Beyond Silicon Valley: Lessons from accelerator programs in small innovation ecosystems

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Abstract

Accelerator programmes have become a prominent policy tool for stimulating entrepreneurship, yet rigorous evidence on their firm-level impact remains limited, especially in smaller economies. This article examines whether participation in Norwegian accelerators can be associated with performance gains for early-stage ventures. Using registry data from 2019 to 2023, we combine participation records from eight SIVA-affiliated accelerators (supported by the Norwegian government) with full-population accounting data and apply propensity-score matching to create a cross-sectional control group comparable on founding year, industry, region and total assets. Five outcomes are analysed one year after programme completion: employment growth, revenue growth, labour productivity, return on total capital and survival. Qualitative semi-structured interviews with former participants provide additional context for interpreting the quantitative results.

Key findings: Accelerator graduates expand employment about 31 percent faster than matched non-participants, a statistically and economically significant effect that interviewees attribute to heightened ambitions and improved recruiting networks. However, no reliable differences emerge in revenue, productivity or capital efficiency, and participants face roughly 2.2 times higher odds of bankruptcy, translating to an eight-percentage-point drop in the survival rate. These findings portray accelerators as effective catalysts for hiring but insufficient, on their own, to boost short-run financial performance and stability, highlighting the need for complementary post-programme funding and mentoring to convert rapid team growth into durable firm success. Given the limitations of this study, future research should adopt longitudinal designs to better capture the long-term effects and establish more direct links between accelerator participation and firm outcomes.

Keywords: Accelerator; Capability–Conversion Effects; Firm Performance and Survival; Norwegian Startup Ecosystem; Policy-Led Entrepreneurial Ecosystems.

1. Introduction

Accelerators—cohort-based programmes that combine structured mentoring, focused curricula, and exposure to investors and partners—have become a widely adopted mechanism for supporting early-stage startups, offering structured mentorship, funding access, and network integration. However, empirical evidence on their effectiveness remains mixed. While some studies highlight benefits such as improved access to capital, increased visibility, and faster product development (Cohen & Hochberg, 2014), findings across the literature remain inconclusive. Research on accelerators continues to yield mixed results, as findings vary depending on the outcome examined, including survival, funding, revenue, or growth (Tekic et al., 2024). Another study emphasises that despite an expanding research base, there is still limited understanding of how accelerator designs influence long-term startup success across varying contexts (Crișan et al., 2019).

Much of the existing literature focuses on U.S.-based accelerators, such as Y Combinator and Techstars. These programs are embedded in capital-rich, mature ecosystems, limiting the generalizability of their outcomes.

Studies focused on elite programmes offer little insight into how accelerators function in smaller, publicly funded ecosystems (Cohen et al., 2019). This gap raises doubts about whether accelerators truly spur growth or simply select firms already on a good trajectory (Tekic et al., 2024).

Most evaluations of Norwegian entrepreneurship policy rely on descriptive or qualitative approaches, and few studies assess firm-level impacts such as revenue, survival, or employment using a quantitative approach (Cappelen et al., 2016; Fjærli et al., 2018). Startup survival rates in Norway remain consistently low. Since 2016, only 44 percent of new Norwegian firms survive their first year, and just 26.5 percent remain after five years (Frøysa Skullerud, 2022, s. 1). These figures illustrate the inherent vulnerability of early-stage ventures and the need for mechanisms that can improve their odds of success. In Norway, currently ranked 24th on the Global Startup Ecosystem Index (Startupblink, 2025), government led organization called SIVA promotes accelerators as a tool to strengthen innovation capacity and help high-potential companies grow and scale (Entrepedia, 2023; Gustavsen, 2013, s. 4). SIVA provides some initial funding for accelerator programs, while they typically also use additional sponsorships to support their activities. Although government promotes accelerator programs following American examples, the effects of such initiatives are still debatable.

This study addresses that gap by conducting a quantitative evaluation of Norwegian startups that have participated in accelerator programs. We leverage cohort rosters from eight SIVA-affiliated programmes, link them to full-population administrative data, and estimate matched effects on employment, revenue, labour productivity, return on total capital (ROTC), and survival one year after programme completion by comparing with the control group. A brief set of alumni interviews complements the quantitative analysis by illuminating mechanisms behind the estimates.

Our contribution is threefold. First, we provide evidence from a smaller, policy-led ecosystem, extending a literature dominated by U.S. exemplars. Second, we evaluate multiple outcomes, distinguishing capacity mobilisation (hiring) from conversion to financial performance and resilience. Third, we stress robustness—through trimming, a longer observation window for early cohorts, and subgroup analyses—to gauge the stability and external validity of the results.

In preview, we observe a consistent and economically meaningful association with employment growth, but no reliable differences in revenue or productivity, weak and unstable changes in ROTC, and a decline in short-run survival. This asymmetry suggests that accelerators may effectively mobilize human resources, while post-programme constraints might limit the conversion of increased capacity into paying customers.

2. Theory and Hypotheses

Since Y Combinator's 2005 debut, >3 000 accelerators have emerged across continents (Hochberg, 2016). Empirical claims about their impact, however, remain fragmented: some studies report dramatic boosts in investment and growth (Hallen et al., 2014), while a recent systematic review finds that accelerator outcomes differ considerably across programs, metrics, and regional contexts (Crișan et al., 2019).

Accelerators are "fixed-term, cohort-based programs that include mentorship and educational components and culminate in public investor presentations" (Cohen & Hochberg, 2014). Core design features include 3–6 months of intensive engagement, peer learning among 5–30 startups, small equity-linked investment or grants, structured curriculum, mentorship and demo day.

A core promise of accelerator programmes is that they create value at two, mutually reinforcing levels. First, they upgrade the human capital of founders sharpening skills, enlarging networks, and reframing entrepreneurial mind-sets. Second, they leave a measurable imprint on the organisation itself, boosting traction indicators such as revenue growth, employment, and access to finance (Hallen et al., 2020; Pauwels et al., 2016). The pathways that carry these effects can be grouped into founder-level outcomes, firm-level outcomes, and the internal mechanisms that connect the two.

Effectuation theory argues that entrepreneurs begin not with a fixed goal but with their existing resources of who they are, what they know, whom they know, and through recursive interaction with stakeholders, jointly shape and refine new market opportunities rather than simply discovering them (Sarasvathy, 2001). *At the founder level*, the accelerator compresses months of experiential learning into intensive, workshop-driven sprints. Embedded sessions on lean experimentation, fundraising, and growth analytics repeatedly emerge in qualitative studies as catalysts of rapid knowledge acquisition (Hallen et al., 2014; Pauwels et al., 2016). Human capital (skills, know-how) and social capital (network ties, trust) are pivotal for entrepreneurial success. Curated introductions to mentors and investors enlarge founders' social capital, a network expansion effect documented both in U.S. survey work (Kwapisz, 2022) and in Israeli longitudinal panel data (Avnimelech & Rechter, 2024). The same study demonstrates that weekly personal mentoring elevates founder capabilities compared with ad-hoc expertise (Avnimelech & Rechter, 2024). Regular feedback reshapes founders' self-image: they stop viewing themselves merely as people with an idea and start acting like leaders chasing opportunities. This shift helps them spot new openings and stay committed longer, reinforcing the resource-based and signaling effects (Tobiassen et al., 2022).

