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BEYOND DEMO DAY:
SORTING AND VALUE ADDED IN STARTUP ACCELERATORS

Youn Baek
Deepak Hegde

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ABSTRACT

We study who joins startup accelerators, how founders sort across programs, and which accelerators improve startup outcomes. Using a comprehensive sample of about 750,000 U.S. startups linked to 329 accelerators, we adapt the teacher value-added framework from education economics to estimate accelerator value added (AVA) while accounting for sorting. Selection is systematic: observably better ventures are more likely to enter accelerators and to sort into higher-AVA programs. Yet accelerator performance is highly dispersed. Most accelerators have negative value added relative to a no-accelerator benchmark, while a small right tail generates large gains. High-AVA accelerators predict better long-term outcomes, including acquisition, employment, revenue, and valuation, and are also more likely to accelerate the shutdown of weaker ventures. We validate AVA using internal applicant data from a large U.S. non-equity accelerator.

Youn Baek
New York University
syounbaek@gmail.com

Deepak Hegde
New York University
Leonard N. Stern School of Business
and NBER
dh99@stern.nyu.edu

1 Introduction

Startup accelerators have become a key gateway for early-stage entrepreneurs. Figure 1 shows that accelerator participation has grown sharply over the past two decades, and many high-profile companies trace their origins to accelerator cohorts (*e.g.*, Airbnb, Dropbox, Stripe, and Coinbase are all alumni of Y Combinator). This rise has made accelerator affiliation both a salient market signal and a potential source of advantage for startups—through certification under asymmetric information and access to investor networks (Cohen, 2014; Gonzalez-Uribe and Leatherbee, 2018; Hochberg and Fehder, 2015; Howell, 2020).

For founders, however, joining an accelerator is not costless. Many programs take equity stakes, often around 5 to 10 percent, while requiring an intensive multi-month commitment of founder time. Accelerators also typically impose mentoring, programming, and milestone pressure that can redirect managerial attention and shape strategic choices at a formative stage of the firm (Hallen et al., 2020). These interventions may create value by improving execution, learning, and investor access. But they may also impose dilution, consume scarce time, or push ventures toward milestones that help short-run fundraising without increasing long-run firm value. Hence, the central question for startup founders is which accelerator, if any, is worth joining.

We address this founders' dilemma by studying three related questions. First, who joins startup accelerators? Second, how do founders sort across programs? Third, which accelerators improve startups' long-term outcomes after accounting for sorting? Answering these questions is challenging because founders self-select into applying, and programs select among applicants. Observed performance differences across accelerators therefore combine sorting and program influence, echoing the classic identification problem in entrepreneurial finance when measuring the effects of intermediaries (Hegde and Tumlinson, 2014; Hochberg et al., 2007; Hsu, 2004; Sørensen, 2007).

Prior evidence shows that some accelerators can improve fundraising and early growth, but three limitations prevent the literature from fully resolving the founders' dilemma. First, many studies examine one program or a narrow setting, limiting what we can infer about the broader accelerator market (Assenova and Yu, 2023; Chang and Assenova, 2025; Fehder, 2024; González-Uribe and Reyes, 2021; Hallen et al., 2020, 2023; Impink et al., 2025; Yu, 2020). Second, selection is first-order: even in careful designs, it is difficult to fully separate treatment effects from endogenous matching between entrepreneurs and intermediaries (Sørensen, 2007). Third, "accelerator participation" is not a single treatment. Programs vary sharply in screening, incentives, networks, and contracting, so average effects can mask economically meaningful variation in program quality (Hochberg and Fehder, 2015). As a result, the existing literature is informative about whether some accelerators can matter, but less informative about which programs systematically add value and whether that value persists beyond early financing outcomes.

We address these gaps using a comprehensive dataset covering approximately 750,000 U.S. startups and 329 accelerators. We adapt the teacher value-added framework from education economics (Chetty et al., 2014a,b) to estimate accelerator value added (AVA) after accounting for sorting. In the education setting, value added measures how much individual teachers improve student outcomes after conditioning on prior performance and observables. We apply the same logic to accelerators, using post-entry fundraising within two years as the short-run outcome from which we extract a signal of persistent program effectiveness. We focus on this horizon because early-stage entrepreneurship is subject to significant financing risk: ventures must repeatedly return to capital markets to continue learning and scaling, so near-term fundraising is a first-order determinant of growth and survival (Black and Strahan, 2002; Kerr and Nanda, 2009; Kerr et al., 2014).

We interpret AVA as a forecast of an accelerator's contribution to short-run post-entry fundraising, and then ask whether this short-run measure predicts longer-run outcomes such as acquisition, growth, valuation, and survival. Our analysis yields four findings. First, selection into accelerators is systematic: observably stronger startups are more likely to enter accelerators and to sort into higher-AVA programs. For example, accelerated startups have about 60% higher ($0.4 million higher) early-stage funding, with corporate accelerators enrolling the most well-capitalized firms of all accelerator types. University/government programs disproportionately enroll science-oriented teams with higher STEM founder shares. This emphasis on founding teams is consistent with experimental evidence that early-stage investors respond strongly to information about teams when making screening decisions (Bernstein et al., 2017). Industry composition also differs systematically. These patterns underscore that accelerator entry reflects substantial sorting on observables, motivating an empirical framework designed to separate treatment from selection.

Second, accelerator performance is highly heterogeneous. Most accelerators –60-80 percent– have negative value added relative to a no-accelerator benchmark, while a small right tail generates large gains.

Third, high-AVA accelerators predict economically meaningful improvements in long-run outcomes, not only short-run funding. A one-standard-deviation increase in AVA is associated with a 1.7 percentage point higher probability of successful acquisition, relative to a baseline mean of 8.0%. Higher AVA is linked to stronger operating performance: a one-standard-deviation increase predicts about a 1.6 percentage point increase in the probability of employment growth, a 1.9% increase in ten-year employment, and roughly a 10.7% increase in ten-year revenue, and a 4.4% increase in ten-year valuation following acceleration. These patterns are robust to controlling for total lifetime funding, suggesting that AVA captures program contributions beyond the simple channeling of capital.

Higher AVA also corresponds to roughly a 0.9 percentage point rise in closure probability, concentrated among lower-performing startups, consistent with accelerators speeding learning and disciplined exit rather than mechanically prolonging weak ventures. This pattern fits the view of entrepreneurship as experimentation, in which intermediaries can add value by accelerating learning and reallocation—not only by increasing survival (Camuffo et al., 2020; Kerr et al., 2014; Yu, 2020).

Fourth, AVA is not merely a restatement of selection. Decompositions show that AVA explains a substantial portion of cross-accelerator differences in post-entry fundraising, with the remainder attributable to sorting on baseline startup characteristics. We further validate AVA using internal applicant and evaluation data from a large non-equity accelerator (Accelerator-X), benchmarking model-implied effects against quasi-experimental comparisons among near-admit finalists.

Our paper makes three main contributions to the literature on how intermediaries shape startup financing, survival, and growth. First, we provide market-wide evidence on selection into accelerators and sorting across programs by linking startups to accelerator cohorts. This complements existing accelerator studies—typically based on a small number of programs—by documenting how founder characteristics, early financing, and geography predict entry and program choice (*e.g.*, Assenova and Amit (2024)).

Second, we adapt the education value-added framework to measure accelerator quality and show how to validate these measures in a setting with strong two-sided selection and a no-participation benchmark (Chetty et al., 2014a,b; Easterly and Pennings, 2025; Nguyen, 2025). Conceptually, this parallels classic challenges in venture capital: observed performance differences across intermediaries reflect both sorting (deal flow) and influence (value creation).

Third, by linking accelerator value added to longer-run outcomes—and validating the estimates using applicant-based evidence from Accelerator-X—we clarify when accelerators do more than increase demo-day visibility. Taken together, our results help reconcile mixed findings on accelerator impacts by showing that average "accelerator effects" can be misleading when there is substantial heterogeneity in program value added (Dalle et al., 2025; Hallen et al., 2020; Yu, 2020). More broadly, our findings suggest that the relevant question is not whether accelerators work on average, but which accelerators actually add value.

The rest of the paper is organized as follows. Section 2 reviews related literature and clarifies how our approach contributes to existing identification strategies in the accelerator setting. Section 3 describes the data, sample construction, and key variable definitions. Section 4 documents selection into and sorting across different types of accelerators. Section 5 presents the value-added framework, estimation, and validation tests. Section 6 uses internal applicant and evaluation data from Accelerator-X to provide complementary validation. Section 7 examines how accelerator value added predicts longer-run startup outcomes and founder returns. Section 8 deals with startup-accelerator match based on accelerator value-add. Section 9 concludes.

2 Literature review

This paper connects three literatures. The first studies the emergence, design, and heterogeneity of startup accelerators. The second examines how entrepreneurial finance intermediaries shape startup outcomes under information frictions and financing constraints. The third develops value-added methods for recovering persistent effectiveness from repeated cohorts. Our paper links these literatures through a common empirical problem: accelerator participation is highly selective, and founders sort across heterogeneous programs. As a result, observed differences in outcomes across accelerators reflect both sorting and program influence. We address this problem by estimating program-level accelerator value added (AVA) and by asking whether this short-run measure of program effectiveness predicts longer-run startup outcomes.

The emergence and design of accelerators. Accelerators are typically defined as fixed-term, cohort-based programs that combine mentorship, structured advice, and investor exposure, often culminating in a Demo Day (Cohen, 2014). Subsequent work distinguishes accelerators from incubators and other intermediaries and emphasizes heterogeneity in business models and incentives—equity-taking versus non-equity, independent versus corporate or university affiliated, and generalist versus sector-focused programs (*e.g.*, Avnimelech et al. (2025)). This heterogeneity implies that "accelerator participation" is not a single treatment: programs differ in equity-stake, screening intensity, curriculum and mentor networks, and the extent to which their incentives align with founder value creation.[1] Our empirical strategy builds on this point by estimating program-specific effectiveness rather than an

[1] Equity-taking accelerators resemble early-stage financiers whose contracts allocate cash-flow and control rights (Kaplan and Strömberg, 2003)

average accelerator effect.

Empirical evidence on accelerator impacts. A second strand estimates the effects of accelerators on startup outcomes, often using variation from specific programs or settings (Camuffo et al., 2020; Fehder, 2024; Gonzalez-Uribe and Leatherbee, 2018). These studies show that some programs increase follow-on funding, growth, or employment, and that impacts can vary across cohorts and industries and with program reputation (*e.g.*,Hallen et al. (2020)). Two limitations remain central for our purposes. First, most evidence comes from a small number of programs or treats accelerators as homogeneous, even though program design and incentives differ sharply. Second, most studies focus on short-run outcomes—especially early fundraising—leaving open whether accelerator-induced gains persist into longer-run trajectories such as exits and survival (Assenova, 2021; Assenova and Yu, 2023; Chang and Assenova, 2025). Our contribution is to measure effectiveness heterogeneity across a broad set of accelerators and to test whether programs that move early funding also move longer-run outcomes.

Selection, sorting, and value added. A third strand studies selection into entrepreneurial finance intermediaries—angels, VCs, corporate investors—and how screening and support interact with entrepreneur characteristics. Accelerators fit naturally into this broader set of intermediaries but are distinctive in three respects: they operate through repeated cohorts, they bundle mentoring and education with investor access, and they include a large mass of small programs with widely varying incentives. These features make selection and sorting first-order: founders self-select into applying and then sort across programs, while programs select among applicants. The resulting outcome differences across accelerators combine treatment and selection, motivating a framework designed to separate the two (Santamaria and Breschi, 2025).

A central theme in entrepreneurial finance is that intermediaries can add value through multiple channels: certification/affiliation under asymmetric information, network access to follow-on investors and specialized services, and—in equity-taking settings—governance and contracting that shape incentives and intervention intensity (Hegde and Tumlinson, 2014; Hochberg et al., 2007; Hsu, 2004; Kaplan and Strömberg, 2003). These channels map naturally to accelerators and motivate our focus on (i) short-run fundraising outcomes to extract signals about accelerator value add and (ii) heterogeneity across programs.

Our measurement approach draws from value-added models in education, which recover the persistent contribution of teachers or schools to student outcomes after conditioning on prior performance and observables (Chetty et al., 2014a,b; Kane and Staiger, 2008; Rothstein, 2017). The key diagnostic in this literature is forecast performance: value-added measures should predict future outcomes for new cohorts out of sample. We translate this logic to accelerators. Pre-entry funding and founder traits play the role of lagged scores, accelerator–cohort effects capture program contributions, and our accelerator value-added estimate is explicitly a leave-cohort-out forecast of impacts on short-run post-entry fundraising. We then discipline concerns about sorting on unobservables using forecast tests and applicant-based validation from Accelerator-X. This focus on short-run fundraising is also motivated by entrepreneurship-as-experimentation: ventures face financing and continuation risk in staged learning, so near-term access to capital is a key margin on which intermediaries can matter (Kerr et al., 2014).

The analogy of our setting to teacher value added is useful but imperfect. As Chetty et al. (2014a) note, in the teacher setting, students sort primarily into schools, not directly to teachers, and most variation in teacher quality lies within schools. In our setting, startups sort directly into the treatment unit itself—the accelerator. This makes sorting a more central concern here than in the teacher setting and raises the burden on matching, forecast validation, and applicant-based benchmarking.

Our approach is complementary to program-specific experimental and quasi-experimental studies. Those designs are well suited to estimating local effects for particular programs. Our objective is different: we seek to measure persistent heterogeneity in accelerator effectiveness across the broader market and to test whether short-run measures of program effectiveness predict longer-run startup outcomes. In that sense, our paper is less about estimating one average accelerator effect than about measuring how much accelerator quality varies and why that variation matters.

3 Data

Data overview. Our empirical analysis combines a near-universe sample of U.S. startups with a comprehensive registry of accelerator participation. We construct a startup dataset spanning founding through financing and outcomes, link startups to accelerators and cohort years, and harmonize measures of pre-entry startup quality, founder characteristics, and post-entry outcomes. This section describes the component datasets, linking procedures, key definitions, and remaining limitations.

Startup universe and data sources. We begin with a comprehensive panel of U.S. startups founded between 2001 and 2024. The panel is constructed by combining two leading commercial datasets –Crunchbase and Pitchbook– on private company activity and venture financing (Retterath and Braun, 2020). Both sources compile information from multiple channels, but they differ in emphasis and process. Crunchbase reports that the vast majority of its company and financing data comes directly from venture partners and an active community of contributors, supplemented by automated methods that track government filings and news publications and by in-house verification. PitchBook reports that it gathers information using web crawlers and secondary sources, processes it with AI/ML tools, and then relies on a specialized team to vet and improve the data before inclusion in the platform. While the two data sources do not perfectly overlap (Lyonnet and Stern, 2024), each offers its own strengths and weaknesses, motivating our decision to use both. We merge and de-duplicate these sources to maximize coverage of early-stage firms and financing events.

Merging and de-duplication. We harmonize firm identifiers across sources based on normalized firm name, headquarters state, and founding year, supplemented by fuzzy matching on names for residual cases (Fellegi and Sunter, 1969; Lindsay et al., 2023). We treat two records as the same startup if they share a domain or if their normalized names match exactly within location; ambiguous matches are resolved conservatively to avoid false merges. We then consolidate financing events by date, round type, and investor lists, prioritizing records with richer metadata when duplicates exist.

Crunchbase includes approximately 700,000 startups, and PitchBook contains about 110,000 startups founded between 2001 and 2024. Using our probabilistic matching procedure, we successfully match 80,000 PitchBook startups to their corresponding records in Crunchbase. The combined dataset therefore covers roughly 750,000 unique U.S. startups. Appendix A.1 reports the match rates and the distribution of discrepancies between sources.

Apart from record linkage itself, another potential challenge in combining the datasets is harmonizing venture funding information. Whenever PitchBook funding data are available, we use PitchBook as the primary source. This procedure creates three groups in the data: Crunchbase-only, PitchBook-only, and the intersection of the two. In all analyses, we include sample fixed effects for startups included in Crunchbase only to account for systematic differences across these groups. From the two data sources, we are able to identify key outcome variables such as post-accelerator venture funding, IPO and acquisition events, the timing of these events, and indicators of business closure. For a smaller subset of startups, we also gather information from Pitchbook on their revenues, employment, and post-financing valuations. These outcomes map naturally to those studied in the entrepreneurial finance literature about capital access, continuation, and exit under uncertainty (Farré-Mensa et al., 2020; Lerner and Malmendier, 2013). More detail on harmonization is provided in section A.3.

Accelerator participation. We follow Cohen (2014) in defining startup accelerators as fixed-term, cohort-based programs that provide education and mentorship to startup founders. We use textual description availble in the Pitchbook data to remove incubators from the accelerator sample because most incubators focus on providing space and other infrastructure for startups, rather than advice or mentorship as accelerators do. We further explain our data choices to identify accelerators in section A.4. Following Yu (2020), we focus on U.S.-based accelerators with at least 30 alumni startups spanning at least two different cohorts. We exclude startups ever affiliated with accelerators that do not meet this threshold, as well as those affiliated with non-U.S.-based accelerators. This procedure yields 329 accelerators, covering 38,979 accelerated startups from 2005 to 2024, including 29,263 accelerated between 2005 and 2022. The average accelerator-year cohort contains 10.98 startups (min = 1, max = 440), while the average accelerator in our sample has accelerated 118.48 startups across all cohort years (min = 31, max = 3,000). Of the 329 accelerators, 21 are corporate-affiliated and 80 are affiliated with government organizations or universities. In terms of business model, 85 are equity-taking accelerators and 244 are non-equity accelerators.

A startup can appear in multiple spells if it participates in more than one accelerator. We define the "entry" cohort as the first observed cohort year in which the startup participates in accelerator j. About 81% of startups in our sample participate in one accelerator with the rest participating in multiple accelerators A.8 reports the sample distribution of startups by the number of accelerators they participated in in our data.

Founder background. We identify founder backgrounds using LinkedIn data provided by Revelio Labs. For all U.S. startups founded between 2001 and 2024, we examine individuals who worked at these firms and held roles such as founder, co-founder, or CEO. From this information, we construct several founder characteristics, including gender, prior entrepreneurial experience, MBA degree, STEM degree, and immigrant status (inferred from whether the individual completed their undergraduate education outside the United States). We also measure founder experience as the number of years between their undergraduate graduation and the startup's founding year, and we record the size of the founding team. To measure university quality, we use the Center for World University Rankings (CWUR). Any institution not covered in the ranking is assigned the lowest score. We use the LinkedIn url to match the LinkedIn founder sample to the corresponding Crunchbase or PitchBook startup record. These founder measures are central lagged-score analogs in early-stage finance because investor screening places substantial weight on teams (Bernstein et al., 2017).

Short-run fundraising outcome (AVA target). Our primary outcome for estimating accelerator value added is post-entry fundraising within a fixed window after accelerator entry. Specifically, we measure (i) an indicator for any follow-on financing event and (ii) the log of total dollars raised within two years of entry, excluding funding that is mechanically bundled with the accelerator program's standard deal terms when relevant. We focus on this two-year window because it is observed for a large share of startups and is plausibly proximate to accelerator inputs such as fundraising advice, network access, and investor introductions. We treat this outcome as the analog of "test scores" in the education value-added setting: it provides a frequent, comparable signal used to forecast persistent program effectiveness. This horizon also aligns with staged entrepreneurial experimentation under financing constraints: ventures face continuation and "financing risk" at each return to capital markets, so near-term fundraising is a first-order margin on which intermediaries can affect trajectories (Kerr and Nanda, 2009). Section 5.5 provides more explanation on the choice of outcome variables to construct accelerator value added.

Pre-entry startup quality measures. To address selection, we construct a set of predetermined covariates measured strictly before accelerator entry. The most important are "lagged-score" analogs: cumulative funding raised prior to entry, the timing of first financing, and pre-entry funding growth (when observed). We also include founder background measures (e.g., prior entrepreneurial experience, education and technical background), industry and geography fixed effects, and founding-year controls. These covariates capture baseline quality and visibility in capital markets—key drivers of both sorting into intermediaries and subsequent fundraising. Section 4 uses these variables to describe selection and sorting; Section 5 uses them to residualize outcomes before estimating value added.

Coverage and measurement limitations. We acknowledge that even comprehensive commercial datasets like Crunchbase and Pitchbook do not observe the full population of startups or all financing events, and coverage may vary systematically with a startup's visibility and investor connections. This concern is especially salient for accelerators and early-stage firms. Crunchbase emphasizes direct reporting from venture partners and community contributors, supplemented by automated tracking of filings and news and manual verification; PitchBook emphasizes crawler- and source-based collection that is then vetted by a specialized team. These processes imply that missingness is unlikely to be random. We therefore (i) triangulate across sources, (ii) report discrepancies and match rates, (iii) focus on within-accelerator cohort variation in the value-added estimation, and (iv) validate the resulting measures using internal applicant and evaluation data from Accelerator-X.

Application data from Accelerator-X (a large U.S. non-equity accelerator) We collected data on all startups that applied to Accelerator-X between 2018 and 2023. Approximately 4,000 startups submitted applications, and about 400 ultimately graduated from the program. Startups first underwent an application review, after which a subset was invited to an interview round. A more selective group was then admitted.

Each startup received evaluation scores from at least two independent reviewers on a scale from 1 to 7. These scores served as an important input into the admissions process, but they were not used as a strict cutoff. After scoring, Accelerator-X directors and evaluation experts met to deliberate on each case, and decisions—especially for startups with mid-range scores—were based on discussion rather than a mechanical rule. These applicant-stage comparisons help address the classic entrepreneurial finance concern that intermediary "quality" rankings may reflect deal flow rather than influence (Sørensen, 2007).

4 Selection into and across accelerators

This section documents selection into accelerators and sorting across accelerator types. The goal is descriptive but essential: because founders choose whether and where to participate—and accelerators select among applicants—raw differences in outcomes across programs combine treatment with sorting. Conceptually, this is the same identification challenge emphasized in entrepreneurial finance when comparing intermediaries such as VCs: observed performance differences reflect both deal flow (sorting) and intermediary influence (Sørensen, 2007). We therefore establish the selection facts first, and then estimate accelerator value added in Section 5 using a framework designed to separate persistent effectiveness from differences in who enters each program.

Table 1 compares startups that ever enter an accelerator to those that never do. Accelerated startups differ systematically in pre-entry characteristics that are central in early-stage finance—most notably baseline financing, founder human capital, and geography (Guzman and Stern, 2015). These differences underscore why "accelerated vs. non-accelerated" comparisons cannot be interpreted causally without strong assumptions: accelerator participation reflects both founder self-selection and program selection. In particular, the founder-team differences align with experimental evidence showing that early-stage investors respond strongly to information about founding teams. (Bernstein et al., 2017)

Table 2 shows that selection varies sharply across accelerator types. Corporate-affiliated accelerators enroll startups with substantially higher pre-entry funding, consistent with targeting more mature or investor-ready ventures. University- and government-affiliated programs enroll less-funded startups but a higher share of technically oriented teams. Equity-taking accelerators tend to sit between these extremes. These patterns reinforce that "accelerator" is not a single treatment: program models differ in screening, incentives, and networks, and these differences shape both who enters and what outcomes we observe. This type-level sorting parallels patterns in venture capital where investor styles, contracting intensity, and networks vary systematically and influence both matching and outcomes (Hochberg et al., 2007; Kaplan and Strömberg, 2003).

Sorting across accelerators is also consistent with an affiliation market: founders may accept lower direct terms (or invest time and effort) to affiliate with high-status programs if affiliation relaxes information frictions and improves access to capital. This logic parallels evidence that entrepreneurs value high-reputation financial affiliation as a credential that shapes investor beliefs and access to financing (Hsu, 2004). Because investor networks are a key determinant of follow-on financing and exit outcomes, differences in program networks are a natural mechanism for heterogeneous effectiveness.

Outcome variables in Table 1 reveal large unconditional differences between accelerated and non-accelerated startups. Among non-accelerated startups, mean post-accelerator funding is \$0.20 million and only 8 percent ever receive any venture financing. Accelerated startups raise \$0.53 million more in post-accelerator funding on average and are 29 percentage points more likely to receive any lifetime funding, with corporate-affiliated programs showing the largest gap (\$1.27 million; 36 percentage points). Exit patterns also differ markedly: accelerated startups are 5.1 percentage points more likely to achieve a successful acquisition (relative to a base rate of 4.5 percent) but also 5.0 percentage points more likely to close (base rate 8.0 percent), while IPO rates are slightly lower (-0.24 percentage points). Equity-taking accelerators drive the sharpest contrasts, with acquisition and closure rates 7.1 and 9.1 percentage points higher, respectively.

These selection patterns are also reflected in the lifecycle trajectories of accelerated startups. Using PitchBook data, Appendix Figure B.1 documents that accelerated startups are more likely to progress to Series A and later rounds, and raise larger amounts, on average, at post-Series B stages relative to non-accelerated counterparts—while early-stage deal sizes are largely indistinguishable. This correlation across the startup lifecycle is consistent with the selection patterns documented above, though whether these results are driven by program treatment or selection deserves further investigation.

Taken together, the evidence in this section establishes that accelerator entry and program choice are strongly patterned by observables related to startup quality and capital-market visibility. This makes selection a first-order issue for any attempt to measure accelerator effects. Section 5 therefore estimates accelerator value added using a value-added framework that (i) conditions on these predetermined covariates, (ii) treats each accelerator as a repeated-cohort production unit, and (iii) constructs AVA as a leave-cohort-out forecast of impacts on short-run post-entry fundraising—an outcome that is frequent and economically central given staged financing and financing risk. If accelerators improve the speed of learning under uncertainty, we should see effects not only on fundraising but also on the timing of both exit and shutdown—an implication of entrepreneurship-as-experimentation models. (Kerr et al., 2014).

