	-4.471 (87.726)	-9.463 (86.610)	-2.444 (86.841)	-10.685 (86.985)
Business experience	0.003 (0.063)	-0.003 (0.063)	0.003 (0.63)	50.188*** (17.999)	47.107*** (17.983)	48.351*** (17.976)	112.769*** (28.289)
Age	0.046** (0.020)	0.047** (0.020)	0.047** (0.020)	8.820* (5.036)	8.231 (5.022)	8.589* (5.033)	8.553* (5.041)
Age-squared	0.001** (0.0003)	-0.001** (0.0003)	-0.001** (0.0003)	-0.137** (0.063)	-0.128* (0.063)	-0.134** (0.063)	-0.132* (0.064)
Capital	0.206 (0.132)	0.202 (0.132)	0.204 (0.132)	111.902*** (26.366)	111.885*** (26.193)	111.355*** (26.345)	110.744*** (26.340)
West	-0.025 (0.145)	-0.031 (0.145)	-0.023 (0.144)	68.484** (29.375)	70.314** (29.214)	69.291** (29.341)	68.014** (29.385)

Center	0.290* (0.150)	0.288* (0.150)	0.289* (0.150)	31.413 (28.550)	33.049 (28.303)	32.387 (28.499)	29.944 (28.477)
North-East	0.044 (0.181)	0.037 (0.181)	0.045 (0.181)	45.741 (38.531)	50.542 (38.296)	46.212 (38.427)	44.551 (38.550)
South-East	0.063 (0.130)	0.057 (0.130)	0.239* (0.138)	34.928 (24.468)	36.798 (24.256)	35.362 (24.428)	35.127 (24.466)
Education secondary general	1.017*** (0.266)	1.010*** (0.265)	1.027*** (0.266)	96.754** (44.295)	95.931** (44.110)	38.863 (46.869)	95.604** (44.188)
Education Vocational	1.037*** (0.269)	1.026*** (0.269)	1.046*** (0.270)	116.494*** (45.166)	117.932** (44.970)	115.231** (45.215)	114.411** (45.066)
Education professional	1.054*** (0.271)	1.047*** (0.270)	1.065*** (0.271)	146.118*** (45.464)	144.883*** (45.300)	145.194*** (45.510)	141.804*** (45.442)
Education higher	1.233*** (0.266)	1.229*** (0.266)	1.245*** (0.267)	259.453*** (46.443)	353.752*** (50.379)	259.495*** (46.481)	256.136*** (46.339)
Average income 1 year before treatment	0.002*** (0.0002)	0.002*** (0.0002)	0.002*** (0.0002)	0.440*** (0.051)	0.440*** (0.051)	0.440*** (0.051)	0.440*** (0.051)
Treated::Male		**0.244**** (0.103)					
Treated::Capital			**-0.261**** (0.107)				
Treated::Education higher					**-141.50***** (31.002)		
Treated::Education secondary general						**84.790***** (28.333)	
Treated::Business experience							**-94.999***** (36.227)
Constant	-2.534*** (0.466)	-2.470*** (0.466)	-2.623*** (0.468)		-99.995 (107.688)	-47.583 (107.745)	-78.513 (106.965)
Observations	6,824	6,824	6,824	6,824	6,824	6,824	6,824
R2				0.238	0.241	0.239	0.239
Adjusted R2				0.237	0.239	0.237	0.237
Log Likelihood	-2,197.282	-2,196.553	-2,196.614				
Akaike Inf. Crit.	4,430.563	4,431.105	4,431.228				
Residual Std. Error				501.726 (df = 6806)	500.947 (df = 6805)	501.542 (df = 6805)	501.464 (df = 6805)
F Statistic				125.337*** (df = 17; 6806)	119.974*** (df = 18; 6805)	118.793*** (df = 18; 6805)	118.948*** (df = 18; 6805)

Note 1: Column 1 reports the coefficients from the binary outcome regression, not the marginal effects. See marginal effects in Appendix B.
Note 2: Robust standard errors in parentheses. Significance: *p<0.1; **p<0.05; ***p<0.01

6. Discussion

The results in this analysis indicate that the business subsidy had a positive effect on the probability to receive positive income during the first year and insignificant or negative effect afterwards; the effect on income has been strictly negative already after several months.

Given the findings in the previous papers on non-Estonian business subsidy for the unemployed (e.g. Caliendo and Künn, 2011; Behrenz et al., 2016), the results seem rather surprising. One possible explanation for the subsidy effect being negative or not significant is that the estimation (2014-2019) takes place during the economic growth period, so that it was relatively easy to re-enter employment without taking a business risk. If it is the case, the treatment effect is negative because the income of the controls (and the fraction of controls with positive income) grew faster.