At the firm-level, the most visible dividend is access to capital. Demo-day exposure and the reputational "badge" of a prestigious accelerator systematically raise the odds of follow-on equity or grant finance (Gonzalez-Uribe & Leatherbee, 2018; Hallen et al., 2020). Signaling theory says clear cues help investors judge young firms they cannot easily vet (Spence, 1973). Getting into a selective accelerator is one such cue. A regression-discontinuity study of about 1 500 Start-Up Chile applicants found that startups receiving the full accelerator package raised 0.34 more funding rounds and earned 41 % more revenue over three years than almost-identical firms who only got a grant (Gonzalez-Uribe & Leatherbee, 2018). The narrow cutoff shows it was the program's mentoring and badge, not selection bias, that created this "seal of quality." Equally salient is improved product–market fit: the enforced cadence of customer discovery and pivoting accelerates validation cycles, mirroring lean-startup logic (Ries, 2011b; Tekic et al., 2024). Participating ventures also report higher business-model professionalism, implementing KPI dashboards, formal legal structures, and IP strategies during the programme (Pauwels et al., 2016). Finally, accelerators influence team development: peer comparison and mentor guidance encourage clearer role division among founders and prompt strategic early hires (Avnimelech & Rechter, 2024; Hackett & Dilts, 2004).

Accelerators often highlight investment raised, jobs created, and survival rates. Scholars, however, argue that sturdier yardsticks such as *revenue growth, labour productivity*, and other *quality-of-growth metrics* give a truer picture of economic impact (Hochberg, 2016; Pauwels et al., 2016). Most Scandinavian research is descriptive (Cappelen et al., 2016; Fjærli et al., 2018). By linking Norway's registry data with accelerator participation records, our Propensity Score Matching-Cross Sectional design will track each firm's revenue, employment, productivity, return on total capital and survival rate before and after entry which directly tests whether Norwegian accelerators accelerate startup success.

2.1. Revenue

Evidence from several well-known programmes points in the same direction: accelerators tend to push revenue curves upward, although the size of the boost depends on how the programme is run. For example, real-time web-analytics data on 103 Y Combinator graduates show that their average growth rate more than doubled, which is roughly a 2.3-fold jump within the first 30 weeks of the programme, and nearly 70 percent of ventures that had stalled began growing again (Tekic et al., 2024). Further south, a regression-discontinuity study of Start-Up Chile found that treated firms were earning 41 percent more revenue three years after entry than otherwise identical firms who only received a grant (Gonzalez-Uribe & Leatherbee, 2018). Qualitative follow-ups of Techstars batches echo the pattern, attributing faster sales traction to the programme's disciplined customer-feedback loops (Hallen et al., 2020). Across studies, the biggest gains appear in cohorts that deliver hands-on, high-frequency mentoring, reinforcing evidence that mentor intensity is a critical ingredient (Avnimelech & Rechter, 2024; Crișan et al., 2019).

Drawing on these insights, our first hypothesis (H1) is *that Norwegian accelerator participants will have faster annual revenue growth than matched non-participants 1–3-years post-program.*

2.1.1. Employment and Productivity

Job creation is one of the headline promises policymakers look for when they subsidise accelerator programmes, yet hard evidence on staffing effects remains relatively sparse. One of the few multi-country surveys covering 13 European accelerators of varying age and sector focus, reports that alumni added about two net employees during the first twelve months after graduation, a modest but still positive bump given the small starting size of most cohorts (Pauwels et al., 2016). Although two new hires may sound minor, this represents roughly a 20-to-30 percent head-count increase for a typical early-stage venture and therefore signals that accelerators can convert soft assets such as mentoring and investor contacts into tangible payroll growth. The survey also flags strong programme heterogeneity: people-intensive verticals (e.g., hardware, deep tech) tend to hire more quickly than software-as-a-service cohorts, suggesting that design fit matters. Another study that linked SIVA incubation records to registry data, followed almost 3 000 incubated firms and matched controls; found sharper gains in jobs, sales, value creation and labour productivity during the first three years, but these advantages vanished by year five, and survival was unchanged which suggest publicly funded programmes may ignite early growth yet struggle to deliver lasting impact (Krokan & Huang, 2024).

In light of this evidence, we hypothesise (H2) *that accelerator-backed ventures will expand full-time employment faster than their matched peers during 1-3 years after the programme.*

Employment is only half the efficiency equation; the other half is *what each employee produces*. Case-based investigations reveal an intriguing pattern: many accelerated startups generate noticeably more output with only a lean uptick in staff, implying that the programmes may teach founders to do “more with less” through disciplined goal-setting, KPI dashboards, and iterative customer feedback cycles (Pauwels et al., 2016). This aligns with the resource-based and human-capital logics discussed earlier, mentoring and peer learning can sharpen managerial capabilities, leading to tighter execution and higher value added per employee. Importantly, labour-productivity gains are harder to achieve than simple head-count growth because they require not just capital inflows but better allocation of time, talent, and tools.

Taken together, these findings motivate our third hypothesis (H3), *which suggests that treated firms should achieve larger gains in labour productivity than their matched peers.*

2.2. Capital Efficiency

Return on Total Capital (ROTC), defined here as Earnings before Interest and Taxes (EBIT) divided by interest bearing debt and equity, gauges how efficiently a venture turns its investments into operating profit. Finance scholars regard ROTC as a fairer assessment of a company’s use of funds to finance its projects compared to e.g. Return on Assets, and functions better as an overall profitability metric (Vipond, 2025). A study of Innovation Norway grants that included ROTC found no gains, even though jobs and sales increased, indicating that efficiency is harder to boost than straightforward growth measures (Cappelen et al., 2015).

Building on the foregoing discussion, we formulate our fourth hypothesis (H4): *accelerator participation should yield larger improvements in return on total capital than those seen in matched non-participants, reflecting stronger operating leverage and signalling effects.*

2.3. Survival Rate

Evidence on whether accelerators lengthen startup life is inconclusive. A policy survey of commercial accelerators notes that their graduates exhibit roughly 23 percent higher survival than typical new firms, an edge largely attributed to selective admission of stronger teams (Butz & Mrożewski, 2021). More rigorous analyses temper that optimism. Del Sarto et al., (2020) find no overall survival premium for Italian accelerator alumni once venture type and market scope are controlled for, while Gonzalez-Uribe & Leatherbee, (2018) report statistically insignificant survival differences around the Start-Up Chile cutoff. The shares of firms not surviving are also due to specific reasons such as bankruptcy, dissolution, mergers and acquisitions, where successful exits such as through mergers and acquisitions, account for less than 1 percent for the first three years of activity in Norway (Fjærli et al., 2013).

Given the mixed results above, we set out our fifth hypothesis (H5), *expecting that accelerator participation lowers the probability of failure.*

3. Methodology

In this chapter, we outline the quantitative research design used to estimate the correlation between participating in an accelerator program and startup success. We describe the context of the study, the overall research design, data sources, construction of key variables, identification strategy, robustness checks, and limitations.

