The Role of Start-up Assistance for New Firms: A Microeconometric Evaluation

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September 2001

Note that this paper is currently under revision.

In this paper, I evaluate the effects of subsidies for start-up firms (start-up assistance) in Germany using a unique firm data set. In order to disentangle the effects of start-up assistance and of non-random assignment to start-up assistance, propensity score matching is applied. Already a short time after assignment, subsidized firms starting between 1990 and 1993 have on average higher capital intensity than matched comparison firms. The effect of start-up assistance on the survival chances of subsidized firms is positive and persists over a long time period, i.e. until the end of the observation period in 1999. However, survival effects vary considerably across industries.

JEL: C14, C50, H25, L21

Keywords: Program Evaluation, Propensity Score Matching, Start-up Assistance, Firm Survival

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Deutsche Ausgleichsbank generously provided access to its internal data base for this research project. I am especially grateful to Daniel Skambracks and Jochen Struck. Financial support by the German Science Foundation is gratefully acknowledged. Moreover, I thank Matthias Almus, Georg Licht and Thorsten Doherr at the Centre for European Economic Research. Valuable comments were received from Dietmar Harhoff, Michael Lechner, Markus Nöth, Thomas Rønde, Konrad Stahl and participants of the 56^{th} ESEM, Lausanne, the 28^{th} Annual Conference of the E.A.R.I.E., Dublin, and the Annual Meeting of the VfS, Magdeburg, in 2001. Research results and conclusions expressed are mine and do not necessarily indicate concurrence by the Deutsche Ausgleichsbank.

1 Introduction

In this paper, I evaluate effects of federal start-up assistance in Germany. This type of subsidy was heavily expanded after the political and economic breakdown of the German Democratic Republic in November 1989. The federal programs administered by the Deutsche Ausgleichsbank (DtA) offer loans with subsidized interest rates and redemption-free periods. More than 22 billion Euro were handed out to young firms between 1990 and 1994. Similar and often long-established programs exist in the U.S., in the U.K. and many other countries in the European Union.[1] Moreover, micro credit programs promoting self-employment and small businesses in Eastern European transition countries will be extended in the near future (Economist 2001). Facing a large number of existing financing programs and the plans for the future, evaluation studies on such programs are very valuable.

The main rationales usually given for subsidizing entrepreneurs who start new firms are the impact of financial constraints on entrepreneurial decisions and the role of new firms for job creation, market restructuring and technological progress. Job creation by young and small firms has inspired policy makers since the widely known and often criticized study of Birch (1979).[2] The population of start-ups in an economy is considered as a counterbalance to concentration tendencies in established industries. In addition, it is a source of new industry formation (Beesley and Hamilton 1984). Finally, young or small innovative firms can contribute to the development and diffusion of new technologies and products.[3]

Young firms are often argued to face higher costs of external capital and a higher risk of capital rationing than older firms. Stiglitz and Weiss (1981) model how informational asymmetries between borrowers and lenders on financial markets can lead to credit rationing. According to Hubbard (1998) and Gompers and Lerner (1999) informational asymmetries are particularly severe in the case of young firms because these firms face idiosyncratic risks that are difficult to evaluate externally. In addition, monitoring young firms is relatively expensive because the involved loans are usually small. Entrepreneurs often emphasize the lack of personal equity and insufficient collateral to obtain debt financing as the major impediment to start and enlarge their investment and innovation activity. Moreover, new firms are usually too young and too small to issue public debt or equity. Evidence presented by Evans and Jovanovic (1989), Holtz-Eakin, Joulfaian, and Rosen (1994a), and Taylor (2000) is consistent with entrepreneurs facing binding financial constraints when deciding upon firm formation and capital use. Empirical results of Holtz-Eakin, Joulfaian, and Rosen (1994b) and Honjo (2000) suggest that financial constraints reduce the survival chances of young firms. However, Cressy (1996a) and Taylor (2000) provide no support for this view.[4]

[1] See Lerner (1999) for a list of U.S. programs, Storey (1994b) for a discussion of U.K. programs, and De, Kaufmann, Niederbach, and Wimmers (1995) for details about programs in several European countries.

[2] See Davis, Haltiwanger, and Schuh (1996) for a more recent investigation of job creation and destruction.

[3] See among others Scherer and Ross (1990) or Geroski (1991).

[4] In addition to the few studies on financial constraints of newly started or very small firms a large literature addresses the impact of financial constraints on firms' investment activities in general. Most of the authors

In accordance with the rationales discussed, the main objective of federal start-up assistance in Germany is to prevent sub-optimal start-up capitalization of ex ante efficient projects and sub-optimal investment activity after market entry. New firms with start-up assistance are expected to perform better and to have higher survival chances than they would in case of a more financially constrained entry. In this study, I focus on firm survival effects because the struggle to survive usually dominates the initial years of start-ups. Precisely, I analyze short- and long-run effects of federal start-up assistance on the survival chances of new firms started between 1990 and 1993 in all major sectors of the East and West German economy.

To evaluate whether start-up assistance has the intended positive effect on the survival chances of subsidized firms, program effects and effects of non-random selection of program participants have to be disentangled. This crucial identification challenge in the context of program evaluation is ignored by most existing studies about the performance of subsidized start-ups in Germany.[5] Moreover, only a few studies use an adequate parametric or non-parametric approach to evaluate one of the programs subsidizing investment activities like research and development, self-employment or firm formation in Europe, U.S. or other countries.[6] In this paper, I apply non-parametric matching on the balancing score as proposed by Rubin (1974) and Rosenbaum and Rubin (1983). Program effects are estimated as the difference between the survival chances in the group of subsidized firms and a matched comparison group. This comparison group is constructed such that it can be assumed to inform about the survival chances of the subsidized firms in the counterfactual situation without subsidy. Similar non-parametric techniques have recently been applied by Dehejia and Wahba (1998, 1999), Heckman, Ichimura, and Todd (1997, 1998), and Lechner (1999, 2000) to evaluate labor market programs. So far, a non-parametric evaluation study of the effects of federal start-up assistance in Germany on firm survival does not exist.

For this study, I can use a large, unique data set containing firms with start-up assistance from the DtA and firms without such assistance. To construct this data set, the internal data base of the DtA was for the first time connected with an external firm sample. The sample of 22,000 firms was randomly drawn from two complementary panel data bases build up with data from the leading German credit rating agency. Additional telephone survey data were collected in 1999.

In the first part of the empirical analysis, determinants of firm selection for federal start-up assistance in Germany are examined. This is interesting in its own right, because the assignment process for start-up assistance in Germany has rarely been investigated so far. It can be shown that, all else equal, better qualified entrepreneurs with less risky projects have higher chances to receive start-up assistance. The evaluation of start-up assistance effects

conclude that financial constraints have a considerable impact on investment decisions. See Hubbard (1998), Schiantarelli (1996), and Winter (1998) for surveys.

[5] See Breitenacher et al. (1994) for a survey of studies that provide simple, unadjusted comparisons between subsidized and non-subsidized firms. Hinz and Ziegler (2001) compare subsidized firms to bank-financed firms and firms without external finance.

[6] I discuss such studies in section 2.3.

on the firm survival chances reveals strong short-run effects of start-up assistance. Most important, the analysis of effect changes over a long time period indicates that positive firm survival effects can not be judged to be a pure result of inefficient "cash-and-carry"-behavior. Subsidized firms do not simply live on the provided loan for some years and exit some time later than matched comparison firms. By contrast, the group of subsidized firms and of matched comparison firms face the same declining instantaneous liquidation risk from the seventh year after market entry between 1990 and 1993 until the end of the observation period in 1999. Hence, the significant and substantial, positive effect of subsidized loans handed out to firms at the moment of start-up is found to persist over a long time period. However, it has to be stressed that the effectiveness of federal start-up assistance varies considerably across industries. Most important, in the transport and communication as well as in the service sectors no significant positive effects can be identified.

The remainder of the paper is set out as follows. Next, I briefly discuss the programs I evaluate, the selection of program participants and related empirical literature. In section 3, I explain the identification problem typically arising in the context of program evaluation and the chosen estimation method. Section 4 contains a brief description of the data base and descriptive statistics characterizing the groups of subsidized and non-subsidized firms in the sample. Empirical results are presented in section 5 and section 6 concludes.

2 Start-up assistance in Germany

2.1 Federal financing programs for young and small firms

In this section, I briefly describe the federal programs subject to evaluation here and sketch the economic situation in Germany at the beginning of the 1990s. All major West German financing programs for young and small firms were extended to East Germany shortly after the political and economic breakdown of the German Democratic Republic in November 1989. During the transition from a planned to a market economy new firms were urgently needed to introduce new technologies, to build up viable industry structures, and to create new jobs. This was the case because the formerly state-owned and mostly large East German firms underwent fundamental restructuring and privatization or liquidation. The restructuring of these firms with a total of more than four million employees forced into unemployment a large fraction of the work force. Industry production largely broke down. Private venture capitalists played only a minor role during the economic transition in East Germany.[7] By contrast, the DtA provided about 22 billion Euro in form of 383,112 loans to young firms within the first five years after the breakdown. The DtA is the second largest public bank in Germany. It handled about 90 percent of all federal subsidized start-up loans during the

[7] Lessat et al. (1999) show that start-up financing by private venture capitalists in Germany never reached the level of 10 million Euro per year before 1994 and only started to increase significantly from 1995 onwards.

1990s.[8]

According to table 1, the DtA provides start-up assistance almost exclusively via one of the following three programs: ERP equity capital assistance program (EKH), ERP business start-up program and DtA business start-up program.[9] The EKH program offers equity capital assistance by providing loans for up to 20 years without redemption during the first 10 years. For East German firms, the interest rate is fixed at zero during the first three years but later on the yearly interest rate increases stepwise. For West German firms, the interest rate is fixed at zero only during the first two years. Collateral is not needed but the applicant is personally liable. These loans count as equity substitute for private lending institutions and may therefore induce further lending. Both, the ERP and the DtA business start-up program provide loans for about 10 to 15 years with subsidized interest rates. In the ERP business start-up program no redemption is due for a maximum of 3 years in West Germany and 5 years in East Germany. In the DtA business start-up program the redemption-free period is restricted to at most 2 years. Collateral or a guarantee provided by a private lender or within a loan guarantee program are necessary.[10]

The variation of the yearly volume and number of DtA loans is shown in table 1. In East Germany it closely reflects the development of the firm formation numbers over time. As East German firm formations per month rose steadily until July 1990 and remained on a very high level until 1991, the volume and number of DtA loans peaked in 1991/1992. After 1991, firm formations and correspondingly the number of DtA loans decreased in East Germany. The decline of the volume started after 1992. In West Germany the level of start-up assistance is much lower than in East Germany.

2.2 Selection of program participants

In the context of program evaluation understanding the selection of program participants is of key importance. Assignment to federal programs promoting start-up activity in Germany depends on the decisions of entrepreneurs, banks and the subsidizing institution DtA as shown in figure 1.

Entrepreneurs may not apply for start-up assistance because of lacking information. Especially East-German firm founders may not have known about the existence of the programs shortly after unification. Moreover, entrepreneurs may not apply because they either need no

[8] To finance the programs the DtA relied on the European Recovery Program (ERP) Fund, funds from the federal budget, and raised additional funds on international capital markets. The ERP Fund was created with Marshall Plan aid Germany received after the Second World War. The federal government extended it substantially after the German unification at the beginning of the 1990s.

[9] Within these programs the DtA subsidizes mainly start-ups but also expanding young or small firms making growth investments or restructuring after ownership changes. As firms with such subsidized expansion financing are not considered in the empirical analysis, I provide no details on expansion financing in the following.

[10] During the 1990s, several federal loan guarantee programs offered guarantees, especially to East German firms.

external financing or anticipate a refusal. Entrepreneurs who dislike interference by outsiders in general, even when it does not involve the loss of any firm control rights, should not be expected to use start-up assistance.[11]

Banks have a strong influence on program assignment due to the so-called "house bank principle". It requires applications for start-up assistance to be passed on to the DtA by a bank willing to handle the potentially approved loan. This mediator role of banks is likely to imply selective program assignment for the following reasons. When a bank is contacted by an entrepreneur, it may refuse to provide venture financing and to pass on a program application to the DtA. Such a refusal occurs if the bank classifies as insufficient either the entrepreneur's abilities or the project's quality. Moreover, diversification can cause customer refusal if the bank has already accepted many similar customers. Concerning accepted customers, a bank has to decide whether it provides all needed external financing or proposes the customer for DtA loans and provides only complementary financing.