Figure 3. Outcomes in the Matched Sample (by treatment year)

Panel (a) Mean Income in the Matched Sample

Panel (b). Fraction of Positive Income in the Matched Sample

Note: Quarterly data is available in Appendix C.

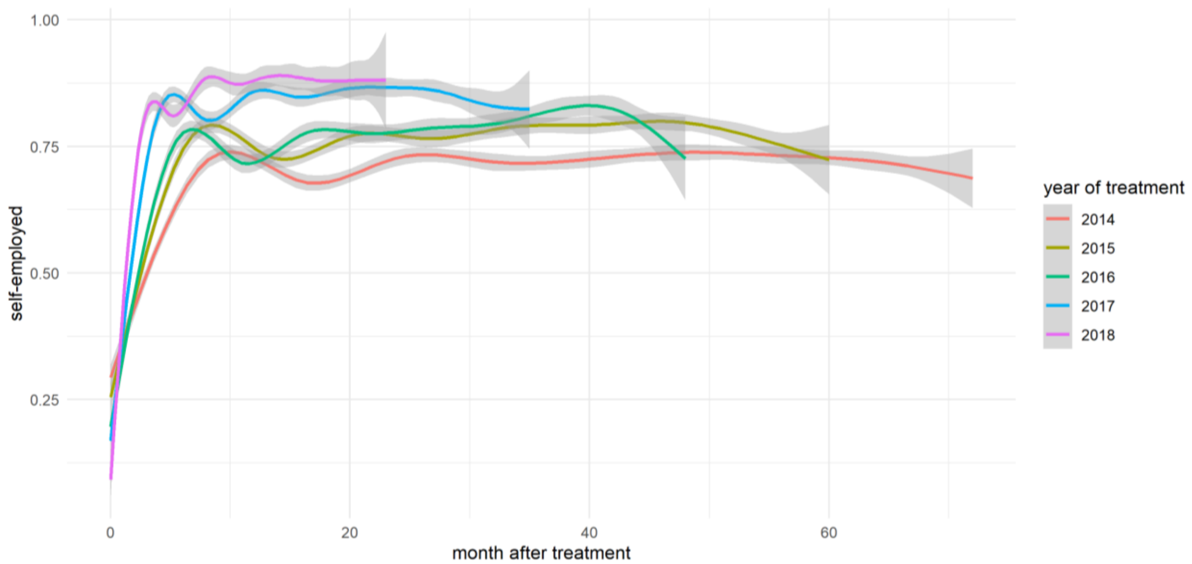
Indeed, there is no sharp decline in the values of the outcome variables for the treated (Figure 3). Instead, the controls have slightly higher growing probability of receiving earnings in some cohorts. Depending on the treatment period, however, the fraction of income being positive may have grown faster for the grant receivers (2017-2018). Still, the average prior income values turned out rather unbalanced for the treated and control group; moreover, the patterns of income change differ for program participants and non-participants substantially. Finally, there is a possibility of other income sources which were not accounted for, such as unemployment insurance benefits, income of those registered as self-employed and dividends. However, the analysis using a constrained dataset yielded similar results (see Appendix D).

Another possible explanation is that the previously unemployed businessmen differ from the rest (Tokila, 2009; Caliendo et al., 2015). It may be the case that the business environment during the studied period was highly competitive, and the subsidy participants were unable to cope with it given their knowledge, skills, experience and other constraints and hence ended up stopping their business activities or stopped gaining profits from them. The treated do seem to experience a slight decline in income around 2 years after treatment in most cases, while the controls who are assumed to work regular jobs experience a boost or at least maintain the same level of income.

Finally, Vilsaar et al. (2014) also report negative effect of the subsidy on income. In line with Card et al. (2017) arguments, the "work first" types of ALMP may indeed have zero or negative effect on participants in the medium and long term.

Importantly, due to the data limitations it is still not possible to firmly state that the actual effect was negative. Even if the effect is indeed negative for the receivers' income, the actual ALMP effect is more complicated, and the societal benefits from the concomitant job creation may have been high enough for the overall effect to be positive. Though the data on the businesses created and their employees was not available in the scope of this thesis, I used the data on the high-up positions held in companies to construct survival rates.