3.1. Norwegian Landscape

In Norway, accelerators have been woven into national innovation strategies as policy tools that offset thin domestic venture-capital markets and advance broader societal objectives such as the green transition (Cappelen et al., 2016; OECD, 2019). Norway's accelerator landscape is a mix of over twenty corporate, impact and private accelerator hubs that have sprung up since 2012 (Entrepedia, 2023). Institutional theory emphasises that accelerators do not operate in a vacuum but are shaped by the policy frameworks and socio-economic goals of their host countries (Autio et al., 2018). Siva, an agency under Norway's Ministry of Trade, Industry and Fisheries, serves as a state-backed tool for regional business development. Created in 1968, its mission is to expand the country's innovation infrastructure and lower entry barriers in areas where normal market incentives fall short. Siva's statutes give it a special duty to foster growth in outlying districts by investing in facilities and programmes (such as accelerator programs) that spur entrepreneurship (Siva, 2025). To sum up, SIVA is a state-owned enterprise develops, owns, and finances a nationwide innovation infrastructure which includes incubators, industrial parks, accelerators, and innovation hubs (Siva, 2025).

Besides SIVA that support infrastructure, there are several other agencies that provide seed funding and other support for startups. For example, Innovation Norway which acts as the national and regional agency for value-creating business development, offering start-up grants, export advisory services, and innovation funding to stimulate sustainable growth and regional development (Innovasjon Norge, 2025a). *Research Council of Norway (RCN) s*upports research-driven innovation by funding high-potential projects and facilitating knowledge-sharing arenas (Forskningsrådet, 2025). *Investinor* is a state-owned investment company that co-invests risk capital alongside founders and private investors through direct stakes, seed- and venture-fund commitments, and matching schemes; it also manages the government's pre-seed and seed-fund mandates (Investinor, 2025). Working in tandem, these agencies cushion accelerators from financial risk which includes, covering staff, space, and basic operating costs that, in return, steer programme goals toward public priorities such as job creation, regional inclusion, and sustainability rather than rapid investor exits (Forskningsrådet, 2025; Innovasjon Norge, 2025a; Investinor, 2025; Siva, 2025)

Norwegian accelerators rely on a public-private blend. Regional welfare programmes draw up to half of their budgets from SIVA or municipal grants, allowing them to offer small, equity-free stipends that lower entry barriers for early founders (Cappelen et al., 2016). Investor-led programmes such as *The Factory* and *Katapult* combine that subsidy with seed cheques in exchange for a modest equity slice, effectively leveraging public grants to crowd-in private capital (Katapult Ocean, 2025; Pauwels et al., 2016; The Factory, 2025). Corporate-backed accelerators, *Equinor-Techstars Energy* is the flag-ship mirroring the classic Techstars deal, trading a minority common-stock stake for cash and deep sector expertise (Cohen & Hochberg, 2014; Equinor & Techstars Energy Accelerator, 2025). This grant-to-equity spectrum shows how Norwegian schemes balance inclusivity with market discipline while maximising founders' access to both cash and capability networks.

Most Norwegian accelerators run small, time-boxed cohorts: 6–10 startups progress through a 12- to 16-week curriculum featuring workshops, weekly mentor sessions and a public Demo Day. Selectivity is moderate, where programmes screen fewer than 120 applicants per cycle yet tight enough to foster peer learning and hands-on mentoring (Pauwels et al., 2016). Thematic focus is common: StartupLab recruits broad-based tech startups, The Factory specialises in fintech and proptech, while Katapult Ocean targets ocean-tech ventures (Katapult Ocean, 2025; StartupLab, 2025; The Factory, 2025). This niche design helps match founders with sector-specific mentors and investors, accelerating product-market validation. Compact batch sizes facilitate rich feedback loops but limit internal benchmarking, highlighting the need for coordinated data collection across programmes (Avnimelech & Rechter, 2024).

3.2. Research Design

This study employs a quasi-experimental, quantitative research design to investigate the association between accelerator participation and subsequent startup performance. In this study, we adopt the research design from a previous study on the effects of incubator programs in Norway, conducted by the Norwegian Statistics Bureau and published in two reports (Cappelen et al., 2015, 2016). To ensure the validity of our research design, the authors have been in direct contact with researchers who have conducted evaluations of incubator programs, gaining valuable insights on how to address existing data limitations. Because startups are not randomly assigned to accelerators, we rely on observational data and statistical techniques to approximate an experimental evaluation (Angrist & Pischke, 2008). Common econometric approaches for policy evaluation with non-randomized data include regression discontinuity, instrumental variables, and difference-in-differences (Fjærli et al., 2018). A difference-in-differences design requires several reliable years of pre- and post-programme data to check parallel trends, but many Norwegian accelerator cohorts are too young for that depth. Accelerator intakes in Norway are recent and patchy: the first cohorts were established in 2016 and do not increase significantly before after Covid-19 in 2021. Additionally, most entrants are only zero to one years old when they enroll, leaving a limited pre-treatment history.

Because there are so few post- and pre-programme observations and start dates are staggered, a difference-in-differences panel would rest on very thin, non-parallel trends. We therefore match accelerator participants with corresponding non-participants on rich pre-treatment covariates and compare their outcomes in a single cross-sectional regression analysis to isolate the effect of the accelerator itself from other confounding factors.

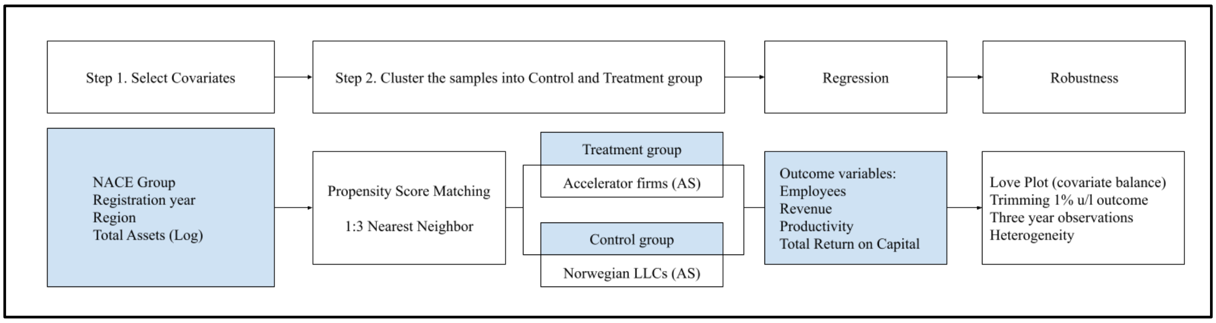
Propensity Score Matching (PSM) (Rosenbaum & Rubin, 1984), provides a non-parametric means of reducing selection bias in quasi-experimental studies. The method begins by estimating each unit's probability of receiving the treatment, known as the propensity score, using observable characteristics. Treated units are then paired with untreated units that have similar scores, creating balance in pre-treatment characteristics that would otherwise confound the analysis. By aligning the two groups in this way, PSM approximates the conditions of a randomised experiment and allows the treatment effect to be identified more clearly. To avoid skewed data, continuous data in the form of total assets start year are log transformed before matching using the MatchIt function in R (FENG et al., 2014).

Given the lack of panel data and limited access to pre-treatment observations, a cross-sectional regression analysis provides a viable and theoretically grounded approach for examining associations between participation in accelerator programs and key firm-level outcomes (Angrist & Pischke, 2008; Wooldridge, 2010).

