Lavoro e disoccupazione: questioni di misura e di analisi

Progetto di ricerca cofinanziato dal Ministero per l'Università
e la Ricerca Scientifica e Tecnologica - Assegnazione: 1998
Coordinatore: Ugo Trivellato

Why do subsidized firms survive longer? An evaluation of a program promoting youth entrepreneurship in Italy

Erich Battistin*, Andrea Gavosto**, Enrico Rettore*

* *Dip. di Scienze Statistiche, Univ. di Padova*
** *Ente Studi Economici - FIAT*

Working Paper n. 1 agosto 1998 (rivisto nel 1999)

Unità locali del progetto:

Dip. di Economia Politica, Univ. di Modena	(coord. Michele Lalla)
Dip. di Economia 'S. Cognetti De Martiis', Univ. di Torino	(coord. Bruno Contini)
Dip. di Statistica, Univ. di Venezia	(coord. Tommaso Di Fonzo)
Dip. di Metodi Quantitativi, Univ. di Siena	(coord. Achille Lemmi)
Dip. di Scienze Statistiche, Univ. di Padova	(coord. Ugo Trivellato)

Dip. di Scienze Statistiche
via S. Francesco 33, 35121 Padova

Why do subsidised firms survive longer? An evaluation of a program promoting youth entrepreneurship in Italy

Erich Battistin, Dip. di Scienze Statistiche, Univ. Di Padova

Andrea Gavosto, Ente Studi Economici Fiat, Torino

Enrico Rettore, Dip. di Scienze Statistiche, Univ. Di Padova

Acknowledgements:
We benefited from comments by Kenneth Carling, Andrea Ichino, Alberto Martini, Paolo Sestito, Ugo Trivellato, Guglielmo Weber and by participants at a seminar at EUI-Florence, at ESEM'98 and at the Mannheim '99 conference on active labour market policies evaluation. In particular, we thank two anonymous referees for carefully reading the draft and for providing usuful hints to improve it. The usual disclaimer applies. Address for correspondence: enrico.rettore@stat.unipd.it.

Abstract. *We evaluate an Italian programme which promotes youth entrepreneurship by issuing substantial benefits to candidate entrepreneurs selected through a screening process and by providing them with some training. Following previous informal analyses of this programme performance the outcome with respect to which we perform the evaluation is the firm survival time. We argue that what matters for the success of the programme is not how effective the subsidy is* per se *in keeping firms alive but how well selected and trained the beneficiaries are. A major implication is that the impact of the subsidy on the firms survival chances is not the parameter of interest here; rather, we need to assess whether as a result of the screening and the training process selected firms are enough good to survive on their own. After constructing a sample of spontaneous firms comparable to the subsidised ones with respect to some observable characteristics by matching and weighting we show that previous results favourable to the programme badly overstated its effectiveness. We also show that a simple criterion based on the pattern of the hazard function casts serious doubts on the programme ability to yield firms whose survival chances do not depend on the subsidy.*

Keywords: *Firms' incentives scheme, Matched comparison group, Observational study, Propensity score*

1. Introduction

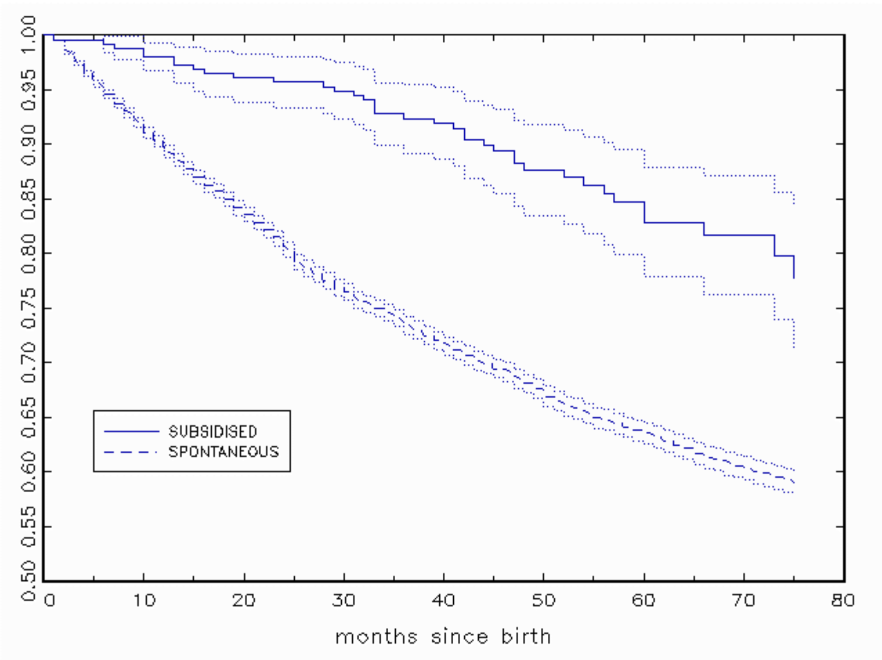
In 1986 the Italian Parliament passed a bill, which we will refer to by its ID code, L.44, in order to promote youth entrepreneurship in the south of Italy, a comparatively disadvantaged area in the country. In its bare essentials the programme, which is still going on after some reshaping, is as follows. Eligible people are youths living in the south of Italy, no matter for their labour force state, who are willing to establish a new firm there. Applicants have to provide a business plan in which they analyse the market prospects and explain why they do believe they could succeed. A panel of referees review the applicants and select the most promising ones. Enrolled projects are then generously subsidised in cash (in the span of time 1986-1992, the period we consider, average subsidy has been Lit. 3,500 million - around 1.7 million Euro) and provided with know how through training and tutorship.

The rationale for such program rests on the belief that good potential entrepreneurs exist in the target area, in particular among youths, who are prevented from starting their firms by market imperfections, in particular by credit market ones. The programme aims at identifying such good potential entrepreneurs and at helping them to overcome market imperfections and start their businesses.

So far, no serious attempt has been done to evaluate how the programme has performed. Despite this, the programme has enjoyed vast popularity, leading the European Commission to include it among the European Best Practice Examples of active labour market policies (European Commission, 1997, p. 44). The reason of this success lies in the fact that subsidised firms appear to live longer than comparable spontaneously born firms (see for instance Centosud, 1995 and Ministero per gli Interventi Straordinari nel Mezzogiorno, 1993).

In fact, the evidence we obtain in this paper is consistent with the anecdotal evidence provided so far. Consider Figure 1 displaying the survival functions for a group of subsidised firms and a group of comparable (with respect to two-digit industry, geographical location and cohort of birth) spontaneous firms (see below for details on the data). Apparently, mortality rates among subsidised firms are much lower than among spontaneous firms. Does it mean that recruited firms are better than spontaneous ones in any meaningful sense? Does it bear any support to the programme?

Figure 1. Survival functions for subsidised and spontaneous firms (with 95% confidence intervals)



By referring to textbook production theory, an obvious explanation for such evidence is that in the span of time during which they receive the subsidy (which is unusually large by the European standards; more details in section 2) recruited firms bear operating costs lower than those borne by spontaneous firms; hence, they make higher profits and are more likely to survive. To establish in a meaningful sense whether recruited firms are good as compared to spontaneous ones one should not content itself at looking at their survival rates; one should assess their efficiency. Or, to turn to an observable counterpart of efficiency, one should look at their survival chances by properly accounting for subsidies to spell out how much the higher survival chances are due to higher efficiency and how much it is due to the subsidy.

This way of looking at the evaluation problem is a direct implication of the rationale for the programme we mentioned few lines above: the subsidy is just a tool to promote successful firms which in the absence of the programme would not start operating. To show the programme is working one should show that selected firms are truly good firms whose survival chances do not rely upon the subsidy (in fact, one should also show there is not any deadweight effect; more on this below). Brancati (1997) places the same emphasis on the role of selection in the L.44 programme.

This view is entirely consistent with some literature on firms supporting schemes (see for instance Storey, 1993). Such literature points out that in the absence of any targeting of such schemes to high quality potential firms high mortality rates are very likely to occur among subsidised firms because low quality firms typically survive on the subsidy and cease soon after its exhaustion. Hence, what matters for the success of such programmes is not how effective the subsidy *per se* is in keeping firms alive but how well selected the beneficiaries are. To prevent this dependence of firms on subsidies only ‘good’ firms should be selected and/or the efficiency of selected firms should be enhanced by providing them with proper training.

This is exactly the point made by the European Commission (1997, p. 44), too: “A solution to improving (start-up grants) effectiveness can be found in the provision of specific training in combination with finance and an appropriate assessment of the feasibility of each business project”. In fact, the L.44 programme perfectly fits such guidelines: subsidised firms go through a highly selective screening process and the entrepreneurs are offered a training programme tailored for each specific case in order to fill their likely lack of experience.