Figure 4. Business Survival Rates (by treatment year)



Note 1: Fraction of self-employed (those registered as self-employed, board members, managers and other – see the full list in Appendix A) in the group of treated, by month after treatment.
Note 2: Most of the treated (around 95%) were registered as managers or board members. Self-employed comprised up to 5% of treated.

Clearly, the treated have remained employed long after receiving the subsidy. Though the rates differ depending on the year of grant, the receivers usually started business activities within the first year and remained involved in them (or, at the very least, in business activities in other companies) for the whole observed period. The results on Figure 4 may hint on the restrictions posed by the available income data for this project.

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Appendix A: Description of the Used Variables

Variable	Definition
Treated	1 if treated
Male	Gender, 1 if male
Mother tongue	1 if Estonian
Number of previous unemployment spells	Number of registrations in EUIF before the last one
Business experience	Having held certain positions in a company before the last registration in EUIF: representative person; entitled representative person; entrepreneur; authorized representative limited partner, self-employed; fund manager; management board member; board member; representative of the company; manager; head of branch
Self-employment experience	Having held certain positions in a company before the last registration in EUIF: self-employed; entrepreneur
ISCO-08 (last employment)	Occupation code for the last job before registry in EUIF. International Standard Classification of Occupations
Age	Age at the time of registry in EUIF
Education	ISCED 2011 (International Standard Classification of Education) codes at the time of registration in EUIF. Early childhood and primary education not included
Region	Administrative regions. "Capital" for Harju county / Northern Estonia
Average prior income	Mean monthly gross income from tax declarations for 12 months before treatment
Income	Gross income from tax declarations
Income being positive	1 if gross income from tax declarations was positive in a given mont
Survival rates	Fraction of treated who held certain positions in a company after treatment: self-employed, entrepreneur, manager, board member, management board member; representative of the company; partner; head of brach

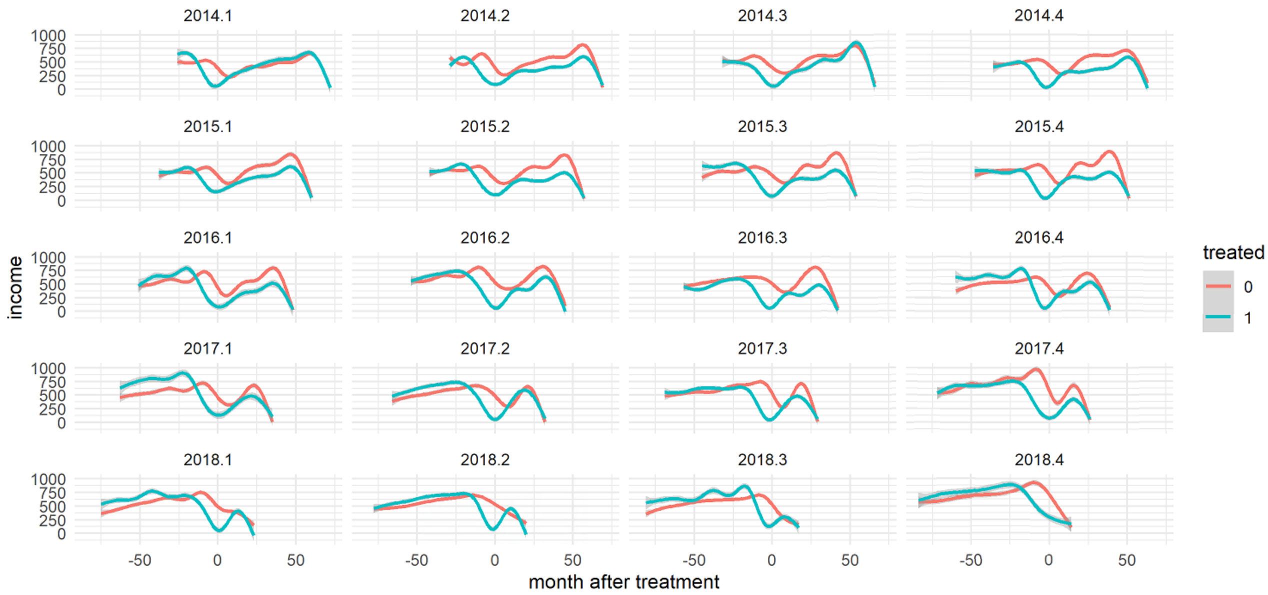
Appendix B: Marginal Effects on Income Dummy

