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Retaining winners: Can policy boost high-growth entrepreneurship?



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ABSTRACT

We analysed the growth impact delivered by a high-growth entrepreneurship policy initiative over a six-year period. Using an eight-year panel that started two years before the initiative was launched and propensity score matching to control selection bias, we found that the initiative had more than doubled the growth rates of treated firms. The initiative had delivered a strong impact also on value-for-money basis. In addition to producing the first robust evidence on the growth impact delivered by a high-growth entrepreneurship initiative, we contribute to public sponsorship theory with the notion of capacity-boosting activities to complement previously discussed buffering and bridging activities.

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1. Introduction

Although entrepreneurship has been an important policy focus for decades, explicit focus on high-growth entrepreneurship is much more recent (Shane, 2009). In the European Union, for example, the ‘Gazelles’ Expert Group of the Europe Innova initiative submitted its final report in 2008 (Autio and Hoeltzl, 2008). The first policy initiatives exclusively facilitating ‘high-potential’ new ventures were launched in the EU around the same time, and academic work on high-growth policies remains nascent (Mason and Brown, 2013). Therefore, although there is increasing experience on how to design high-growth policy initiatives, little is known about whether such policies actually work. Our objective in this paper is to provide an early examination of the ability of policy to accelerate the growth of high-potential new firms.

Evidence-based policy requires evidence to support it (Sanderson, 2002). Solid evidence is particularly important where policy decisions involve trade-offs across alternative courses of action – i.e., choosing ‘A’ over ‘B’ (Pawson, 2006). High-growth entrepreneurship is a good example of such trade-offs because high-growth policy initiatives select new firms that have potential and motivation for achieving rapid growth

(Autio et al., 2007). Given scarce resources, high-growth policy trades off against more inclusive entrepreneurship policies. It is therefore important to have evidence that such policies are fit for purpose.

High-growth entrepreneurship policy is typically justified by evidence that in any given cohort, only a small proportion of all new entrepreneurial ventures create the bulk of economic benefits – such as new jobs (Acs, 2008; Birch et al., 1997; Shane, 2009). This concentration of impact potential is one of the more widely accepted ‘truths’ in entrepreneurship research and policy (Mason and Brown, 2013). However, concentration of impact does not automatically guarantee that policies to facilitate rapid growth in new firms would be effective – or even feasible. Indeed, it is equally widely accepted that ‘picking winners’ is difficult – and that governments probably are poorly equipped to make this selection (Storey, 1994; Coad et al., 2014). This contrast between phenomenon-based policy justifications and scepticism regarding governments’ ability to effectively implement such policies again underlines the need for solid evidence regarding the effectiveness of high-growth entrepreneurship policy.

Although the need for evidence is evident, assessing the effectiveness of high-growth policy initiatives is challenging. Participation in such initiatives is subject to double selection: only some new ventures self-select to apply for such initiatives, and not all applicants qualify. There is also out-selection, as not all qualifying applicants complete the programme. It can take years for growth effects to materialise: only a handful of high-growth policies have long enough track record for meaningful impact

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evaluation.¹ Controlled experiments (the gold standard of impact evaluation) would be prohibitively expensive. Finally, not all policy initiatives track the performance of their subjects systematically enough to support impact evaluation. Because of such challenges, most policy evaluations struggle to contain selection biases and risk sampling on the outcome variable. Solid evidence on effectiveness high-growth policy remains virtually non-existent.

We address the above gaps by analysing the impact delivered by a Finnish high-growth entrepreneurship policy initiative, the NIY Programme of the Finnish National Technology Agency, Tekes.² A recognised leader in high-growth entrepreneurship policy (Mason and Brown, 2013), Finland is one of the few countries with a long enough history of high-growth initiatives to support the longitudinal design required for unbiased impact evaluation. Started in 2008, NIY is the first Finnish policy initiative that explicitly targets high-potential new firms. In addition to the long time series, we also have rich data to control for selection effects: although the NIY initiative started in 2008, we started collecting data on the underlying population and control groups already in 2006.

We draw on the emerging theory of public sponsorship (Amezcuca et al., 2013; Jourdan and Kivleniece, 2014; Schwartz, 2009) to argue that high-growth entrepreneurship policy should be able to deliver a positive contribution on the growth of high-potential new firms. Because our empirical context consists of a case study of a single – although well-documented – policy intervention, our analysis remains exploratory and evidence-producing, rather than explanatory and hypothesis-testing. We do not ask which features make high-growth entrepreneurship policy initiatives impactful (or not), but rather, whether such initiatives *can* be effective in the first place, given the argued incapacity of governments to effectively facilitate high-growth new firms. We present evidence that selective, hands-on policy interventions that tie support to milestones can enhance new firm growth beyond the selection effect. We find the impact of the NIY Programme to have been robust against the post-2008 global financial crisis. The effect has also been substantial in value for money terms, as one Euro of public funding has generated an average excess increase in sales of 1.11 Euro beyond trend growth by 2013, with the sales growth trend suggesting that the supported firms have been moved to a higher trend-line of growth.

Our research offers several contributions. First, we summarise characteristics that differentiate high-growth entrepreneurship policy initiatives from ‘generic’ entrepreneurship policy. We hope our compilation will serve as a helpful checklist for policy planning. Second, we provide a coherent theoretical grounding for the study of high-growth entrepreneurship policy and extend the applications of the public sponsorship theory towards growth facilitation. Third, we move beyond the ‘picking winners’ argument by describing a policy initiative which, rather than ‘picking’ winners, *retains* winners by applying a series of performance milestones. Finally, we report evidence that policy initiatives can effectively facilitate high-growth new ventures.

In the following, we first review emerging literature on public sponsorship and high-growth entrepreneurship policy and present a list of distinctive characteristics of high-growth policy initiatives. We then present our general proposition. After this, we describe our empirical context and present our methodological choices, data and analysis. We conclude by discussing implications for further research and for high-growth entrepreneurship policy practice.

2. Public sponsorship and new venture growth

Public policy interventions are justified when market mechanisms fail and the production of public-good benefits is possible (Mahoney et al., 2009). Policies facilitating new firm creation and growth meet both criteria. Lacking a track record, new firms face an uphill struggle in accessing and mobilising resources and an elevated hazard to survival (Aldrich, 2008; Stinchcombe, 1965). New firms are also an important source of economic and social benefits, such as job creation, innovation and economic dynamism (Acs and Audretsch, 1988; Acs et al., 2014; Davis et al., 1996). Recognising these benefits, governments are keen to correct the perceived market failures that new firms face in their quest to establish themselves as viable going concerns (Audretsch et al., 2007). Recently, some researchers have taken to calling such interventions ‘public’ or ‘organisational’ sponsorship (Amezcuca et al., 2013; Flynn, 1993; Jourdan and Kivleniece, 2014; Schwartz, 2009).

Distinct from policies that promote specific *activities*, public sponsorship promotes new *organisations* – notably, new entrepreneurial firms. The range of desired organisational outcomes promoted through public sponsorship extends beyond performing a given activity within an established organisational context (e.g., execution of R&D projects, as would be the goal of a classic R&D subsidy) to cover more complex outcomes – specifically, the entry, survival, and growth of new firms.

To achieve these outcomes, public-sector operators provide two broad sponsorship functions: ‘buffering’ and ‘bridging’ (Amezcuca et al., 2013). With buffering, governments provide resources to shelter fledgling firms against adverse effects of internal resource scarcity and external resource dependencies. In addition to financial subsidies, such resources can include, for example, low-cost office space, training and consulting services, tax breaks, and privileged access to government contracts. Bridging facilitates the connectivity of new firms with important external stakeholders. Bridging may include, e.g., networking activities, field building, branding, referral, and tie facilitation with business angels and venture capitalists. Fundamentally, both buffering and bridging seek to ameliorate resource constraints and attenuate resource dependencies that underlie the elevated hazard to survival in new firms (Singh et al., 1986).

Thus far, public sponsorship has mainly focused on increasing the ‘production’ of new entrepreneurial firms by lowering barriers to entry and reducing the hazard of exit (Amezcuca et al., 2013; Schwartz, 2009). This has implied a generalist approach to supporting new firms of all kinds, in the hope that if a greater number of new businesses survive, more firms will also succeed and create new jobs (Shane, 2009). The dominant focus on enhancing survival also implicitly acknowledges the stylised fact that governments have no business picking winners, given the difficulty of recognising future high-growth businesses *ex ante* (Cantner and Kösters, 2012).

