Long-Term Effects of Start-Up Subsidies for the Unemployed: A Sensitivity Analysis

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Abstract

Turning unemployment into self-employment has become an increasingly important part of active labor market policies (ALMP) in many OECD countries. Germany is a good example where the spending on start-up aids for the unemployed accounted for nearly 17% of the total spending on ALMP in 2004. In contrast to other policies—like vocational training, job creation schemes, or wage subsidies—the empirical evidence on the effectiveness of such programs is scarce; especially regarding long-term effects. This paper aims to close this gap. We use survey data from a large sample of participants in two distinct start-up programs (and a control group of unemployed who did not enter these programs). Based on conditional propensity score matching methods we estimate the long-term effects of the programs against nonparticipation. Our results show that at the end of the observation period, both programs are effective with respect to different outcome variables. Additionally, we conduct extensive sensitivity checks and also consider effect heterogeneity. We finally conclude that programs aimed at turning the unemployed into entrepreneurs may be a promising active labor market policy, both in Germany and elsewhere.

Keywords: Start-Up Subsidies, Evaluation, Long-Term Effects, Self-Employment
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1 Introduction

The financial promotion of self-employment proposals has long been part of Germany's ALMP. Already in 1986 the *Bridging Allowance* (BA, "Überbrückungsgeld") was introduced providing financial support for unemployed workers, i.e., basically unemployment benefits are paid during the first six months, to start up their own business. As part of the fundamental labor market reforms in Germany which initially started in 2003 ("Hartz-Reform") the authorities extended the supply of self-employment schemes by launching an additional program in 2003, referred to as *Start-up Subsidy* (SUS, "Existenzgründungszuschuss"). This program consisted of monthly payments of €600 in the first year, €360 in the second and finally €240 in the third year. Furthermore, participants in SUS were restricted to a maximum income of €25,000 per year. Therefore, these schemes differentiate sharply in terms of the amount of financial support and duration; consequently either program attracted different individuals.

When approaching the question why it might be a good idea to turn unemployment into self-employment as part of ALMP, we have to consider the objectives of start-up programs. Providing financial support to unemployed individuals in order to become self-employed is generally associated with a double dividend. The supported individuals become self-employed and therefore exit their own unemployment immediately and, in addition, may potentially create further jobs within newly founded businesses and thus reduce unemployment in the economy even further. Another appealing feature of start-up programs is definitely avoiding low-quality, low-paid employment by giving unemployed individuals the opportunity to become self-employed. This, in turn, puts the unemployed in a position that they do not have to accept every job offer. Moreover, the majority of programs of ALMP aims to increase human capital and therefore increase individuals probability to find a job. This is not the main objective of start-up schemes but it is argued that during the period of self-employment individuals also increase their employability and human capital indirectly which makes those individuals in case of failure more attractive to potential employers compared to individuals who remained unemployed. It is also likely that individuals meet potential employers due to business activities during their self-employment. Beside all advantages of start-up programs on the individual level, there are also some important macroeconomic reasons for the authorities to support self-employment among the unemployed. Entries of new firms actually encourage competition and consequently productivity (*survival of the fittest*); it also generates new products and innovations. Potentially, this promotes efficient markets and technology diffusion and will therefore lead finally to economic stability and economic growth, i.e., increase wealth. (see Fritsch, 2008; Storey, 1994).

However, there are not only promising features related to the financial promotion of start-ups by the unemployed. First of all, it may be the case that supported individuals may have become self-employed even in the absence of financial support. This is referred to as deadweight effect and the scope of that loss is difficult to determine.[1] Another concern addresses crowding out effects. Incumbent firms or non-subsidized firms are likely to be displaced by start-ups with financial support. Finally, firms may also substitute employees by subsidized self-employed workers. Due to a highly regulated labor market in Germany, however, such substitution effects are unlikely to occur in practice and can therefore be neglected.[2]

The discussion of pros and cons of start-up schemes that provide direct financial support generated some alternative suggestions. For instance, one possible alternative might be to simplify access to credits at low interest rates for the unemployed. Due to asymmetric information, the unemployed are likely to face credit constraints (no collateral etc) which may hinder them to start their own business or increases capital costs remarkably. However, we argue that the provision of capital at low interests rates does not generate the same number of start-ups and are not as effective as programs such as BA and SUS, this is due to two reasons. First, unemployed individuals face hardly any credit constraints in Germany (see Kritikos, Kneiding, and Germelmann, 2006). Additionally, Caliendo and Kritikos (2007b) show in an ex post analysis that around 50% (37%) of initial SUS (BA) supported individuals had no need for start-up capital. Therefore, we argue that credit constraints are not the main hurdle for the unemployed on their way to become self-employed. Second, providing only simplified access to loans would attract more risky individuals and would systematically rule out individuals with low or medium risk attitudes. However, Caliendo, Fossen, and Kritikos (2008) show that especially individuals in the medium range of the risk distribution are more successful in terms of survival of their business. Therefore, programs which are rather attractive for individuals with high risk attitudes may potentially rule out entrepreneurial resources to some extend since individuals with low/medium risk attitudes are likely to become not self-employed without some kind of

[1]Meager (1993) provides an estimate of the deadweight effect of the Bridging Allowance for Germany and concludes that the effect is rather small (about 10%).

[2]Dietrich (1996) states that about 3.9% of the working population is dependent self-employed (full or part-time) in Germany. For the UK, Böheim and Mühlberger (2009) find 1.5% in dependent self-employment. These figures clearly suggest that substitution effects with respect to subsidized self-employment obviously play only a minor role.

risk compensation, e.g., in form of direct financial support.

Despite this ongoing debate on turning unemployment into self-employment, the existing evaluation literature on such schemes is rather scarce and does only provide evidence for short- to medium-term periods. This is definitely a shortcoming because in most studies the period of financial support overlaps the observation period. However, in order to reliably assess the effectiveness of such programs an evaluation beyond the period of financial support is required. Our study aims at closing this research gap and provides long-term evidence for *Bridging Allowance* and *Start-up Subsidy*. Moreover, we consider effect heterogeneity.

The paper is organized as follows. Section 2 provides information on the institutional background of start-up programs in Germany and briefly quantifies the meaning of such schemes in comparison to other programs of ALMP. Afterwards, in Section 3 we give an review on the existing literature in that field. The main part of our paper contains Section 4 which includes identification and estimation strategy, a description of the dataset, main results as well as effect heterogeneity. In Section 5 we test our results with respect to unobserved heterogeneity before we finally conclude in Section 6.

2 Self-employment subsidies as part of ALMP in Germany

Initially introduced in 1986 the *Bridging Allowance* was the only program providing support to unemployed individuals who wanted to start their own business until 2003. Its main goal was to cover basic costs of living and social security contributions during the initial stage of self-employment. More specifically, during the first six months the recipient of BA receives the same amount the he or she would have received if he or she had remained unemployed. Since the unemployment scheme also covers social security contributions (including health insurance, retirement insurance, etc.) a lump sum for social security is granted, equal to 68.5% of the unemployment support that would have been received in 2003. Unemployed individuals are entitled to BA conditional on their business plan being approved externally, usually by the regional chamber of commerce. Thus, approval of an individual's application does not depend on the case manager at the local labor office.

In January 2003, an additional program was introduced to support unemployed people in starting a new business. This *Start-up Subsidy* was introduced as part of a large package of ALMP programs introduced through the 'Hartz reforms'.[3] As it is the case to the BA, the main goal of SUS is to secure the initial phase of self-employment. It focuses on the provision of social security to the newly self-employed person. The support comprises of a lump sum payment of €600/month in the first year. A growth barrier is implemented in SUS such that the support is only granted if income does not exceed €25,000 per year. The support shrinks to €360/month in the second and to €240/month in the third year. In contrast to the BA, SUS recipients are obligated to pay into the statutory pension insurance fund and may claim a reduced rate for statutory health insurance (see Koch and Wießner, 2003). When the SUS was introduced in 2003, applicants did not have to submit business plans for prior approval, but have been required to do so since November 2004, as it was already the case with the BA. Moreover, parallel receipt of BA and SUS is excluded. See Table 1 for more details on both programs.

Table 1: Terms and conditions of both programs

	Start-up Subsidy	**Bridging Allowance**
Entry conditions:	-Unemployment benefit *receipt*. -Income is restricted to €25,000 per year. -Approval of a business plan was subsequently introduced in November 2004. -Below 65 years of age.	-Unemployment benefit *entitlement*. -No income restrictions. -Approval of the business plan. -Below 65 years of age.
Support:	-Participants receive a fixed sum of €600 in the first year, €360 (€240) in the second (third) year. -Claim has to be renewed every year.	-Participants receive UB for six months. -To cover social security liabilities, an additional lump sum of 68.5% is granted.
Other:	-Participants have to become a member in the legal pension insurance and take advantage of a reduced rate in the legal health insurance.	-Social security is left at individuals discretion.

Due to the institutional framework, it is rather rational to choose BA if unemployment benefits are fairly high and/or if the income generated through the start-up firm is expected to exceed €25,000. Both programs were replaced in August 2006 by a single new program—the new start-up subsidy program (*Gründungszuschuss*)—which will not be analyzed here.[4]

Additionally, becoming self-employed in Germany is at least in some occupational groups highly restrictive. For instance, beside typical self-employed occupations such as physicians, lawyer or tax consultants, in Germany for several handcraft occupations it

[3]See Caliendo and Steiner (2005) for an overview of the most relevant elements of the 'Hartz reforms'.

[4]See Caliendo and Kritikos (2007a) for information and a critical discussion of the features of the new program.

is also required to occupy an advanced certificate in order to be allowed to become self-employed, e.g., master craftsman or an engineer. In comparison to other countries this is highly restrictive and definitely will hinder many unemployed individuals to found their own business. However, Cressy (1996) argues that such preconditions for entry into self-employment tend to significantly enhance survival of businesses. In addition to entry restrictions, the German system prevents entrepreneurs from becoming a CEO again if a business went bankrupt once.

Anyway, Table 2 contains number of entries into start-up programs as well as other programs of ALMP in West Germany. While in 2003 and 2004 entries into start-up programs increased remarkably beyond 2005 number of entries declined again. The theory actually provides two main explanations why individuals choose to become self-employed with respect to economic conditions. For instance, during periods of economic growth individuals are *pulled* into self-employment due to good business opportunities compared to regular employment. In contrast, in periods of economic recession unemployment is usually high and therefore individuals start self-employment due to limited opportunities in the regular labor market, i.e., unemployed are *pushed* into self-employment. For the case of unemployed individuals one would expect that they become self-employed mainly according to the latter point, that is worse labor market perspectives forces them to start their own business in order to escape unemployment. Based on these explanations, figures in Table 2 indicate that the majority of unemployed individuals rather start their own business due to *necessity*, that is the unemployed are *pushed* into self-employment. Due to substantial labor market reforms unemployment rates[5] declined in West Germany beyond 2005 and consequently the unemployed faced more job opportunities in the regular labor market. Consequently entries into start-up programs decreased after 2004. Therefore, unemployed individuals generally seem to favor dependent employment and consider self-employment rather as an opportunity to avoid unemployment in periods of limited job offers. In addition, we also asked participants in SUS and BA for their motives to become self-employed (allowing for multiple options). It turned out that about 80% became self-employed in order to exit unemployment, while roughly 45% wanted to be their own boss and only 30% report that they found a market niche. Therefore, the majority of the start-ups out of unemployment are obviously *pushed* into self-employment in order to escape unemployment. In contrast, Caliendo and Kritikos (2007b) show that basically both explanations apply to the unemployed, the *necessity* as well as the *opportunity* argument.

