The ambiguous effects of public assistance to youth and female start-ups between job creation and entrepreneurship enhancement

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JEL codes: J68; L26; R50

Keywords: entrepreneurship, self-employment, program evaluation

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Abstract

Public support to start-ups often has the dual ambition of fostering self-employment of disadvantaged individuals while nurturing entrepreneurship. In this paper we evaluate a female and youth start-up program recently implemented in Tuscany (Italy), which provides public guarantees and subsidized interest rates to new firms. Under the assumption of strong ignorability of the assignment mechanism, we use a propensity score matching approach to draw inference on causal effects of the program on firms' survival and job creation. Results suggest that public support in this area may have rather ambiguous effects. It helps females and young people escape unemployment or inactivity, and may lead to further job creation. Unfortunately, all this occurs at the price of committing public resources towards entrepreneurial projects that hardly gain efficiency over time.

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1 Introduction

In several regions and countries, start-up programs lie at the intersection of the labor and the enterprise policy packages. These programs often target groups of individuals that are vulnerable in the labor market, such as females and young adults, and promote their self-employment. The new firms might also create new job opportunities for non-entrepreneurs. At the same time, start-up programs are usually described as interventions that should stimulate and enhance the latent entrepreneurial potential of a given area and, as such, may fall under the enterprise, or regional development, policy label (Román et al., 2013). The way in which these programs are viewed strongly influences any judgment regarding their effectiveness. The vast majority of available evaluations, in particular those from German-speaking countries, look at start-up programs as part of the labor activation policy toolkit and report positive effects in terms of self-employment and, to a lesser extent, job creation, which is usually viewed as a success (Caliendo, 2016; Dvouletý et al., 2016). Much less is known about the other face of these programs, namely their ability to promote a self-sustainable entrepreneurship. The literature provides little evidence on this ability, and the existing evidence suggests that it cannot be taken for granted (e.g., Battistin et al., 2001; Caliendo et al., 2015). This paper adds to both strands of the evaluation literature on start-up programs and investigates whether the self employment, the job creation and the entrepreneurship promotion goals go hand in hand. In addition, as Italian studies regarding this topic are only few to date, our work also adds fresh evidence on start-up programs implemented in Italy. Our goal is to assess causal effects of a regional program recently implemented in Tuscany (Italy), aimed at stimulating investments in youth and female start-ups through small bank loans assisted by public guarantees and subsidized interest rates. To this end we use a semi-parametric matching approach, in combination with survival analysis techniques under the assumption of strong ignorability of the program assignment mechanism (Rosenbaum and Rubin, 1983). Results show that the policy is successful, at least on a temporary basis, in promoting self-employment and, to a lesser extent, further job creation. Unfortunately, they also suggest that the policy is not fully able to promote efficiency and self-sustainability in new firms.

The paper proceeds as follows. Section 2 recalls the rationale for public intervention in support of start-ups and reviews the existing literature on the causal effects of these programs. Section 3 presents the program under investigation and the data used. Section 4 describes the statistical approach taken and defines the causal effects of interest. Section 5 illustrates and discusses results. Section 6 concludes the paper.

2 Rationale for public intervention and previous results

In an enterprise policy perspective, the provision of public support to start-ups is generally justified by the existence of credit or other finance-related constraints affecting this type of firms. As recalled by Peneder (2008), positive lending decisions of banks or of other investors are usually hampered by asymmetric information regarding the quality of the entrepreneurial project and its prospects, whatever the type of applicant firm. There are, however, additional burdens for start-ups, which cannot inherit from the past any self-financing capacity and typically face considerable failure probability. First, they may find it hard to borrow capital because of their lack of reputation and their difficulty to provide collaterals. Second, both the effort that lenders are called to make to assess the merit of a new, small entrepreneurial project and the related transaction costs may be high compared to the required volume of finance, which may act as a disincentive to lending. In order to overcome such barriers, governments may provide start-ups with a range of supports, including subsidies, direct loans or partial credit guarantees.

In an active labor market policy perspective, public intervention in favor of start-ups is justified by some additional arguments. As discussed in Caliendo et al. (2015), the barriers recalled earlier may higher if the aspiring entrepreneur is an unemployed person or someone whose work is undervalued in paid employment, such as females or youths, rather than a “regular” entrepreneur. These individuals may start up a business out of necessity, in the attempt to escape unemployment or also, mostly in the case of females, to achieve a satisfactory work-life balance. Relative to other nascent entrepreneurs, these individuals are more likely to lack personal and family financial means to set up and expand the business and less likely to obtain credit from banks. Often, they are also endowed with moderate self-confidence and insufficient experience, business knowledge, contacts and social networks, which may call for supplementary public supports such as coaching and mentoring (Pfeffer and Reize, 2000). The aid is expected here to compensate for the additional disadvantages in terms of finance from which these individuals may suffer, allowing a friendlier, yet temporary, environment where they can develop their business idea, the human capital and the networks required to survive.

Depending on how they are looked at, and by whom, start-up programs for females, youths and unemployed may be object of opposite appraisals. On the one hand, the idea that individuals with doubtable entrepreneurial talent may receive public support to set up new firms is obviously criticized by the entrepreneurship literature. It is claimed that governments

should refrain from supporting firms that, already at first sight, exhibit low potential (Shane, 2009) – also acknowledging that the picking-the-winner mantra that is so popular in the industrial policy literature (e.g. Pack and Saggi, 2006) may be hard to translate into practice. On the other hand, start-up programs are viewed as a promising option by labor economists, as the results of more traditional active labor market programs on these individuals' employability are not always excellent (e.g. Card et al., 2010; Caliendo and Künn, 2015; Caliendo and Schmidl, 2016).

Compared to other types of labor or enterprise schemes, empirical evidence regarding the causal effects of start-up programs for youths, females and the unemployed is scarce. In these studies, causal effects are often estimated using semi-parametric propensity-score-matching techniques under unconfoundedness assumptions on the assignment mechanism (Rosenbaum and Rubin, 1983; Imbens, 2004).