The generalist focus has dominated entrepreneurship policy for decades, although the relative emphasis on buffering and bridging has evolved over time. In response to the focus Birch (1979) helped introduce upon new firms in industrial policy, the 1980s and 1990s witnessed a wave of policy initiatives to encourage new firm creation. Such initiatives included new business incubators; subsidised loans; initiatives seeking to alleviate regulatory burden; and, for example, initiatives to turn unemployed into entrepreneurs (Shane, 2009). While some new firm creation policies elevated entry, it was soon realised that mere creation of new businesses is not very helpful if the resulting new firms are of poor quality. For example, initiatives to get unemployed to start new businesses are not very productive (Shane, 2009). Specifically, the finding that small firms were effective in innovation (Acs and Audretsch, 1988)

¹ Examples include the Dutch Mastering Growth Programme and UK’s Gateway2Investment (g2i) Programme, see Autio et al. (2007).

² NIY is the Finnish acronym for “young innovative growth companies” (see <http://www.tekes.fi/en/funding/companies/funding-for-young-innovative-growth-companies/>).

prompted policies to support innovative new firms through science parks and R&D loans and subsidies.

However, the majority of science park tenants remained small, expert-intensive engineering outlets (Siegel et al., 2003).³ Converting innovation into rapid firm growth requires skill, much of which can only be gained through experience. This insight spawned numerous initiatives to boost the supply of equity funding towards new ventures from late 1990s onwards (Bottazzi and Da Rin, 2002; EU Commission, 1998). The idea was that equity funding comes with hands-on participation that helps unlock growth potential (Da Rin et al., 2006). Many early funding schemes were heavily subsidised from public funds, however, and the growth of private-sector equity funding was slower. It was also learned that effecting new venture growth is difficult without extensive contacts that open doors to growth opportunities (Sorenson and Stuart, 2001).

As experience from public sponsorship activities has accumulated, the understanding of the contribution of entrepreneurship towards economic growth has grown more nuanced. A particularly influential realisation concerned the importance of high-impact new ventures (Acs, 2011; Autio, 2007; Birch et al., 1997). This realisation has prompted an increasing policy focus on high-impact entrepreneurial businesses (Autio and Hoeltzl, 2008; Henrekson and Johansson, 2010). This trend, however, would appear to conflict the stylised fact that picking winners is no business of governments. We therefore take a closer look at the 'picking the winners' argument.

The 'picking winners' argument made its way into entrepreneurship policy conversation in the late 1980s (Birley, 1987), but the argument only gained wider traction in the early 1990s (Storey, 1994). This term was first introduced in the context of technology foresight in 1983, with the message that picking winners was exactly what governments *should* do: invest in emerging technologies that held promise of significantly impacting economic development (Irvine and Martin, 1984). This concept clashed with the Thatcher government's ideological stance that such choices should be left to 'the market' (Martin, 2010). It was in this form (i.e., that governments should *not* pick winners) that the concept was introduced into the entrepreneurship policy discussion (Storey, 1994). Here, two key arguments were that, first, predicting the success of any given venture is difficult even for venture capital professionals, and second, that by favouring some firms over others, the government may unwittingly crowd out viable alternatives (see David et al., 2000).

While informing survival, the public sponsorship theory has paid less attention to new venture growth. The most salient arguments have focused on buffering and bridging (Amezcuca et al., 2013). The buffering argument is fundamentally passive, as new ventures are assumed to survive if insulated against harsh market realities. On the other hand, bridging is fundamentally a legitimacy and externality argument: field creation enhances collective legitimacy of new types of firms, facilitating access to external resources and enhancing survival. Bridging also promotes knowledge spillovers and experience exchange. These mechanisms should help facilitate not only survival, but also, growth.

We suggest that to better understand the impact of public sponsorship on organisational growth, capacity-boosting mechanisms need to be considered ('boosting' for short, to continue the series of b's). While insulation may promote survival, survival does not automatically translate to growth. To achieve growth, firms need to actively pursue it – and they need an organisational capacity to effect growth (Zahra et al., 2006). We therefore propose a third

mechanism through which public sponsorship may yield benefits: the boosting of organisational capacities for growth.

How could public sponsorship help boost organisational capacity for growth? Thus far, only few studies have explored the anatomy of high-growth entrepreneurship policy initiatives. In their study of nine countries, Autio et al. (2007) concluded that one way to side-step the picking winners problem is by introducing a selection logic that emphasises *retention* over selection. In this logic, relatively loose selection criteria would be used in initial selection, and support would grow more substantial as the firm meets growth milestones. The initial selection should emphasise strong growth motivation and require some check of ability, but the capacity for growth would be demonstrated by meeting milestones. They also recommended public–private partnerships to provide customised, hands-on support to build organisational capacity for growth. Finally, they emphasised networking among peers to disseminate experience-based insights on how to effect organisational growth. Mason and Brown (2013) additionally emphasised hands-on support for internationalisation and the implementation of good governance structures to strengthen the new firm's capacity to design and implement a proactive growth strategy.

The review above suggests that public sponsorship could facilitate new venture growth by emphasising boosting mechanisms. Although the one-off selection logic of picking winners has been widely rejected, it appears that the picking winners problem could be alleviated by capacity boosting – i.e., targeting high-potential new ventures with policy initiatives that are (1) highly selective; (2) emphasise strong growth motivation as a key selection criterion; (3) control milestone achievement and condition progressively more substantial and hands-on support on the achievement of specific milestones; (4) promote the exchange of experiential insights on how to effect rapid organisational growth; and (5) rely on public–private partnerships for hands-on, capacity-boosting support. We advance that public sponsorship that emphasises capability boosting should be able to overcome shortcomings inherent in the 'picking winners' approach. We therefore propose:

Proposition 1 – Policy initiatives that are selective, impose milestones and focus on capacity boosting are able to accelerate new firm growth.

Our goal being to explore whether high-growth entrepreneurship policy *can* effectively sponsor new venture growth, this proposition is designed to test whether the null hypothesis can be rejected. The null hypothesis is that no impact can be observed. We are not going to test whether any or all of the above listed characteristics can be linked to organisational growth outcomes either individually or as a group. As the classic discussion by Pressman and Wildavsky (1984) demonstrates, the successful implementation of any given type of policy initiative is always subject to uncertainty. We also do not imply that the above characteristics are exclusive to high-growth entrepreneurship initiatives only, nor do we take a position on how many of the characteristics need to be in place in order for a given policy initiative to qualify as a 'high-growth' initiative. Consistent with our objective of evidence production, the rejection of the null hypothesis will suffice – i.e., the demonstration with reasonable confidence that a given policy initiative has produced a discernible impact on the growth of treated new firms. We next discuss our empirical context.

3. Empirical context: the NIY Programme in Finland

Our objective implies an evidence-producing rather than a hypothesis-testing research design. As such, exploring the impact of a high-growth entrepreneurship policy initiative is challenging. Especially in new firms, growth is a noisy outcome (Davidsson and

³ As many of these studies readily acknowledged, their designs did not allow control for selection bias. However, the reported evidence itself was widely noticed.

Wiklund, 2006; Delmar et al., 2003). New firms often grow in spurts, with periods of rapid growth punctuated by periods of slower growth and even decline. Because of the need to build legitimacy, it can take a long time to effect growth in new firms (Stinchcombe, 1965). Because of biases introduced through selection, a quasi-experimental design is required that permits the demonstration of a meaningful impact of the initiative on growth trend before and after the intervention. These challenges mean that to satisfactorily demonstrate a growth effect, longitudinal data is needed from both treated and meaningfully similar untreated firms, with a long enough time period to permit the examination of growth trends both before and after the intervention occurred. These are important challenges, in terms of both data access and given that policy initiatives explicitly targeting high-growth entrepreneurship are relatively young.⁴

Given these challenges, we chose Finland as our empirical context. Finland is widely recognised as a leader in high-growth entrepreneurship policy (Mason and Brown, 2013): it has experimented with this policy focus from mid-2000s onwards. Finland is also a small and homogeneous country where access to information is open and policy initiatives routinely evaluated. This ensures good data coverage. Policy agencies in Finland are also relatively few and centralised in the high-growth entrepreneurship area, with the Ministry of Trade and Employment coordinating the efforts of subsidiary agencies. This reduces noise from policy overlaps. Finally, we were able to persuade the National Technology Agency Tekes to cooperate in data collection over eight years. This is a remarkably long time horizon for any policy agency, given there was no guarantee that the outcomes would eventually show the agency in favourable light. We thus had a rare, almost unparalleled empirical window to study a phenomenon of increasing policy relevance.

We selected the NIY Programme of Tekes as our empirical window. NIY is the first Finnish entrepreneurship policy initiative that explicitly seeks to facilitate the growth of new, entrepreneurial firms. In planning since 2007 and launched in 2008, the NIY Programme seeks to accelerate the growth and internationalisation of high-potential innovative new firms in Finland. The NIY Programme exhibits all characteristics of high-growth entrepreneurship policy initiatives, as reported in the literature review: selectiveness, emphasis on growth motivation, capacity building, hands-on support, networking, public-private collaboration and the use of performance milestones.