Finally, DtA officials decide upon approval of start-up assistance. First, they have to check whether an application fulfills the program requirements for start-up assistance. These can be summarized as follows:

1. The firm project is found promising, insufficiently financed and not already ongoing when the entrepreneur applies for assistance.

2. The applying entrepreneur is sufficiently qualified to pursue the project and preferably less than 56 years old. Entrepreneurs who start their first firm project are preferred.

3. The firm is not fully affiliated to a parent firm, i.e. no subsidiary.

Second, DtA officials may deviate from selection according to these program requirements because of distorted incentives.[12] The DtA needs political support for continuing its programs. The political debate about start-up assistance in Germany is controversial and causal effects of start-up assistance on success perspectives of otherwise financially constrained firms are difficult to measure and to communicate. Thus, DtA officials may be tempted to select projects with high success chances even if these could be financed privately. Such a "picking-the-winner"-strategy guarantees a high number of survivors among the subsidized firms that could be used to proclaim program success.

Despite these arguments, the decisions by DtA officials are probably the least important source of selective program assignment. As can be seen in table 1, the DtA refuses only a few percent of all loan applications between 1990 and 1994 in East and West Germany. Only in 1990 and 1991 the refusal rates for West German firms reach about 9 percent. These high

[11] See for example Hutchinson (1995) and Cressy and Olofsson (1997) on such demand-sided financial constraints.

[12] Distortions related to the provision of subsidies are addressed in a large public finance and public choice literature. See Laffont and Tirole (1993) for a detailed discussion of principal-agent problems in procurement and regulation.

West German rates simply reflect the fact that most of the program funds available right after the unforeseen breakdown of the German Democratic Republic in 1989 were needed for subsidizing start-ups in East Germany.

2.3 Related empirical literature

A large number of microeconometric evaluation studies focuses on labor market programs offering job training, wage subsidies and job search assistance.[13] By contrast, evaluation studies on firm programs are still rare.

In their surveys on research and development (R&D) subsidies, Klette, Møen, and Griliches (2000) and David, Hall, and Toole (2000) cannot include but a few firm-level studies addressing the need to control for selective program participation when analyzing R&D program effects on firm performance. Lerner (1999) and Wallsten (2000) evaluate the largest U.S. initiative subsidizing R&D activity in small high-technology firms, the Small Business Innovation Research (SBIR) program.[14] Their studies differ with respect to the econometric approach and the time horizon of the evaluation.[15]

Lerner (1999) analyzes long-run effects on firm performance 10 years after program participation. Three ex ante plausible reasons for effects of SBIR awards are discussed: encouragement of innovation activities with positive externalities, information provision to potential investors by certifying firm quality and distortions in the process of award allocation. Despite the consideration of selection effects, Lerner models no program selection equation. Instead, he investigates how the effects of SBIR research awards on sales and employment vary between industries and regions. The data base contains about 550 firms with research awards and 300 comparison firms with initial evaluation awards as well as 600 non-subsidized comparison firms matched with respect to size and industry or location. OLS regressions show no significant correlation of employment or sales growth between 1985 and 1995 with an indicator variable for research awards before 1985. But employment and sales are significantly positively correlated with an interaction between the award dummy and the regional volume of early-stage financing provided by venture capitalists. In industry-specific regressions this result is valid only for high-technology industries. An indicator for multiple awards before 1985 is not significantly positively correlated with firm growth between 1985 and 1995. Taken together, the results are consistent with a certification role of SBIR awards for investors but also with distortions in the process of award allocation.

Wallsten (2000) studies short-run effects of SBIR awards granted between 1990 and 1992 on firm employment in 1993. His simultaneous, fully parametric model consists of two equations explaining the received number of evaluation and research awards and an outcome equation.

[13] See Heckman, LaLonde, and Smith (1999) for a survey of this literature.

[14] To finance the program, federal agencies have set aside a percentage of their external R&D budget since 1982 - so far more than US$ 10 billion.

[15] Busom (2000) and Toivanen and Niininen (2000) present related studies on R&D programs directed not only to small but also large firms.

The total SBIR budget from which a certain firm can win awards serves as instrumental variable to identify the endogenous award variable in the outcome equation. Wallsten (2000) uses data on about 370 awardees, 90 rejected firms and 22 eligible, publicly traded firms that did not apply. According to the three stage least-squares estimates, large and patent-intensive firms, i.e. firms with good growth and innovation perspectives receive more SBIR grants than others. But SBIR research awards increase subsequent firm employment only insignificantly. Therefore, selective award allocation causes the significant correlation between SBIR grants and employment appearing inappropriate in single equation model. Moreover, estimates for the small sub-sample of 81 publicly traded firms show that the awarded dollar amount crowds out firm-financed R&D expenditures in 1992, i.e. shortly after the awards have been granted.

The following microeconometric studies evaluate firm programs that do not address R&D activity but self-employment and entrepreneurship. Pitt and Khandker (1998) look at three major group-based micro credit programs in Bangladesh.[16] Within these programs, non-agricultural self-employment of persons from poor rural households is financed and accompanying non-financial services are provided. The data base is a stratified random sample with about 300 households non-eligible for program participation and 1,500 eligible ones. 900 households were program participants. Pitt and Khandker (1998) use simultaneous, fully parametric models with two gender-specific participation equations and an outcome equation. They take unobservable village- and household-specific heterogeneity into account by fixed-effect estimation and by exploiting discontinuity of program eligibility rules with respect to land ownership. Accordingly, results depend on the assumption of independence between land ownership and unobserved household-specific characteristics.[17] Estimation results show that controlling for unobservable heterogeneity matters. The main evaluation results are significant positive effects of credit amounts borrowed by program participants since 1986 on household expenditure, women's asset holding as well as school enrollment of boys and a significant negative effect on men's labor supply measured in 1991 or 1992.[18] Moreover, the effects of lending to women tend to be stronger than those of lending to men.

Pfeiffer and Reize (2000) analyze the effects of a small German program providing small subsidies, called bridging allowances, to unemployed people who try to build up self-employment. The authors use a simultaneous, fully parametric model of program assignment and firm performance to take selection effects into account. Their data set contains about 320 subsidized firms and more than 10 times as many non-subsidized firms. Due to data restrictions Pfeiffer and Reize (2000) can only evaluate short-run effects during the first two years after switching to self-employment, and they have to use a noisy firm survival indicator. According to the estimation results for East Germany, receipt of bridging allowances is negatively related to the firm survival indicator and the result is significant at the 7-percent level. The regression

[16] Morduch (1999) surveys recent work on microfinance.

[17] See Morduch (1998) for a critic of this assumption and the one used to identify gender-specific participation.

[18] These results can only partly be reconciled with those of Morduch (1998). He presents difference in difference comparisons using the same data base as Pitt and Khandker (1998).

for West Germany indicates a negative, but insignificant impact. Furthermore, no significant effect of receiving bridging allowances on employment growth can be identified.

Battistin, Gavosto, and Rettore (2001) evaluate an Italian program started in 1986 and promoting youth entrepreneurship in southern Italy. The sample contains about 250 subsidized and a large pool of non-subsidized firms. Following Dehejia and Wahba (1999), the authors choose a non-parametric approach closely related to the matching procedure I apply to construct a comparison group for evaluating program effects. The comparison group they use is similar to the group of subsidized firms only with respect to three crude variables: industry classification, location and firm formation cohort. Subsidized firms survive much longer than firms in the comparison group but after five years the hazard rates of both groups range at the same level.

3 Evaluation and Econometric Methodology

3.1 Definition of causal effects and identification

The following question is addressed in this evaluation study: What is the average effect of receiving start-up assistance on the survival chances of subsidized firms compared to the counterfactual situation where these firms received no assistance? To answer this question the causal effect of assignment to start-up assistance on the chosen outcome variable, i.e. survival chances, has to be estimated. Simply reporting the empirical association between assignment and outcome cannot answer it. A suitable framework to guide the empirical analysis is the model of potential outcomes and causal effects introduced by Roy (1951) and Rubin (1974).[19] Within this framework, the question of interest can be formalized as the average treatment effect on the treated firms, denoted by θ:

$$\theta := E(Y^t - Y^c \mid S = 1) = E(Y^t \mid S = 1) - E(Y^c \mid S = 1). \tag{1}$$

Two potential outcome variables, Y^t and Y^c are defined. Y^t denotes the outcome in state t with treatment and Y^c denotes the outcome in the comparison state c without treatment. For each firm either Y^t or Y^c can be observed depending on the treatment assignment mechanism. The binary assignment indicator S shows whether a firm belongs to the group of treated, i.e. subsidized firms ($S = 1$) or not ($S = 0$). $E(Y^t \mid S = 1)$ and $E(Y^c \mid S = 1)$ denote the expected values of Y^t and Y^c for the group of treated firms ($S = 1$). To allow for causal analysis in this framework the so-called stable unit treatment value assumption (SUTVA) is supposed to hold. SUTVA states that the value of a potential outcome for firm i, with index i running over all firms in the population, is the same for all variations of the treatment allocation in the population giving firm i the same sort of treatment.[20] Most important,

[19] See Holland (1986) and Prantl (1997) for a more detailed discussion.

[20] See Rubin (1990) and Angrist, Imbens, and Rubin (1996).

SUTVA implies that indirect and general equilibrium effects of start-up assistance are ignored in the following.[21]

θ cannot be identified directly because the sample analogue of $E(Y^c \mid S = 1)$, i.e. the mean of the outcome in the state without treatment for treated firms, is not observable. Instead, the lack of identification has to be overcome by data collection design or plausible assumptions. Angrist and Krueger (1999) and Heckman, LaLonde, and Smith (1999) review such identification strategies. If the data at hand were collected in an experimental context with random assignment of firms to start-up assistance, the potential outcomes would be independent from the assignment mechanism. $E(Y^c \mid S = 1) = E(Y^c \mid S = 0)$ would hold and the untreated firms could serve as an adequate comparison group. The mean of the outcome Y^c in the comparison group could be used to estimate the unobservable $E(Y^c \mid S = 1)$ and thus the causal effect θ.

To evaluate start-up assistance in Germany I have to use non-experimental data. Several factors discussed in section 2.2 affect assignment to start-up assistance as well as the outcome survival chances. Thus, the requirement of random assignment is not fulfilled.[22] Suppose that all factors influencing assignment and outcome are captured by a vector X of observable covariates unaffected by the treatment. Then, a weaker condition, called random assignment conditioning on covariates, allows to construct an adequate comparison group (Rubin 1977). According to this conditional independence assumption (CIA), the potential outcome in the state without treatment is independent of assignment conditional on X taking a value x:

$$Y^c \amalg S \mid X = x \tag{2}$$

where $\amalg$ denotes independence. If CIA is valid in all the support of X, then $E(Y^c \mid S = 1, X = x) = E(Y^c \mid S = 0, X = x)$. To see how the causal effect θ can be estimated based on CIA, rewrite the not directly identifiable part of equation (1) as follows: $E[Y^c \mid S = 1] = E[E(Y^c \mid S = 1, X = x) \mid S = 1]$. This shows that the sample analogue of $E[E(Y^c \mid S = 0, X = x) \mid S = 1]$ in a sufficiently large sample can be used to estimate $E[Y^c \mid S = 1]$.

3.2 Estimation Method

Conditioning on $X = x$ can be implemented by a procedure of exact matching. Each treated firm has to be matched to an untreated one equal with respect to X. Then, $E[Y^c \mid S = 1]$ can be estimated by the sample analogue of $E[E(Y^c \mid S = 0, X = x) \mid S = 1]$ which is the

[21] See section 6 for further discussion because firm financing programs are unlikely to fulfill SUTVA literally. This is also the case for most other programs addressing economic agents interacting on markets. R&D-programs, for example, impose even stronger problems with SUTVA because they are usually just intended to generate spillover effects.

[22] See for example Bates (1990), Brüderl, Preisendörfer, and Ziegler (1992) or Cressy (1996a) on factors affecting firm survival chances. These are likely to also determine program assignment according to section 2.2.

corresponding mean in the matched comparison group of untreated firms. Obviously, such a method can hardly be applied if X is a high-dimensional vector. Rosenbaum and Rubin (1983) discuss a property useful to reduce dimension. Define the propensity score $P(X)$ as assignment probability conditional on $X = x$ with $P(x) = P(S = 1 \mid X = x)$ and $0 < P(x) < 1$. In addition, define the balancing score $b(X)$ as a function of X with the following property: $E[P[S = 1 \mid b(X) = b(x)]] = P(x)$. If CIA holds, i.e. if the potential outcomes are independent of assignment conditional on $X = x$ then they are also independent of assignment conditional on $b(X) = b(x)$. It follows that: $E[Y^c \mid S = 1, b(X) = b(x)] = E[Y^c \mid S = 0, b(X) = b(x)]$.