5 Accelerator value added: Estimation and validation

Tables 1 and 2 show entrepreneurs systematically select into and sort across different accelerators. The central challenge is therefore isolating accelerator treatment effects from selection. A closely related problem arises when comparing entrepreneurial finance intermediaries: high-performing VCs or accelerators may appear to "cause" better outcomes because they match with stronger ventures. In education, observed differences across teachers likewise reflect both sorting and true effects. To address this challenge, we adapt the value-added framework developed by (Chetty et al., 2014a,b)

The key insight in CFR is to treat value added as a forecast object: a teacher's estimated impact based on prior cohorts should predict outcomes for new students in subsequent cohorts, conditional on observables, and should remain stable out of sample. Test scores are a noisy metric, but they are widely observed and plausibly closer to teacher inputs than long-run outcomes; CFR show that value-added forecasts also predict downstream outcomes such as adult earnings.

This framework offers a natural analogy for accelerators (see Figure 2). Accelerators provide structured mentoring and investor access over a fixed program duration, but participation is highly selective. In entrepreneurial finance terms, accelerators are intermediaries that could affect outcomes through certification, coaching and business development, and network access (Hegde and Tumlinson, 2014; Hochberg et al., 2007; Hsu, 2004; Kaplan et al., 2009). The challenge is to distinguish these channels from selection. We therefore extract signals of accelerator effectiveness from a high-frequency outcome—short-run post-entry fundraising—and then test whether the resulting measure predicts long-run outcomes such as exits, survival, and operating performance.

We proceed in four steps. First, we define accelerator value added (AVA) as a leave-cohort-out forecast of an accelerator's causal effect on short-run post-entry fundraising, measured within a fixed window (two years) after program entry. The two-year fundraising window is a natural target because early-stage entrepreneurship is staged experimentation subject to financing risk; near-term access to capital governs continuation. Second, we describe the estimation pipeline: residualizing outcomes by pre-entry covariates ("lagged-score" analogs), aggregating to accelerator–cohort signals, and applying empirical-Bayes shrinkage that accounts for cohort size and drift in program effectiveness. Third, we evaluate AVA using out-of-sample forecast bias tests. Fourth, we decompose cross-accelerator differences in outcomes into selection versus value added and connect AVA to longer-run outcomes in Section 6.

5.1 Estimating accelerator value added: setup

Let startup i participate in accelerator j in cohort year t. Let μ_{jt} denote accelerator j's cohort-specific value added in year t. We consider every startup that entered accelerators in year t as same cohort. This maps directly to the education setting in CFR, with startups replacing students and accelerator–cohorts replacing teacher–classes. For expositional simplicity, we first describe the model under equal cohort sizes; our implementation adjusts for cohort size heterogeneity following CFR (see Appendix A of Chetty et al. (2014a)).

Let A^*_{it} denote startup i's post-entry fundraising within two years after joining accelerator j in cohort year t. We model

$$A^*_{it} = \mu_{jt} + X'_{it}\beta + \varepsilon_{it}, \tag{1}$$

where X_{it} is a vector of predetermined startup characteristics such as pre-entry funding history, founder background, and startup industry and geography. The term μ_{jt} captures the accelerator-cohort effect on short-run fundraising, and ε_{it} captures idiosyncratic shocks. We normalize the coefficient on μ_{jt} to unity in this step because value added is measured in the same units as the residualized outcome. Our objective is to construct a forecast of μ_{jt} that predicts outcomes for future cohorts, not to treat each cohort's fixed effect as a precise measure of quality.

While one could estimate μ_{jt} directly using accelerator-by-cohort fixed effects, such estimates rely on outcomes from a single cohort and are noisy, especially when cohort sizes are small. Moreover, accelerator effectiveness may drift over time due to partner turnover, evolving mentor networks, or program redesign. Estimating separate cohort effects therefore risks capturing transitory fluctuations rather than persistent program quality. Following Chetty et al. (2014a), we pool information across cohorts of the same accelerator to form a leave-cohort-out prediction of current cohort effectiveness that optimally trades off signal and noise.

5.2 Estimation procedure

We estimate AVA in three steps—residualization, cohort aggregation, and leave-cohort-out forecasting—mirroring CFR's empirical Bayes/best-linear-predictor construction.

We first remove predictable components of post-accelerator short-term performance by residualizing post-accelerator two-year venture funding with respect to startup characteristics:

$$A_{it} = A^*_{it} - X'_{it}\widehat{\beta} = \mu_{jt} + \varepsilon_{it}, \tag{2}$$

where $\widehat{\beta}$ are obtained by regressing post-accelerator two year venture funding A^*_{it} on startup characteristics X_{it} and accelerator fixed effects δ_j so that

$$A^*_{it} = \widehat{\delta}_j + X'_{it}\widehat{\beta} + \widehat{r_{it}} \tag{3}$$

We estimate $\widehat{\beta}$ after controlling for accelerator fixed effects $\widehat{\delta}_j$. This within-accelerator estimation prevents the control variables X_{it} from absorbing variation attributable to accelerator quality. Intuitively, A_{it} is the part of post-entry fundraising not explained by predetermined startup quality.

Accelerator VA and startup performance is assumed to follow a stationary process such that $\mathbb{E}[\mu_{jt} \mid t] = \mathbb{E}[\varepsilon_{it} \mid t] = 0$, $Cov(\mu_{jt}, \mu_{j,t+s}) = \sigma_{\mu s}$, and $Cov(\varepsilon_{it}, \varepsilon_{i,t+s}) = \sigma_{\varepsilon s}$ for all t.

Then in the next step, we construct a leave-year-out measure of accelerator value added by predicting the accelerator value added in the current period using the accelerator value added in the previous periods. This is equivalent to estimating the linear projection of current cohort outcomes on past cohort outcomes. Let $\overline{A}_{jt}$ denote the cohort-level average residual post-accelerator venture funding for accelerator j's cohort in year t, defined as $\overline{A}_{jt} = \frac{1}{n}\sum_{i\in\{i:j(i,t)=j\}} A_{it}$ and let $A_j^{-t} = (\overline{A}_{j1}, \cdots, \overline{A}_{j,t-1}, \overline{A}_{j,t+1}, \cdots, \overline{A}_{j,T})'$ represent the vector of mean residual venture funding before cohort year t prior to or after year t. We approximate the conditional expectation $\mathbb{E}[\mu_{jt} \mid A_j^{-t}]$ using a linear predictor,

$$\widehat{\mu_{jt}} = \sum_{s\neq t} \psi_s \overline{A}_{js} \tag{4}$$

where the weights ψ are obtained by minimizing the mean squared prediction error.

$$(\widehat{\psi_1}, \cdots, \widehat{\psi_{t-1}}, \widehat{\psi_{t+1}}, \cdots, \widehat{\psi_T}) = \underset{\psi_1,\ldots,\psi_{t-1},\psi_{t+1},\cdots,\psi_T}{\arg\min} \sum_j \left(\overline{A}_{jt} - \sum_{s\neq t}\psi_s \overline{A}_{js}\right)^2 \tag{5}$$

By construction, the resulting value added estimates are leave-year-out measures of accelerator quality, which avoids mechanical bias arising from using the same cohort outcomes both to construct and to evaluate accelerator value added.

5.3 Validation I: forecast bias (calibration)

A central validity check in the value-added literature is whether estimated value added predicts outcomes for new cohorts out of sample. Following Chetty et al. (2014a), we test forecast unbiasedness by regressing residualized outcomes on predicted AVA constructed from other cohorts:

$$A_{it} = \delta_t + \lambda \widehat{\mu}_{jt} + \xi_{it}, \tag{6}$$

where A_{it} denotes residualized outcomes and $\widehat{\mu}_{jt}$ is the predicted accelerator value added. If $\widehat{\mu}_{jt}$ is an unbiased forecast of the true accelerator effect, then $\lambda = 1$.

Under random assignment of startups in year t, we have $\mathbb{E}[\varepsilon_{it} \mid \widehat{\mu}_{jt}] = 0$. Combining Equations 1 and 6, the coefficient λ can be written as

$$\lambda = \frac{\mathrm{Cov}(A_{it}, \widehat{\mu}_{jt})}{\mathrm{Var}(\widehat{\mu}_{jt})} = \frac{\mathrm{Cov}(\mu_{jt}, \widehat{\mu}_{jt})}{\mathrm{Var}(\widehat{\mu}_{jt})}. \tag{7}$$

We define forecast bias as

$$B(\widehat{\mu}_{jt}) \equiv 1 - \lambda = -\frac{\mathrm{Cov}(\varepsilon_{it}, \widehat{\mu}_{jt})}{\mathrm{Var}(\widehat{\mu}_{jt})}, \tag{8}$$

where ε_{it} denotes unobserved startup-level determinants of outcomes from Equation 1.

Forecast unbiasedness requires that, conditional on observed covariates, the remaining unobserved heterogeneity is not systematically correlated with estimated value added. Random assignment is sufficient but not necessary for forecast unbiasedness.

5.4 Validation II: accelerator-level bias

Forecast unbiasedness does not fully rule out sorting or selection into accelerators. Even if the estimates are forecast unbiased, accelerator-level value added may still be biased. This distinction between forecast bias and deeper selection-driven bias is central in debates over value-added models (Rothstein, 2017), and it is especially relevant here because accelerator participation involves two-sided selection by founders and programs.

We view forecast unbiasedness as an informative and stringent criterion for model validity, and evaluate this condition in the construction of accelerator value-added in the sections that follow. First, even if accelerator value-added estimates contain accelerator-level bias, the predictive power of value-added that is forecast unbiased can still be highly informative for founders and investors. Second, any accelerator-level bias must satisfy a highly restrictive structure when value-added estimates are forecast unbiased: accelerators with higher true value-added must be biased downward, while those with lower true value-added must be biased upward, with magnitudes precisely aligned so that the biases cancel in the aggregate. Such an exact negative relationship between true quality and estimation bias constitutes a knife-edge correlation structure (see Appendix B of Chetty et al. (2014a)). Absent this highly restrictive configuration, sorting or selection on unobservables would necessarily generate detectable forecast bias.

To illustrate this point, let $\widehat{\mu}_j^{VA} \equiv \lim_{t\to\infty} \widehat{\mu}_{jt}$ denote the probability limit of the estimated value added for accelerator j as the number of cohorts grows large, and let μ_j denote the true accelerator value added. The asymptotic accelerator-level bias in the VA estimate is defined as:

$$\widehat{\mu}_j^{VA} = \mu_j + \omega_j, \tag{9}$$

so that the value added estimate is unbiased at the accelerator level if $\mathrm{Var}(\omega_j) = 0$. It follows from the definition of forecast bias that if the value added estimate $\widehat{\mu}_j^{\mathrm{VA}}$ is forecast unbiased,

$$\begin{aligned} 1 - B(\widehat{\mu}_j^{VA}) = 1 - 0 &= \frac{\mathrm{Cov}(A_{it}, \widehat{\mu}_j^{\mathrm{VA}})}{\mathrm{Var}(\widehat{\mu}_j^{\mathrm{VA}})} \\ &= \frac{\mathrm{Cov}(\mu_j + \varepsilon_{it}, \mu_j + \omega_j)}{\mathrm{Var}(\widehat{\mu}_j^{\mathrm{VA}})} \\ &= \frac{\mathrm{Var}(\mu_j) + \mathrm{Cov}(\mu_j, \omega_j)}{\mathrm{Var}(\mu_j) + \mathrm{Var}(\omega_j) + 2\mathrm{Cov}(\mu_j, \omega_j)}. \end{aligned} \tag{10}$$

This expression implies that $\mathrm{Var}(\omega_j) = -\mathrm{Cov}(\mu_j, \omega_j)$. Therefore, if there is accelerator-level bias ($\mathrm{Var}(\omega_j) > 0$), it must be negatively correlated with the true value added μ_j ($\mathrm{Cov}(\mu_j, \omega_j) < 0$).

In Section 6, we will directly tackle accelerator-level bias using a finalist sample who were almost admitted to a prominent nonequity accelerator. The knife-edge structure of the accelerator-level bias provides a useful foundation for constructing bias-corrected estimators and for conducting meta-analyses in future research.

5.5 Empirical procedure

We first construct a sample of non-accelerated startups that are comparable to accelerated startups. We then describe our choice of outcome variables and then explains the estimation of the model.

Constructing a sample of non-accelerated startups. In the original teacher value-added model, the primary goal is to identify effective teachers. The relevant counterfactuals concern assignment to different teachers, conditional on attending the same school. In the accelerator setting, however, the key counterfactual is not just assignment to different accelerators, but whether a startup goes through an accelerator at all. This difference also makes our setting harder than the teacher case: startups sort directly into the institution whose value added we estimate, whereas students typically sort into schools first and only indirectly into teachers. To estimate accelerator value added, we therefore need to construct an appropriate comparison group of 'self-taught,' non-accelerated startups that were plausibly at risk of being accelerated.

We construct a comparison sample of non-accelerated startups through matching. In our dataset of approximately 0.7 million startups, about 38,000 participate in accelerators. The remaining startups form a highly heterogeneous group: some may have been unable to enter an accelerator, while others may not have sought accelerator participation in the first place. Directly comparing accelerated and non-accelerated startups would therefore conflate accelerator effects with differences in underlying startup quality. By matching accelerated startups to observably similar non-accelerated startups, we construct a comparison group that more closely approximates the counterfactual outcomes of accelerated firms in the absence of accelerator participation.

To construct a comparable control group, we first assign each non-accelerated startup to a block defined by headquarters state, founding year, and industry sector (PitchBook industry classification with six broad categories). Within each block, we compute the average accelerator entry year among accelerated startups and assign the largest integer less than or equal to this average as a pseudo accelerator entry year for non-accelerated startups. This procedure aligns non-accelerated startups with accelerated startups in terms of geographic location, cohort timing, and industry composition.

We then implement entropy balancing, which reweights non-accelerated startups so that the means of key early-stage characteristics exactly match those of accelerated startups (Hainmueller, 2012). Specifically, we balance on early-stage funding, founder characteristics (number of founders, serial entrepreneurship, gender composition, MBA background, technical background, immigrant status, university quality, and experience), early patenting activity, and fixed effects for industry group (40 categories), state, and founding year. Entropy balancing preserves all treated observations while assigning weights to control observations that minimize deviation from the original sample subject to these exact balance constraints.

Accelerated startups are assigned an entropy weight of one, while non-accelerated startups receive continuous entropy weights $w_i \geq 0$. To implement these weights in estimation routines that do not support probability weights, we convert them into integer replication counts using a stochastic rounding procedure. Let N_T denote the number of accelerated startups. We scale the weights such that the total weight of the control group equals N_T and define the implied replication count for non-accelerated startup i as

$$x_i = N_T \cdot \frac{w_i}{\sum_{j \in C} w_j}, \tag{11}$$

where C denotes the set of non-accelerated startups. We then decompose x_i into its integer and fractional components,

$$x_i = k_i + r_i, \tag{12}$$

where $k_i = \lfloor x_i \rfloor$ and $r_i \in [0, 1)$.

Each non-accelerated startup is deterministically replicated k_i times. In addition, it is replicated once more with probability r_i, implemented through an independent Bernoulli draw. This stochastic rounding step ensures that, in expectation, the total number of control observations equals the number of treated observations and that the weighted distribution of covariates in the control group matches that of the treated group. Observations with larger fractional weights are therefore more likely to be replicated, while those with small weights are less likely to appear in the expanded sample. After applying this procedure, we expand the dataset according to the resulting replication counts and drop control observations with zero weight. In Figure B.4, we examine the robustness of the forecast-unbiasedness result to different seeds used in the stochastic rounding step.

Balance diagnostics reported in Table 2 confirm that this procedure yields a narrow matched subsample of non-accelerated startups whose joint distribution of observable characteristics closely mirrors that of accelerated startups.

Finally, for startups associated with multiple accelerators, we treat each affiliation as a separate observation in our dataset. Pre- and post-accelerator funding amounts are adjusted relative to the specific entry year of each respective program. To accurately isolate the 'value-add' of each affiliation, we calculate subsequent funding by taking into account the aggregate of all funding received prior to that specific accelerator's start date. This approach allows us to control for entry at different points in time. Data on multiple accelerator is given in Section A.8.

Outcome variable. To estimate value added, we must first choose a reasonable short-term outcome from which we can extract a signal of accelerator quality. Table 3 assesses the forecast unbiasedness of alternative accelerator value-added (VA) measures constructed under different first-stage specifications. Across columns (1)–(4), we estimate the same forecast regression that relates realized post-accelerator funding outcomes to pre-estimated accelerator VA; the only difference across columns is how the VA measure is constructed in the first stage. Following Chetty et al. (2014a), forecast unbiasedness is evaluated by testing whether the coefficient on accelerator VA is close to one.

The results show that baseline VA measures that control only for industry and founding year (column 1) understate true accelerator effects, yielding coefficients well below one. In contrast, once early-stage funding and standard fixed effects are incorporated into the VA estimation stage (columns 3 and 4), the forecast coefficient approaches unity, indicating that these VA measures provide approximately unbiased forecasts of accelerator quality. This finding implies that controlling for prior funding and fixed effects is sufficient to eliminate systematic bias in the VA estimates.

Based on this validation exercise, our baseline specification adopts the VA measure from column (3), which controls for early-stage funding and fixed effects but excludes founder characteristics. This choice balances forecast accuracy with parsimony and avoids conditioning the VA measure on potentially endogenous founder attributes.

Another issue concerns the time horizon over which early-stage financing is measured. Table B.1 shows that the forecast-unbiasedness condition is satisfied when venture funding is measured over the first two to five years after accelerator entry, but not when it is measured over only the first year. One possible explanation for this failure in the first year is the timing of accelerator demo days. Some accelerators admit cohorts in the winter, such that firms graduate the following year and may have limited opportunity to raise new funding before the calendar year ends. As a result, funding outcomes in the first year after accelerator entry may mechanically understate post-accelerator financing activity. We therefore adopt a two-year horizon as our baseline measure of early-stage financing. This choice reflects our interest in assessing whether short-term outcomes can serve as informative predictors of the long-term effects of accelerators on startup performance.

Using a two-year horizon as the baseline outcome introduces an additional sample restriction. Because our data are truncated in 2024, startups that enter an accelerator in 2023 or 2024 do not have a full two-year window in which to observe post-accelerator outcomes. For these cohorts, the value-added measure would be driven largely by early-stage funding rather than by realized post-accelerator performance. To avoid this mechanical bias, we exclude startups that entered an accelerator in 2023 or 2024 from the estimation sample.

We take the logarithm of both pre- and post-accelerator venture funding to mitigate skewness. Because taking logs of zero is undefined, we add a small constant before transformation. However, this introduces another challenge, as an arbitrary choice of constant in $\ln(x + c)$ can influence the results (Chen and Roth, 2024). To address this, we add \$150,000, the typical size of pre-seed funding provided by accelerators (Santamaria and Breschi, 2025), to all funding values before taking logs. This choice anchors the transformation in a meaningful benchmark while minimizing sensitivity to arbitrary scaling.

In addition, we exclude accelerator deal funding from both pre- and post-accelerator financing amounts. Some accelerators provide funding as part of the accelerator deal, while others do not. This is to ensure that our measure of post-accelerator financing does not mechanically reflect the receipt of cash from the accelerator itself.

For completeness, we include all sources of external financing (grants, angel investment, and venture capital) in constructing our funding measures. As shown in Table A.4, venture capital accounts for the vast majority of total funding, with grants and angel investment playing a relatively minor role on average.

We use the `vam` package in Stata (Stepner, 2013) to estimate accelerator value added and to quantify how much each accelerator contributes to startup outcomes. As has been explained in Section 5.1, the model allows for correlation in value added across different cohorts within the same accelerator (Chetty et al., 2014a). As illustrated in Figure B.2, the correlation declines over time and nears zero after nine years. Consequently, we allow for drift in accelerator value-added for a nine-year period, assuming the value-added remains constant at the Year 9 level thereafter.

5.6 Validation of the value added model

Figure 5 displays the distribution of accelerator value added. We construct this distribution by averaging estimated value added across cohorts for each accelerator. Approximately 80 percent of accelerators exhibit negative value added relative to the no-accelerator benchmark. We also estimate the model with early-stage funding in 3 years and 5 years in Figure B.6 and the results do not change. Because the number of startups varies across accelerators, we also plot the same histogram weighted by the number of startups in each accelerator in Figure B.7. 65 percent of startups joined value destroying accelerators.

Following PitchBook (2023), we identify a set of prominent accelerators and highlight their estimated value added in the figure. In addition to those listed by PitchBook, we also annotate several accelerators selected based on their relevance to our analysis. Although the value-added estimates are expressed in log funding terms, we translate them into funding levels by exponentiating the estimates and scaling them by mean post-accelerator funding reported in Table 1.

As discussed previously, the core novelty of our approach is to document this heterogeneity across accelerators using rigorous methods originally developed in a different context. This framework advances our understanding of accelerators by revealing substantial cross-accelerator variation and provides a basis for linking short-term signals to long-term outcomes.

However, a key empirical concern with the value added model is whether the estimated value-added reflects the causal impact of accelerators or merely the results of selection and sorting (Chetty et al., 2017; Rothstein, 2017). We address this issue by validating the teacher value-added framework through multiple exercises. We briefly outline each validation exercise here, with more exhaustive details and robustness checks reserved for the Appendix when necessary.

Evaluating forecast bias. Panel (a) of Figure 3 illustrates a tight relationship between realized early-stage financing and estimated accelerator value added. This pattern is consistent with the results reported in Table 3, which show that the value-added estimates are strongly predictive of subsequent funding outcomes (forecast unbiasedness).

We next examine whether sorting on observable pre-accelerator characteristics contributes to forecast bias. We consider two potential sources of such bias: the startup's own pre-accelerator funding growth rate and the early-stage financing of its accelerator classmates.

First, a startup's funding growth rate prior to accelerator entry may reflect unobserved momentum or quality that is correlated with both accelerator selection and post-accelerator outcomes. If startups with stronger funding trajectories systematically sort into higher value-added accelerators, this pre-trend could bias the value-added estimates. Following Rothstein (2017), we residualize the startup's pre-accelerator funding growth rate with respect to baseline controls and accelerator fixed effects, regress the residualized outcome on this measure, and compute the implied predicted component. We then regress this predicted component on accelerator value added. Panel (a) of Figure 4 shows a nearly flat relationship (the estimated coefficient is 0.002 while the standard error is 0.002), indicating that sorting on pre-accelerator funding momentum does not generate meaningful forecast bias.[2]

[2]Table B.2 confirms the robustness of our baseline results by including the startup's pre-accelerator funding growth rate as an additional control variable. The estimated accelerator value-added (AVA) remains stable in both magnitude and statistical significance, suggesting that our measures are not merely picking up unobserved pre-existing trends in startup performance.

Second, we examine whether sorting based on peers' early-stage financing contributes to forecast bias. The baseline specification controls only for the focal startup's own early-stage funding. However, the average early-stage funding of a startup's accelerator classmates (i.e., firms in the same accelerator–cohort) is not explicitly controlled for and may capture additional dimensions of selection into particular accelerators. Specifically, we construct the classmate mean of early-stage funding, defined as the average amount of financing raised by a startup's cohort peers prior to accelerator entry, excluding the focal startup itself. We apply the same residualization procedure as above and regress the predicted component on accelerator value added. Panel (b) of Figure 4 shows a nearly flat relationship (the estimated coefficient is -0.005 while the standard error is 0.003), indicating that this type of accelerator-level selection does not explain forecast bias either.

Accelerator-level bias at a nonequity accelerator We compare the estimated accelerator value added from the teacher value-added model with quasi-experimental estimates from a prominent nonequity accelerator for which we observe the full universe of applicants, their evaluation scores, and graduation outcomes. Using this subsample, we estimate the causal impact of the accelerator by comparing admitted startups with runner-up finalists who advanced to the final interview stage. The resulting estimates closely match the value-added estimates obtained from the teacher value-added framework, providing further validation of the model. We also provide a systematic framework for updating VA estimates in the presence of accelerator-level bias, drawing on the core logic of the value-added model. We revisit this setting in greater detail in Section 6.

Quantifying omitted variable bias. Whenever we estimate the impact of accelerator value added on startup outcomes, we report Oster bounds to assess sensitivity to unobserved confounders (Oster, 2019). While this approach cannot definitively establish that accelerator value added is a clean causal estimate net of selection, it allows us to quantify how strong unobserved factors would need to be to overturn our main results.

6 Evidence from Accelerator-*X*

Applicant-based validation The value-added approach is designed to be informative in settings with strong selection, but it remains possible that AVA partly captures residual sorting on unobservables. This concern is central in entrepreneurial finance: differences in outcomes across intermediaries can reflect both deal flow (sorting) and influence (value creation). We therefore complement the market-wide analysis with applicant-level evidence from Accelerator-X, which allows us to compare outcomes for admitted startups to outcomes for observationally similar finalists who were not admitted.

6.1 Data description

Data We collected internal records on all startups that applied to Accelerator-X between 2018 and 2023. The data include applicant characteristics, evaluation scores (both at the application stage and, for a subset, at the interview stage), and admission outcomes. We link applicants to post-application outcomes in the combined startup panel used in the main analysis. Accelerator-X is a large U.S. non-equity accelerator; to preserve confidentiality, we suppress program-identifying details beyond its broad focus and scale. Because the data contain rich evaluation information, they allow a sharper comparison than "accelerated vs. not" in the population data.

6.2 Identification strategy

Our primary validation compares admitted startups to non-admitted startups within a narrow pool of finalists. Specifically, we restrict attention to startups that reached the interview stage (or finalist stage) and estimate the effect of admission conditional on evaluation information observed by the program. The identifying assumption is not full random assignment; rather, admission is plausibly quasi-random at the margin conditional on a rich set of scores and observables. We treat this as a "runner-up" design: among startups that cleared similar screening hurdles and received similar evaluations, small differences in admission decisions provide quasi-experimental variation in program participation.

This applicant-based comparison directly targets the concern that high-AVA programs merely select better ventures.[3] It holds deal flow approximately fixed—at least on observables and evaluation information—and asks whether program admission shifts outcomes. This is complementary to forecast tests in the value-added framework and provides a bridge from market-wide heterogeneity measurement to quasi-experimental validation.

