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Start-up Subsidies for the unemployed: Why do they seem so effective?

TAMÁS BAKÓ – JUDIT KÁLMÁN-GYÖRGY MOLNÁR

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ABSTRACT
By using Hungarian administrative data we evaluate the impact of a start-up subsidy programme on the labour market integration of the unemployed. When - following the generally accepted method - the control group included everyone who could have participated in the programme but did not, the effect of the support was positive and consistent with previous research. However, in contrast to numerous other active labour market programmes, a distinctive aspect of start-up support schemes is that the unemployed person leaves unemployment status immediately upon entry. Therefore, we also created a second control group, where only those members of the first control group were included, who exited unemployment at the same time as the treatment group. Although statistically significant positive effects were also found in the second control group, the effect size was only one-third to one-quarter of that in the first control group. Additionally, this second model is highly susceptible to unobserved heterogeneity. Thus, it seems that the strong positive effect is mainly due to the fact that a significant proportion of the members of the first control group perform worse than the members of the second control group in terms of the unobserved characteristic that is important for the labour market. Our results show that the support mostly helps groups with less favorable labor market prospects, so tightening the eligibility criteria could improve the efficiency of the programme.

JEL codes: H43, J68
Keywords: Start-up subsidies, Self-employment, Evaluation, Effect heterogeneity

Tamás Bakó
HUN-REN CERS
and Budapest Metropolitan University
bako.tamas@krtk.hun-ren.hu

Judit Kálmán
Corvinus University of Budapest
judit.kalman@uni-corvinus.hu

György Molnár
HUN-REN CERS
molnar.gyorgy@krtk.hun-ren.hu

Vállalkozásindítási támogatások munkanélkülieknek: Miért tűnnek olyan hatékonynak?

BAKÓ TAMÁS – KÁLMÁN JUDIT–MOLNÁR GYÖRGY

ÖSSZEFOGLALÓ

Magyar adminisztratív adatok felhasználásával értékeltük egy vállalkozás indítást támogató program hatását a munkanélküliek munkaerő-piaci integrációjára. Amikor - az általánosan elfogadott módszert követve - a kontrollcsoportba mindenki bekerült, aki részt vehetett volna a programban, de mégse vett részt, a támogatás hatása pozitív volt, és összhangban volt a korábbi kutatásokkal. Azonban számos más aktív munkaerő-piaci programmal ellentétben az vállalkozás alapítást támogató programok sajátossága, hogy a munkanélküli a belépést követően azonnal kilép a munkanélküli státuszból. Ezért létrehoztunk egy második kontrollcsoportot is, amelybe csak az első kontrollcsoport azon tagjait vontuk be, akik a kezelt csoporttal egy időben léptek ki a munkanélküliségből. Annak ellenére, hogy a második kontrollcsoport esetében is statisztikailag szignifikáns pozitív hatások mutatkoztak, a hatás nagysága csak harmada-negyede volt az első kontrollcsoportnál megfigyelt hatásnak. Ezenkívül ez a második modell nagyon érzékeny a nem-megfigyelt heterogenitásra. Úgy tűnik tehát, hogy a kimagasló pozitív hatás döntően abból fakad, hogy az első kontroll csoport tagjainak egy jelentős része a munkapiac számára fontos nem megfigyelt jellemző szempontjából gyengébb teljesítményt mutat, mint a második kontrollcsoport tagjai. Eredményeink azt mutatják, hogy a támogatás a munkaerő-piaci kilátásaikat tekintve kevésbé tehetős csoportok számára a leghasznosabb, így a jogosultsági kritériumok szigorítása javíthatja a program hatékonyságát.

JEL: H43, J68

Kulcsszavak: vállalkozásindítási támogatások, önfoglalkoztatás, hatásvizsgálat, heterogénhatások

1. Introduction

For some of the unemployed, either because of their business opportunities or out of necessity, self-employment can be a solution to labour market integration. However, unemployed people who aspire to start a business have to overcome serious obstacles. Due to their restricted financial resources, they frequently encounter significant credit constraints and are more prone to discrimination in capital markets. Their lack of confidence and skills, coupled with devalued social capital, further impedes their entrepreneurial endeavours (Hatala, 2005). In order to address these challenges, numerous countries provide start-up subsidies (SUS) for the unemployed. In some cases, these programmes also include counselling and assistance with the preparation of a business plan, as well as capital allowance. However, their primary objective is to provide financial support to ensure the livelihood of the unemployed in the initial months following the commencement of a business venture. The relatively limited but growing body of literature on this topic indicates that these programmes have a predominantly positive effect on the labour market integration of the unemployed (Odding at al., 2024, Tübbicke, 2024, Behrenz et al., 2016; Wolff et al., 2016, Caliendo & Künn, 2011).

The present study evaluates the effectiveness of the principal Hungarian SUS program based on rich administrative data. Our contribution to the existing literature can be described in three points. Firstly, although there are a few Latin American and Central European studies based on data from the 1990s, the overwhelming majority of studies dealing with the effectiveness of SUS for the unemployed are from highly developed countries. Therefore, it is important to examine whether or not similarly positive effects can be demonstrated in the Central European context based on data after EU enlargement.

Secondly, our novel research design reflects the fact that ALMPs can be divided into two groups, depending on whether or not participants exit unemployment at the same time as entering the programme. To illustrate, in the case of training, retraining, and job search assistance, participants typically remain unemployed following their entry into the programme and can only exit unemployment at some point subsequent to the programme's end (if they succeed at all). In contrast, in other types of programmes, such as hiring or start-up subsidies, participants leave unemployment as soon as they enter the programme. That is why, our approach differs from that of the majority of previous studies in that it compares programme participants with two distinct control groups. The first group includes all those who, despite having the opportunity to participate in the programme, did not do so - regardless of whether they left unemployment at the same time as the participants in the programme or not. This is the generally accepted control group that has been used in the vast majority of studies to date. The second control group is a subset of the first, consisting only of those who leave unemployment at the same time as the treatment group. This approach allows for a more detailed analysis of the impact of SUS programmes and provides a basis for comparison with similar types of ALMPs.

Thirdly, from a policy perspective, it is crucial to reveal which groups the SUS program provides the most help to. The research to date indicates that the SUS programmes is the most effective among those with unfavourable labour market prospects, in this respect low education was the most studied characteristic (Caliendo and Künn, 2011, Behrenz at al, 2016, Odding et al, 2024, Rodriguez-Planas, 2008). One of the key objectives of this research is to extend the range of characteristics that can be used to identify the groups that derive the greatest benefit from the SUS programme.