[5]For West Germany the Federal Statistical Office reported an unemployment rate of 8.0% for 2001. Subsequently it increased sharply to 11.0% in 2005 before it declined again to 8.4% in 2007.

Table 2: Entries into programs of ALMP in West Germany (in thousand)

	BA	SUS	New SUS	VT	TM	WS
2000	92.6	–	–	337.9	285.9	120.4
2001	64.5	–	–	261.2	338.5	101.0
2002	89.0	–	–	273.2	545.4	114.4
2003	114.4	64.2	–	154.0	694.3	96.5
2004	137.3	113.8	–	124.0	788.5	93.9
2005	120.0	57.3	–	91.1	607.2	86.0
2006	83.6	27.0	25.4	173.0	671.1	152.1
2007	–	–	96.5	246.2	719.1	160.7

Source: Bundesagentur für Arbeit (various issues).

In Table 2 it is also noticeable that start-up programs are throughout comparable in terms of number of entries to other programs of ALMP, such as *Wage Subsidies* (WS) or *Vocational Training* (VT). On the other hand, entries into *Training Measures* (TM) are more than three times as much but, of course, one has to keep in mind that TM are rather short-term, i.e., with a maximum duration of three months and an average duration of two weeks. Accordingly, entrance requirements are much lower. Moreover, as we can see the scope of the *New Start-up Subsidy* (New SUS) is below the cumulated number of entries in BA and SUS. Finally, we conclude that the meaning of start-up programs increased in recent years but also seems to depend in particular on the overall economic conditions.

3 Literature review

For Germany, Baumgartner and Caliendo (2008) provide an evaluation of both programs for the short- and medium-run. They find strong positive employment and income effects for participants in both programs compared to a group of non-participants, i.e., other unemployed individuals who do not enter these programs. However, the authors underscore the preliminary character of their results, as the majority of start-up subsidy participants still received financial support during the observation period. Therefore, the survival rate is likely to further decrease after financial support expires totally. Furthermore, Wießner (1998) provide descriptive evidence for entries into Bridging Allowance in 1994/95. He finds on average 70.4% self-employed after about three years since start-up. In an earlier study Pfeiffer and Reize (2000) analyze the effect of Bridging Allowance on survival rates in self-employment during the first year after entry. They do neither find any differences in survival probability nor in employment growth between supported and non-subsidized firms in West Germany[6].

To the best of our knowledge, in an international context the presence of empirical evidence on start-up schemes is rather scarce. Due to economic conditions and therefore potential effect heterogeneity with respect to direction as well as amount of impact, hence we consider results for developed and developing/transition countries separately. First of all, let us discuss studies on start-up programs for the unemployed in the context of developing countries. For Argentina, Almeida and Galasso (2008) investigate the impact of financial and technical assistance for welfare beneficiaries on their way to self-employment. To be precise, individuals receive payments for inputs or equipment as well as assistance by local institutions (e.g. universities). The authors observe a period of 12 months in 2004/2005 and use difference-in-differences method. They find an increase in total working hours but no significant income effects due to the program. However, for young and high educated individuals they are able to identify positive income effects. The authors also mention that the results are only valid in the short-run. Moreover, Rodriguez-Planas (2008) investigates a start-up program for Romania. Here, the participants obtained professional assistance through counseling or short-term entrepreneurial training. In addition, working capital loans are offered. Using propensity score matching method she finds positive employment effects but no income gains for participants compared to nonparticipants. For a subgroup of low educated individuals she reveals strong positive employment effects, in particular

[6] In fact, for East Germany they find a lower survival probability for supported firms.

because of comparative bad labor market performance among nonparticipants. As a further study, O'Leary (1999) considers self-employment schemes for Poland and Hungary. In Poland the scheme provides loans at market rates of interests to the unemployed combined with the attractive option that 50% of repayments will be waived if firms survive at least for two years. In contrast, the Hungarian program consists of a subsidy in the amount of the unemployment benefit level paid for up to 18 months. In addition, it also captures half of the costs for training and counseling. O'Leary (1999) observes a period of 21 months since start-up and finds large and positive employment effects for both countries using regression models. While he is also able to identify strong positive earning effects for Hungary, in Poland the income effect is negative[7]. Among participants O'Leary (1999) finds high survival rates in self-employment and additional employment effects, i.e., 81% (62%) are self-employed after 21 (50) months since start-up and 0.31 (0.83) additional workers are hired by subsidized firms in Hungary (Poland).

After having presented empirical evidence for developing/transition countries we now have a closer look to developed countries. For the case of Sweden, Carling and Gustafson (1999) provides a comparative study between employment subsidies and self-employment grants for the unemployed. Using duration analysis they find that individuals in subsidized employment have a higher probability of re-entering unemployment than recipients of self-employment grants. Therefore, they conclude that self-employment grants are more effective in avoiding unemployment. Cueto and Mato (2006) analyze the success of self-employment subsidies for a particular districts in Spain using duration analysis. Beside being interested in the determinants of survival (duration) in self-employment they also estimate a competing risk model to distinguish between business failures and other reasons why businesses were closed. Based on data for individuals who received the subsidy between 1996 and 2000, survival rates for 2-5 years can be observed and the survival is approximately 93% after two and 76% after five years. With respect to failure of businesses Cueto and Mato (2006) state that about 53% exit self-employment voluntarily, mainly due to job opportunities in the regular labor market or too little income from self-employment. For New Zealand, Perry (2006) evaluates Enterprise Allowance grants—an integrated programme that provides business skills training as well as financial aid— between 1993 and 1995 using conditional difference-in-differences. The author's results (measured up to two years after participation) indicate statistically significant beneficial effects for the participants, where the outcome variable is 'not registered unemployed'. Meager, Bates, and

[7]The negative earning effect in case of Poland O'Leary (1999) primarily attributes to firms reluctance for full disclosure to the tax authorities.

Cowling (2003) evaluate business start-up subsidies by Prince Trust to young people in the UK. They are not only interested in certain characteristics and survival of start-ups but also compare labor market outcomes between participants and a comparison group (similar in terms of age, gender, region and previous employment status). Based on multinomial and standard logistic regressions the authors conclude that participating in the programme does not have any significant impact on subsequent employment or earning chances. Nonetheless, descriptively they find a fraction of 69.1% in self-employment among participants after 18 months. Kelly, Lewis, Mulvey, and Dalzell (2002) consider a program consisting of an allowance paid up to 52 weeks as well as training and counseling in Australia. The authors find a survival rate of 56.2% in self-employment after three years since start-up. An earlier summary of findings for Denmark, France, West Germany, UK and USA is provided by Meager (1996). He states that the evidence does not allow a conclusive assessment of the overall effectiveness of such schemes.

To summarize, the existing literature on start-up schemes for the unemployed mostly reveals positive or insignificant effects with respect to labor market outcomes. Negative impacts are rather scarce which in turn suggests an overall effectiveness of such schemes. However, the evidence varies with respect to countries, institutional design of the support and entrance conditions. However, we would like to highlight that the existing studies mainly possess rather short- to medium-run observation periods what in turn possibly biases the results upwards because of initial locking-in effects due to financial support or assistance. Therefore, the demand for long-run evidence on start-up schemes for the unemployed is throughout highlighted. This is the main contribution of our study.

4 Empirical Analysis

In this section we will estimate causal effect after having presented our data and discussed the identification strategy as well as the implementation of the underlying estimation procedure.

4.1 Data

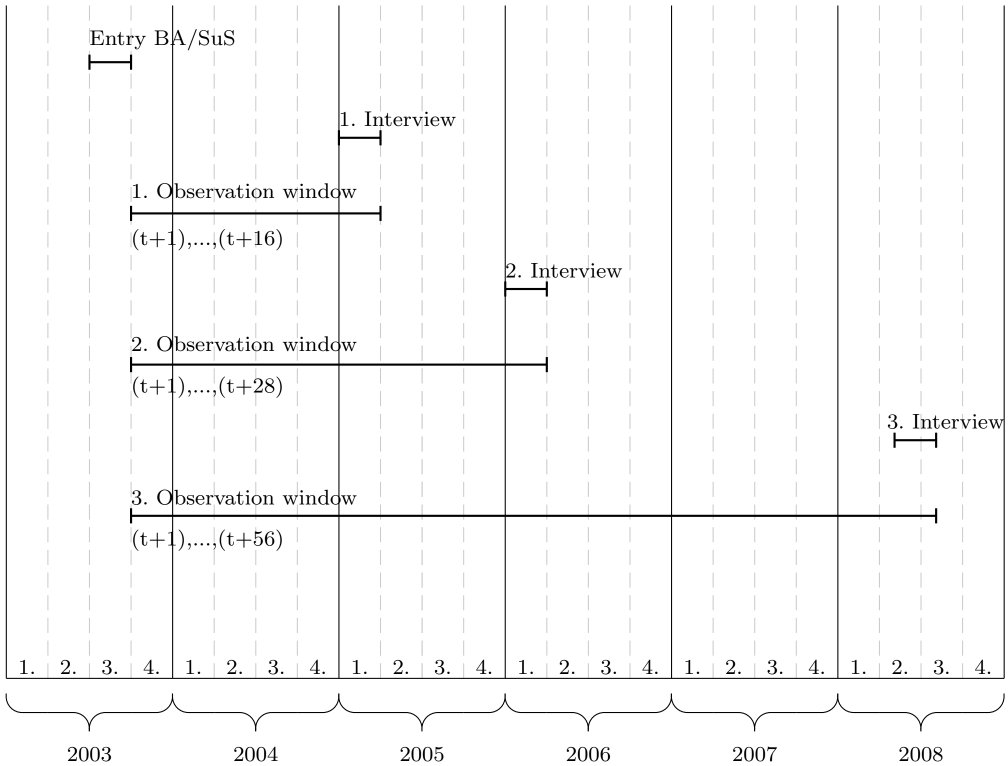
We use an unique data set which combines administrative data from the 'Federal Employment Agency' (FEA) with survey data. For the administrative part we use data based on the 'Integrated Labour Market Biographies' (ILMB) of the FEA, containing relevant register data from four sources: employment history, unemployment support recipience, participation in active labor market measures, and job seeker history. Since the administrative data are only available with a certain time lag and more importantly do not provide any information on the employment status and/or income of self-employed individuals, we enriched the ILMB data with information from a computer-assisted telephone interview. To do so, we randomly drew participants from each programme who became self-employed in the third quarter of 2003. Since we wanted to compare them with nonparticipants, we had to choose a comparison group. Choosing such a group is a heavily discussed topic in the recent evaluation literature. Although participation in ALMP programmes is not mandatory in Germany, the majority of unemployed persons participate at some point in time since the FEA can at least partly force them to participate by threatening to cut unemployment benefits by a certain amount otherwise. Thus, comparing participants to individuals who never participate is inadequate, since it can be assumed that the latter group is particularly selective.[8] Sianesi (2004) discusses this problem for Sweden and argues that those who never participate did not enter a programme because they had already found a job. Additionally, since we did not know the future employment/participation status of the comparison group before the interviews took place, we restricted this comparison group to those who were unemployed in the third quarter of 2003, eligible for participation in either of the two programmes, but did not join a programme in this quarter. What should be kept in mind is that these comparison group members might participate in some ALMP programme after this quarter.[9]

[8] Furthermore, it should be noted that using individuals who are observed to never participate in the programmes as the comparison group may invalidate the conditional independence assumption due to conditioning on future outcomes (see discussion in Fredriksson and Johansson, 2008).

[9] The actual number of nonparticipants who participated in any ALMP programme after this quarter is rather low. Approximately 15% of nonparticipants were assigned to programs of ALMP and only 2% participated in SUS or BA within our observation period.