In evaluating the effects of accelerator participation, it is important to distinguish between different types of treatment effects. Most notably, the Average Treatment Effect (ATE) refers to the expected impact of treatment across the entire population, while the Average Treatment Effect on the Treated (ATT) captures the average impact specifically for those firms that received the treatment. Given that participation in accelerator programs is not randomly assigned and that the control group is constructed through observational matching, this study focuses on estimating the ATT. Furthermore, the possibility of heterogeneous treatment effects is acknowledged, meaning that the program's impact may vary across firms depending on characteristics such as size, sector, or initial performance. While the current design does not allow for formal modelling of such heterogeneity, it remains essential to interpret the ATT as an average effect that may mask underlying variation. This distinction aligns with the framework outlined by Blundell & Dias (2009), which posits that treatment effects may vary across individuals due to both observed and unobserved characteristics.

In summary, the chosen design enables us to mimic a controlled experiment by pairing similar startups and controlling for baseline disparities, thereby providing a clearer signal of the accelerator programs' impact on startup success (Angrist & Pischke, 2008).

Figure 1: Research process.



3.3. Data and Research Design

Our study adopted the research design from a previous evaluation of incubator programs in Norway, conducted by the Norwegian Statistics Bureau (Cappelen et al., 2015, 2016). Our analysis leverages two primary data sources, which we integrate to build a panel dataset of Norwegian startups and their performance from 2019 to 2023. To retrieve the necessary data, Siva provided an overview of the 8 incubators offering accelerator programs, out of a total of 35 national incubators supported (Siva, 2025). This covers multiple accelerator initiatives across Norway capturing the key "treatment" information for our study, although specific and structured participant information was not available directly from Siva. An Excel spreadsheet of 766 companies with treatment year (the year they underwent an accelerator) was compiled after input from the individual programs, and organization numbers were manually collected at public registries and added, which may have introduced potential errors before further processing (Barchard & Pace, 2011; Boddy, 2016).

Econometric information was provided by ENIN.AI, a credit risk and anti-fraud analysis company, which offers a comprehensive registry of Norwegian firms providing real-time, updated firm-level information (ENIN.AI, 2025b). Their access to company-related information includes announcements from the Company Register Centre, database called Doffin, court meetings, pledges, and news from published articles and media (ENIN.AI, 2025a). Before matching, the dataset records each firm's identification number, founding year, NACE industry code, region, and annual financial and employment figures. From the ENIN.ai registry we extract both the outcome variables we will analyse, such as revenue and employee count, and the control variables needed for matching, including firm age, industry, region, and total assets. The ENIN.AI data effectively serves as our source of longitudinal performance measures and firm demographics for virtually the entire population of Norwegian companies.

The data was then imported into Posit Cloud (formerly RStudio Cloud), an open-source software for data scientists, for further analysis and structuring (Posit, 2025). Through the use of Application Programming Interfaces (APIs) to communicate with Posit Cloud, key metrics can be matched on organization number or other variables (Amazon, 2025). T

After merging these sources on firm identifiers, we construct a panel dataset that tracks both *treated* and *control* firms from treatment year to one year post-treatment. The treated group comprises every startup that entered a SIVA-affiliated accelerator between 2019 and 2022, ensuring that each firm has a minimum one-year window between programme completion and outcome measurement. The pool of potential controls consists of firms founded in the same years, located in the same region, starting off with approximately identical total assets and within the same NACE industry group.

To preserve comparability, we retain only observations with complete financial and employment data in the snapshot year from which to construct matching covariates. We further restrict the analysis to firms treated or observed within the 2019–2022 window, as employment data are only available from 2019. Although some accelerator participants were founded as early as 1995, those entering a programme before 2015 are rolled into a single "pre-2015" cohort and recoded with an effective establishment year of 2015. All control firms are drawn from startups founded between 2015 and 2022, ensuring that treated and control units are operating under similar macro-economic conditions. Performance measurements are compared from the treatment year to the following year for all companies. For companies with minimum three years of financial data (i.e.treatment in 2019 and 2020), an additional comparison has been made as part of the robustness checks. By combining the firm-level performance data from ENIN.AI with the targeted program participation data from SIVA, we obtain a dataset suitable for evaluating the impact of accelerators at the firm level.

As a qualitative method to complement the quantitative analysis and probe the mechanisms behind the statistical effects, we conducted five anonymous semi-structured interviews with founders or senior managers in treated firms. Informants were randomly selected from the twenty best-performing accelerator companies on the significant outcome variables. All interviews were carried out by phone between 17 and 18 June and followed an interview guide.

3.4. Variables

To obtain an unbiased estimate of the treatment effect, it is crucial to control for observable firm-level characteristics that may influence both the likelihood of participating in an accelerator and subsequent performance outcomes. In line with previous empirical work, we include a set of covariates that capture key pre-treatment attributes, helping to reduce selection bias and ensure greater comparability between treated and control firms (Cappelen et al., 2015). These covariates are used in both propensity score estimation and outcome analysis to strengthen the internal validity of our results.

- *Treatment Indicator:* For each firm-year, we define a binary dummy variable indicating whether the firm participates in the accelerator program. We assign the treatment variable a value of 1 for accelerator firms, and these firms also have a separate variable indicating the year of participation. Control firms have a value of 0 throughout, as they never receive the treatment. This allows us to differentiate between the pre- and post-treatment.
- *Registration Year:* Interpreted as a continuous variable. The company registration date was selected for matching. (Altinn, 2025).
- *Industry Classification:* Each firm's industry is identified by their main NACE group (European industry standard).
- *Region:* We categorize firms by using the former five zones of payroll taxes in Norway (Eastern Norway, Western, Southern, Mid, Northern Norway) to account for regional economic differences (Sikt, 2018).
- *Total Assets (log):* This variable represents the book value of assets and serves as a size indicator, ensuring that a treated startup is matched with a control of similar scale before the treatment (Cappelen et al., 2015).

Table 1: Company characteristics before and after matching procedure.

Company characteristics before and after matching procedure							
	Before matching				After matching		
Cont.variables	*SIVA*	*Non-Siva*	*SMD*		*Siva*	*Non-Siva*	*SMD*
Total assets	0.87	13.1	-0.09	***	0.87	12.7	-0.06
Binary variables							
Main industries							
J	0.50	0.07	1.08	***	0.50	0.49	0.02
M	0.26	0.14	0.3	***	0.26	0.28	0.06
C	0.08	0.03	0.2	***	0.08	0.09	0.03
G	0.08	0.13	-0.17	***	0.08	0.07	0.03
N	0.03	0.05	-0.1		0.03	0.03	0.04
A	0.01	0.02	-0.05		0.01	0.01	0.02
R	0.01	0.02	-0.09		0.01	0.01	0.01
Region							
Østlandet	0.36	0.55	-0.4	***	0.36	0.35	0.02
Vestlandet	0.37	0.23	0.3	***	0.37	0.4	0.05
Nord-Norge	0.11	0.08	0.08		0.11	0.09	0.04
Sørlandet	0.1	0.06	0.13	*	0.09	0.09	0.02
Trøndelag	0.07	0.08	-0.01		0.07	0.08	0.02
No of firms	285	176,391			285	855	

Note: Total assets in 1,000,000 NOK. * p<0.10, **<0.05, and *** p<0.01 indicate significance at 10, 5, and 1 percent levels, respectively.

Table 1 shows that matching sharply reduces observable differences. Before matching, accelerator firms were far more concentrated in the main industry category J (50 percent versus 7 percent), leading to large and highly significant standardised mean differences. After matching, every SMD falls below 0.06, and the distributions of assets, industry, and region are brought closer together between the 285 treated firms and their 855 matched controls. The sample is therefore well balanced and suitable for credible outcome comparisons.