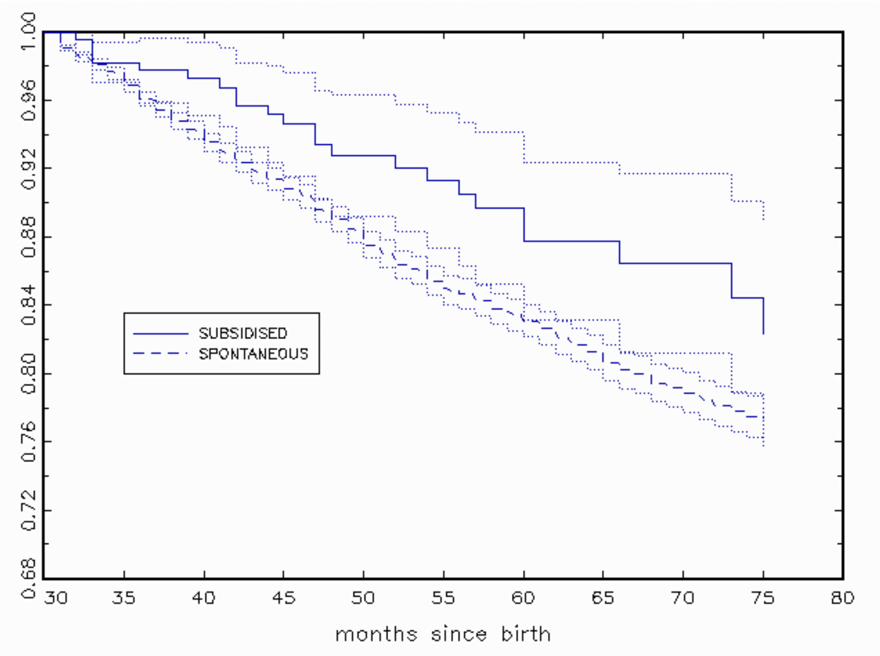
Obviously, in a programme targeted to high quality potential firms there is plenty of room for dramatic deadweight effects since if any firm is to overcome the alleged market imperfections and to become an actual firm it is likely to be an high quality one. Hence, besides showing that selected firms are high quality ones, to show the programme is working one should also provide evidence that in the absence of the program they would have not started operating. Notice however that to question the programme it would be enough to prove that selected firms are not high quality ones. No matter for the existence of deadweight effects, if the programme fails to promote successful firms it fails altogether.

The question we address in this paper is the following one: is it because of the screening and training mechanisms that L.44 firms are able to survive much longer than spontaneous firms? Or, is it just because they receive large subsidies?

In the former case, we would conclude that one necessary condition to maintain the programme is met, the other necessary condition being the absence of any deadweight effect. In the latter one, we would conclude against the programme. On the other hand, we will not address the deadweight effects issue.

To preview our conclusion, our results suggest that if one takes the start-up advantage into account, subsidised firms <u>do not</u> live any longer than spontaneous firms. A simple way to contrast the pro-programme-looking evidence in Figure 1 is to look at the survival functions for spontaneous and subsidised firms, respectively, conditioning on the pool of firms still alive at the age of thirty months (Figure 2). At that age a significant proportion (but not all) of the subsidy is over so that surviving spontaneous firms bear costs which are closer to (but still lower than) those borne by spontaneous firms. Apparently, most of the difference in the mortality rates documented in Figure 1 disappear in Figure 2 suggesting that it is the subsidy to enhance the survival rates for subsidised firms, not their efficiency. In the following we elaborate on this.

Figure 2. Survival functions for subsidised and spontaneous firms conditional on surviving up to the 30th month (with 95% confidence intervals)



The paper is organised in the following way. In section 2 we provide more details on the working of the programme. In section 3 we establish a link between the firm latent quality, its efficiency, and the observable outcome, its survival time, by referring, albeit loosely, to Jovanovic (1982); we show why a longer survival time among subsidised firms does not imply a higher level of efficiency; finally, we develop a test on the programme effectiveness to yield firms whose surviving chances do not rely on subsidies. In section 4 we explain how we control for observable heterogeneity. In section 5 we present empirical results (the data set, which is derived from Social Security files, is described in an appendix). Concluding remarks follow.

2. A description of the program

L.44/1986 applies to companies which are not yet established at the time of the application. Eligibility criteria are the following. Beneficiaries have to operate within certain industries: agriculture, manufacturing and business services (this excludes trade, for instance); the majority of partners have to be between 18 and 29 years of age, or, alternatively, all partners have to be younger than 35; the companies have to be located in the southern regions of the country.

Notice that, differently from other start-up grants programmes in Europe, one needs not to be unemployed to be eligible.

Selected companies are provided with both financial help and services. The financial help consists of:

1. A non-refundable grant to provide start-up capital, up to Lit. 5 billion (around Euro 2.5 million) and up to the 60% of the overall disbursement (which can include the business plan, the purchase of land, buildings, equipment and electrical, water and telephone connections). Its exact amount varies according to the region and the age of the partners. A special bonus is offered when companies are made mainly of women.

2. A loan is offered on special conditions, allowing for an interest rate 70% lower than the market one; it may cover up to 30% of the start-up capital and lasts for 10 years.
3. A non-refundable grant of up to Lit. 1 billion (Euro .5 million) for the first year of life of the company and Lit. 0.75 billion (Euro .37 million) for the second year are provided towards operating costs, including interest rates and rents. Only wages and salaries are excluded.

Enrolled entrepreneurs are also offered training courses on how to run their company. Besides, each subsidised firm is watched over by a tutor - an established company in the same industry - during the first spell of activity.

There are a number of constraints on the beneficiaries. The benefits are not available if a company receives other forms of subsidy. Equipment and buildings cannot be diverted to purposes other than those specified in the business plan, for 5 and 10 years respectively. Shares of the company cannot be transferred to non-eligible people for 10 years (this is in order to prevent 'normal' entrepreneurs to exploit the benefits).

As for the selection of beneficiaries, candidates have to present a business plan, the linchpin of the whole decision process. On the basis of it, in fact, the *Committee for the development of new youth entrepreneurship,* which is in charge of implementing the scheme (and which has now become a proper company, owned by the Treasury) makes a first screening. The selected projects are then evaluated by a panel of referees. Finally, the Committee makes a proposal to the Treasury.

As a matter of fact, selection criteria have been very tight: out of the 4,000 plans submitted by 1993, only 20% were passed. On the other hand, in sharp contrast with other European start-up grants programmes, the benefits are very generous. To give an idea, in the time span 1986-'93 local administrations in the South granted subsidies for Lit. 1,004 billion to 3,142 starting firms, i.e. an average amount of Lit. 300 million. In the same period, the average subsidy under the L.44 scheme was more than ten times as much, i.e. Lit. 3,500 million, granted to 792 firms.

Since 1993 the scheme has been slightly modified. As we already mentioned the Committee has been transformed in a joint stock company, entirely owned by the Treasury; subsidies can be delivered to firms operating outside the South provided they belong to the areas of the EU objectives 1, 2 and 5a; the initial grant has been extended to three years.

3. A framework for evaluation

The purpose of this section is twofold. Firstly, we want to find an empirical measure of the effectiveness of the programme. As we mentioned earlier, the aim of the programme is to allow potential 'good' entrepreneurs in disadvantaged areas to start their businesses, who would otherwise be prevented by market imperfections. We show that, under general conditions, firms' survival is related to their efficiency: hence, if one compares survival rates between subsidised firms and a sample of spontaneous ones, which are made comparable according to very precise criteria (defined in section 4), this provides a test of the effectiveness of the programme, by which we mean the ability both to select 'good' candidates to start with and to train them properly. Clearly a proper comparison has to take into account the fact that subsidised firms can rely on an initial subsidy, something which is missed from previous analyses. Even so, as we shall see, the comparison can nevertheless be biased in favour of subsidised firms since, given the design of the programme, they can carry on substantial benefits even after the deadline of the initial subsidy.

As for the second objective of this section, we develop a test of the programme's effectiveness which relies on the shape of the hazard function. If hazard is increasing over time and such increasing pattern is peculiar to subsidised firms, then it can be argued that the initial selection process failed to pick up 'good' projects that are able to survive after the demise of the subsidies.

3.1. A simple model of firms selection in and out of an industry

To clarify which aspect of the scheme is under trial in this paper we need to introduce a little notation. Let the indices *i* and *t* run over firms and periods, respectively. To produce a level q_{it} of output at input prices p_{it}^{f} firm *i* in a specific industry has to bear the cost

$$c\left(q_{it}, p_{it}^{f}\right) x_{it},$$

with *c(.,.)* common to all firms in the industry, constant over time; x_{it} is a firm-specific characteristic reflecting firm's inefficiency, which we allow to vary over time to account for learning-by-doing processes.