As shown in two recent surveys of the international literature (Caliendo, 2016; Dvouletý et al., 2016), firms' survival is the main outcome considered in existing studies, as it mirrors the length of self-employment. Some contributions also check if these programs lead to a "double dividend", by ensuring further job creation or, more rarely, innovation. Finally, a number studies investigate other aspects such as, for example: whether aided self-employment guarantees a higher personal income than other job-search channels (e.g., Andersson and Wadensjö, 2007; Caliendo, 2009; Almeida and Galasso, 2010; Caliendo and Künn, 2011, 2014, 2015); whether start-up programs raise the chances of reintegration into the labor market after the business fails (Baumgartner and Caliendo, 2008, Wolff et al., 2016); whether they facilitate the work-life balance of women and, in turn, foster fertility (Caliendo and Künn, 2015); or ; whether they improve occupational satisfaction (Caliendo and Künn, 2011).

The overall results of this empirical literature do give cause for a certain optimism, as they suggest that start-up programs may constitute an effective social policy (Caliendo, 2016; Dvouletý et al., 2016), which turns out to be particularly appropriate with disadvantaged groups, such as females or young people (Caliendo and Künn, 2011; 2015).With respect to firm survival and self-employment length, positive effects are found in most studies. There is also some favorable evidence regarding start-up programs' ability to promote occupational satisfaction and reintegration into paid employment in case the business fails, while mixed conclusions are reached regarding their effects on personal income and women's fertility outcomes. As to the issue whether start-up programs, in addition to self-employment prospects, ensure a double-dividend in terms of job creation, evidence is still controversial, as the positive results found in several descriptive studies (Caliendo and Kritikos, 2010;

Caliendo, 2016) are not confirmed when a causal approach is taken, as suggested by Pfeffer and Reize (2000). Apart from the latter contribution, this issue remains unexplored in causal studies and, therefore, is investigated in this article.

As tools for promoting entrepreneurship, start-up programs give less cause for optimism. The small set of causal studies that take this approach focus on either German or Italian programs. In a recent work, Caliendo et al. (2015) compare over a medium-term horizon survival and business outcomes of German subsidized start-ups out of unemployment to those of similar firms having regular founders. They find that subsidized start-ups lag behind regular ones in terms of business growth and innovation, although the subsidy guarantees longer survival prospects. Earlier Italian studies suggest to pay attention not only to differences in survival rates or functions but also to differences in cumulative or instantaneous hazards, as they may reveal a missing part of the story. For example, Battistin et al. (2001) analyze a program providing youth start-ups in Southern Italy with subsidies and some managerial coaching. If their survival function is compared to that of similar start-ups from regular founders, these firms enjoy longer survival prospects. However, the inspection of the pattern of the instantaneous hazard functions reveals that it is only a temporary advantage that depends on the availability of the subsidy, rather than being grounded on serious efficiency gains induced by the subsidy itself and by the related coaching. In the same vein, Mealli and Pagni (2001) analyze the effects of a subsidy (combined with and interest subsidy in order to ease further bank borrowing) provided to youth start-ups in Tuscany through a program that is a predecessor of the one investigated in this study. They also find that the program guarantees longer survival prospects but argue that the non-concave shape of the cumulative hazard function suggests that the overall efficiency of subsidized firms might be poor. In contrast to this evidence, but remaining within the Italian boundaries, Pellegrini and Muccigrosso (2016) find that the subsidization of start-ups through one of the major national programs for firm investments (Law 488/1992) leads to persistently lower cessation risk. This result supports the idea that projects endowed with higher potential are more attracted by (and selected into) regular industrial schemes that offer aids of considerable size, whereas less selective start-up support schemes for disadvantaged social groups are likely to attract entrepreneurial projects with lower potential.

3 The program and the data

We analyze data from a start-up program, named "Fare impresa" (Doing business) implemented in Tuscany from 2011, which supports youth and female businesses in their

early stages, with reference to a wide range of economic activities in the manufacturing, trade and tourism sectors. Supports are granted to both newly established companies (less than 2 years old at the time of the application or firms that will be established within 6 months from the receipt of support), as well as to expanding enterprises that were 2-5 years old at the time of application. Applicants may be youth aged 18-40 and, with no age limit, females and subsidized unemployed.

From 2011 to 2015, the program provided new firms with a public guarantee aimed at easing the receipt of bank loans for the realization of investments, combined with an interest subsidy.[1] Financial operations could have a duration of 16-120 months, with the guarantee covering up to 80% of the loan requested to the bank. Implementation mainly occurred through a specialized financial intermediary owned by the regional government. The intermediary was entrusted by the government itself with the screening of applications and the decision about the guarantee to the advantage of creditworthy firms, but also with provision of assistance in writing down the investment project appropriately. Firms that obtained the guarantee from the intermediary could also take advantage from an interest subsidy directly offered by the regional government, provided that their loan request was later accepted by the bank. The number of projects that received support during the first phase is 1,939. Of these, 1,656 (85.4%) later obtained the loan from the bank, with an average waiting time of 7 weeks, while 283 (14.6%) did not in spite of the positive assessment of the guarantee body. Loan denial occurred for unknown reasons. The main conjecture about these reasons is that loan policies may be different across banks. For example, the credit approval process of some banks could be based on, or attribute smaller weight to, aspects that are important for other banks or for the guarantee body, such as soft information and relationship lending. Furthermore, large banks that operate nationwide are usually less interested in serving small borrowers and in contributing to local economic development than regional or mutual banks (Alessandrini et al., 2009).Therefore, the loan request from a small borrower might have higher probability of success if the latter is client of a local bank.

To perform our empirical analysis we combine three main distinct data sources. The first of these is represented by the administrative archive of the companies participating in the program, held by the regional government and by its financial intermediary (named Fidi

[1] From 2015 onwards, the program provides microcredit without passing through banks, combined with a consultancy voucher. Our analysis does not consider this very recent season of the program.