Variable	1 year after treatment	2 years after treatment
Treated	**-0.0362***** (0.0123)	**-0.0231*** (0.0140)
Male	-0.0183 (0.0124)	-0.0274** (0.0140)
Mother tongue - Estonian	0.0570*** (0.0199)	0.0430* (0.0225)
Number of previous unemployment spells	0.0034 (0.1112)	0.0611* (0.1244)
Business experience	0.0000 (0.0137)	0.0070 (0.0156)
Age	0.0138*** (0.0045)	0.0092* (0.0050)
Age squared	-0.0002*** (0.0001)	-0.0001* (0.0001)
Capital	0.0363 (0.0273)	0.0120 (0.0311)
West	0.0148 (0.0304)	-0.0232* (0.0346)
Center	0.0644** (0.0318)	0.0489 (0.0361)
North-East	0.0012 (0.0392)	0.0003 (0.0441)
South-East	0.0116 (0.0270)	-0.0166 (0.0307)
Education secondary general	0.2065*** (0.0477)	0.2079*** (0.0604)
Education professional	0.2226*** (0.0478)	0.2253*** (0.0615)
Education vocational	0.2143*** (0.0478)	0.2116*** (0.0614)
Education higher	0.2616*** (0.0466)	0.2583*** (0.0603)
Average income 1 year before treatment	0.0002*** (0.0000)	0.0003*** (0.0000)

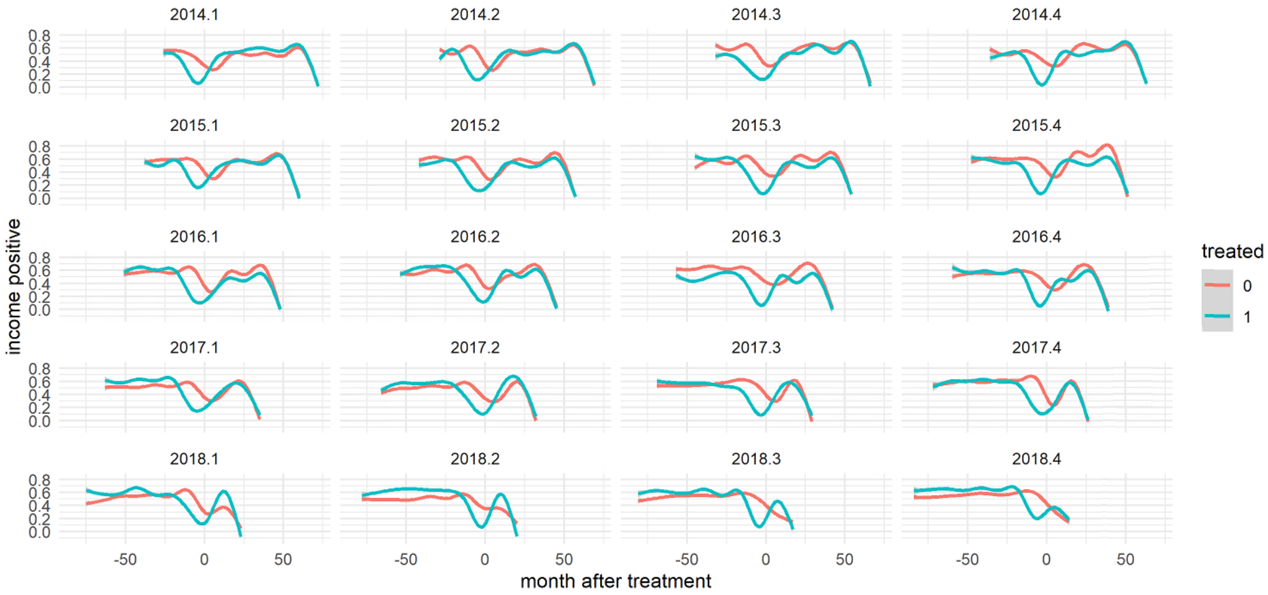
Notes: Robust standard errors. Significance: *p<0.1; **p<0.05; ***p<0.01

Appendix C: Outcomes in the Matched Sample (by treatment quarter)