Let $\pi_{it} = p_{it}^{q} q_{it} - c(q_{it}, p_{it}^{f}) x_{it}$ be the profit at time *t* for firm *i*. In each period the firm maximises its expected current profit

$$\max_{q} E\left\{\pi_{it}\right\},$$

where p_{it}^{q} is the output price; $u_{it} = \pi_{it} - E\left\{\pi_{it}\right\}$ is a random component varying both across firms and over time reflecting factors unknown to the agents at the time they draw their decisions[1]. Conditional on the optimal level of q_{it} the firm attains a profit which differs from the expected one due to the unanticipated outcome u_{it}. In the following we assume that input and output prices are constant across firms and we drop the index *i* from them.

Entrepreneurs enter and exit the industry by comparing expected profits in the industry to returns from alternative investments. Let τ be the threshold such that the firm quits at period *t* if and only if $\pi_{it} < \tau$.

Since under plausibly general regularity conditions expected profits increase as *x* decreases, more efficient firms are more likely to enter the industry and to survive longer[2]. That is, the survival time *D* is negatively correlated to the level of inefficiency *x*. Otherwise stated, the hazard function $h(t \mid x) = f(t \mid D \geq t, x)$ for a *x*-inefficient firm (*f(.)* is the survival time density) increases with respect to *x* at each point in time: among firms still alive after *t* periods the more efficient ones are the most likely to survive further.

Exploiting such monotonic relationship between *x* and *h(.|x)*, we can use *h(.|x)* itself as an index of (in-)efficiency. If *x* was time-invariant then the following stochastic ordering would hold due to the dynamic selection process

$$(x \mid D \geq t) \succ (x \mid D \geq s), \ \forall (t,s) \mid s > t,$$

that is, the pool of firms still alive at time *t* would be (stochastically) less efficient than the pool of firms still alive in any subsequent period. Such stochastic ordering is just reinforced if firms learn

---

1 In Jovanovic (1982) u_{it} reflects the agent's ignorance on x which s/he can learn about only by operating his/her firm.

2 In the Jovanovic (1982) framework this result is formally derived.

by doing. Let we stress the dependence of x on firm age by writing x_t. If x_t decreases as the firm grows older then $F_{x|D\geq t}$, the cumulative distribution function (cdf in the following) of x among firms still alive after t periods, moves to the left as t increases both because of dynamic selection and because surviving firms become more efficient.

Let

$$h(t) = \int h(t \mid x) dF_{x|D\geq} t$$

be the aggregate hazard rate among firms still alive after t periods. Does the selection model introduced so far bear any implication for the pattern of *h(t)*? Standard econometrics of duration data would imply that in the presence of heterogeneous agents the aggregate hazard function should decline over time. In our case study such a declining pattern would signal that as time flows the composition of the pool of surviving firms increasingly shifts towards more efficient firms which, being more efficient, die at a lower rate.

In fact, there is some evidence from several empirical studies witnessing an increasing pattern for *h(t)* during firms infancy which turns to a declining pattern as firms grow older. Harhoff, Stahl and Woywodes (1998) find such non monotonic pattern for medium to large size German firms in some industries, with *h(t)* peaking in the age class 2-5 year (or even later for large firms). Storey (1994, p. 93) provides a similar evidence with reference to a pool of firms from United Kingdom, with *h(t)* peaking by the end of the second year of life. Caves (1998) in his review on firms turnover reports that such evidence seems to be quite widespread. He also reports on some evidence that when hazard rates can be measured by months they increase for the most of the first year.

An explanation for such non monotonic pattern is in Pakes and Ericson (1998). Moving from Jovanovic (1982) they point out that if firms make their entry investment unsure of their success ill-fated firms, namely firms which in our notation exhibit high values of *x*, need some experience to convince themselves of their unfitness, hence to quit. As a consequence, it might take a bit of time for the efficiency-driven dynamic self-selection of firms to emerge inducing the declining pattern of *h(t)*.

Notice however that in the Pakes and Ericson (1998) explanation despite the non monotonic pattern of the hazard function it still holds true that as time passes the pool of surviving firms becomes increasingly more efficient.

3.2. A rationale for the program

Within this framework the rationale for the L.44 program rests on two premises. Firstly, there should exist (among eligible agents) some efficient potential firms - or at least potential firms capable of becoming efficient thanks to proper training - that is firms with 'high' expected profits due to 'low' values for *x*, which in the absence of the scheme would not enter the industry due to market imperfections. Secondly, it should be feasible to screen out inefficient applicants, to enrol (eventually) efficient ones and to let them attain a satisfactory efficiency level by training them.

As for the first premise, the market imperfections alluded to can be thought to work the following way. Banks do not observe *x*, the applicant's potential firm inefficiency, and estimate it by conditioning on the applicant's CV. Young people CV's tend not to be enough informative on *x* because they are too short for banks to usefully condition on them. As a consequence $\text{var}\{x \mid CV\}$, a measure of the uncertainty banks face when deciding whether or not to issue a loan, is larger for young people. Presumably, banks' policy is not to issue loans if uncertainty is above a specified level. Such policy tends to preclude young people from getting loans even if their (potential) true *x* was low.

To reinforce such outcome adverse to young people, it is also likely that young people (potential) firms are on the mean less efficient than firms run by more experienced entrepreneurs, at least at the outset. Since banks are not in the position to mandate applicants participation in a training program to fill in their lack of experience, young people would be less likely to receive loans even if their *x* were precisely estimated by banks.

As for the second premise, here we are at the heart of this paper: whether applicants were properly selected (and went through an effective training process) is the crucial question we will try to answer. On the other hand, we will not try to assess whether the programme originated any deadweight effect, since the evidence we had access to do not allow us to establish, even only tentatively, whether subsidised firms would have started operating even in the absence of the programme.

A simple way to assess the efficiency level of subsidised firms is by exploiting the link between efficiency and survival chances we established in the previous section. In principle, by comparing the survival rates of subsidised firms after the initial grant has ceased to those of a comparable sample of spontaneous firms one could establish how subsidised firms rank relative to spontaneous firms in term of efficiency.

Notice however that obtaining an unbiased comparison is not a straightforward task since L.44 firms may enjoy substantial benefits even after the initial subsidy has expired. To see this, let us go back to our notation.

Let $p_t^{f,44}$ be the price at which subsidised firms buy inputs at time *t* and $p_t^{f,S}$ the corresponding prices for spontaneous firms. Due to the scheme the inequality

$$p_{jt}^{f,44} < p_{jt}^{f,S}, \ t = 1, T_j$$

holds for at least some *j*, with *j* running over inputs. The spell of time during which subsidised firms face comparatively lower input prices depends on the input. Remember that L.44 firms receive a 2-year grant toward purchase of goods and services: hence several inputs other than capital are bought at a lower price during the first two years. Furthermore, L.44 firms receive a contribution on capital expenses and a rebate on the mortgage interest rate: hence the user cost of capital is lower than the market one up to the period in which the initial endowment of capital is completely replaced.

As a result, <u>conditional on *x*</u> any subsidised firm bears lower costs and gets higher profits than a spontaneous firm. Whatever the level of output *q*, up to the period in which subsidies exhaust the following inequality holds

$$c(q, p_t^{f,44})x < c(q, p_t^{f,S})x, \ t = 1, \max_j T_j$$

Such impact of subsidies on profits also bears consequences on the subsidised firms stopping decision since <u>conditional on *x*</u> and up to $\max_j T_j$ any subsidised firm is more likely to stay in the industry than a spontaneous firm

$$h(t \mid x, p_t^{f,44}) < h(t \mid x, p_t^{f,S}), \quad t = 1, \max_j T_j.$$

Let $h(t\,|\,44, p_t^{f,44})$ be the aggregate hazard function for the subsidised firms in which we stress the fact that they are buying their inputs at a subsidised price and let $h(t\,|\,S, p_t^{f,S})$ be the aggregate hazard function for the spontaneous firms. The following decomposition holds

$$h(t\,|\,44, p_t^{f,44}) - h(t\,|\,S, p_t^{f,S}) = \left[h(t\,|\,44, p_t^{f,44}) - h(t\,|\,S, p_t^{f,44})\right] + \left[h(t\,|\,S, p_t^{f,44}) - h(t\,|\,S, p_t^{f,S})\right].$$

$h(t\,|\,S, p_t^{f,44})$ is the counterfactual aggregate hazard function the spontaneous firms would exhibit if they received the same subsidy as the actually subsidised firms. The first term in brackets on the r.h.s. reflects differences in the composition of the two groups with respect to x since by conditioning on $p_t^{f,44}$ both groups bear exactly the same operating costs. The second term in brackets on the r.h.s. reflects the impact on the spontaneous firms aggregate hazard function of switching the input prices from $p_t^{f,S}$ to $p_t^{f,44}$.