Toscana). This archive includes information on main features of the firms to which the guarantee and the right to the interest subsidy were assigned, such as: date of establishment; business sector; legal form; location of the investment project; the indication of whether the firm is founded by females or a youths; and of whether it is a newly established or an expanding start-up. In addition, the administrative archive reports details on the type of investment the company intends to carry out; the dates on which the guarantee is requested to and finally granted by the intermediary; the name of the bank to which the loan request is submitted; and the date on which the company eventually obtains the loan from the bank (if any).

The second data source, required to establish how long each start-up remains alive, is the Business Register maintained by the Chambers of Commerce. All Italian firms are obliged to register here the date of occurrence of a number of important events related to their business life, including cessation. The third data source is represented by the regional Job Information System (Sistema Informativo Lavoro), which reports all the communications that firms are required to make to public Employment Services every time they hire an employee (as well as when an employee resigns or is dismissed). Firms are also required to report a series of characteristics of the employment relationship, such as: the type of contract; its expected duration; and so forth. This data source enables us to investigate whether and to what extent start-ups that participate in the program create jobs apart from the entrepreneurs' self-employment opportunity.

Out of 1,939 projects that obtained the guarantee, 102 were ascribable to neither youth nor female entrepreneurs. These projects are probably attributable to elderly unemployed males but, as the authorities managing the program were not able to confirm this circumstance, these few firms were excluded from our analysis. Table 1 reports descriptive statistics regarding the remaining 1,837 firms with the guarantee (G = 1). The probability that a youth or female start-up obtains the bank loan once the public guarantee is available amounts to 85,1% (1,563 firms receive the loan). Loans are requested both to local/mutual and to larger banks that operate nationwide. Youth start-ups account for 76% and female start-ups for 57% of the population. There is an overlap between the two types of firms, in that some start-ups are established by females aged 18-40 (611 firms, of which 526 receive the loan). Newly-established firms largely exceed expansions. Most firms assume the legal form of a sole proprietorship and belong to the service sector. Table 1 also makes a distinction between the group of firms that is later accorded the loan from the bank (L = 1) and the group of firms that is not (L = 0). Firm characteristics do not differ much across the two groups.

The presence of two groups of similar firms that, according to the evaluation performed by the intermediary, both deserve the guarantee but may ultimately receive or not receive the loan from the bank provides us with the opportunity to carry out a comparison between the two groups that, in principle, should not suffer too much from selection bias. Obviously, a direct, unadjusted comparison of the outcomes observed in the two groups is insufficient to uncover causal effects in our observational setting. In what follows, the loan eventually obtained by the bank, accompanied by the interest subsidy, will be also referred to as treatment.

4 Empirical strategy and research questions

Similarly to most of the previous program evaluation literature regarding start-up programs we invoke the stable-unit- treatment-value assumption (SUTVA) and unconfoundedness to identify causal effects. We then use propensity-score-matching techniques to estimate them. We view our problem in the light of the potential-outcomes framework put forward by Rubin (Imbens and Rubin, 2015). SUTVA rules out hidden versions of treatments and interference between units (firms), that is, it posits that the potential outcomes of one firm are unaffected by the specific treatment assigned to the other units. Given the small size of the program and of the related loans, it may be hard to envision relevant spillover or displacement effects, which makes non-interference a plausible hypothesis. Under this assumption, for each unit we can define two potential outcomes for each outcome variable, Y; they are the value of Y if the firm receives a loan from the bank, $Y_i(1)$, and the value of Y if the firm does not receive a loan from the bank, $Y_i(0)$. The effect of the treatment (loan) is defined, for each firm i , as the difference between the firm's two potential outcomes, $Y_i(1)-Y_i(0)$. In this paper we focus on treatment effects for the subpopulation of the treated firms.

Inspired by the literature recalled in Section 2, the main outcomes we focus on are firm survival, which constitutes a proxy for the length of self-employment, and the related hazard of cessation, which instead allows an assessment of the contribution of the program to the creation of entrepreneurial capacity. Formally, let $\tilde{Y}_i(l)$ denote the survival time for firm i after the guarantee is obtained, given assignment to treatment l, l=0,1. In a given time point t, the causal effect of the treatment on survival and on the hazard of cessation for the treated firms are, respectively, defined as follows:

$$ATT_S(t) = S_{1|L=1}(t) - S_{0|L=1}(t) = Pr(\tilde{Y}_i(1)>t \mid L=1) - Pr(\tilde{Y}_i(0)>t \mid L=1)$$

$$ATT_h(t) = h_{1|L=1}(t) - h_{0|L=1}(t) =$$

$$[lim_{\Delta t\to 0}\, P(t < \tilde{Y}_i(1) \leq t+\Delta t \mid \tilde{Y}_i(1)>t, L=1)/\Delta t] - [lim_{\Delta t\to 0}\, P(t<\tilde{Y}_i(0)\leq t+\Delta t \mid \tilde{Y}_i(0)>t, L=1)/\Delta t]$$

Note that the survival $\tilde{Y}_i(l)$ might be censored if firm i is still alive at the end of the study when exposed to treatment l. Let $C_i(l)$ be the time to censoring for firm i given assignment to treatment l. Henceforth, we assume that the time to censoring $C_i(l)$ is conditionally independent of $Y_i(l)$ given the covariates for each i=1,…,N and l=0,1. Let $Y_i(l)=min(\tilde{Y}_i(l), C_i(l))$ denote the time to death or censoring for firm i given assignment to treatment l, l=0,1.
From a "double dividend" perspective, another outcome of interest is the number of contracts that a new firm activates. Therefore we are also interested in the average causal effect for the treated firms on the number of opened job positions. Let $Y_i(l)$ now denote the number of job positions opened by firm i after the guarantee, given assignment to treatment l, l=0,1. The causal effect of interest is defined as follows:

$$ATT_J = E[Y_i(1)- Y_i(0) \mid L=1].$$

We assess this effect at three different time points: within 12 months, between 12 and 24 months, and between 24 and 36 months after the guarantee.
Unfortunately for each unit only one of the two potential outcomes is observed for each response variable, namely, the potential outcome associated with the treatment actually received. Therefore in order to identify and estimate the causal effects of interest, we need to introduce some assumption on the treatment assignment mechanism. In particular, we assume that the assignment mechanism is *strongly ignorable*. This assumption has two components:

(i) *Unconfoundedness*: $Y_i(0),Y_i(1) \perp L_i \mid \boldsymbol{X}_i$, where $\boldsymbol{X}_i$ is a vector of pre-treatment covariates observed for each firm i, i.e. treatment assignment is independent of the potential outcomes conditional on the observed pre-treatment covariates. In other words, we assume that loan receipt occurs at random within the cells defined by the pre-treatment covariates;
(ii) *Overlap*: $0 < Pr(L_i=1|\ \boldsymbol{X}_i=\boldsymbol{x}) < 1$, i.e. that there is no observable characteristic that determines treatment conditions, which leaves room for *coeteris paribus* comparisons.

The plausibility of unconfoundedness heavily relies on the quality and on the amount of the information contained in the vector $\boldsymbol{X}$. However, as this information is inevitably constrained

by the available data, we will assess our identification assumption through a sensitivity analysis (e.g., Rosenbaum, 2002; Imbens and Wooldridge, 2009; Ichino et al., 2008).

If unconfoundedness enables the point identification of unknown counterfactual quantities as a function of the observed data alone, the way in which these unknown quantities can then be estimated is another matter. To this end, we resort to propensity-score-matching techniques, in accordance with most of the previous literature on start-up programs. It is well known that a propensity score is an univariate summary of the information contained in the vector of pre-treatment covariates, which is defined as $e_i = e(X_i)=P(L_i =1|X_i)$. This summary has two fundamental properties (Rosenbaum and Rubin, 1983): i) it is a balancing score, in the sense that it guarantees – at least with sufficiently large samples – that observations with the same value of the propensity score have the same distribution of observable characteristics independently of the treatment; ii) if treatment assignment is strongly ignorable given $\boldsymbol{X}_i$, then it is also strongly ignorable given the propensity score. The two properties together enable us to use this univariate summary instead of the covariates to match similar firms from different treatment groups. Despite its many advantages, the specification and estimation of a propensity score may be easier said than done (e.g., Smith and Todd, 2005). To address this issue, Imai and Ratkovic (2014) put forward a generalized-method-of-moments estimator of the propensity score where a single model determines both the conditional probability of treatment assignment and optimized covariate balancing. We employ this powerful covariate-balancing propensity score (CBPS) in our study. Using logit models, we estimate two distinct CBPSs for the groups of youth and female start-ups, then use them to match each treated youth or female firm (L = 1) to its nearest untreated neighbor. As the number of potential control firms that do not receive the loan (L = 0) is relatively low, matching occurs with replacement within each group. The pre-treatment covariates included in the propensity-score models are the following (see also Table 1): a dummy for the legal form of the start-up (1 = sole-proprietorship, 0 = otherwise); its sector of affiliation (1 = manufacturing, 2 = trade, 3 = hotel/restaurant, 4 = travel agency/rental; 5 = entertainment/recreation; 6 = hairdresser/beauty parlor; 7 = other sector); a dummy for youth firms (= 1 in the CBPS related to female start-ups) or a dummy for female firms (= 1 in the CBPS related to youth start-ups); a dummy for brand new start-ups (= 1; 0 = expanding start-up); the number of employees already hired before the guarantee was obtained, per type of contract. In order to account for the idea, recalled in earlier sections, that banks might have different attitudes towards small projects and local development, we also include a dummy for loans requested to a local or mutual bank. Finally, we characterize the local labor system (equivalent to a

travel-to-work area) the firm belongs to in terms of: unemployment rate in 2011 (source: ISTAT –National Institute of Statistics); size of local demand, through a dummy for areas having a resident population of at least 60,000 in 2011 (source: ISTAT); the alternatives that the area where the firm is located offers to firms wishing a loan in terms of different banks that can be approached, summarized by a Gini index of corporate concentration of local bank branches in 2011 (source: ISTAT – Statistical register of local units). The results of the CBPS estimation stage are reported in the Appendix A. The estimated CBPSs also guarantee that there is overlap in the distributions of the covariates of treated and control firms in the subsamples of youth and female firms (Appendix B).

The causal effects of interest are estimated using nearest-neighbor estimators, whereas the survival functions are estimated using the Kaplan-Meier estimator.

It is worth noting that we need to account for the fact that firms die over time when we estimate causal effects on the number of opened job positions in post-treatment years (e.g., Robins et al., 2000; Wooldridge, 2007). Under the assumption that there are no unmeasured confounders for both treatment and loss to follows-up due to death, we apply the nearest neighbor estimator to outcomes weighed by the inverse of probability of surviving. Specifically, let $C_{i,s}$ be a binary indicator equal to 1 if firm i dies in the year s, s=1,2,3 and let $Y_{i,s}$ be the observed number of job positions opened by firm i during the s^{th} year, s=1,2,3. We have $w_{i,s=1}$ =1 for all i, because all firms participating in the study are alive for at least one day, and thus can hire new employees in the first year,

$$w_{i,2}=Pr(C_{i,1} = 0 \mid \boldsymbol{X}_i, L_i)/Pr(C_{i,1} = 0 \mid \boldsymbol{X}_i, L_i,\boldsymbol{Y}_{i,1}),$$

and

$$w_{i,3}=\{Pr(C_{i,1}=0|\boldsymbol{X}_i, L_i)Pr(C_{i,2}=0|C_{i,1}=0, \boldsymbol{X}_i, L_i)\}/\{Pr(C_{i,1}=0|\boldsymbol{X}_i, L_i,\boldsymbol{Y}_{i,1})Pr(C_{i,2}=0|C_{i,1}=0, \boldsymbol{X}_i, L_i, \boldsymbol{Y}_{i,1}, \boldsymbol{Y}_{i,2})\}.$$