In addition to the covariates and indicators used in the matching and regression, we consider five main outcome variables to capture different dimensions of startup performance in line with previous firm performance analysis by SSB (Cappelen et al., 2015): *(1) Employment, (2) Revenue, (3) Value Creation, (4) Employee Productivity, and (5) Survival Rate.* The outcome variables are measured at the time of treatment and one year after treatment, whereas survival rate are verified and calculated for fiscal year 2023.

- *Employment:* Measured as the number of employees in the firm
- *Revenue (Sales):* Measured as total annual sales revenue (operating income) of the firm,
- *Employee Productivity:* Measured as *labour productivity per employee*, calculated in R with revenue divided by the number of employees. This indicator measures the firm's efficiency in generating value per worker. An improvement in this ratio suggests gains in efficiency or technological improvements at the firm.
- *Return on Total Capital (ROTC):* Measured as *EBIT divided by total capital*, the latter consisting of short-term debt, long-term debt, and shareholders' equity, quantifying how the company's capital structure generates return (Vipond, 2025).
- *Survival rate*: By identifying registered bankruptcies and comparing the two groups, the survival rate can be observed.

In the cross-sectional analysis, we will primarily evaluate changes in these outcomes *before vs. after accelerator participation* for the treated firms relative to the control firms. We compute percentage point changes in survival rates, logarithm (Log) to the number of employees (log-emp) and Inverted Hyperbolic Sine (IHS)-based transformations to retain observations with zero or negative values and reduce skewness, particularly for financial indicators like revenue (ihs-rev), productivity (ihs-prod), and ROTC (ihs-rtc). Our treatment effect estimates will largely be interpreted as differences in growth of these performance metrics between the treated and control groups. To facilitate this, baseline levels of the outcomes (pre-treatment values) are taken into account in the matching procedure (described below) or as control covariates. All variables are carefully constructed and cleaned to handle most data issues (e.g., outliers, missing values). Extreme outlier values in financial variables trimmed in certain analyses to prevent distortion of results, further discussed under robustness checks.

3.6. Limitations

The propensity-score-matched cross-sectional approach narrows observable differences between accelerator participants and non-participants, yet it cannot establish causality with the same confidence as a randomised experiment or a Difference in Differences panel regression. Several limitations should be kept in mind when interpreting the findings. Propensity score matching may also be less effective when applied to categorical structural variables such as NACE group, where firms operate under fundamentally different industry conditions.

Also, the concern of selection on unobservables remains. Although matching equalises firms on recorded characteristics, unmeasured attributes such as founder ability, social networks, or product quality may still correlate with both accelerator admission and subsequent performance, thereby confounding the estimated relationships.

Further, one must acknowledge that outcomes are observed only in the first year following programme completion. This short horizon captures the initial impact period but offers no information on longer-term persistence, convergence, or reversal of effects. Any conclusions pertain strictly to the early post-programme phase.

One of the limitations of this study stems from the aggregation of firms founded before 2015 into a single “pre-2015” cohort. While this change was applied only to 21 companies out of 766 companies and we followed a design that was used in earlier studies (Krokan and Huang, 20024), it introduces potential bias, as the actual establishment year of these firms is not fully represented. Although we controlled for firm age in the matching process using total assets and registration year as covariates, this method might not completely capture the nuanced effects of firm age on performance. Future studies could address this limitation by adopting a more granular approach to age categorization or extending the observation period to observe long-term effects more comprehensively.

Additionally, while total assets were included as a pre-treatment control variable in the matching process, we did not directly control for other crucial pre-treatment performance indicators such as revenue or profitability in the regression analysis, mainly because many of the firms are start ups. This oversight may have led to selection bias, as firms with higher pre-treatment performance may be more likely to enter accelerator programs. Future research should include a broader set of pre-treatment variables, such as liquidity, profitability, and growth trends, to further mitigate potential biases and strengthen the internal validity of the analysis, especially if a longer pre-treatment history is applicable.

It is also important to note that the treatment register is incomplete. The SIVA data set covers eight publicly funded accelerators, yet some control firms may have joined private or corporate programmes that are not reported. Such misclassification would attenuate estimated differences and obscure true programme effects.

Robustness checks exclude exits or control for survival to maintain transparency and avoid unjustified assumptions or more complex adjustments. This will not identify when the failure occurred or, although at rates below 1 percent for most Norwegian firms, if the exit is caused by a successful outcome as a consequence of mergers and acquisitions.

Moreover, the analysis centres on five quantitative outcomes: revenue, employment, labour productivity, return on total capital, and survival. It does not assess other potential benefits of acceleration, such as innovation output, follow-on funding, or network expansion. The study, therefore, provides a partial view of programme performance. Beyond this, the research treats all accelerator cohorts as a single intervention. Programmes differ in curriculum depth, mentor quality, equity terms, and sector focus. Substantial heterogeneity in effectiveness could mean that the estimated average masks large positive or negative effects for specific accelerators.

Ultimately, the evaluation period coincides with relatively favourable macroeconomic conditions for Norwegian startups, including the COVID-19 shock, during which the national interest rate was at its lowest level in over 20 years and online companies soared. Workplace closures, the move to remote work, and social-distancing requirements during the pandemic contributed to an acceleration of digitalisation and technology enabled ventures (Stephan et al., 2021).

Future economic environments, shifts in the venture-funding climate or available technology may alter the relationship between accelerator participation and firm performance. A notable disruptive technology emerged in November 2022 with the launch of the Large Language Model (LLM) Chat GPT, enabling startups to do in-depth forecasting, market research, monetization strategies, pricing models, tracking performance metrics and much more in seconds (Marianantoni, 2023; OpenAI, 2025). Taken together, these limitations imply that the reported estimates should be viewed as correlations under current Norwegian conditions rather than definitive causal effects. They nevertheless provide a rigorous starting point for policy discussion and highlight areas where richer data or longer observation windows would improve the evidence base.

4. Results

Based on propensity score–matched samples, the relationship between accelerator participation and firm-level outcomes is examined using linear regression models. Dependent variables include changes in employment, revenue, productivity, return on total capital, and firm survival, with controls for firm size, registration year, region, and industry. To assess the credibility of the estimated effects, several robustness procedures have been implemented. Covariate balance is evaluated through standardized mean differences and visual inspection using a Love plot, confirming improved similarity between treated and control firms after matching. Sensitivity to extreme values is addressed through regressions using trimmed samples, while longer time horizons are considered to test the consistency of the results. Additional analyses investigate variation in treatment effects across main industry and regions with substantial representation in the sample. These strategies strengthen the empirical basis for interpreting the results and ensure that conclusions are not driven by model-specific assumptions or data limitations.

The baseline model combines regression with propensity-score matching, using registration year, industry code, region, and total assets to construct the score. Outcome variables include employee count, revenue, labour productivity, return on total capital, and survival. The average treatment effect on the treated (ATT) is calculated as the difference in mean one-year growth rates between accelerator participants and their matched controls. Table 2 summarises the baseline estimates from the propensity-score-matched cross-section regression. For each outcome the coefficient represents the average difference between accelerator participants and their three nearest matched controls, measured from the treatment year to one year post-participation. Number of employees have been transformed using the logarithmic (log) function, and revenue, productivity, and return on total capital have been transformed using the inverse hyperbolic sine (IHS) function to be comparable even with negative outcomes.