If we could compare the two groups conditioning on input prices (including the user cost of capital) that is if we could identify the first term in brackets on the r.h.s., the inefficiency index *h(t|.)* would allow us to make across firms efficiency comparisons. Instead, on observing the inequality

$$h\left(t\,|\,44, p_t^{f,44}\right) < h\left(t\,|\,S, p_t^{f,S}\right)$$

as we actually observe (at least over some years since the firms' start; see below), we cannot say to which extent this is because of the first term of the decomposition, namely because the pool of subsidised firms is more efficient than the pool of spontaneous firms, and to which extent it is because of the second term of the decomposition, namely just because they face lower input prices, which is exactly what advocates of the programme missed to recognise commenting on the spectacularly lower mortality rates experienced by subsidised firms.

Also notice that if we could establish the comparison between the two groups at any t larger than $\max_j T_j$ the second term of the decomposition would vanish, and the resulting comparison across the two groups would not be biased towards subsidised firms, since at any such t the subsidy is entirely gone, hence the hazard rate for subsidised firms still alive only reflects their efficiency. The problem is that while we do know when the subsidy towards operating costs exhausts, namely by the end of the second year of life, it is not known when the subsidy towards initial investment is over since to get such information one should know when the initial endowment of capital is entirely replaced.

The implication for our analysis is that even comparing subsidised firms to spontaneous ones at any t slightly beyond the second year of life, the resulting ranking is very likely to be biased towards the programme. In fact, given the size of the subsidy towards initial investments the bias is likely to be quite severe, a fact one needs to keep in mind on looking at the results we shall show in section 5.

3.3. Testing whether subsidised firms survive on subsidies

To complete the derivation of our evaluation tools we need to elaborate further. Let's assume that as soon as π_{it} falls below τ it does not turn upward above the threshold in any subsequent period.

Then, the event $(\pi_{it} < \tau < \pi_{it-1})$ becomes the same as $D = t$: the (unique) point in time in which the profit path crosses the threshold uniquely determines the firm survival time.

Then, let

$$\pi_{it} = p_{it}^{q} q_{it} - c(q_{it}, p_{it}^{f,44}) x_{it}$$

be the current profit at time t which subsidised firm i looks at to decide whether to quit or to stay. Also let

$$x_{it} = \alpha_i + tr_i(t).$$

Let us stress again the dependence of π_{it} on three factors: α_i is the individual specific effect reflecting the level of inefficiency of firm i at the time in which selection takes place; $tr_i(t)$ as a function of t is the (possibly individual specific) path along which the inefficiency decreases over time due to training and learning-by-doing; finally, as time passes $p_{it}^{f,44}$ increases due to the decline of the subsidy and the profit declines accordingly.

The firm survival chances depend both on the pattern and on the level of π_{it}. Other things being equal, as the subsidy and the profit decline the firm becomes more likely to quit. To prevent the firm survival chances from being affected by the subsidy decline, it takes that the other two factors affecting the profit level counterbalance the subsidy decline. That is, it must happen that either α_i is sufficiently high – i.e., the firm is sufficiently efficient from the outset -, or training delivered under the program is effective in improving the firm efficiency over time (or both).

The three components of the program and their roles are apparent here:

1. screening: firms should be selected exhibiting reasonably high α_i;
2. training: their efficiency should be enhanced by training (and learning by doing);
3. financing: to overcome the market imperfections, selected firms should be financially supported.

Apparently, it is in the foundations of the program that firms' survival chances eventually must not rely on the subsidy. They must rely on the level of efficiency subsidised firms eventually reach. In this respect, if the efficiency at selection is enough high and/or it is improved upon to a proper degree during the start-up period then subsidised firms should not be induced to quit as the subsidy declines.

To formalise such requirement it is straightforward to require that the hazard function for each specific firm does not increase over time. On aggregating over firms, we get the (weaker) requirement that the mean hazard function

$$E\left\{h(t \mid x, p_t^{f,44})\right\} = \int h(t \mid x, p_t^{f,44}) dF(x)$$

must not increase over time. This requirement is not directly testable since we can only identify the mean hazard rate for firms still alive at time t

$$E\left\{h(t \mid x, p_t^{f,44}) \mid D \geq t\right\} = \int h(t \mid x, p_t^{f,44}) dF(x \mid D \geq t),$$

which is a non random subgroup out of the subsidised firms pool. This is exactly the same dynamic self-selection problem we met with spontaneous firms: since firms are heterogeneous with respect to their efficiency, surviving firms exhibit a level of the hazard rate lower than the quitting firms.

Moreover, the slope of $E\left\{h(t\,|\,x,p_t^{f,44})\,|\,D\geq t\right\}$ - the mean hazard function among surviving firms - is downward biased with respect to the slope of $E\left\{h(t\,|\,x,p_t^{f,44})\right\}$ - the mean hazard function in the population.

As a consequence, we cannot conclude anything on the pattern of $E\left\{h(t\,|\,x,p_t^{f,44})\right\}$ except in one case: if $E\left\{h(t\,|\,x,p_t^{f,44})\,|\,D\geq t\right\}$, the identifiable hazard function, increases over time then *a fortiori* $E\left\{h(t\,|\,x,p_t^{f,44})\right\}$ increases over time, as well. Which would allow us to conclude that subsidised firms are induced to quit as the subsidy decline.

4. Econometrics

The comparison group we use is a pool of spontaneous firms drawn from the Social Security archive. Up to now we referred to an ideal pool of firms belonging to the same industry and operating in a homogeneous environment. In practice, firms included in our sample are very heterogeneous. In particular, even based on the rather poor information we have (see the Appendix) it is very likely that severe heterogeneity comes in with respect to technology, prices and economy wide conditions firms face during their life. In the following we will proxy such heterogeneity by two-digit industry, location and cohort of birth. In fact, two firms belonging to the same cohort of birth, operating in the same geographical area and in the same industry are very likely to bear similar technological constraints, to buy their inputs and to sell their outputs at similar prices and to face the same macroeconomic ups and downs.

Let *z* be the variable denoting the observable heterogeneity and let us stress the dependence of the hazard function on *z* by writing $h\left(t\,|\,x,z,p^f\right)$ (with *x* denoting the firms inefficiency and p^f the input prices as in section 3). Let y_t be the binary outcome dying/surviving at period *t* conditioning on surviving at least up to that period. The mean value of y_t for a firm at a level of inefficiency *x,* with characteristics *z* and buying its inputs at price p^f is $h\left(t\,|\,x,z,p^f\right)$. Let us rewrite y_t as

$$y_t = h(t\,|\,x,z,p^f) + \varepsilon_t$$

with ε_t capturing variation of y_t around its conditional mean due to unobservable heterogeneity.

Let *I=44,*S be the binary variable indexing the status with respect to the programme.

On taking the mean of y_t conditional on *I* we find

$$h(t\,|\,I) = \int\ h(t\,|\,x,z,p^f)dF_{x,p^f|z,I}dF_{z|I} + E\left\{\varepsilon_t\,|\,I\right\}$$

showing that the comparison of the aggregate hazard functions across the two groups besides reflecting the differences with respect to *x* and to p^f also reflects the differential composition of the two groups with respect to the observables *z* as well as the unobservables ε_t. This is disturbing since it precludes using the aggregate hazard function as a tool to make across groups efficiency comparisons. Suppose that by moving to a time period *t* enough away from the firms birth to make us confident the residual subsidy is negligible we found that *h(t|44)* is smaller than *h(t|S)*. Such evidence would be useless since we would not be able to establish whether subsidised firms are less efficient or they are comparatively disadvantaged by adverse *z* and ε_t.

We develop our evaluation resting on the following

Identifying restriction: $y_t \perp I \mid x, z, p^f$, $\forall t$.

In words, the restriction asserts that if we could randomly select one firm out of the subsidised pool and one firm out of the spontaneous pool such that they exhibit exactly the same level of inefficiency and the same characteristics *z* and then we let them buy their inputs at the same price p^f the two firms will exhibit the same hazard function.

The result we rely on in our empirical analysis is in the following

Theorem: *Let S be a subset of the spontaneous firms population such that the equality* $F_{z|S} = F_{z|44}$ *holds. Then the aggregate hazard function h(t|I), I=S, 44, depends on I only through* $F_{x,p^f|I}$.