Conditioning on $b(X) = b(x)$ rather than on $X = x$ can be realized by matching each treated firm to an untreated firms similar in terms of $b(X)$ such that the distribution of the balancing score in the treatment group equals the one in the matched comparison group. Due to the balancing score property the corresponding distributions of X will then also be balanced. $E[Y^c \mid S = 1]$ can be estimated by the sample analogue of $E[E(Y^c \mid S = 0, b(X) = b(x)) \mid S = 1]$ in the matched comparison group. Matching on the balancing score simplifies the estimation problem whenever $b(X)$ is defined as having a lower dimension than X. The most simple matching procedure, called matching on the propensity score, just uses the one-dimensional assignment probability $P(X)$ for matching. $P(X)$ and consequently any dimension reducing $b(X)$ is usually unknown and must be estimated. In this study the estimation of the assignment model is interesting in itself because the assignment process for start-up assistance in Germany has rarely been investigated so far.

In the following I use matching on the balancing score to estimate the treatment effect θ in equation (1). Recent applications of matching estimators can be found in Angrist (1998), Dehejia and Wahba (1998, 1999), Heckman, Ichimura, Smith, and Todd (1998) and Lechner (1999, 2000). Matching estimators are more intuitive and simpler than for example nonparametric kernel estimators. In contrast to parametric approaches, matching estimators allow for various functional forms of the conditional expectations. Moreover, no restrictions have to be imposed on individual causal effects, i.e. effects can be heterogeneous within the population.[23]

The applied estimator allows for matching several treated firms to the same comparison firm as proposed by Dehejia and Wahba (1998) and Lechner (2001a). Such a matching procedure with replacement is useful in the context at hand because some types of firms are frequent in the group of treated firms but not among potential comparison firms (see section 5.2). The estimation algorithm proceeds as follows:

1. Specify and estimate a binary choice model to obtain an estimate of the propensity score. Use the sample of all treated and potential comparison firms for estimation. Given choice-based sampling of the data as explained in section 4.1 apply a weighted maximum likelihood estimator as proposed by Manski and Lerman (1977).

[23] See Heckman, Ichimura, Smith, and Todd (1998), Lechner (1999) and Rubin (1990).

2. Construct the balancing score vector $\left[x'\hat{\beta}/\hat{\sigma}, v\right]$ by using the predicted unbounded propensity score $x'\hat{\beta}/\hat{\sigma}$ and the vector v with all particularly important components of X.[24]

3. Choose a firm j belonging to the treatment group with $j = 1, ..., N^t$ and remove it from that group.

4. Find the firm in the group of potential comparison firms that is as close as possible to the firm chosen in step 3 in terms of the balancing score vector. To do this, compute for each firm k in the comparison group with $k = 1, ..., N^p$ the following distance to firm j chosen in step 3:

   $d_{jk} = \left(x_j'\hat{\beta}/\hat{\sigma_j}, v_j\right)' - \left(x_k'\hat{\beta}/\hat{\sigma_k}, v_k\right)' \qquad \forall \quad k = 1, ..., N^p$

   Then compute for each firm combination the Mahalanobis distance measure

   $MD_{jk} = d_{jk}' Cov^{-1} d_{jk} \qquad \forall \quad k = 1, ..., N^p$

   where Cov is the estimated variance-covariance matrix of $\left[x'\hat{\beta}/\hat{\sigma}, v\right]$ in the group of potential comparison firms. Choose the nearest neighbor to firm j as its matched comparison firm, i.e. firm k with the smallest Mahalanobis distance. Do not remove k from the group of potential comparison firms such that it can be used again.

5. Repeat steps 3 and 4 until no firm in the treatment group is left.

6. Check the quality of the matching performed in steps 3-5. If the distribution of X in the treatment and the matched comparison group are insufficiently balanced, refine the specification of the program assignment model and repeat steps 2-5 (see section 5.2).

7. Estimate the average treatment effect on the treated using the matched comparison group (see section 5.3). The estimator can be written as

$$\hat{\theta} = \frac{\sum_j w_j \cdot y_j^t - \sum_l w_l \cdot y_l^c}{\sum_j w_j} \quad \forall \quad j = 1, ..., N^t \quad \text{and} \quad \forall \quad l = 1, ..., N^m. \tag{3}$$

   The variable y_j^t denotes the outcome observed for the treated firm j and y_l^c the outcome observed for the comparison firm l which is matched to at least one treated firm. The variable w_j indicates the sampling weight of firm j. The variable w_l denotes the sampling weight of a matched comparison firm l. It is calculated as the sum of weights of all treated firms (j) to which comparison firm l is matched: $w_l = \sum_{(j)} w_{(j)} \; \forall \; (j) = 1, ..., N^l$ with $N^l \leq N^t$ and $\sum_l N^l = N^t$. The construction of w_l implies $\sum_j w_j = \sum_l w_l$.

[24] According to Lechner (2000) using the linear index $x'\hat{\beta}/\hat{\sigma}$ instead of the bounded propensity score $\Phi\left(x'\hat{\beta}/\hat{\sigma}\right)$ avoids the asymmetric assessment of comparison firms occurring if $\Phi\left(x'\hat{\beta}/\hat{\sigma}\right)$ is close to 0 or 1 and matching is based on a symmetric metric.

The estimator $\hat{\theta}$ can be interpreted as a difference between the weighted averages of the observed outcomes in the treatment group and the matched comparison group.[25]

4 Data and descriptive statistics

4.1 Data bases

This study is based on a large, unique data set containing firms with start-up assistance from the DtA and firms without such assistance. To construct the data set, the internal data base of the DtA was for the first time connected with an external firm data base.

The external data base used is a stratified random sample of 22,000 firms from two large, complementary firm panels kept at the Centre for European Economic Research (ZEW). 10,000 firms were drawn from the East German panel covering about one million firms in September 1999. Another 12,000 firms were drawn from the West German panel with about 1,600,000 firms in September 1999.[26] The data for both panels are provided by the leading German credit rating agency, Creditreform, approximately every six months. Information collection by Creditreform is an ongoing process and the frequency of information updating varies among firms. Therefore, the panels differ from panels based on data explicitly collected for scientific purposes.[27] A typical firm record in the panels contains address data and information on characteristics of the firm and its owners. Information on firm formation and liquidation events is also available.

The sample covers all East and West German regions and all industries in the manufacturing, construction and trade sector as well as most service industries. For all firms in the sample Creditreform registered a formation date between January 1, 1990 and December 31, 1993. Panel data is available for the period until December 31, 1999 such that short- and long-run effects of start-up assistance can be evaluated.

Two characteristics of the sample are important in the context of a firm survival analysis: the quality of the liquidation data and the sampling rule. Survival analyses using only the information about the firm record status encoded by Creditreform provide no valid survival estimates. However, Creditreform stores a lot of information on firm liquidations in free flow text. I extracted this information from about 4,000 text pages for all 22,000 firms

[25] The variance of the matching estimator is computed as proposed by Lechner (2001a). The applied formula imposes the assumptions of independent observations, of fixed propensity score values and of fixed weights. The latter is a simplifying assumption because the propensity score values are derived from a binary probit estimation and the weights w_l are determined within the matching procedure. The approximate variance estimator had to be used because inference based on bootstrapping would have implied a prohibitively high computational burden in the context of this study. See Lechner (2002) for an application where the results based on variance approximation and on bootstrapping differ only slightly.

[26] See Almus et al. (2001) for details about the sample construction.

[27] More details about panels based on Creditreform data can be found in Stahl (1991), Harhoff, Stahl, and Woywode (1998), and Almus, Engel, and Prantl (2000).

in the sample. A large telephone survey conducted in 1999 provides further data about the activity status of 5,928 firms in the sample.[28] Hence, reliable liquidation data can be used for this study. When drawing the sample, firm groups having a high liquidation risk according to Creditreform's encoded information were oversampled approximately twofold. This disproportional stratified choice-based sampling feature guarantees sufficient variation in the sample to analyze firm survival but it has to be taken into account in all estimation and test procedures.[29]

4.2 Identification of the treatment group and construction of the final sample

To merge the internal DtA data base and the firm sample a computer based search algorithm was used.[30] The algorithm linked all data entries in both data bases by means of a heuristic comparison procedure if coincidence of firm and owner-specific names, address information and birth dates of firm owners was high. Then, extensive manual checking of the computer-generated links was conducted. Several tests of data consistency indicated a highly reliable merge between both data bases. Altogether, 4,571 of the 22,000 firms in the firm sample were found to be subsidized by the DtA.

The econometric analysis is based on a reduced sample with 7,874 firms because of several exclusion restrictions discussed in detail in appendix B. The applied restrictions can be summarized as follows. Only start-ups between 1990 and 1993 with less than 50 employees at market entry in firm groups eligible for the considered financing programs are kept in the sample. The firm records had to provide the needed information about the firm and the entrepreneur. A few firms with data inconsistencies were eliminated. I discarded the firms participating in DtA programs not evaluated here and those receiving DtA loans in the second year after their firm formation year or even later. The latter avoids mixing the effects of start-up assistance with those of expansion assistance also given within the same programs. As these restrictions reduced the sample size considerably, several alternative regressions were estimated for larger samples.[31] The alternative results give no reason to worry about sample selection affecting the estimation of the program participation model or the evaluation of program effects on firm survival. Table 8 contains descriptive statistics for the final sample.[32]

The final sample covers 1,351 firms with start-up assistance, called treatment, and 6,523 untreated firms. These constitute the group of potential comparison firms. On average, the

[28] Almus et al. (2001) describe this telephone survey and the resulting data base called "ZEW Entrepreneurship Study".

[29] See Manski and McFadden (1981) for a detailed discussion of the topic.

[30] The merge procedure is described in more detail in appendix A.

[31] See appendix A for details.

[32] Definitions of all firm- and entrepreneur-specific variables based on Creditreform data and of several location-specific variables from the statistics of the Bundesanstalt für Arbeit, the Bundesamt für Bauwesen und Raumordnung and the Bundesbank can also be found there.

treated firms received 70,103 Euro as ERP or DTA start-up loan and 38,486 Euro as ERP equity capital assistance.[33] According to the Deutsche Bundesbank interest rates of ERP and DtA start-up loans are on average about 2.5 percentage points lower than respective market rates (Deutsche Bundesbank 1991).[34] Interest rate subsidies in the EKH program are much higher as already indicated in section 2.1.

4.3 Differences between the groups of treated and untreated firms

Subsidized firms do not constitute a random sample from the sample population according to the descriptive statistics of selected variables presented in table 2. For each variable I conducted a two-tailed test of the null hypothesis stating equality of weighted means in the treatment group and the group of potential comparison firms.[35] The test results indicate many statistically significant and substantial differences between both groups with respect to firm- and entrepreneur-specific characteristics. In comparison with untreated firms, treated firms are more often franchisees. They are more often organized as sole proprietorships or commercial partnerships and less often as limited liability firms or stock corporations. Affiliations to incumbent firms occur less often in the treatment group than among potential comparison firms. Entrepreneurs in the treatment group hold more often a high educational degree like a master craftsman degree or a university diploma in engineering than those in the group of untreated firms.[36] Moreover, they belong more often to the medium age class (30-44 years), less often to the lowest age class (17-29 years) and are more often female. Treated firms are started more often in 1991 and 1992 than untreated ones and less often in 1990 and 1993.

According to table 2, both groups differ also significantly with respect to industry classification and location-specific characteristics. Treated firms belong more often than untreated firms to the sectors manufacturing, construction as well as retail trade and less often to wholesale and intermediate trade, transport and communication, and services. The majority of untreated firms are located in West Germany in contrast to the majority of treated firms. Compared to untreated firms, treated start-ups are more likely to be found in weakly populated districts and in districts with high unemployment rates. Finally, they are more frequent in districts

[33] 64.99 percent of the treated firms received equity capital assistance and also at least one loan in the DtA or ERP business start-up program. 6.88 percent of the treated firms received nothing but equity capital assistance and 28.13 percent only received loans in the business start-up programs.

[34] Presumably, this comparison underestimates the subsidy because private lenders typically charge start-up firms above-market rates.

[35] Test results for the East and the West German subsamples are not displayed because nearly all results discussed in the following hold for both parts of the sample.

[36] 28.79 percent of all start-ups in the sample have several entrepreneurs, i.e. several owner persons with management function. In such cases, I selected the entrepreneur-specific information of the managing owner with the highest equity share. In case of firms without share data the entrepreneur-specific information of the oldest managing owner was chosen instead. Alternative ways of encoding were tested, but did not lead to different results.

with a high bank office density and a low share of bank offices belonging to commercial banks. Accordingly, the selection process seems to depend on characteristics of the banking industry at the firm location.