Estimation We estimate admission effects using:

[3] We are unable to implement a regression discontinuity design in this setting as Accelerator-X does not use a mechanical score cutoff for admission. Instead, borderline applicants are admitted or rejected based on committee deliberations which take into account factors not directly linked to startup success like cohort balance across industry sub-fields, and founder background Since the assignment mechanism is not discontinuous in the score, neither sharp nor fuzzy RD designs are feasible. Even so, the comparison between admitted startups and strong applicants, especially the finalists, provides credible quasi-experimental variation, because these groups are very similar in observable characteristics but differ in whether they received the accelerator treatment.

$$Y_i = \alpha + \tau Admit_i + f(S_i) + X_i'\gamma + \varepsilon_i \quad (13)$$

where Y_i is a post-application outcome (e.g., two-year post-entry fundraising, acquisition, closure), $Admit_i$ indicates admission, S_i denotes the vector of evaluation scores, and X_i includes predetermined covariates. Standard errors are robust to heteroskedasticity and clustered at the cohort level where appropriate. We view τ as a local estimate within the finalist margin that benchmarks our market-wide AVA estimates.

6.3 Balance tests

We first test whether admitted and non-admitted finalists differ systematically in predetermined characteristics and pre-trends. We examine differences in founder background, pre-application funding history, and other observable predictors of success, and we verify that admitted and non-admitted finalists have similar predicted outcomes based on the evaluation information available at the time of decision. These balance checks support the interpretation that, within the finalist pool, admission is not primarily sorting on observables (Bernstein et al., 2017).

Table 5 reports balance tests for these comparisons. Consistent with the idea that finalists closely resemble admitted startups, we find no significant differences in early-stage venture funding, prior accelerator affiliations, or earlier accelerator participation. This similarity is present both when comparing admitted startups with all applicants, and when focusing only on finalists. These results support the credibility of our empirical approach, since admission decisions among top applicants appear to have a large idiosyncratic component.

6.4 Results

Table 6 reports the estimated effects of joining Accelerator-X on post-accelerator funding outcomes. Across specifications, Accelerator-X graduates raise substantially more venture funding after the accelerator than comparable non-admitted startups. The indicator Accelerator-X graduate equals one for startups that were admitted to and completed the program.

The first-round evaluation score is captured by the application score, while the second-round assessment is measured by the interview score. Both scores range from 1 to 7. Columns (1)–(3) show that the interview score is a stronger predictor of subsequent venture funding than the application score, consistent with the idea that evaluators obtain more precise information about startup quality during in-person interviews.

In Column (4), once actual admission to Accelerator X is included, neither score remains a significant predictor of post-accelerator funding. This suggests that the information contained in the scores is largely absorbed by the admission decision itself.

Columns (6)–(8) further restrict the sample to startups that reached the interview stage and progressively add early-stage funding and founder characteristics as controls. The estimated effect of joining accelerator X remains positive and statistically significant across these specifications. In the most relevant comparison, which restricts the sample to interviewed finalists and includes the full set of covariates (Column 8), joining Accelerator X is associated with an increase of about 0.39 log points in venture funding within two years after the accelerator. Relative to the control group mean of $0.65 million, this corresponds to a large and economically meaningful increase in early-stage financing.

The applicant evidence is consistent with multiple entrepreneurial finance channels. If accelerators primarily provide certification, we would expect a concentrated effect on near-term fundraising with weaker mapping into long-run operating outcomes. If accelerators operate through networks and governance-like support, we would expect more persistent effects on exits and scale. Our evidence favors the latter interpretation: the admission effect is not confined to an immediate funding bump and is accompanied by differences in longer-run outcomes.

Bias correction. To estimate the value added of the Accelerator-X program, we use two different approaches and compare the implied dollar effects. In the teacher value-added model (Table 4), we regress log post-accelerator funding on a binary indicator for Accelerator X participation (1 = joined Accelerator X; 0 = non-accelerated startups). Standard errors are clustered at the accelerator-cohort level. This yields an estimated effect of $\hat{\beta}_X^{\mathrm{VA}} = 0.09$ log points (s.e. = 0.01). The mean post-accelerator funding in the non-Accelerator X group is $2.55 million. In a separate Accelerator X-specific sample (Table 6), the estimated effect is $\hat{\beta}_X^{\mathrm{Q}} = 0.39$ log points (s.e. = 0.19), with a non-Accelerator X group mean of $0.65 million (Superscripts $^{\mathrm{VA}}$ and $^{\mathrm{Q}}$ indicate that they are derived from the VA model and quasi-experimental setting).

To estimate the dollar value of the treatment effects, we apply the delta method to convert log point estimates into levels. For the VA model, the estimated effect of 0.09 log points (s.e. = 0.01) translates to approximately $240,000 (s.e. = $28,000), using a baseline mean of $2.55 million in the non-accelerator group. For the Accelerator X sample, the effect of 0.39 log points (s.e. = 0.19) corresponds to approximately $309,000 (s.e. = $182,000), using a baseline mean of $0.65 million.

We test the null hypothesis that the two dollar effects are equal:

$$H_0 : \hat{\beta}_X^{\text{VA}} = \hat{\beta}_X^{\text{Q}}.$$

The difference is $\hat{\beta}_X^{\text{VA}} - \hat{\beta}_X^{\text{Q}} \approx -\$69{,}000$. The standard error of the difference is

$$\text{s.e.}(\hat{\beta}_X^{\text{VA}} - \hat{\beta}_X^{\text{Q}}) = \sqrt{(\text{s.e.}(\hat{\beta}_X^{\text{VA}}))^2 + (\text{s.e.}(\hat{\beta}_X^{\text{Q}}))^2} \approx \$184{,}000.$$

The t-statistic is approximately -0.37, which corresponds to a two-sided p-value of 0.71. We therefore fail to reject the null hypothesis that the two methods produce statistically similar dollar effects.

Yet, it is also possible that external benchmarks based on other accelerator samples may not perfectly align with our value-added (VA) estimates. In Appendix B.5, we therefore adopt a framework of systematic belief updating. Rather than treating each applicant-based benchmark as a singular test of "truth," we interpret it as an informative signal that refines prior beliefs about accelerator effects. In this framework, benchmarks discipline the prior in the value-added model instead of prompting case-by-case comparisons. This approach allows us to quantify how external evidence should recalibrate initial VA estimates.

7 Impact on startup outcomes

This section asks whether accelerator quality—measured by AVA as a forecast of impacts on two-year post-entry fundraising—predicts longer-run outcomes that matter in entrepreneurial finance: successful exits, survival, and real measures of scale. The goal is not to treat fundraising as "success," but to use it as a high-frequency signal and then test whether that signal maps into downstream outcomes. This distinction helps separate certification/attention stories (short-run effects only) from mechanisms that change fundamentals, such as network access, governance, and faster learning under staged financing (Hochberg et al., 2007; Hsu, 2004; Kaplan and Strömberg, 2003; Kerr et al., 2014).

Impact on funding. Table 4 examines the relationship between accelerator value added and subsequent startup financing outcomes. All value-added (VA) measures on the right-hand side are constructed using early-stage funding observed within two years after accelerator entry. The first outcome therefore provides a direct test of forecast unbiasedness: if accelerator VA correctly summarizes expected early-stage funding, the coefficient should be close to one.

Panel A shows that this condition is satisfied. When early-stage funding two years after accelerator entry is regressed on accelerator VA, the estimated coefficient is statistically indistinguishable from one. This result indicates that the VA measure is forecast-unbiased with respect to the outcome used in its construction. At longer horizons, the coefficients are no longer mechanically constrained to equal one. Instead, the estimates in Columns (3) and (4) show that accelerator VA continues to strongly predict early-stage funding three years after entry, with coefficients exceeding one, indicating substantial persistence and amplification of accelerator effects over time.

Panel B extends the analysis to more distant and distinct outcomes. Accelerator VA remains a strong predictor of early-stage funding four and five years after entry, as well as of financing accumulated at any point after accelerator participation. Although these coefficients are not mechanically tied to unity, they remain large and precisely estimated. This pattern indicates that short-run accelerator performance contains meaningful information about longer-run startup financing trajectories.

To address concerns about omitted variable bias, we calculate the Oster (2019) delta, which measures the stability of the coefficient when additional control variables are included. If the coefficient remains stable while the R-squared increases, it suggests limited scope for omitted variable bias. A common rule of thumb is that if the absolute value of Oster's delta exceeds one, unobservables would need to be more strongly correlated with both accelerator VA and the outcome than the included controls in order to explain away the estimated effect. In all specifications, the estimated delta far exceeds this threshold, indicating that the results are unlikely to be driven by omitted variables.

Table B.4 further illustrates the contribution of our value-added approach relative to prior work. Column (1) shows that the average effect of accelerator participation is statistically indistinguishable from zero. This suggests that simply being accelerated does not confer a measurable advantage on average. Column (2) reveals, however, that this null average masks substantial heterogeneity: startups that attend high value-added accelerators significantly outperform those that do not. We take the average of accelerator value added across cohorts and assigned an indicator variable for accelerators with higher value added than non-accelerated (self-taught) startups on average. This decomposition contrasts with existing research which typically estimates a single accelerator treatment dummy and interprets it as the effect of acceleration per se. Our framework instead identifies the marginal return to accelerator quality, the gain from attending a program whose value added is meaningfully above the counterfactual of no acceleration. The results suggest that what matters is not whether a startup is accelerated, but which accelerator it attends.

7.1 Exits and survival

Table 7 links accelerator value added to long-run binary outcomes. We continue to use AVA constructed from two-year post-entry fundraising as in Table 4, but now the dependent variables are longer-run outcomes: IPO, successful acquisition, and closure.

Panel A reports results for binary exit outcomes. Accelerator VA has a positive, but not a statistically significant, effect on IPO probability, but it is strongly positively associated with successful acquisitions. A one-unit increase in log VA is associated with a roughly 18 percentage point increase in the probability of a successful acquisition (Panel B), and this estimate is stable across specifications—moving from 18.4 in the baseline to 17.9 after adding both startup controls and lifetime funding. At the same time, accelerator VA is also positively associated with closure (Panel C), with estimates ranging from 9.7 to 15.1 percentage points. This pattern suggests that higher-value-added accelerators increase the likelihood of reaching a definitive outcome—either positive or negative—consistent with accelerating firm trajectories rather than uniformly preventing failure. Taken together, higher-AVA accelerators increase the likelihood that startups reach a definitive outcome—either positive (acquisition) or negative (closure)—consistent with accelerating trajectories rather than uniformly preventing failure.

This "accelerated resolution" pattern fits the view of entrepreneurship as staged experimentation under financing risk, where effective intermediaries can add value by speeding learning and capital reallocation—not only by increasing survival (Ewens et al., 2018; Kerr and Nanda, 2009; Kerr et al., 2014).

The inclusion of total lifetime funding in Column (3) leaves the key coefficients largely unchanged across all panels. This indicates that the long-run effects of accelerator value added are distinct, and go beyond their impact on facilitating access to venture capital.

To put these magnitudes into perspective, a one-standard-deviation increase in accelerator VA (0.09 log points) is associated with a 1.7 percentage point increase in the probability of a successful acquisition, relative to a base rate of 8.0 percent. Scaling further, a 0.2 log-point increase—roughly the boost from joining Y Combinator relative to no accelerator—implies a 3.7 percentage point increase. In comparison, prior research shows that a one-standard-deviation increase in H-1B worker access increases exits by 1.4 percentage points (Dimmock et al., 2022), while new airline routes between startups and VCs increase exits by 1 percentage point (Bernstein et al., 2016).

A concern is that closure could partly reflect mechanical shutdown following acquisition or IPO (e.g., acquired products discontinued). We therefore sharpen the interpretation by excluding IPOs—and in some specifications, excluding both IPOs and successful acquisitions—so that closure reflects failure rather than post-exit organizational changes (see Panel C). Once successful exits are excluded, accelerator VA remains strongly positively associated with closure, indicating that the closure effect is driven by startups that neither went public nor were successfully acquired. This reinforces the interpretation that higher-AVA accelerators compress the lifecycle by accelerating both success and failure. Such "up-or-out" dynamics are a natural implication of experimentation models where improved access to capital and information increases the speed of reallocation (Kerr and Nanda, 2009).

Panel D examines patenting outcomes. Accelerator VA exhibits no statistically meaningful relationship with patenting, with coefficients near zero and insignificant across specifications. This pattern is informative: accelerators that are most effective at increasing short-run fundraising and acquisition likelihood are not necessarily those that increase measured innovation output, consistent with accelerators operating primarily through financing and commercialization channels rather than directly increasing invention.

7.2 Employment, revenues and valuation

Table 8 examines whether exposure to higher-AVA accelerators predicts subsequent firm scale, using employment, revenue, and valuation measures. A key limitation is that these outcomes are observed only for a subset of firms and at irregular times; we therefore construct outcomes that are robust to uneven measurement timing. For each domain, Column (1) measures whether a startup exhibits post-entry growth (either by appearing in the data post-entry when previously missing, or by exceeding its early-stage value). Columns (2) and (3) summarize medium- and longer-run performance using the maximum value observed within three and ten years after accelerator entry. We include evaluation-year fixed effects to absorb differences in measurement timing.

Across all three of these outcomes, accelerator VA is positively and significantly associated with subsequent firm scale, consistent with high-AVA programs affecting trajectories beyond fundraising.

Evidence for entrepreneurial learning. Table 7 indicates that startups from higher-value-added accelerators are more likely to close. To better understand the mechanism behind this result, we examine how founders respond to intermediate performance signals. Unlike closure, which is a long-run outcome, employment, revenue, and valuation are realized earlier and can inform a founder's decision to continue or exit. If accelerators enhance learning, we would expect founders from higher-value-added programs to be more responsive to these intermediate signals—shutting down when performance is weak rather than persisting. To test this, Table 9 regresses an indicator for closure on value added interacted with each of these intermediate outcome variables, including the full set of startup-level controls and fixed effects from our earlier specifications. For each outcome, we consider three measures: an indicator for whether the outcome increases post-program (*Up*), the log maximum outcome within three years post-program (*Max3*), and the log maximum outcome within ten years post-program (*Max10*). The central focus of this analysis is the interaction term between accelerator value added and intermediate performance metrics. Across all specifications, the estimated interaction coefficient is negative. This indicates that startups from higher-value-added accelerators are less likely to close when they exhibit stronger intermediate outcomes. The flip side of this result is that when signals about business performance are weak, startups from these accelerators are more likely to close. This pattern is consistent with a learning mechanism: founders in higher-value-added accelerators appear to resolve uncertainty faster, shutting down underperforming ventures rather than persisting with them.

7.3 Timing and competing risks

Prior accelerator studies often find that accelerated startups close earlier than matched non-accelerated startups. We revisit this result using accelerator value added as the key explanatory variable and estimate Fine–Gray competing-risks models for closure and successful exit (IPO or acquisition). The competing-risks framework is natural here because closure and successful exit are mutually exclusive absorbing states: it lets us trace how accelerator quality shifts the cumulative incidence of each outcome over time. This focus on timing is motivated by experimentation under financing risk: intermediaries may create value by accelerating learning and reallocating capital earlier in the life cycle. We report subhazard ratios in Table 10 and plot implied cumulative incidence functions in Figure 6.

Our analysis proceeds in two steps. First, we estimate the cumulative incidence of business closure while treating IPOs and acquisitions as competing events. Second, we estimate the cumulative incidence of successful exit while treating closures as competing events. This framework explicitly accounts for the mutually exclusive nature of failure and successful exit and focuses on how accelerator quality shifts the probability of each outcome over time. All specifications include industry sector, accelerator-entry-year, and founding-year fixed effects, and standard errors are clustered at the accelerator–class level.

The subhazard ratios in Table 10 capture how accelerator value added affects the cumulative probability that a startup experiences closure or successful exit, conditional on the presence of competing risks. Accelerator value added is defined as the log improvement in post-accelerator funding attributable to the accelerator's treatment effect. A one-unit increase therefore corresponds to approximately a doubling of post-accelerator funding relative to the counterfactual.

Column (1) shows that higher accelerator value added is associated with an increased cumulative incidence of business closure, while Column (2) reveals a large and robust positive association between accelerator value added and the cumulative incidence of successful exit. Taken together, these results suggest that startups exposed to higher-value-added accelerators transition more rapidly into definitive outcomes. Rather than simply increasing failure risk, higher accelerator quality appears to accelerate selection, increasing the probability of both successful exits and closures over the observed horizon.

Because competing-risks models with high-dimensional fixed effects are computationally demanding, we implement a complementary approach using linear probability hazard models that allow us to include our full set of fixed effects and startup-level controls. Specifically, we estimate linear probability models in which the dependent variable equals one if a failure or a successful exit occurs within three, five, or seven years after founding. These specifications allow us to residualize outcomes and include our full set of fixed effects and startup-level controls.

The results from these linear probability models, reported in Table B.3, are qualitatively consistent with the Cox models. Accelerator value added is positively associated with both the probability of failure and the probability of successful exit over three-, five-, and seven-year horizons. For example, a one-unit increase in accelerator VA is associated with an increase of about 3 percentage points in the probability of failure within three years and about 5 percentage points within five years, while raising the probability of successful exit by roughly 5 percentage points within three years and more than 10 percentage points within five years. This robustness across modeling approaches suggests that the main finding does not depend on the particular functional form of the hazard model.

These results indicate that higher-quality accelerators compress the lifecycle of startups by accelerating both failure and success. Conditional on survival, startups in high-value-added accelerators are more likely to exit through IPO or acquisition; conditional on weak underlying prospects, they are more likely to shut down earlier. This pattern is consistent with accelerators functioning as institutions that reduce uncertainty, improve capital allocation, and speed up learning and selection, rather than merely prolonging firm survival.

7.4 Selection versus treatment

Figure 7 provides a simple decomposition of accelerator performance into selection and treatment components. We first estimate accelerator fixed effects from a model of log post-accelerator funding two years after entry, controlling for cohort, founding year, industry, region, and other baseline characteristics. These fixed effects capture the combined influence of startup selection into accelerators and the treatment effects generated by accelerator programs. We then regress the estimated fixed effects on accelerator value-added estimates obtained from the teacher value-added framework. The fitted values reflect the portion of accelerator performance explained by value added, while the residual variation captures selection. The fitted relationship has an R^2 of approximately 0.54, suggesting that value-added explains 54% of cross-accelerator heterogeneity, while the remaining variation reflects selection-related differences across programs.

Sørensen (2007) show that roughly one third of the association between venture capital and IPO outcomes can be attributed to value added, while the remaining two thirds reflect selection. Our setting may differ for two different reasons. First, rather than focusing on IPO outcomes, we examine post-accelerator venture funding, which is a more proximate performance measure and may be more directly affected by accelerator programs. Second, accelerators typically intervene at an earlier stage of the startup lifecycle than venture capital investors.

Variance decomposition. Finally, we use a Shapley value decomposition to quantify the relative contribution of accelerators across different outcome measures. Specifically, we regress each unresidualized outcome on industry and year fixed effects, founder and startup characteristics, and accelerator value added, and decompose the resulting R-squared using the Shapley value method, which is invariant to the ordering of covariates. As shown in Table 8, accelerator value added accounts for a substantial share of the variation in early-stage funding and employment, but very little of the variation in IPO outcomes or patenting activity.

8 Sorting by accelerator value added

To investigate which startups benefit most from accelerator value added, we examine the distribution of estimated value added across accelerator type, geography, industry, and startup characteristics.

Figure 9 plots kernel density estimates of accelerator value added by organizational type and affiliation. The figure shows that equity-taking accelerators exhibit slightly higher value added than non-equity programs. Similarly, corporate-affiliated accelerators tend to perform better on average, while university-affiliated accelerators are marginally less effective.

We further group PitchBook verticals into broader industry categories in Panel (a) of Figure 10. The classification scheme is described in Section A.6, and a more detailed breakdown of vertical-level estimates is presented in Figure B.8.

Panel (b) of Figure 10 reports the distribution of average accelerator value added by startup state. The results indicate that traditional startup hubs—such as California, Massachusetts, and New York—benefit more from accelerator participation than states with lower startup activity. Figure B.9 presents the corresponding distribution by accelerator location. Consistent with the previous pattern, accelerators located in established startup hubs exhibit higher average value added.

Table 11 examines how startup and founder characteristics relate to the value added they receive from accelerators. All columns restrict the sample to accelerated startups, and the dependent variable is a continuous measure of accelerator value added. Columns (2)–(4) further condition on accelerator type to explore whether sorting patterns differ across equity-taking, seed-funding, university, and corporate accelerators.

Across all specifications, we find strong evidence that higher-quality and more promising startups sort into accelerators that ultimately deliver higher value added. Startups with greater early-stage venture funding, female founders, serial entrepreneurs, STEM founders, immigrant founders, and founders from more prominent universities all experience significantly higher value added. Patent-producing teams and larger founding teams also appear to benefit more.

Columns (2)–(4) show how accelerator type correlates with value added. Equity-taking accelerators deliver substantially more value added, while seed-funding accelerators produce a smaller but still positive boost. In contrast, university-affiliated accelerators deliver significantly less value added, while corporate accelerators show no statistically significant difference from independent accelerators.

Tradeoffs in joining equity accelerator. The estimates reported in Table 11 are not causal. Nevertheless, they pose an interesting question to founders who are selecting between two different accelerators. Suppose a founder is admitted to two accelerators that are observably similar in their selection criteria based on founder and startup characteristics. If the two programs have competing schedules, which one should the founder choose? The results indicate that equity-taking accelerators exhibit, on average, 0.012 log points higher accelerator value added, which will translate into higher probability of successful acquisition. Does this difference imply that founders are better off choosing an equity-taking accelerator? In other words, is the associated tradeoff between higher value added and equity dilution worthwhile?

To answer this question, we focus on acquisition value and assume that venture capital funding raised by startups is fully spent on hiring workers and purchasing inputs, and that none of this funding directly accrues to founders as personal income. We therefore abstract from founders' cash payoffs from venture funding and focus exclusively on acquisition value.

The expected acquisition payoff can be written as

$$\mathbb{E}[p(f(e))\,(1-e)\,V], \tag{14}$$

where $p(f(e))$ denotes the probability of acquisition, which is increasing in accelerator value added $f(e)$, and where $f(e)$ may itself be an increasing function of the equity share $e \in [0, 1]$. The term V denotes the acquisition value. In the Appendix, we show that V is not affected by the equity share.

By the chain rule, the change in expected acquisition value resulting from a marginal change in the equity share can be written as

$$(\Delta p\,(1-e) - p\,\Delta e)\mathbb{E}[V], \tag{15}$$

where $\Delta p = \frac{\partial p(f(e))}{\partial e}$ denotes the marginal change in the probability of acquisition induced by a change in the equity share, and Δe denotes the marginal change in the accelerator equity share. We evaluate this expression at a baseline equity share of $e = 0$ and a baseline acquisition probability of $p = 0.080$, taken from Table 7.

To facilitate this comparison, we compute the expected benefit and cost of joining each accelerator as a function of its equity share using the estimated value added. Table 7 shows that the mean acquisition probability is 8.0 percent, and that a one–log-point increase in accelerator value added is associated with a 18.4 percentage point increase in the probability of acquisition. We assume an acquisition value of 322 million USD, which corresponds to the mean acquisition value in our data. For each startup, we compute the implied benefit and cost, and then average these quantities at the accelerator level. The resulting distributions are shown in Figure 11. In panel (b), we observe a leftward shift in the distribution of acquisition value for equity-taking accelerators. Once we account for the tradeoff between the benefits and costs of accelerator participation, equity dilution appears to exert a more negative effect on founders' expected acquisition payoffs.

9 Conclusion

Startup accelerators have become a prominent institution in early-stage entrepreneurial finance, yet their economic role has remained unclear because participation is highly selective and programs are heterogeneous. This paper shows that the relevant question is not whether accelerators work on average, but which accelerators, if any, improve startup outcomes. To answer that question, we adapt the teacher value-added framework from education economics to estimate accelerator value added (AVA), a measure of persistent program effectiveness based on short-run post-entry fundraising, and then examine whether this measure predicts longer-run startup outcomes. We focus on near-term fundraising because it is widely observed and economically central in a setting of staged experimentation and financing risk, while long-run outcomes are rarer and noisier (Kerr et al., 2014).

Three findings stand out. First, selection and sorting into accelerators are first-order: founder traits and early financing strongly predict participation, and entrant pools differ sharply across program types. Second, effectiveness is highly dispersed. Most accelerators exhibit negative estimated value added relative to a no-accelerator benchmark, while a small right tail generates large gains. These results shift the relevant choice for founders from "join an accelerator or not" to which accelerator to join (if any) and suggest substantial scope for misallocation when founders cannot distinguish high- from low-AVA programs (Sørensen, 2007).

Third, accelerators that improve short-run fundraising also predict longer-run outcomes. High-AVA programs are associated with higher rates of successful acquisition and stronger measures of scale, and they also accelerate shutdown for weaker firms—consistent with faster learning and selection rather than mechanically extending firm life. This pattern is difficult to reconcile with a pure "certification" story in which affiliation boosts funding without changing fundamentals. Instead, it is consistent with accelerators affecting trajectories through channels emphasized in entrepreneurial finance, including network access and, in equity-taking settings, governance-like support that shapes incentives and execution (Hochberg et al., 2007; Howell, 2020; Hsu, 2004; Kaplan and Strömberg, 2003; Kerr et al., 2014).

Methodologically, the paper illustrates how value-added approaches can be used to evaluate entrepreneurial intermediaries when selection is strong and long-run outcomes are noisy. We validate AVA using out-of-sample forecast tests and benchmark it using applicant-based quasi-experimental evidence from a large non-equity accelerator (Accelerator X), where admission among near-margin finalists predicts economically meaningful changes in outcomes. Together, these exercises support interpreting AVA as capturing persistent program effectiveness rather than merely ranking accelerators by observable deal flow (Bernstein et al., 2017; Chetty et al., 2014a,b).