The paper is organised as follows: we review the literature, after that we provide a brief presentation of the Hungarian Start-up Subsidy scheme for the unemployed. In Section 4, we describe our data, the formation of treatment and control groups, the creation of the outcome and control variables, and present the descriptive statistics along with the econometric method and estimation procedure. The main results as well as the heterogeneous effects are presented in Section 5. After that, we perform a detailed sensitivity analysis, the results of which are described in Section 6. Finally, in Section 7, we summarise our findings and raise further research questions.

2. Literature

In developing and transition countries, only a limited number of studies have addressed the evaluation of the impact of programmes designed to support the unemployed in becoming self-employed. An early example is the study by O'Leary (1999), which examined the effect of the SUS on Hungarian and Polish data and found positive employment effects for both countries. However, due to the relatively small number of observable characteristics, the use of non-administrative data, and the lack of a sensitivity analysis, the results should be interpreted with caution. Rodriguez-Planas (2008) conducted a study to assess the efficacy of a multifaceted self-employment initiative in Romania. The programme provided participants with access to counselling, short-term entrepreneurial training, and loans for working capital. She found a positive effect in terms of employment, while no significant difference was detected between participants and non-participants in the case of income. Based on Romanian follow-up survey data Rodriguez-Planas and Benus (2010) found that self-employment assistance – besides training and retraining as well as public employment and relocation services – had positive effects on the labour market integration of the participants. Using Argentinian household survey, Almeida and Galasso (2007) evaluated the short-run labour market effect of self-employment assistance programme for welfare beneficiaries. They found that the program reduced the probability of having an outside job and increased the total number of hours worked, but did not prove to be effective in increasing the average income of the participants. To the best of our knowledge, all other studies since 2000 are based on data from developed countries.

Carling and Gustafson (1999) conducted a comparative analysis of employment subsidies and self-employment grants for the unemployed in Sweden. Their findings indicated that individuals engaged in subsidised employment exhibited a higher probability of re-entering the unemployment pool compared to those who received self-employment grants. Using French data, Crépon and Duguet (2003) study the effect of initial capital and of its structure (loan, or start-up subsidy) on the survival of firms three years after their founding. The authors showed that the start-up subsidy has a positive effect on the survival of firms if the founder was previously unemployed, but has no detectable effect if the company was created by a previously employed person. Pfeiffer and Reize (2000) study the effect of bridging allowances on the survival of firms created by unemployed people in East and West Germany. In West Germany, the effect of subsidy is insignificant, so the authors conclude that the influence of bridging allowances on firm survival is not positive. At the same time, a significant negative effect was shown in Eastern Germany, i.e. firms participating in start-up support had a shorter survival time than non-participating firms with similar characteristics. The authors explain this result with the cash and carry effect: some of the unemployed who received support for their business were only interested in making money in the short term and closed their business as soon as the programme ended.

The long-term effects of start-up subsidies for the unemployed were first demonstrated by Caliendo and Künn (2011). The authors use administrative and survey data to assess the effects of two programmes to support the unemployed to become entrepreneurs in West Germany. They find positive long-run effects for both programmes on employment probabilities and earnings. Michaelides and Benus (2012) were the first to examine the impact on employment of an American self-employment support programme that provided training only - without any financial support. The authors concluded that, for the unemployed, self-employment training had a positive effect on overall employment even five years after entering the programme.

To the best of our knowledge, only two studies have been conducted to date, in which the treated group was compared with different control groups. One of these is the study by Behrenz et al. (2016), which compares SUS participants with three different control groups (all eligible non-participants; job-search assistance recipients only; participants in other active labour market programmes). Based on Swedish administrative data, the authors showed that the probability of moving into unsubsidised employment or education is significantly higher for SUS participants than for the members of all above-mentioned groups. It is worth noting that the results of the comparisons with the different control groups did not show much difference in terms of the strength of the effect. In the other study, Odding et al. (2024) argued that the traditional control group, which includes non-applicants, and rejected non-applicants, may cause selection bias, thereby violating the conditional independence assumption (CIA). In order to test the violation of CIA the authors compare (1) accepted applicants versus non-participants (non applicants and rejected applicants), (2) accepted applicants versus rejected applicants; and (3) applicants versus non-applicants. The authors found strong positive employment effects for all three of the above models.

Caliendo and Tübbicke (2021) evaluated the 2011 reform of the German SUS programme for the unemployed. Their results showed that the reform was successful in increasing average employment effects by tightening eligibility conditions and reducing subsidy levels. To the best of our knowledge, there has not been a study since the 2000s that has not found a significant positive employment effect associated with SUS programmes. Nevertheless, it is worth reviewing which groups of the unemployed the SUS provides the most help for. Based on the results so far, there seems to be a consensus that we can expect a more favourable employment effect for people with low education than for people with higher education (Caliendo and Künn 2011, Behrenz at al., 2016, Rodriguez-Planas, 2008). Caliendo and Künn (2011) found that the Bridging Allowance (BA) and SUS programmes for the unemployed were more effective for German participants than for non-German participants. Furthermore, Caliendo and Künn (2014) concluded that the employment impact of BA and SUS programmes depends on the prevailing economic conditions at the local level. Oddis at al. (2024) found that the effect of the programme on employment is larger for non-western immigrants than for native Dutch or western immigrants.

3. Start-up subsidy for the unemployed in Hungary

Start-up subsidies for the unemployed have already been applied by Hungarian labour market policy since 1996.[1] These programmes were typically implemented by the Public Employment Service (PES) and its regional offices in cooperation with the Vocational Training Centres and the National Employment Fund. Thanks mainly to EU funding, the range of SUS schemas has been steadily expanding since 2010, differing mainly in terms of the target groups reached and their complexity. Using the panel database of participants, we found that the general SUS programme had the highest number of participants and observations of all SUS schemes over the period under review[2]. It is also noteworthy that the general SUS programme has remained essentially unchanged beyond the examined period, whereas no other SUS scheme is currently available. For the reasons mentioned above, we have chosen the general SUS programme, which is funded exclusively by the Hungarian state, as the subject of our study. Only those who had been officially registered as unemployed for a minimum of one month were eligible to receive support and there were no additional criteria such as age, education or place of residence. It should be noted that there was no mandatory training component prior to or during participation in the SUS programme. However, the regional PES offices did provide some limited business consultation. Furthermore, external consulting or training costs could have been partially covered by the scheme. The support comprised two principal elements. The first was the provision of a basic income, akin to a minimum wage, for a period of six months to individuals who had been unemployed and were starting a business. This income was tied to the actual minimum wage and was higher than the unemployment benefit. The second element was the possibility of applying for a lump sum capital grant for the purpose of starting a business. This grant was non-refundable and amounted to 3.5 million Hungarian forints (HUF), which is equivalent to between 10,000 and 12,000 euros.