To minimize the survey costs we used a crude propensity score matching approach to preselect[10] somewhat similar unemployed individuals who were subsequently interviewed three times. The first interview took place in January/February 2005, the second in January/February 2006 and finally the third in May/June 2008[11]. This enables us to observe the labor market activity of individuals for at least 56 months after start-up. However, the survey consists of both longitudinal as well as cross-section data and Figure 1 illustrates the survey design graphically.

Figure 1: Survey design



Remark: Detailed information with respect to the structure of the first two interviews are provided by Caliendo, Steiner, and Baumgartner (2005).

We restrict our sample to males in West Germany since we are interested in the success of programs which support unemployed individuals to become self-employed and therefore males are generally more likely to become full-time self-employed than females. Furthermore, in West Germany individuals face better labor market conditions compared

[10]The potential comparison group consisted of roughly 640,000 individuals. Control individuals (for the interview) were chosen to resemble the distribution of some key variables—including gender, region, age, previous unemployment duration, qualification, and nationality—in the population of the treated individuals. To do so, we estimated a 'crude propensity score' based on these variables and chose for every participant nonparticipants with a similar propensity score as interviewees. See Forschungsverbund IAB, DIW, SINUS, GfA, infas (2005) for a detailed description of the sample construction.

[11]A minor part of the interviews (4%) took also place in July 2008.

to East Germany. Consequently, by restricting the sample to males in West Germany we avoid several side-effects, such as labor supply decisions, macroeconomic constraints etc.

Table 3 provides the number of realized interviews in the respective waves. In case of SUS, for instance, we initially started with 1,116 individuals who became self-employed in III/2003. Only 811 responded to the second interview and finally we end up with 486 after the last interview in 2008. Therefore, our final sample consists of 486 SUS participants, 780 BA recipients and 929 nonparticipants. First of all, we provide descriptive statistics measured at entry into program, i.e., 3rd quarter 2003, separated for participants (SUS and BA) and nonparticipants in Table A.1 in the Appendix. Since we used a crude matching approach to make individuals similar, the sample of nonparticipants does not represent a random sample of unemployed individuals. Clearly, this does not affect our estimation and interpretation strategy but should be kept in mind when interpreting the differences. However, let us briefly discuss the most important variables for the participants. First of all, it turns out that both programs basically differ in terms of the observable characteristics of the participants. In fact, SUS attracts rather younger and lower educated individuals with less employment duration and lower earnings in the past. This is exactly what we would expect because the financial support in case of BA depends on previous earnings and is only paid for a rather short-term period of six months. Hence, individuals with low earnings in the past are only eligible to a minor support if they would choose BA and it is rather rational for those to choose SUS because it is comparable low (€600 in the first year) but the subsidy can be extended up to three years. On the other side, individuals with higher earnings want to secure their high entitlement and, consequently, choose BA. Moreover, participants seem to be locally equally distributed throughout West Germany. A very interesting issue, however, is that self-employment seems to be influenced by intergenerational transmission, i.e., the fraction with parental self-employment among participants is higher than among nonparticipants.

Moreover, we also provide in Table 3 the labor market status of participants and nonparticipants 28 and 56 months after start-up.[12] First of all, let us take a closer look at participants. It is visible that the fraction of self-employed individuals decreases from 71.5% to 67.9% in case of former BA recipients and from 67.9% to 59.7% for firms initially

[12] As we can see in Table 3 the number of realized interviews decreased partly dramatically. On average, we are only able to observe 45% of all participants and 37% of nonparticipants for the whole period of 56 months. We checked our results with respect to potential selection process due to panel attrition and find positive selection, i.e., individuals who perform relatively well in terms of labor market outcomes are more likely to respond. We apply *Sequential Inverse Probability Weighting* to adjust for the selection process due to panel attrition (see Wooldridge, 2002).

Table 3: Realized interviews and labor market status

	SUS	BA	NP
Number of realized Interviews			
January/February 2005	1,116	1,665	2,530
January/February 2006	811	1,207	1,448
May/June 2008	486	780	929
Labor market status at the 2nd Interview (January/February 2006)			
Self-employed	67.6	71.5	12.7
Regularly employed	11.7	14.0	35.9
Unemployed or in ALMP	15.2	11.1	35.9
Others	5.6	3.4	15.5
Labor market status at the 3rd Interview (May/June 2008)			
Self-employed	59.7	67.9	14.1
Regularly employed	20.9	21.1	49.1
Unemployed or in ALMP	11.7	6.7	19.9
Others	7.6	4.3	16.9

Note: All results are percentages if not indicated differently.

supported by SUS. Therefore, the decline in self-employment is more than twice as high as for SUS (-8.2%) than for BA (-3.6%). The fact that the subsidy expired entirely in-between the 2nd and 3rd interview mainly induces this remarkable drop in the case of SUS and, of course, indicates that the financial support was at least for some businesses necessary to survive. But on the other side, BA recipients experienced the loss of the subsidy already after six months since start-up and hence the decline in the survival rate from the 2nd to 3rd interview is much lower.

However, if we are talking about ALMP we are usually more interested in the degree of integration into the labor market, i.e., the fraction who is either self-employed or regularly employed. Here, 56 months after start-up we end up with about 81% of SUS and 89% of former BA participants well integrated in the first labor market. For nonparticipants only 63% are either self-employed or regularly employed.

Table 4: Income 56 months after start-up

	SUS	BA	NP
Total income	1,672.0	2,336.0	1,581.1
	(1,720.4)	(1,962.9)	(1,601.6)
	[1,276.3]	[1,942.3]	[1,338.0]
Working income	1,498.5	2,167.4	1,302.8
	(1,780.2)	(2,006.3)	(1,662.5)
	[1,145.3]	[1,815.2]	[1,190.1]

Note: Depicted are the average monthly net incomes in Euro; standard deviation and median are provided in parentheses and square brackets respectively.

In addition to the labor market status we also provide in Table 4 net income measures, again, separated for both programs and also nonparticipants. Thereby, income is measured 56 months after start-up and total income captures beside the working income also transfer payments such as unemployment benefits, pensions or child benefit. We can see that former BA recipients have a higher income in terms of both working and total income compared to SUS participants. This is actually not surprising because highly educated individuals with high earnings in the past were more likely to choose BA (see Table A.1). Moreover, it is visible that nonparticipants earn on average less than participants but considering the median of the income distributions the difference to SUS participants almost vanishes.

Finally, we want to emphasize that these results are only descriptively and hence the difference between participants and nonparticipants can not be interpreted as causal. We will provide causal effects for both programs, i.e., compared to nonparticipation, in Section 4.4 after having discussed identification and estimation issues.

4.2 Identification of causal effects

We base our analysis on the potential outcome framework, also known as the Roy(1951)-Rubin(1974) model. The two potential outcomes are Y^1 (individual receives treatment, $D = 1$) and Y^0 (individual does not receive treatment, $D = 0$). The actually observed outcome for any individual i can be written as: $Y_i = Y_i^1 \cdot D_i + (1 - D_i) \cdot Y_i^0$. The treatment effect for each individual i is then defined as the difference between her potential outcomes: $\tau_i = Y_i^1 - Y_i^0$. Since we can never observe both potential outcomes for the same individual at the same time, the fundamental evaluation problem arises. We will focus on the most prominent evaluation parameter, which is the average treatment effect on the treated (ATT), and is given by:

$$\tau_{ATT} = E(Y^1 \mid D = 1) - E(Y^0 \mid D = 1). \tag{1}$$

Given equation (1), the problem of selection bias can be straightforwardly seen since the second term on the right hand side is unobservable. It describes the hypothetical outcome without treatment for those individuals who received treatment. Since with non-experimental data the condition $E(Y^0 \mid D = 1) = E(Y^0 \mid D = 0)$ is usually not satisfied, estimating ATT by the difference in sub-population means of participants $E(Y^1 \mid D = 1)$ and non-participants $E(Y^0 \mid D = 0)$ will lead to a selection bias. This bias arises because participants and non-participants are selected groups that would have different outcomes, even in absence of the programme and might be caused by observable or unobservable factors.[13] We apply propensity score matching and thus rely on the conditional indepen-

[13]See, e.g., Caliendo and Hujer (2006) for further discussion.

dence assumption (CIA), which states that conditional on observable characteristics (W) the counterfactual outcome is independent of treatment ($Y^0 \amalg D|W$).[14] This assumption identifies the average treatment effect on the treated. That is:

Assumption 1 *Unconfoundedness for Comparison Group:* $Y^0 \amalg D|W$,

where $\amalg$ denotes independence. Clearly, the CIA is a very strong assumption and the applicability of the matching estimator depends crucially on its plausibility. Blundell, Dearden, and Sianesi (2005) argue that the plausibility of such an assumption should always be discussed on a case-by-case basis. Only variables that influence the participation decision and the outcome variable simultaneously should be included in the matching procedure. Hence, economic theory, a sound knowledge of previous research, and information about the institutional setting should guide the researcher in specifying the model (see, e.g., Smith and Todd, 2005 or Sianesi, 2004).

Both economic theory and previous empirical evaluation studies highlight the importance of socio-demographic and qualificational variables. Regarding the first category we can use variables such as age, marital status, number of children, nationality (German or foreigner), and health restrictions. Additionally, we also use information whether individuals want to work full-time or part-time, and hence we might be able to approximate the labor market flexibility of these individuals. A second class of variables (qualification variables) refers to the human capital of the individual, which is also a crucially important determinant of labor market prospects. The attributes available are school degree, job qualification, and work experience. Furthermore, previous evaluation studies also point out that unemployment dynamics and labor market history play a major role in driving outcomes and programme participation. Hence, we use career variables describing the individual's labor market history. The available data in this regard is quite extensive (inter alia: nearly complete seven-year labor market history; daily earnings from employment; amount of daily unemployment benefits; duration of last unemployment spell, employment status before unemployment, previous profession, etc.). Heckman, Ichimura, Smith, and Todd (1998) also emphasize the importance of drawing treatment and comparison groups from the same local labor market and giving them the same questionnaire, where the latter is ensured in our data. To account for the situation on the local labor market, we use a classification of similar and comparable labor office districts derived by the FEA (see Blien, Hirschenauer, Arendt, Braun, Gunst, Kilcioglu, Kleinschmidt, Musati, Roß, Vollkommer, and Wein, 2004, for details). The institutional structure and the selection

[14] See Imbens (2004) or Smith and Todd (2005) for recent overviews regarding matching methods.

process into programmes provide further guidance in selecting the relevant variables. As we have seen from the discussion in Section 4.1, the two programmes differ among other things in the size of the subsidy. Whereas the SUS is a lump sum, the BA depends on the amount of the unemployment benefits. Hence, we include the daily unemployment transfer payment before the start of the programme as an explanatory variable. In contrast to many other studies we are also able to include the remaining duration of unemployment benefits, which probably plays a determining role in these individuals' decision.[15] Finally, one major component influencing the decision to become self-employed is referred to as intergenerational transmission, that is whether parents have been self-employed or not (see Georgellis, Sessions, and Tsisianis, 2005). It is also argued that this variable is also likely to capture some unobserved characteristics, i.e., better "entrepreneurial genes".

Based on this exhaustive data, we argue that the CIA holds in our application. The set of variables is extensive and covers nearly all variables which have been identified to be important in previous evaluation studies of labor market policies. However, we test the sensitivity of the results with respect to remaining bias in observed variables (after matching) and time-invariant unobserved differences between participants and nonparticipants.