Our findings have practical implications. For founders, the relevant question is not whether "accelerators work," but which programs have historically added value for observationally similar startups, and whether the implicit cost of participation—including equity dilution and managerial attention—matches expected benefits. For investors, accelerator affiliation is informative, but it is more informative when combined with evidence on the historical performance of the specific program. For universities and policymakers, the rapid spread of accelerators should not be taken to imply that all such programs are equally effective. Expanding the number of programs is not the same as expanding the number of high-value programs.

Several extensions of our work can sharpen welfare interpretation and mechanisms. First, direct measures of investor access—such as the entry of new investors, syndicate composition, or network position—would help separate network-based value creation from certification or coaching effects of accelerators. Second, linking program participation to media attention and investor outreach would test whether the effects operate primarily through visibility and attention. Third, richer data on contracts and program terms would clarify how equity stakes and control rights shape incentives and intervention intensity. These directions would deepen our understanding of when accelerators function mainly as certifiers and when they operate as value-creating entrepreneurial finance intermediaries.

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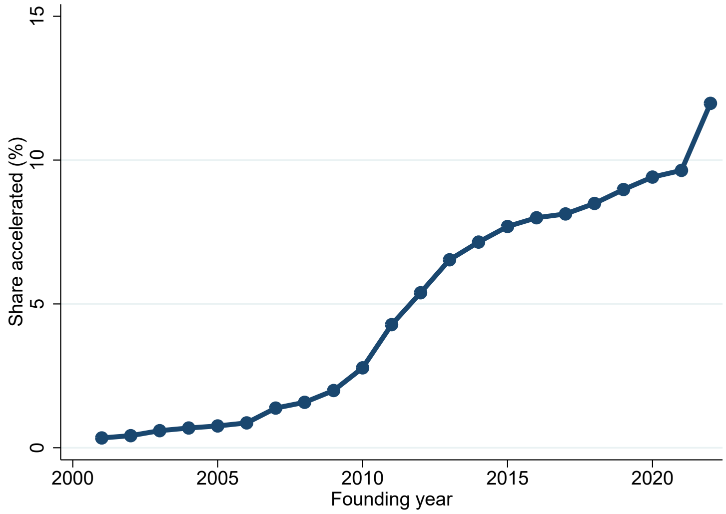
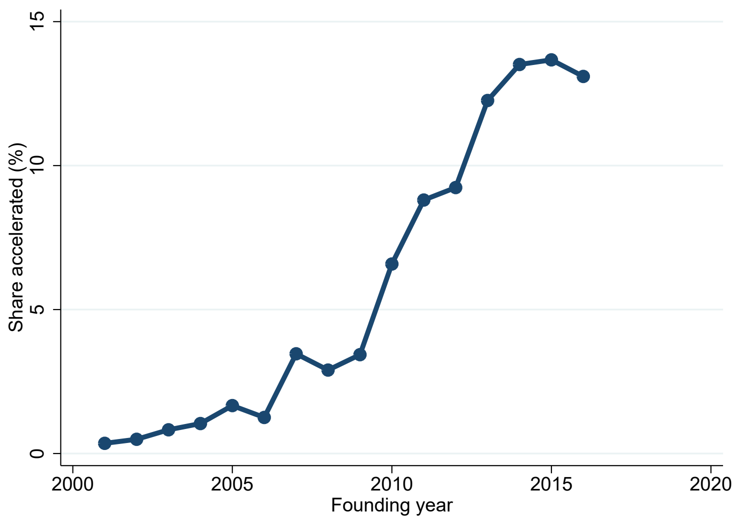
Figure 1: Growing prevalence of accelerated startups

(a) Share of accelerated startups among all startups

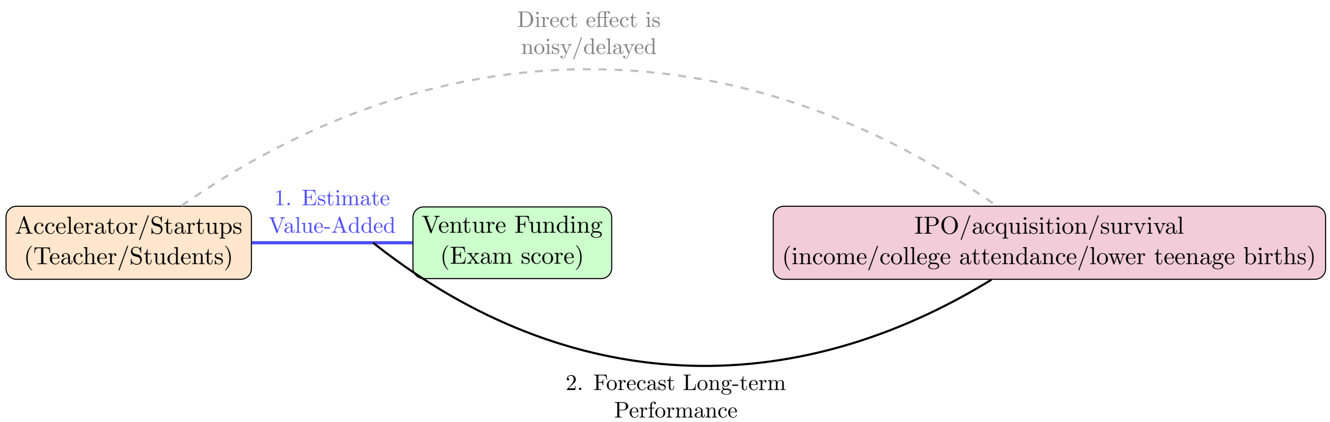


(b) Share of accelerated startups among successful exits



Note: These figures use a merged PitchBook-Crunchbase dataset constructed for this study (see Section 3 for details). Panel (a) shows the share of startups that participated in an accelerator among all startups by founding year. Panel (b) shows the share of accelerated startups among all successful exits by founding year. A successful exit is defined as either an initial public offering or an acquisition with a transaction value exceeding twice the total amount of venture funding raised. Panel (b) is truncated at the 2016 founding cohort, as firms exit on average at age eight, and exit outcomes for more recently founded firms remain incomplete.

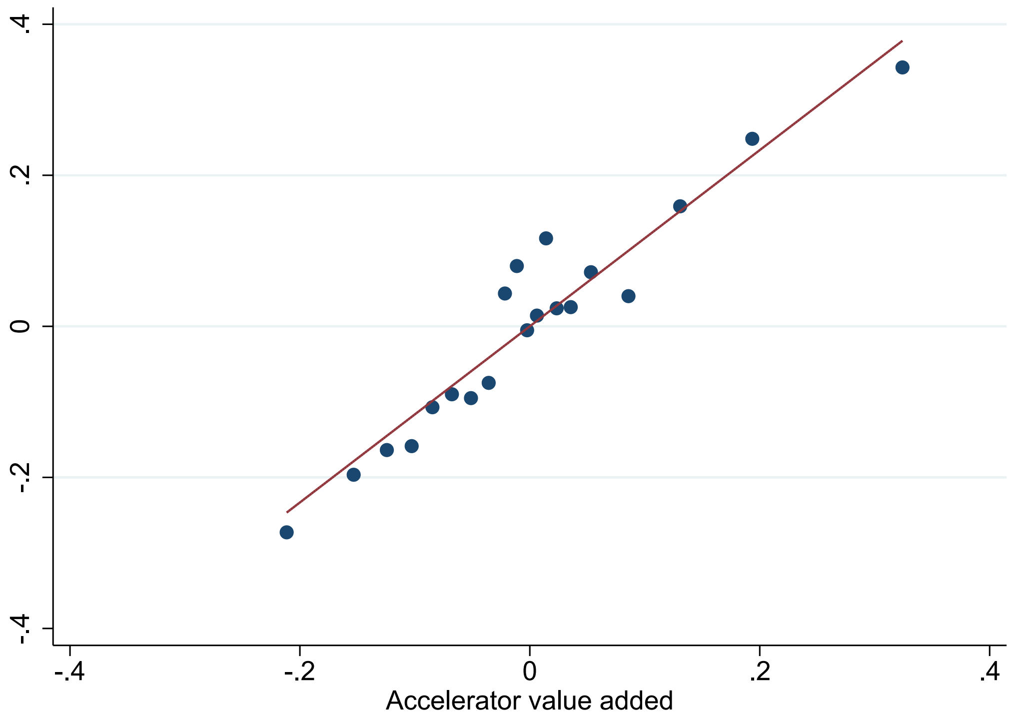
Figure 2: Conceptual framework: value-added by accelerators vs. teachers



Note: This figure illustrates the empirical strategy for estimating value-added (VA), drawing an analogy between the teacher VA literature (Chetty et al., 2014a,b) and the accelerator context. The model relies on two steps: (1) extracting the “signal” or treatment effect of an entity (accelerator or teacher) on immediate, observable outcomes (funding or test scores), and (2) validating that this short-term signal is a significant predictor of long-term success (exits/survival or adult income/college attendance). The dashed line represents the difficulty of measuring the direct impact on long-term outcomes without the intermediate signal due to noise and time lags.

Figure 3: Accelerator value added and post-accelerator venture funding

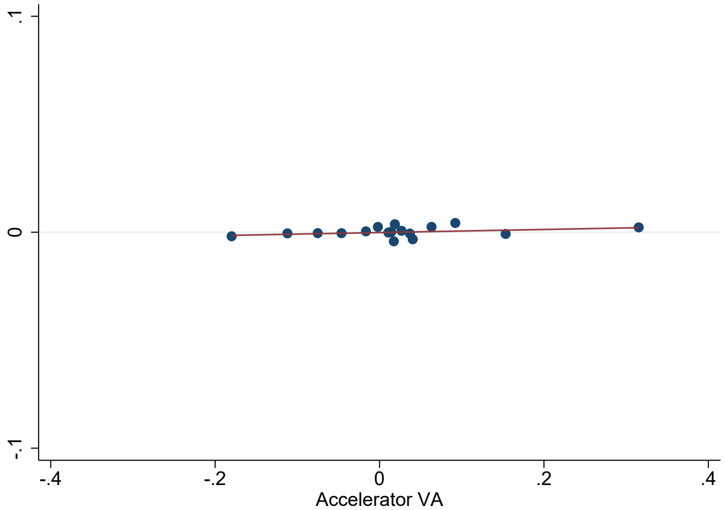
(a) log early stage financing within 2 years of accelerators



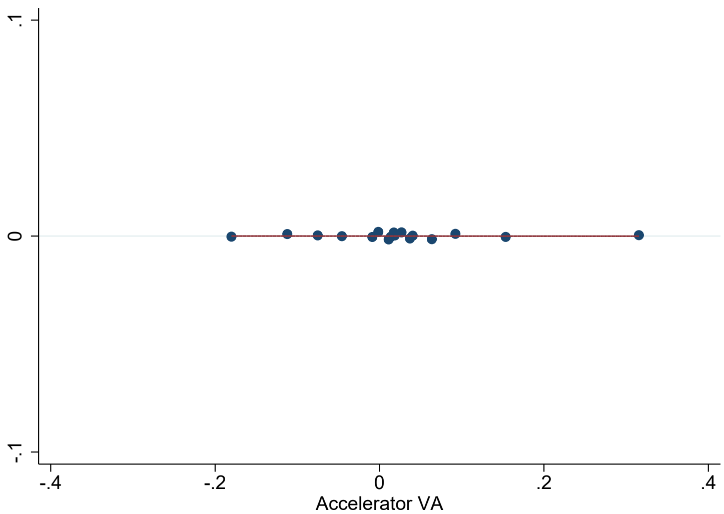
Note: The panel reports a binned scatter plot of actual log early-stage financing within two years of joining an accelerator, plotted against accelerator value added (log). To construct the binned scatter plot, we residualize both the outcome variable (actual log early-stage financing) and accelerator value added with respect to the full set of controls, including founding-year, industry, region, and accelerator fixed effects; the total number of founders; the shares of the founding team who are serial entrepreneurs, female, MBA holders, STEM degree holders, or immigrants; measures of founder experience and university prominence; and an indicator for missing founder information from LinkedIn profiles. We then divide accelerator value added into 20 equally sized bins and plot the mean of the residualized outcome within each bin against the mean of accelerator value added in the same bin.

Figure 4: Accelerator value added and post-accelerator venture funding

(a) Prediction using startup funding growth rate



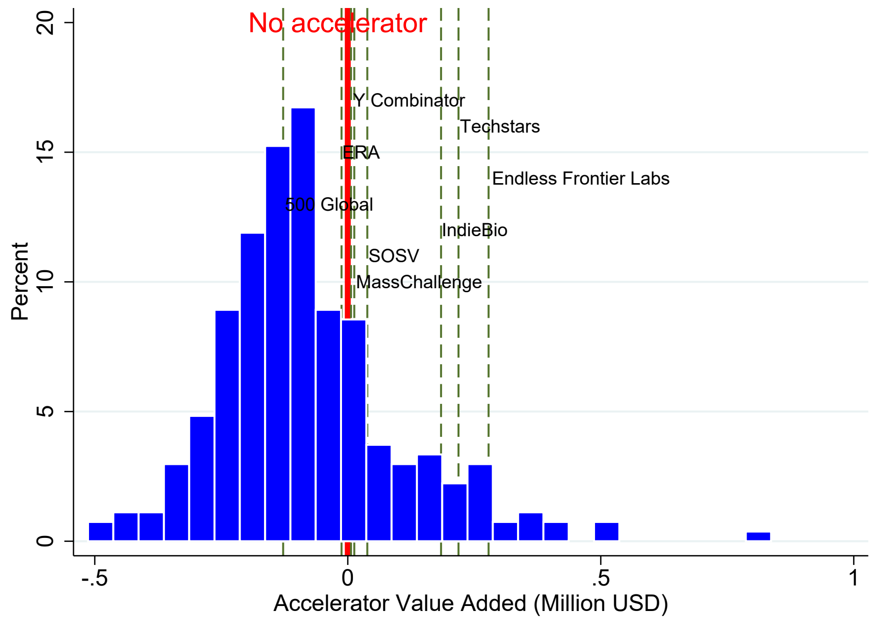
(b) Prediction using classmates' early-stage funding



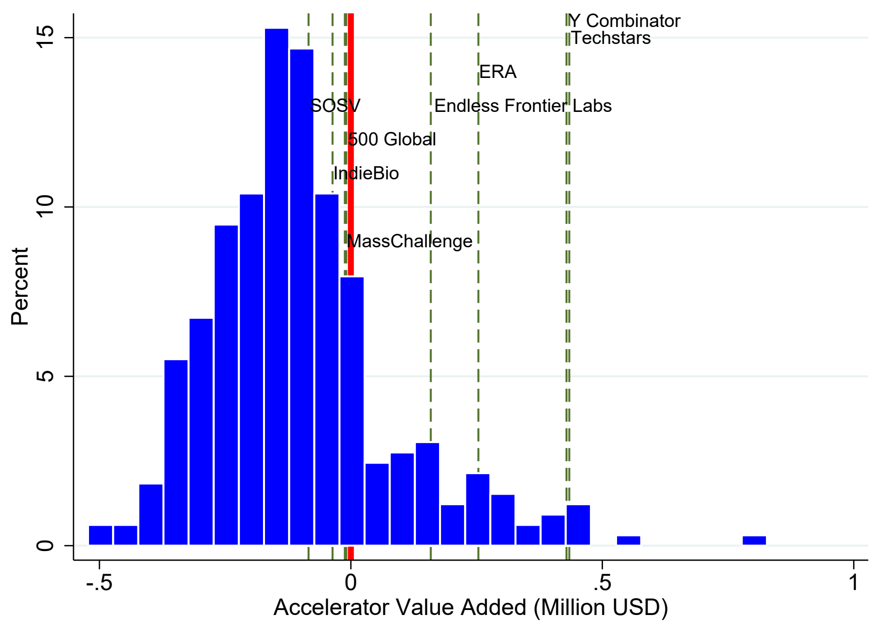
Note: The two panels report placebo tests using binned scatter plots of predicted log early-stage financing within two years of joining an accelerator, plotted against accelerator value added (log). Panel (a) constructs predicted financing based on the startup's own pre-accelerator funding growth rate, while Panel (b) constructs predicted financing based on early-stage financing of other startups within the same accelerator cohort. In both cases, we first estimate the relationship between actual financing and the respective predictor using the full set of controls described in Table 4, then use the fitted relationship to generate predicted values for each startup. To construct the binned scatter plots, we residualize both the predicted outcome and accelerator value added with respect to the full set of controls, including founding-year, industry, state, and accelerator fixed effects; the total number of founders; the shares of the founding team who are serial entrepreneurs, female, MBA holders, STEM degree holders, or immigrants; measures of founder experience and university prominence; and an indicator for missing founder information from LinkedIn profiles. We then divide accelerator value added into 20 equally sized bins and plot the mean of the residualized outcome within each bin against the mean of accelerator value added in the same bin.

Figure 5: Distribution of accelerator value added

(a) Value added, 2022

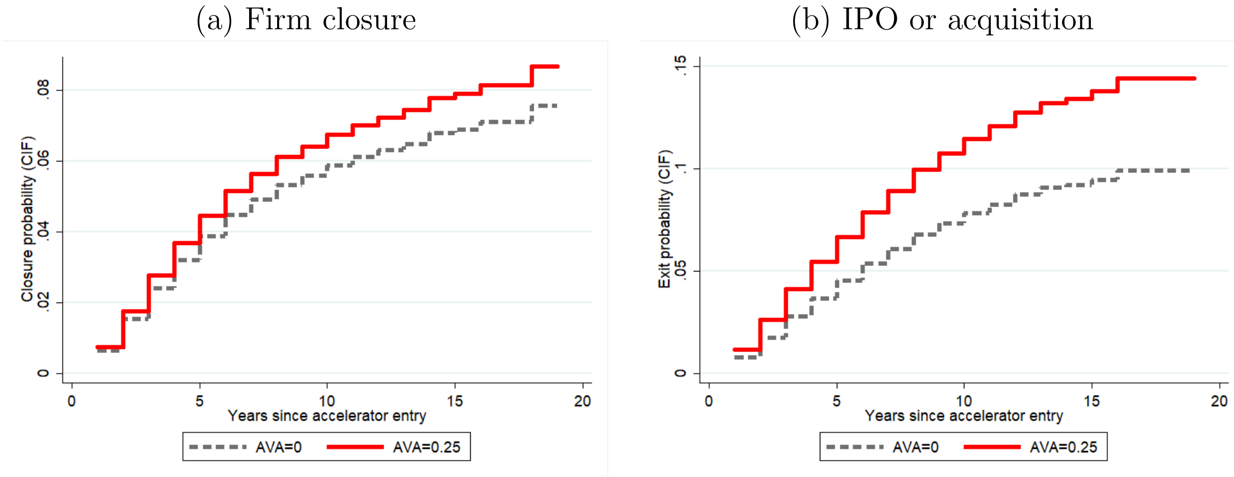


(b) Value added average across all cohorts, 2005-2022



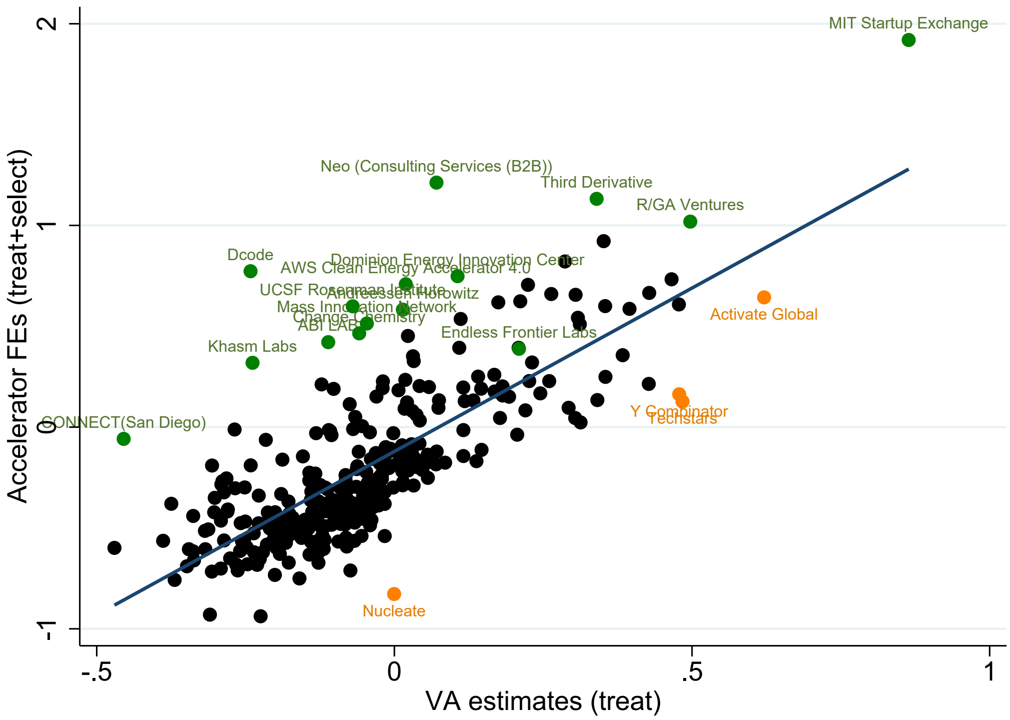
Note: The value added by accelerators is estimated based on their predicted impact on early-stage funding within a two-year window following program entry. Accelerator value added is reported at the accelerator–cohort level. To obtain accelerator-level estimates, accelerator value added is averaged across all available cohorts for each accelerator. Accelerator value added is originally calculated as the log value of post-accelerator funding attributable to the accelerator treatment effect, and is then converted into dollar terms by multiplying the mean post-accelerator funding by the exponentiated log accelerator value added. 80 percent of accelerated startups was found to be lower than that of their non-accelerated counterparts.

Figure 6: Accelerator value added and startup outcomes: Exit hazard estimates



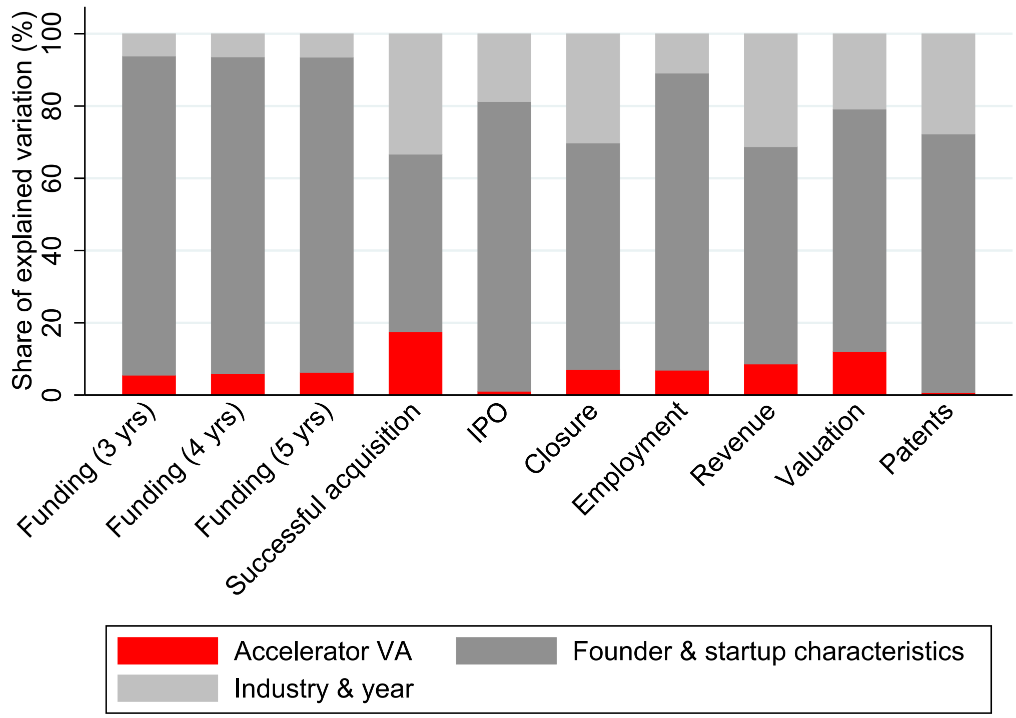
Note: This figure plots predicted cumulative incidence functions (CIFs) from Fine–Gray competing-risks models for business closure and successful exit (IPO or acquisition). Closure and exit are treated as mutually competing events. All models include industry, founding-year, and accelerator-entry-year fixed effects, with standard errors clustered at the accelerator-class level. Curves are evaluated at baseline covariate values. The two lines correspond to accelerators with log accelerator value added equal to 0 (no-accelerator benchmark) and 0.25.

Figure 7: Selection versus value add of accelerators, 2005-2022



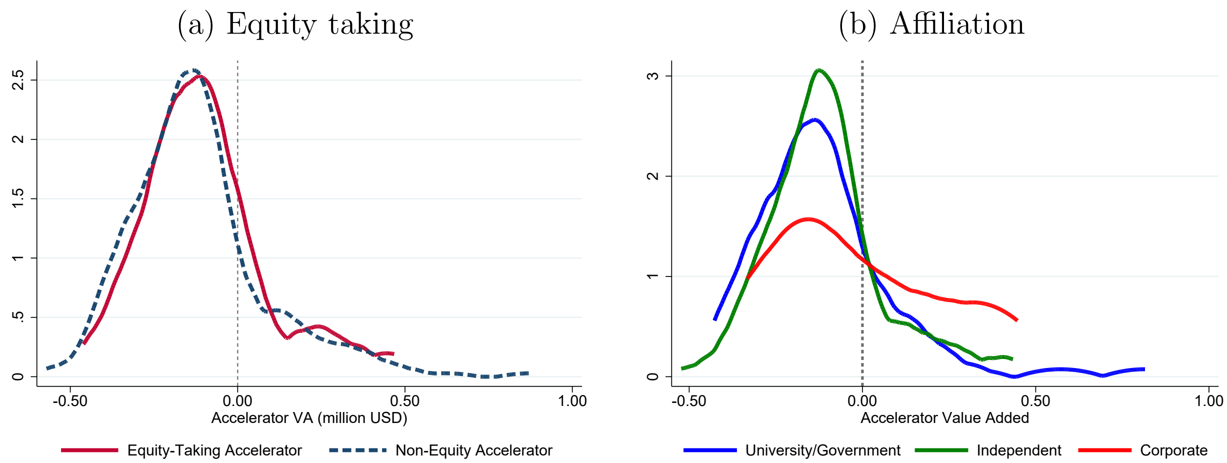
Note: The figure decomposes accelerator performance into treatment (value added) and selection components. We first estimate accelerator fixed effects from a model of log post-accelerator funding two years after entry, controlling for cohort, year, region, and industry fixed effects. These fixed effects capture the combined influence of selection and treatment. We then regress the estimated fixed effects on accelerator value-added estimates obtained from the teacher value-added model. The fitted component reflects accelerator effects attributable to accelerator value added, while the residual variation captures selection. The regression line has an R^2 of 0.54, indicating that value-added explains approximately 54% of cross-accelerator variation in post-accelerator funding, with the remaining 46% interpreted as selection-related differences. Orange markers indicate accelerators where value-added effects are stronger than average, while green markers indicate accelerators where selection plays a relatively larger role. Selected accelerators are labeled for illustration.