Estimates of all the probabilities contained in the two previous are obtained using logit models.[2]

Because of data constraints, our vector of pre-treatment covariates includes little information on the characteristics of the entrepreneurs, preventing us to control for aspects that –

[2] The mean of both $w_{i,2}$ and $w_{i,3}$ amounts to one. The minimum value of $w_{i,2}$ is 0.777, its maximum value is 2.551. The minimum value of $w_{i,3}$ is 0.594, its maximum value is 2.328.

according to labor economists – might deserve some attention, such as exact age, educational level attained, parental self-employment and the individual labor or self-employment history. The lack of information regarding these aspects calls for an assessment of the plausibility of results obtained under the assumption of unconfoundedness, which will be done using the approach proposed by Ichino et al. (2008).

Table 1 – Descriptive statistics on youth and female start-ups participating in the program

	G =1		G=1, L=1		G=1, L=0	
	Mean	*S.D.*	*Mean*	*S.D.*	*Mean*	*S.D.*
Youth start-up (1/0)	0.762	0.426	0.760	0.427	0.770	0.422
Female start-up (1/0)	0.571	0.495	0.576	0.494	0.540	0.499
Newly established firm (1/0)	0.921	0.270	0.925	0.264	0.901	0.299
Sole proprietorship (1/0)	0.606	0.489	0.597	0.491	0.657	0.476
Firm activity (categorical):						
manufacturing	0.111	0.314	0.107	0.310	0.131	0.338
trade	0.327	0.469	0.332	0.471	0.296	0.457
hotel/restaurants	0.276	0.447	0.285	0.451	0.226	0.419
travel agency/rental	0.035	0.183	0.032	0.176	0.051	0.221
entertainment/recreation	0.024	0.155	0.024	0.152	0.029	0.169
hairdresser/beauty parlor	0.122	0.328	0.130	0.336	0.080	0.272
other	0.105	0.306	0.090	0.287	0.186	0.390
No. of employees hired prior to the guarantee with:						
permanent contract	0.624	2.596	0.626	2.663	0.617	2.173
fixed-term contract up to 2 months	0.147	1.034	0.153	1.084	0.113	0.678
fixed-term contract 2-5 months	0.206	1.162	0.208	1.190	0.193	0.988
fixed-term contract 5-12 months	0.237	1.143	0.242	1.109	0.208	1.325
fixed-term contract 12+ months	0.292	4.472	0.310	4.819	0.193	1.273
Loan is requested to a local/mutual bank (1/0)	0.531	0.499	0.553	0.497	0.401	0.617
Firm is located in a LLS* with 60,000+ inhabitants (1/0)	0.198	0.399	0.198	0.399	0.197	0.399
Corporate concentration of bank branches in the firm's LLS*	0.809	0.157	0.809	0.157	0.813	0.113
Unemployment rate in the firm's LLS	0.085	0.016	0.085	0.016	0.087	0.193
Loan amount (Euros)			59,217	1,278		
Guarantee amount (Euros)	46,699	923	47,239	1,014	43,763	2,213
Guarantee amount / Loan amount			0.799	0.001		
N	1,837		1,563		274	
$Pr(L = 1 \mid G = 1)$	0.851					