NIY selects its participants from a pool for applicants. While Tekes makes the final decision, this decision is informed by an external expert panel comprising new venture experts and venture capitalists who interview each applicant. To be eligible for the NIY Programme, the new venture must: (1) exhibit strong growth motivation and good growth potential; (2) show a good-quality business plan and demonstrate capacity to implement it; (3) show evidence of promising business activities and customer references; (4) demonstrate a competitive advantage that can help it reach a strong market position; and (5) have a committed and competent management team (Tekes, 2013). The NIY Programme is open for young (under six years old) firms that employ less than 50 people with a maximum sales turnover of €10M and a balance sheet totalling €10M at most. The applicants must also have recorded at least 15% R&D expenditure during the previous three years, and they must be domiciled in Finland (Tekes, 2013).

⁴ Google Scholar searches suggest that terms such as “high-growth entrepreneurship” and “policy” did not start co-appearing in the scholarly literature until mid-2000s onwards. We checked several combinations such as ‘high-growth entrepreneur’ AND ‘policy’; ‘gazelle’ AND ‘policy’; ‘high-impact entrepreneur’ AND ‘policy’.

Table 1
NIY participants (treated firms) by start year and funding phase.

NIY Phase	Year the firm was selected into the NIY Programme					
	2008	2009	2010	2011	2012	Sum
Phase 1	–	–	–	4	30	34
Phase 2	1	–	5	33	1	40
Phase 3	–	6	10	2	–	18
Completed NIY	12	12	7	3	–	34
Interrupted	8	13	9	4	–	34
Sum	21	31	31	46	31	160

Table 2
Age, length of participation in NIY, and Tekes funding received by NIY firms by NIY funding phase.

Phase (12/2012)	Firms	Age (2013)	NIY participation	NIY funding (mean)
Phase 1	34	4.2 years	244 days	€255k
Phase 2	40	5.3 years	635 days	€631k
Phase 3	18	6 years	961 days	€983k
Completed NIY	34	6.7 years	950 days	€1013k
Interrupted	34	6.7 years	749 days	€488k

The primary focus of the NIY Programme is capacity boosting for growth, but it also offers bridging services. As a boosting service, NIY Programme offers financial support for commissioning expert services for business planning, growth strategy development and strengthening the management competencies of the firm. Up to €1M public funding can be granted in several instalments for developing the participating firms’ organisational capacities in a hands-on fashion.⁵ As a bridging service, the NIY Programme facilitates active networking among its participants and exchange of experiences and good practices. NIY also facilitates links with domestic and international venture capitalists. In itself, the NIY Programme operates as a branding mechanism that enhances the credibility of its participants.

The NIY Programme applies milestone design and uses an external evaluation panel. Upon selection, Tekes sets customised milestones for each participant. Participating firms need to meet their milestones in order to remain in the programme. In the first phase, the participants have to successfully embark on a growth track and demonstrate ability to compete in international markets. At the end of each phase, participants present their progress to an evaluation panel of venture capital investors, business angels and board professionals. The panel assesses participants’ growth potential, development needs, and their suitability as an investment target. During consecutive phases, the participating firm has to progressively build up its organisation to sustain rapid growth, and also, attract non-trivial external funding. Depending on the firm’s rate of growth, funding support can be granted in one or more instalments until the firm is eight years old.

By the end of 2012, 160 innovative new firms had received NIY funding. Of these, 34 firms had completed the Programme. A further 34 firms had exited the Programme because of failure to meet milestones. The rest of the participating firms were continuing the Programme in different phases, as shown in Table 1.

The milestone configuration is illustrated in Table 2. The 160 firms that had participated in the NIY Programme by the end of year 2012 are grouped by funding phase. By the end of year 2012, those firms that had completed the full NIY Programme had received, on average, some €1M funding from Tekes; they were 6.7 years old; and they had spent 950 days in the Programme. For those firms

⁵ As NIY does not offer low-cost office space or subsidies to cover operating costs, it does not strictly offer buffering services. It does support R&D, though, which has a buffering effect.

that participated in the first phase of NIY only, the mean age was 4.2 years; the length of participation was 244 days, and the funding received amounted to €255k per participating firm.

4. Data and methodology

4.1. Data

According to a recent estimate, the Finnish technology-based new firm (TBNF) population consists of approximately 12,000 firms less than seven years old. TBNFs are young, entrepreneurial firms that develop and commercialise new technologies through their products and services or apply new technologies as a distinctive aspect of their operations. In this context, 'entrepreneurial' means that the firm is not majority-owned by an industrial conglomerate. Technology-intensity is defined in terms of industry sector (i.e., NACE) affiliation and includes technology-intensive services (e.g., internet service providers). Of the 12,000 firms that meet these criteria in Finland, some 1200 are estimated to be strongly growth oriented (Autio et al., 2014). This group constitutes the base population for our analysis. Relative to the estimated overall population of TBNFs in Finland, NIY participating firms represent only 1.3%, and relative to the sub-population of strongly growth-oriented TBNFs, NIY firms represent 13%. Thus, the coverage of the NIY Programme is quite notable relative to the target population of strongly growth-oriented TBNFs.

We collected comprehensive longitudinal data from both the treatment and control groups. The treatment group consisted of TBNFs that had applied and were admitted into the NIY Programme (also referred to as 'treated firms' later). The performance of this group was compared against two control groups. The first control group consisted of TBNFs that were customers of Tekes (a sign of technology intensity) but had not applied for the NIY Programme (also referred to as 'non-applied firms' later). The second control group consisted of TBNFs that had applied to the NIY Programme but had not been admitted (also referred to as 'untreated firms' later). We started collecting data on the first control group two years before the NIY Programme started, in 2006.

We collected longitudinal data on all groups starting either in 2006 (the Finnish population of TBNFs) or in 2008 or later (NIY applicants). The data was collected from annual tax filings, bi-annual mail and web surveys, surveys conducted during the application phase (i.e., after the firm had applied for the NIY Programme but had not yet been informed of the application outcome), and from Tekes records (project information and funding information). We conducted two waves of data collection per year between 2008 and 2013. The survey questionnaires queried the firms' growth orientation and growth strategies (i.e., how aggressively the firm pursued a growth strategy) and the firms' internationalisation strategies. Multi-item scales were used to capture constructs such as growth orientation, growth strategies, internationalisation orientation and perceived barriers to growth (notably, external resource mobilisation).

We concentrate on the 160 treated firms that were selected to participate in the NIY Programme by the end of year 2012. Our analysis does not differentiate between those firms that did not complete the programme and those that received full support. To construct the untreated outcome of untreated firms (i.e., the counterfactual) we use as control group NIY applicants that were not accepted in the Programme ('untreated firms'). All NIY applicants received our survey questionnaires. In both treated and untreated groups we model selection through survey respondents. In the results section we also descriptively compare NIY firms' growth against the broader reference group that comprised TBNFs that were customers of Tekes but did not apply to the NIY Programme ('non-applied firms').

4.2. Data

Given that we have data to control selection into the intervention as well as performance data both before and after the intervention, we chose propensity score matching with non-parametric regression to estimate the average treatment effect on treated firms (Heckman et al., 1997). This method is called difference-in-difference matching when applied to two-point data. The idea behind propensity score matching is to replace random comparison of treated and untreated study objects, which is not feasible in observational studies, by a comparison of treated and untreated study objects that are matched by observable pre-treatment attributes. Non-parametric regression uses all possible control (i.e., untreated) units within a given time span as matches, whereas most other matching algorithms use 1-to-1 or 1-to- n matching.

The matching method makes two important assumptions. First, it assumes that conditional on a set X of observed firm attributes and unobserved fixed effects, treatment participation (selection) is random for those firms that have similar qualities (conditional independence assumption also known as selection on observables, or un-confoundedness). Second, for each value of X there is a positive probability of being treated or untreated (common support condition). When these conditions apply, the treatment assignment is said to be strongly ignorable (Rosenbaum and Rubin, 1983). In the case of Average Treatment effect on Treated firms (ATT for short), these conditions may be relaxed (Heckman et al., 1997). For the former, mean independence is sufficient condition, while for the latter it suffices that the probability of being treated (or not) is strictly below one but may equal zero. Propensity score matching with difference-in-difference approach allows removing bias resulting from unobserved differences as long as these are fixed in their influence on outcomes. Thus, an additional assumption constraining our model is that unobserved factors are stable in their impact on firm-level sales growth and that sales growth trends are the same for both treated and untreated firms (parallel trend assumption). Under these circumstances the realised sales growth of treated firms corresponds to a genuine treatment effect above trend, whereas the untreated firms' sales growth equals the trend.