Especially the substantial group differences with respect to firm- and entrepreneur-specific characteristics suggest a selection into DtA programs depending on variables that also influence firm survival. Therefore, the selection process must be controlled for when estimating program effects. One appropriate approach has been discussed in section 3 and will be applied in the next section.

5 Empirical Results

In the following, I present the results of analyzing assignment to start-up assistance and of evaluating its effects on subsidized firms. Section 5.1 contains the estimation results of the program assignment model. These inform about determinants of assignment to federal start-up assistance in Germany and are essential prerequisites for the evaluation of causal effects by applying a matching estimator. The subsequent application of the matching procedure and the achieved balancing quality are described in section 5.2. In section 5.3, I discuss estimated long-run effects on the firm survival quota and the effects on firm size and investment per employee shortly after assignment. Moreover, effects on survival as well as hazard functions during a period of nine years are considered.

5.1 Program assignment

A fully parametric binary probit model is used to estimate the program assignment equation. Since the observations are choice-based sampled from the parent population I apply the weighted maximum likelihood estimator introduced by Manski and Lerman (1977). The dependent variable S is coded as 1 if firm i received start-up assistance in the firm formation year or the subsequent year and 0 otherwise for all $i = 1, ..., N$. X is the vector of independent variables.[37]

At first, I estimated a conventional probit model where the expected value of S conditional on $X = x_i$ is modeled as follows:

$$E\left[S \mid X = x_i\right] = P\left[S = 1 \mid X = x_i\right] = \Phi\left(x_i'\beta/\sigma\right) \qquad \forall \quad i = 1, ..., N. \tag{4}$$

$\Phi()$ is the cumulative distribution function of a standard normal distribution evaluated at $x_i'\beta/\sigma$. It is assumed that the error term in the latent model conditional on $X = x_i$ follows a normal distribution with mean $\mu = 0$ and variance $\sigma^2 = 1$. β is the parameter vector to be estimated.

[37] All independent variables are measured in the firm formation year or the subsequent year except otherwise indicated in table 8.

Several available independent variables are not included in the finally chosen X. They turned out not to be significantly missing at the 10-percent significance level in Wald tests for omitted variables. These variables are gender of the entrepreneur, which is sometimes argued to affect bank lending decisions and the unemployment rate at the district level, often used to proxy local business cycle conditions. A Wald statistic for industry-related structural differences between East and West Germany turned out to be significant at the 1-percent significance level. Thus, I included seven interactions between industry indicators and the indicator for location in East or West Germany into the final specification.[38] However, the coefficients of entrepreneur-, firm-, location- and time-specific variables differed not significantly between East and West Germany.

As violating the main stochastic assumptions of the probit model can lead to inconsistent parameter estimates I conducted several tests. A Lagrange multiplier test of normality indicated no rejection of the null hypothesis of a normally distributed error term at any usual significance level. Using Lagrange multiplier tests, the null hypothesis of homoscedasticity was rejected for one variable in X at the 5-percent significance level.[39] Wald tests showed violation of the homoscedasticity assumption for three variables at least at the 10-percent significance level.

Therefore, I finally estimated a probit model that allows for multiplicative heteroscedasticity.[40] The expected value of S conditional on $X = x_i$ and $Z = z_i$ is modeled as

$$\begin{aligned} E\left[S \mid X = x_i, Z = z_i\right] &= P\left[S = 1 \mid X = x_i, Z = z_i\right] \\ &= \Phi\left(x_i'\beta / exp(z_i'\gamma)\right) \qquad \forall \quad i = 1, ..., N. \end{aligned} \tag{5}$$

The variance of the error term is not normalized to 1. Instead, it depends on the vector Z of independent variables as follows:

$$\sigma_i^2 = \left[exp\left(z_i'\gamma\right)\right]^2. \tag{6}$$

I included all three elements in X into the vector Z for which homoscedasticity was rejected. The results of the heteroscedastic probit estimation are shown in table 3.

The program participation equation has to be specified adequately to avoid biased estimates of causal effects. Most important, all variables that might simultaneously influence program assignment as described in section 2.2 and the interesting outcome variable, i.e. firm survival chances, have to be included in X and thus in the propensity score estimate. In the following, I will explain for each independent variable I use in the final model how it is expected to relate to the decisions of entrepreneurs, banks and DtA officials in the process of program assignment. Then, I will discuss the corresponding estimation result.

Entrepreneurial success is usually argued to increase in human capital (Bates 1990, Cressy 1996a). Therefore, banks are assumed to prefer ceteris paribus (c.p.) well educated entrepreneurs as borrowers. DtA officials are also likely to prefer such entrepreneurs because of

[38] See Greene (2000) for a discussion of a Wald test for structural change.

[39] See Verbeek (2000) for a description of the Lagrange multiplier tests of normality and of homoscedasticity. I adapted the test statistics to include probability weights.

[40] See Harvey (1976) and Greene (2000).

the program requirements and the potentially existing incentives to inflate program success records by picking winners. Moreover, well educated people may be better informed about the existence of DtA programs and thus more likely to apply for start-up assistance. The estimation results are consistent with these expectations. According to table 3, entrepreneurs with a master craftsman degree, a university degree in business and administration or in engineering receive significantly more often start-up assistance than entrepreneurs who have only completed an apprenticeship or any other type of low education.

The age of the entrepreneur can be interpreted as a proxy for experience, risk attitude or wealth (Bates 1990, Holtz-Eakin, Joulfaian, and Rosen 1994b, Cressy 1996b). Entrepreneurs who build up a firm relatively late in life are likely to start with a high level of business and work experience that promotes commercial success. Older entrepreneurs are usually assumed to choose less risky projects than young entrepreneurs and to have accumulated more wealth that can be used as collateral. In addition, the curriculum vita of older persons may provide more information and thus allow for a more precise prediction of the expected entrepreneurial success. Consequently, risk averse banks and DtA officials with distorted incentives should c.p. prefer older entrepreneurs as borrowers. In contrast, especially entrepreneurs starting their first firm project shall be preferred according to program requirements. According to table 3 the logarithm of age has a statistically significant quadratic effect on program participation. This age effect attains its maximum at an age of about 32 years. As this is 6 years less than the mean age of entrepreneurs in the sample, program requirements are not obviously violated. After peaking at 32 years the effect decreases quite slowly such that it remains positive until the maximal age in the sample. The result would be in accordance with a preferred selection of older entrepreneurs by DtA officials. Nevertheless, as argued earlier distorted incentives of DtA officials should be only of minor empirical relevance. The result rather indicates strong selectivity at the bank level.

Firms started by a team of entrepreneurs are c.p. probably endowed with a better human capital stock than start-ups of individual entrepreneurs because deficiencies in one person's education or experience can be compensated by other team members (Cressy 1996a). If a higher human capital stock implies an informational advantage, then team start-ups are more likely to apply for start-up assistance than firms of individual entrepreneurs. Consistent with this arguing, a significant positive coefficient of the team size variable is shown in table 3.

Some firms enter the market with a diversified firm concept or as franchisees. Because of simple portfolio theoretical rules diversification can serve as a risk-reducing investment strategy if projects with imperfectly positively correlated returns are combined (Rose 1992). Moreover, Jovanovic (1993) mentions that diversified firms may realize higher profits than non-diversified firms by exploiting economies of scope. Franchisees copying already tested firm concepts are probably less risky than newly designed projects. Using well-known trademarks can facilitate access to customers. In addition, the founders of franchisees may have been carefully selected and trained by the franchiser (Rubin 1978). Consequently, diversified firms and franchisees should c.p. be preferred as borrowers in case of risk averse bank lending behavior. Consistent with this expected behavior, table 3 exhibits a significant positive coefficient of the indicator

for diversified firms and of the indicator for franchisees.

The legal form of start-ups can be interpreted as an indicator for project risk.[41] Entrepreneurs who want to start a high risk project tend to choose a legal form with limited liability. According to Stiglitz and Weiss (1981), an entrepreneur with limited liability might even increase his residual firm value after start-up by shifting to a riskier project with the same or a lower expected value than the initial project. If banks tend to be risk-averse when selecting credit customers and thus program participants, then limited liability firms have c.p. less chances to receive start-up assistance. Moreover, entrepreneurs who choose a legal form with limited liability may be reluctant to accept the personal liability condition within the equity capital assistance program or to provide collateral for a loan in a business start-up program. In accordance with these expectations, the coefficient of the limited liability indicator is negative and significant.[42]

Program requirements preclude the assignment of start-up assistance to temporary firm projects. Entrepreneurs with temporary firm projects in Germany usually start a civil law association. This can explain the significant negative coefficient of the indicator variable for civil law associations in table 3.

As mentioned in section 4.1, firms with full affiliations to a parent firm were excluded from the sample because they are not eligible for the DtA programs evaluated here. Due to this program requirement partly affiliated firms may c.p. have lower chances to receive start-up assistance than independent start-ups. In accordance with this expectation, the estimation indicates a significantly lower assignment probability for partly affiliated firms than for independent start-ups.

The estimation results discussed so far show that firm- and entrepreneur-specific characteristics have strong impacts on program assignment. Next, I examine location-specific factors. As discussed earlier, start-up assistance at the beginning of the 1990s was mainly directed to East Germany. Thus, the indicator variable for firms located in West Germany has a statistically significant and substantial negative coefficient.[43] Control variables for East German states indicate significant deviation of the program assignment probability in East Berlin, Mecklenburg-West Pommerania, Saxony and Thuringia from the probability in the reference state Saxony-Anhalt. General agglomeration effects are captured best by a quadratic polynomial of the logarithm of the number of inhabitants per square kilometer in the district the firm is located. The population density effect peaks at about the median of the population density

[41] Harhoff, Stahl, and Woywode (1998) discuss this in detail.

[42] Storey (1994a) finds that limited companies in U.K. are more likely to use bank loans or overdraft than firms without limited liability. He explains his result with higher credibility and seriousness of incorporated firms. At first sight, Storey's and my results seem to be at odds, but this is probably not the case because of two reasons. First, I analyze assignment to subsidized medium- or long-term loans whereas Storey (1994a) uses an endogeneous variable that pools all types of loans including overdrafts typically used for short-term financing. Second, in contrast to my German data set, his data is based on a retrospective survey covering only surviving U.K. firms.

[43] Alternatively, I used controls for individual West German federal states but this did not improve the fit of the model significantly.

distribution and is positive over the whole distribution. Thus, the assignment probability is highest for firms in fringe districts.

Selection of firms for start-up assistance on the bank level may c.p. depend on competition in the banking sector contacted by the entrepreneur because lending relationships are known to depend on credit market competition.[44] In competitive markets banks may c.p. be more reluctant to sink screening costs for the evaluation of entrepreneurs than in less competitive markets because the potential credit customer can easily switch to another bank. Instead, it may be optimal to offer an application for start-up assistance to many unscreened or only lightly screened customers and to sell small accompanying bank loans to all these customers in order to exploit portfolio diversification effects. In accordance with this arguing, a linear and a quadratic term of the local density of bank offices turn out to be significant in table 3. The density of bank offices in the district of firm location is used as a proxy for competition intensity.[45] The assignment probability increases with bank office density, which proxies competition intensity, until a very high level is reached. Then, the effect turns around but remains positive until the maximal office density in the sample.

According to popular opinion, commercial banks serve new and small firms not as well as savings or cooperative banks in Germany. The variable 'local share of bank offices belonging to commercial banks' does not capture such an effect.

Finally, program assignment varies between firm formation cohorts and industries. Start-ups in 1991 have a significantly higher and those in 1993 a significantly lower participation probability than start-ups in 1990. East German firms in the transport and communication or service sectors are c.p. significantly less likely to participate than retail trade firms. In contrast, the participation probabilities of manufacturing, construction and wholesale trade firms are very similar to the probability of retail firms. For West German firms participation probabilities are lowest in the intermediate trade industry and highest in the electrical engineering, mechanical engineering and metal processing industries. Assignment chances differ significantly between East German firms in low and high technology industries. Consider, for example, electrical engineering firms in Saxony-Anhalt, started in 1990 by one entrepreneur with apprenticeship as a limited liability firm without firm affiliations, diversification or a franchise contract. Such firms have a participation probability of 64.33 percent if they belong to a high technology industry compared to 39.51 percent if they belong to a low technology industry. This significantly increased assignment chance of innovative and thus potentially risky East German firms is an exception. The estimated coefficient for West German firms

[44] Petersen and Rajan (1995) analyze the effect of credit market competition on lending relationships in the United States. Harhoff and Körting (1998) investigate lending relationships in Germany.