4. Data and Methodology

4.1 Data

Our empirical analysis is based on an administrative dataset from Hungary, provided by the HUN-REN Centre for Economic and Regional Studies databank, covering the period from 2003 to 2017. This rich individual-level dataset covers half of the randomly selected Hungarian

[1] The 1996 amendment of Act No 4 of 1991 on the promotion of employment and unemployment benefits.
[2] We have data on participants in active labor market programs between 2009 and 2017. During this period, nearly 20 different programmes helping the unemployed become entrepreneurs were available for longer or shorter periods of time.

population aged 0-74 in 2003 and includes the linked records of the PES, tax, pension and health authorities. The database contains detailed individual-level information on employment, unemployment spells, including data on participation in active labour market programmes, earnings, social benefits, pensions and use of health services. In addition, we also used the T-Star database containing economic, social and infrastructure data at the settlement level. The T-Star database is managed by the Central Statistical Office and contains extensive settlement data for the given year or one of its dates, broken down by settlement. These data do not come from data collection by the Central Statistical Office, but from state administration procedures.

4.2 Treatment group and control groups

Within the period covered by the database, those who received support from the general SUS programme in the first half of 2011 formed the treatment group. When selecting the period, it was important to ensure that there was a sufficiently long period of time after the end of the programme to monitor employment and that no other SUS programme was available during the selected period. The most serious shortcoming of the data available to us is that we cannot identify who among the treated received only income support and who also received a non-repayable capital grant. In line with our research objectives, we created two control groups. The larger control group includes all unemployed people who could have participated in the programme in the first half of 2011 but did not, regardless of whether they left unemployment at a similar time as the treated or not. In addition to the first control group, a second, narrower control group was created, which included only those unemployed people who could have participated in the programme but did not, and who left unemployment in the same period as the treated.

4.3 Outcome and control variables

The success of such programmes in helping the unemployed to get back into the labour market can be measured along a number of dimensions. As is common in the literature, we used the binary outcome variable 'self-employed or regular employed' as a measure of programme effectiveness at 6, 12 and 24 months after entry. Since the aim of the programme is to reintegrate the unemployed into the labour market, 'regular employment' is as favourable an outcome as self-employment. Moreover, starting a business allows the unemployed to develop contacts and to gain self-confidence (Molnár, 2017) that may help them to find regular work in the future. In addition to examining labour market status at a given point in time, we also used the total number of months worked in 'regular employment or self-employment' as another outcome variable. This outcome variable was calculated for both the 24 months after entry and the 24 months after the programme, as well as after the end of the programme. Another important outcome variable used in many papers is income. However, we do not examine this in the present study because we have serious doubts about the reliability of our data on entrepreneurial income. According to Lelkes and Benedek (2011), the self-employed only declare around 23% of their wages, compared to 96% for the employed, so that - taking into account the fact that the control group has a higher proportion of employees after leaving unemployment than the treatment group - we may underestimate the impact of the programme.

An important feature of the econometric method applied is the identification of non-participants who are similar to the treated on all relevant pre-treatment characteristics. The linked administrative data we used allowed us to include the most important pre-treatment characteristics in the analysis. In addition to basic demographic variables such as gender, age and education, we also controlled for the type of job sought. Accordingly, we created three dummy variables: (1) manual jobs, (2) non-managerial or white-collar jobs, and (3) managerial jobs; manual jobs were the reference category in the estimations.

Employment history may also influence the likelihood of the unemployed person becoming self-employed, so we created additional dummy variables indicating (1) whether the unemployed person was an entrepreneur at any time since 2003, (2) whether the unemployed person held a managerial position at any time since 2003, and (3) whether the unemployed person is a career starter or not. We have created a variable that indicates how many months the individual was unemployed in the two years prior to entering the programme, as this may be another important indicator of the unemployed person's chances of finding a job on the labour market and their ability to create a permanent job.

For people with disabilities, self-employment can be an important and viable option for re-entering the labour market, offering greater flexibility in terms of workload, daily schedule and commuting (Pagán, 2009). To control for the possibility that PES officials treated people with disabilities differently, we include in the propensity score estimation a dummy variable indicating whether or not the unemployed person received a disability benefit. We also include in the model a dummy variable that takes the value 1 if the unemployed person participated in any ALMP in the 24 months prior to entering the programme.

To characterise the financial situation of the unemployed, we created two variables, one containing the total amount of unemployment benefits in the 6 months before entering the programme, and the other the total income in the 12 months before entering the programme. It is worth noting that the share of entrepreneurial income in total income before treatment is not significant, so it was not distorted to the same extent as after treatment.

Substantial differences in the number of job opportunities and the economic environment can be observed within the country, so we used dummy variables to control for each region (the reference category was the Central Hungary region during the estimations). As socio-economic differences do not only occur at regional level, we also included a dummy variable indicating whether the district requires complex development or not - a widely used indicator of backwardness. The value of this dummy variable depends on a composite index consisting of social and demographic indicators, housing and living conditions, the local economy and labour market, as well as infrastructure and environmental indicators[3]. The use of the composite indicator of district development as a control variable is justified by the fact that the more developed a district is, the higher the share of start-up subsidy recipients among the unemployed.

4.4 Descriptive statistics

Table A1 in the appendix presents the descriptive statistics of the sample for the first half of 2011, disaggregated by participants and two distinct categories of non-participants. In comparison to the first and second control groups, a significantly higher proportion of those who received treatment had attained a secondary or higher level of education. Additionally, a greater number of these individuals had previously engaged in entrepreneurial or managerial activities. In accordance with the differences in educational attainment, a significantly higher proportion of those treated are seeking white-collar and managerial roles compared to the members of the first or second control group. Among those treated, there is a somewhat smaller proportion of individuals entering the labour market for the first time, but a larger proportion of those who have previously participated in an active labour market programme.