The first step of the matching analysis is to estimate the propensity score, $p(x)$ through probit or logit regression. This is the conditional probability for a given firm to be selected into the NIY Programme. We define selection into the NIY Programme as the 'treatment', or D . For selected (i.e., treated) firms, $D = 1$, otherwise 0. The binary choice model thus takes the form:

$$D_{it} = \begin{cases} 1 & \text{if } \beta X_{it} + \epsilon_{it} > 0 \\ 0 & \text{otherwise} \end{cases}$$

Here, D_{it} is binary variable that defines the firm's participation (selection) status in year t . X_{it} is a vector of factors that affect the firm's probability of being selected into the NIY Programme, and ϵ_{it} is the error term. Since selection takes place on observable attributes at the time of selection, we include measures from that time point in the vector X_{it} .

The next step of the matching analysis is to apply the received propensity score to estimate the average effect of the policy intervention. Since our performance data is longitudinal and covers time both before and after the treatment, we apply a difference-in-differences estimator (Heckman et al., 1997). This estimator compares the difference in the outcome before and after the treatment of treated units against the difference in the outcome of untreated units during the same period. The average treatment effect on treated firms (denoted as ATT) for the difference-in-difference estimator can be written as:

Table 3
Descriptive statistics.

Variable	Treated firms	Untreated firms
	<i>n</i> = 160 Mean	<i>n</i> = 66 Mean
Financial resource mobilisation	4.27**	4.80
Growth orientation	5.71***	5.15
Growth self-confidence	4.14	4.42
Perceived feasibility of international growth	6.25**	5.79
Year of founding	2007	2005
Sales (year 2007)	€369k	€385k
Sales (year 2008)	€441k	€402k
Region.Capital (1 = Helsinki region, 0 otherwise)	0.65+	0.51
Region.Tampere (1 = Tampere region, 0 otherwise)	0.05	0.04
Region.Oulu (1 = Oulu region, 0 otherwise)	0.05	0.02
Industry_HTM (1 = HTM, 0 otherwise)	0.02	0.04
Industry_KIHTS (1 = KIHTS, 0 otherwise)	0.63***	0.32

Note: +, *, **, ***, indicate significance levels of 0.1%, 1%, 5% and 10%, respectively (two tailed).

$$ATT = \frac{1}{N_t} \sum_{i \in T \cap S_p} \left[\left(Y(1)_{it} - Y(0)_{it-1} \right) - \sum_{j \in C \cap S_p} \omega_{ij} \left(Y(0)_{jt} - Y(0)_{jt-1} \right) \right]$$

Here, D_{it} is treatment outcome for firm i , and $Y(0)_j$ is non-treatment outcome (comparison group) for unit j . T is post-treatment period, and $t - 1$ is the pre-treatment period. N_t is the number of observations in the treatment group T , C denotes the set of control units, S_p denotes the region of common support, and ω_{ij} is the weight used to match control units with each treatment unit. Each treated firm has a difference, $(Y(1)_{it} - Y(0)_{it-1})$, and its multiple matched pairs have average differences $\sum_{j \in C \cap S_p} \omega_{jt} (Y(0)_{jt} - Y(0)_{jt-1})$. The difference between these two values yields the difference-in-difference that measures the average change in outcome that is the treatment result for each treated case i . The difference-in-differences estimate for treated cases is obtained by taking the average over all treated cases in the sample.

Two limitations apply to empirical applications of propensity score analysis with non-parametric regression. First, since there is no parametric test available, bootstrapping is used to estimate the standard error of the sample mean difference between treated and untreated cases. Although this is common practice, it is suggested that results should be interpreted with care especially in case of small samples (Guo and Fraser, 2010). Second, because of the first problem, researchers should study the sensitivity of their results by experimenting with different bandwidth levels and by trimming (i.e. discarding non-parametric regression results in regions where the propensity scores for the untreated cases are sparse). If the estimated treatment effects exhibit sensitivity to different bandwidth levels and different trimming schedules, the results should be interpreted with particular caution (Guo and Fraser, 2010). This, however, was not the case in our study (see Table 5).

4.3. Variables

In the empirical estimation, our outcome variable is sales growth. Objective descriptions of firm performance, realised sales in particular, are easier to compare across different sectors than, for example, balance sheet assets. In technology-based new firms (TBNFs), the number of employees tends to lead sales growth, and it tends not to be as volatile as sales. However, sales may or may not materialise after recruiting new employees. In contrast, sales represent demonstrable, real commitments by the customers of the TBNF to take on its technology offering. We operationalised sales growth as the log difference of sales between the year before the firm applied to the NIY Programme and realised sales during each of the three subsequent years. Thus, three different measures

Table 4
Estimation of propensity scores (probit model).

Variable	Coefficient	Std. Err.
Financial resource mobilisation	0.19*	0.11
Growth orientation	0.18*	0.10
Growth self-confidence	-0.19*	0.11
Perceived feasibility of international growth	0.26*	0.14
Founded_year	0.11	0.08
Region.Capital	0.12	0.36
Region.Tampere	0.65	0.65
Region.Oulu	0.37	0.68
Industry_HTM	0.90	0.77
Industry_KIHTS	0.58*	0.32
Constant	232.18	176.44
Pseudo R squared: 0.19		
LR: 23.03*		
Number of observations: 87		

Note: +, *, **, ***, indicate significance levels of 0.1%, 1%, 5% and 10%, respectively.

of log differences in sales were used. From these three measures, the first log difference of sales (i.e., the difference from $t - 1$ to t) is used to control for the parallel development assumption. The last two measures (i.e., the difference from $t - 1$ to $t + 1$ and the difference from $t - 1$ to $t + 2$) are used to model programme impact. Table 3 shows the sales level for 2007 and for 2008 for both treated and untreated firms. Note that there is no initial difference in the sales of the treated and untreated firms in either year.

In the probit model of the PSM analysis procedure we use self-reported survey items as well as sector, location and firm age to identify factors that drive selection into the NIY Programme. The choice of predictor variables was based on three principles. First, given the low number of observations, the number of predictor variables is also limited. Second, since we wanted to utilise maximally the heterogeneity in survey answers, we chose to apply single-item variables instead of multi-item scales. Third, as NIY uses clear selection criteria, the variables in the probit model needed to reflect these.

As a reminder, NIY selection criteria emphasise international growth potential of the firm, its growth orientation, and the suitability of the firm as an investment target. Given the qualitative nature of the selection criteria, we believe that survey data, collected at the time of applying, presents a valid basis for modelling the information on which the selection is based. Moreover, Angrist and Pischke (2009) suggest that predictors that could appear as outcome variables or variables that are affected by the treatment status should be avoided. In our case, this applies particularly to the number of employees. Consistent with these principles, we selected four predictor variables from our survey data that emphasise the suitability of the firm as an investment target, internationalisation, and growth orientation. First, the firm's perception regarding how easy it would be to access and mobilise financial resources proxied the suitability of the firm as an investment target. Second, to measure growth motivation we asked the entrepreneurs to indicate their preference between two conflicting goals: "we prefer longevity over growth" or "we prefer growth over longevity". Third, related to both growth and resources, we asked respondents to indicate their agreement with the statement: "we could easily acquire the resources to grow rapidly". Fourth, we requested an estimate on the feasibility of international growth with the following item: "it makes more sense for us to grow internationally than domestically". All these data were collected with a survey questionnaire before the admission decision was known. As we have three cohorts of NIY applicants, the survey data for the cohort of 2008 dates from year 2008, for the cohort of 2009 from year 2009, and for the cohort of 2010 from year 2010, respectively.

The ‘financial resource mobilisation’ variable was measured with a reverse scored⁶ seven-point Likert scale (1 = very easy, 7 = very difficult). As seen in Table 3, the treated firms perceived financial resource mobilisation as slightly easier (mean = 4.3) than untreated firms (mean = 4.8; $p < 0.05$). ‘Growth orientation’ was measured with a 7-point Likert scale (1 = fully agree). Table 3 shows that treated firms (mean = 5.71) exhibited higher growth orientation than untreated firms (mean = 5.15; $p < 0.05$). ‘Growth self-confidence’ was also measured with a seven-point Likert scale (1 = fully agree). In this case, untreated firms (mean = 4.42) exhibited greater growth self-confidence than their accepted counterparts (mean = 4.14), but the difference was not statistically significant. ‘Feasibility of international growth’ was also measured with a seven-point Likert scale (7 = fully agree). Treated firms (mean = 6.25) perceived internationalisation as more feasible than untreated firms (mean = 5.79; $p < 0.05$). Statistically significant differences are not a problem as such, since the propensity score matching technique pairs treated firms with closely similar untreated firms.