In addition to the CIA, it has to be assumed that:

Assumption 2 *Weak Overlap:* $Pr(D = 1 \mid W) < 1$,

for all W. This implies that there is a positive probability for all W of not participating, i.e., that there are no perfect predictors which determine participation. These assumptions are sufficient for identification of the ATT, which can be written as:

$$\tau_{ATT}^{MAT} = E(Y^1|W, D = 1) - E_W[E(Y^0|W, D = 0)|D = 1], \tag{2}$$

where the first term can be estimated from the treatment group and the second term from the mean outcomes of the matched comparison group. The outer expectation is taken over the distribution of W in the treatment group.

As matching on W can become hazardous when W is of high dimension ('curse of dimensionality'), Rosenbaum and Rubin (1983) suggest the use of balancing scores $b(W)$. These are functions of the relevant observed covariates W such that the conditional distribution of W given $b(W)$ is independent of the assignment to treatment, that is, $W \amalg D|b(W)$. The propensity score $P(W)$, i.e., the probability of participating in a programme, is one possible balancing score. For participants and nonparticipants with the

[15] Lechner and Wunsch (2006) evaluate the effectiveness of ALMP (excluding start-up subsidies) in East Germany using a very similar set of variables.

same balancing score, the distributions of the covariates W are the same, i.e., they are balanced across the groups. Hence, assumption 1 can be re-written as $Y^0 \amalg D|P(W)$ and the new overlap condition is given by $Pr(D = 1 \mid P(W)) < 1$.

Moreover, for identification of causal effects it is required to rule out general equilibrium effects, i.e., treatment participation of one individual has potentially an impact on outcomes of other individuals. This assumption is referred to as *Stable-Unit-Treatment-Value-Assumption* (SUTVA). Imbens and Wooldridge (2008) argue that the validity of such an assumption depends on the scope of the program as well as resulting effects. They infer that for the majority of labor market programs the SUTVA is potentially fulfilled because such programs are usually of small scope with rather limited effects on the individual level. However, due to relatively many entries into SUS and BA (see Table 2) we have to deal with this issue more seriously in our study. First of all, we check the correlation between entry density and overall labor market conditions.[16] We find a positive correlation between both measures. However, it is unclear whether either entries into subsidized self-employment have an positive impact on the overall local labor market conditions or more attractive regions encourage simply more entries into self-employment. In a second step, we detect that programs are more effective in areas with relatively low entry density. However, at the same time areas with low entry density have a higher probability to be characterized by bad local labor market conditions. Therefore, we argue the SUTVA is fulfilled in our case since entries into both programs do not have an impact on the outcomes of other individuals but rather overall local labor market conditions influence outcomes. We will come back to that point in Section 4.5.

4.3 Estimation procedure

After having discussed identification issues we proceed with the estimation of causal effects. As mentioned already we apply propensity score matching method and therefore we start with estimating the propensity scores for participation in the respective programs versus nonparticipation using *probit*-estimation. We test different specifications following economic theory and previous empirical findings as discussed above. But we also check econometric indicators such as significance of parameters or pseudo R^2 to find the final

[16]The entry density is measured as number of entries into BA or SUS divided by the stock of unemployed individuals on a local level. Local labor market conditions are captured by a standardized categorization of districts with similar labor market conditions (see Blien, Hirschenauer, Arendt, Braun, Gunst, Kilcioglu, Kleinschmidt, Musati, Roß, Vollkommer, and Wein, 2004).

specification.[17] The results of the *probit*-estimation can be found in Table A.2 in the Appendix. However, we want to briefly discuss the main components which influence the selection into treatment. In particular, variables such as age, duration of previous unemployment, regional cluster, information with respect to previous earnings and the intergenerational transmission turn out to be mostly important for the selection into SUS. In the case of 'BA vs. NP' the duration of previous unemployment, indicators for the labor market history and also intergenerational transmission have a remarkable impact. This is actually what we would expect due to the institutional background of both programs, that individuals with higher previous earnings are more likely to choose BA. In addition, we also provide a graphical illustration of the common support of estimated propensity scores in Figure A.1 in the Appendix. As we can see the distribution of the propensity scores are biased towards the tails, i.e., participants have on average an higher probability to become self-employed than nonparticipants. Nevertheless, participant's propensity score distribution overlaps the region of the propensity scores of nonparticipants completely. Therefore, Assumption 2 is fulfilled.

After having estimated the propensity scores we proceed with the estimation of the average treatment effects on the treated as depicted in Equation 2. Therefore, several matching procedures are suggested by the literature (see Caliendo and Kopeinig, 2008, for an overview). There is actually no obvious rule in the literature for choosing a matching algorithm it is rather more a trade-off in terms of variance and bias of the estimate. We run different matching algorithm and end up with the insight that in our case the results do not really differ and therefore decide to choose *Kernel*-Matching[18] since it allows us to increase the efficiency of the estimate due to exploiting as much as information as possible. Furthermore, we are able to apply *bootstrapping* in order to obtain a valid estimate for the standard errors.

To assess the matching quality, i.e., whether the matching procedure was able to balance the distribution of observable variables between participants and nonparticipants, we summarize in Table 5 some quality measures, such as a simple *t-test* of equal means, *Mean Standardized Bias* and *Pseudo* R^2 for the unmatched and matched sample, respectively.[19] First of all, we provide in the upper part of Table 5 the number of variables with

[17] For a more extensive discussion on the estimation of propensity scores we refer to Heckman, Ichimura, Smith, and Todd (1998) and Caliendo and Kopeinig (2008) among others.

[18] Precisely, we apply an *Epanechnikov Kernel* with an bandwidth of *0.06*.

[19] For a more intensive discussion with respect to assessing the matching quality we refer to Caliendo and Kopeinig (2008).

Table 5: Matching Quality

	Start-up Subsidy		Bridging Allowance	
	Before matching	After matching	Before matching	After matching
t-test of equal means[a)]				
1%-level	19	0	9	0
5%-level	29	2	15	0
10%-level	36	2	17	0
Standardized Bias				
Mean Standardized Bias	14.088	5.150	8.565	2.194
Number of variables with Standardized Bias of a certain amount				
$< 1\%$	2	12	3	22
1% until $< 3\%$	4	14	11	18
3% until $< 5\%$	4	14	6	7
5% until $< 10\%$	15	15	21	9
$\geq 10\%$	31	1	15	0
Pseudo-R^2	0.447	0.224	0.105	0.007

[a)] Depicted is the number of variables which are significant different between treated and controls. The decision bases on a simple *t-test* of equal means. In total, there are 56 observable variables.

different means between participants and nonparticipants by using a *t-test*.[20] For instance, we can see that for SUS 29 variables are statistically different in terms of means at the 5% level in the unmatched sample. However, after matching differences are only present anymore in two cases between treated and non-treated individuals. In fact, for the case of BA after matching we even find no differences in terms of observable characteristics suggesting a successful matching. However, Caliendo and Kopeinig (2008) note that one weakness of using a t-test to assess the matching quality is that the results do not report anything about the bias reduction due to matching. Therefore, we also provide the *Mean Standardized Bias* and the number of variables with a Standardized Bias of a certain amount in Table 5. It is visible that in case of 'SUS vs. NP' ('BA vs. NP') the *Mean Standardized Bias* declines from initially 14% to 5% after matching (9% to 2%). Caliendo and Kopeinig (2008) argue that a *Mean Standardized Bias* below a value between 3% and 5% after matching generally indicates a success of the matching approach. Finally, we also reestimated the propensity scores using the matched sample as suggested by Sianesi (2004). After matching took place the distribution of the covariates should be well balanced and hence the resulting *Pseudo* R^2 from the propensity score estimation should be pretty low. In Table 5 we observe a sharp drop in *Pseudo* R^2 for both programs also suggesting a successful matching. Finally, we argue that the applied matching procedure sufficiently balances the distribution of observable characteristics between both groups.

As outcome variables we use discrete outcomes capturing employment prospects in order to figure out whether programs lower the risk of being unemployed. In fact, we

[20] We consider the distribution of observable characteristics between participants and nonparticipants before and after matching based on 56 variables in total.

apply “Not unemployed” and “Self-employment or regular employment” as indicators for employment prospects due to two reasons: First, nonparticipants are less likely to become self-employed than participants. Hence, comparing participants and nonparticipants with respect to self-employment would, of course, bias the causal effects upwards. Second, the main objective of ALMP is to integrate individuals into the labor market which definitely includes being regularly employed as a success. Of course, being not registered as unemployed includes self-employment and regular employment but also captures an upper bound for the labor market integration, i.e., independence of unemployment or social benefits. Basically, the discrete outcome variables take on the value one if the individual is either “not unemployed” or “self-employed or regularly employed” and zero otherwise. Moreover, we use the “Working income” and “Total income” as continuous outcome variables. This will allow us to infer whether programs generate a monetary advantage compared to nonparticipants.

4.4 Main results

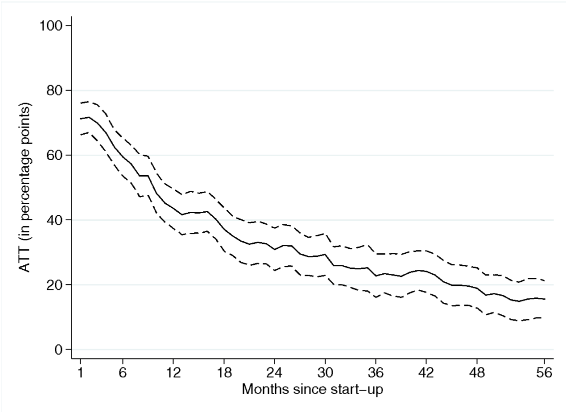
First of all, we provide causal effects with respect to employment prospects plotted over time in Figure 2. Depicted is the difference between participants and nonparticipants in terms of respective outcome variables, that is the average treatment effect on the treated as presented in Equation 2. Beside the graphical illustration we also provide in the first two columns of Table 6 the related exact values for selected points in time. As one can see in Figure 2 the effects are positive and significant at all time for either outcome variables. To be precise, 56 months after start-up participants in SUS (BA) have an 15.6% (10.6%) higher probability to be not registered as unemployed compared to nonparticipants. Regarding the integration into the labor market, i.e., either to be self-employed or regularly employed, we detect that the employment probability of participants is 22.1%-points higher for SUS and 14.5%-points for BA participants in comparison to nonparticipants. These are strong positive long-run effects and really vast compared to findings of evaluation studies investigating other programs of ALMP in Germany, such as vocational training or job creation schemes (see, e.g., Caliendo, Hujer, and Thomsen, 2008; Fitzenberger, Osikominu, and Völter, 2008; Lechner and Wunsch, 2008, amongst other).