Table 2: Results baseline model.

Results baseline model				
Outcome variable	Estimates	Std error	95% Confidence interval	
No of employees	0,31***	0.03	0.11	0.26
Revenue	-0.05	0.39	-0.82	0,71
Productivity	0.44	0.36	-0.28	1.15
Return on total capital	-0.15	0.09	-0.33	0.02
Survival rate	0.79**	0.28	0.23	1.35

Note: * p<0.10, **<0.05, and *** p<0.01 indicates significance at 10, 5, and 1 percent levels, respectively. Estimates are a combination of log, ihs and odds rate, and must be individually interpreted. See following hypothesis overview for more details.

Table 3: Hypotheses 1 (revenue).

Regressions results dependent variable: Change in ihs revenue					
Predictor	Coefficient	Std. Error	t-value	p-value	Sign
Intercept	-1294.00	160.10	-8.08	< .001	***
Treatment	-0.05	0.39	-0.14	0.889	
Log (assets)	-0.02	0.07	-0.22	0.83	
Reg.year	0.64	0.08	8.08	<.001	***
Region: Østlandet	0.6	0.62	0.96	0.336	
Region: Trøndelag	1.13	0.82	1.39	0.166	
NACE: E	1.99	2.97	0.67	0.503	
NACE: I	-0.39	3.62	-0.11	0.914	
NACE: Q	-0.36	3.23	0.11	0.912	
Model Summary:					
$R^2 = 0.0703$	Adjusted $R^2 = 0.05284$	$F(21, 1118) = 4.026$		$p < 0.001$	N = 1140

Note: The table reports unstandardized coefficients from an OLS regression with robust standard errors. The dependent variable is the inverse hyperbolic sine (IHS) transformation of change in revenue. *p < .10, **p < .05, **p < .01

The regression results indicate that accelerator participation does not translate into higher sales during the first year after programme completion. The treatment coefficient is –0.05 in IHS units, and its p-value of 0.89 confirms the estimate is far from statistical significance. Put simply, accelerator firms and their matched counterparts record almost identical changes in revenue when other factors are held constant. The model explains only a small share of the variance in revenue growth, with an adjusted R^2 of about 0.05, underscoring how difficult it is to predict early sales performance from observable firm characteristics.

Among the covariates, baseline size measured by log assets has a negligible and non-significant effect, suggesting that larger resource bases do not guarantee faster short-run revenue expansion. Regional location and the industry categories displayed in the table are also insignificant, pointing to limited geographic or sectoral influence once firms are matched. Registration year is the only variable with a strong positive association (β = 0.64, p < 0.001) indicating that newer startups tend to record larger revenue gains in IHS terms. Overall, the evidence offers no support for Hypothesis 1, which predicted that accelerator participation would lead to faster revenue growth than that achieved by comparable non-participants.

Table 4: Hypotheses 2 (employment growth).

Regressions results dependent variable: Change in log number of employees					
Predictor	Coefficient	Std. Error	t-value	p-value	Sign
Intercept	-63.50	10.82	-5.87	< .001	***
Treatment	0.31	0.03	11.66	< .001	***
Log (assets)	0.01	0.01	2.25	0.025	*
Reg.year	0.03	0.01	5.87	<.001	***
Region: Østlandet	0.06	0.04	1.49	0.14	
Region: Trøndelag	0.08	0.06	1.44	0.15	
NACE: E	-0.40	0.20	-2.01	0.045	*
NACE: I	0.69	0.24	2.84	0.005	**
NACE: Q	-1.09	0.22	-5.01	<.001	***
Model Summary:					
$R^2 = 0.178$	Adjusted $R^2 = 0.163$	$F(21, 1118) = 11.54$	$p < 0.001$	$N = 1140$	

Note: The table reports unstandardized coefficients from an OLS regression with robust standard errors. *p < .10, **p < .05, **p < .01

The regression strongly supports the idea that accelerators speed up hiring. The treatment coefficient is 0.31 log-points and highly significant (p < 0.001). In percentage terms this corresponds to roughly a 31 percent jump in head-count during the first year after graduation, even after we account for firm size, age, region and industry. For a typical participant that started the program with seven employees, the result means about two more full-time jobs. This is a meaningful increase and shows that accelerators can help startups hire more people early on. The model explains a modest but respectable share of the variance in employment growth (adjusted $R^2 \approx 0.16$). Beyond the treatment effect, a few controls matter. Larger firms, measured by log assets, hire slightly faster (β = 0.01, p = 0.025), and newer firms also grow head-count more quickly, as indicated by the positive and highly significant registration-year coefficient (β = 0.03, p < 0.001). Regional covariates for e.g. Østlandet and Trøndelag are not significant, suggesting that location adds little explanatory power once other factors are held constant.

Industry effects are mixed. Firms in the utilities group (NACE E) grow more slowly (β = –0.40, p = 0.045), while those in information and communication (NACE I) expand faster (β = 0.69, p = 0.005). Human health and social work activities (NACE Q) show a pronounced negative coefficient (β = –1.09, p < 0.001). Still, these sectoral differences do not reduce the significance of the central result: the accelerator effect on hiring is large, robust and clearly distinguishable from zero. Consequently, the evidence fully supports Hypothesis 2, affirming that accelerator-backed ventures enlarge their workforce more rapidly than matched non-participants in the first post-programme year.

Table 5: Hypotheses 3 (labour productivity).

Regressions results dependent variable: Change in ihs productivity					
Predictor	Coefficient	Std. Error	t-value	p-value	Sign
Intercept	-1079.00	149.80	-7.21	< .001	***
Treatment	0.44	0.36	1.20	0.229	
Log (assets)	-0.02	0.06	-0.37	0.712	
Reg.year	0.53	0.07	7.21	<.001	***
Region: Østlandet	0.45	0.58	0.77	0.441	
Region: Trøndelag	0.15	0.77	0.20	0.844	
NACE: E	-0.83	2.78	-0.30	0.765	
NACE: I	-1.32	3.39	-0.39	0.697	
NACE: Q	-3.95	3.02	-1.31	0.191	
Model Summary:					
$R^2 = 0.05429$	Adjusted $R^2 = 0.03653$	$F(21, 1118) = 3.056$	$p < 0.001$	$N = 1140$	

Note: The table reports unstandardized coefficients from an OLS regression with robust standard errors. The dependent variable is the inverse hyperbolic sine (IHS) transformation of change in productivity. *p < .10, **p < .05, **p < .01

The regression provides no compelling evidence that accelerator participation raises labour productivity in the first post-programme year. The treatment coefficient is 0.44 in inverse-hyperbolic-sine (IHS) units, but the p-value of 0.23 shows the effect is not statistically significant; the 95 percent confidence interval easily spans zero. With an adjusted R^2 of approx. 0.04, the model explains little of the variation in productivity change.

Most control variables are likewise uninformative. Baseline firm size (log assets) and the regional covariates contribute no significant signal. Registration year is the only strong predictor: newer firms record higher productivity gains ($\beta = 0.53$, $p < 0.001$), a pattern consistent with early catch-up dynamics. Industries Utilities (E), Information and Communication (I), and Human Health and Social Work (Q) are all insignificant, suggesting no sector-specific advantage once firms are matched. Taken together, the results indicate that any efficiency benefits from accelerator participation either require more than a year to materialise or are offset by integration costs associated with rapid hiring. Hypothesis 3, which predicted larger productivity gains for treated firms, is therefore not supported.