As additional predictor variables in the probit model, we used firm age, industry sector, and the geographic location of the firms. Although all firms in both treatment and non-treatment groups were young, it may be that slightly older firms would have had more time to commercialise their ideas and therefore be more likely to join the NIY Programme. Therefore it was necessary to control firm age in the probit model. Furthermore, growth rates may vary by sector, and regional entrepreneurial ecosystems may differ in their ability to support venture growth. Therefore, both the industry sector of the firms and their location were controlled in the model.

As can be seen in Table 3, the treated firms (founded in 2007 on average) were slightly younger than the untreated firms (founded in 2005 on average). Two dummy variables were used to control industry sector effects. Industry_HTM dummy indicates whether the firm operated in high-technology manufacturing, as defined by Eurostat (yes = 1). Industry_KIHTS dummy indicates whether the firm operated in knowledge-intensive high technology services, as defined by Eurostat (yes = 1). Also other sector dummies were created but were eventually excluded from the model (no significant effect on outcomes was observed). As the industry distribution shows in Appendix A, most of the firms operated in knowledge-intensive high-technology services. Firms in this category mostly provide software design and programming services. Finally, three region dummies were included in the model. The first of these, Region_Capital indicated whether the firm was located in the Helsinki capital region. Region_Tampere indicated whether the firm was located in the Tampere region. Region_Oulu indicated whether the firm was located in the Oulu region. Also two additional location dummy variables were tested but eventually excluded from the analysis, as they predicted NIY selection perfectly.

5. Analysis and results

We present two kinds of analysis here: a descriptive analysis of sales growth patterns and the PSM analysis that controls for selection effects. The descriptive analysis is shown to illustrate sales growth patterns. In this analysis – which does not control for selection effects – we use both control groups, as explained above – i.e., the non-accepted NIY applicants (‘untreated firms’) and Tekes

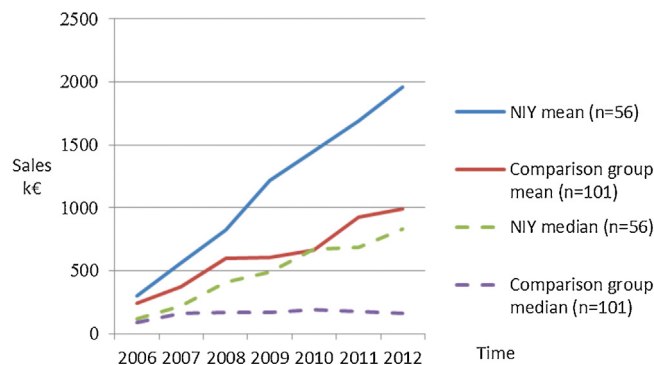


Fig. 1. Mean and median sales for the treated NIY firms' group and non-applied firms' comparison group between 2006 and 2012 (k€).

customer TBNFs who did not apply for admission into the NIY Programme (‘non-applied firms’). Fig. 1 shows treated firms’ ($n = 56$) and non-applied firms’ ($n = 101$) sales growth between 2006 and 2012 for those firms for which we had full time series of sales data from 2006 to 2012. The Y axis unit is thousands of Euro. The NIY group consists of all firms that have participated the NIY Programme, independent of the duration of their participation.

Fig. 1 shows that the starting levels of mean and median sales were similar for both groups. For mean sales, both groups started from approximately €300k in year 2006, and the median sales were approximately €100k for both groups in the same year. Interestingly, the development of both mean sales and median sales is similar in both groups until year 2008, when the NIY Programme started. After 2008, the median sales curve plateaued for the non-applied firms’ group, and its mean sales growth curve stagnated. Note that this stagnation coincides with the start of the global financial crisis. The development in the non-applied firms’ group is in contrast with the treated firms’ group, whose mean and median sales kept growing in 2009–2012. In 2012, the mean of sales for the treated firms was approximately €2M, while for the comparison group the mean of sales was approximately €1M. The difference in the development of median sales is similar. As such, these figures provide a tentative suggestion that the NIY Programme may have had an effect on its participating firms that goes beyond selection – a suggestion reinforced by the statistical analyses that follow.

Whereas Fig. 1 illustrates the sales growth of all treated firms against firms that did not apply for the NIY Programme, we next compare treated firms against untreated NIY firms without matching.⁷ In Fig. 2, the mean sales levels for both treated and untreated firms are plotted on the same scale independent of whether the firms applied to NIY in year 2008, 2009, or 2010. Thus, on X axis, zero denotes the start (acceptance) of the intervention (or non-acceptance). Values to the left from zero indicate pre-treatment time, and values to the right from zero refer to post-treatment time. Again in this graph, the Y axis represents sales in thousands of Euro, and the analysis includes all firms for which we had full sales time series from 2007 to 2012 (40 treated firms and 41 untreated firms).

In essence, the graphs show that before the treatment, the sales growth is broadly similar in both groups, and major differences only appear after the treatment begins. For the treated firms’ group, the mean sales in the two years before the treatment was €351k, and it grew to €967k by the year when firms were accepted to the NIY Programme. The growth of treated firms is largely similar to growth

⁶ To reduce data contamination due to questionnaire design (i.e., the tendency of respondents to score sets of consecutive questions similarly when completing a questionnaire), some scales were reverse scored in the questionnaire. This reverse scoring was subsequently removed in the analysis and tables to enhance coherence and readability.

⁷ We are not illustrating trends relative to the out-selected NIY firms (i.e. firms that were admitted to NIY but did not complete the programme), because we only had full time series data for six of those firms from 2006 to 2012.

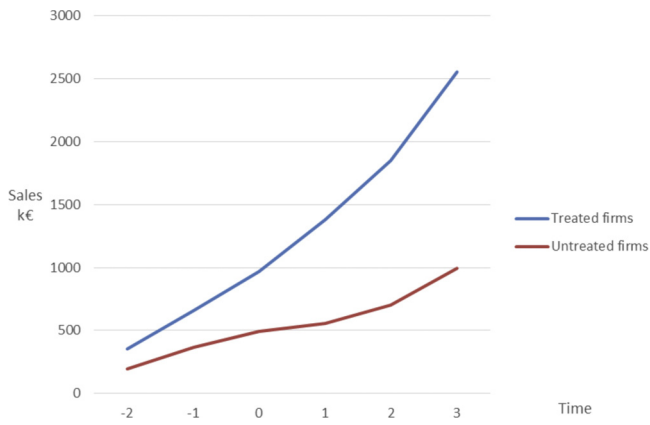


Fig. 2. Mean sales for treated firms' group and untreated firms' group from two years before application time to three years after the application time (k€).

realised in the untreated firms' group: for the firms that applied but were not accepted to NIY, the mean sales two years before the NIY application was €193k, and it had grown to €490k by the year the firms applied but were not accepted to the Programme. In visual check, differences between the two groups only seem to appear during the post-treatment period. While the mean sales for the treated group are €2554k three years after the intervention start, the untreated firms' development has led to mean sales of €996k. Thus, although both figures exhibit positive development, there is a notable difference in the post-treatment sales growth.

Together, the graphs in Figs. 1 and 2 suggest a positive effect of the NIY intervention on the sales growth of its participating firms. Note that the graphs illustrate unmatched data, whereas our statistical tests use matched data, so the graphs and statistical tests are not directly comparable. That noted, the graphs in Fig. 2 support the correctness of our assumption regarding parallel trend between treated and untreated groups prior to treatment. This assumption receives further support in the analyses that follow (see also Appendix B)

We next consider the PSM analysis.⁸ For this analysis we used more observations, as we no longer required full time series of sales data from 2006 to 2012. We first estimated a probit model to predict the probability of acceptance into the NIY Programme (1 = accepted) and obtain the propensity score $p(X)$ for selection. Table 4 shows the results of this estimation. Our survey-based explanatory variables appear efficient in predicting selection into the NIY Programme ($p < 0.1$). The easier the respondents perceived financial resource mobilisation, the more probable it was that the firm was admitted into the NIY Programme; the more the respondents emphasised growth over survival as a strategic objective, the more probable it was that the firm was admitted into the NIY Programme ($p < 0.1$); and the more feasible the respondent perceived international growth to be, the greater the probability of admission into the NIY Programme ($p < 0.1$). The growth self-confidence also exercised an effect on selection at 10% level, but the sign of this term is opposite to expectation: the more difficult the respondents perceived growing their business, the higher was the probability of selection into the NIY Programme. This may mean that the selected firms are more realistic concerning the difficulty of achieving growth and concerning the challenge of managing organisational growth. As for the other variables, the dummy for knowledge-intensive high-technology services was the only sig-

nificant predictor, indicating that firms operating in IT sectors had a higher probability of being accepted into the NIY Programme.