Moreover, in case of initial BA recipients the positive effect seems to be somewhat stable after three years since start-up indicating that survived firms are well integrated in the market. However, for SUS supported individuals we do not find such a flat curve towards the end of the observation period. We argue that due to longer lasting financial

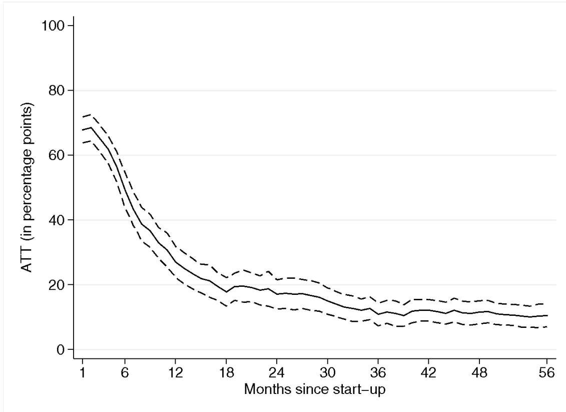
Figure 2: Causal effects of start-up subsidy and bridging allowance over time

Outcome variable: "Not unemployed"

SUS vs. NP

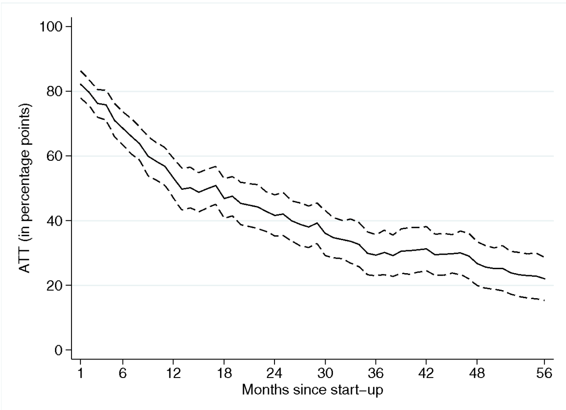


BA vs. NP

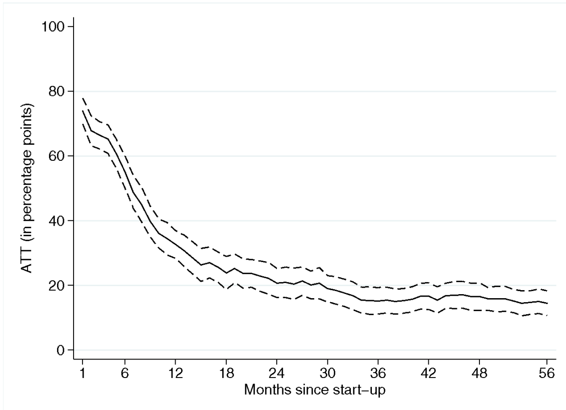


Outcome variable: "Self-employment or regular employment"

SUS vs. NP



BA vs. NP



Note: Depicted are average treatment effects on the treated (solid line), i.e., the difference in outcome variables between participants and nonparticipants. In addition, we provide the 5% confidence intervals (dashed lines) which base on *bootstrapped* standard errors with 200 replications.

support the adjustment process at the market is still ongoing. Because of this and the fact that the control group for BA participants is more competitive in the labor market than the assigned control group for SUS participants, the higher effects for SUS can not directly be contrasted to the results for BA participants. In Table 6 we also provide the cumulated effects over time which reveal that within our observation period of 56 months participants in SUS (BA) spent on average 23.5 (14.6) months more in self-employment or regular employment than nonparticipants, i.e., half of the overall observation period (one quarter of the period). One may argue that cumulating the effects over the entire period will capture locking-in effects since participants received financial support and were, therefore, better off. We take care of this by providing a partly cumulated effects in addition to the total cumulated effects. The main difference is that we cumulate the effects over the period beyond financial support only. For the case of SUS, we find that participants are on average 5.5 months more self-employed or regularly employed than nonparticipants which actually depicts 20% of the remaining period of 20 months. For BA participants we find an partly cumulated effect of 10.8 months, that is 20% of the remaining period of 50 months. Therefore, we can see that the locking-in effect seems to be much more important in case of SUS because the cumulated effect in relation to the underlying period decreased much more serious than for BA. Moreover, to answer the question of income gains for participants we provide the causal effects for income differences at the end of the observation period in the bottom of Table 6. The results unambiguously show that participants earn significantly more than nonparticipants for both programs.

Table 6: Causal effects of start-up subsidy and bridging allowance

	Estimation sample		Optimal Subpopulation $\hat{\mathbb{A}}$	
	SUS vs. NP	BA vs. NP	SUS vs. NP	BA vs. NP
Number of observations				
$\hat{\alpha}$			0.1049	0.1206
Treated	472	756	428	725
Controls	853	853	673	842
Outcome variable: "Not unemployed"				
Difference in percentage points				
After 6 months	59.4	49.3	59.0	48.8
After 36 months	22.9	10.9	20.2	11.3
After 56 months	15.6	10.6	15.0	11.1
Difference in months				
Total cumulated effect ($\sum_{t=1}^{56}$)	18.7	12.2	17.9	12.4
Partly cumulated effect[a)]	3.9	8.5	3.7	8.7
Outcome variable: "Self-employment or regular employment"				
Difference in percentage points				
After 6 months	68.5	55.0	68.8	54.7
After 36 months	29.4	15.3	27.4	15.8
After 56 months	22.1	14.5	21.1	15.3
Difference in months				
Total cumulated effect ($\sum_{t=1}^{56}$)	23.5	14.6	22.9	14.9
Partly cumulated effect[a)]	5.5	10.8	5.2	11.1
Outcome variable: "Income 56 months after start-up"				
Difference in €/month				
Working income	435	618	410	613
	(135)	(110)	(137)	(118)
Total income	270	485	257	480
	(121)	(110)	(153)	(105)

Note: Depicted are average treatment effects on the treated as the difference in outcome variables between participants and nonparticipants. The optimal subpopulation $\hat{\mathbb{A}}$ is defined by a restricted propensity score distribution, i.e., only individuals with $\hat{e}(X_i) \in [\hat{\alpha}, 1-\hat{\alpha}]$ are included. All effects for "Not unemployed" and "Self-employment or regular employment" are statistically significant (1%-level). For the income effects standard errors are in parentheses; standard errors base on *bootstrapping* with 200 replications.
[a)] To avoid locking-in effects we alternatively provide a partly cumulated effect for the period beyond financial support only (SUS: $\sum_{t=37}^{56}$, BA: $\sum_{t=7}^{56}$).

In addition to the results for the full estimation sample we also provide *Optimal Subpopulation Average Treatment Effects* (OSATE) in the last two columns of Table 6 as suggested by Crump, Hotz, Imbens, and Mitnik (2009). The basic idea is that an optimal subset $\mathbb{A}$ is defined by choosing an optimal cut-off value α that balances the trade-off between two opposing variance components. On the one hand side, by discarding observations the variance of the estimator increases simply due to smaller sample size. However, on the other hand the variance of the estimand will in turn decrease if one excludes observation with covariate values outside the range of the comparison group. Finally, Crump, Hotz, Imbens, and Mitnik (2009) argue that balancing these opposing variance components yields the optimal cut-off value α and consequently $\mathbb{A}$. Supposing the distribution of the outcome is homoscedastic the authors derive an algorithm[21] to construct the optimal cut-off value α. This procedure has the appealing feature that it only depends on propensity scores and hence it does not require any outcome information. Based on the optimal subpopulation $\mathbb{A}$ the average treatment effects can be straightforwardly calculated. However, Imbens and Wooldridge (2008) note that restricting the estimation sample will lower external validity of the estimate but it probably enhances internal validity.

We apply the method suggested by Crump, Hotz, Imbens, and Mitnik (2009) and determine the optimal cut-off value $\hat{\alpha}$ for 'SUS versus NP' and 'BA versus NP' respectively using the estimated propensity scores $\hat{P}(W_i)$ (see Table A.2). According to this, we constructed the optimal subpopulation $\hat{\mathbb{A}}$ by dropping all observations outside the optimal range, i.e., using only individuals with $\hat{P}(W_i) \in [\hat{\alpha}, 1-\hat{\alpha}]$. In the upper part of Table 6 we provide the number of observations for the initial sample and for the optimal subsample $\hat{\mathbb{A}}$. Moreover, Table 6 also contains the optimal cut-off values $\hat{\alpha}$. Therefore, the optimal subpopulation $\hat{\mathbb{A}}$ in case of 'SUS versus NP' consists of individuals with $\hat{P}(W_i) \in [0.1049, 0.8951]$ reducing the number of treated individuals (controls) from initially 472 to 428 (853 to 673). In case of 'BA versus NP' we find an optimal range of $\hat{P}(W_i) \in [0.1206, 0.8794]$ and consequently we had to drop 31 treated individuals and 11 controls. Even though the optimal range is more wider for 'SUS versus NP' the loss of observations is more dramatic than for 'BA versus NP'. But this is actually not surprising because in Figure A.1 we can see that the distribution of the propensity scores is more separated between participants and nonparticipants for 'SUS versus NP' than for 'BA versus NP'.

The *Optimal Subpopulation Average Treatment Effects* in Table 6 do hardly show any substantial changes in comparison to the results for the full estimation sample. The amount of deviation is slightly larger for SUS than for BA since the scope of the sample

[21] For the implementation in STATA Crump, Hotz, Imbens, and Mitnik (2009) provide *optselect.ado*. This file is available via the authors.

reduction is much bigger for the case of SUS and therefore the subpopulation is more restrictive than for BA.

To sum up, our results suggest that supporting unemployed individuals by SUS or BA has been a success in terms of both employment prospects as well as income measures compared to nonparticipation.

4.5 Effect heterogeneity

After having presented and discussed the main effects in Section 4.4 we now want to take a closer look on effect heterogeneity. Therefore, we conduct the complete estimation procedure, i.e., propensity score estimation and *kernel*-matching, for different subgroups of our sample with respect to education, job qualification, age, nationality and local labor market conditions. This is in particular interesting because it reveals for which groups of individuals such programs are most effective. For a comprehensive overview we summarize all results in Table 7 and for the sake of convenience we also provide the main results from Table 6 in the upper part of Table 7. This allows a direct comparison to the decomposed results below.

First of all, let us start with results regarding educational attainment. We split the sample into low and highly educated individuals. In doing so, the category highly educated captures individuals who graduated from upper secondary school, while no degree, lower or middle secondary school graduation is defined as low education. It seems that low educated participants in both programs perform better in terms of employment prospects than highly educated. This is mainly driven by the fact that highly educated controls have an higher probability to be employed at all time than the respective low educated comparison group.[22] In other words, the highly educated control group is more competitive in the labor market which can also be seen in Figure A.2 where we depict the probability levels over time. Hereby, the treatment group is captured by solid lines while the controls are depicted by dashed lines. As one can see for both programs in the upper part of Figure A.2 the dashed black line for the low educated controls is clearly below the dashed grey line for the highly educated controls. While the solid lines for low and highly educated treated individuals are closer together. This basically confirms that the low educated

[22]In Table A.3 in the Appendix we provide the labor market status for the unmatched sample separated by the subgroups and treatment status. As we can see in case of educational level, the fraction of highly educated controls in self-employment or regular employment is throughout larger than for low educated controls for both programs. Of course, these are descriptive results for the unmatched sample but nevertheless it supports the assertion that highly educated controls are more competitive in the labor market than low educated controls.

control group performs relatively worse and consequently the effects are bigger for that group. This in turn indicates that providing individuals with bad labor market chances an opportunity to exit unemployment by becoming self-employed is throughout effective. For the income effects in Table 7 we cannot detect such an obvious pattern as for the employment prospects. While in case of ‘SUS vs. NP’ the low educated participants yield much higher income effects compared to nonparticipation than the highly educated do, in case of ‘BA vs. NP’ it is reverse, i.e., the highly educated are better off than there counterparts. This basically suggests that highly educated BA recipients who survived in self-employment are also very successful in terms of income even though the controls are more competitive for respective group.

Secondly, we use professional qualification and assign all individuals with tertiary or technical college education to be highly qualified while skilled or unskilled workers are supposed to be low qualified. As we can see in Table 7 the effect pattern is similar to the previous analysis with respect to educational attainment which is mainly because professional qualification and education are highly correlated. Moreover, the probability levels for low and highly qualified individuals in Figure A.2 shows also a similar pattern suggesting that the comparatively low competitiveness of the control groups mainly drives the different effects.