[45] Founders of new firms contact mainly bank offices within a very restricted radius around their firm's location. To proxy concentration of the local credit market, Petersen and Rajan (1995) use a crude categorical indicator based on the Herfindahl index of commercial bank deposit concentration in the area the firm is headquartered. I construct a different, continuously measurable proxy: the density of bank offices measured as the number of bank offices per number of potential bank customers, i.e. people aged between 18 and 65 years in the district the firm is located. This variable is assumed to be positively correlated with competition intensity in the local credit market.

in high technology industries and the coefficients of firm- and entrepreneur-specific variables discussed so far suggest rather risk-averse selection of program participants.

The presented program assignment equation allows for a more extensive selection control than in related studies evaluating firm programs. Nevertheless, important variables might have been ignored or captured insufficiently. It can be argued that the available data about entrepreneurial education and the firm concept allow only a crude control for entrepreneur motivation and business plan quality.[46] Such doubts cannot be dispelled completely. However, the particular situation in East Germany after unification may ease the justification of assuming independence conditional on the vector X of covariates observable here because of the following arguing. 67.51 percent of all subsidized firms in the sample are located in East Germany and faced a very unstable environment in the beginning of the 1990s. For example, non-anticipated re-privatization of the firm location forced many entrepreneurs either to risk a relocation or to close their firm. Market development was highly uncertain and industry structures changed rapidly due to the transition from a planned to a market economy. The East German banking industry was just emerging and bank clerks started to collect experiences in screening East German entrepreneurs. Given all these characteristics of the transition period, it can be argued that bank clerks and DtA officials were hardly able to classify the success chances of start-ups during the years after the unification systematically better than it can be done on the basis of the observable covariates used here.

Summing up, the results of the program assignment equation are consistent with the view that better qualified entrepreneurs with less risky projects are more likely to receive start-up assistance than others. Program assignment is shown to be highly selective and especially the mediating banks seem to have a strong influence. The results confirm that econometric approaches for the evaluation of federal firm financing programs in Germany have to control adequately for program selection in order to allow for estimation of causal program effect.

5.2 Matching and Balancing Quality

The matching estimator described in section 3.2 can only lead to an adequately matched comparison group if CIA can be assumed to hold in the whole support of X. This condition is not fulfilled in the context of estimating treatment on the treated effects if the distribution of the propensity score in the potential comparison group does not cover the whole interval of the distribution in the treatment group.[47] Figure 2 indicates that the interval of interest is nearly perfectly covered by the distribution in the potential comparison group. The computation of the unbounded propensity score is based on the estimated coefficients in table 3. The mass of the treated firm distribution lies to the right of the untreated firm distribution, which simply indicates the already discussed differences between both groups.

[46] Variables that capture the financial background of the entrepreneurs, their pursuit of independence, and the implied dislike to external finance might also be needed.

[47] See Rosenbaum and Rubin (1985) and Heckman, Ichimura, Smith, and Todd (1998).

The graphs also show that the frequency of treated firms is slightly higher in the rightmost region than the frequency of potential comparison firms. Therefore, matching without replacement could imply strongly biased matches in this area (Dehejia and Wahba 1998). To avoid this, the matching algorithm described in section 3.2 allows for drawing the same comparison firm more than once.

The chosen balancing score includes the estimated propensity score, the firm formation year and eight indicators for West German firms, for limited liability firms and for firms in the manufacturing, construction, wholesale and intermediate trade, retail trade, transport and communication as well as the service sector. The most important matching variables in the balancing score, the indicator for West German firms and the propensity score were given higher weights than other matching variables.[48]

After applying the matching procedure, I tested the achieved balancing between the distribution of X in the matched comparison and the treatment group using 1,342 matched pairs.[49] High balancing quality is crucial because otherwise causal effect estimation would provide biased results. To illustrate the balancing quality, table 4 shows the sample means of selected variables in the treatment group and the matched comparison group. For all firm characteristics, the differences between the group-specific sample means are small and statistically insignificant in two-sided t-tests. Most important, the sample means of the propensity score itself do not differ significantly. Even more, the distributions of the propensity score in both groups coincide quite closely according to the kernel density estimates in figure 3. Moreover, both groups show similar standard deviations of the selected continuous variables. All these results indicate that the treatment and the matched comparison group are well balanced. Due to the chosen balancing score vector the balancing quality in the industry and cohort sub-samples to be used in section 5.3 is very high, too.[50]

The advantage of bias-reduction when using a matching procedure with replacement comes at the cost of drawing some comparison firms repeatedly despite availability of almost identical, only slightly less close comparison firms. This could substantially reduce the precision of estimated causal program effects. To investigate excessive use of single comparison firms, I used a measure proposed by Lechner (2001b). The measure is a concentration ratio, computed as the sum of weights w_l in the highest decile of the weight distribution in the sample of matched comparison firms divided by the sum of weights $\sum_l w_l \quad \forall\, l = 1, ..., N^m$. The resulting value of 30.56 percent is as high as some values reported by Lechner (2001b). In addition, 77.54 percent of all matched comparison firms were only drawn once. Therefore, the repeated use of single comparison firms was not excessive.

[48] The weights were fixed at 10. Using other weights led to less balancing between treatment group and matched comparison group.

[49] I used 1,342 instead of all 1,351 matched pairs. Nine pairs were eliminated because they contained comparison firms liquidated at a time after start-up before the paired treated firm received start-up assistance.

[50] Only in the sub-samples of start-ups in 1991 and of start-ups in the manufacturing sector the means of up to three variables differ significantly at the 5-percent level. Among these variables are the population density, two human capital indicators, and the indicators for diversification, franchise contracts or firm affiliations. Test results are available upon request.

5.3 Effects of start-up assistance

In this section, I analyze long-run effects of start-up assistance on the survival of subsidized firms. In addition, I investigate the adaptations of labor and capital use shortly after assignment to start-up assistance. Finally, I show how the effects on survival as well as hazard functions change over time.

Table 5 displays the estimated average effects on the survival rate of start-ups between 1990 and 1993 measured at the end of the observation period, i.e. December 31, 1999. The group of all treated firms has a survival rate of 79 percent whereas the rate for the group of all matched comparisons is only 64 percent. This difference implies a significant positive program effect of 15 percentage points. In other words, due to start-up assistance the share of subsidized firms operating until 1999 is 23 percent higher than it would have been without start-up assistance.

Effect estimates for various sub-samples in table 5 show how the overall effect on firm survival is composed. Comparing the effect estimates for East and West German firms indicates a 42 percent higher effect for East than for West German firms. Cohort-specific effect estimates indicate significant positive effects for start-ups in 1990 and 1993 that are higher than the overall effect. Lower, but still significant positive effects arise for start-ups in 1991 and 1992. This ordering of cohorts with respect to effect size is driven by the effects in the East German sub-sample of each cohort. Looking at industry-specific estimates, the effect appears to be most substantial for retail trade firms, i.e. 47 percent higher than the overall effect. The effect estimates for manufacturing, construction as well as wholesale and intermediate trade firms are at about the level of the overall effect. The smallest and even insignificant effects occur in the transport and communication and the service sectors. Splitting the industry samples along the indicator for East and West German firms and estimating program effects for all resulting sub-samples reveals the main components of the significant positive overall effect in West Germany. These are the effects in the trade sector as all other industry-specific effect estimates are small and insignificant.

Summing up, a significant positive average effect of federal start-up assistance in Germany on the long-term survival rate of subsidized firms can be observed when using the sample of all treated firms and matched comparison firms. Industry- and cohort-specific effects are all positive, albeit effect size varies considerably. Most importantly, estimated effects for start-ups in the transport and communication as well as the service sectors turn out to be insignificant.

After discussing program effects on the firm survival rate at the end of 1999 I now investigate whether these long-run effects are accompanied by adaptations of labor and capital use occurring shortly after assignment to start-up assistance.[51] This sheds light on the question whether subsidized firms would have started with a less efficient production plan in case of no start-up assistance. Table 6 contains the average program effects on firm employment shortly

[51] For effect calculation I used the first available employment and capital information recorded after assignment to start-up assistance. Recording took place at latest two years after assignment and for most firms I used the first information recorded at all.

after assignment. The effect estimate for the whole sample shows that subsidized firms do not directly respond to the receipt of start-up assistance by increasing the average number of workers significantly. For the sub-samples of West German start-ups in the sectors construction, wholesale and intermediate as well as retail trade or in the 1992 cohort the effects turn out to be positive and significant at least at the 10-percent significance level. This is not the case for any of the analyzed sub-samples including East German firms. The reported insignificant effects are at odds with the view that new firms start with a sub-optimal number of employees due to financial constraints (Mata and Portugal 1994). Instead, the results suggest that it doesn't pay for most founder teams to scale up their project directly after assignment to start-up assistance. Lerner (1999) gives a similar argument referring to key researchers in small high-technology firms when discussing Wallsten (2000). Wallsten (2000) finds no significant employment effects one year after the provision of a SBIR research grant.

In contrast to these mostly insignificant short-run effect on firm employment, significant and substantial positive investment effects can already be observed in the short-run. Table 7 exhibits that subsidized firms choose to invest on average 5252 Euro or 44 percent more in tangible assets per employee than matched comparison firms. The capital variable used for the calculations captures a specific type of investment. It indicates the amount invested in tangible assets like office equipment or machinery.[52] Significant positive effects on the capital intensity variable can be observed for all start-up cohorts. The industry-specific results show that the positive overall effect results mainly from the effects for manufacturing and trade firms. This particular pattern probably reflects the varying importance of tangible assets between industries.

The behavior of subsidized firms shortly after receipt of start-up assistance is consistent with the view that they would otherwise have been more capital constrained and forced to choose a less efficient production plan. Right from the beginning they opt for a higher level of capital intensity than matched comparison firms. However, they do not immediately scale up their projects by employing significantly more workers.

When evaluating the survival effects of start-up assistance for new firms it is crucial to find out whether the positive average effect of start-up assistance on the survival rate in 1999 is merely a result of "cash-and-carry"-behavior or not. Analyzing effect changes over time is helpful in this context. If subsidized firms simply life of the provided loan, the survival chances should be very high during the first years after receipt of the loan. Afterwards the instantaneous liquidation risk should rise considerably and even reach a higher level than the risk of matched comparison firms.

Figure 4 shows the estimated survival functions for the group of all treated firms and for the group of all matched comparison firms.[53] The survival function of the treatment group remains at a higher level than the survival function of the matched comparison group during

[52] The available data base provides no other variables indicating capital structure or investment for a sufficiently large sub-sample of firms.

[53] See Cox and Oakes (1984) for details on the product-limit estimator of the survival function I applied.

the entire observation period. This simply reflects the already discussed positive effect on the survival rate in 1999. Comparing the curvature, both survival functions provide first evidence pointing to changes of the effect size over time. After starting slightly concave, the curve of matched comparison firms drops considerably from the third year to the sixth year after firm formation and only slightly afterwards. Thus, it displays a clearly convex pattern from the third year onwards which indicates considerable negative duration dependence of the related hazard function. The curve for treated firms is only lightly curved and remains concave for more than the first four years.

Figure 6 displays the hazard function estimates.[54] The hazard function of matched comparison firms shows the pattern typically observed for newly founded firms.[55] It increases initially and declines from the beginning of the fourth year after firm formation onwards, i.e. negative duration dependence is revealed. In contrast, the hazard rate of the subsidized firms increases until the end of the fourth year. Afterwards it fluctuates around a rather constant level until the seventh year and then decreases.

Most important in the evaluation context, the hazard rates of both comparison groups are at about the same level from the seventh year onwards. This is also illustrated in figure 5. The graph indicates nearly no difference between the survival functions estimated for those treated and matched comparison firms that survive the initial six years after firm formation. Using a log-rank test, the null hypothesis of equal survival functions cannot be rejected at any usual significance level.[56] Thus, the survival and hazard rate evidence indicates the following: During the first six years, 16 percent of the subsidized and 32 percent of the matched non-subsidized firms are liquidated. From the seventh year onwards the remaining 84 percent of the subsidized firms face a similar instantaneous liquidation risk as the best 68 percent of the matched firms.

Before interpreting this result, it has to be investigated whether the hazard rate comparison is affected by selection effects. Ham and LaLonde (1996) discuss that selection effects resulting from different exit processes in the treatment and the matched comparison group can gradually destroy the initial balancing between both groups and thus invalidate hazard rate comparisons. Here, I found no significant differences with respect to firm characteristics measured at market entry for the pairs of survivor sub-groups from the treatment and matched comparison group in all considered periods.[57] Therefore, I assume that the hazard rate comparison is not affected by unbalanced comparison groups.

