# Spillover Effects of Startup Accelerator Programs: Evidence from Venture-Backed Startup Activity*

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April 20, 2019

We examine the spillover effects of seed accelerator programs—fixed-term, cohort-based educational and mentorship programs for startups—on venture-backed seed and early stage technology startup activity in the regions in which they locate. Accelerators are often opened with an eye towards galvanizing technology entrepreneurship activity in the region through provision of peer effects/role models. We use a difference-in-differences approach that utilizes the staggered introduction of such programs combined with matching methods and synthetic control methodologies to assess the impact of an accelerator's arrival on the volume of seed and early stage VC deals completed in the region, excluding the accelerator's own portfolio companies. The arrival of an accelerator is associated with a significant increase in the volume of seed and early stage deals external to the accelerator cohorts; an increase driven both by outside investor groups and the emergence of new local early-stage investors, and supporting the notion that an accelerator can lead to peer effects and provision of role models in the ecosystem that encourage additional local entrepreneurial spillover activity. The findings suggest that the introduction of such programs can have a general effect on the equilibrium of the regions in which they locate, rather than merely an effect of treatment on the treated, and suggests a role for accelerator programs in galvanizing latent regional interest in entrepreneurial activity.

* We thank Jean-Noel Barrot, Eric Floyd, Naomi Hausman, Steve Kaplan, Joshua Krieger, Daniel Lee, Josh Lerner, Fiona Murray, Ramana Nanda, Scott Stern, Juan Carlos Suarez Serrato and seminar participants at Boston College, MIT, MIT Innovation Lab Conference, Duke University, the NBER IPE Conference, Hebrew University, McGill University, University of Virginia Darden, University of Amsterdam, Tilburg University, Erasmus University, SXSW Interactive, the NBER-AIEA Annual Meeting, and the American Economic Association annual meetings for helpful comments and suggestions. This research is funded under NSF grant number 1462008. Both authors are grateful for broader financial support from the Kauffman Foundation. Hochberg is grateful for funding from the Batten Institute at the University of Virginia. Fehder is grateful for funding from Skolkovo Technical University. Both authors are research affiliates of the MIT Innovation Initiative. Please direct correspondence to hochberg@rice.edu (Hochberg) or fehder@marshall.usc.edu (Fehder). All errors are our own.

## 1. Introduction

A large literature documents the geographic concentration of entrepreneurial activity (Glaeser and Kerr, 2009; Glaeser et al., 2010; Chatterji et al., 2014). This geographic clustering of entrepreneurial activity leads to wide disparities in regional economic growth (Acs and Armington, 2006). The roots of this clustering have been attributed to both Marshallian factors (e.g. labor pooling, knowledge spillovers) as well as to social mechanisms such as peer effects and culture (Glaeser and Kerr, 2009). Increasingly, policy makers have sought to generate interventions designed to overcome shortcomings that may deter the growth of such regional entrepreneurial clusters. One popular regional intervention in recent years is the accelerator model. These "startup bootcamps" concentrate a cohort of entrepreneurial ventures, and provide resources and mentorship critical to the entrepreneurial production function to a small group of startups. While the proliferation of accelerator programs targeted towards regional economic development has been rapid, evidence on the role and efficacy of these programs as regional interventions is scant. Accelerator programs are relatively small in scale, with an average of around twelve startups per cohort (Cohen et al., 2019). Thus, for such programs to be relevant in shifting the capacity of regions to foster high growth ventures, they must be expected to engender broader spillover effects on the regions in which they are housed. For policy makers and economists alike, a critical question is whether accelerator programs can indeed produce local entrepreneurship spillovers.

In this paper, we attempt to assess the impact that accelerator programs can have on the entrepreneurial ecosystem of the regions in which they are established, and whether they indeed produce spillovers that lead to proliferation of high-growth, technology-based entrepreneurial activity in the local area. Contemporaneous studies have found significant treatment impacts for accelerator participants for specific individual programs (e.g. Gonzalez-Uribe and Leatherbee, 2016, Fehder, 2018), primarily attributed to education or to social factors such as peer effects and increased social capital: since entry into entrepreneurship is heavily influenced by an individual's estimates of their potential success (Manso, 2016), social processes that influence the formation of these subjective probability estimates are critical to the entrepreneurial process. Unsurprisingly, prior studies have found significant spillovers to entrepreneurial peers in terms of fostering new entrepreneurial entry (Giannetti and Simonov, 2009; Markussen and and Røed, 2017). Peer effects and social capital, however, would not necessarily be limited to the individual participants of accelerator programs. Accelerator programs may provide peer effects and role models for entrepreneurs in the region in which they locate more generally; this, in fact, is a primary justification cited by policy makers in establishing and funding such endeavors in their regions.[1] These social channels quickly attenuate with geographic distance, and thus, local interventions may be important for facilitating such effects. Consistent with these motivations, we present evidence that suggests that the establishment of an accelerator in regions typically not considered to be clusters of entrepreneurial activity is associated with substantial regional startup activity spillover effects.