Proof: $E\{y_t \mid I\} = E\{h(t \mid x, z, p^f) \mid I\} + E\{\varepsilon_t \mid I\}$. The identifying restriction implies $E\{\varepsilon_t \mid I\} = E\{\varepsilon_t\}$ which is equal to 0. As for the first term on the r.h.s.

$$E\{h(t \mid x, z, p^f) \mid I\} = \int h(t \mid x, z, p^f) dF_{x,p^f|z,I} dF_{z|I} = \int h(t \mid x, z, p^f) dF_{x,p^f|z,I} dF_z$$

the last equality following from the theorem premise. ***Q.E.D.***

As an implication of the theorem once we get a comparison group distributed with respect to *z* the same as the subsidised group we feel confident that if we find any difference across the two groups either in the level or in the pattern of the aggregate hazard function it is due to differences with respect to either p^f or *x*.

One simple way to obtain a comparison group distributed with respect to *z* the same as the subsidised group is to *match* to each subsidised firm exhibiting characteristics *z* a specified number *k* of spontaneous firms with the same characteristics. Then, we can compare the level and the pattern of the hazard functions across the two groups in a completely non-parametric fashion.

To further clarify crucial aspects of our analysis three remarks are in order on the treatment of observable and unobservable heterogeneity, on the nature of the parameter of interest and on the solution we adopt to control for *z*, respectively.

Firstly, the typical problems an analysis of duration data aims to solve are *i*) to model the dependence of the failure rate on some observable characteristics of the units and ii) to establish whether the frequently encountered declining pattern of the hazard function reflects true duration dependence or simply reflects the presence of unobservable heterogeneity. Notice that in our problem it is not this way.

On the one hand, to the evaluation of the L.44 programme the dependence of the hazard function on the observable heterogeneity *z* is a nuisance not a feature of primary interest. We only need to control for *z* to allow the difference across the two groups in the cfd $F_{x,p^f|I}$ to emerge through the aggregate hazard functions.

On the other hand, we do not try to distinguish between true vs spurious duration dependence. The model we set in section 3 allows for both kinds of duration dependence. True negative duration dependence emerges because the longer firms survive the more they become fit hence able to survive further. Besides, during their infancy subsidised firms also experience some training which reinforces the true dependence of the survival chances on the elapsed survival time. Spurious negative duration dependence emerges because firms are *x*-heterogeneus at the outset inducing the well known declining pattern of the aggregate hazard rates. In the subsidised vs spontaneous firms comparison we aim at establishing how selected firms rank as compared to spontaneous ones in term of efficiency but we do not try to separately identify the role played by the screening vs the training components of the program in determining the actual ranking. In the analysis of the pattern of the subsidised firms hazard function again we only aim to assess whether screening and training jointly counterbalance the incentive to quit due to the decline of the subsidy.

Secondly, most of the recent literature on program evaluation deal with problems in which the parameter of interest is the difference between the outcome y_i^T the *i*-th unit would experience by being exposed to a treatment and the outcome y_i^{NT} the same unit would experience by being denied the treatment; $y_i^T - y_i^{NT}$ is the impact of the treatment on the *i*-th unit (see Rubin, 1974). In our case study the outcome is the firm survival time. Given the size of the subsidy each enrolled firm receives no one can seriously doubt that the program impact $y_i^T - y_i^{NT}$ is large on each firm[3].

The point here is that such impact is not the interesting parameter for the L.44 programme. The academic literature and the European Commission guidelines we quoted argue that the programme goal is to promote firms capable of surviving on their own. To get such goal beneficiaries must be carefully selected and properly trained. It is true that they also receive a subsidy but only because, so the argument goes, otherwise they would not start operating due to the market imperfections. The $y_i^T - y_i^{NT}$ parameter tells us nothing on the central questions: did the panel of referees identify good applicants? Were they properly trained? On the other hand, we do provide some answers to those questions by identifying the efficiency of recruited firms as compared to that of spontaneous ones and by checking whether the survival chances of recruited firms depend on the subsidy.

There is an illuminating way of contrasting a classical impact analysis to the analysis we develop in this paper based on the decomposition of $h(t\,|\,44, p_t^{f,44}) - h(t\,|\,S, p_t^{f,S})$ we derived in section 3.2. The term $h(t\,|\,S, p_t^{f,44}) - h(t\,|\,S, p_t^{f,S})$ of that decomposition is straightforwardly interpreted as the mean impact of the subsidy on the comparison group, while the term $h(t\,|\,44, p_t^{f,44}) - h(t\,|\,S, p_t^{f,44})$ is akin to, but not the same as, the selection bias in a 'y^T vs y^{NT}'-type evaluation problem. It is not just a selection bias because as we explained in section 3.2 it reflects the difference across the two groups with respect to *x* at time *t* as a result of both the selection of the subsidised group and the training subsidised firms went through. In a classical impact analysis it is the first term, the impact, to be the parameter of interest, while the second term, the selection bias, is a nuisance. In the present analysis it is the other way round: the quasi-selection bias is the parameter of interest while the impact of the subsidy is a nuisance.

Looking at the evaluation problem this way bears a key practical implication. In a 'y^T vs y^{NT}'-type evaluation problem the programme impact on single agents cannot be identified since we cannot observe both y^T and y^{NT} on the same agent. This is the fundamental problem of causal inference as Holland (1986) effectively named it. One has to resort to identifying mean impacts which can be done by contrasting the mean outcome experienced by a group of agents went through the treatment to the mean outcome experienced by a group of agents denied the treatment. To identify the mean impact the agents included in the comparison group must be exactly alike the agents included in the

3 In fact, if the programme advocates were right the counterfactual event for each recruited firm would be zero: in the absence of the programme they would have not started operating. Hence, the impact of the programme on subsidised firms would be equal to the observed survival time.

treatment group except for their status with respect to the treatment. In a non experimental setting, a common (but by far not unique; see Heckman and Robb, 1985) way to satisfy such requirement is to control for all the observable characteristics of the agents likely to bias the comparison between the treatment and the comparison group. Formally, by letting *w* be the characteristics one conditions on and *I* the binary variable indexing the status with respect to the treatment, the following Conditional Independence Assumption (CIA) must hold

$$(y^T, y^{NT}) \sim I \mid w$$

(see Rubin, 1974). In words, let the agent *i* be assigned to the treatment and the agent *j* be denied the treatment; let the two agents be equal with respect to *w*. CIA requires that if the agent *j* was assigned to the treatment (or the agent *i* was denied the treatment) the difference in the outcomes we would observe between the two agents, $y_i^T - y_j^T$ (or $y_i^{NT} - y_j^{NT}$), would not depart from zero systematically.

Carefully notice that to solve our evaluation problem we do not want to condition on a set of observables such that CIA is satisfied. If as a result of the selection process it was the case that enrolled firms are more efficient than spontaneous ones, hence more likely to survive, it is exactly that differential in the survival chances we want to identify since it would document that the panel of referees is doing the right thing. A mind experiment to reveal such differential would be to issue the subsidy to a sample of (otherwise comparable) spontaneous firms and then to compare the survival chances across the two groups. Such mind experiment would identify the quasi-selection bias term in the decomposition in section 3.2.

In fact, we do some controlling by matching on *z*, but such controlling is not aimed to fulfill CIA, that is to compensate for all the differences in the composition of the two groups. Matching on *z* is needed because the two groups must be composed the same way with respect to any variable but *x* affecting the hazard rates to let the differential composition of the two groups with respect to *x* emerges through the hazard rates.

Thirdly, in our view sweeping out heterogeneity with respect to *z* by matching offers some advantages over parametric and semi-parametric regression-like methods. We aim at comparing the mean value of y_t in the two groups controlling for differences in the composition with respect to *z*. By specifying a parametric functional form for the dependence of the mean value of y_t on *z* we get under the threat of missing the right specification which might turn into a bias of the comparison across the two groups. Instead, by building a comparison group composed the same way as the subsidised group with respect to *z* we do not need any further controlling for *z*. Thus, we avoid the explicit modelling (and the risk of mis-specification thereof) of the way in which *z* affects the survival chances. Further, as we explained there are good reasons to believe the baseline hazard function is not the same in the two groups, hence a modelling of hazards as flexible as possible is valuable.

Besides controlling for the firm's characteristics listed at the beginning of this section, in principle one might argue that we should also control for the age of the entrepreneur(s) running the firms since the entrepreneur age, which is presumably correlated to his/her experience, might enhance the firm chances to survive. Since in our data set we do not have the entrepreneur age available and our comparison group is most likely made up of firms run by entrepreneurs exceeding the age limit for eligibility there is room for some bias against the programme.

In fact, the evidence resulting from other studies in the field are not straightforwardly against our comparison group. Harhoff, Stahl and Woywodes (1998) find that the owner age is either irrelevant for or negatively associated to the survival time depending on the industry and on the reason why the firm ceased operating. Such evidence implies that the bias resulting from our comparison group, if any, is in favour of the programme not against it. Bates (1990) and Cressy (1996) find a non monotonic relationship linking the survival chances to the owner age, mildly increasing up to middle age and then decreasing, an evidence suggesting that the bias, if any, is not severe and its sign needs not be against the programme. On the whole, there seems not to be any compelling evidence against using our comparison group.