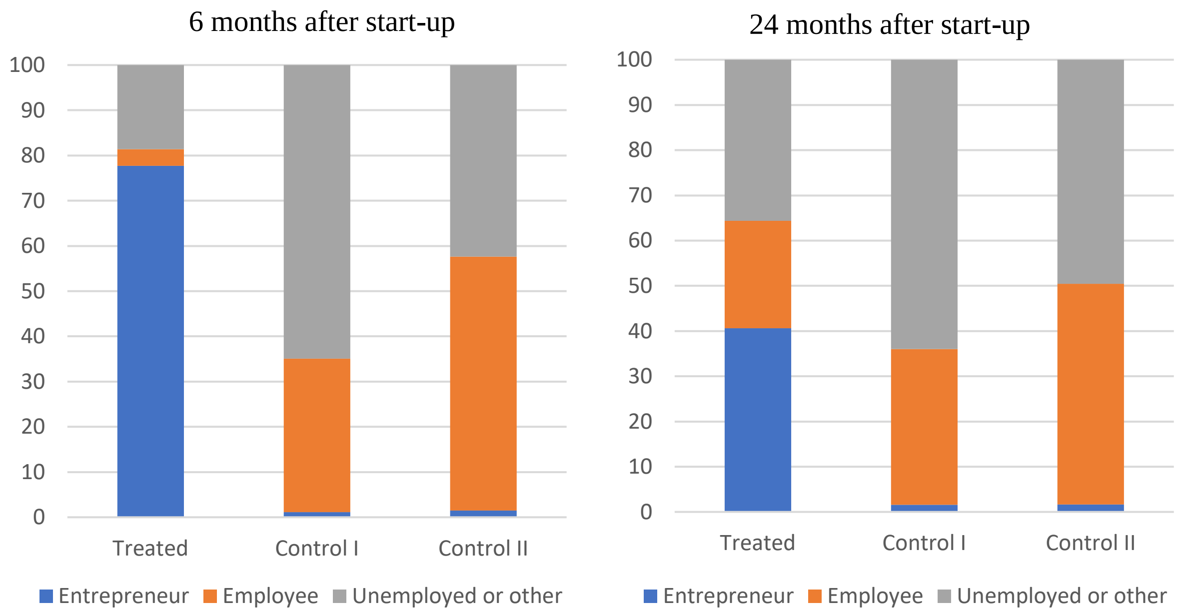
If we look at the income situation, we see that the treated received on average 67% more unemployment benefits during the 6 months before entering the programme, and that their total income in the 12 months before entering the programme was on average 46% higher than that of the members of the first control group. In the case of the second control group, we see that their total income during the 12 months before leaving unemployment was on average 12%

[3] The content of the composite index is specified in Government decree 290/2014 and is calculated by the Hungarian Central Statistical Office.Districts with a value of the composite indicator below average belong to the so-called favoured category. This category contains the category of districts to be developed and those districts in the worst situation belonging to the lowest 10 percent are called to be developed with a complex programme.

higher than that of the treated, but at the same time their total unemployment benefits received during the 6 months before leaving unemployment was only 63% of that of the treated. A slightly smaller proportion of the treated live in Central Hungary, the most developed region of the country, but at the same time a smaller proportion live in the most underdeveloped districts requiring complex development compared to the members of the first and second control groups.

Figure 1 shows the labour market status 6 and 24 months after entry for participants and the different groups of non-participants. Our first observation is that six months after the start of the programme, almost twenty percent of the treated are unemployed or inactive. It seems that for some of the entrepreneurs the business was only sustainable during the period of support and as soon as it ended, they became unemployed again. Another observation is that, over time, the proportion of self-employed among the treated decreases, while the proportion of employees increases. These data raise the possibility that for some of the unemployed, becoming a self-employed person was just a stepping stone to becoming an employee.

Figure 1. Labour market status



It is worth noting that in the 24th month after the start of the programme, almost 65 per cent of the treated people are entrepreneurs or employed, while in the first and second control groups this ratio is 35.99 and 50.46 per cent respectively. Of course, the above differences in employment rates may also be due to the fact that the treated groups are better off than the control groups in terms of labour market characteristics. The purpose of the econometric method we use is precisely to eliminate possible biases caused by non-random selection.

4.5 Econometric method and estimation procedure

The objective of our empirical strategy is to demonstrate a causal effect between participation in the programme and subsequent labour market integration of the unemployed. To this end, we employ the potential outcome approach, also known as the Rubin model (Rubin, 1974). Denote the potential outcome Y^1 if the individual participates in the program $(D = 1)$ and Y^0 if the individual could participate, but does not $(D = 0)$. Accordingly, the treatment effect can be calculated for each individual as the difference in potential outcomes: $\tau_i = Y_i^1 - Y_i^0$. It is not possible for the same person to be in the treatment and control groups at the same time, so the two possible outcomes are not observed. Alternatively, we can assess the average treatment effect on the treated (ATT), which can be calculated as follows: $\tau_{ATT} = E(D = 1) - E(D = 1)$. As previously stated, the ATT may be subject to bias due to the non-random assignment of individuals to the treated and control groups. This may result in the two groups exhibiting differences in terms of factors that influence the outcome. To mitigate this selection bias, propensity score matching techniques can be employed to ensure a balanced distribution of observed characteristics between the treatment and control groups.

The assumption of conditional independence (CIA) is a fundamental tenet of matching methods, which posit that program participation and outcomes are independent, conditional on the observed characteristics. However, the identification of the average treatment effect (ATT) requires not only the CIA but also that the probability of non-participation is positive for all distributions of the observable characteristics, a condition known as the overlap assumption.

In the first step of the matching procedure, we used a logit model to estimate the propensity score for participation in the program versus non-participation. The results of the logit estimation for both models are presented in Table A2 of the appendix.

The results of the propensity score estimation were as expected: where there was a relatively large gap in descriptive statistics between the treated and the control groups, a significantly nonzero coefficient was obtained in the logistic estimation. The only exceptions to this are the sought-after profession categories, where the descriptive statistics show a large difference, yet the propensity score coefficient is not significant. This is because job aspirations are strongly related to educational attainment.

In order to test the fulfilment of the overlap condition, the distribution of propensity scores was plotted, and as Figure A1 of the Appendix shows, the propensity score distributions of participants and non-participants were found to be completely overlapping. Subsequently, the average treatment effect on the treated was estimated using the propensity score matching method.