Figure 8: Shapley value decomposition



Note: This figure displays Shapley value decompositions of the explained variation in post-accelerator outcomes. These results build on those reported in Tables 4, 7, and 8. The Shapley value assigns to each component its average marginal contribution to model fit across all possible orderings of regressors. We regress the unresidualized outcome variables on accelerator value added, founder and startup characteristics, and industry and industry sector fixed effects (six categories). State fixed effects are excluded to avoid multicollinearity in the Shapley value decomposition. Founder and startup characteristics include early-stage funding, founding team size, prior entrepreneurial experience, gender composition, educational background, immigration status, and early-stage patenting. Early-stage funding ($ 2y post) funding raised within two years after joining an accelerator. Acquisition denotes acquisitions in which the transaction value exceeds twice the total venture funding raised prior to exit. Valuation is defined in Table 8 as the change in log valuation from the pre-program level to the maximum valuation observed within ten years. For firms with missing early-stage valuations, we impute the value as log(1). Employment and revenue are defined accordingly.

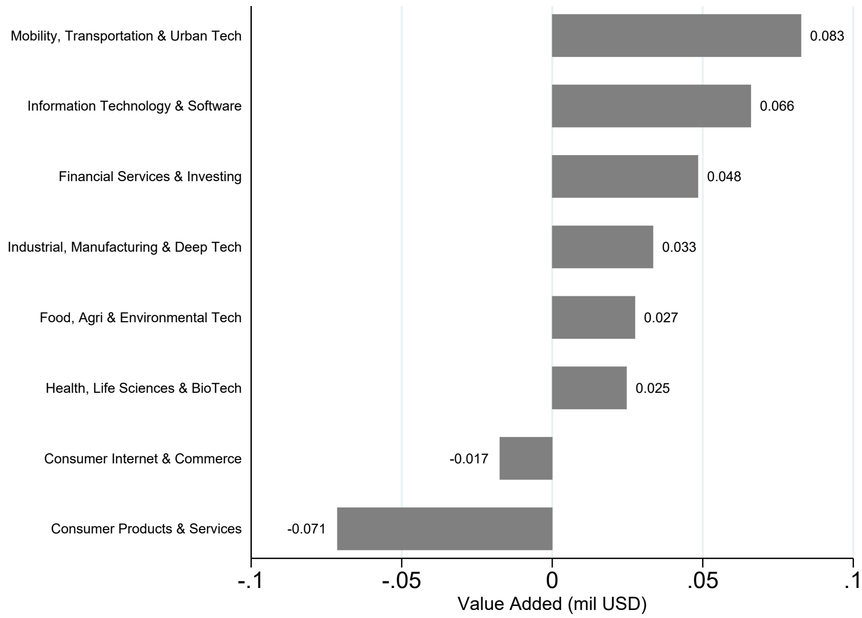
Figure 9: Distribution of accelerator value added by accelerator type



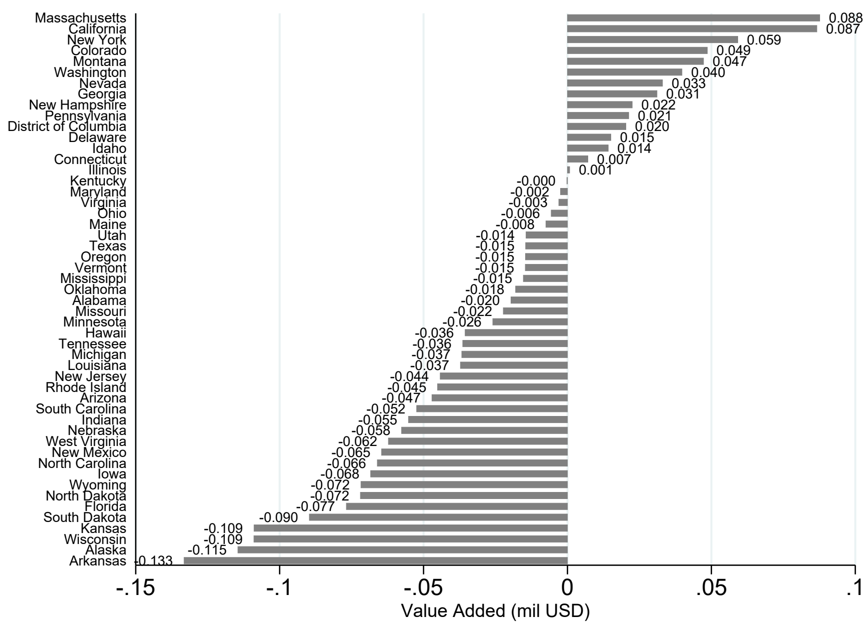
Note: This figure presents kernel density plots of accelerator value added, measured in million USD, where zero corresponds to the no-accelerator benchmark. To construct these distributions, we average accelerator value added across cohorts for each accelerator, given by $\frac{1}{N_j}\sum_t \sum_{i\in\mathcal{C}_{jt}} \widehat{\mu}_{jt}$, where t indexes all available cohorts of accelerator j and $\mathcal{C}_{jt}$ denotes the set of startups in cohort t. Accelerators are classified according to whether they take equity (Panel (a)) and by corporate or university affiliation (Panel (b)).

Figure 10: Accelerator value added by vertical and state

(a) Acclerator value added by startup industry vertical



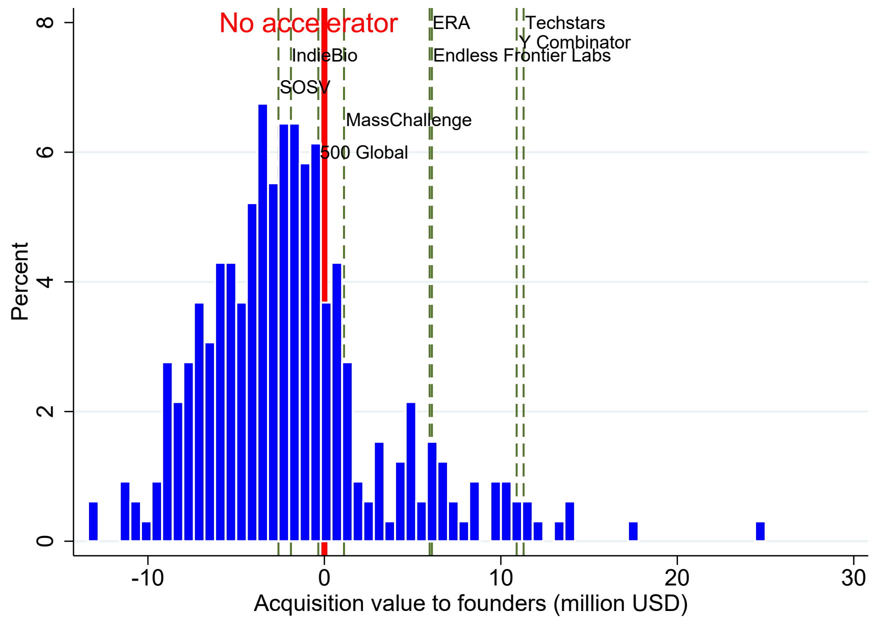
(b) Acclerator value added by startup state



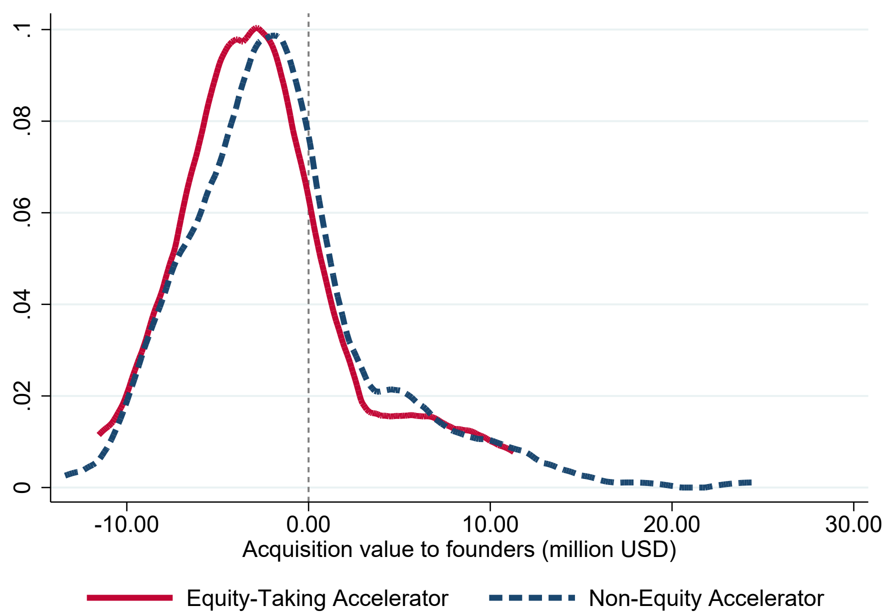
Note: This figure plots the distribution of accelerator value added across industry verticals and U.S. states. Accelerator value added is estimated using a teacher value-added model and is measured at the accelerator–cohort level. Each startup is assigned to all applicable PitchBook verticals, and for each vertical we compute the average accelerator value added across associated startups. An analogous procedure is applied to startups' headquarters states, where we calculate the average accelerator value added across startups located in each state. A more granular breakdown of industries is given in Section B.8

Figure 11: Acquisition value to founders

(a) Value to founders



(b) Value by type



Note: This figure translates accelerator value added into acquisition value accruing to founders, following the discussion in Section 8, which accounts for the equity share taken by each accelerator. For each accelerator and cohort, value added is averaged across all participating startups. We then compute the implied increase in acquisition value by multiplying this average value added by the estimated increase in acquisition probability attributable to accelerator participation, and subtract the expected cost associated with the equity share retained by the accelerator. Panel (a) shows the distribution of implied founder value across all accelerators. Panel (b) decomposes this distribution by accelerator type.

Table 1: Balance Table: Accelerated startups vs non-accelerated startups

	Non-Accelerated Startups				Adjusted Difference (vs. Non-Accelerated)				
	Mean	Min	Max	Obs.	Any Accelerator	Equity Taking	Univ./ Govt.	Corp. Affil.	Indep.
log early-stage funding	-1.73	-2	7	589578	0.47^{***}	0.35^{***}	0.35^{***}	0.93^{***}	0.47^{***}
					(0.01)	(0.01)	(0.02)	(0.04)	(0.01)
Post-accelerator funding (mli USD)	0.20	0	1100	589578	0.53^{***}	0.64^{***}	0.22^{***}	1.27^{***}	0.53^{***}
					(0.03)	(0.04)	(0.07)	(0.19)	(0.03)
D[any lifetime funding]	0.08	0	1	589578	0.29^{***}	0.27^{***}	0.23^{***}	0.36^{***}	0.29^{***}
					(0.00)	(0.00)	(0.01)	(0.01)	(0.00)
Early-stage patents	0.03	0	380	492021	0.02^{**}	-0.02^{***}	-0.04^{***}	0.00	0.03^{***}
					(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
No. of founders	1.62	1	10	134877	0.43^{***}	0.51^{***}	0.50^{***}	0.31^{***}	0.42^{***}
					(0.01)	(0.02)	(0.03)	(0.04)	(0.01)
Women founders	0.24	0	1	134877	0.03^{***}	0.00	0.03^{***}	0.10^{***}	0.02^{***}
					(0.00)	(0.00)	(0.01)	(0.01)	(0.00)
Serial entrepreneurs	0.29	0	1	134877	-0.02^{***}	-0.00	-0.06^{***}	-0.01	-0.02^{***}
					(0.00)	(0.01)	(0.01)	(0.01)	(0.00)
MBA	0.14	0	1	134877	0.03^{***}	0.01^{*}	0.04^{***}	0.05^{***}	0.02^{***}
					(0.00)	(0.00)	(0.01)	(0.01)	(0.00)
STEM	0.28	0	1	134877	0.13^{***}	0.13^{***}	0.18^{***}	0.10^{***}	0.12^{***}
					(0.00)	(0.01)	(0.01)	(0.02)	(0.00)
Immigrant	0.15	0	1	134877	0.02^{***}	0.03^{***}	0.01	0.00	0.02^{***}
					(0.00)	(0.00)	(0.01)	(0.01)	(0.00)
University quality	47.01	44	100	134877	1.99^{***}	1.93^{***}	2.58^{***}	2.27^{***}	1.85^{***}
					(0.11)	(0.17)	(0.31)	(0.45)	(0.12)
Founder experience	7.93	0	36	129364	-0.70^{***}	-1.28^{***}	-1.29^{***}	0.09	-0.72^{***}
					(0.08)	(0.11)	(0.22)	(0.31)	(0.09)
Exit outcomes (percent):									
IPO	0.46	0	100	589578	-0.24^{***}	-0.24^{***}	-0.74^{***}	0.62^{*}	-0.23^{***}
					(0.05)	(0.06)	(0.11)	(0.33)	(0.05)
Successful Acquisition	4.48	0	100	589578	5.12^{***}	7.15^{***}	1.74^{***}	4.81^{***}	5.51^{***}
					(0.20)	(0.34)	(0.44)	(0.83)	(0.23)
Closure	7.95	0	100	589578	4.98^{***}	9.06^{***}	2.10^{***}	1.41	5.44^{***}
					(0.23)	(0.39)	(0.53)	(0.86)	(0.26)
PitchBook industry sectors:									
Business Products and Services (B2B)	0.06	0	1	589578	0.07^{***}	0.06^{***}	0.07^{***}	0.02^{***}	0.08^{***}
					(0.00)	(0.00)	(0.01)	(0.01)	(0.00)
Consumer Products and Services (B2C)	0.18	0	1	589578	0.02^{***}	-0.00	0.02^{***}	0.09^{***}	0.01^{***}
					(0.00)	(0.00)	(0.01)	(0.01)	(0.00)
Financial Services	0.00	0	1	589578	0.01^{***}	0.02^{***}	0.01^{***}	0.01^{***}	0.02^{***}
					(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Healthcare	0.10	0	1	589578	0.09^{***}	0.05^{***}	0.18^{***}	0.12^{***}	0.08^{***}
					(0.00)	(0.00)	(0.01)	(0.01)	(0.00)
Information Technology	0.64	0	1	589578	-0.22^{***}	-0.12^{***}	-0.31^{***}	-0.24^{***}	-0.20^{***}
					(0.00)	(0.00)	(0.01)	(0.01)	(0.00)
Materials and Resources	0.00	0	1	589578	0.01^{***}	0.00^{***}	0.02^{***}	-0.00	0.01^{***}
					(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations				589578	29255	11219	4288	1437	23530
Fixed Effects					Founding Year and State				

Note: Columns (1)–(4) report summary statistics for non-accelerated startups. Columns (5)–(9) report adjusted differences relative to non-accelerated startups from OLS regressions of each characteristic on indicators for accelerator participation and accelerator type, including founding year and state fixed effects. Early-stage financing is defined as cumulative funding up to the accelerator entry year for accelerated startups and up to an imputed entry year for non-accelerated startups based on their block (state × founding year × industry); detailed construction is described in Section 5.5. Founder characteristics (Women founders, Serial entrepreneurs, MBA, STEM, Immigrant) denote the share of founders with each attribute. University quality is the average ranking of founders' alma maters, and founder experience measures years of pre-founding work experience (winsorized at the 1st and 99th percentiles). Early-stage patents measure the number of patent applications filed prior to accelerator entry. Successful acquisition, closure, and IPO are indicator variables; a successful acquisition is defined as an acquisition in which transaction value exceeds twice total venture funding raised. PitchBook industry sectors are indicator variables. Standard errors are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2: Covariate balance before and after entropy-balanced matching

	Before Matching			After Matching		
Variables:	No accel.	Accel.	Diff.	No accel.	Accel.	Diff.
Early-stage funding (mil USD)	0.685	1.620	0.935***	1.554	1.620	0.066
	(11.571)	(13.202)	(0.061)	(16.933)	(13.202)	(0.109)
Number of founders	1.617	2.100	0.483***	2.099	2.100	0.002
	(0.969)	(1.155)	(0.008)	(1.441)	(1.155)	(0.014)
Serial entrepreneur	0.295	0.284	-0.011***	0.285	0.284	-0.001
	(0.412)	(0.369)	(0.003)	(0.382)	(0.369)	(0.004)
Female founder	0.231	0.261	0.029***	0.265	0.261	-0.004
	(0.387)	(0.373)	(0.003)	(0.386)	(0.373)	(0.004)
MBA	0.139	0.176	0.037***	0.174	0.176	0.002
	(0.316)	(0.323)	(0.002)	(0.332)	(0.323)	(0.003)
STEM founder	0.288	0.483	0.195***	0.487	0.483	-0.004
	(0.415)	(0.424)	(0.003)	(0.441)	(0.424)	(0.004)
Immigrant founder	0.147	0.182	0.035***	0.182	0.182	0.000
	(0.338)	(0.352)	(0.003)	(0.358)	(0.352)	(0.004)
University prominence	46.979	49.663	2.684***	49.603	49.663	0.060
	(8.723)	(11.992)	(0.071)	(12.465)	(11.992)	(0.127)
Founder experience	7.954	7.773	-0.181**	7.861	7.773	-0.088
	(9.830)	(8.799)	(0.076)	(9.447)	(8.799)	(0.095)
Early-stage patents	0.166	0.197	0.030	0.216	0.197	-0.019
	(3.427)	(2.164)	(0.026)	(6.367)	(2.164)	(0.049)
Founder info. missing	0.780	0.525	-0.254***	0.524	0.526	0.002
	(0.414)	(0.499)	(0.002)	(0.499)	(0.499)	(0.004)
PitchBook industry sector:						
Business Products and Services (B2B)	0.067	0.137	0.070***	0.139	0.137	-0.002
	(0.250)	(0.344)	(0.001)	(0.346)	(0.344)	(0.002)
Consumer Products and Services (B2C)	0.183	0.205	0.022***	0.205	0.205	-0.000
	(0.387)	(0.403)	(0.002)	(0.403)	(0.403)	(0.003)
Financial Services	0.004	0.018	0.014***	0.019	0.018	-0.001
	(0.064)	(0.134)	(0.000)	(0.136)	(0.134)	(0.001)
Energy	0.017	0.021	0.004***	0.020	0.021	0.000
	(0.129)	(0.142)	(0.001)	(0.141)	(0.142)	(0.001)
Healthcare	0.103	0.193	0.090***	0.194	0.193	-0.001
	(0.304)	(0.395)	(0.002)	(0.396)	(0.395)	(0.003)
Information Technology	0.619	0.408	-0.211***	0.406	0.408	0.002
	(0.486)	(0.491)	(0.003)	(0.491)	(0.491)	(0.004)
Materials and Resources	0.007	0.018	0.010***	0.017	0.018	0.001
	(0.086)	(0.132)	(0.000)	(0.129)	(0.132)	(0.001)
Observations	710,672	38,968	749,640	38,972	38,968	77,940

Note: This table reports covariate balance between accelerated and non-accelerated startups before and after matching based on entropy balancing. "Difference" reports the mean difference between the two groups (accelerated-nonaccelerated), with standard errors in parentheses. The comparison sample is constructed using a two-step entropy-balancing procedure. Entropy balancing first defines a target control distribution that matches the treated group on pre-treatment characteristics (Hainmueller, 2012). We then construct a subset of non-accelerated startups that replicates the mean characteristics of the accelerated group, using integer weights derived from entropy balancing. Balancing variables include: early-stage funding (in million USD); founder team composition measured by the number of founders and the shares of founders who are serial entrepreneurs, female, MBA holders, STEM/technical degree holders, and immigrants; founder background measured by average founder experience and university prominence; early-stage patents; and an indicator for missing LinkedIn founder information. In addition, we balance on a full set of fixed effects for founding year, state, and PitchBook industry sector (N=7). Early-stage financing is defined as cumulative funding up to the accelerator entry year for accelerated startups and up to an imputed entry year for non-accelerated startups based on their block (state × founding year × industry); detailed construction is described in Section 5.5. Founder and startup characteristics from the number of founders to patents are available only for startups successfully linked to LinkedIn; therefore, these variables have smaller sample sizes. The match rate and linkage procedure are discussed in Table **??** and Section A.1. Missingness is accounted for by including the missing information indicator in the balancing procedure. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: Assessment of forecast unbiasedness

	(1)	(2)	(3)	(4)
	log funding			
DV:	2 years after accelerator			
Accelerator VA (log)	0.71***	0.79***	1.07***	1.18***
	(0.05)	(0.07)	(0.09)	(0.11)
Observations	63,419	63,419	63,419	63,419
R-squared	0.16	0.11	0.05	0.02
Industry FE	Y	Y	Y	Y
Founding year FE	Y	Y	Y	Y
Founder controls		Y		Y
Pre-accelereator funding			Y	Y
State FE			Y	Y

Note: This table assesses the forecast unbiasedness of accelerator value added estimates estimated with different covariates. In all columns, we estimate the same forecast regression, which relates realized startup outcomes to pre-estimated accelerator VA. The regressions reported in columns (1) through (4) differ only in how accelerator VA is constructed in this first-stage estimation; the forecast regression itself is identical across columns. The outcome variable is log funding raised within two years after joining an accelerator. In all forecast regressions, we control for early-stage funding, startup and founder characteristics described in Table **??**, and include industry fixed effects, state fixed effects, founding-year fixed effects, and accelerator–cohort fixed effects. Standard errors are clustered at the accelerator–cohort level. Accelerator VA is estimated in a separate first-stage using a value-added model (VAM) that allows for drift in accelerator effects over time, with a drift limit of nine years (Stepner, 2013). Column (1) uses a baseline VA measure estimated controlling only for industry and founding year. Column (2) constructs VA by additionally controlling for founder characteristics, including the number of founders, the shares of serial entrepreneurs, female founders, MBA holders, STEM degree holders, and immigrant founders, as well as founder experience, university prominence, early-stage patents, and an indicator for missing founder information. Column (3) constructs VA by controlling for early-stage funding (in million USD) in the VA estimation stage, without including founder characteristics. Column (4) uses the full VA specification, incorporating both founder characteristics and early-stage funding, along with state fixed effects. Following Chetty et al. (2014a), forecast unbiasedness is evaluated by regressing realized outcomes on the estimated VA. A coefficient on accelerator VA close to one indicates that the VA measure provides an unbiased forecast of accelerator value added. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Accelerator value added and funding

Panel A:	(1) log funding after 2 years joining accelerator	(2)	(3) log funding after 3 years joining accelerator	(4)
Accelerator VA (log)	1.18***	1.04***	1.44***	1.32***
	(0.09)	(0.09)	(0.10)	(0.10)
Mean of DV	-0.00	-0.00	-1.29	-1.29
Oster Delta		14.52		9.35
Observations	63418	63418	63418	63418
R-squared	0.0233	0.0557	0.1102	0.1484
N Founding Year FE	22	22	22	22
N State FE	52	52	52	52
N Cohort FE	18	18	18	18
N Industry FE	190	190	190	190
Sample FE	2	2	2	2
Startup controls	N	Y	N	Y

Panel B:	log funding after 4 years joining accelerator		log funding after 5 years joining accelerator	
Accelerator VA (log)	1.60***	1.46***	1.74***	1.59***
	(0.10)	(0.11)	(0.11)	(0.12)
Mean of DV	-1.22	-1.22	-1.18	-1.18
Oster Delta		9.15		9.07
Observations	63418	63418	63418	63418
R-squared	0.1245	0.1670	0.1332	0.1788
N Founding Year FE	22	22	22	22
N State FE	52	52	52	52
N Cohort FE	18	18	18	18
N Industry FE	190	190	190	190
Sample FE	2	2	2	2
Startup controls	N	Y	N	Y

Note: This table reports coefficients from a two-step estimation procedure where startup outcomes are first residualized to account for baseline differences across firms and accelerators. Each outcome variable is regressed on early-stage venture funding, founder characteristics, and a comprehensive set of fixed effects including founding year, industry sector, and state. The residuals from these regressions are then regressed on the predicted measure of accelerator value added. To ensure the results are interpretable, the mean of the dependent variable reported at the bottom of the table is calculated using the raw outcomes before the residualization process was applied. All accelerator VA in the table are estimated from the early-stage financing within two years of joining accelerators. Founder controls used in the first-stage residualization include the total number of founders and the shares of the team who are serial entrepreneurs, female, MBA holders, STEM degree holders, or immigrants, as well as measures of founder experience and university prominence. For startups where LinkedIn information was unavailable, missingness is accounted for by the inclusion of a missing information indicator. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5: Accelerator-X: Balance test

	(1)	(2)	(3)	(4)
DV	Pre accelerator VC		Other accel before	Other accel after
Accelerator-X graduate	1.23	1.14	0.02	0.05
	(0.78)	(0.77)	(0.02)	(0.04)
Observations	3,939	834	834	834
R-squared	0.01	0.03	0.01	0.02
Controls	Y	Y	Y	Y
App year FE	Y	Y	Y	Y
Track FE	Y	Y	Y	Y
Interviewed		Y	Y	Y