There are three further possible arguments against our comparison group one might figure out on which it is worth saying a word. Firstly, if the panel of referees is recruiting efficient applicants which would have started even in the absence of the programme, the comparison group turns out worse than it would have been in the absence of the programme since the panel of referees is moving good firms from the comparison group to the subsidised one. Given the scale of the programme we believe the resulting bias is negligible. Secondly, if only (potential) firms run by good managers apply for the programme because bad managers are not well informed and if such bad managers start their firms the comparison group turns out sistematically worse as a result of the self-selection into application. We believe the occurrence of such self-selection is very unlikely since the programme quickly became very popular in Italy due to a widespread advertising and due to the size of the subsidies it issues. Thirdly, if (potential) firms had to wait to know whether they have been selected before starting operating it could be that brilliant eligible agents self-select out of the program to avoid wasting their time. In fact, immediately after application it is up to them to wait and see or to start. Hence, the optimal strategy to an eligible agent willing to start the firm immediately is to apply and then to start, no matter for how good s/he is.

Turning back to our matching procedure, we had some *z*-strata in which the required number of matches was not available. As a consequence, the distribution of *z* after matching was not the same in the two groups. To compensate for such compositional differences we use the following weighting procedure which builds on Dehejia and Wahba (1995).

Again, let the comparison of the mean value of *y* in the two groups be the parameter of interest, with *y* the binary outcome dying/surviving at a specific time period conditioning on surviving at least up to that time period. If $F_{z|44} \neq F_{z|S}$, then comparing $E\{y|44\} = \int E\{y|44,z\} dF_{z|44}$ to $E\{y|S\} = \int E\{y|S,z\} dF_{z|s}$ reflects differences between the two groups both in the argument of the integral, $E\{y|.,z)\}$ and in the distribution with respect to which the integral is evaluated, $F_{z|.}$. Dehejia and Wahba (1995) point out that to control for differences with respect to $F_{z|.}$ it suffices to choose a reference distribution (that is to choose the distribution with respect to which mean values are evaluated) and to evaluate the mean value of $E\{y|I,z)\}$, *I=44,S* with respect to the chosen distribution. By choosing $F_{z|44}$ as the reference distribution, we compare $E\{y|44\}$ to

$$E\{y|S\}_{44} = \int E\{y|S,z\} dF_{z|44} = \int E\{y|S,z\} \frac{dF_{z|44}}{dF_{z|s}} dF_{z|s},$$

with the weights

$$w(z) = \frac{dF_{z|44}}{dF_{z|s}}$$

acting on spontaneous firms by down-weighting (up-weighting) those exhibiting characteristics *z* over-represented (under-represented) with respect to the distribution of *z* in the pool of subsidised firms.

By applying the Bayes theorem, *w(z)* can be expressed the following way

$$w(z) = \frac{\Pr(44 \mid z)\Pr(S)}{\Pr(S \mid z)\Pr(44)},$$

Pr(44|z) being the *propensity score* as defined by Rosenbaum and Rubin (1983), that is the probability to observe a subsidised firm conditional on *z* in the population represented by the available sample. By assuming a logistic specification for the *propensity score*

$$\Pr(44 \mid z) = \frac{\exp\{\beta' z\}}{1+\exp\{\beta' z\}},$$

the unknown *w(z)* can be replaced by its sample counterpart

$$\hat{w}(z) = \exp\left\{\hat{\beta}' z\right\}\frac{n_S}{n_{44}},$$

where $\hat{\beta}$ is the ML estimate of the logistic coefficients and n_S, n_{44} are the numbers of spontaneous and subsidised firms in the sample, respectively.

The hazard function at period *t* for subsidised firms is estimated by the conventional ratio number of firms dying during period *t* to number of firms at risk of dying at the beginning of period *t*

$$\hat{h}(t \mid 44) = \sum_{\{i \in 44\}} I(D_i = t) / \sum_{\{i \in 44\}} I(D_i \geq t).$$

It is well known that this way of estimating the hazard function allows one to deal with incomplete spells, namely the survival times of units which are still alive at the time in which the observation of the phenomenon stops (see Cox and Oakes, 1984). The sampling variance of the estimate is $h(t \mid 44)[(1-h(t \mid 44)]/n(t)_{44}$. The corresponding estimate for a hypothetical population of spontaneous firms exhibiting the same composition with respect to *z* as that of subsidised firms is obtained exploiting the weights just introduced to down-weight (up-weight) over-represented (under-represented) spontaneous firms

$$\hat{h}(t \mid S)_{44} = \sum_{\{i \in S\}} p(z_i)\ I(D_i = t)$$

$$p(z_i) = \frac{w(z_i)}{\sum_{\{i \in S\}} w(z_i) I(D_i \geq t)},$$

with sampling variance $h(t \mid S)_{44}[1-h(t \mid S)_{44}]\sum_{\{i \in S\}} p(z_i)^2 I(D_i \geq t)$. Notice that if the two groups were balanced with respect to *z*, then the *propensity score* would not depend on *z*, *w(z)* (and *p(z)*) would be constant over units and the estimated hazard function for spontaneous firms would collapse to the standard one. Survival functions are estimated by the usual product-limit estimator.

One might argue that by using weights derived from a logistic specification of the propensity score we are in fact introducing a parametric component in our non parametric analysis. Any misspecification of the propensity score in principle could bias the analysis of the hazard rates. In practice, testing the specification of the propensity score is very easily done. Exploiting theorem 3 by Rosenbaum and Rubin (1983, p.45), if the propensity score is properly specified it must happen that the distribution of z for the spontaneous firms after the weighting procedure has been applied is equal to its counterpart for subsidised firms. A simple informal test is then to graphically compare the two distributions, which in our case leads to accept the specification[4] (Dehejia and Wahba, 1999, p. 1057, implement a specification test on the propensity score based on the same idea).

Since the non parametric estimate of the hazard functions turned out rather noisy, to better identify the relevant patterns we smoothed the $\hat{h}(t|.)$ estimates of the hazard functions by fitting splines. With spontaneous firms we used third degree polynomials, with knots at months 25 and 50, constraining the derivatives up to the second one at knots. With subsidised firms we used second degree polynomials constraining derivatives up to the first one, with knots at 20 and 40. The reason for using lower degree polynomials to smooth $\hat{h}(t|44)$ is that due to the much smaller sample size (see section 5) the estimate is much more noisy for the subsidised group hence we plugged in more smoothing by using lower degree polynomials[5]. Moreover, to force the smoothed values into the admissible range, we mapped them through the function $k*\Phi(.)$, with $\Phi(.)$ the cdf of the standard normal and k chosen in the same order of magnitude as the observed hazard rates[6].

To evaluate the sampling variance of the smoothed estimates we used the following simulation procedure. Let $\tilde{h}(t|I)$ be the smoothed estimate for group *I*, *I=44,S*. Let *n(t|I)* be the actual number of firms still alive at time *t* belonging to group *I*. The number of firms quitting at *t* is simulated as a drawing from the binomial random variable $Bi(n(t|I), \tilde{h}(t|I))$. By replicating such simulation on each time period we obtain one pseudo-sequence of firm quits which allow us to obtain a pseudo estimate of the hazard function by applying the same estimation and smoothing procedures as on the observed sequence. Let $\tilde{h}(t|I)_m^s$ be the pseudo estimate obtained at the *m*-th simulation. By replicating that pseudo estimate a large number of times we are in the position to evaluate the sampling variance of $\tilde{h}(t|I)$ as

$$\frac{1}{M}\sum_{m=1}^{M}[\tilde{h}(t|I)_m^s - \tilde{h}(t|I)]^2$$

with M the number of replicates.

5. Empirical analysis

Data exploited in the analysis refer to 256 subsidised firms, to which we matched 11375 spontaneous firms. Firms were born in the time span January 1987 to December 1992 and tracked through December 1994. Information are derived from Social Security files.

Birth and death dates, hence the spell length, recoverable from the files are not those one would like to obtain for economic analysis purposes. Strictly speaking they register when a specific firms started (and ceased) employing people. As a consequence, the resulting duration underestimates the true one. This is likely not to be a serious problem for our purposes, since we just need to compare durations in the two groups: conditioning on industry, location and cohort of birth we cannot figure out any sensible reason to believe that underestimation selectively acts on one of the two groups. More details on the data set are in the Appendix.