Table 5 shows the estimated average treatment effects for the treated firms before ('Unadjusted mean difference') and after accounting for selection effects (subsequent rows). All analyses are carried out using kernel matching and imposing common support condition. Confidence intervals are calculated using clustered standard errors by industry. As outcome variables we have three different log differences: $\log[\text{sales}(t)] - \log[\text{sales}(t-1)]$; $\log[\text{sales}(t+1)] - \log[\text{sales}(t-1)]$; and $\log[\text{sales}(t+2)] - \log[\text{sales}(t-1)]$. As a reminder, the first outcome variable tests the assumption of parallel trend prior to the start of the treatment and does not include any treatment effect. For this variable, a non-significant difference between treated and non-treated firms would signal that the parallel trend assumption cannot be rejected. The two remaining outcome variables capture treatment effects after one and two years of treatment, respectively. The sensitivity of the results was checked by applying different bandwidths and by trimming (e.g., Guo and Fraser, 2010). The results in Table 5 suggest that the NIY Programme has had a positive impact on the sales growth of its participating firms beyond the selection effect. In Table 5, the unadjusted mean difference shows the difference in log differences between *unmatched* treated and untreated firms (i.e., not accounting for selection effects). The first row shows that for the first outcome variable there is a positive difference of differences, but this is not statistically significant. As there is no statistically significant difference in pre-treatment growth between treated and untreated firms, the assumption of parallel trends is upheld. As for the two other outcome variables that cover different treatment periods, we see a positive, statistically significant difference in log differences between treated and untreated groups. These statistics (without matching) indicate that post-treatment sales growth for treated firms outperformed the sales growth of untreated firms. The next row ('Bandwidth (default)') shows the differences in log-differences *after* kernel matching. In this case, the sales growth differences are larger and statistically significant for post-treatment periods, whereas the difference for the pre-treatment period is also larger but not statistically significant. Thus, the parallel trend assumption is again upheld. Note that for difference-in-difference point estimates, the statistical significance denoted by "*" does not take into account that the propensity score is estimated. Therefore, for these also the bootstrapping-based 95% confidence interval for the difference of log differences is also provided. For post-treatment periods this interval does not include zero, indicating that the difference-in-differences is statistically significant. For the pre-treatment difference-in-differences, the confidence interval suggests that the difference is not statistically significant, consistent with the parallel trends assumption. As Table 5 further shows, the results of the PSM analysis are consistent across trimming and bandwidth specifications, and also, across different dependent variables. We also ran bootstrapping with clustering for industry, and results from this analysis are consistent with our overall findings.

In summary, therefore, we conclude that there is good evidence that the NIY Programme has created a significant average treatment effect on treated firms (ATT) which is persistent over the inspection span of three years.⁹ Point estimates suggest that the two-year average treatment effect on treated firms is 1.20, and three-year

⁸ We also conducted robustness checks using the 2-stage Heckman approach. Also this method confirmed substantial growth effect by the NIY Programme after controlling for selection effects.

⁹ Note that our design is conservative, as we included in the treated group also firms that were accepted into NIY but did not complete the programme. On average, the out-selected NIY firms had received €488k support per firm. As the out-selected NIY firms had not received full treatment, the effect of this group is to drag down the growth performance of the treated group.

Table 5
Estimated average treatment effects for the treated firms on sales growth: difference-in-difference estimation (DID) by kernel matching (common support condition imposed in all analyses).

Group and comparison	Outcome measures: differences in logarithms of sales		
	$\log[\text{sales}(t)] - \log[\text{sales}(t-1)]$	$\log[\text{sales}(t+1)] - \log[\text{sales}(t-1)]$	$\log[\text{sales}(t+2)] - \log[\text{sales}(t-1)]$
Mean difference			
Unadjusted mean difference (Std. Err.)	0.44 (0.28)	0.70* (0.34)	0.84* (0.38)
Number of observations	Treated, $n = 66$ Untreated, $n = 64$	Treated, $n = 67$ Untreated, $n = 58$	Treated, $n = 64$ Untreated, $n = 54$
Bandwidth (default)			
Difference-in-difference point estimate (Bias corrected 95% confidence interval)	0.88 (-0.28 to 1.62)	1.20* (0.27–2.39)	1.30* (0.10–2.46)
(Bias corrected 95% confidence interval, clustered by industry)	[-1.15 to 1.99]	[0.20–1.70]	[0.09–2.93]
Number of observations (on support)	Treated: 41(35) Untreated: 46(46)	Treated: 42(36) Untreated: 40(40)	Treated: 40(35) Untreated: 37(37)
Sensitivity analyses			
Changing bandwidth			
Bandwidth = 0.01			
Difference-in-difference point estimate (Bias corrected 95% confidence interval)	1.06 (-0.18 to 1.82)	1.42* (0.16–2.51)	1.59* (0.32–2.96)
(Bias corrected 95% confidence interval, clustered by industry) (6/6/5 cases excluded)	[-1.05 to 2.67]	[0.68–2.06]	[0.35–2.85]
Bandwidth = 0.08			
Difference-in-difference point estimate (Bias corrected 95% confidence interval)	0.84 (-0.30 to 1.73)	1.16* (0.09–1.92)	1.21* (0.46–2.04)
(Bias corrected 95% confidence interval, clustered by industry) (6/6/5 cases excluded)	[-1.99 to 1.50]	[0.19–1.70]	[0.04–2.98]
Trimming			
Imposing 9% trimming condition			
Difference-in-difference point estimate (Bias corrected 95% confidence interval)	0.96 (-0.94 to 1.60)	1.29* (0.31–2.07)	1.49* (0.51–2.34)
(Bias corrected 95% confidence interval, clustered by industry) (3 cases excluded)	[-1.32 to 2.02]	[0.60–2.62]	[0.50–3.74]

Note: ***, **, and * indicate significance levels of 0.1%, 1% and 5%, respectively (2-tailed). For DID point estimates, standard error does not take into account that the propensity score is estimated.

average treatment effect on treated firms is 1.30. According to Taylor approximation, log difference equals percentage change for 'small' changes. Given this, we conclude that within the time span of two years, treated firms (i.e., participants of the NIY Programme) have grown their sales 120 percentage points faster than untreated firms, and they have grown 130 percentage points faster than untreated matched firms over a three-year period. Moreover, comparing matching results to unadjusted mean differences shows that there is bias in unmatched results which is 50 percentage points for the difference over two-year growth and 46 percentage points over the three-year period.

The literature on matching suggests that after the propensity score analysis is carried out, it is important to check matching performance in terms of balance (Guo and Fraser, 2010). Checking balance means analysing whether it is possible to make untreated firms more similar to the treatment group firms with respect to variables that were included in the probit model. This issue is addressed in Appendix B. In summary, this robustness check shows that the matched sample consists of more similar firms than firms in an unmatched sample of untreated firms, suggesting that the matching procedure has been effective.

Although the PSM analysis suggests a significant growth effect beyond selection, one may still question whether the policy initiative has delivered 'value for money'. As is clear from the description of the NIY Programme, this is not a low-cost policy initiative. Therefore, although evidence of genuine impact is suggested, it is worthwhile to consider the delivered outcome relative to funding invested. Table 6 shows our 'value for money' calculation concerning the impact generated relative to funds invested into the programme. To illustrate the trends, Table 6 reports data from all 27 treated firms that were accepted to the NIY Programme in 2008–2010; that have completed the NIY Programme, and for

which we had full time series of sales data that covered at least one year of pre-treatment sales growth. As we needed a long time series to discern the impact of NIY on sales growth trend, the resulting sample represents a sub-set of all NIY accepted firms. For example, firms that we founded during the year they joined the NIY Programme are excluded from this analysis, because evidence on pre-treatment growth trend is missing.

All the firms in our value-for-money analysis had received full NIY support totalling 1 M€ over the treatment period. These are compared against a matching sample of 44 untreated firms (firms that applied for the NIY Programme but were not accepted) for which we had complete time series data from the same periods. Note that our value-for-money estimate is conservative, as we include in the cost side also support given to treated firms that failed to complete the NIY Programme, yet only consider sales growth achieved by those firms who did complete the Programme¹⁰. These two groups offer the longest time series at our disposal, and we compare their sales growth from 2007 to 2013 and predicted sales growth from 2014 to 2015. The 2008 cohort of treated firms grew its sales by 440% from 2007 to 2013. Its 'surplus' sales growth relative to untreated sales growth represented, in 2013, 1.03€ of extra sales for every support euro invested through the NIY Programme (=1/0.97). For the 2009 cohort of treated firms, the corresponding figures are 363% sales growth from 2008 to 2013 and a support

¹⁰ As a further conservative measure, in all analyses of NIY participating firms we have removed outliers whose performance would unduly distort the analysis. The outstanding example is SuperCell, the developer of the Clash of Clans iPad game, which grew its sales revenue from 200k€ in 2011 to 860M€ in 2013. The capital gains taxes paid by the owners of SuperCell in 2014 alone (they sold a 49% equity stake to Japanese equity investor Softbank for USD 1.5 Billion in 2013) will have easily covered the cost of the entire NIY Programme.