We also conducted the analysis separately for individuals with an age of 30 years or younger as well as for individuals beyond the age of 30 years. Here, the effects for both programs are reverse. The results suggest that SUS tends to be more effective for participants above the age of 30 years while BA seems to be more effective for younger participants. Figure A.2 reveals that this is mainly due to reversely bad labor market performance of the controls in either case. For instance, there is hardly any difference for SUS participants (solid lines) but at all times a considerable higher share of young controls is employed or self-employed, i.e., the black dashed line lies above the grey dashed line. The reverse case applies for BA. Probably more experienced (>30 years) BA controls are more likely to be employed or self-employed. This makes in particular sense because as shown in Section 4.1 BA attracts rather highly educated individuals with higher earnings in the past. Apparently, for these individuals experience is important in order to find a job in the labor market and therefore older BA control individuals perform better in the labor market. On the other side, low educated individuals with bad labor market performance in the past (mainly attracted by SUS) have the less chances in the labor market the older those are. Consequently, SUS provides those a real opportunity to escape unemployment.

Table 7: Effect heterogeneity: Causal effects of start-up subsidy and bridging allowance

	SUS vs. NP	BA vs. NP
	Main results	
Self-employed or regularly employed		
After 36 months (in %-points)	22.9	10.9
After 56 months (in %-points)	15.6	10.6
Total cumulated effect ($\sum_{t=1}^{56}$, in months)	18.7	12.2
Working income (t=56, in €/month)	435	618

	SUS vs. NP		BA vs. NP	
	Educational level			
	Low	High	Low	High
Self-employed or regularly employed				
After 36 months (in %-points)	29.6	25.5	20.0	10.6
After 56 months (in %-points)	23.7	17.6	19.2	11.7
Total cumulated effect ($\sum_{t=1}^{56}$, in months)	24.5	19.0	17.1	12.8
Working income (t=56, in €/month)	616	(-100)	416	768
	Professional qualification			
	Low	High	Low	High
Self-employed or regularly employed				
After 36 months (in %-points)	27.3	16.3	15.8	12.7
After 56 months (in %-points)	20.5	11.5	17.1	12.4
Total cumulated effect ($\sum_{t=1}^{56}$, in months)	23.5	15.4	16.1	12.5
Working income (t=56, in €/month)	628	-464	486	865
	Age			
	≤ 30	> 30	≤ 30	> 30
Self-employed or regularly employed				
After 36 months (in %-points)	21.9	27.0	20.1	15.7
After 56 months (in %-points)	(8.7)	21.3	10.5	16.2
Total cumulated effect ($\sum_{t=1}^{56}$, in months)	17.9	23.4	18.7	14.8
Working income (t=56, in €/month)	543	374	914	573
	Nationality			
	Native	Non-German	Native	Non-German
Self-employed or regularly employed				
After 36 months (in %-points)	27.3	20.6	15.9	10.6
After 56 months (in %-points)	20.0	15.7	14.2	14.5
Total cumulated effect ($\sum_{t=1}^{56}$, in months)	22.0	21.1	15.3	12.4
Working income (t=56, in €/month)	(305)	(249)	612	587

	SUS vs. NP			BA vs. NP		
	Local labor market condition					
	Bad	Medium	Good	Bad	Medium	Good
Self-employed or regularly employed						
After 36 months (in %-points)	20.3	31.8	24.7	22.0	16.3	14.5
After 56 months (in %-points)	18.2	24.2	18.4	19.1	14.2	15.9
Total cumulated effect ($\sum_{t=1}^{56}$, in months)	19.7	23.7	21.0	17.4	14.5	14.4
Working income (t=56, in €/month)	576	(450)	(235)	674	402	616

Note: Depicted are average treatment effects on the treated as the difference in outcome variables between participants and nonparticipants. The educational level is decomposed into 'high' education capturing individuals who graduated from upper secondary school and 'low' education including individuals which have either no school degree, or finished lower or middle secondary school. With respect to professional qualification we define individuals with tertiary or technical college education as 'highly' qualified, while skilled or unskilled workers are categorized as 'low' qualified. The classification of local labor market conditions relies on a standardized categorization of districts (see Blien, Hirschenauer, Arendt, Braun, Gunst, Kilcioglu, Kleinschmidt, Musati, Roß, Vollkommer, and Wein, 2004). Basically, 'bad' conditions are characterized by high unemployment rates, the 'medium' category captures areas with average unemployment rates and 'good' conditions embraces districts with rather low unemployment rates. Effects which are not significant different from zero at the 5%-level are in parentheses; standard errors base on *bootstrapping* with 200 replications.

With respect to nationality Table 7 reports slightly higher effects for natives than for Non-Germans. Figure A.2 shows that the higher effects for natives are basically driven by the success of the participants. It is visible that control groups do not really differ for both groups, i.e., dashed lines almost overlap. This in turn suggests that SUS and BA seem to be even more efficient for native participants.

Finally, we consider different types of local labor market conditions. Therefore, the classification of local labor market conditions relies on a standardized categorization of districts (see Blien, Hirschenauer, Arendt, Braun, Gunst, Kilcioglu, Kleinschmidt, Musati, Roß, Vollkommer, and Wein, 2004). Basically, bad conditions are characterized by high unemployment rates, the medium category captures areas with average unemployment rates and good conditions embraces districts with rather low unemployment rates. Overall, programs tend to be more effective in regions with rather worse labor market conditions. For instance, for 'SUS vs. NP' the employment effects are the highest in regions with 'medium' conditions and even the income effects are only significantly positive in areas with bad labor market conditions. This basically supports the hypothesis that SUS and BA are most effective for rather problematic areas because such programs provide a real opportunity to regular employment and indeed in regions characterized by high unemployment rates and low labor market dynamics job opportunities are limited.

To sum up, we find that SUS and BA are basically most effective for individuals either with rather low labor market performance, i.e., low educated/low qualified persons, or unemployed individuals living in areas facing limited labor market perspectives. As an attractive side effect we want to highlight that potential deadweight effects are probably smallest for those individuals since they start their own business mainly due to *necessity* and it is most likely that these participants would not have become self-employed given sufficient job opportunities in the regular labor market. Therefore, we argue deadweight effects should be pretty small for such individuals.

5 Sensitivity analysis

After having presented strong positive effects for both programs, we now want to check the robustness of our results with respect to the violation of underlying assumptions of propensity score matching.[23] The most restrictive assumption depicts definitely the CIA, i.e., if selection into treatment is only due to observable characteristics and not determined by unobserved characteristics. The violation of the CIA would lead to biased results and with non-experimental data it is impossible to test this assumption directly. In this study we compare entries into subsidized self-employment with other unemployed individuals

[23] Furthermore, we applied Inverse Probability Weighting (IPW) as an alternative approach for estimating ATT as suggested by Imbens (2004). This method also relies on the CIA. Using IPW we find hardly any substantial differences for the employment effects but slightly higher income effects. Therefore, we argue that our results are relatively robust with respect to the estimation method.

and therefore it is throughout conceivable that although participants and nonparticipants are equal in terms of observable characteristics both groups probably differ in unobserved terms to some extent. Therefore, in this section we want to extensively discuss a potential violation of the CIA. To do so, we firstly use a *bounding approach* initially suggested by Rosenbaum (2002) which basically simulates an unobserved component and tests up to which degree the results are robust. Using this approach does not answer the question whether the CIA is fulfilled or not but rather gives an impression how robust the results are with respect to unobserved heterogeneity. Moreover, as a second sensitivity check we apply *conditional difference-in-differences* (DID) to allow for unobservable but temporally invariant differences in outcomes between participants and nonparticipants what obviously relaxes the strong CIA.

5.1 Bounding approach

First of all, we want to test our results with respect to unobserved heterogeneity using the *bounding approach* suggested by Rosenbaum (2002). Based on that idea Becker and Caliendo (2007) provide an useful implementation in STATA (*mhbounds.ado*) which we are going to apply here. The main idea is that in presence of unobserved factors identical individuals with respect to observable characteristics (x_i) have different probabilities to receive treatment. Therefore, an artificial factor Γ will be introduced to simulate an unobserved term. The underlying test statistic basically tests to which extend this unobserved factor Γ will influence the significance of the results. This is, of course, just a very brief description of the test procedure and hence for further details we refer to Becker and Caliendo (2007). But before presenting the test statistics we want to explicitly emphasize that this procedure does not test the justification of the CIA itself because that is actually impossible with non-experimental data. Instead it tests to which degree the results are sensitive with respect to unobserved terms.

In our study we find strong positive effects and therefore we are only interested in the test-statistic for the upper bound under the assumption that we have overestimated the treatment effect.[24] To put it in other words, if unobserved factors lead to positive selection, i.e., those who participate have always a higher employment probability even in the absence of treatment, Q^+ will become insignificant for a certain value of Γ. To ease the test decision we also provide respective p-values (p^+).

[24]We neglect the contrary case Q^- which is the lower bound under the assumption that the effects are underestimated because we find positive effects and due to construction of the test-statistic the lower bound will be always significant.

Table 8: Unobserved heterogeneity: Mhbounds

Γ	Not-unemployed				Self-employment or regular employment			
	SUS vs. NP		BA vs. NP		SUS vs. NP		BA vs. NP	
	Q^+	p^+	Q^+	p^+	Q^+	p^+	Q^+	p^+
After 36 months since start-up								
1.00	4.963608	0.000000	7.894250	0.000000	6.781637	0.000000	10.631149	0.000000
1.25	3.591345	0.000164	6.347955	0.000000	5.185760	0.000000	8.832536	0.000000
1.50	2.489725	0.006392	5.119310	0.000000	3.903610	0.000047	7.400116	0.000000
1.75	1.569157	0.058306	4.101810	0.000020	2.833003	0.002306	6.213142	0.000000
2.00	0.777031	0.218570	3.233391	0.000612	1.913234	0.027859	5.200418	0.000000
2.25	0.080453	0.467938	2.475486	0.006653	1.1061142	0.134339	4.317440	0.000008
2.50	0.369840	0.355751	1.802569	0.035728	0.3861308	0.349700	3.534599	0.000204
2.75	0.930431	0.176074	1.196910	0.115671	0.1170226	0.453421	2.831243	0.002318
3.00	1.443624	0.074422	0.645718	0.259231	0.7088676	0.239204	2.192416	0.014175
Critical values								
1%	1.50 - 1.55		2.30 - 2.35		1.85 - 1.90		2.90 - 2.95	
5%	1.70 - 1.75		2.55 - 2.60		2.05 - 2.10		—	
10%	1.80 - 1.85		2.70 - 2.75		2.15 - 2.20		—	
After 56 months since start-up								
1.00	3.721316	0.000099	8.596416	0.000000	4.473471	0.000004	10.331853	0.000000
1.25	2.355214	0.009256	7.163100	0.000000	2.861920	0.002105	8.616018	0.000000
1.50	1.252289	0.105232	6.034373	0.000000	1.557560	0.059669	7.253844	0.000000
1.75	0.325485	0.372407	5.106941	0.000000	0.460381	0.322621	6.127887	0.000000
2.00	0.306964	0.379436	4.320818	0.000008	0.346495	0.364485	5.169143	0.000000
2.25	1.011090	0.155987	3.638923	0.000137	1.181895	0.118624	4.334600	0.000007
2.50	1.643147	0.050176	3.036824	0.001195	1.931301	0.026723	3.595710	0.000162
2.75	2.217923	0.013280	2.497621	0.006251	2.612170	0.004498	2.932602	0.001681
3.00	2.746116	0.003015	2.009166	0.022260	3.237181	0.000604	2.330912	0.009879
Critical values								
1%	1.25 - 1.30		2.80 - 2.85		1.30 - 1.35		—	
5%	1.40 - 1.45		—		1.45 - 1.50		—	
10%	1.45 - 1.50		—		1.55 - 1.60		—	

Note: Reported results are achieved by using *mhbounds.ado* (see Becker and Caliendo, 2007). Critical values are related to the exact values of Γ where results turn insignificant.