Note: This table tests for balance between accelerated and non-accelerated startups in the finalist sample of the non-equity accelerator. Columns (1) and (2) examine differences in early-stage financing. Columns (3) and (4) test for differences in an indicator for whether a startup joined another accelerator before or after participating in this non-equity accelerator. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Effects of joining accelerator-X

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DV: log funding 2 years after accelerator							
	Control group mean (million USD): 0.65							
Accelerator-X graduate				0.48***	0.39**	0.43**	0.51***	0.39**
				(0.16)	(0.16)	(0.19)	(0.17)	(0.19)
Application Score	0.16***		0.06	0.01	-0.02			
	(0.02)		(0.14)	(0.14)	(0.13)			
Interview score		0.20***	0.17***	0.08	0.04	0.08		
		(0.05)	(0.06)	(0.07)	(0.06)	(0.08)		
log early-stage funding					0.22***	0.13**	0.13**	0.15**
					(0.05)	(0.05)	(0.05)	(0.06)
Number of founders						0.08	0.03	0.03
						(0.10)	(0.10)	(0.11)
Serial entrepreneur						-0.08	0.05	-0.10
						(0.28)	(0.29)	(0.31)
Female founder						0.06	0.07	0.01
						(0.26)	(0.26)	(0.30)
MBA						-0.86***	-0.82***	-0.73**
						(0.28)	(0.30)	(0.33)
STEM founder						0.20	0.24	0.21
						(0.24)	(0.25)	(0.28)
Immigrant founder						-0.33	-0.31	-0.32
						(0.21)	(0.21)	(0.25)
University prominence						-0.01	-0.00	-0.00
						(0.01)	(0.01)	(0.01)
Founder experience						-0.00	0.00	0.01
						(0.01)	(0.01)	(0.01)
Founder info. missing						0.51	0.51	0.43
						(0.33)	(0.33)	(0.36)
VA (million USD)				0.40	0.31	0.35	0.43	0.31
Observations	3,485	809	600	600	600	546	565	539
R-squared	0.06	0.05	0.05	0.06	0.11	0.18	0.16	0.23
App year FE	Y	Y	Y	Y	Y	Y	Y	Y
Track FE	Y	Y	Y	Y	Y	Y	Y	Y
Founding year FE						Y	Y	Y
Interviewed						Y	Y	Y
PB industry sector						Y	Y	
PB industry code								Y

Note: The dependent variable is the logarithm of total funding raised within two years of joining the accelerator. Application review score captures the evaluation score assigned during the first-round application screening. Interview score records the evaluation score from interviews conducted only for applicants who passed the initial application review. Both scores are continuous variables and typically range from 1 to 7. Accelerator X graduate is a dummy variable equal to one if the startup successfully completed (graduated from) the accelerator program, and zero otherwise. Founding year and industry sector indicators (six industry categories) are constructed using PitchBook data. Columns (6)–(9) restrict the sample to startups that were invited to and completed the interview stage. All specifications include application year and track fixed effects, and selected specifications additionally control for founder characteristics. Value Added (VA) in millions of USD is calculated by multiplying the log value added by the mean post-accelerator funding of the control group. Standard errors are reported in parentheses. Statistical significance is denoted by *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table 7: Accelerator value added: Exit and innovation outcomes

	(1) Base	(2) +Controls	(3) +Lifetime Funding
Panel A: D[IPO] ×100			
Accelerator VA (log)	0.10	0.46	-0.19
	(0.52)	(0.52)	(0.51)
Lifetime funding (log)			0.58***
			(0.10)
Mean of DV	0.66	0.66	0.66
Oster δ		-0.49	-0.41
R-squared	0.045	0.051	0.055
Panel B: D[Successful Acquisition] ×100			
Accelerator VA (log)	18.40***	18.62***	17.94***
	(1.95)	(2.02)	(2.04)
Lifetime funding (log)			0.61***
			(0.19)
Mean of DV	7.96	7.96	7.96
Oster δ		-9.11	4.99
R-squared	0.061	0.063	0.064
Panel C: D[Closure] ×100			
Accelerator VA (log)	9.69***	12.39***	15.12***
	(2.23)	(2.28)	(2.31)
Lifetime funding (log)			-2.45***
			(0.18)
Mean of DV	11.61	11.61	11.61
Oster δ		-3.98	-2.67
R-squared	0.093	0.125	0.130
Panel D: Log Patents, 5 years after accelerator			
Accelerator VA (log)	-0.02	-0.01	-0.08**
	(0.04)	(0.04)	(0.04)
Lifetime funding (log)			0.06***
			(0.01)
Mean of DV	0.14	0.14	0.14
Oster δ		0.53	-1.54
R-squared	0.131	0.178	0.203
Observations (A–C)		63,418	
Observations (D)		29,423	
Founding Year FE		Yes (22)	
State FE		Yes (52)	
Cohort FE		Yes (18)	
Industry FE		Yes (190/179)	
Sample FE		Yes (2)	
Startup controls	N	Y	Y

Note: This table presents additional outcome variables using the same two-step estimation procedure as Table 4. Panel A reports results for binary exit outcomes (IPO, successful acquisition, and closure), measured as percentages. Panel B shows additional results: log patents (restricted to startups with LinkedIn data) and closure results excluding successful exits. Columns 3-5 in Panel B demonstrate that the positive relationship between accelerator value added and closure is driven by startups that neither went public nor were successfully acquired, suggesting these closures represent actual failures rather than successful outcomes. All regressions include the same set of fixed effects and founder controls as described in the previous table. *** p<0.01, ** p<0.05, * p<0.1 *** p<0.01, ** p<0.05, * p<0.1

Table 8: Accelerator value added: Operating and valuation outcomes

	Employment up (dummy)	Max employment (3 yrs post accel.)	Max employment (10 yrs post accel.)	Max employment (10 yrs post accel.)
	(1)	(2)	(3)	(4)
Accelerator VA (log)	0.176***	0.236**	0.205***	0.112***
	(0.031)	(0.094)	(0.041)	(0.030)
Lifetime funding (log)				0.128***
				(0.004)
Mean DV	0.202	2.741	0.968	0.968
Observations	37,443	9,814	10,875	10,875
R-squared	0.180	0.645	0.496	0.587
N Founding Year FE	22	22	22	22
N State FE	52	51	52	52
N Accelerator Cohort FE	18	16	17	17
N Industry FE	190	156	160	160
N Evaluation year FE	.	16	16	16

	Revenue up (dummy)	Max revenue (3 yrs post accel.)	Max revenue (10 yrs post accel.)	Max revenue (10 yrs post accel.)
	(1)	(2)	(3)	(4)
Accelerator VA (log)	0.036***	0.854	1.128***	0.032
	(0.010)	(0.536)	(0.397)	(0.346)
Lifetime funding (log)				0.626***
				(0.041)
Mean DV	0.035	0.478	0.923	0.923
Observations	37,443	877	1,322	1,322
R-squared	0.054	0.465	0.473	0.582
N Founding Year FE	22	20	22	22
N State FE	52	37	41	41
N Accelerator Cohort FE	18	15	17	17
N Industry FE	190	87	106	106
N Evaluation year FE	.	16	16	16

	Valuation up (dummy)	Max valuation (3 yrs post accel.)	Max valuation (10 yrs post accel.)	Max valuation (10 yrs post accel.)
	(1)	(2)	(3)	(4)
Accelerator VA (log)	0.039***	1.675***	0.492***	0.210***
	(0.011)	(0.342)	(0.093)	(0.071)
Lifetime funding (log)				0.172***
				(0.007)
Mean DV	0.039	3.404	1.241	1.241
Observations	37,443	975	1,420	1,420
R-squared	0.054	0.613	0.539	0.716
N Founding Year FE	22	19	22	22
N State FE	52	39	41	41
N Accelerator Cohort FE	18	15	17	17
N Industry FE	190	89	109	109
N Evaluation year FE	.	16	16	16

Note: Panel A presents employment outcomes: Column (1) reports an indicator equal to one if a startup's post-program employment exceeds its pre-program valuation, or if a valuation is observed post-accelerator when none was recorded early-stage; Columns (2)-(3) show log maximum valuation observed within 3 and 10 years post-program; Column (4) additionally controls for lifetime funding of startups to address the concern that accelerator value-added may reflect differences in post-program fundraising and nothing else. Panel B presents revenue outcomes with analogous specifications. All regressions control for founding year, industry, accelerator cohort, city fixed effects, and startup characteristics including number of founders, share of serial entrepreneurs, female founders, MBA holders, technical degree holders, immigrants, mean university quality, LinkedIn data availability, founder experience, and pre-program patent applications. Sample excludes Crunchbase-only startups without valuation or employment information. Standard errors clustered at the accelerator cohort level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Evidence for learning

X=	Revenue			Employment			Valuation		
	(1) Up	(2) Max3	(3) Max10	(4) Up	(5) Max3	(6) Max10	(7) Up	(8) Max3	(9) Max10
Dep. var.: D[Closure]									
X × VA	-0.26***	-0.02	-0.04**	-0.14***	-0.07***	-0.24***	-0.25***	-0.01	-0.13
	(0.05)	(0.03)	(0.02)	(0.04)	(0.02)	(0.04)	(0.05)	(0.03)	(0.09)
X	-0.07***	-0.01**	-0.01***	-0.03***	-0.02***	-0.06***	-0.07***	-0.01	-0.04**
	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.02)
VA	0.13***	0.14**	0.03	0.13***	0.04*	0.08***	0.13***	0.12*	0.02
	(0.02)	(0.07)	(0.05)	(0.02)	(0.02)	(0.02)	(0.02)	(0.06)	(0.05)
Observations	37443	877	1322	37443	21176	26434	37443	977	1421
R^2	0.161	0.307	0.324	0.160	0.209	0.188	0.161	0.278	0.304
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Column mnemonics follow Table 8: *Up* = indicator equal to one if the outcome increases post-program; *Max3* = log maximum outcome within 3 years post-program; *Max10* = log maximum outcome within 10 years post-program. X denotes the startup outcome measure interacted with accelerator VA. Controls include number of founders, share of serial entrepreneurs, female founders, MBA holders, technical degree holders, immigrants, mean university quality, LinkedIn data availability, founder experience, and pre-program patent applications. Fixed effects include founding year, industry, accelerator cohort, and state. X denotes the startup outcome measure used to construct accelerator VA. Sample excludes Crunchbase-only startups without valuation or employment information. Standard errors clustered at the accelerator cohort level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 10: Accelerator value added: Fine–Gray competing exit risk estimates

	(1) Closure (CIF)	(2) Successful Exit (CIF)
Accelerator VA (log)	1.756*	4.932***
	(0.588)	(0.891)
ln_pre	1.240***	1.442***
	(0.034)	(0.020)
Number of founders	1.002	1.074***
	(0.026)	(0.014)
Serial entrepreneur	1.647***	1.400***
	(0.171)	(0.092)
Female founder	0.685***	0.693***
	(0.100)	(0.048)
MBA	0.954	1.088
	(0.128)	(0.077)
STEM founder	0.976	0.946
	(0.113)	(0.065)
Immigrant founder	0.979	0.826**
	(0.102)	(0.064)
University prominence	1.003	1.003*
	(0.003)	(0.002)
Founder info. missing	4.669***	1.388***
	(0.573)	(0.135)
Founder experience	0.980***	0.999
	(0.004)	(0.003)
N	62483	62483
Log-Likelihood	-44052.51	-45649.39

Note: This table reports Fine–Gray competing risks models for business closure and successful exit (IPO or acquisition). Closure and exit are treated as mutually competing events. Subhazard ratios (exponentiated coefficients) are reported, with standard errors in parentheses. Time is measured in years since accelerator entry. All models include industry sector, accelerator-entry-year, and founding-year fixed effects. Standard errors are clustered at the accelerator-class level. The sample is restricted to startups founded between 2001 and 2024 and observed for up to 20 years after accelerator entry. *** p<0.01, ** p<0.05, * p<0.1.

Table 11: Accelerator value added and startup matching

	(1)	(2)	(3)
DV: log accelerator value added x 100			
log early-stage funding	1.31***	1.24***	1.31***
	(0.15)	(0.14)	(0.15)
Number of founders	0.63***	0.50***	0.68***
	(0.12)	(0.10)	(0.12)
Serial entrepreneur	1.33***	0.78***	1.18***
	(0.33)	(0.29)	(0.32)
Female founder	-0.05	0.54*	-0.13
	(0.35)	(0.32)	(0.35)
MBA	-1.19***	-0.38	-0.87**
	(0.44)	(0.35)	(0.42)
STEM founder	0.87**	0.78**	1.05***
	(0.38)	(0.32)	(0.37)
Immigrant founder	0.38	-0.16	0.30
	(0.40)	(0.35)	(0.39)
University prominence	0.08***	0.08***	0.09***
	(0.01)	(0.01)	(0.01)
Founder experience	-0.05***	-0.03*	-0.06***
	(0.02)	(0.02)	(0.02)
early-stage patents	-0.03	-0.08	-0.04
	(0.05)	(0.05)	(0.05)
Founder info. missing	0.65*	0.87**	0.92**
	(0.39)	(0.37)	(0.38)
Raised series B early-stage	-5.05***	-3.06***	-4.83***
	(1.13)	(0.96)	(1.11)
Average equity stake (%)		1.26***	
		(0.24)	
Average accelerator deal (mil USD)		21.35***	
		(3.53)	
University or government affil.			-4.82***
			(1.11)
Corporate affil.			-0.54
			(1.84)
Observations	29289	29289	29289
R-squared	0.156	0.277	0.171
Founding year FE	Y	Y	Y
Industry FE	Y	Y	Y
Cohort FE	Y	Y	Y
State FE	Y	Y	Y

Note: The dependent variable is accelerator value added, defined as the log value of post-accelerator funding attributable to the accelerator treatment effect. The dependent variable is regressed on control variables, with standard errors clustered by accelerator–cohort. The sample includes accelerated startups only. *** p<0.01, ** p<0.05, * p<0.1

Online Appendices

Appendices

A Data appendix

A.1 Record linkage between Crunchbase and PitchBook

Crunchbase–PitchBook linkage To link firms across Crunchbase and PitchBook, we construct a probabilistic record linkage procedure that combines standardized firm identifiers, geographic information, and founding-year consistency. Our goal is to minimize false positives while retaining high coverage among U.S. startups founded after 2000 (Fellegi and Sunter, 1969; Lindsay et al., 2023). This procedure yields a linkage between Crunchbase and PitchBook that is robust to minor discrepancies in firm naming conventions and founding-year reporting, while minimizing the risk of spurious matches.

We begin by restricting both datasets to U.S.-based operating companies founded after 2000. From Crunchbase, we retain organizations whose primary role is "company" and whose country code is the United States. From PitchBook, we keep firms headquartered in the United States. We extract firm names, founding years, headquarters state, and ZIP codes where available.

Firm names are standardized by lowercasing, removing punctuation and non-alphanumeric characters, collapsing whitespace, and stripping common legal suffixes (e.g., "Inc.", "LLC", "Ltd."). ZIP codes are standardized to five-digit format, and U.S. states are harmonized to two-letter abbreviations. We allow ZIP codes to be missing but require non-missing firm name, founding year, and state.

Linkage is performed using the `Splink` package in Python with a DuckDB backend, implementing a probabilistic Fellegi–Sunter model. We generate candidate record pairs using multiple blocking rules of varying strictness, including combinations of state, ZIP code, standardized firm name prefixes, and founding year proximity. Specifically, candidate pairs are blocked on exact agreement in state and firm-name prefixes (first three or four characters), with additional rules incorporating ZIP codes and allowing founding years to differ by up to two years. This multi-stage blocking strategy balances computational feasibility with recall.

For candidate pairs, we compute similarity scores using string distance metrics (Levenshtein distance and Jaro–Winkler similarity) on standardized firm names, along with exact-match indicators for founding year and state. Model parameters are estimated via expectation–maximization (EM), with the non-match distribution initialized using random sampling of firm pairs.

We compute posterior match probabilities for all candidate pairs and retain matches with a predicted match probability of at least 0.5. To further reduce ambiguity, we impose a one-to-one matching constraint, retaining only the highest-probability match for each Crunchbase firm and each PitchBook firm. The resulting linked dataset therefore assigns at most one PitchBook identifier to each Crunchbase firm and vice versa.

As a final validation step, we further refine the matched sample using an independent string-similarity check. Specifically, after obtaining predicted match probabilities from the probabilistic linkage model, we recompute the Jaro–Winkler similarity between the standardized firm names from Crunchbase and PitchBook for all matched pairs.

We retain only matches with a predicted match probability above 0.7 and a Jaro–Winkler similarity exceeding 0.95. This additional filtering step serves as a conservative safeguard against residual false positives that may arise from coincidental agreement in location or founding year. The final linked dataset therefore consists of high-confidence matches that satisfy both probabilistic model-based criteria and stringent name-level similarity thresholds.

Linkedin linkage Following Jin et al. (2025), we use the LinkedIn URL of each startup to match startups to founder profiles. We match 175,219 out of 750,143 startups, a matching rate comparable to that reported in Jin et al. (2025). In the matched sample that restricts non-accelerated startups to those similar to accelerated startups, the share of startups with founder information increases to 53 percent.

A.2 Construction of sample startups

We provide explanations for the sample construction process. We retrieved PitchBook data from Wharton Research Data Services (WRDS) in July 2025 and Crunchbase data through an API in September 2025. Our goal is to construct a sample of U.S.-based startups affiliated with U.S.-based accelerators. We therefore exclude any startups associated with non–U.S.-based accelerators from the analysis.

PitchBook We start with 131,983 US-based startups founded between 2001 and 2024. We have 148,299 observations due to multiple accelerator participation. In these cases, startups appear as duplicate observations. More detail on multiple accelerators are provided in section A.8. We drop 63 observations whose acceleration year is before 2005. We drop 9,732 startups affiliated with non-US accelerators. We then drop 695 startups whose acceleration year is 2025, resulting in 137,809 startups in the PitchBook sample.

Crunchbase We start with US-based startups founded between 2001 and 2024 (732,233 startups). We drop startups affiliated with non-US accelerators according to CB (4,157 startups dropped). This results in 728,076 startups.

Merging PitchBook and Crunchbase We merge the PitchBook sample with Crunchbase sample. Among PitchBook 137,809 observations, 86,946 are merged with Crunchbase startups. This yields 793,059 startups. The number of observations increase due to multiple accelerator participation.

We drop accelerated startups that are tagged as accelerator-affiliated according to CB data among the CB-only sample. We then keep only accelerators with more than 30 alumni and at least two cohorts so that startups affiliated with small or short-history accelerators are all dropped. We drop startups with missing city or industry information.

This leaves a final sample of 749,968 startups, of which 30,653 are accelerated startups. They have 38,989 observations due to multiple accelerator participation. There are 329 accelerators in the final sample.

We summarize the above process in the following bullet points:

- PitchBook only sample (identify accelerator status using PitchBook)
  - Startups affiliated with US-based accelerators
    - Accelerators with more than 30 alumni startups (keep)
    - Accelerators with less than 30 or 30 startups (drop)
  - Startups affiliated with non-US based accelerators (drop)
- PitchBook and Crunchbase sample (identify accelerator status using PitchBook)
  - Startups affiliated with US-based accelerators (keep)
    - Accelerators with more than 30 alumni startups (keep)
    - Accelerators with less than 30 or 30 startups (drop)
  - Startups affiliated with non-US based accelerators (drop)
- Crunchbase only sample (identify accelerator status using Crunchbase)
  - Startups affiliated with non-US based accelerators (drop)
  - Startups affiliated with US-based accelerators (drop)[4]

[4] This is because of the issue we raised about accelerator tags in the Crunchbase data.

A.3 Harmonizing variables in Crunchbase and PitchBook

Industry harmonization To harmonize industry classifications across PitchBook and Crunchbase, we construct a data-driven industry crosswalk using startups that appear in both databases. We first restrict the sample to firms matched across PitchBook and Crunchbase and extract each firm's primary PitchBook industry code and its list of Crunchbase categories. We then split multi-category assignments in Crunchbase into individual category observations and compute the frequency with which each Crunchbase category co-occurs with a given PitchBook primary industry code. For each Crunchbase category, we assign the PitchBook industry code with which it most frequently co-occurs, resolving ties deterministically using alphabetical ordering.

A.4 Accelerator affiliation

We use PitchBook rather than Crunchbase for accelerator affiliation data. Crunchbase's coverage of accelerator affiliation is incomplete: prior studies have supplemented it with external accelerator data (Hallen et al., 2023), and our own validation confirms that accelerator tagging is less comprehensive in Crunchbase than in PitchBook, particularly for programs that do not provide seed funding (e.g., Google for Startups and Endless Frontier Labs). Because Crunchbase is organized around investment capital, accelerators offering only mentorship and advisory support without direct funding are systematically underrepresented.

Crunchbase also includes false positives in accelerator identification. For instance, it records Techstars as an investor in Uber and Y Combinator as an investor in OpenAI's pre-seed round. In reality, these reflect individual contributions—David Cohen of Techstars invested personally in Uber's angel round, and Jessica Livingston contributed to OpenAI through pledge member donations—neither of which constitutes accelerator involvement. PitchBook, by contrast, distinguishes clearly between accelerator rounds and other deal types, allowing us to avoid misclassifying companies like Uber or OpenAI as accelerator-backed when drawing on their broader investment histories.

PitchBook classifies accelerators and incubators together under the "Accelerator/Incubator" investor type. Incubators typically provide workspace and networking opportunities but lack the structured mentorship and fixed-term cohort structure that define accelerators. To address this issue, we refine the classification following the definition in Cohen (2014).

Our classification proceeds in three steps. First, we compile a list of investors whose primary or secondary investor types include the accelerator/incubator tag in the PitchBook investor file. Second, we automatically include all investors whose textual descriptions contain the terms "accelerator," "acceleration firm," or closely related expressions. Second, for those without these terms, we include the investor only if its description contains at least one defining feature of an accelerator: a fixed-term structure (e.g., "12-week," "three-month" program), a cohort-based design (e.g., "batch," "class," "program," "fellowship"), a mentorship component (e.g., "mentoring," "advisory," "training"), or bootcamp-style intensity (e.g., "boot camp," "demo day," "intensive"). We also exclude organizations that identify as venture studios. We restrict the sample to U.S.-based accelerators with at least 30 alumni startups and 2 cohorts following Yu (2020). Applying these filters yields 329 distinct U.S.-based accelerators.

We classify accelerator types using information available in the PitchBook. For equity-taking accelerators, we rely on the PercentAcquired variable in accelerator deals. If an accelerator acquires an equity stake in the startup, we classify it as an equity-taking accelerator. We then use the DealSize field to determine whether the accelerator provides seed funding. For example, MassChallenge provides seed funding but does not take equity, while the Founder Institute does not provide seed funding but is equity-taking. To take into account data entry errors in the PitchBook, we classify an accelerator as non–equity-taking or non–seed-funding if less than 5 percent of its accelerator deals involve equity or seed funding. Small shares of equity-taking or seed-funding deals can arise for reasons unrelated to the accelerator's true business model, such as miscoding in the data or co-investing alongside other accelerators or venture capital firms within the same funding round. In our data, several accelerators known to be non–equity-taking still show 1 to 2 percent of deals with reported equity acquisition, which we attribute as noise.

We also classify accelerators by organizational affiliation, distinguishing corporate, university-affiliated, and independent accelerators. Programs such as Google for Startups or Microsoft for Startups are categorized as corporate accelerators. Berkeley SkyDeck and Stanford StartX are examples of university-affiliated accelerators, and Y Combinator represents an independent accelerator. To systematize the classification, we embed accelerator website descriptions into a large language model and ask it to classify each program as corporate or university-affiliated based on its content. Then we manually validate each accelerator.

Missing deal years for accelerator participations are imputed using a forward and backward filling method based on the startup's transaction sequence. While PitchBook identifies accelerator-related transactions, the specific year of the deal is occasionally missing. We impute these missing values by leveraging the chronological sequence of a startup's funding history. Specifically, we first apply a forward-fill, assigning the year of the immediately preceding funding round to the missing deal. For cases where the initial rounds are missing dates, we subsequently apply a backward-fill, utilizing the year of the following transaction. For the remaining cases where deal years are entirely absent (2,526 firms out of 38,990 accelerated startups), we assign the founding year as the accelerator participation year.

A.5 Variable definitions and sources

Outcome variables Venture funding data are drawn from both Crunchbase and PitchBook. When funding information is available in both sources, we prioritize PitchBook data, for the reasons discussed in Section A.3. Information on IPOs, acquisitions, firm survival, and exits or closures is also obtained from both PitchBook and Crunchbase. Closures and IPOs are defined in a straightforward manner. We classify acquisitions as high-value exits when the acquisition price exceeds twice the sum of all pre- and post–Series B venture funding, including venture capital, angel funding, and grants.

Valuation and employment data are taken exclusively from PitchBook. Accordingly, regressions using these outcome variables are estimated on samples restricted to firms with available PitchBook data. Valuation measures correspond to post-money valuations as reported by PitchBook.

Patent data are obtained from Revelio Labs. As patent outcomes are only observed for firms that can be linked to the Revelio Labs dataset, regressions involving patent output are restricted to this matched sample.

Control variables We construct our founder-level dataset by extracting professional histories from Revelio Labs by searching for firms established from 2001 onward. To isolate the core entrepreneurial team, we identify individuals whose self-reported job titles in the Revelio records match a regular expression pattern for "founder" or "co-founder." We dropped data with implausible team sizes. For instance, the technology firm LinkedIn appears in the data with 63 (co)founders, reflecting a reporting anomaly rather than the actual founding team. To ensure our analysis is not distorted by these outliers, we exclude any startup that reports more than 10 co-founders.