Table 6: Hypotheses 4 (return on total capital).

Regressions results dependent variable: Change in ihs return on total capital					
Predictor	Coefficient	Std. Error	t-value	p-value	Sign
Intercept	-5.20	36.55	-0.14	0.887	
Treatment	-0.15	0.09	-1.74	0.083	*
Log (assets)	-0.03	0.02	-1.75	0.080	*
Reg.year	0.003	0.02	0.16	0.872	
Region: Østlandet	-0.17	0.14	-1.23	0.219	
Region: Trøndelag	-0.02	0.19	-0.13	0.899	
NACE: E	0.72	0.68	1.06	0.289	
NACE: I	-1.47	0.83	-1.79	0.075	
NACE: Q	0.02	0.74	0.03	0.977	
Model Summary:					
$R^2 = 0.02393$	Adjusted $R^2 = 0.005496$		$F(21, 1112) = 1.298$	$p = 0.1656$	$N = 1134$

Note: The table reports unstandardized coefficients from an OLS regression with robust standard errors. The dependent variable is the inverse hyperbolic sine (IHS) transformation of change in total capital. *p < .10, **p < .05, **p < .01

The model offers only weak and unstable evidence that accelerators affect capital efficiency. The treatment coefficient is –0.15 in inverse-hyperbolic-sine (IHS) units with a p-value of 0.083, making it marginally significant at the ten percent level but not at stricter thresholds. The sign is negative, meaning accelerator firms show a slight decline in return on total capital relative to controls. Baseline firm size also has a marginal negative effect ($\beta = –0.03$, $p = 0.08$), while all other covariates, including registration year, region, and industry dummies, are insignificant. Overall model fit is low, with an adjusted R^2 of roughly 0.006 and an F-test that fails conventional significance, indicating very limited explanatory power.

Because the negative treatment effect is small, only weakly significant, and attached to a poorly fitting model, we treat it as inconclusive. Additional checks with alternate matching specifications and outlier trimming cause the coefficient to lose significance entirely, confirming its lack of robustness. Therefore, the data does not provide persuasive support for Hypothesis 4, which anticipated higher returns on total capital for accelerator participants.

Table 7: Hypotheses 5 (survival rate).

Binary logistics regressions predicting bankruptcy (odds ratios)					
Predictor	Odds ratio	Std. Error	z-value	p-value	Sign
Intercept	8.33e+254	126.36	4.65	< 0.001	***
Treatment	2.20	0.28	2.78	0.005	**
Log (assets)	1.05	< 0.001	-0.41	0.680	
Reg.year	0.75	0.06	-4.67	< 0.001	***
Region: Østlandet	0.80	0.50	-0.45	0.651	
Region: Trøndelag	0.39	0.85	-1.10	0.274	
NACE: E	3.88e-07	1722.34	-0.01	0.993	
NACE: I	8.33	1.68	1.26	0.208	
NACE: Q	2.87e-07	1876.21	-0.01	0.994	

Model Summary:
Null deviance = 464.32 on 1139 degrees of freedom
Residual deviance = 420.67 on 1118 degrees of freedom AIC = 464.67 N = 1140

Note: Odds Ratios (ORs) are calculated by exponentiating the logistic regression coefficients. An OR > 1 indicates higher odds of bankruptcy, while OR < 1 indicates reduced odds. Statistical significance is based on z-tests using robust standard errors. *p < .10, **p < .05, **p < .01

A binary logistic regression was estimated using a generalized linear model (GLM) with a logit link and binomial family, where the dependent variable (bankrupt_flag) indicates whether a company went bankrupt (1) or not (0) between 2019 and 2023. The estimated coefficients from the logistic regression were subsequently exponentiated to obtain odds ratios, which provide a more interpretable measure of effect size. While the raw coefficients represent changes in the log-odds of bankruptcy, the odds ratios indicate the multiplicative change in the odds of bankruptcy associated with a one-unit change in each predictor, holding all other variables constant. As a result, treated firms have significantly higher odds of bankruptcy compared to untreated firms, with an odds ratio of 2.20 ($p = 0.005$), suggesting that treatment is associated with a more than twofold increase in bankruptcy risk. In contrast, registration year is negatively associated with bankruptcy, with an odds ratio of 0.75 ($p < 0.001$), implying that firms registered more recently have significantly lower odds of going bankrupt. Firm size shows no significant effect on bankruptcy risk (OR = 1.05, $p = 0.680$). Neither regional controls (e.g., Østlandet or Trøndelag) nor the selected sectoral covariates (e.g., NACE E, I, Q) exhibit statistically significant associations with bankruptcy in this model. Notably, some NACE categories yield extreme or near-zero odds ratios, which may reflect sparse data or perfect separation and should be interpreted with caution. The model has good convergence (16 Fisher scoring iterations) and a reduction in deviance (null deviance = 464.32, residual deviance = 420.67), indicating improved fit relative to the intercept-only model.

4.1. Robustness Checks

To assess whether the estimated differences between accelerator participants and matched non-participants are a result of modelling choices or firm-level observations, we conduct a series of sensitivity analyses. We begin with balance diagnostics, which are tests that verify whether the treated firms and the matched control firms are indeed similar in all the characteristics used for matching. After propensity-score matching, we compare each covariate, such as registration year, industry, region, and total assets, between the two groups.

Next, we implement a *doubly robust adjustment* by combining two bias-reduction steps in one estimator: first, propensity-score matching aligns treated and control firms on observable characteristics; second, an outcome regression is performed on the matched sample, including the PSM covariates as additional controls. The estimate is considered "doubly robust" because it remains statistically consistent if either the matching model or the outcome regression model is correctly specified.

To address skewness in the outcome variables, we applied *log transformations* to the outcome variable for employees. We used the *inverse hyperbolic sine* (IHS) transformation for revenue, productivity, and return on total capital. These adjustments mitigate the influence of extreme values while retaining interpretability for zero and negative observations. The IHS approach is particularly useful for handling semi-continuous financial data. All transformations were conducted prior to estimation to ensure a consistent scale across treated and control units.

For additional mitigation on skewed performance variables, *outlier influence* is examined by trimming the top and bottom one per cent of each continuous outcome. Point estimates remain within their original confidence intervals, implying that extreme cases do not drive the results. Functional-form dependence is explored by converting outcomes to logarithms and by substituting revenue per employee for labour productivity; the direction and significance of the effects persist across these transformations.

To reflect that most firm closures represent failure and complete loss of output and employment, we code exits as zero in our regressions. This avoids survivorship bias and ensures that treatment effects capture both growth and failure. Finally, we test for *heterogeneity* by re-estimating treatment effects within subsamples defined by region (Østlandet and Vestlandet respectively versus other regions) and sector (technology-intensive NACE group J versus less technology-intensive sectors). While magnitudes vary, the sign of the impact is uniformly identical to the baseline regression, indicating that no single subgroup is solely responsible for the overall pattern.