Table 8 contains test results separately for the outcome variables "Not unemployed" and "Self-employment or regular employment" and for 'SUS vs NP' and 'BA vs NP'. We consider the outcome variables once after 36 months since start-up in the upper part of Table 8 and after 56 months in the lower part.[25] Underneath the detailed test-statistics and respective p-values we provide the exact values of Γ at which results turn insignificant. First of all, in case of absence of unobserved influence, i.e., $\Gamma = 1.0$, we can see that the test statistic for the upper bounds are throughout significant indicated by $p^+ < 0.05$ for all cases. Starting from that point we slightly increase the value of Γ step-by-step. As mentioned already this actually simulates an ascending influence of unobserved factors. We find mixed results for both programs. While for BA results seem to be pretty stable against unobserved selection bias, results for SUS turn insignificant at certain levels of Γ. For instance, regarding "Self-employment or regular employment" after 56 months since start-up, this is in the lower and rear part of Table 8, we can see for 'SUS vs. NP' that the results will be insignificant at the 5%-level for $\Gamma > 1.45$. It is also interesting that

[25] We also conducted the test for different points in time but the results hardly differ.

results become significant again for values of $\Gamma > 2.75$. This indicates negative treatment effects and is basically due to large positive unobserved heterogeneity. However, results for the case of BA are significant up to $\Gamma = 3$ suggesting that selection into BA is relatively unaffected by unobserved factors.

Finally, we argue that our results are slightly sensitive with respect to positive unobserved selection for SUS and robust for BA. We have to keep that in mind when we interpret the results. But again be aware that the results do not reveal whether unobserved factors are present or not and therefore whether the CIA is fulfilled or not. The test only suggests to what extend unobserved factors would influence the significance of the results.

5.2 Conditional DID

However, the DID estimator relaxes the strong CIA and allows for unobservable but temporally invariant differences in outcomes between participants and nonparticipants. This is achieved by comparing the conditional before/after outcomes of participants with those of nonparticipants and gives, hence, and impression whether the CIA is justified in our case. DID matching was firstly suggested by Heckman, Ichimura, Smith, and Todd (1998). It extends the conventional DID estimator by defining outcomes conditional on the propensity score and using semiparametric methods to construct the differences. Therefore, it is superior to DID as it does not impose linear functional form restrictions in estimating the conditional expectations of the outcome variable, and it re-weights the observations according to the weighting function of the matching estimator (Smith and Todd, 2005). If the parameter of interest is ATT, the conditional DID estimator is based on the following identifying assumption:

$$E[Y_t^0 - Y_{t'}^0 | P(W), D = 1] = E[Y_t^0 - Y_{t'}^0 | P(W), D = 0], \tag{3}$$

where (t) is the post-treatment and (t') the pre-treatment period. It also requires the common support condition to hold and can be written as:

$$\tau_{ATT}^{CDID} = E(Y_t^1 - Y_{t'}^0 | P(W), D = 1) - E(Y_t^0 - Y_{t'}^0 | P(W), D = 0). \tag{4}$$

Before we can proceed with estimating the effects using DID we have to define the reference level for the pre-treatment period. We basically use three different time periods: (1) the last two and a half years just before entry into treatment (Jan 2001 - June 2003), (2) the two and a half years before period 1 (July 1998 - Dec. 2000) and (3) the complete 5 years before entry into treatment (July 1998 - June 2003). We cumulate monthly employment outcomes over these periods. As post-treatment outcomes we choose the total cumulated and partly cumulated effects as depicted in Table 6. In addition to employment outcomes we also calculate treatment effects using DID for income measures, i.e., average monthly total and working income in 2002. The results are depicted in Table 9.

Table 9: Conditional DID: Cumulated employment and income effects of start-up subsidy and bridging allowance

	SUS vs. NP	BA vs. NP
Outcome variable: "Not unemployed" (in months)		
Reference level: Total post-treatment period ($\sum_{t=1}^{56}$)		
Matching result	18.7	12.2
DID1	17.7	12.2
DID2	17.9	11.7
DID3	16.9	11.7
Reference level: Unsubsidized post-treatment period (SUS: $\sum_{t=37}^{56}$ BA: $\sum_{t=7}^{56}$)		
Matching result	3.9	8.5
DID1	2.9	8.5
DID2	3.1	8.0
DID3	(2.1)	8.0
Outcome variable: "Income 56 months after start-up" (in €/months)		
Working income		
Matching result	435	618
DID4	475	656
Total income		
Matching result	270	485
DID5	288	480

Note: Effects in parentheses are not significant different from zero at the 5%-level; standard errors base on *bootstrapping* with 200 replications.
Reference level for pre-treatment period: DID1: Jan. 2001 - June 2003; DID2: July 1998 - Dec. 2000; DID3: July 1998 - June 2003; DID4: average monthly total income in 2002; DID5: average monthly income from employment in 2002.

As one can see, the deviation from the initial matching results are rather small. For instance, in case of 'BA vs. NP' using the total cumulated effect we find participants being on average 12.2 months more in employment or self-employment than nonparticipants. Using DID the results vary from 11.7 to 12.2. Even the income effects are pretty close to the matching results. This evidence suggest that controlling for unobserved heterogeneity does not add essential information and consequently indicates that the CIA seems to be valid for our analysis.

6 Conclusion

In this study we investigate long-term effects for programs designed to turn unemployment into self-employment in West Germany. We show that the programs under scrutiny (SUS and BA) attract different groups of participants in terms of observable characteristics. The individuals participating in BA are more educated individuals with higher earnings in the past while SUS rather attracts individuals with worse labor market performance, i.e., that are on average lower educated with less employment periods in the past. Therefore, ALMP in Germany provides financial support to become self-employed for a wide range of unemployed individuals.

Using an unique dataset consisting of administrative and survey data we are able to follow individuals up to five years after they launched their business. We find that 56 months after start-up about 81% of SUS and 89% of BA participants are well integrated in the labor market. In contrast, for the nonparticipants we only find a fraction of 63% in self-employment or regular employment. These remarkable results for participants indicate that both programs have lasting success in bringing the unemployed out of unemployment.

In order to assess the effectiveness of SUS and BA we estimate long-term causal effects of the programs against nonparticipation based on conditional propensity score matching methods. In particular, we use the probability of being employed (either self-employed or as an employee) and personal income as outcome variables. Our results show that at the end of the observation period, both programs are effective with respect to these outcome variables. In fact, total cumulated effects show that participants in SUS (BA) are on average 23.5 (14.6) months more in employment or self-employment than nonparticipants. Taking into account that our observation window consists of 56 months in total, these results are remarkable. To validate our findings we conduct several sensitivity checks. We find that our results seem to be robust against unobserved heterogeneity. Regarding effect heterogeneity we estimate causal effects for different subgroups with respect to educational attainment, job qualification, age, nationality and local labor market conditions. The results suggest that both programs are in particular effective for low educated/qualified individuals and in regions characterized by adverse labor market conditions. With respect to nationality and age the results are mixed.

In summary, we conclude that in contrast to other German ALMP programs that have been evaluated recently, we find a high degree of integration in the labor market among participants and also strong positive effects for both programs compared to nonparticipation.

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A Appendix

Table A.1: Descriptive statistics

	SUS	BA	NP
Number of observations	472	756	853
Age bracket			
18 to 24 years	0.068	0.026	0.049
25 to 29 years	0.131	0.095	0.095
30 to 34 years	0.174	0.126	0.130
35 to 39 years	0.153	0.242	0.212
40 to 44 years	0.176	0.200	0.210
45 to 49 years	0.127	0.160	0.165
50 to 64 years	0.172	0.151	0.138
Marital status (Ref.: Single)			
Married	0.472	0.648	0.594
Number of children in household			
No children	0.708	0.595	0.639
1 child	0.144	0.155	0.145
2 or more children	0.148	0.250	0.216
Health restriction that affect job placement (Ref.: No)			
Yes	0.078	0.033	0.057
Nationality (Ref.: German)			
Non-German	0.328	0.265	0.249
Desired working time (Ref.: Part-time)			
Full-time	0.977	0.992	0.984
School leaving certificate			
No degree	0.028	0.007	0.014
Lower secondary degree	0.405	0.290	0.370
Middle secondary degree	0.250	0.233	0.223
Specialized upper secondary school	0.104	0.193	0.150
Upper secondary school	0.214	0.278	0.243
Occupational group			
Manufacturing	0.040	0.011	0.018
Agriculture	0.333	0.233	0.277
Technical occupations	0.038	0.160	0.108
Services	0.517	0.565	0.539
Others	0.072	0.032	0.059
Professional qualification			
Workers with tertiary education	0.123	0.259	0.200
Workers with technical college education	0.068	0.112	0.106
Skilled workers	0.559	0.515	0.549
Unskilled workers	0.250	0.114	0.145
Duration of previous unemployment			
$<$ 1 month	0.133	0.074	0.014
$\geq$ 1 month - $<$ 3 months	0.150	0.222	0.223
$\geq$ 3 months - $<$ 6 months	0.212	0.249	0.251
$\geq$ 6 months - $<$ 1 year	0.288	0.316	0.339
$\geq$ 1 year - $<$ 2 years	0.155	0.124	0.150
$\geq$ 2 years	0.061	0.015	0.023
Professional experience (Ref.: Without professional experience)			
With professional experience	0.824	0.860	0.877
Last employment			
Duration of last employment	32.394	54.041	41.963
	(40.987)	(54.358)	(49.076)
Placement propositions			
Number of placement propositions	5.367	3.758	5.181
	(8.563)	(6.921)	(7.664)
Average daily income from regular employment in first half of 2003			
BEH	9.969	25.783	20.700
	(21.571)	(41.503)	(34.970)
Unemployment benefit level (in Euro)	24.363	40.405	33.167
	(11.436)	(15.275)	(14.322)
Remaining unemployment benefit entitlement (in months)	4.752	7.317	7.054
	(5.759)	(6.380)	(6.397)
Employment status before job-seeking			
Employment	0.591	0.782	0.769
Self-employed	0.053	0.024	0.036
School attendance/never employed before/apprenticeship	0.123	0.073	0.063
Unemployable	0.083	0.042	0.059
Others, but at least once employed before	0.131	0.070	0.066
Others	0.019	0.009	0.007
Regional cluster			
II a	0.013	0.024	0.028
II b	0.153	0.159	0.135
III a	0.127	0.071	0.088
III b	0.083	0.091	0.110
III c	0.222	0.237	0.244
IV	0.127	0.144	0.117
V a	0.036	0.042	0.038
V b	0.165	0.148	0.176
V c	0.074	0.083	0.064
Intergenerational transmission			
Parents are/were self-employed	0.284	0.284	0.155

Note: Standard deviation in parentheses.