The degree of balance in the distribution of observable variables between the treated and non-treated groups is illustrated by the various measures of matching quality presented in Table 3 of the Appendix. In the initial row of the table, we indicated the number of variables that exhibit a statistically significant difference between the participants and non-participants as determined by the t-test. It can be seen that, prior to the matching process, 19 and 17 of the 23 variables exhibited statistically significant differences. However, following the matching, no significant differences were observed for any variable. Moreover, there is a notable reduction in the mean and median values of the standardised bias, which also suggests that the matching process was effective. Another indicator of successful covariate balancing is the pseudo R^2 of the propensity score estimation, which is considerably lower in the matched sample than in the raw sample.

5. Results

The following section presents the main findings from the two control groups, along with an analysis of the specific subgroups where the programme demonstrated the greatest efficacy. The main results of the matching process are presented in Table 1. The first column (Model I) comprises the estimation results for the largest control group, namely those who were unemployed during the first six months of 2011 and were therefore eligible for the programme. As can be observed, the programme's impact is positive for all selected points in time, as well as for all specified time intervals. However, the strength of the impact gradually decreases over time. In the sixth month after entering the programme, the employment probability of participants was 36.8 percentage points higher than that of non-participants. By the 24th month, this difference had decreased to 16.9 percentage points.

With regard to the cumulative employment effect, the findings revealed that participants spent, on average, 5.23 months longer in employment or self-employment than non-participants over the 24-month period following their entry into the programme. The cumulative effect on employment from the start of the programme may be biased upwards, as the treated people receive financial support for their subsistence during the first six months of the programme. For this reason, we have also provided an estimate of the cumulative effect from the end of the subsidy, for which we have only taken into account the effects created in the period after the end of the initial financial support. Participants in the programme spent, on average, 4.5 months longer as employees or entrepreneurs than members of the control group during the observation period. Overall, the results are similar to those obtained by Caliendo and Künn (2011) on German data.

Table 1. Casual effects of start-up subsidy

	Model 1	Model 2
Difference in percentage points	Employee or Entrepreneur	Employee or Entrepreneur
6 months after start-up	36.8***	18.3***
	(2.07)	(1.96)
12 months after start-up	26.21***	8.86***
	(2.12)	(2..00)
24 months after start-up	16.9***	5.03**
	(2.25)	(2.10)
Difference in months		
24 months after start-up	5.23***	1.629***
	(0.37)	(0.370)
24 months after end of subsidy	4.5***	0.9**
	(0.404)	(0.397)
Observations:	287652	145246

Notes: Table shows the average treatment effects on the treated. Standard errors are in parentheses and were calculated according to Abadie and Imbens (2009). The statistical significance at the level of 10%, 5%, and 1% is represented by *, ** and ***, respectively.

We argue that the results obtained with the first control group do not provide a complete picture of the programme's effectiveness. It is also important to compare those treated with unemployed people who, like them, also left unemployment in the first half of 2011. In this case, as we expected, the results are far less favourable (see model 2, table 1). Although the effects observed at months 12 and 24 are still significant and positive, their strength is only less than one third of the corresponding values in Model 1. As for the cumulative effect after the subsidy ends, perhaps even more spectacular is the decline in the magnitude of the effect, which is less than a quarter of the values obtained when estimating model 1. It seems that the considerable positive impact observed in the case of the first model is likely attributable to the fact that a significant proportion of the members of the first control group perform worse than the members of the second control group in terms of the unobserved characteristic that is important for the labour market.

5.2 Effect heterogeneity

One of the noteworthy features of start-up subsidy programme under examination is its availability to almost all unemployed people. Consequently, it can be postulated that the degree of assistance provided will vary contingent on the specific characteristics of the individual in question. Identifying the groups that would derive the greatest benefit from participation could help the development of a more targeted start-up subsidy programme than the current one. To this end, we looked for characteristics that are easily verifiable by public employment services, but which have received little or no attention in previous research.

The first characteristic under consideration is the total number of months spent unemployed in the 24 months prior to exit. The sample was divided into two parts: the first comprising those who spent more than 12 months in total, and the second comprising those who spent less than or equal to 12 months in unemployment during the 24 months preceding exit from unemployment. Subsequently, the complete estimation procedure was repeated on both subsamples, and the resulting data are presented in Table A4. It can be seen that participants with more months of unemployment in the two years prior to entry perform better in terms of employment prospects; in the case of the first model, the cumulative effect from the end of the program is 3.7 months larger than for those with fewer months of unemployment. In the case of the second model, the difference is even more convincing, we see significant positive cumulative effects only among those with more months of unemployment.

Another important characteristic to consider is whether the individual in question received unemployment benefits prior to joining the programme. The absence of such benefits suggests that the unemployed person's financial reserves are likely to be limited, which may have a detrimental impact on the likelihood of success in becoming an entrepreneur and the extent to which individuals may be inclined to pursue this exit strategy. Accordingly, the sample was divided into two groups based on whether the participants and non-participants received unemployment benefits within the six months preceding their unemployment status. Our results show that support is much more effective among those who did not receive unemployment benefits before entering the programme. This also confirms the previous finding (O’Leary, 1999, Caliendo and Künn, 2011) that this program provides the greatest help to those who are characterized by poor financial and labour market prospects. This phenomenon may be linked to the 6-month minimum wage benefit: at the start-up stage, the livelihood of those who have no other source of income is guaranteed.

A substantial amount of empirical evidence has been gathered concerning the gender-specific impact of other active labour market programmes. A comprehensive meta-analysis conducted by Bergeman and van der Berg (2008) revealed that active labour market policies, including skill-training programmes, monitoring and sanctions, job search assistance, and employment subsidies, had a notable positive effect on employment outcomes for women, with the observed effects being more pronounced than those observed for men. While Caliendo and Künn (2015) demonstrated a positive impact on employment outcomes for a female unemployed sample in the context of German start-up programmes, it remains unclear whether the support exerts a more pronounced effect on women's employment prospects than on men's. In the case of the first model (see table A5), we found that female participants perform better than their male counterparts with a difference of 1.56 months in terms of the cumulative effect from the end of the program. However, in the second model, the difference in impact between male and female participants is negligible.