The accelerator programs we examine are fixed-term, cohort-based, “boot camps” for startups that offer educational programming, expose startup founders to wide variety of mentors and investors, and culminate in a pitch event, or “demo day” (Cohen et al, 2018). Recent estimates of the number of accelerators range from 300+ in the U.S. to over 2000 worldwide, spanning six continents, and the number is growing rapidly (Cohen and Hochberg 2014). Ex ante, whether such programs can have significant impact in developing new entrepreneurial clusters is unclear. Brad Feld, a founder of Techstars, one of the earliest accelerator programs, describes his rationale for its establishment "*as an experiment to help create more early-stage startups in Boulder*."[2] In subsequent writing, Feld has suggested that accelerators are important tools for enhancing the entrepreneurial capacity of regions and thus should be launched in nearly every city (Feld, 2012).

In contrast, Paul Graham, the founder of Y Combinator, the first and perhaps most well-known accelerator program, argues that "*[p]eople sometimes think they could improve the startup scene in their town by starting something like Y Combinator there, but in fact it will have near zero effect. I know because Y Combinator itself had near zero effect on Boston when we were based there . . . People came from all over . . . and afterward they went wherever they could get more funding—generally Silicon Valley*".[3] In Graham’s view, if this happened in Boston, which at the time had the second highest allocation of venture capital in the world, this same concern would be even more pronounced in regions with less activity than Boston. Can an accelerator truly serve to attract startups (and investors) to a city, beyond its own portfolio companies? Can it encourage and retain the talent required to build a venture worth funding? These conflicting views highlight two

[1] Based on interviews with a variety of accelerator founders and surveys of accelerator founders conducted by the Seed Accelerator Rankings Project and the media, see for example Ryan (2010) on the attempt to catalyze entrepreneurship in Boston’s Seaport District through support of MassChallenge, a local accelerator.

[2] http://www.feld.com/archives/2013/04/government-shouldnt-be-in-the-accelerator-business.html

[3] http://paulgraham.com/maybe.html

differing views on whether regional development is a reasonable economic rationale for establishing accelerators.

Accordingly, we focus our efforts on the capacity of startup accelerators to facilitate increased entrepreneurial ecosystem activity in relatively low-intensity entrepreneurship regions, where the growth rationale motivating the establishment of accelerators is more pressing. To measure spillovers, we wish to examine measures of startup activity that are not attributable to participants in the accelerator program itself. As noted by Guzman and Stern (2015), common measures of new business creation such as new business starts do not properly capture the type of high-growth entrepreneurial startup companies which policy makers are trying to encourage when establishing an accelerator. We therefore use VC-backed companies as a proxy for technology-oriented high growth potential entrepreneurial activity, and focus on seed and early stage deals as a proxy for new startup creation. To capture the spillover effects on high-growth potential technology-oriented entrepreneurship, the primary regional outcome measure we employ is the volume of seed and early stage venture capital (VC) deals in the region, excluding any investments in startups that participate in the accelerator program.

That said, measuring the impact of accelerators on regional-level startup activity is complicated by the fact that there is no source of *guaranteed* exogenous variation in the location of accelerators. Importantly, however, there is considerable anecdotal and survey evidence suggesting that the locational choices of many accelerators are rooted in the birthplace of founders who found success elsewhere and returned home hoping to help their hometowns (Hallen et al., 2014, Seed Accelerator Rankings Project Surveys 2013-2017), rather than in selection of regions poised for entrepreneurial growth. In support of this anecdotal and survey evidence, we show that the accelerators we study are founded to encourage economic development, rather than primarily because entrepreneurial growth was already expected in the region. Using data on accelerator founding reasons obtained from the Seed Accelerator Rankings Project (SARP), we document that 91.6% of the accelerators in the regions we study state that they were founded with a primary objective of "ecosystem building" or "regional development." This contrasts sharply with the objectives stated by founders of accelerators in traditional, established entrepreneurial clusters such as San Francisco or Boston, nearly all of whom state their primary objective as "return on invested capital."