5. Discussion

Assessing accelerator effectiveness through a matched cross-section analysis has yielded a mixed picture. Our guiding star was the thought that accelerators truly accelerate companies, and that an increase in revenue as put forth in *Hypotheses 1* should be the clearest indicator. Results indicate nearly identical changes in revenue compared to their matched counterparts, besides younger firms showing some signs of increased sales. Several factors can explain the null result. One year may simply be too short for new hires and network contacts to turn into paying customers, or the Norwegian accelerator programs may simply have too infrequent mentoring or less "push" on participants compared to their international counterparts that achieve such increase (Avnimelech & Rechter, 2024; Crișan et al., 2019).

On employee growth, one clear trend emerges: participants expand their teams significantly faster compared to non-participants. On average, a roughly 31% increase in headcount at statistically strong levels offers direct support for *Hypothesis 2* on employment increase, equivalent to about two more full-time jobs (Pauwels et al., 2016). Input from former participants enabled a more holistic understanding of the drivers behind employee growth, with 4 of the 5 respondents stating that the accelerator program had made them "increase their ambitions" and decide to "go all in" as a direct result. Several interviewees mentioned that they "would have remained small" if it had not been for the accelerator and that it "changed their way of thinking about business", possibly as direct results from elevated founder capabilities (Avnimelech & Rechter, 2024).

However, the additional growth in staff does not translate to improved productivity. With no signs of improved revenue, it comes at no surprise that the desired revenue-per-employee increase from *Hypothesis 3* remains inconclusive. Findings are not significant besides for younger firms, again potentially caused by improved accelerator performance or that they are more capable of doing "more with less" than their older counterparts (Pauwels et al., 2016). Effects from LLMs cannot yet be taken into account, and more longitudinal data is required for sufficient interpretations.

Considering whether investors and lenders earn a return on their capital is sought to be answered in *Hypothesis 4*, but the regression model offers only weak and unstable evidence. Several reasons may account for this null finding. Even with newly hired employees, these often need time to become fully productive, so any ROTC gains may emerge only after a longer horizon. In addition, many accelerator curricula emphasize experimentation and learning over immediate operational efficiency, temporarily lowering output and falling in line with general trends of Norwegian startups (Cappelen et al., 2015).

Hypothesis 5 on survival rate provided a contradictory and unexpected outcome, with accelerator graduates showing about 2.2 times higher odds of exit. Consistent with other studies, rapid post-programme scale-up happens at the expense of financial success (Crișan et al., 2019; Lall et al., 2020). If cash inflows lag, liquidity pressure increases and the risk of failure climbs. Two founders expressed frustration on the limited availability of risk capital in Norway, with one claiming "most capital is allocated towards low risk real estate", as indicated in table 2, showing most companies in Norway operate within NACE group L - Real Estate Activities. The same founder also asked for accelerators to "improve information on availability of public funding", attending an equity-free program that might give teams limited working capital for rapid market expansion. Their comments reinforce the liquidity-risk mechanism suggested by the quantitative data. Finally, a significant finding is an increased survival rate for younger firms, signalling a possible improvement of accelerator performances as the programs mature.

Robustness checks support these conclusions. Re-estimating the model with alternative matching algorithms, trimming extreme observations, and extending the time horizon to three years for cohorts with available data yields an even stronger employment increase at 51%.

Key findings: Employment growth at approx.31% first year and 2.2 higher odds ratio of bankruptcy apply for all treated companies, and minor employment growth and increased odds of survival for younger companies. Other outcome variables remain inconclusive.

6. Conclusion

This study explored whether accelerators stimulate a stronger entrepreneurial drive and improve early firm outcomes in Norway's publicly supported, capital-scarce context. By leveraging cohort rosters from eight SIVA-affiliated programmes linked to administrative data, we identified a consistent and economically meaningful association between accelerator participation and employment growth one year after completion. However, we did not find reliable effects on revenue or labour productivity, and observed weak and unstable changes in return on total capital, alongside a deterioration in short-run survival rates. These findings suggest that accelerators may primarily act as capability catalysts—mobilizing human resources and refining organizational routines—while the conversion to financial performance appears to be delayed and dependent on complementary assets and market access.

Contributions to Theory

We introduce a capability–conversion framework for evaluating accelerators. The capability effects—such as upgrades in human and social capital and improvements in execution discipline—appear early, while conversion effects—such as revenue growth, efficiency, and survival—may require additional time and complementary factors (e.g., capital, distribution channels, or anchor customers) and therefore might lag. In smaller, publicly supported ecosystems like Norway's, the effectiveness of accelerators is contingent on the broader ecosystem context. This aligns with calls to move beyond generalizations based on U.S. accelerators and to consider how accelerator design and context interact (e.g., selection versus treatment dynamics). This framing helps explain mixed findings in the literature, suggesting that heterogeneous outcomes are a predictable feature of multi-stage interventions rather than simply statistical noise.

Implications for Practice

Founders should approach accelerators as intensive capability-building programs and plan explicitly for a post-programme conversion runway. This includes sequencing hiring based on validated demand, budgeting for extended cash cycles, and securing pilots or distribution experiments before graduation. Mentors and programme managers can enhance impact by focusing on pipeline quality, pricing discipline, and conversion milestones, rather than solely refining the pitch.

Implications for Policy

If the goal is sustainable firm performance rather than short-term employment growth, accelerators should be paired with targeted post-programme instruments that support the conversion phase—such as milestone-based vouchers for customer pilots or procurement pathways with public or corporate anchors. Additionally, policy should include accreditation standards for mentoring intensity and sales enablement. Harmonized national tracking of post-programme outcomes would facilitate iterative programme improvements and adaptive funding. Ultimately, in Norway's context, the critical challenge lies in the conversion of capabilities to performance, rather than the formation of capabilities themselves.

Future Research Agenda

Our study provides valuable insights into the short-term effects of accelerator participation, yet it also highlights several important avenues for future research.

First, given the limitations of using a one-year observation period, future studies should explore longer time horizons to capture the full impact of accelerator participation on firm performance. Extending the follow-up period to several years post-treatment would allow researchers to assess the persistence of treatment effects and better understand the long-term benefits or drawbacks of accelerator programs. Additionally, the limited process measures used in this study—such as mentoring intensity, access to follow-on finance, and customer acquisition—constrain our ability to infer long-term effects. Future research should integrate richer process data, such as detailed mentoring activities and access to funding, to provide a more nuanced understanding of how accelerators influence firm trajectories over time.

Second, future studies should account for industry-specific factors, such as time-to-market and levels of innovation, which can significantly affect firm outcomes. Given Norway's comparative advantages in sectors like maritime/offshore engineering and renewables, along with its highly educated engineering workforce and rapid technology adoption (Amby, 2024; Distriktsdepartementet, 2023), sector-tailored accelerator programs and partner networks could enhance the conversion of capabilities into long-term performance. Future research could explore how accelerator designs can be optimized for different industries to maximize the conversion of human capital and network gains into sustainable firm growth.

Moreover, previous performance indicators play a critical role in determining firm growth and survival. Therefore, future studies should explicitly control for revenue and profitability, in both the matching and regression analyses. This would allow for a more precise estimation of treatment effects and help mitigate potential selection bias.

Finally, our study suggests that accelerators primarily impact human capital—such as team growth—rather than financial performance in the short run. Future research could explore the specific mechanisms driving these effects, such as the role of mentoring intensity, access to networks, and capital availability, and examine how these factors interact over time to shape firm success.

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