Finally, we wish to address a feature that has not yet been the subject of discussion, but which could prove to be of significant policy relevance. The data, as presented in Table A1. in the Appendix, indicate that 9% of the members of the first control group and 12.5% of those who had received treatment had already participated in some form of active labour market programme during the 24 months prior to entering the SUS programme. The results of the first model show that the effect of the SUS is stronger among those who have not participated in a labour market programme in the 24 months prior to entry than among those who have already participated in at least one active labour market programme before (see table A6). A similar conclusion can be reached for the second model, although the effect sizes are much smaller and the cumulative effects after entry are not significant in either subsample.

6. Sensitivity Analysis

The following section examines the robustness of the results with respect to potential deviations from the identification assumptions.

6.1 Expansion of explanatory variables and application of alternative matching methods

Although using a wide range of conditioning variables during the estimation of propensity scores may enhance the similarity between participant and non-participant groups; this approach may also exacerbate the common support region problem. Moreover, a substantial body of research has shown that the results of propensity score matching are sensitive to the choice of covariates (Heckman, Ichimura, Smith, & Todd, 1998; Heckman, Ichimura, & Todd, 1997; Lechner and Wunsch, 2013). Therefore, only the most crucial variables with potential theoretical relevance were included in the baseline estimates.

On the other hand, the database permits the estimation of propensity scores to be augmented with the incorporation of additional conditioning variables. Such variables include data on healthcare expenses, with the total costs of outpatient care and all healthcare expenses incorporated into the logit regressions. For both variables, the total costs over the 12 months prior to leaving unemployment were considered. Additionally, to characterise the economic environment of the district, the natural logarithm of the number of individual businesses per thousand inhabitants was included during the propensity score estimation.

Tables A7 and A8 in the appendix presents the results of the extended models for the two control groups, applying three distinct matching techniques. As can be observed, the results obtained for the first and second control groups are highly comparable to the baseline estimate.

6.2 Rosenbaum Bounding approach

It is reasonable to ask the question how strong the effect of a potential unobserved variable must be to challenge the results by influencing the selection process? To answer this question, we use the Rosenbaum's bounding approach (Rosenbaum, 2002). The idea of this method is to introduce an artificial factor to simulate the unobserved term and then testing to what extent this factor affects the significance of our results (Becker and Caliendo, 2007).

Given that we showed a significant positive effect in both model 1 and model II (see table A7), the question is whether the unobserved factor leads to positive selection, which may result in an overestimation of the treatment effect. That is why we indicated only the test-statistics for the upper bound (Q^+) and the respective p-values (p^+). Table A9 contains the test statistics of the two outcome variables for both models. One is the binary 12-month "Entrepreneur or employed", while the other is the continuous 24-month cumulative "Entrepreneur or employed" outcome variable.

The absence of unobserved heterogeneity is indicated by a value of 1 for the artificial variable (Γ), in which case a significant positive effect is found for both models and all outcome variables (p^+<0.05). From this starting point, we increased the value of Γ by 0.1 at each step up to 2 in order to simulate an ascending influence of the unobserved factors. Regarding to the short-term (12-month) effects, we found that the first model is robust to the effect of the unobserved variable, the result remains significant up to $\Gamma = 2$, while the second model is not robust, the result is already insignificant at $\Gamma = 1.3$ (at 5 percent significance level). Similar results were also obtained in the case of cumulated effects, the first model is highly robust with regard to both cumulated variables, while the results of the second model are quite sensitive to the influence of the unobserved variable. Although we do not know for sure whether there is unobserved heterogeneity in the case of the second model either, based on the tests we can only say that the results of the second model should only be treated with sufficient caution.

7. Conclusion

In this study, we examined the impact of an active labour market programme supporting the unemployed to become entrepreneurs on employment in Hungary. The scheme provides the participants with support equal to a maximum of 6 months' minimum wage, as well as - based itied to the fulfilment of strict requirements, the most important condition was that the applicant had to be registered unemployed for at least 1 month before the application and only those who also applied for capital support had to submit a business plan. The vast majority of previous research has concluded that this type of support is effective and, at least based on German data, is superior to many other active labour market programmes in terms of effectiveness. Our analysis was carried out using propensity score matching methods and rich administrative data. We were interested in whether the SUS intended for the unemployed has an effect on employment in Hungary, and if so, whether this effect changes depending on whether the members of the control group exit unemployment at the same time as the treated ones or not. According to our results, the support has a positive effect on employment in both cases, but the strength of the effect is much smaller, if the members of the control group exit unemployment at the same time as the treated ones. In this case, the effect is not only smaller, but also more uncertain, as the results are sensitive to unobserved heterogeneity. Based on all of this, it seems that the outstanding effect of the SUS programs for the unemployed largely stems from the fact that the that a significant proportion of the members of the first control group (those who do not exit unemployment at the same time as the treated) perform worse than the members of the second control group in terms of the unobserved characteristic that is important for the labour market.

In order to reveal which groups the program has the greatest impact on, we re-estimated the effect of subsidy for subgroups stratified by different characteristics. Our results confirmed previous experiences, according to which the program is especially effective for the disadvantaged. These include those who are often exposed to unemployment or who no longer receive unemployment benefits. A new result is that, compared to men, the program has a somewhat greater effect on the employment prospect among women. We also found that among those who have not participated in any ALMP within two years prior to entry, the program is more effective, so this result can help make this policy tool more targeted and thus more effective.

While the present study has yielded several novel findings, it is not without limitations. Firstly, the treatment was not homogeneous, with some individuals receiving income support alone, while others received both income support and non-refundable capital support. A further avenue for investigation, should the data permit, would be to ascertain whether the impact of the programme on employment differs according to whether the unemployed also receive capital support. Furthermore, the limitations of the available data preclude an examination of the potential deadweight loss associated with the programme. Such an outcome may occur if the unemployed individual would have established the business in question even in the absence of the subsidy, and would have achieved a similar level of success. In order to estimate the deadweight loss, it would be essential to identify those unemployed persons who, although eligible for the SUS scheme for the unemployed, for some reason did not participate and nevertheless left unemployment as self-employed.