* Local labor system

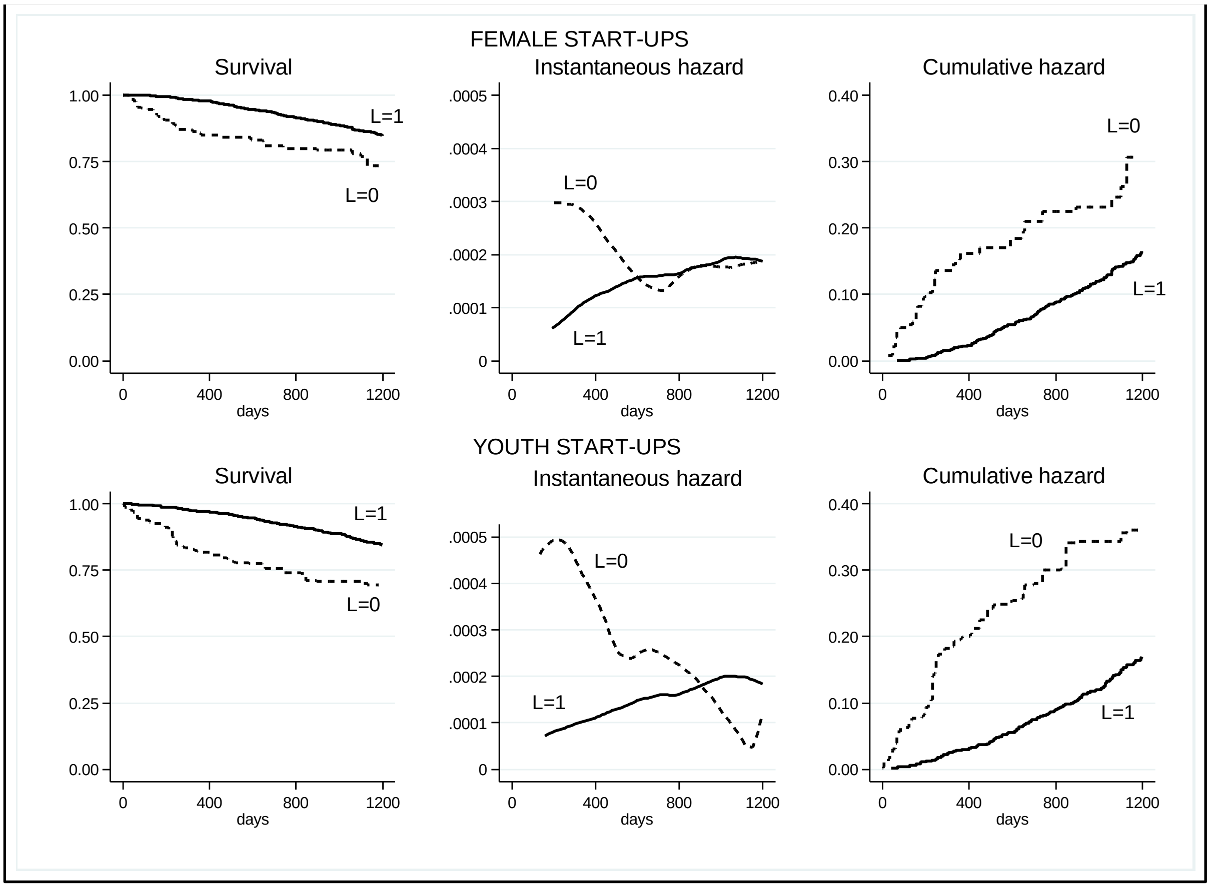
5 Results

5.1 Effects on firm survival

Figure 1 displays the survival, cumulative and instantaneous hazard functions that were estimated for treated start-ups under both the treatment and the counterfactual scenarios. These graphs provide a clear intuition of the treatment effect estimates we report right after.

For treated firms, the survival functions under treatment are always above those that would have been obtained if those firms had received no loan, which suggests that the program helped survival of female and youth start-ups. A similar conclusion can be reached based on cumulative hazards.

Figure 1 – Survival, Instantaneous hazard and Cumulative hazard functions of treated firms (L = 1) and in the counterfactual scenario (L = 0)

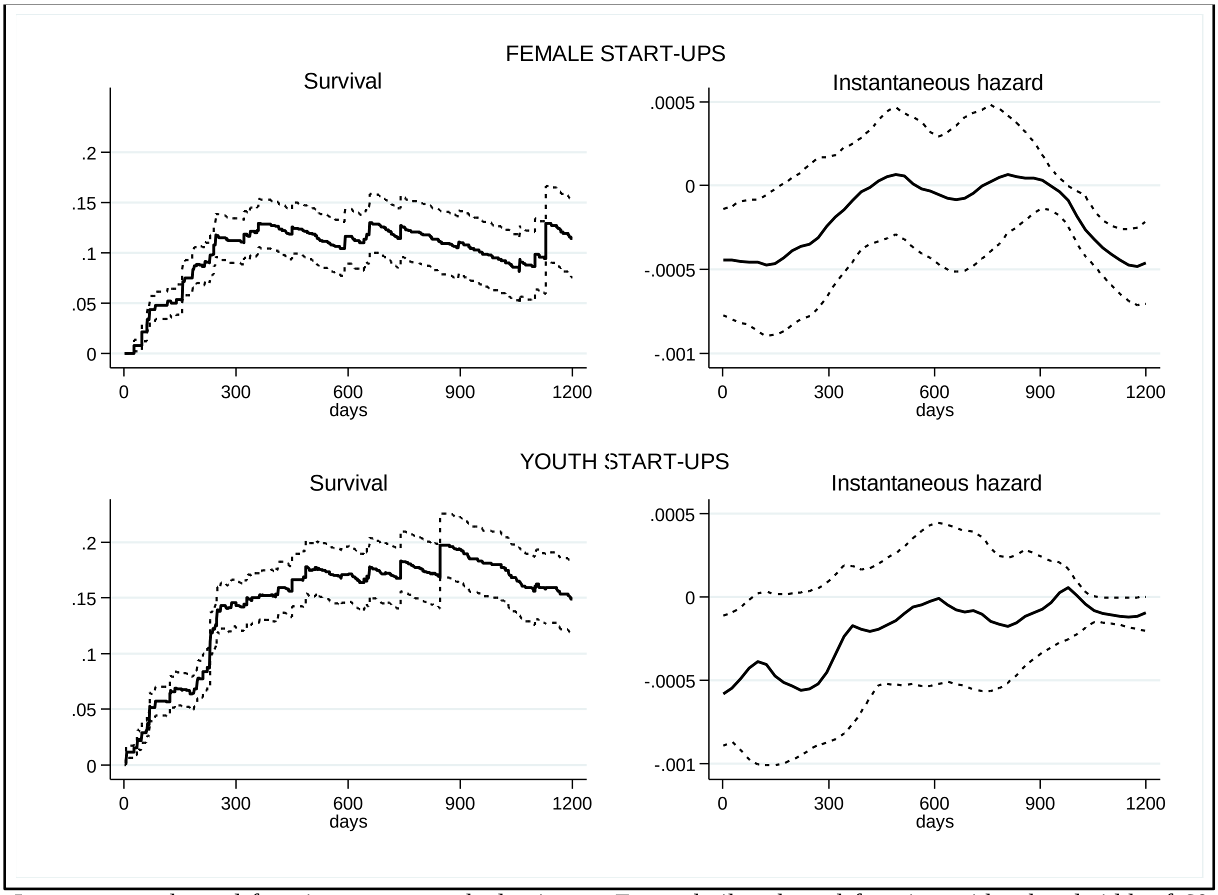


Instantaneous hazard functions are smoothed using an Epanechnikov kernel function with a bandwidth of 60 days. Number of treated observations: 901 female start-ups; 1,188 youth start-ups. Notice that 526 treated start-ups fall under both the female and the youth category.

However, a closer look at the cumulative hazard functions of treated firms reveals that they are not concave, which implies that risk of cessation is increasing in time, rather than decreasing as one would expect if – following the predictions of economic theory – surviving

firms were more efficient than those that die. This is mirrored by the instantaneous hazard functions of treated firms (the derivatives of the cumulative hazard functions) that grow almost throughout the observation period, reaching the level of risk they would have faced without the guaranteed loan quite soon.