[54] To estimate the hazard function I used a life-table estimator. The hazard function in period t equals the number of firms liquidated in the six-month period t divided by the number of firms at risk of liquidation at the beginning of period t. The number of firms at risk is adjusted for right-censored spells. See Cox and Oakes (1984) for further details.

[55] See for example Brüderl, Preisendörfer, and Ziegler (1992) and Wagner (1994).

[56] See Kalbfleisch and Prentice (1980) for the standard version of the test. Since I used probability weights, a modified version of the standard test was applied.

[57] For each period, two-tailed tests of the null hypothesis stating equality of weighted means in both comparison groups were applied. Results are available upon request.

The time pattern of the hazard rate of subsidized firms is clearly consistent with strong short-run effects of start-up assistance and with dependence on the provided DtA financing and the subsidized capital costs. The diminishing of the financial buffer over time and the increase of capital costs due to program rules may cause many of the observed late liquidation decisions of subsidized firms. However, the available evidence does not suggest that the initial positive effects will be completely erased over time. The results rather show that both groups face the same decreasing instantaneous liquidation risks from the seventh year onwards. Building on this, a rough measure of the additional employment due to the assigned start-up assistance can be calculated. On average, each firm with a subsidized loan of about 110,000 Euro employs one person for eight years it would not employ in case without start-up assistance. This rough measure is based on the employee numbers reported shortly after assignment of start-up assistance. It ignores the fact that subsequent average employment growth may differ between subsidized and matched comparison firms. The measure should be treated as a lower bound of the additional employment caused by start-up assistance because of the results of Almus and Prantl (2002). Their analysis shows that start-up assistance increases the average annual employment growth rate of surviving subsidized firms significantly.[58]

6 Conclusions

This paper offers an evaluation of start-up assistance provided to new German firms by the Deutsche Ausgleichsbank (DtA). To separate the effects of start-up assistance from effects of non-random assignment to assistance a non-parametric matching on the balancing score is applied. For the empirical analysis I use a large, unique firm data set including 7,874 firms started between 1990 and 1993 in the manufacturing, construction, and trade sector as well as most service industries. 1,351 of the firms received start-up assistance within the firm formation year or the subsequent year.

I address the question whether start-up assistance has the intended effect of causing on average higher survival chances of subsidized firms than in the counterfactual situation where the same firms are not subsidized. Thus, the empirical analysis presented here is confined to evaluating the net effect on the survival chances of subsidized firms. It must be taken into account that the high level of start-up assistance reached in East Germany after unification makes additional indirect effects plausible. First, start-up assistance might lead to displacement effects that are ex ante ambiguous from a welfare perspective.[59] Second, the programs could have general equilibrium effects on the firm formation decision of potential entrepreneurs and thereby on the number of start-ups.[60] Other indirect effects might concern consumers,

[58] In Almus and Prantl (2002) growth rates are measured during the first nine years after market entry.

[59] Revolving door effects, i.e. repetitive replacements of similar entrants without technological progress, without improvements of customer service or the like probably imply a waste of resources and reduce welfare. In contrast, displacement of old, inflexible firms using technologies that are no longer up-to-date by new, innovative and flexible organizations are likely to increase welfare.

[60] Storey (1994b) discusses this aspect as one objective of small business policies which is often mentioned

financial institutions, and other economic agents. Indirect and general equilibrium effects are difficult to identify in general, and especially for an economy in transition like East Germany after unification. Moreover, they cannot be evaluated using the data available for this study.

The presented evaluation of program effects on program participants constitutes an important initial evaluative step because federal start-up assistance in Germany is shown to satisfy a crucial condition for program success: start-up assistance has a significant, substantial and persistent positive net effect on the survival chances of subsidized firms. Already a short time after receipt of start-up assistance the subsidized firms adapt their production plan by choosing higher capital intensity than matched comparison firms. This short-term reaction is in line with the view that start-up assistance relaxes capital constraints and induces shifts to more efficient production plans. The variation of the effects on liquidation risk and survival chances over time is consistent with strong short-run effects of start-up assistance. Most important, the positive firm survival effect can not be judged to be a pure result of inefficient "cash-and-carry" behavior. Subsidized firms do not simply live on the provided loan for some years and exit some time later than matched comparison firms. Both, the group of subsidized firms and of matched comparison firms face the same declining instantaneous liquidation risk from the seventh year after market entry between 1990 and 1993 until the end of the observation period in 1999. However, it has to be stressed that the effectiveness of federal start-up assistance varies considerably across industries. Most important, in the transport and communication as well as in the service sectors no significant positive effects can be identified.

Due to the observed variation of program effects in time and across industries it seems a worthwhile direction for further research to investigate the effects of heterogeneous start-up assistance. Moreover, this study is among the first evaluation studies on firm financing programs using non-parametric evaluation methods. As such programs are usually much more expensive than the relatively often evaluated active labor market programs, similar studies of other firm financing programs are an interesting issue. For example, applying non-parametric matching or other non-parametric evaluation techniques can provide new insights about the effects of R&D programs in the U.S. and Europe as well as about the impact of micro credit programs in low-income and transition countries.

but rarely convincingly justified.

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Tables and Figures

Table 1: Federal financing programs for young and small firms in Germany

Year	Volume (Million Euro)		# Loans		Refusal Rate in %	
	East	West	East	West	East	West
All programs for young and small firms managed by DtA						
1990	1,565.35	762.49	38,403	27,172	2.4	9.2
1991	4,376.74	832.52	104,031	27,756	4.1	8.8
1992	4,521.63	704.65	70,658	19,763	6.5	6.1
1993	4,305.13	591.52	48,768	12,960	2.5	1.4
1994	3,415.43	1193.20	33,601	24,655	2.8	2.9
Equity capital assistance Program (EKH)						
1990	262.91	219.20	9,492	8,604	4.4	12.6
1991	1,619.14	257.83	48,185	9,859	5.0	11.4
1992	1,814.26	152.34	34,707	4,515	8.4	8.1
1993	1,510.80	NA	22,511	NA	4.3	NA
1994	1,411.98	163.63	16,048	4,929	5.2	7.8
ERP/DtA Business Start-up Programs						
1990	1,302.44	525.98	28,909	17,136	1.7	9.0
1991	2,757.60	550.35	55,824	16,388	3.7	8.3
1992	2,681.38	532.17	35,789	13,681	5.2	5.4
1993	2,578.06	547.51	25,941	11,294	1.1	1.4
1994	1,858.92	976.82	17,317	17,944	0.6	1.4

Notes: The upper third of the table exhibits information on all programs for young and small firms managed by the DtA, i.e. on the three programs in the lower part of the table and some special programs. NA denotes that information for the EKH Program in 1993 is not available due to program interruption in 1993. Refusal rate indicates the share of applications refused by the DtA in the respective year and program class. Rates displayed under the heading "ERP/DtA Business Start-up Programs" relate only to the ERP business start-up program because rates for the small accompanying DtA business start-up program were not calculated by the DtA.

Source: Deutsche Ausgleichsbank (2000) and unpublished tables with refusal rates provided by the DtA.

Table 2: Potential Comparison Group versus Treatment Group

Variable	Mean/Share	
	Potential Comparison Firms	Treated Firms
apprenticeship	0.7511	0.6087***
master craftsman	0.0934	0.1822***
business administration	0.0309	0.0270
engineering	0.0966	0.1612***
other academic degrees	0.0281	0.0210
age, 17-29	0.2271	0.1987**
age, 30-44	0.5442	0.5840**
age, 45-72	0.2287	0.2173
female	0.1071	0.1617***
team size	1.3236	1.3364
diversified	0.1491	0.1669
franchisee	0.0255	0.0412**
ltd. liability & stock corporation	0.3657	0.2979***
civil law association	0.0994	0.0951
commercial partnership	0.0105	0.0202**
sole proprietorship	0.5244	0.5868***
firm affiliation	0.0713	0.0317***
cohort 1990	0.2623	0.2213***
cohort 1991	0.2482	0.3402***
cohort 1992	0.2049	0.2622***
cohort 1993	0.2846	0.1763***
manufacturing	0.1051	0.1608***
construction	0.1551	0.2329***
wholesale & intermediate trade	0.1240	0.0823***
retail trade	0.2684	0.3450***
transport & communication	0.0638	0.0416***
services	0.2835	0.1374***
high technology	0.0823	0.0722
West Germany	0.6728	0.3269***
population density	1067.87	620.24***
unemployment rate	9.62	12.80***
bank office density	0.6788	0.8532***
commercial bank share	0.1903	0.1254***

Note: The table shows weighted means and shares for the groups of 6,523 potential comparison firms and of 1,351 treated firms. *** (**, *) indicates significance of the t-test statistic in a two-tailed test at the 1% (5%, 10%) level. The tested null hypothesis is equality of the means in the potential comparison group and the treatment group.

Table 3: Estimation Results of the Probit Assignment Model with Heteroscedasticity

Independent Variable	Coefficient	Robust Standard Error
X **- Vector**		
master craftsman	0.3220***	0.0603
business administration	0.2045*	0.1136
engineering	0.2915***	0.0595
other academic degrees	0.1185	0.1208
ln(age-16)	2.2497***	0.3627
$\ln(\text{age-16})^2$	-0.4058***	0.0636
team size	0.1055***	0.0371
diversified	0.1089**	0.0489
franchisee	0.2673**	0.1111
ltd. liability & stock corporation	-0.1541***	0.0514
civil law association	-0.1518**	0.0742
commercial partnership	0.3566**	0.1436
firm affiliation	-0.4745***	0.0953
West Germany	-1.1821***	0.0919
East Berlin	-0.4072***	0.0858
Brandenburg	0.0336	0.0920
Mecklenburg West-Pommerania	-0.1325*	0.0731
Saxony	-0.1568**	0.0785
Thuringia	-0.2705*	0.1418
ln(population density)	0.5614***	0.2169
$\ln(\text{population density})^2$	-0.0501***	0.0186
bank office density	0.3933***	0.1187
$(\text{bank office density})^2$	-0.0741**	0.0360
commercial bank share	0.0840	0.3399
cohort 1991	0.1276***	0.0495
cohort 1992	0.0758	0.0528
cohort 1993	-0.2889***	0.0568
East*electr. & mech. engin. & metal proc.	0.0418	0.1094
West*electr. & mech. engin. & metal proc.	0.6816***	0.1311
Other manufacturing	0.0720	0.1063
Structural & civil engineering	0.0178	0.0867
East*other construction	0.1100	0.0901
West*other construction	0.4243***	0.1319
East*wholesale trade	0.0084	0.1085
West*wholesale trade	0.7190***	0.2130
East*intermediate trade	-0.1954	0.1619
West*intermediate trade	-0.7545***	0.2461
West*retail trade	0.3558***	0.1035
transport & communication	-0.2512**	0.1007
accommodation & food services	-0.2565***	0.0965
East*business-related services	-0.4458***	0.1183
West*business-related services	-0.0734	0.1378

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Independent Variable	Coefficient	Robust Standard Error
other services	-0.1668	0.1349
East*high technology	0.4544***	0.1120
West*high technology	-0.0572	0.1233
intercept	-5.1819***	0.8370
Z - Vector		
East*wholesale trade	-0.5450**	0.2309
West*wholesale trade	-0.5545**	0.2547
East*high technology	0.6676***	0.2274
Model (Wald-stat., χ^2 (df))	970.80***	(45)
log Likelihood (# obs.)	-3102.12	(7874)

Notes: The dependent variable in the probit regression is the assistance indicator (1: start-up assistance within the firm formation year or the subsequent year; 0: else). Reference firms have the following characteristics: one entrepreneur with apprenticeship or inferior education, no diversification or franchise contract, sole proprietorship, no firm affiliation, cohort 1990, retail trade industry in Saxony-Anhalt. *** (**, *) indicates that the coefficient differs significantly from zero at the 1% (5%, 10%) significance level.