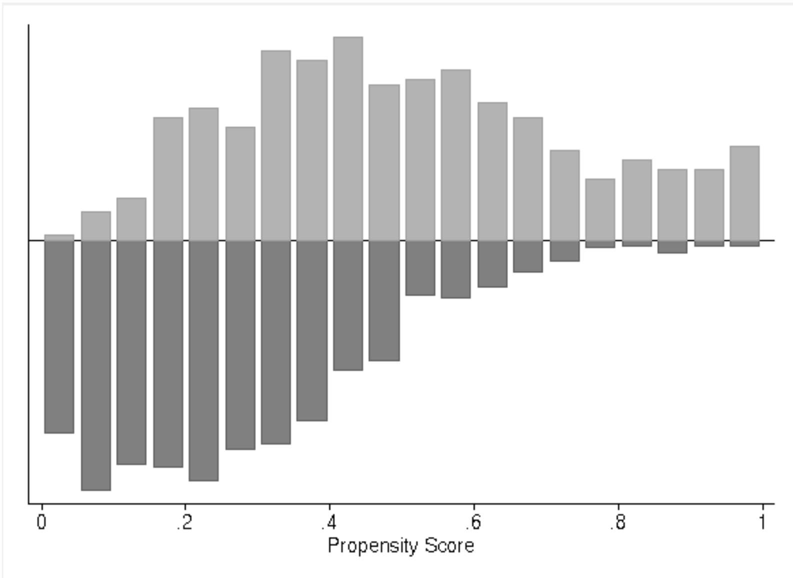
Table A.2: Propensity score estimation

	SUS vs. nonparticipation	BA vs. nonparticipation
Age bracket (Ref.: 18 to 24 years)		
25 to 29 years	0.430**	0.354*
30 to 34 years	0.508**	0.254
35 to 39 years	0.266	0.291
40 to 44 years	0.361*	0.119
45 to 49 years	0.433**	0.196
50 to 64 years	0.863***	0.316
Marital status (Ref.: Single		
Married	−0.098	0.009
Number of children in household (Ref.: No children)		
one child	0.184	−0.105
Two or more children	0.089	−0.160
Health restriction that affect job placement (Ref.: No)		
Yes	−0.129	−0.090
Nationality (Ref.: German)		
Non-German	0.095	0.164**
Desired working time (Ref.: Part-time)		
Full-time	−0.037	0.135
School leaving certificate (Ref.: No degree)		
Lower secondary school	−0.081	0.228
Middle secondary school	0.069	0.293
Specialized upper secondary school	−0.063	0.333
Upper secondary school	0.038	0.288
Occupational group (Ref.: Manufacturing)		
Agriculture	−0.250	0.100
Technical occupations	−0.705**	0.261
Services	−0.395	0.089
Others	−0.597**	−0.342
Professional qualification (Ref.: Workers with tertiary education)		
Workers with technical college education	0.126	−0.038
Skilled workers	0.071	0.042
Unskilled workers	0.198	0.066
Duration of previous unemployment (Ref.: $<$ 1 month)		
$\geq$ 1 month - 3 months	−1.634***	−0.893***
$\geq$ 3 months - $<$ 6 months	−1.488***	−0.943***
$\geq$ 6 months - $<$ 1 year	−1.639***	−1.069***
$\geq$ 1 year - $<$ 2 years	−1.765***	−1.118***
$\geq$ 2 years	−1.316***	−1.145***
Professional experience (Ref.: without professional experience)		
with professional experience	−0.123	−0.251**
Last employment		
Duration of last employment	0.001	0.002**
Placement propositions		
Number of placement propositions	−0.006	−0.010**
Employment status before job-seeking (Ref.: Employment)		
Self-employed	0.290	−0.373*
School attendance/never employed before/apprenticeship	0.362**	0.225
Unemployable	0.197	−0.072
Others, but at least once employed before	0.458***	0.246*
Others	0.352	0.456
Regional cluster (Ref.: II a)		
II b	0.730**	0.224
III a	0.744**	0.043
III b	0.545*	0.157
III c	0.609*	0.118
IV	0.911***	0.183
V a	0.636*	0.415
V b	0.707**	−0.041
V c	0.782**	0.262
Remaining unemployment benefit entitlement (in months)	−0.028***	−0.024***
Unemployment benefit level (in Euro)	−0.029***	0.024***
Average daily income from regular employment in first half of 2003		
BEH	−0.002	−0.002*
Intergenerational transmission		
Parents are/were self-employed	0.476***	0.453***
Constant	1.240**	−0.607
Number of observations	1,325	1,609
Pseudo R^2	0.196	0.105
Log-likelihood	−693.612	−995.964

Note: Coefficients are estimated using a *probit*-model. * 10%, ** 5%, *** 1% significance level.

Figure A.1: Common support of estimated propensity scores

SUS vs. NP



BA vs. NP

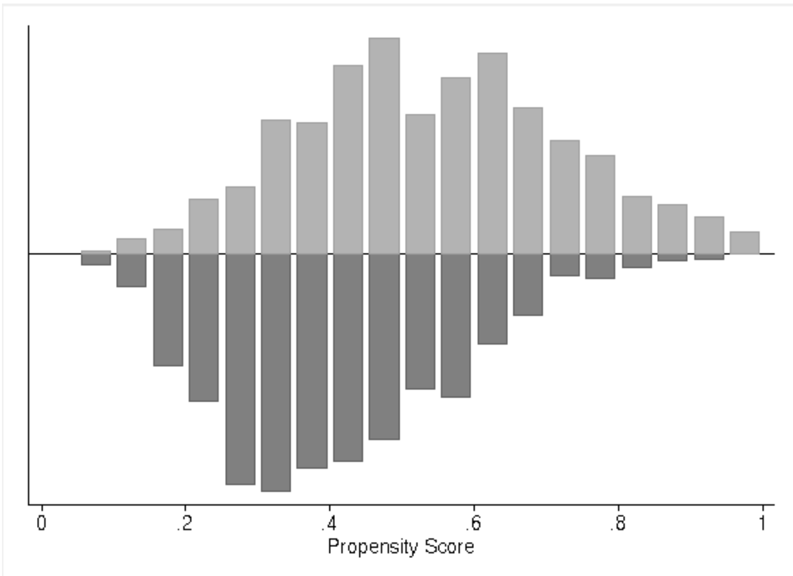


Table A.3: Effect heterogeneity: Labor market status 56 months after start-up

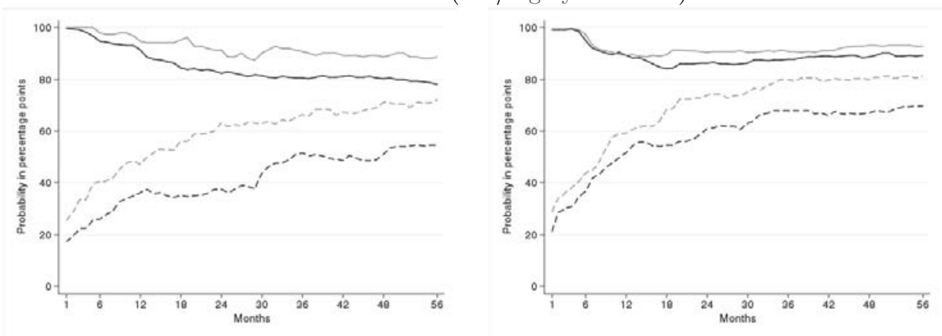
	SUS	BA	NP	SUS	BA	NP
	Educational level					
	Low			High		
Self-employed	56.5	67.3	9.3	68.6	68.0	22.5
Regularly employed	21.7	18.5	50.3	18.6	24.7	47.0
Unemployed or in ALMP	14.7	9.2	23.1	3.5	3.2	14.3
Others	7.1	5.1	17.3	9.1	3.1	16.2
	Professional qualification					
	Low			High		
Self-employed	56.7	67.3	9.5	75.7	69.3	27.1
Regularly employed	21.9	18.8	50.1	15.5	25.5	46.5
Unemployed or in ALMP	13.5	8.6	22.9	2.3	2.9	11.5
Others	7.9	5.3	17.6	6.6	2.2	14.9
	Age					
	≤ 30			> 30		
Self-employed	57.3	65.5	7.3	60.7	68.5	16.1
Regularly employed	25.6	19.9	67.6	18.9	21.3	43.6
Unemployed or in ALMP	12.6	10.7	10.8	11.4	5.8	22.6
Others	4.5	3.8	14.3	9.0	4.4	17.6
	Nationality					
	Natives			Non-German		
Self-employed	63.3	70.7	15.3	53.1	61.3	11.5
Regularly employed	20.1	20.7	50.8	22.3	21.9	45.3
Unemployed or in ALMP	8.8	4.4	17.2	17.0	12.2	26.0
Others	7.7	4.2	16.7	7.5	4.6	17.2

Note: All results are percentages.

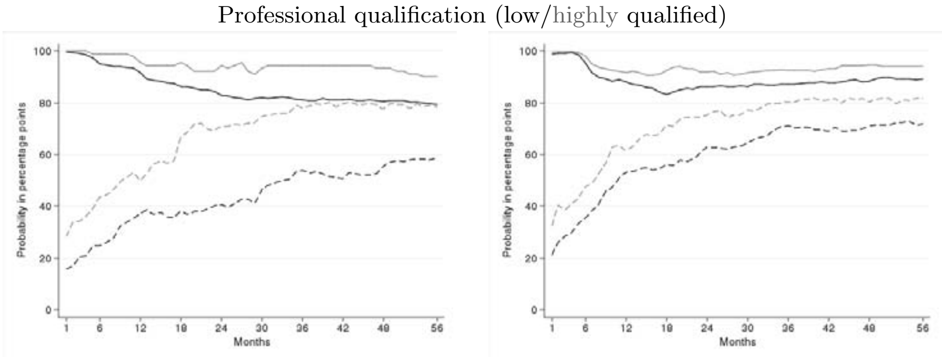
Figure A.2: Effect heterogeneity: Probability levels among participants and nonparticipants

SUS vs. NP　　　　BA vs. NP

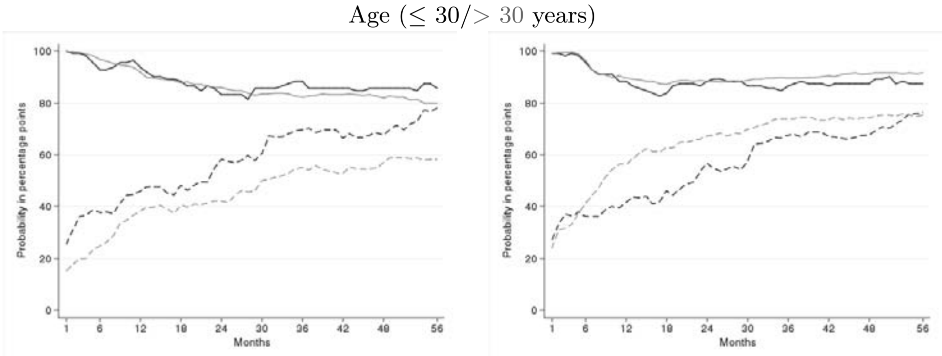
Educational level (low/highly educated)



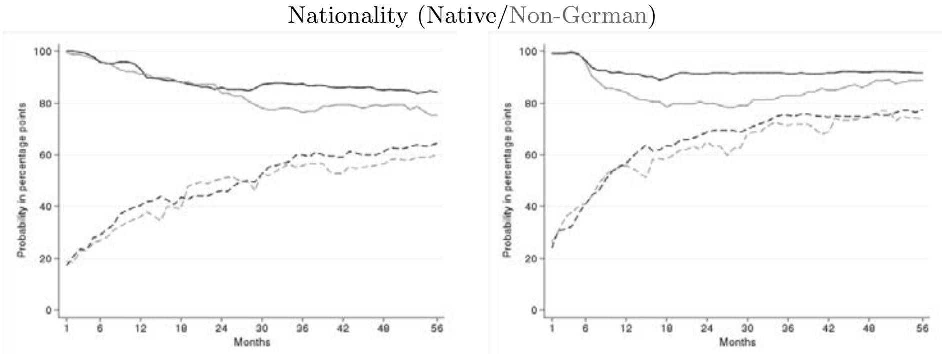
Professional qualification (low/highly qualified)



Age (≤ 30/> 30 years)



Nationality (Native/Non-German)



—— Treated　Controls - - -

Note: Outcome variable: "Self-employment or regular employment".