For each identified founder, we systematically classify their educational background into different categories of human capital by leveraging the `field`, `field_raw`, `degree`, and `degree_raw` variables. First, we identify business expertise by flagging individuals whose `degree` or `degree_raw` description explicitly mentions an "MBA" or a "Master of Business Administration." Second, we define a founder as possessing a technical degree if their `field` or `field_raw` records contain terms explicitly linked to STEM disciplines, including engineering, information technology, mathematics, or medicine. This search pattern also incorporates core scientific disciplines such as physics, biology, chemistry, and nursing, as well as quantitative fields like statistics and data science.

To further enrich the profile of each founding team, we determine immigrant status and career seniority based on undergraduate records. We define a founder as an immigrant if their undergraduate institution is located outside of the United States. We also establish a founder's professional timeline by identifying the graduation year of their first undergraduate degree. Additionally, we identify serial entrepreneurs by analyzing the chronological work history within the Revelio profiles; a founder is classified as such if they have a recorded "founder" or "co-founder" role at a different firm prior to the establishment of the current startup.

To account for the variation in university prominence, we incorporate university quality scores from the Center for World University Rankings (CWUR) for the period 2012–2015. We map each founder's undergraduate institution to its corresponding CWUR global ranking score. In cases where a university appears in multiple years within the 2012–2015 window, we assign the latest available score to reflect the institution's most recent global standing. Universities not listed in the rankings are assigned a baseline score of 44, representing the empirical lower bound of the CWUR distribution. Experience of the founder is calculated as the gap between startup founding year and the undergraduate graduation year.

Because our unit of analysis is the startup, we aggregate these individual characteristics—MBA status, technical background, immigrant status, serial entrepreneurship, and university quality—by calculating the mean across all founders within a firm to create a team-level composition score.

Table A.1: Variable Definitions and Data Sources

Variable	Definition	Source
Outcome and Treatment Variables		
IPO	Indicator equal to one if the firm goes public.	PitchBook, Crunchbase
Successful acquisition	Indicator equal to one if the firm is acquired and the acquisition value exceeds twice the total amount of venture funding raised (venture capital, angel funding, and grants).	PitchBook, Crunchbase
Venture funding	Total amount of angel, grant, seed, pre-seed, and Series A or later rounds funding raised after accelerator entry. Accelerator deal funding is excluded.	PitchBook, Crunchbase
Valuation	Post-money valuation reported after accelerator participation.	PitchBook
Employment	Number of employees reported after accelerator participation.	PitchBook
Revenue	Firm revenue reported after accelerator participation.	PitchBook
Patent output	Patents filed by the firm after accelerator entry.	Revelio Labs
Accelerator affiliation	Indicator equal to one if the firm participates in a U.S.-based accelerator program, excluding venture studios and incubators.	PitchBook
Founder and Firm Characteristics		
Number of founders	Number of identified founders in the startup's founding team.	LinkedIn (Revelio Labs)
Share of serial entrepreneurs	Fraction of founders who previously held a founder or co-founder position at another firm.	LinkedIn (Revelio Labs)
Share of female founders	Fraction of founders who are female.	LinkedIn (Revelio Labs)
Share with MBA	Fraction of founders holding an MBA or Master of Business Administration degree.	LinkedIn (Revelio Labs)
Share with technical degree	Fraction of founders whose field of study is in a STEM discipline, including engineering, information technology, mathematics, medicine, or natural sciences.	LinkedIn (Revelio Labs)
Share of immigrants	Fraction of founders whose undergraduate institution is located outside of the United States.	LinkedIn (Revelio Labs)
Mean university quality	Average global ranking score of founders' undergraduate institutions based on the Center for World University Rankings (CWUR). Universities not listed in CWUR are assigned a baseline score of 44.	CWUR, LinkedIn (Revelio Labs)
Founder experience	Average number of years between each founder's undergraduate graduation year and the firm's founding year.	LinkedIn (Revelio Labs)
early-stage patenting	Number of patents granted up to accelerator entry year.	Revelio Labs
LinkedIn missing	Indicator equal to one if no founder profile can be matched in the LinkedIn (Revelio Labs) data.	LinkedIn (Revelio Labs)

A.6 Classification of verticals

Table A.2: Classification of PitchBook Verticals into Broad Industry Categories

Broad Category	**PitchBook Verticals**
Consumer Internet & Commerce	E-Commerce; Mobile Commerce; Gaming; Esports; Real Estate Technology
Financial Services & Investing	Impact Investing; FinTech; B2B Payments; Mortgage Tech; InsurTech; Cryptocurrency/Blockchain
Consumer Products & Services	Beauty; Cannabis; Pet Technology
Health, Life Sciences & BioTech	HealthTech; Digital Health; Life Sciences; Oncology; FemTech; LOHAS & Wellness; Wearables & Quantified Self
Information Technology & Software	Artificial Intelligence & Machine Learning; Big Data; CloudTech & DevOps; SaaS; Cybersecurity; Internet of Things; Mobile; Virtual Reality; Augmented Reality; Ephemeral Content; TMT; Marketing Tech; HR Tech; AdTech; Legal Tech; AudioTech; EdTech
Industrial, Manufacturing & Deep Tech	Manufacturing; Advanced Manufacturing; Industrials; 3D Printing; Robotics and Drones; Nanotechnology; Infrastructure; Construction Technology; Supply Chain Tech
Mobility, Transportation & Urban Tech	Mobility Tech; Ridesharing; Car-Sharing; Micro-Mobility; Autonomous Cars; Space Technology
Food, Agri & Environmental Tech	CleanTech; Climate Tech; AgTech; FoodTech; Restaurant Technology; Oil & Gas

A.7 PitchBook industry definitions

Table A.3: PitchBook Industry Taxonomy: Structure Overview

Level	Description	Approx. Count	Example
Sector	Top-level industry bucket	7	Information Technology
Group	Subsector grouping within Sector	$\sim$40	Software (within IT)
Code Name	Most granular category used in classification	$\sim$200	Vertical Market Software

Notes: PitchBook classifies firms hierarchically. Each startup is assigned to a *Sector*, then a *Group*, and finally to one or more granular *Code Name* categories. The table reports the taxonomy depth and approximate number of categories at each level. The examples correspond to common categories used in the PitchBook industry classification.

A.8 Multiple accelerator participation

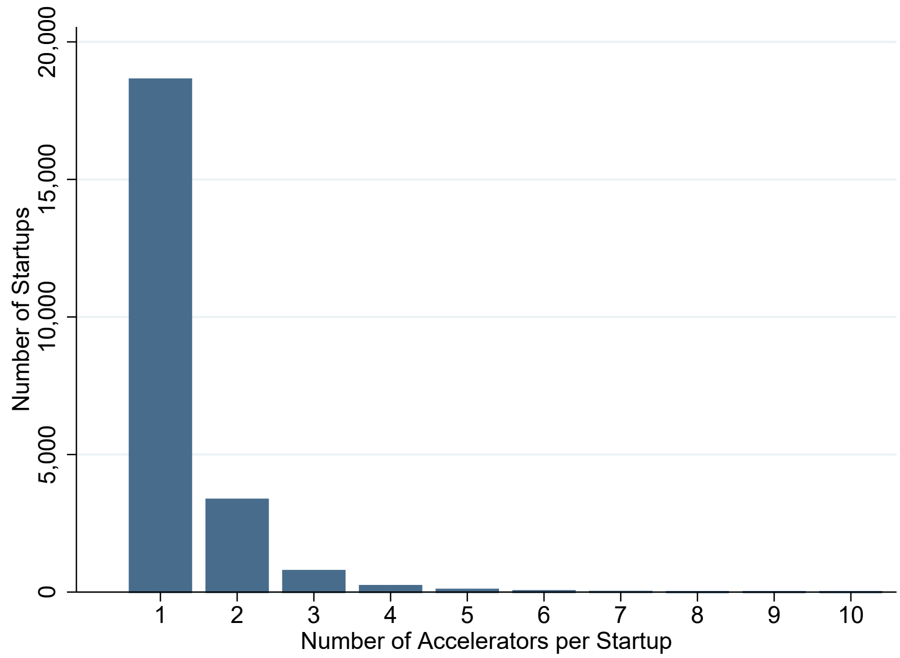


Figure A.1: Distribution of Accelerator Participation

Note: The sample includes 23,127 unique startups participating in 329 accelerator programs. Approximately 19.4% of startups participate in multiple accelerators (2+), and 4.9% participate in three or more accelerators (3–10). The average number of accelerators per startup is 1.27.

A.9 Funding source

Table A.4: Summary Statistics: Lifetime Funding by Source

Variable	Obs	Mean (million USD)	Std. Dev.	Min	Max
Grant Funding	63,037	0.056	4.915	0	1183.6
Angel Funding	63,037	0.235	3.476	0	481
VC Funding	63,037	2.464	12.689	0	1100

Notes: This table reports summary statistics for lifetime funding amounts by source. The sample corresponds to the main estimation sample used in Table 4, consisting of accelerated startups and entropy-matched control firms between 2005 and 2022. Funding variables are measured as cumulative amounts raised over the firm's lifetime. All values are expressed in millions of U.S. dollars.

B Additional results

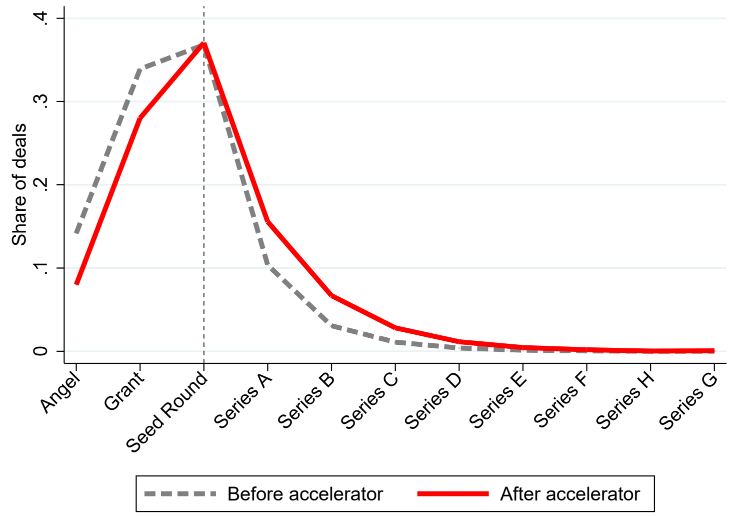
B.1 Life cycle of accelerated startups

We examine the data on the lifecycle of accelerated startups. Panel (a) of Figure B.1 presents a histogram of the distribution of financing rounds before and after startups' first accelerator entry among U.S. accelerated firms. The distribution exhibits a rightward shift following accelerator participation. Angel and grant funding become less frequent, while the incidence of seed rounds remains roughly unchanged. In contrast, Series A and later venture capital rounds become more likely after accelerator entry. Taken together, these patterns suggest that the transition from seed to Series A funding represents a critical stage in firm growth and frequently occurs after accelerator participation.

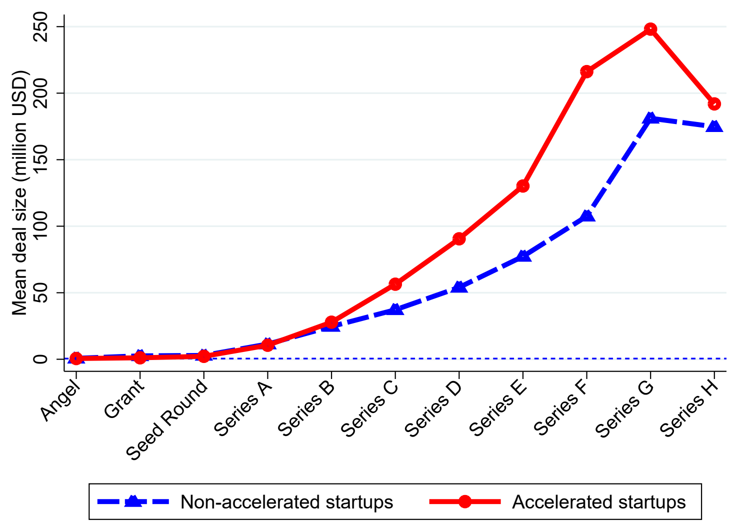
Panel (b) of Figure B.1 reports the mean deal size by financing round, comparing accelerated and non-accelerated startups when funding occurs. The figure shows little difference in deal size up to the Series A stage. However, a pronounced divergence emerges from Series B onward: accelerated startups that reach later-stage funding rounds raise substantially larger amounts than their non-accelerated counterparts. This pattern indicates that while accelerators may not affect the size of very early funding rounds, they are associated with larger financings at more advanced stages of the startup lifecycle. In this sense, our objective is to assess when accelerated and non-accelerated startups begin to diverge in terms of their average deal size. Funding outcomes up to the Series A stage appear sufficiently early in the firm lifecycle that, in the aggregate data, they are largely indistinguishable between accelerated and non-accelerated startups. By contrast, progression to seed or Series A funding represents a critical milestone for accelerated startups.

Figure B.1: Financing structure and deal size around accelerator entry

(a) Financing round composition before and after accelerator entry



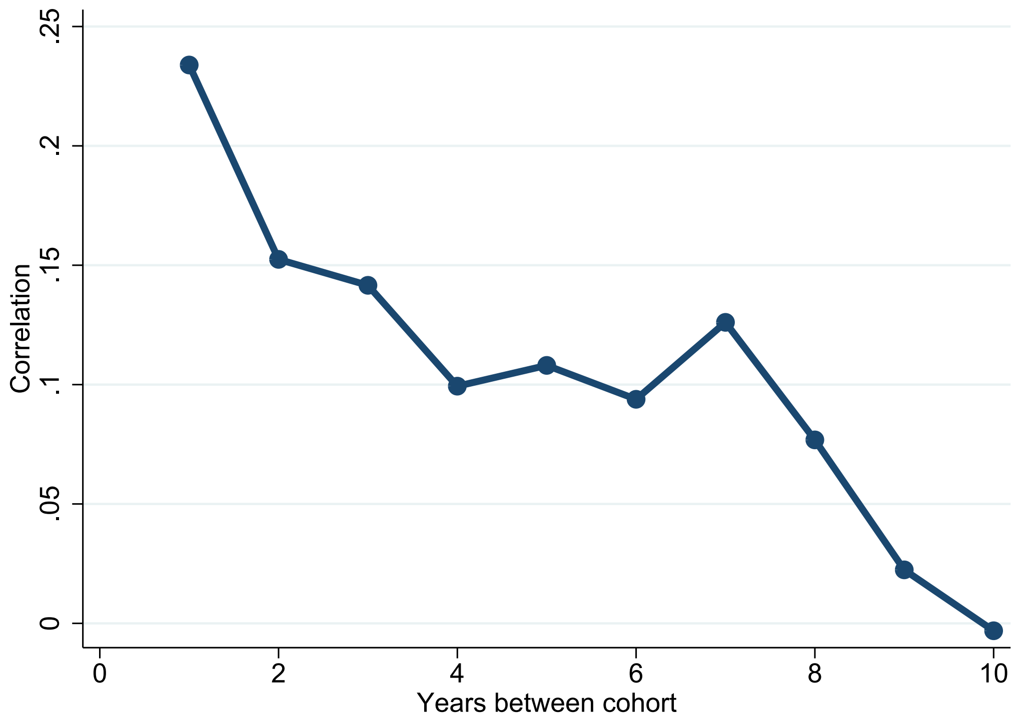
(b) Mean deal size across financing rounds



Note: Figure (a) presents a histogram of the distribution of financing round types surrounding firms' first participation in an accelerator. Figure (b) plots the mean deal size across different financing rounds. The data are drawn from PitchBook and are restricted to U.S.-headquartered firms founded after 1991. For Figure (a), the sample includes deals from 2001 onward for firms with at least one recorded accelerator deal. Accelerator entry is defined as the first financing event classified as "Accelerator/Incubator." For each firm, deals are classified as occurring before or after this initial accelerator deal based on the deal sequence number. For each financing round type, the figure reports the share of total deals in that category among all deals observed before (dashed line) and after (solid line) accelerator entry, aggregated across firms. For Figure (b), the sample includes all U.S.-based startups, regardless of accelerator participation, and is divided into two groups based on accelerator status. Mean deal size is computed conditional on a financing round occurring; startups that do not raise capital in a given round (e.g., firms without a Series C round) are not included in the calculation of the mean for that round.

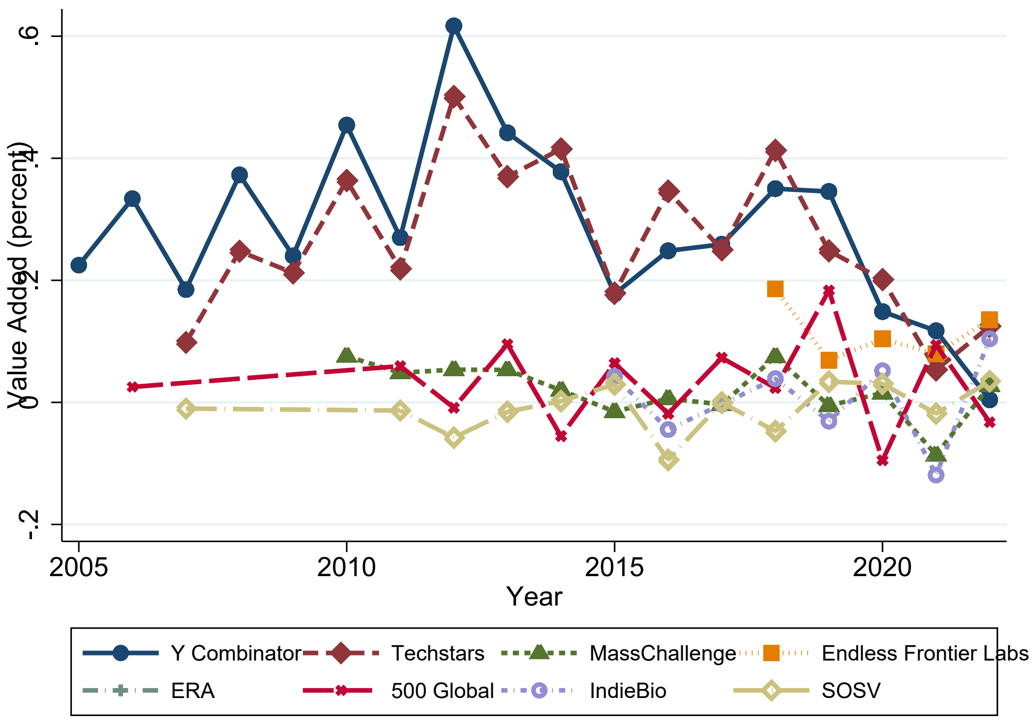
B.2 Correlation in value added across years

Figure B.2: Drift in accelerator value added across years



Note: The figure shows the correlation between mean post-accelerator venture funding residuals across startups taught by the same accelerator in different years. To calculate these correlation, we residualize post-accelerator funding using within-accelerator variation with respect to our baseline control variables. We then calculate a precision-weighted mean post-accelerator funding across accelerator cohorts for each accelerator-year. We calculate the autocorrelation coefficients as the correlation across years for a given accelerator, weighting by the sum of startups taught in the two years.

Figure B.3: Trend in value add estimates



Note: The figure shows the trend in the value add estimates of select accelerators. The average value across all cohorts are displayed in Panel (b) of Figure 5.

B.3 Forecast unbiasedness under different time horizons

Table B.1: Forecast unbiasedness under different time horizons

	X years=				
	1 year	2 years	3 years	4 years	5 years
	(1)	(2)	(3)	(4)	(5)
	DV: log early stage financing within *X years* of joining accelerator				
Accelerator VA (log)	1.30***	1.05***	1.05***	1.00***	0.99***
	(0.11)	(0.09)	(0.07)	(0.06)	(0.06)
Observations	63418	63418	63418	63418	63418
R-squared	0.03	0.06	0.07	0.08	0.09
N Founding Year FE	22	22	22	22	22
N State FE	52	52	52	52	52
N Industry FE	190	190	190	190	190
N Accel. Cohort FE	18	18	18	18	18
N Sample FE	2	2	2	2	2

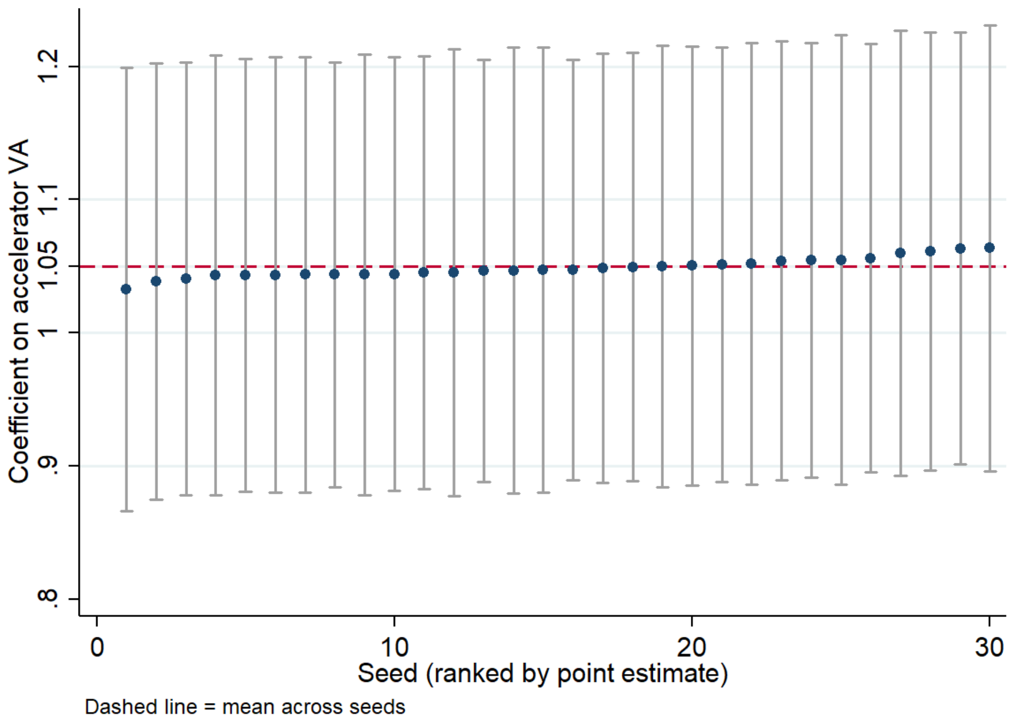
Note: This table reports a two-step estimation of accelerator value added. In the first step, we estimate accelerator by cohort-specific value added using a teacher value-added model (VAM), where post-entry outcomes are residualized with respect to early-stage funding, founder characteristics, and fixed effects (Chetty et al., 2014a; Stepner, 2013). In the second step, we regress residualized log early-stage financing on the estimated accelerator value added. Each column reports the same regression specification but with different dependent variables, defined as log early-stage financing accumulated within 1 to 5 years after joining an accelerator. Control variables include early-stage funding, founder team composition (number of founders, shares of serial entrepreneurs, female founders, MBA holders, STEM degree holders, and immigrants), founder experience, early-stage patenting activity, university prominence, and an indicator for missing founder information. All specifications include founding-year, state, industry, accelerator-cohort, and sample fixed effects. Standard errors are clustered at the class level. A coefficient close to one implies forecast unbiasedness, indicating that the estimated accelerator value added provides an unbiased predictor of subsequent early-stage financing outcomes.

Table B.2: Inclusion of pre-accelerator funding growth rate

	(1)	(2)
DV:	log funding 2 years after accelerator	
Accelerator VA (log)	1.06***	1.09***
	(0.09)	(0.09)
Observations	63,419	63,419
R-squared	0.05	0.05
Industry FE	Y	Y
Founding year FE	Y	Y
Cohort FE	Y	Y
State FE	Y	Y
Sample FE	Y	Y
Startup controls	Y	Y
Prior funding	Y	Y

Note: This table reports a two-step estimation of accelerator value added (VA). In the first step, we estimate accelerator-by-cohort VA using a teacher value-added framework. In Column (1), VA is estimated controlling for pre-entry funding levels and fixed effects (industry, founding year, state, and sample). In Column (2), we additionally control for the pre-entry funding growth rate when estimating VA. In the second step, we regress residualized log funding accumulated within two years after accelerator entry on the estimated accelerator VA. The residualization includes the same set of startup-level controls and fixed effects as in the baseline specification: prior funding, founder characteristics (team size, shares of serial entrepreneurs, female founders, MBA holders, STEM degree holders, immigrants), founder experience, early patenting activity, university prominence, an indicator for missing founder information, and fixed effects for founding year, state, industry, accelerator cohort, and sample. Standard errors are clustered at the class level. A coefficient close to one implies forecast unbiasedness, indicating that the estimated accelerator VA provides an unbiased predictor of subsequent early-stage financing outcomes.

Figure B.4: Seed sensitivity across 30 Seeds



Note: This figure reports a seed-sensitivity analysis of the estimated coefficient on accelerator value added. After constructing entropy-balancing weights to match accelerated and non-accelerated startups on pre-accelerator characteristics, control observations are integerized through stochastic rounding. Because this rounding step depends on random seeds, we repeat the entire estimation procedure 30 times using different seeds. In each replication, we re-expand the weighted sample, re-estimate accelerator VA using the VAM procedure, and regress the resulting accelerator score on estimated VA and pre-accelerator outcomes with controls. Dots report the estimated coefficient on accelerator VA from each replication, while vertical bars denote 95% confidence intervals. The dashed horizontal line indicates the mean estimate across all seeds.