Table 6
Value-for-money estimation of the NIY Programme impact.

NIY 2008	Year							Prediction	
	2007	2008	2009	2010	2011	2012	2013	2014	2015
Sales. sum (k€)	9223	12,827	26,992	33,727	45,809	44,547	49,797	60,986	68,271
NIY firms (n = 11)									
Change from 07							4.40	5.61	6.40
Sales. sum (k€) untreated (n = 15)	6258	10,320	12,271	14,758	18,948	19,687	23,259	25,988	28,717
Change from 07							2.72	3.15	3.59
Additional sales of treated over untreated firms							15,519	22,685	25,948
Support per extra sales Euro							0.97	0.66	0.58
NIY 2009	Year							Prediction	
	2008	2009	2010	2011	2012	2013	2014	2015	
Sales. sum (k€) NIY firms (n = 12)	7149	12,976	12,648	20,497	26,204	33,092	35,867	40,699	
Change from 08						3.63	4.02	4.69	
Sales. sum (k€) untreated (n = 14)	3888	4621	4577	6010	9337	9216	10377	11538	
Change from 08						1.37	1.67	1.97	
Additional sales of treated over untreated firms						16,147	16,787	19,483	
Support per extra sales Euro						1.20	1.16	1.00	
NIY 2010	Year							Prediction	
	2008	2009	2010	2011	2012	2013	2014	2015	
Sales. sum (k€)		1762	2912	2463	4522	12,714	10,967	12,906	
NIY firms (n = 4)									
Change from 09						6.22	5.22	6.32	
Sales. sum (k€) untreated (n = 15)		4990	4063	4387	5951	5691	6032	6373	
Change from 09						0.14	0.21	0.28	
Additional sales of treated over untreated firms						17,691	12,352	10,655	
Support per extra sales Euro						0.46	0.66	0.76	

multiplication effect of 0.83€ surplus sales growth for every euro of NIY support. For the 2010 cohort of treated firms, the corresponding figures are 622% (growth from 2009 to 2013) and 2.18€ surplus sales growth for every NIY support euro (note the small n in this cohort, however). The weighted average for the support multiplication effect for all cohorts in 2013 is 1.11€ surplus sales growth per support euro invested, and for the 2008 and 2009 cohorts it is 1.22€ and 1.34€. Assuming a linear sales growth trend, the extra sales return for every NIY euro will be 1.23€ (weighted mean across all cohorts). If we did not include support spent on firms that were accepted but did not complete the Programme, the extra sales return for every NIY euro would be 1.43€.

While these numbers should be read with caution given the small sample sizes (because high-growth policy initiatives are highly selective, the numbers tend to be small) and the general volatility of technology-based new firm growth, they nevertheless suggest considerable return on policy investment in the case of the NIY Programme. Note that we are looking at changes in growth trend, and sales increments are repeated annually and are expected to persist over time. We have also assumed a linear growth trend, although sales growth in TBNFs tends to be subject to non-linear 'breakthrough' effects, once their technology is accepted by the market and positive externalities kick in. Given that the focus of NIY is on boosting an organisational capacity to achieve and sustain rapid growth, we believe that the growth trend assumption is reasonable. This said, the number of treated firms with sufficient data for this analysis remains small, and the value-for-money analysis presented here is worth re-visiting as evidence accumulates.

6. Discussion

High-growth entrepreneurship has been increasingly drawn into policy focus, as governments have recognised that not all new firms contribute equally to the economy. This has contributed to an increased interest in policy initiatives specifically targeted at facilitating high-growth entrepreneurial activity. This interest has

accentuated two relevant gaps in high-growth entrepreneurship policy. First, although there is much activity in the high-growth entrepreneurship space, the lack of appropriate performance data has meant that there has been little evidence that such policies can actually deliver. Second, the policy focus on high-growth entrepreneurship has re-introduced the age-old dilemma: do governments have any business in 'picking winners'?

These gaps mean that government interest is occasionally tempered by skepticism. For example, [Coad et al. \(2014:91\)](#) noted: [high-growth firms] are unlikely to be useful vehicles for policy given the difficulties in predicting which firms will grow.... To advance policy, therefore, it seems important to remedy both gaps. To address the dearth of theorising in this area we unpicked the 'picking winners' argument and suggested that governments may still have a role to play in *retaining* winners. To provide theoretical grounding for analyses in this domain, we also applied public sponsorship theory to frame our discussion regarding the effectiveness of entrepreneurship policy initiatives that seek to facilitate high-growth new firms. Complementing the buffering and bridging arguments previously advanced in the public sponsorship theory, we proposed a third mechanism through which public sponsorship can advance new firm growth: capacity boosting. Whereas the buffering and bridging mechanisms largely promote passive survival, boosting is required to facilitate new firm growth. This calls attention to active, hands-on policy initiatives that develop the organisational capacity for growth. While this observation is not new to policy practitioners, it nevertheless has not been extensively discussed in the public sponsorship theory. This addition is important, since it emphasises an active role for policy that goes beyond resource provision, and also, emphasises public-private partnerships. As a contribution to policy practice, we identified distinguishing characteristics of growth-oriented policy initiatives, and also, demonstrated that such approaches can deliver a real impact on new firm growth.

Another key contribution of this paper is the production of evidence to inform policy. Our data was collected in bi-annual

waves over the period from 2006 to 2013, with control group data collection starting two years before the policy initiative itself was launched. Such datasets are rare in any policy domain and practically non-existent in entrepreneurship policy initiatives. This setup provided a rare opportunity to produce as bias-free estimate as possible of the impact of the policy initiative without actually implementing a fully randomised, double-blind experimental design.

In addition to the carefully collected longitudinal data, a strength of our quasi-experimental design was that we came closer to comparing apples and apples, by selecting our control group from among firms that had applied for the NIY Programme but had missed out on selection. The continued data collection from this group ensured high-coverage longitudinal data from both treated and untreated businesses. The results from the PSM analysis confirm, consistent with our proposition, that the NIY Programme has had a strong, positive impact on the sales growth of its participating firms. For example, the difference in log difference (as an approximation of percentage change) of sales for treated cases after one year in programme and untreated cases (average treatment effect for treated cases) was 1.2, indicating that the NIY firms had grown their sales 120% faster than their propensity score matched pairs in the untreated group. This difference is both statistically very significant (2-tailed significance) and persists over three years. Importantly, the growth-boosting effect is achieved during a period of severe financial recession, the tremors of which continue to be felt in the Finnish economy even today. Our robustness checks also testify of the robustness of this pattern, as our findings are consistent for two- and three-year log differences and with different propensity score matching methods. The robustness of our findings across different propensity score matching techniques lends credence to the conclusion that the growth enhancing impact is due to the contribution made by the NIY Programme itself, and not because of the selection effect. Finally, our 'value for money' analysis suggests that the NIY Programme has also delivered good value for policy investment.

We offer several implications for both entrepreneurship theory and policy practice. In entrepreneurship policy research, many important issues concerning types and effectiveness of entrepreneurship policy have not been settled (Minniti, 2008; Acs et al., 2014). These include high-impact entrepreneurship support; the way it works; and the question of whether it is possible to generate a genuine impact on participating firms (Cumming, 2007). This study contributed to the understanding of how high-impact entrepreneurship support operates by applying the public sponsorship theory lens on the phenomenon (Amezcuca et al., 2013). Our key proposition was that policy initiatives that exhibit a high degree of selectiveness; that stage support according to milestone achievement; and that solicit active public-private collaboration can be effective in facilitating the growth of new ventures. While contributing to the public sponsorship theory with our focus on capacity boosting, we have also contributed towards policy practice by highlighting characteristics of policy initiatives that are potentially salient in facilitating high-growth entrepreneurial activity.

We have benefited from privileged access to a longitudinal dataset that has allowed us to estimate the impact of a high-growth policy intervention in unprecedented detail. Nevertheless, we have not been able to implement a fully randomised experiment, and our findings are therefore subject to some limitations. Despite our best efforts we did not receive pre-treatment survey data on all treated and untreated firms, as some applicants failed to complete our pre-selection questionnaire. We believe it is reasonable to assume that non-response has been random across successful and unsuccessful applicants, and no unobserved heterogeneity has been introduced. However, this assumption cannot be checked due to missing pre-selection survey data. Moreover, our analysis did not

differentiate between those firms that did not complete the NIY Programme and those that received full support. We encourage future researchers to consider this within-programme selection. When it comes to propensity score matching, we assumed that it is possible to observe firm attributes that drive selection into the NIY Programme and use such attributes to control selection bias in the statistical model. While we have confidence in the selection variables used in our model, it is always possible that some factors may have been overlooked. Note, however, that the difference-in-difference framework used in this study provides some remedy for the unobserved variable bias, as it controls for the impact of unobserved variables as long as this impact on sales growth is stable over time. As our analysis suggested that both treated and untreated firms were on a similar pre-treatment development trend (i.e., parallel trend assumption was upheld), this provides legitimation for the use of the difference-in-difference model. Another limitation concerns statistical inference. When applying nonparametric regression analysis of propensity scores in finite samples, the extent to which asymptotic properties apply is not always clear (Guo and Fraser, 2010). Therefore, it has been suggested that especially in case of small samples, the results should be interpreted with care (Guo and Fraser, 2010). In this study we followed the recommendation to calculate confidence intervals for the differences of log differences of sales by bootstrapping. We also tested the sensitivity of our findings against alternative designs – notably, different bandwidths, and found our findings to be robust against such changes (see Table 5). While this analysis supports the veracity of our conclusions, this method, too, may be subject to sample size limitations.