(a) Mean Income in the Matched Sample

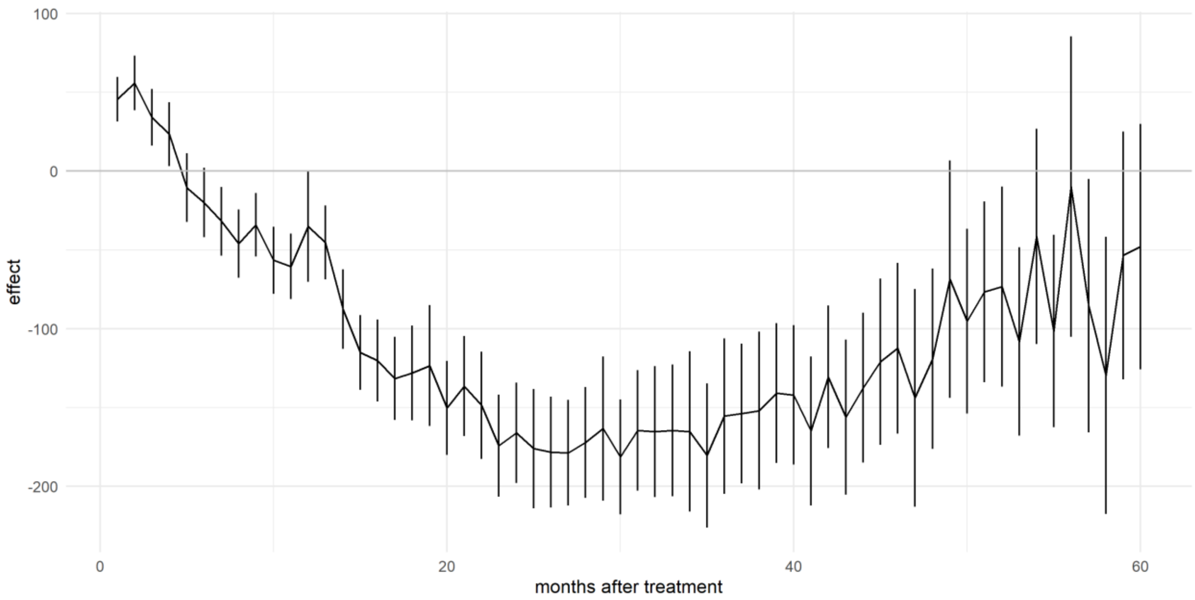


(b) Fraction of Positive Income in the Matched Sample

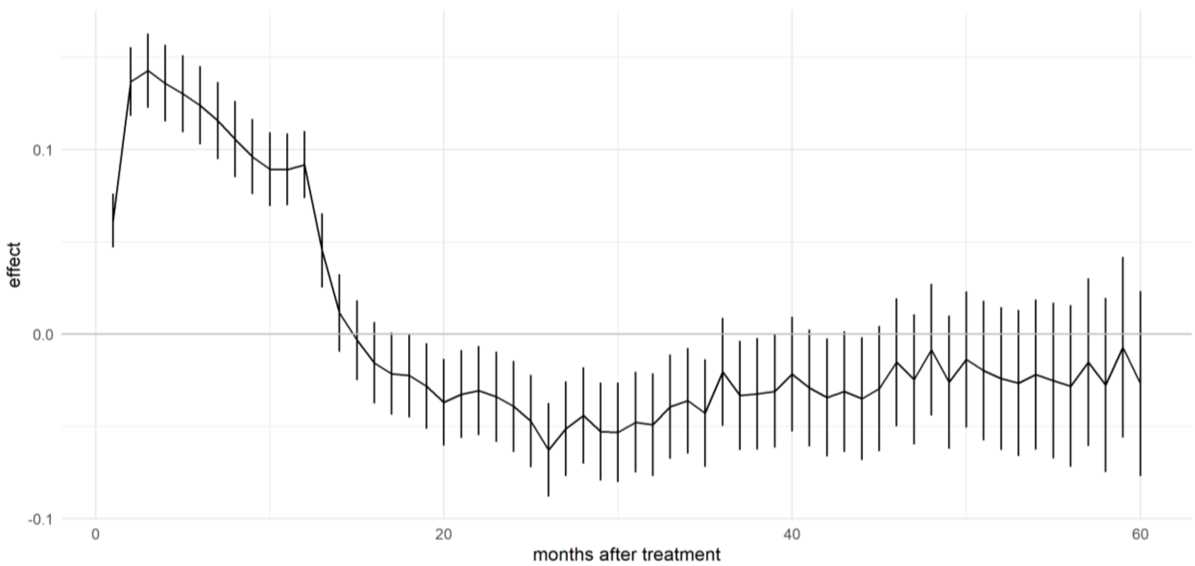


Appendix D: Results from Constrained Dataset

(a) Treatment Effects on Income



(b) Marginal Effects of Treatment on Income Being Positive



Notes: The results excluding the individuals who were registered as self-employed in the first year after treatment. The treatment effects on income in this variation are in fact slightly higher in absolute terms than those reported in Figure 2.

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