Figure 2 – Causal effects on survival, $ATT_S(t)$, and instantaneous hazard, $ATT_h(t)$, accompanied by their 95% confidence intervals



Instantaneous hazard functions are smoothed using an Epanechnikov kernel function with a bandwidth of 60 days. Number of treated observations: 901 female start-ups; 1,188 youth start-ups. Notice that 526 treated start-ups fall under both the female and the youth category.

Figure 2 shows the estimates of the average treatment effects on the treated, ATT_S and ATT_h, along with their 95% point-wise confidence intervals (De Luna and Johansson, 2009). The causal effect of the treatment on the probability of survival is positive for both female and youth start-ups. It arises immediately after the receipt of the guaranteed loan, and then it remains rather stable. The idea is that treatment improves the duration of self-employment by guaranteeing an advantage that is likely to be due to the immediate availability of money, rather than to processes that might take place over a longer time horizon. In fact, also the causal effect of the treatment on instantaneous hazard of cessation is negative for a short

while only and vanishes much earlier than the loan repayment deadline of 16-120 months. In line with previous findings, our start-up program has ambiguous effects, in that it does help females and young people escape unemployment or inactivity, but this occurs at the price of promoting entrepreneurial projects that hardly gain efficiency. In sum, any judgment on these programs inevitably depends on which goal, between entrepreneurship and employment, occupies a higher rank in the economic policy agenda.

Causal effects were estimated under the assumption of unconfoundedness, which implies that the observable pre-treatment covariates are sufficient to enable the identification of causal effects. This assumption is not testable but it might be worthwhile to perform sensitivity analysis to assess whether modest departures from it can change the results substantially. In this work we conduct a sensitivity analysis using the Monte Carlo simulation-based approach proposed by Ichino et al. (2008), using a binary outcome, Y, equal to 1 if a firm does not die during the observation period and 0 otherwise. This approach starts by envisioning a situation where the unconfoundedness assumption does not hold conditional on the observed covariates alone, because there is also a relevant unobserved binary confounder U that should be accounted for: $Y_i(0), Y_i(1) \perp L_i \mid \boldsymbol{X}_i, U_i$.[3]

Under the assumption that the binary unobserved confounder U is independent of the observed covariates conditional on Y_i and L_i, the distribution of U is fully characterized by the choice of four parameters: $p_{ly} = Pr(U_i=1 \mid L_i=l, Y_i=y), l=0,1; y=0,1$.

Given arbitrary (but meaningful) values of the parameters p_{ly}, a value of U is simulated for each firm according to its observed values of the treatment status and the outcome. Then U is treated as any other observed covariate and, in particular, we include U in the set of matching variables in the CBPS function used to estimate the propensity score and to estimate the ATT by nearest-neighbor matching. Using a given set of values of the sensitivity parameters, we repeat the matching estimation of the ATT 500 times, then we obtain an estimate of the final ATT by averaging all the ATTs over the distribution of the simulated U. The sensitivity parameters p_{ly} are chosen by fixing $Pr(U_i=1)$=0.6 and $Pr(U_i=1 \mid L_i=1, Y_i=1) - Pr(U_i=0 \mid L_i=1, Y_i=0)$=0 and by varying the parameters $d = Pr(U_i=1 \mid L_i=0, Y_i=1) - Pr(U_i=0 \mid L_i=0, Y_i=0)$, and $s = Pr(U_i=1 \mid L_i=1) - Pr(U_i=0 \mid L_i=0)$, which, respectively, measure the marginal (w.r.t. the covariates) association between U and Y in the absence of treatment and between L and U. In other words, d and s capture the outcome effect of U in the absence of treatment and the effect of U on the selection into treatment, respectively.

[3] Examples of possible confounders that cannot be observed in the available data were provided in Section 4.

Table 2 – Causal effects on the probability of survival and their sensitivity to the presence of an unobserved confounder U

	FEMALE START-UPS				YOUTH START-UPS			
No *U*	0.135^{***} (0.050)				0.215^{***} (0.046)			
Neutral *U* ($d = 0; s = 0$)	0.154^{**} (0.060)				0.170^{***} (0.053)			
Search for a killer *U*	$s = 0.1$	$s = 0.2$	$s = 0.3$	$s = 0.4$	$s = 0.1$	$s = 0.2$	$s = 0.3$	$s = 0.4$
$d = 0.1$	0.158^{**}	0.150^{**}	0.138^{*}	0.128	0.169^{***}	0.165^{***}	0.158^{**}	0.144^{*}
	(0.063)	(0.067)	(0.075)	(0.085)	(0.055)	(0.060)	(0.066)	(0.076)
$d = 0.2$	0.147^{**}	0.131^{**}	0.113	0.079	0.161^{***}	0.147^{**}	0.131^{**}	0.103
	(0.061)	(0.064)	(0.070)	(0.080)	(0.054)	(0.058)	(0.063)	(0.071)
$d = 0.3$	0.141^{**}	0.117^{*}	0.081	0.030	0.155^{***}	0.136^{**}	0.107^{*}	0.055
	(0.061)	(0.064)	(0.068)	(0.072)	(0.055)	(0.059)	(0.062)	(0.064)
$d = 0.4$	0.130^{**}	0.102	0.052		0.145^{***}	0.121^{**}	0.078	0.010
	(0.061)	(0.063)	(0.064)		(0.055)	(0.057)	(0.058)	(0.055)

Standard errors in parentheses. Significance legenda: * p<0.10; ** p<0.05; *** p<0.01.

As reported in Table 2, in the absence of any unobserved confounder *U*, the guaranteed loan leads to an increase of survival probability both for female (13.5%) and youth start-ups (21.5%), which is in line with the results presented earlier in this Section.

If we posit a neutral *U* such that $d = s = 0$, results change a little but still remain positive and highly significant. Now, how big should *d* and *s* be in order to "kill" these positive results? Table 2 shows that treatment effects almost always decrease as we increase the strength of (and control for) the unobserved confounder *U*. However, we may notice that, in order to find insignificant treatment effects, the strength of *U* has to be considerably high, both for female and youth start-ups. Since it may be hard, in our application, to envision so much influential unobserved confounders, we may conclude that our results are robust to reasonable failures of the unconfoundedness assumption that made their identification possible.