B.4 Dummy variable specification

	(1)	(2)
DV: log funding 2 years after accelerator		
D[Accelerated]	-0.05	-0.18***
	(0.04)	(0.03)
D[Accelerated by high VA]		0.36***
		(0.03)
Observations	63,433	63,433
R-squared	0.25	0.26
Controls	Y	Y

Note: This table follows the same specification as Table 4, but replaces the continuous accelerator value-added measure with dummy variable specifications. Column (1) includes a single indicator for whether the startup was accelerated. Column (2) additionally includes an indicator for whether the startup was accelerated by a program whose estimated value added exceeds the average outcome among non-accelerated startups, allowing the effect of participation to vary by program quality. All accelerator value-added estimates used to construct this classification are based on early-stage financing within two years of joining the accelerator. All specifications include founding year fixed effects (22), U.S. state fixed effects (52), accelerator cohort fixed effects (18), industry fixed effects (192), and sample fixed effects (2). Founder controls include the total number of founders and the shares of the team who are serial entrepreneurs, female, MBA holders, STEM degree holders, or immigrants, as well as measures of founder experience and university prominence. For startups where LinkedIn information was unavailable, missingness is accounted for by the inclusion of a missing information indicator. *** p<0.01, ** p<0.05, * p<0.1

B.5 Bias correction

Recall from Section 5.1 that if accelerator value added is forecast-unbiased but biased at the accelerator level, then

$$\mathrm{Var}(\omega_j) = -\mathrm{Cov}(\mu_j, \omega_j) > 0,$$

where ω_j denotes accelerator-level bias and μ_j the true value added. Thus, accelerator-level bias can coexist with forecast-unbiasedness only if estimation errors are negatively correlated with true effects, implying systematic shrinkage toward the mean.

Suppose we observe an external benchmark estimate $\hat{\mu}_j^Q$ for a subset of accelerators using quasi-experimental methods, with

$$\hat{\mu}_j^Q = \mu_j + \varepsilon_j^Q, \qquad Var(\varepsilon_j^Q) = V_j^Q,$$

Suppose we have a sufficient number of external benchmark estimate from different accelerators. We could estimate the calibration regression

$$\hat{\mu}_j^{VA} = \gamma + \theta\, \hat{\mu}_j^Q + \eta_j.$$

Under forecast-unbiasedness and in the absence of systematic shrinkage, the calibration slope should satisfy $\theta = 1$. To see why $\theta < 1$ constitutes a test for shrinkage, note that

$$\theta = \frac{\mathrm{Cov}(\hat{\mu}_j^{VA}, \hat{\mu}_j^Q)}{\mathrm{Var}(\hat{\mu}_j^Q)} = \frac{\mathrm{Cov}(\mu_j + \omega_j,\ \mu_j + \varepsilon_j^Q)}{\mathrm{Var}(\mu_j) + V_j^Q} = \frac{\mathrm{Var}(\mu_j) + \mathrm{Cov}(\mu_j, \omega_j)}{\mathrm{Var}(\mu_j) + V_j^Q}.$$

Because $\mathrm{Cov}(\mu_j, \omega_j) = -\mathrm{Var}(\omega_j) < 0$, accelerator-level bias reduces the numerator relative to the denominator.

A finding of $\theta < 1$ may reflect (i) attenuation due to measurement error in $\hat{\mu}_j^Q$ (V_j^Q) or (ii) over-dispersion of VA estimates due to accelerator-level bias (ω_j). Importantly, benchmark precision is typically heterogeneous across accelerators because applicant sample sizes and research designs differ. This heterogeneity can be leveraged in future work: under classical attenuation, estimated slopes should move toward one as benchmark variances V_j^Q shrink, whereas systematic shrinkage in VA estimates would persist even among high-precision benchmarks. Accordingly, researchers can (i) implement precision-weighted calibration using observed V_j^Q and (ii) conduct sensitivity analyses restricting attention to benchmarks with small $SE(\hat{\mu}_j^Q)$ to assess whether $\theta < 1$ remains after minimizing attenuation. Interpreting $\theta < 1$ as shrinkage therefore requires either sufficiently precise benchmarks or an explicit correction for measurement error. When this condition holds, a bias-corrected VA estimate is

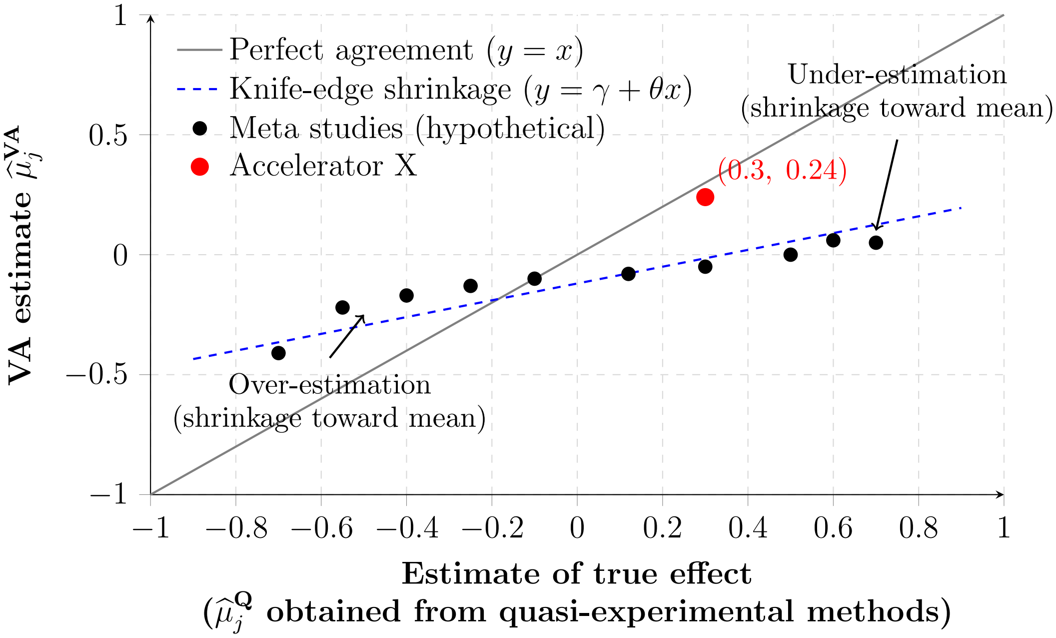
$$\tilde{\mu}_j^* \equiv \frac{\hat{\mu}_j^{VA} - \gamma}{\theta}.$$

Figure B.5 illustrates this idea in a hypothetical scatter plot. This framework shifts attention from case-by-case agreement to the slope θ, which aggregates validation evidence across accelerators. If $\theta < 1$, disagreement must take a mean-reverting form consistent with knife-edge shrinkage, and the appropriate correction.

We acknowledge that this applicant design is local: it identifies the effect of admission for startups near the margin of selection in Accelerator X and may not generalize to all applicants or to other programs. Moreover, admission is not literally randomized; our inference relies on rich controls and balance within the finalist pool. For these reasons, we treat the applicant evidence as validation and benchmarking rather than as the sole identification strategy.

Nevertheless, taken together, the applicant validation supports interpreting AVA as more than a proxy for selection. The evidence suggests that at least for a large non-equity accelerator, program participation causally increases short-run fundraising and shifts longer-run outcomes in the direction predicted by the market-wide AVA estimates. This triangulation strengthens the case for focusing on heterogeneity across accelerators and for treating accelerator choice as an intermediary-choice problem in entrepreneurial finance.

Figure B.5: Knife-edge mean reversion in accelerator-level validation



Note: Each dot represents an accelerator for which both quasi-experimental value added estimates μ_j obtained from independent validation studies, and a VA estimate $\widehat{\mu_j}$ from VA model (Chetty et al., 2014a) are available. The solid line indicates perfect agreement ($y = x$). The dashed line illustrates a knife-edge shrinkage pattern in which high-effect accelerators are underestimated (points fall below the $y = x$ line) and low-effect accelerators are overestimated (points fall above the $y = x$ line) by the VA model. The red point highlights the accelerator X estimates from this paper: one from VA model $\hat{\mu}_X^{\mathrm{VA}} = 0.24$ (million USD) and the other one from quasi-experimental methods $\hat{\mu}_X^{\mathrm{Q}} = 0.3$ (million USD). Underestimation indicates that the true accelerator value added (denoted by the black dots) is understated by the value-added model, as the dots lie to the right of their corresponding model-based estimates.

B.6 Linear probability hazard models

Table B.3: Linear probability hazard models

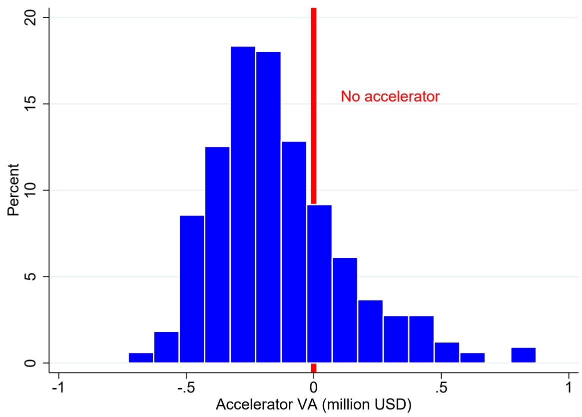
	(1)	(2)	(3)	(4)	(5)	(6)
	LPM of Failure			LPM of Successful Exit		
	3-Year	5-Year	7-Year	3-Year	5-Year	7-Year
Accelerator VA (log)	0.03***	0.04***	0.06***	0.05***	0.11***	0.14***
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)
Observations	63,418	63,418	63,418	63,418	63,418	63,418
R-squared	0.04	0.08	0.09	0.03	0.03	0.04
Industry FE	Y	Y	Y	Y	Y	Y
Founding year FE	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Sample FE	Y	Y	Y	Y	Y	Y
Startup controls	Y	Y	Y	Y	Y	Y
Mean DV	0.0220	0.0438	0.0588	0.0206	0.0401	0.0575

Note: The dependent variables are horizon-specific indicators of whether a startup experiences a given first event within x years of accelerator entry, where $x \in \{3, 5, 7\}$. For each horizon x, we restrict the sample to the corresponding risk set of startups that can be observed for at least x years, i.e., startups founded in accelerator entry year $\leq 2024 - x$. We define the event year as the earliest of (i) the closure year, (ii) the successful-exit year, and (iii) the censoring year 2024. The variable $\text{Failed}_{i,x}$ equals one if the earliest event is closure and occurs within x years of accelerator entry, and zero otherwise. Similarly, $\text{Exit}_{i,x}$ equals one if the earliest event is a successful exit (IPO or acquisition) and occurs within x years of accelerator entry, and zero otherwise. Thus, successful exits are treated as competing events in the failure specification (and vice versa) through the first-event definition. A successful exit is defined as either an IPO or an acquisition in which the acquisition price exceeds twice the total funding raised. As in Table 4, the outcome variables are residualized with respect to cohort, founding-year, state, and industry-code fixed effects. Standard errors are clustered at the accelerator-cohort level.
*** p<0.01, ** p<0.05, * p<0.1

B.7 Estimating value added with different time horizons

Figure B.6: Distribution of accelerator value added

(a) Early funding (post 3 yr)



(b) Early funding (post 5 yr)

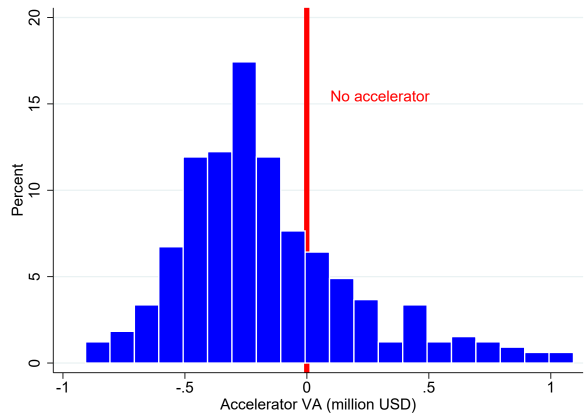
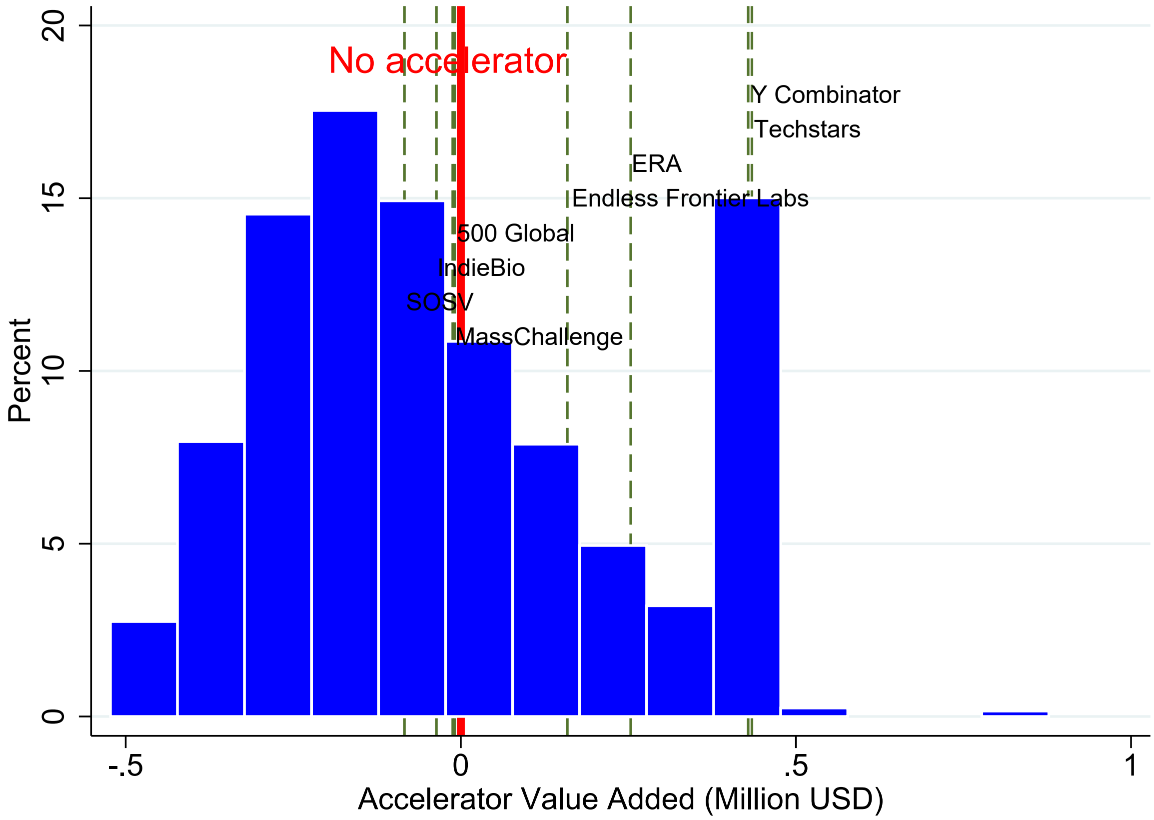


Figure B.7: Distribution of accelerator value added (weighted by startups)



Note: The value added by accelerators is estimated based on their predicted impact on early-stage funding within a two-year window following program entry. Accelerator value added is reported at the accelerator–cohort level. To obtain accelerator-level estimates, accelerator value added is averaged across all available cohorts for each accelerator. Accelerator value added is originally calculated as the log value of post-accelerator funding attributable to the accelerator treatment effect, and is then converted into dollar terms by multiplying the mean post-accelerator funding by the exponentiated log accelerator value added. The histogram is weighted by the total number of startups in each accelerator. 60 percent of accelerated startups was found to be lower than that of their non-accelerated counterparts.

B.8 Value added by vertical and state

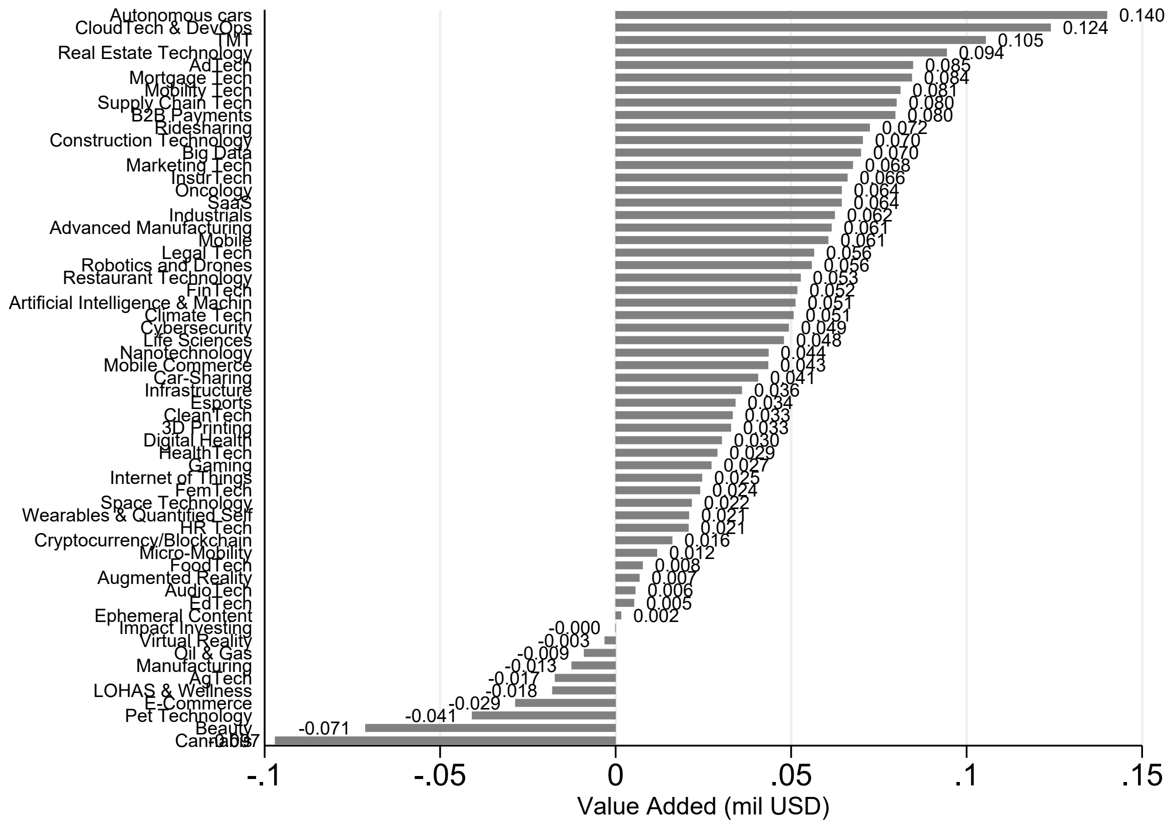


Figure B.8: Accelerator value added by startup vertical

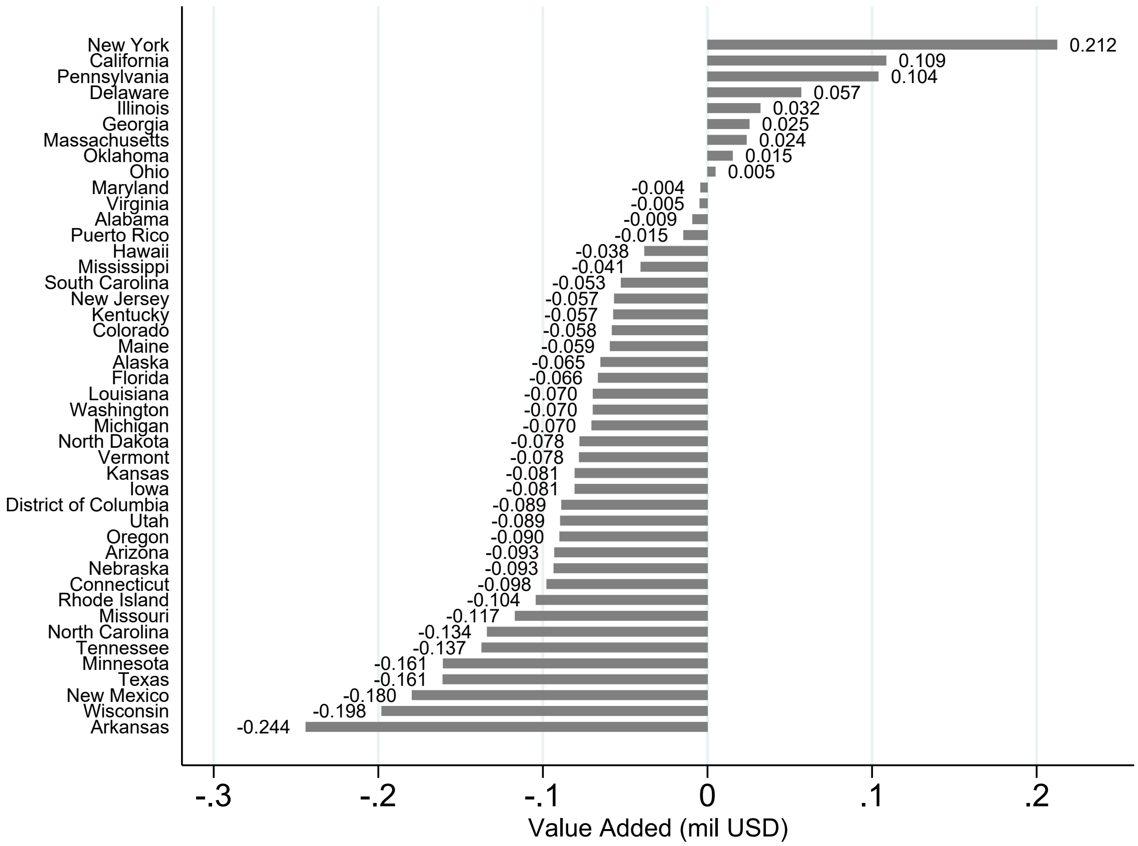
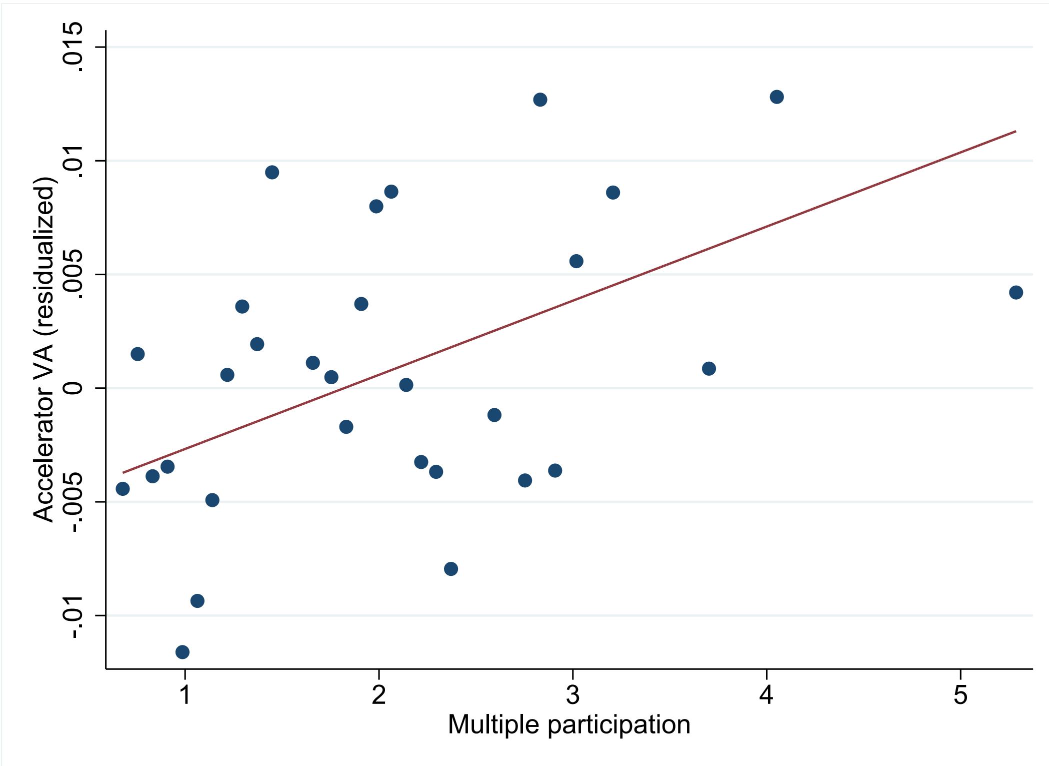


Figure B.9: Accelerator value added by accelerator state

B.9 Value added by multiple accelerator participation

Figure B.10: Value added over multiple accelerator participation



Note: This figure focuses on startups that entered multiple accelerator programs. We order accelerator participations chronologically within firm and residualize accelerator value added with respect to accelerator-entry year and firm fixed effects. The binscatter plot shows a positive relationship between residualized accelerator value added and the participation order, indicating that later accelerator participation is associated with higher estimated value added. Note that this figure differs from the serial entrepreneurs sample.

B.10 Hazard model: IPO and acquisition

Table B.4: Cox Proportional Hazard Models: Closure, IPO, and Acquisition

	Closure	IPO	Acquisition
	(1)	(2)	(3)
Accelerator VA (log)	1.821*	1.667	6.178***
	(0.648)	(0.813)	(1.269)
log early-stage funding	1.215***	1.791***	1.321***
	(0.034)	(0.064)	(0.017)
Number of founders	1.016	1.008	1.095***
	(0.021)	(0.059)	(0.014)
Serial entrepreneur	1.750***	1.225	1.483***
	(0.155)	(0.258)	(0.100)
Female founder	0.696***	0.914	0.662***
	(0.096)	(0.231)	(0.056)
MBA	0.969	1.086	1.073
	(0.109)	(0.282)	(0.072)
STEM founder	1.033	1.532*	0.941
	(0.108)	(0.355)	(0.067)
Immigrant founder	0.981	1.036	0.823**
	(0.095)	(0.205)	(0.063)
University prominence	1.004	1.002	1.004*
	(0.003)	(0.006)	(0.002)
Founder info. missing	4.883***	0.826	1.638***
	(0.654)	(0.173)	(0.172)
Founder experience	0.979***	1.011	0.996
	(0.004)	(0.009)	(0.003)
N	61118	61118	61118
Log-Likelihood	-47263.12	-2987.72	-46295.46

Note: Exponentiated coefficients; Standard errors in parentheses All models include industry and founding-year fixed effects. Standard errors clustered at accelerator-class level. Hazard ratios reported. *** p<0.01, ** p<0.05, * p<0.1