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APPENDIX

Table A1

Descriptive statistics

	Treated	Control 1	Control 2
Number of Observations	946	286706	144300
Age	37.58	38.85	37.42
Age squared	1645.04	1644.99	1521.57
Men	0.498	0.523	0.566
School achievement (Ref.: Elementary)			
Vocational	0.342	0.333	0.355
Secondary	0.421	0.264	0.271
Tertiary	0.151	0.054	0.058
Manager before	0.098	0.039	0.042
Entrepreneur before	0.185	0.065	0.063
A sought-after profession (Ref.: Bluecollar)			
Whitecollar	0.270	0.159	0.161
Manager	0.081	0.031	0.033
Beginner	0.053	0.084	0.087
Program participation during the two years prior to exit (Ref.: No)	0.126	0.091	0.103
Total number of months affected by unemployment in the two years prior to exit	15.81	18.56	16.37
The amount of UB for 6 month before exit (HUF)	661195.40	393924.70	419733.80
The amount of total income during one year prior to exit (HUF)	278104.50	191077.40	312629.50
District requires complex development (Ref. No)	0.114	0.177	0.181
Disabled (Ref.: No)	0.013	0.033	0.019
All health expenses incurred in the year before exit	16220.63	25108.18	18145.89
All outpatient care expenses incurred in the year before exit	9110.84	9419.01	7679.53

Natural logarithm of the number of self-employment per thousand people in the district	4.694	4.683	4.689
Hungarian regions (Ref.: Central Hungary, NUTS2 level)			
Central Transdaubia	0.124	0.117	0.123
Western Transdanubia	0.136	0.085	0.090
South Transdaubia	0.201	0.124	0.119
North Hungary	0.130	0.186	0.183
North Great Plain	0.219	0.232	0.234
South Great Plain	0.111	0.162	0.164

Note: Numbers are percentages unless otherwise stated.

Table A2

Propensity score estimation

	Model I	Model II
Age	0.222***	0.191***
	(0.0290)	(0.0296)
Age squared	-0.00295***	-0.00248***
	(0.000361)	(0.000370)
Men	-0.0268	-0.0515
	(0.0690)	(0.0697)
School acievement (Ref.: Elementary)		
Vocational	1.217***	1.166***
	(0.126)	(0.126)
Secondary	1.646***	1.631***
	(0.131)	(0.132)
Tertiary	2.029***	2.009***
	(0.168)	(0.169)
Manager before (Ref.: No)	0.134	0.127
	(0.120)	(0.121)
Entrepreneur before (Ref.: No)	0.741***	0.802***
	(0.0900)	(0.0904)
A sought-after profession (Ref.: Bluecollar)		
Whitecollar	-0.0338	-0.0164
	(0.0939)	(0.0945)
Manager	0.163	0.204
	(0.152)	(0.153)
Beginner (Ref.: No)	0.0655	-0.265
	(0.176)	(0.176)
Program participation during the two years prior to exit (Ref.: No)	0.302***	0.162
	(0.1000)	(0.100)
Total number of months affected by unemployment in the two years prior to exit	-0.0430***	-0.00538
	(0.00661)	(0.00699)
The amount of UB for 6 month before exit	1.10e-07***	2.41e-07***
	(3.72e-08)	(4.22e-08)
The amount of total income during one year prior to exit	-2.66e-07***	-5.67e-07***
	(9.64e-08)	(1.12e-07)
District requires complex development (Ref. No)	-0.239**	-0.293***

	(0.109)	(0.109)
Disabled (Ref.: No)	-0.414	-0.0759
	(0.294)	(0.295)
Hungarian regions (Ref.: Central Hungary, NUTS2 level)		
Central_td	0.333**	0.241
	(0.149)	(0.149)
Western_td	0.670***	0.607***
	(0.146)	(0.147)
South_td	0.864***	0.845***
	(0.138)	(0.139)
North_hun	0.0722	0.0310
	(0.149)	(0.149)
North_gp	0.392***	0.354**
	(0.139)	(0.140)
South_gp	-0.0706	-0.125
	(0.152)	(0.153)
Constant	-10.45***	-9.872***
	(0.605)	(0.615)
Observations	287,652	145,246

Note: * 10%, ** 5%, *** 1% significance level.

Table A3

Matching quality

	Model 1		Model 2	
	Before matching	After matching	Before matching	After matching
T-test of equal means*	19	0	17	0
Standardized bias				
Mean standardized bias	18.8	2.1	15.3	3
Median standardized bias	16.9	1.8	14.6	3.1
Pseudo-R	0.067	0.002	0.066	0.005

*The cells show the number of variables that are significantly different between treated and control subjects based on the t-test.