A final limitation concerns implications for practice. By emphasising retention of winners as a solution to the 'picking winners' dilemma, this study evokes another dilemma venture capitalists face. We noted earlier that predicting future growth is difficult even for venture capitalists. It is equally difficult to decide when to stop throwing good money after bad money. A shift of emphasis from selection to out-selection requires discipline in out-selection decisions. Public sector operators, who are the ultimate source of funding for policy initiatives, will likely find such decisions difficult; this was the anecdotal feedback we received from practitioners involved with the NIY Programme. Further research should explore ways to guide out-selection decisions.

These limitations notwithstanding, we believe that this study nevertheless presents the most robust design yet implemented for the study of the impact of high-growth entrepreneurship policy initiatives. We conclude that our hypothesis is supported: policy initiatives that feature characteristics commonly ascribed to high-growth entrepreneurship policy *can* deliver a non-trivial, value-adding impact on the growth of technology-based new firms.

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Appendix A.

Table A.1 shows industry sector, location and age distributions for treated firms and untreated firms. Concerning industry sector we apply the Eurostat categorisation of manufacturing and services industries according to their technology intensity. NACE revision 2 code aggregates are grouped into high-technology, medium high-technology, medium high-technology and low-technology and to

Table A.1
Industry distribution of treated firms and untreated firms.

Sector	Nace codes	Treated firms (%)	Untreated firms (%)
High-technology manufacturing	21, 26	4	5
Medium high-technology manufacturing	20, 27–30	3	5
Medium low-technology manufacturing	19, 22–25, 33	0	4
Low-technology manufacturing	10–18, 31–32	3	3
Knowledge-intensive high-technology services	59–63, 72	58	35
Other		32	49

Table A.2
Geographical distribution of treated firms and untreated firms.

Area	Treated firms (%)	Untreated firms (%)
Southern Finland	80	66
Western Finland	4	13
Eastern Finland	9	9
Northern Finland	7	9
Åland	0	0
Missing	0	3

Table A.3
Age distribution of treated firms and untreated firms.

Group	Share (%) of firms in age group in years by year 2013				
	1–2 years (%)	3–4 years (%)	5–6 years (%)	7–8 years (%)	9–10 years (%)
Treated firms	4	18	31	26	12
Untreated firms	0	0	0	60	40

knowledge-intensive high-technology services. We use the aggregation at two-digit level to minimise chances that individual firms can be identified in the reported data.

Table B.1
Comparing variables before and after propensity score matching.

Variable	Matching Group	Mean 2010		Bias (%)	Reduction in Bias (%)	t-test	
		Treated	Control			t	p > t
Financial resource mobilisation	Unmatched	4.09	4.71	–41	85	–1.92	0.05
	Matched	4.25	4.16	6.3		0.28	0.78
Growth orientation	Unmatched	5.68	5.15	37	95	1.73	0.08
	Matched	5.51	5.48	1.9		0.08	0.93
Growth self-confidence	Unmatched	4.21	4.45	–16	80	–0.75	0.45
	Matched	4.48	4.53	–3.2		–0.14	0.89
Perceived feasibility of international growth	Unmatched	6.21	5.80	34.8	91	1.61	0.11
	Matched	6.17	6.20	–2.9		–0.13	0.89
Founding year (age)	Unmatched	2005	2004	51	91	2.37	0.02
	Matched	2005	2005	1.2		0.06	0.95
Region.Capital	Unmatched	0.65	0.50	32	77	1.50	0.13
	Matched	0.62	0.59	7.1		0.30	0.63
Region.Tampere	Unmatched	0.04	0.08	–15	0.9	–0.70	0.48
	Matched	0.05	0.09	–14		–0.59	0.57
Region.Oulu	Unmatched	0.04	0.04	2.5	–481	0.12	0.90
	Matched	0.05	0.08	–14		–0.49	0.62
Industry.HTM	Unmatched	0.02	0.08	–27	95	–1.25	0.21
	Matched	0.02	0.02	1.1		0.07	0.94
Industry.KIHTS	Unmatched	0.63	0.41	44	75	2.09	0.04
	Matched	0.62	0.57	11		0.46	0.65

Appendix B.*Matching performance for propensity score analysis*

Table B.1 illustrates propensity score matching performance in terms of achieved balance. Table B.1 shows that the matching was successful. This can be seen by comparing the mean values of the matching variables for treated and untreated (i.e., control) firms before and after the matching procedure. As an example, the mean value for the 'Growth orientation' variable is 5.68 for unmatched treated firms and 5.12 for unmatched untreated firms. For the matched treated firms, the 'Growth orientation' variable has a mean value of 5.51, which compares against the mean of 5.48 for matched untreated firms. Thus, the mean difference is much smaller after matching, showing that the matching procedure has been effective for this variable. The table further shows that the remaining mean difference after matching is not statistically significant for any variable, again testifying of matching effectiveness. The Region_Oulu dummy was the only variable for which the mean difference increased with matching (indicated by negative reduction in bias). However, the t-test shows that the difference between treated and untreated firms was not significant either prior to or after matching, so matching effectiveness was not affected in this case. In summary, through matching, a reference group has been created that is more similar to treated firms than without matching.

Appendix C.*Pre-selection questionnaire survey*

The tables below show scales from which measurement items were drawn to model selection into the NIY Programme. The pre-selection and post-selection surveys queried the following topics: The firm itself and its products and services; Resources and competences; Strategic goals and growth strategies; Internationalisation; Product features and operating environment; Perceptions of Tekes services used. Full survey items can be found in the appendix of one co-author's doctoral dissertation (Rannikko, 2012).

The 'financial resource mobilisation' item was drawn from the scale shown in Table C.1. The 'finance' item was used (in the anal-

Table C.1
Resource mobilisation scale.

How easy is it for you to access ...	Easy						Difficult	
	←	←					→	→
... Finance	1	2	3	4	5	6	7	
... Competent managers	1	2	3	4	5	6	7	
... Consulting and advice	1	2	3	4	5	6	7	
... Competent personnel	1	2	3	4	5	6	7	
... Distribution channels	1	2	3	4	5	6	7	

Table C.2
Growth orientation scale.

We prefer high profitability over rapid growth	−3	−2	−1	0	1	2	3	We prefer rapid growth over profitability
We prefer high technical achievement over rapid growth	−3	−2	−1	0	1	2	3	We prefer rapid growth over high technical achievement
We prefer longevity over rapid growth	−3	−2	−1	0	1	2	3	We prefer rapid growth over longevity

Table C.3
Growth self-confidence scale.

How would you evaluate your ability to grow rapidly?	Disagree						Agree	
	←	←					→	→
We have the necessary skills to grow rapidly	1	2	3	4	5	6	7	
It would be easy to grow rapidly	1	2	3	4	5	6	7	
We could easily acquire the resources to grow rapidly	1	2	3	4	5	6	7	
Competition in this business is tough	1	2	3	4	5	6	7	

Table C.4
International growth orientation scale.

Please evaluate following items for your firm	Disagree						Agree	
	←	←					→	→
It makes more sense for us to grow internationally than domestically	1	2	3	4	5	6	7	
Our marketing competence is better applied internationally	1	2	3	4	5	6	7	
Our personnel is better utilised in international markets	1	2	3	4	5	6	7	
Our current products support international markets better	1	2	3	4	5	6	7	
Our reputation and brands support internationalisation better	1	2	3	4	5	6	7	
Our current customer relations support international growth	1	2	3	4	5	6	7	

yses we reverse scored the scale for internal consistency and ease of interpretation).

The 'growth orientation' item was drawn from the scale presented in Table C.2. The last scale item was used. In the analysis the measurement scale was transformed to run from 1 to 7.

The 'growth self-confidence' item was drawn from the scale presented in Table C.3. The second scale item was used.

The 'internationalisation orientation' item was drawn from the scale presented in Table C.4. The second item was used.

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