Table 4: Matched Comparison Group versus Treatment Group

Variable	Mean/Share	
	Matched Comparison Firms	Treated Firms
propensity score	-0.5481	-0.5375
apprenticeship	0.6349	0.6082
master craftsman	0.1713	0.1823
business administration	0.0280	0.0272
engineering	0.1472	0.1621
other academic degrees	0.0186	0.0202
age, 17-29	0.1928	0.1998
age, 30-44	0.6159	0.5830
age, 45-55	0.1914	0.2171
female	0.1550	0.1608
team size	1.3438	1.3368
diversified	0.1958	0.1675
franchisee	0.0306	0.0409
ltd. liability & stock corporation	0.2989	0.2989
civil law association	0.1064	0.0947
commercial partnership	0.0235	0.0203
sole proprietorship	0.5712	0.5861
firm affiliation	0.0272	0.0318
cohort 1990	0.2183	0.2218
cohort 1991	0.3413	0.3395
cohort 1992	0.2649	0.2632
cohort 1993	0.1755	0.1755
manufacturing	0.1618	0.1618
construction	0.2328	0.2328
wholesale & intermediate trade	0.0824	0.0824
retail trade	0.3464	0.3464
transport & communication	0.0419	0.0419
services	0.1347	0.1347
high technology	0.0682	0.0724
West Germany	0.3280	0.3280
population density	666.45	620.53
unemployment rate	12.81	12.79
bank office density	0.8385	0.8501
commercial bank share	0.1281	0.1253

Note: The table shows weighted means and shares for the groups of 984 matched comparison firms and of 1,342 treated firms. *** (**, *) indicates significance of the t-test statistic in a two-tailed test at the 1% (5%, 10%) level. The tested null hypothesis is equality of the means in the matched comparison group and the treatment group.

Table 5: Estimated Average Long-run Effects on Firm Survival

All Firms
(East German Firms; West German Firms)

Firm Group	# Matches	Mean for Matched Firms $\hat{E}(Y^c \mid S=1)$	Mean for Treated Firms $\hat{E}(Y^t \mid S=1)$	Causal Effect $\hat{\theta}$
Industries				
manufacturing	222	0.66	0.81	0.16***
	(124;98)	(0.65;0.67)	(0.82;0.80)	(0.18**;0.13)
construction	330	0.67	0.79	0.12***
	(247;83)	(0.64;0.74)	(0.78;0.79)	(0.14***;0.05)
whol. & int. trade	114	0.61	0.73	0.13*
	(77;37)	(0.61;0.59)	(0.70;0.80)	(0.09;0.21*)
retail trade	433	0.60	0.82	0.22***
	(289;144)	(0.57;0.65)	(0.82;0.81)	(0.25***;0.16***)
trans. & comm.	64	0.71	0.79	0.08
	(50;14)	(0.67;0.83)	(0.79;0.80)	(0.12;0.03)
services	179	0.67	0.74	0.07
	(118;61)	(0.67;0.68)	(0.74;0.76)	(0.07;0.08)
Cohorts				
cohort 1990	297	0.64	0.83	0.19***
	(159;138)	(0.61;0.68)	(0.83;0.83)	(0.22***;0.16***)
cohort 1991	469	0.61	0.76	0.14***
	(331;138)	(0.61;0.63)	(0.76;0.75)	(0.15***;0.11*)
cohort 1992	354	0.68	0.77	0.09**
	(264;90)	(0.69;0.66)	(0.76;0.79)	(0.07;0.13*)
cohort 1993	222	0.62	0.85	0.23***
	(151;71)	(0.56;0.76)	(0.86;0.83)	(0.30***;0.07)
Total				
	1342	0.64	0.79	0.15***
	(905;437)	(0.62;0.67)	(0.79;0.80)	(0.17***;0.12***)

Notes: The table shows weighted results for the share of start-ups surviving until December 31, 1999. All firms were started between January 1990 and December 1993. The sample of 984 matched comparison firms and 1,342 treated firms is used. *** (**, *) indicates significance at the 1% (5%, 10%) level of the t-test statistic in a two-tailed test. The tested null hypothesis is equality of the means in the matched comparison group and the treatment group.

Table 6: Estimated Average Short-run Effects on Firm Employment

All Firms
(East German Firms; West German Firms)

Firm Group	# Matches	Mean for Matched Firms $\hat{E}(Y^c \mid S=1)$	Mean for Treated Firms $\hat{E}(Y^t \mid S=1)$	Causal Effect $\hat{\theta}$
Industries				
manufacturing	222	7.69	7.10	-0.60
	(124;98)	(10.15;4.70)	(9.26;4.47)	(-0.90;-0.23)
construction	330	10.11	11.01	0.91
	(247;83)	(12.42;3.08)	(13.18;4.43)	(0.76;1.34**)
whol. & int. trade	114	5.12	5.38	0.26
	(77;37)	(6.21;2.75)	(6.05;3.90)	(-0.16;1.16*)
retail trade	433	3.35	3.77	0.42
	(289;144)	(3.78;2.53)	(4.00;3.31)	(0.23;0.78*)
trans. & comm.	64	3.45	3.86	0.41
	(50;14)	(3.81;2.19)	(4.29;2.33)	(0.48;0.14)
services	179	4.53	5.06	0.54
	(118;61)	(4.65;4.27)	(5.52;4.10)	(0.87;-0.17)
Cohorts				
cohort 1990	297	4.77	5.48	0.72
	(159;138)	(6.17;3.21)	(7.21;3.51)	(1.04;0.30)
cohort 1991	469	5.73	7.33	1.60
	(331;138)	(6.58;3.68)	(8.62;4.27)	(2.03*;0.58)
cohort 1992	354	8.04	6.28	-1.76
	(264;90)	(9.80;3.01)	(7.02;4.20)	(-2.78;1.19*)
cohort 1993	222	4.61	5.39	0.78
	(151;71)	(5.16;3.41)	(6.25;3.52)	(1.09;0.11)
Total				
	1342	5.93	6.30	0.37
	(905;437)	(7.20;3.35)	(7.48;3.89)	(0.29;0.54*)

Notes: The table shows weighted results for the number of employees measured shortly after the firm received start-up assistance. The sample of 984 matched comparison firms and 1,342 treated firms is used. *** (**, *) indicates significance at the 1% (5%, 10%) level of the t-test statistic in a two-tailed test. The tested null hypothesis is equality of the means in the matched comparison group and the treatment group.

Table 7: Estimated Average Short-run Effects on Capital Intensity

All Firms
(East German Firms; West German Firms)

Firm Group	# Matches	Mean for Matched Firms $\hat{E}(Y^c \mid S=1)$	Mean for Treated Firms $\hat{E}(Y^t \mid S=1)$	Causal Effect $\hat{\theta}$
Industries				
manufacturing	222	12373	23824	11452***
	(124;98)	(12996;11615)	(23103;24700)	(10107**;13085***)
construction	330	7792	8193	401
	(247;83)	(7299;9290)	(7209;11181)	(-89;1891)
whol. & int. trade	114	12933	21227	8294**
	(77;37)	(11075;16981)	(23951;15292)	(12876***;-1689)
retail trade	433	10861	15223	4363***
	(289;144)	(10703;11162)	(16044;13652)	(5340**;2490*)
trans. & comm.	64	27752	37515	9763
	(50;14)	(24895;37933)	(42606;19374)	(17711**;-18559)
services	179	16254	21465	5211
	(118;61)	(16536;15660)	(20013;24521)	(3477;8861*)
Cohorts				
cohort 1990	297	10348	13882	3534**
	(159;138)	(8292;12632)	(12830;15087)	(4538**;2455)
cohort 1991	469	13078	16913	3835**
	(331;138)	(12818;13698)	(15999;19074)	(3181;5376*)
cohort 1992	354	9986	18052	8066***
	(264;90)	(9010;12782)	(18524;16711)	(9514***;3929)
cohort 1993	222	14973	20941	5968*
	(151;71)	(16734;11129)	(21571;19567)	(4837;8437***)
Total				
	1342	11996	17247	5252***
	(905;437)	(11633;12738)	(17170;17406)	(5537***;4667***)

Notes: The table shows weighted results for the capital intensity, i.e. the Euro amount invested in tangible assets per employee measured shortly after the firm received start-up assistance. The sample of 984 matched comparison firms and 1,342 treated firms is used. *** (**, *) indicates significance at the 1% (5%, 10%) level of the t-test statistic in a two-tailed test. The tested null hypothesis is equality of the means in the matched comparison group and the treatment group.

Figure 1: Selection for Start-up Assistance

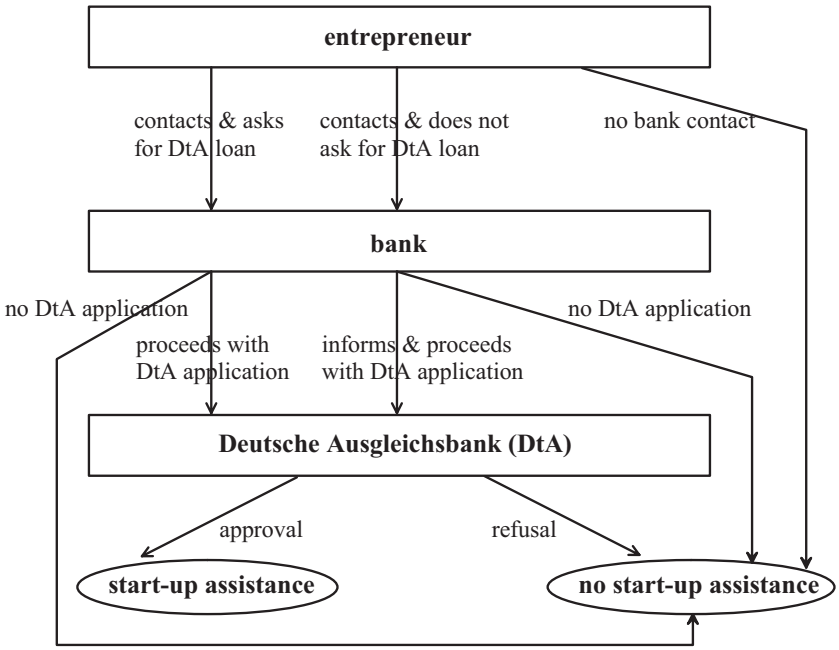


Figure 2: Propensity Score Distributions for Potential Comparison Firms and Treated Firms

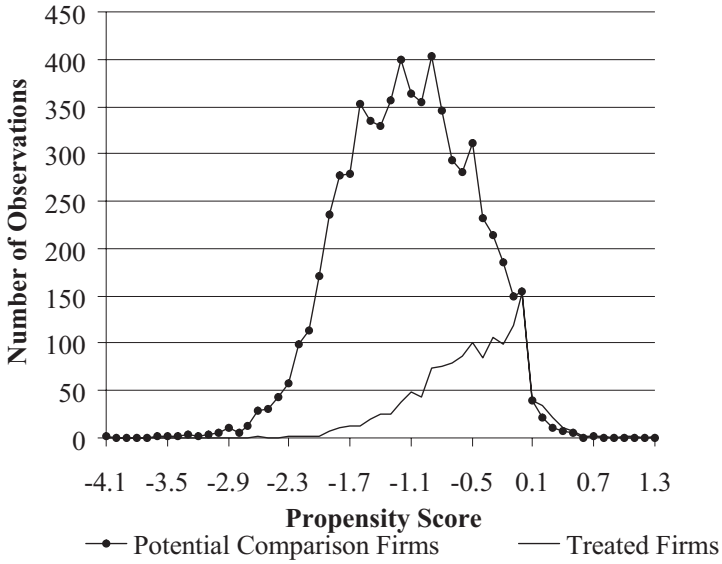


Figure 3: Kernel Density Estimates of the Propensity Score Distribution for Matched Firms and Treated Firms

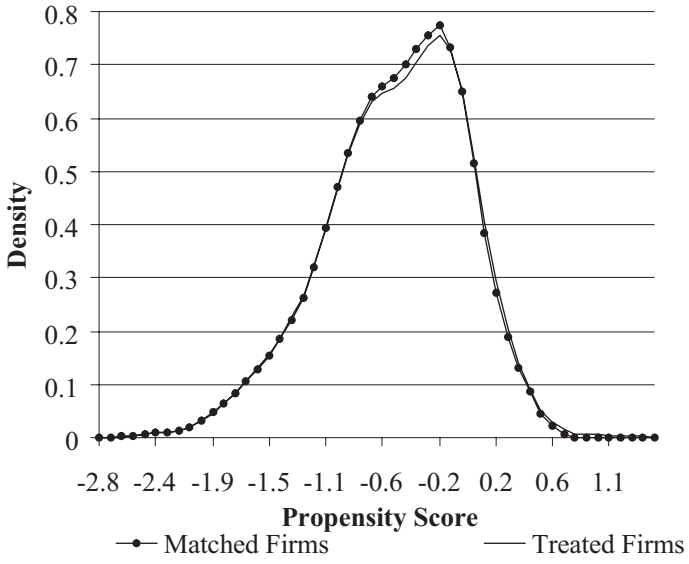


Figure 4: Estimates of the Survival Function for Matched Firms and Treated Firms

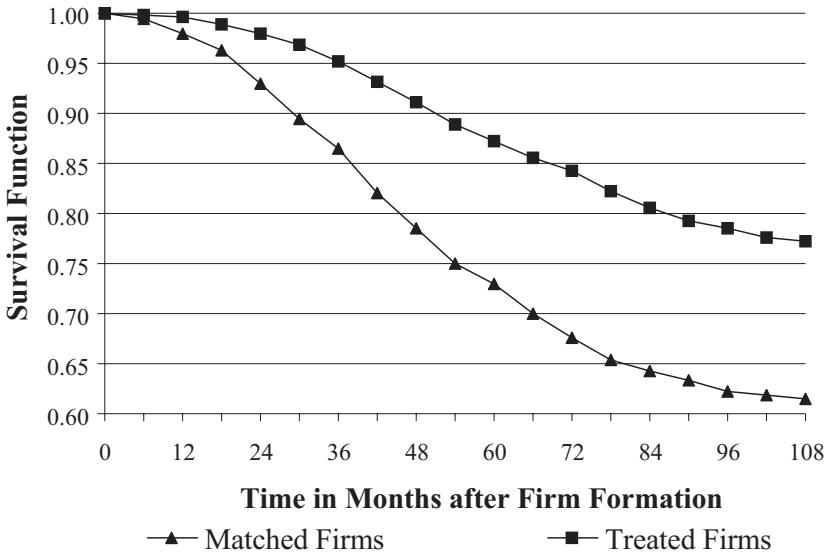


Figure 5: Estimates of the Survival Function for Matched Firms and Treated Firms surviving at least 72 month