5.2 Effects on further job creation

Table 3 reports the causal effects of the loan on the number of job positions opened after the guarantee was obtained. We find some evidence that the program helps job creation in the short run for both female and youth firms, although the estimated effects are of moderate size. Specifically, for female start-ups we find positive effects of the program on the number of permanent positions opened in the first two years and on the number of temporary positions opened in the first year. Youth start-ups take more time to hire new employees: we

find a non-negligible positive effect of the program on the number of permanent positions only in the second year.

Table 3 – Effects on further job creation: ATT_J

	FEMALE START-UPS			*YOUTH START-UPS*		
Fixed-term positions opened within 12 months	0.726**	(0.287)	[901]	0.787	(0.515)	[1,188]
Fixed-term positions opened 12-24 months afterwards	-0.0696	(0.412)	[882]	0.453	(0.651)	[1,159]
Fixed-term positions opened 24-36 months afterwards	0.218	(0.300)	[834]	0.540	(1.076)	[1,099]
Permanent positions opened within 12 months	0.515**	(0.253)	[901]	0.215	(0.156)	[1,188]
Permanent positions opened 12-24 months afterwards	0.350**	(0.154)	[882]	0.372*	(0.213)	[1,159]
Permanent positions opened 24-36 months afterwards	0.0719	(0.167)	[834]	0.129	(0.135)	[1,099]

Standard errors in parentheses, Number of treated observations in brackets. Significance legenda: * p<0.10; ** p<0.05; *** p<0.01.

These results support the idea that start-up programs can guarantee a double dividend: not only they may improve self-employment prospects of the entrepreneurs(s), as suggested in Section 5.1, but also they may lead to further job creation.

6. Concluding remarks

In this study, we evaluated a female and youth start-up program recently implemented in Tuscany (Italy), providing public guarantees and subsidized interest rates to new firms to ease their access to medium- to long-term bank loans aimed at the realization of investments. To the best of our knowledge, there is only a couple of Italian studies regarding this topic to date, therefore our work adds fresh evidence on start-up programs implemented in Italy. Assuming strong ignorability, we used a matching approach, implementing the covariate balancing propensity score recently put forward by Imai and Ratkovic (2014), to estimate the causal effects of the program on firm survival, which mirrors the length of the founders' self-employment, on the firms' hazard of cessation and on further job creation. In choosing these outcome variables, we blended the approach of labor economists, which look at start-up programs as a way to fight unemployment, with that of entrepreneurship scholars, which focus on the quality of the entrepreneurial projects that receive support.

Results suggest that public support in this area may have rather ambiguous effects. On the one hand, the program helped females and young people escape unemployment or inactivity, and also led to further job creation. On the other hand, all this occurred at the price of committing public resources towards entrepreneurial projects that did not improve their efficiency and self-sustainability over time. The fact that new employment is created thanks

to the public support offered to relatively inefficient firms may be judged in opposite ways, according to the priority that is attributed by policymakers and by the public opinion to employment generation relative to entrepreneurship enhancement.

In the light of these results, we argue that there is a need to improve these programs so that start-ups receive not only financial support, but also appropriate coaching and/or mentoring.

Acknowledgements

This study builds on a previous research report commissioned to IRPET – Regional Institute for Economic Planning of Tuscany by the Presidency of Tuscany’s Regional Government.

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APPENDIX A

Table A1 – Estimated CBPS coefficients

	FEMALE START-UPS		YOUTH START-UPS	
Intercept	1.350*	(0.694)	0.887	(0.650)
Youth start-up (1/0)	0.054	(0.117)		
Female start-up (1/0)			0.147	(0.102)
Newly established firm (1/0)	0.166**	(0.075)	0.475***	(0.082)
Sole proprietorship (1/0)	-0.526***	(0.079)	-0.385***	(0.117)
Firm activity: (baseline: other sector)				
manufacturing	0.395***	(0.121)	0.564***	(0.122)
trade	0.750***	(0.167)	0.801***	(0.125)
hotel/restaurants	0.689***	(0.159)	0.860***	(0.115)
travel agency/rental	0.148	(0.123)	0.446***	(0.131)
entertainment/recreation	0.484***	(0.127)	0.322***	(0.080)
hairdresser/beauty parlor	1.230***	(0.172)	1.040***	(0.097)
No. of employees hired prior to the guarantee with:				
permanent contract	-0.048	(0.080)	-0.020	(0.301)
fixed-term contract up to 2 months	-0.012	(0.170)	0.058	(0.354)
fixed-term contract 2-5 months	0.026	(0.203)	0.002	(0.297)
fixed-term contract 5-12 months	0.246***	(0.079)	-0.030	(0.797)
fixed-term contract 12+ months	-0.035	(0.141)	0.187	(0.195)
Loan is requested to a local/mutual bank (1/0)	0.371***	(0.096)	0.547***	(0.087)
Firm is located in a LLS with 60,000+ inhab. (1/0)	0.032	(0.092)	0.066	(0.085)
Corporate concentration of bank branches in the firm's LLS	0.336***	(0.119)	-0.014	(0.129)
Unemployment rate in the firm's LLS	-6.140***	(0.107)	-4.040***	(0.120)

Standard errors in parentheses. Significance legenda: * p<0.10; ** p<0.05; *** p<0.01

APPENDIX B

Table B1 – Distribution of the estimated propensity scores for female and youth start-ups

	Mean	Std. Dev.	Min	25th perc.	50th perc.	75th perc.	Max
FEMALE START-UPS							
Treated	0.860	0.052	0.648	0.828	0.868	0.898	0.998
Untreated	0.831	0.062	0.635	0.805	0.841	0.873	0.939
YOUTH START-UPS							
Treated	0.857	0.057	0.623	0.825	0.870	0.897	0.999
Untreated	0.820	0.076	0.627	0.788	0.837	0.875	0.972