4 Histograms available from the authors on request.

5 The degree of the polynomials plays the same role here as the bandwidth in a kernel-based smoothing problem. Decreasing the degree of the polynomial is the same as increasing the bandwidth of the kernel (see Härdle, 1990, p. 56)

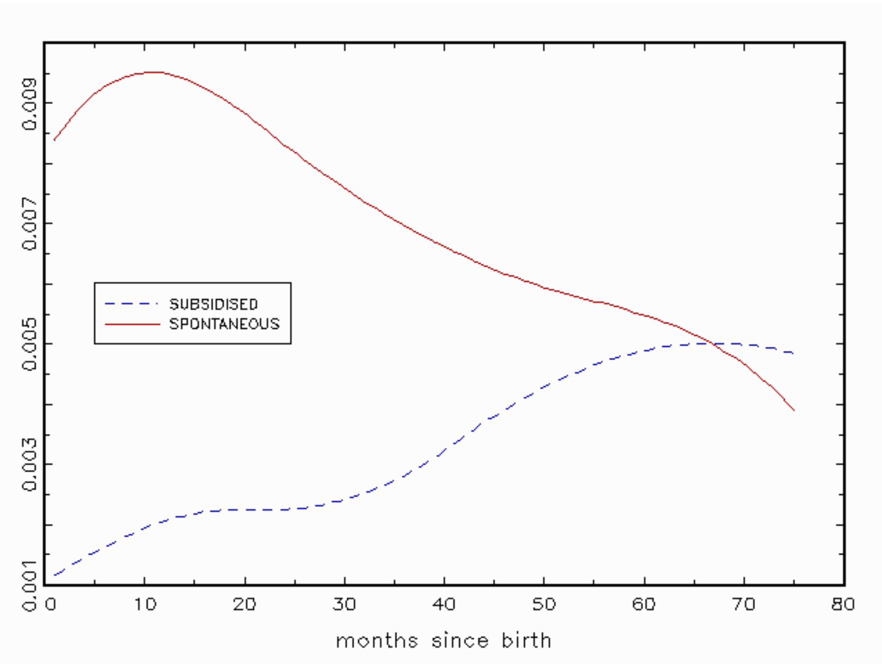
6 We obtained a heuristical validation of the robustness of our procedure by experimenting with different values of *k*.

In Figure 1 non-parametric estimates of survival functions are reported for subsidised and spontaneous firms along with their 95% confidence intervals. Two main evidences emerge:

- As we pointed out in section 1, subsidised firms survive definitely much longer than spontaneous firms. This is precisely the kind of evidence based on which the scheme gained a consensus over the last decade. However, as we argued in previous sections this evidence by no means imply that subsidised firms are more efficient than spontaneous ones.
- Spontaneous firms survival function displays an apparent convexity pointing to a possible negative duration dependence, whereas the subsidised firms one looks concave, at least locally pointing to a possible positive duration dependence. In particular, *S(t|44)* starts decreasing more steeply around month 30, that is approximately when subsidies towards operating costs exhaust[7].

As for the estimation of the hazard functions, results are in Figure 3. The hazard function for spontaneous firms exhibits a definite <u>declining pattern</u> starting before the end of the first year of life. Instead it is increasing over the first ten months of life. This evidence is entirely consistent with evidence from other countries we quoted in section 3.1. According to the discussion therein, the interpretation is straightforward. After a short span of time during which they get information on how fit they are, spontaneous firms dynamically self-select out of the industry based on their efficiency: more efficient firms exit at a lower rate and as a result the mean hazard function declines over time.

Figure 3. Hazard functions for subsidised and spontaneous firms



Contrary to the pattern emerging for spontaneous firms, subsidised firms exhibit an apparent spell of <u>increasing hazard rates</u> lasting approximately 5.5 years. In principle, one might say that such pattern is consistent with evidence from other countries. For instance, Harhoff, Stahl and Woywodes (1998) find that failure rates for firms in selected size classes are increasing up to the

[7] As we said, subsidies towards operating costs cover costs borne during the first two years of life, but actual issuing of the subsidies is delayed up to six months.

fifth year of life. The crucial point for our analysis is that such pattern is entirely at odds with the pattern of the hazard rates for a pool of spontaneous firms belonging to the same cohort of birth, operating in the same (two-digit) industries and in the same geographical areas.

To test whether such increasing pattern reflects a true feature of $h(t|44)$ or is trivially due to sampling errors we derived the distribution under the null hypothesis

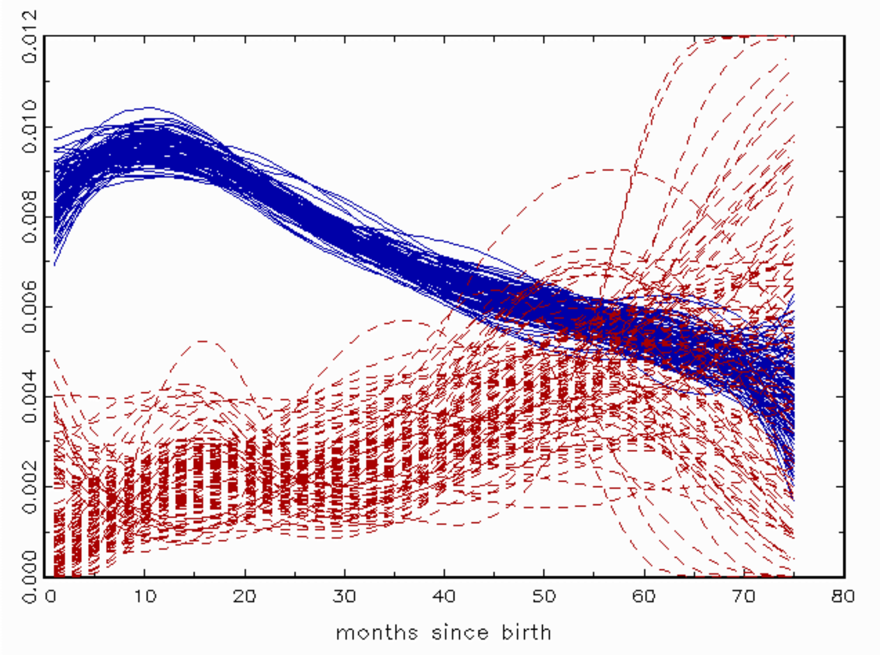
H_0 : $h(t|44)$ constant over time

of a statistic associated to the estimated hazard function. Since under the alternative hypothesis it is *a priori* plausible that $h(t|44)$ starts increasing around the end of the second year, that is when a significant fraction of the subsidies exhaust, we calculate the probability under H_0 to observe a sequence of increasing hazard rates over the time span month 24 to at least month 67 (the observed turning point). The p-value is evaluated by simulating 1000 pseudo-values following the same procedure we describe at the end of section 4 the only difference being that here we replace $h(\tilde{t} \mid I)$ by $\hat{h}_0(I)$ the estimated hazard rate under the null hypothesis. The number of firms quitting at time t is simulated as a drawing from the binomial random variable $Bi(n(t|I), \hat{h}_0(I))$. Pseudo-estimates of the hazard function are obtained on each simulated sequence of firm quits by applying the same estimation and smoothing procedures as on the observed sequence.

The p-value turns out to be .029 signalling a departure from H_0 towards the particular alternative we are considering. A simple explanation for the much longer period over which the hazard function for subsidised firms increases follows from the discussion in section 3. There is a substantial span of time just after the second year of life during which the level of efficiency at selection and the efficiency improvements during the start-up period are not enough high to outperform the incentive to quit as the subsidy declines. In this respect, contrary to the intended goals, the program is yielding a pool of firms whose surviving chances do depend on subsidies.

To appreciate how much sampling variance matters, following Bowman and Azzalini (1997, p. 82) we replicated 100 times the simulation procedure described at the end of section 4 obtaining 100 pseudo-estimates of the hazard functions. In Figure 4 such pseudo-estimates are drawn. As a direct consequence of the much larger sample size, the estimated hazard function for spontaneous firms is more reliable than the one for subsidised firms.

Figure 4. Sampling variance of estimated hazard functions for subsidised and spontaneous firms



To draw the implication of Figure 4 for the efficiency comparison across the two groups notice that somewhere in the range (40,50) the two hazard functions become statistically equal. Let's pretend for a while that in such time range subsidies are already gone. Then we are allowed to use the hazard function as a tool to compare subsidised to spontaneous firms with respect to their efficiency. The conclusion would be that among firms still alive in that time range subsidised firms are on the mean as efficient as spontaneous ones. Even if we could believe the pretence, our conclusions would be much less *pro* programme than it has been advocated so far. To see this notice that in the time range (40,50) approximately 90% and 70% among subsidised and spontaneous firms, respectively, are still alive (see Figure 1). That is, after discarding the worst 10% among subsidised firms we are left with a pool of firms which on the mean is just as efficient as the best 70% among spontaneous firms. In fact, there is little sign of excellence here.