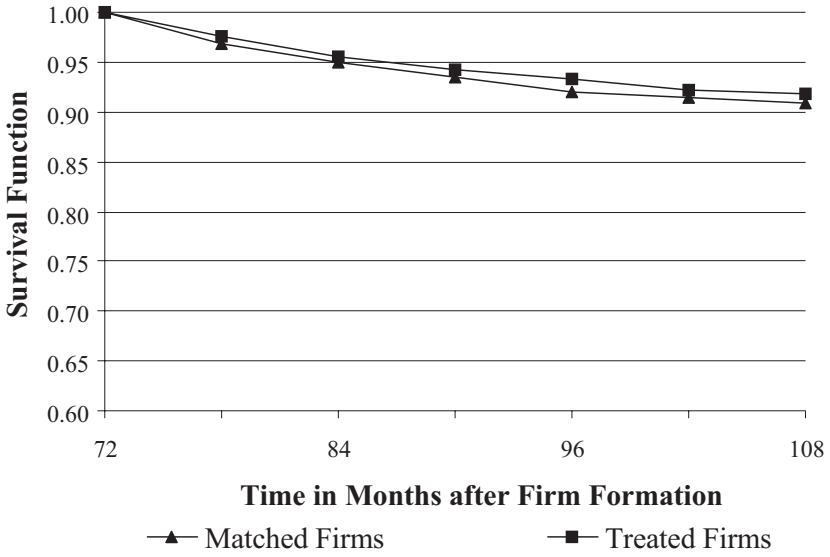
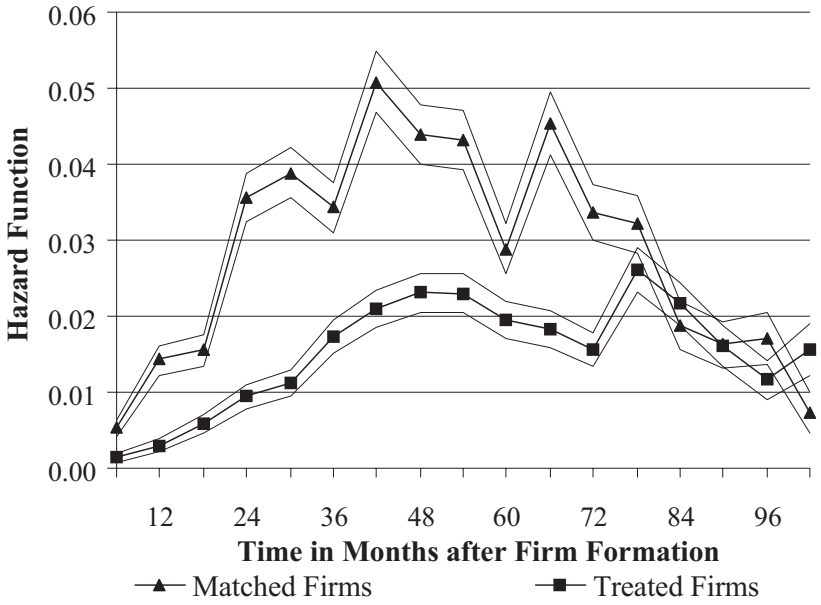


Figure 6: Estimates of the Hazard Function for Matched Firms and Treated Firms (with 95% confidence intervals)



Appendix

A Merge between the internal DtA data base and the firm sample

The data set used for the empirical analysis here combines information from two large data bases. For the first time, the internal data base of the DtA was connected with a firm sample from an external data base.[61]

The internal DtA data base contains information about 775,781 loans approved between 1990 and 1999.[62] The available information about each case collected at the moment of application includes the name, address, and birth date of the applying person or firm as well as some characteristics of the loan and the firm project. The name, address and birth data information was used to connect the DtA database to the firm sample.

At first, two temporary data bases were generated. One was the temporary data base with all 490,211 unique combinations of loan applicant name, birth date and address information in the DtA data base. The other temporary data base contained all 410,655 unique combinations of firm or firm owner name, birth date and address information in the firm sample. In this temporary data base all birth date corrections as well as name and address changes recorded by Creditreform between 1990 and 1999 were taken into account.

Then, all entries in both temporary data bases were compared one by one using a computer based search algorithm. It determined the degree of coincidence between names, birth dates, street names, location names, and ZIP codes in both entries by means of a heuristic comparison procedure.[63] A coincidence measure between 0 and 100 indicating the determined degree of coincidence was assigned to each pair of compared entries. Coincidence of compared words occurring frequently in the DtA data base increased the measure less than coincidence of rarely occurring ones. Coincidence of the names (birth dates, streets, location names, ZIP codes) increased the measure at most by a value of 40 (25, 10, 13, 12). A value of 100 was assigned if two compared entries coincided perfectly.

Finally, all pairs of compared entries with a coincidence value of at least 50 were linked together. Of all these links, 10,297 links could be used without further inspection because the assigned coincidence measure indicated perfect coincidence of name, birth date and at least 2 of the 3 address criteria. 30,944 links were checked by hand in order to separate wrong links from those with a low coincidence value because of typing errors, spelling differences, or identifiable street name and ZIP code changes in East Germany after unification.

[61] Due to data protection rules the data bases were merged within the DtA headquarter. An anonymous version of the connected data set could be used outside.

[62] In section 2.2 I mentioned that the DtA refuses a small number of applications per year. However, information on these cases is not available in encoded form and can not be used in this study.

[63] Similar procedures have already been used for connecting other large data bases at the ZEW.

According to the remaining checked links 4,571 firms in the sample of 22,000 firms were found to be subsidized by altogether 11,561 DtA loans between 1990 and 1999. The number of loans turns out to be that high because the majority of subsidized firms received equity capital assistance and also at least one loan in the DtA or EPR business start-up program. The distribution of DtA loans over time and federal states in the sample reflects the distribution in the parent population of the DtA data base quite closely. Inconsistent links resulting from connecting one loan to more than one firm concerned only 122 of all 22,000 firms. Altogether, the computerized, heuristic comparison procedure and extensive manual checking led to a reliable merge between both data bases.

B Construction of the final sample

The econometric analysis is based on reduced samples because the following exclusion restrictions were applied to the original firm sample of 22,000 firms:

1. 4,391 firms were excluded in order to restrict the sample to a well-defined population of firms starting their market activity during the period of interest, i.e. between January 1, 1990 and December 31, 1993. The analysis of Creditreform's free flow text showed for the 4,391 firms that the formation dates in 1990 or later encoded by Creditreform are secondary ones. Secondary formation dates indicate legal form changes, relocations or ownership changes.

2. I eliminated 1,683 firms because of missing values or typing errors in basic firm-specific variables.

3. 6,832 firms were discarded because either the human capital or the capital information was missing. To test for sensitivity of the empirical results with respect to this exclusion restriction a program assignment equation was estimated for the finally used sample and for the larger sample including firms with missing human capital or capital information. Both estimations provided very similar results. They are available upon request.

4. I dropped 89 firms with more than 50 employees at market entry because such large firms may represent undetected dependent start-ups or no newly founded firms at all, especially if they did not receive start-up financing by the DtA.

5. I discarded 171 subsidiaries and 159 firms started by owners that were all older than 55 years because these firm groups are not eligible for the considered financing programs.

6. I eliminated those 46 firms with inconsistent links resulting from connecting one DtA loan to more than one firm.

7. 184 firms were dropped because they participated in small DtA programs not evaluated here. 571 subsidized firms were deleted because they received DtA loans in the second

year after their firm formation year or even later. This avoids mixing the effects of start-up assistance with those of expansion assistance also given within the same programs. The empirical results when keeping firms with expansion assistance in the sample are quite similar to the results discussed in the paper. The estimation results for the larger sample are available upon request.

The resulting final sample contains 7,874 firms.

Table 8: Definition of Variables and Descriptive Statistics

Variable	Definition	Mean/ Share	Standard Deviation
Continuous Firm and Entrepreneur Characteristics			
survival time	duration of market activity in days until liquidation or censoring date	2203.58	968.74
size	number of employees	4.2490	5.9196
capital intensity	Euro amount invested in tangible assets per employee	14986.68	34361.75
team size	number of managing owners	1.3474	0.6089
age	age of the managing owner (see notes)	37.4496	9.0406
Discrete Firm and Entrepreneur Characteristics			
subsidized	start-up assistance within the firm formation year or the subsequent year	0.1716	
survival status	survival until end of observation period (12/31/1999)	0.6085	
diversified	industry classifications in more than one 2-digit sector	0.1603	
franchisee	franchisee	0.0244	
ltd. liability & stock corp.	limited liability firm, stock corporation (GmbH, AG)	0.4094	
civil law association	civil law association (GBR)	0.0918	
comm. partnership	commercial partnership (KG, OHG)	0.0128	
sole proprietorship	sole proprietorship (Einzelunternehmen, Gewerbebetrieb)	0.4859	
firm affiliation	partly affiliated, i.e. at least one owner firm but not fully affiliated	0.0702	
female	female managing owner (see notes)	0.1191	
apprenticeship	apprenticeship, low education (see notes)	0.7269	
master craftsman	master craftsman (see notes)	0.1052	
business administration	graduate degree in business administration (see notes)	0.0304	
engineering	graduate degree in engineering (see notes)	0.1092	
other academic degrees	other graduate degrees (see notes)	0.0283	
Industry Indicators			
manufacturing	manufacturing	0.1147	
construction	construction	0.1800	
wholesale & int. trade	wholesale and intermediate trade	0.1243	
retail trade	retail trade	0.2659	
transport & comm.	transport and communication	0.0585	
services	services	0.2565	
high technology	high technology industry in the manufacturing or service sector	0.0810	

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Variable	Definition	Mean/ Share	Standard Deviation
Cohort Indicators			
cohort 1990	firm formation in 1990	0.2634	
cohort 1991	firm formation in 1991	0.2718	
cohort 1992	firm formation in 1992	0.2165	
cohort 1993	firm formation in 1993	0.2483	
Continuous and Discrete Regional Variables			
unemployment rate	unemployment rate at district level in firm formation year (see notes)	10.1295	5.2717
population density	(# inhabitants/square kilometer) in 1992 at district level	1004.15	1181.48
bank office density	((# bank offices * 1000) / # inhabitants in 1992 between 18 and 65) at district level (see notes)	0.7013	0.4354
commercial bank share	(# commercial bank offices / # all bank offices) at district level	0.1812	0.1628
West Germany	firm location in West Germany	0.6149	

Notes: The table contains non-weighted descriptive statistics for the sample of 7,874 firms. Entrepreneur-specific variables refer to the managing owner with the highest equity share if the firm has more than one managing owner. In cases with multiple managing owners, but missing share data the entrepreneur-specific information of the oldest managing owner was chosen instead. Unemployment rates are taken from the statistics of the Bundesanstalt für Arbeit. However, East German unemployment rates for 1990 and 1991 are not available. Therefore, the earliest available monthly unemployment rate, measured in May 1992 is used for East German start-ups in 1990 and 1991. In all other cases I use the unemployment rate in September of the firm formation year. The population density information comes from Bundesamt für Bauwesen und Raumordnung. The number of bank offices in West German and East German districts, taken from the statistics of the Bundesbank is measured in December 1990 and in December 1991, respectively.