Figure A1: Overlap plot

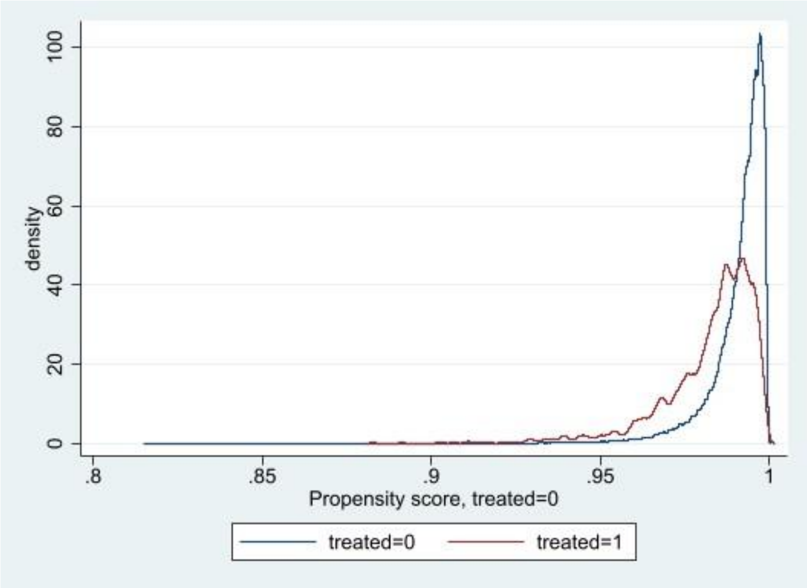

Table A4: Effect heterogeneity by unemployment months and unemployment benefit

Difference in percentage points	Model 1 Months <=12	Model 1 Months >12	Model 2 Months <=12	Model 2 Months >12	Model 1 UB No	Model 1 UB Yes	Model 2 UB No	Model 2 UB Yes
6 months after start-up	18.3***	43.8***	3.72***	20.4***	54.1***	20.5***	25.6***	7.34***
	(3.28)	(2.38)	(3.08)	(2.50)	(2.93)	(2.51)	(3.10)	(2.44)
12 months after start-up	13.75***	28.86***	4.01	12.19***	38.83***	13.46***	21.3***	0.26
	(3.69)	(2.38)	(3.53)	(2.53)	(3.14)	(2.65)	(3.21)	(2.56)
24 months after start-up	7.16**	21.3***	4.01	5.85**	28.05***	4.38	11.2***	-0.61
	(3.57)	(2.59)	(3.53)	(2.73)	(3.41)	(2.70)	(3.39)	(2.75)
Difference in months during								
24 months after start-up	3.17**	7.50***	0.39	1.51***	8.57***	3.29***	4.175***	0.119
	(0.61)	(0.45)	(0.542)	(0.48)	(0.55)	(0.46)	(0.555)	0.428)
24 months after the end of subsidy	2.84***	6.54***	0.074	0.2***	7.65****	2.68***	3.26***	-0.485
	(0.65)	(0.48)	(0.580)	(0.02)	(0.606)	(0.49)	(0.613)	(0.458)
Observations	64002	223650	47110	98136	179165	108487	70540	74706

Notes: Table shows the average treatment effects on the treated. Standard errors are in parentheses and were calculated according to Abadie and Imbens (2009). The statistical significance at the level of 10%, 5%, and 1% is represented by *, ** and ***, respectively.

Table A5: Effect heterogeneity by gender

	Model 1		Model 2	
Difference in percentage points	Men	Women	Men	Women
6 months after start-up	35***	37.7***	18.8***	19.3***
	(2.89)	(2.86)	(2.78)	(2.75)
12 months after start-up	18.57***	28.46***	6.11***	13.2***
	(2.86)	(2.85)	(2.96)	(3.04)
24 months after start-up	12.9***	16.8***	4.96*	8.73***
	(3.03)	(2.90)	(2.76)	(3.04)
Difference in months during				
24 months after start-up	4.52***	6.33***	1.788***	2.095***
	(0.54)	(0.51)	(0.523)	(0.486)
24 months after the end of subsidy	3.92***	5.48***	1.185***	1.24***
	(0.58)	(0.55)	(0.56)	(0.529)
Observations	150408	137244	82207	63039

Notes: Table shows the average treatment effects on the treated. Standard errors are in parentheses and were calculated according to Abadie and Imbens (2009). The statistical significance at the level of 10%, 5%, and 1% is represented by *, ** and ***, respectively.

Table A6: Effect heterogeneity by previous ALMP participation

	Model 1		Model 2	
Difference in percentage points	ALMP Yes	ALMP No	ALMP Yes	ALMP No
6 months after start-up	26.26***	35.09***	17.36***	16.85***
	(5.18)	(2.21)	(4.53)	(2.07)
12 months after start-up	21.35***	25.52***	-0.28	12.54***
	(6.32)	(2.29)	(5.66)	(2.23)
24 months after start-up	16.07***	16.62***	0.42	5.47**
	(6.26)	(2.32)	(5.52)	(2.38)
Difference in months during				
24 months after start-up	2.94***	5.66***	1.634	1.37***
	(1.08)	(0.4)	(2.027)	(0.385)
24 months after the end of subsidy	1.9	4.98***	1.029	0.66
	(1.19)	(0.42)	(2.13)	(0.42)
Observations	18752	268900	9268	135978

Notes: Table shows the average treatment effects on the treated. Standard errors are in parentheses and were calculated according to Abadie and Imbens (2009). The statistical significance at the level of 10%, 5%, and 1% is represented by *, ** and ***, respectively.

Table A7: Robustness results of Model I

	Model 1		
	Kernel-propensity-score matching	Kernel-propensity score matching with exact matching	Multivariate distance-matching with exact matching
Difference in percentage points			
6 months after start-up	35***	35.9***	39.63***
	(1.48)	(1.49)	(1.33)
12 months after start-up	24.43***	25.33***	27.94***
	(1.68)	(1.68)	(1.54)
24 months after start-up	16.04***	16.09***	19.97***
	(1.75)	(1.76)	(1.61)
Difference in month during			
24 months after start-up	5.58***	5.64***	6.41***
	(0.276)	(0.278)	(0.249)
24 months after the end of subsidy	4.74***	4.88***	5.685***
	(0.32)	(0.321)	(0.293)
Observations		287652	

Notes: Table shows the average treatment effects on the treated. Standard errors are in parentheses and were calculated according to Abadie and Imbens (2009). The statistical significance at the level of 10%, 5%, and 1% is represented by *, ** and ***, respectively

Table A8: Robustness results of Model II

	Model 1I		
	Kernel-propensity-score matching	Kernel-propensity score matching with exact matching	Multivariate distance-matching with exact matching
Difference in percentage points			
6 months after start-up	16.9***	17.45***	17.4***
	(1.44)	(1.45)	(1.31)
12 months after start-up	11.22***	10.24***	10.55***
	(1.66)	(1.67)	(1.53)
24 months after start-up	6.23***	5.22***	6.31***
	(1.74)	(1.75)	(1.61)
Difference in month during			
24 months after start-up	1..71***	1.66***	1.76***
	(0.268)	(0.268)	(0.244)
24 months after the end of subsidy	0..94***	0.9***	1.022***
	(0.313)	(0.312)	(0.289)
Observations		145246	

Notes: Table shows the average treatment effects on the treated. Standard errors are in parentheses and were calculated according to Abadie and Imbens (2009). The statistical significance at the level of 10%, 5%, and 1% is represented by *, ** and ***, respectively

Table A9: Sensitivity to unobserved heterogeneity

Outcome	Model I			Model II		
	Gamma	sig	hat	Gamma	sig+	hat+
12 months	1	0.000	11.703	1	0.000023	4.07604
	1.3	0.000	8.90756	1.1	0.000979	3.09668
	1.5	0.000	7.39876	1.2	0.013775	2.20364
	1.7	0.000	6.08743	1.3	0.083279	1.38335
	1.9	0.000	4.92752			
	2	0.000	4.39404			
24 months after start-up	1	0.000	5.5	1	0.000078	1.5
	1.3	0.000	4	1.1	0.005779	1
	1.5	0.000	3	1.2	0.083411	0.5
	1.7	0.000	2.5	1.3	0.369374	0
	2	0.000	1			
24 months after the end of subsidy	1	0.000	4.5	1	0.01243	1
	1.3	0.000	3	1.1	0.160594	0.49999
	1.5	0.000	2			
	1.7	0.000	1.5			
	2	0.018	0.5			