Things are likely to be even worse for the programme, though. Given the size of the subsidy towards investments it is very likely that in the range (40,50) subsidised firms are still bearing a user cost of capital substantially lower than the market one. Hence, the index we are using to compare subsidised firms to spontaneous ones is biased in favour of subsidised firms.

Contrary to the programme advocates claim, comparing the survival chances of subsidised firms to those of spontaneous ones suggests that subsidised firms do not pop out as particularly brilliant.

6. Concluding remarks

In this paper we move from the evidence that subsidised firms survive much longer than comparable (with respect to two-digit industry, location and cohort of birth) spontaneous firms. Over the last decade similar evidence have been used to advocate that enrolled firms are remarkably better than spontaneous firms. Eventually, the European Commission included the L.44 programme in its active labour market Best Practice Policies list.

Our assessment of such claim develops along the following steps. *i*) The programme aims at promoting successful firms which in the absence of the programme would not start. Hence, to conclude that the programme is working one should provide evidence that recruited firms perform well and that they would have not started as spontaneous firms. ii) The only available measure related to firms efficiency is their survival chances which increases as the firm efficiency increases. Unfortunately, since survival chances also increases by providing firms with subsidies any comparison between subsidised firms and spontaneous ones based on such measure is biased towards subsidised firms. iii) To obtain a meaningful comparison across the two groups one should look at the survival chances after the subsidy is gone.

The evidence we obtain out of this line of analysis is much less pro-programme than it has been claimed in the past.

Firstly, our results suggest that after taking into account the survival chances enhancing impact of the subsidy at best selected firms are as good as the best 70% among spontaneous firms. We emphasise 'at best' because there are two reasons to believe things are even worse. i) The previous statement properly applies only to a sub-set of subsidised firms obtained by discarding the worst 10% among selected firms. Apparently, by including the worst 10% the ranking would worsen. ii) Since we cannot completely control for the subsidies the index we use to perform efficiency comparisons is very likely to overstate the subsidised firms efficiency. The intuition for such dramatic contrast between our conclusion and previous claims is simple. Previous analysts did not spell out how much the hazard rates for subsidised firms rest on their efficiency and how much it rests on the subsidy.

Secondly, we found evidence that a few months after their birth they start dying at an increasing rate as the amount of subsidies decreases, a pattern which is peculiar to subsidised firms. A straightforward interpretation of such evidence is that subsidised firms surviving chances depend on subsidies. This is consistent with previous findings on this kind of programmes which led someone to wonder (see Storey, 1993) whether the promotion of starting-up firms is worthwhile.

On the whole, our results cast some doubts on the programme ability to promote firms whose surviving chances do not depend on subsidies. Moreover, as far as spontaneous firms are used as a benchmark there is no evidence of any higher survival chances. In this respect, the answer to the question in the title is rather straightforward: subsidised firms live longer than spontaneous ones just because they are subsidised.

7. Appendix

In this paper we use mainly information from the Social Security files. In Italy each firm with at least one employee at work in the reference period has to fill a monthly report to the Social Security Agency (INPS), with name, location, industry of the firm, the overall number of employees and whether the firm is still active. In order to match the domain of operation of L.44 we considered only data from January 1987 to December 1994, which refer to firms located in the southern regions of Italy. We only included firms that started before December 1992, in order to compute hazard rates on at least 24 months of data.

We used Social Security data both in order to analyse firms which received the subsidy under the scheme and to build our comparison group. A limitation of the Social Security database is that firms in agriculture are excluded, since they are classified under a separate archive. Hence we were unable to match the information for 117 subsidised firms[8]. According to such exclusions, the target population in our analysis is slightly different from the program one.

Identification of L. 44 firms was quite troublesome. In fact, only the names of the firms selected under the scheme are publicly known. Hence we had to retrace their fiscal identification number from a separate archive and match it to Social Security files. In the process we were not able to track 20% of the firms in our target population, missing data being presumably due to either registration errors in the two archives (this is known to be an issue with the Social Security archive) or firms outside the archive domain (firms with no employee; see below). Eventually, we were left with 256 firms.

Besides excluding agriculture, Social Security data have other limitations worth commenting on. Firstly, the files report on firms with a positive number of employees.

Secondly, classification by industry is known not to to be very reliable in the in the Italian Social Security files. Notice however that as far as classification errors do not depend on the firm's status with respect to the program they do not bias our comparison.

On the other hand the Social Security archive exhibits one key feature for our purposes, in that it includes all firms born in the period under consideration, not only those still alive at the time we entered the archive to get the data. Hence, the estimated survival time distributions we get are not length biased.

The comparison group has been constructed by matching to each L. 44 firm (a target of) 50 firms from the archive, spontaneously born in the same geographical area (in the same *provincia*[9], or as a second best, in one of the surrounding *provincie*) and in the same two-digit industry. To compensate for minor unbalancing with respect to location, industry and cohort of birth resulting from insufficient matches in some strata the comparison group have been weighted as described in the main text. Eventually we selected 11375 firms to take part in the comparison group.

---

8 61 subsidized firms were not located in the South due to changes in the eligibility rules which took place in more recent years. They have been ignored in our work. Finally, 48 firms born after December 1992 whose spell of observation is too short for our purposes have been omitted from the analysis.

9 There are 103 *provincie* in Italy

References

Bates, T. (1990): Entrepreneur Human Capital Inputs and Small Business Longevity. The Review of Economics and Statistics LXXII(4), 551-559.

Bowman, A.W., Azzalini, A. (1997): Applied Smoothing Techniques for Data Analysis. Oxford Science Publications, New York.

Brancati, R. (1997): Politiche attive del lavoro. Un intervento di successo nel Mezzogiorno d'Italia: caso o strategia? La legge per la nuova imprenditorialità giovanile. In: Borzaga, C., Brunello, G. (Eds.): L'impatto delle politiche attive del lavoro in Italia. Edizioni Scientifiche Italiane, Firenze.

Caves, R. E. (1998): Industrial Organization and New Findings on the Turnover and Mobility of Firms. Journal of Economic Literature XXXVI(dec.), 1947-1982.

Centosud (1995): Politiche di enterprise creation in Italia, vol.1. Roma, mimeo.

Cox, D.R., Oakes D. (1984): Analysis of Survival Data. Chapman and Hall, London.

Cressy, R. (1996): Are Business Startups Debt-Rationed?. The Economic Journal 106(sept.), 1253-1270.

Dehejia, R., Wahba, S. (1995): Causal Effects in Non-Experimental studies. Harvard University, mimeo.

Dehejia, R., Wahba, S. (1999): Causal Effects in Nonexperimental Studies: Reevaluating the Evaluation of Training Programs. Journal of the American Statistical Association 94, 1053-1062.

European Commission (1997): Joint Employment Report. Directorate General V, Bruxelles.

Jovanovic, B. (1982): Selection and the Evolution of Industry. Econometrica 50(3), 649-670.

Härdle, W. (1990): Applied Nonparametric Regression. Cambridge University Press, London.

Harhoff, D., Stahl, K., Woywodes, M. (1998): Legal Form, Growth and Exit of West German Firms – Empirical Results for Manufacturing, Construction, Trade and Service Industries. The Journal of Industrial Economics XLVI(4), 453-488.

Heckman, J.J., Robb, R. (1985): Alternative Methods for Evaluating the Impact of Interventions. In: Heckman, J.J., Singer, B. (Eds): Longitudinal Analysis of Labor Market Data. Econometric Society Monograph n.10. Cambridge University Press, Cambridge.

Holland, P.W. (1986): Statistics and Causal Inference. Journal of the American Statistical Association 81, 945-960.

Ministero per gli Interventi Straordinari nel Mezzogiorno (1993): Stato di attuazione della legge 28 febbraio 1986 n.44. Mondo Economico (suppl.), n.5.

Pakes, A., Ericson, R. (1998): Empirical Implications of Alternative Models of Firm Dynamics. Journal of Economic Theory 79, 1-45.

Rosenbaum, P., Rubin, D.B. (1983): The central role of the propensity score in observational studies for causal effects. Biometrika 70(1), 41-55.

Rubin, D.B. (1974): Estimating the causal effects of treatments in randomised and non randomised studies. Journal of Educational Psichology 66, 688-701.

Storey, D. (1993): Should we abandon the support to start-up businesses?. Working Paper n.11, Warwick Business School.

Storey, D. (1994): Understanding the Small Business Sector. Routledge, London.

Working Papers già pubblicati

1. E. Battistin, A. Gavosto e E. Rettore, *Why do subsidized firms survive longer? An evaluation of a program promoting youth entrepreneurship in Italy*, Agosto 1998.

Le richieste di working papers vanno indirizzate alla Sig.ra Patrizia Piacentini, Dipartimento di Scienze Statistiche, via C. Battisti 241-243, 35121 Padova, oppure all'indirizzo e-mail patrizia.piacentini@unipd.it.