Second, we document that, in contrast to the founders of accelerators in established entrepreneurial clusters, accelerator founders in the regions in our sample typically have a long-term connection to the region in which they open an accelerator; in particular, such founders are extremely likely to have attended high school in the MSA in which they launch the program. This considerably restricts the potential set of choice models to describe the selection of MSAs in which to establish an accelerator in our sample, as they seem to be opened by local talent. Together, these two patterns serve to help alleviate immediate concerns about the endogeneity of which regions receive accelerators.

We then mimic the approaches of other studies faced with similar program evaluation settings with potentially non-random staggered entry (e.g. Autor, 2003). First, we carefully match Metropolitan Statistical Areas (MSAs) that are 'treated' with an accelerator program to other MSAs that are very similar in terms of pre-treatment levels, growth, and trends in VC-backed startup activity. We then exploit employ a staggered-entry difference-in-differences model with MSA and year fixed effects to control for time-invariant differences between treatment and control groups. Additionally, we fit MSA-specific time-trends to control not only for differences in level of financing across treated and untreated regions (the parallel trends assumption in DD models) but also differences in the growth rate (Autor, 2003), in order to capture any pre-trends in funding patterns that might not be captured fully in the matching process. This approach identifies our coefficients of interest off of changes from average trend that coincide in time (within a year) with the founding of an accelerator. The inclusion of MSA-specific linear trends in our DD specifications should reduce concerns about the *timing* of the arrival of an accelerator considerably; we further employ multiple methods to test the robustness of our results, including a triple-differences model comparing funding availability for a more-affected versus less-affected industry, visual inspection of pre-trends, and fractional logit models of early and later stage deals, to ameliorate the more obvious concerns along this dimension and show consistency with a causal interpretation.

As an alternative, we additionally employ a synthetic control approach (e.g. Abadie and Gardeazabal, 2003; Abadie et al., 2010). Synthetic control methods strongly complement our difference-in-differences approach by allowing us to control for *unobserved* and *time-varying* confounders that might endogenously affect the establishment of an accelerator in a particular region. If, for instance, Cincinnati became a particularly attractive location for startups starting in

2010 specifically (e.g. because of its concentration of consumer packaged goods companies), synthetic control methods control for this time-varying impact of unobservables, while difference-in-differences cannot (Abadie and Gardeazabal, 2003; Abadie et al., 2010). We use the approach outlined in Abadie et al. (2010) and extended by Cavalloet al. (2013) to multiple cases, and construct a synthetic control for each of our treated MSAs, using an optimally-weighted convex combination of untreated MSAs to construct the synthetic counterfactual. To the best of our knowledge, ours is the first paper to apply this quasi-experimental design to a topic in the entrepreneurship literature.

Our estimates suggest a significant potential role for accelerators in creating spillovers of entrepreneurial activity in the local ecosystem. In our difference-in-differences model with a strictly matched sample, fixed effects, and linear time trends, the arrival of an accelerator is associated with an annual increase of 103% in the number of seed and early stage VC investments in the MSA involving startups that *did not* attend the accelerator. Additionally, not only do our estimates suggest an increase in the number of venture backed startups in the regions, but they also suggest that the amount of funding provided to these companies increases disproportionately: we observe an increase of 265% in the log total dollar amount of seed and early stage funding provided in the region, again excluding accelerator portfolio companies.

Importantly for regional entrepreneurial capacity, when we examine the source of these deals, we find that the arrival of an accelerator is associated with a 88% increase in the number of distinct investors investing in the region. Importantly, this increase in the number of distinct investors following the establishment of an accelerator comes both from attraction of active investors from outside the region as well as an increase in *local* investment groups, suggesting an increase in overall regional capacity. Our results using the synthetic control approach mirror the results obtained using the matched-sample difference-in-differences methodology. Treated MSAs experience significantly higher deal volume, excluding accelerator participants, and exhibit emergence of new investor groups after the arrival of an accelerator.

In the absence of a natural experiment, however, we are explicit about the limitations of our empirical approaches. We do not make definitive claims for causality, though we provide evidence to alleviate concerns about many of the more obvious potential sources of endogeneity in our sample. In sum total, however, our findings would be consistent with a causal effect.

Overall, our findings appear to be consistent with the argument that establishing an accelerator can have a significant effect on high-growth entrepreneurial activity in the regional ecosystem, beyond its effects on its own participants. The data are consistent with policy makers' beliefs that accelerators can have significant influence on the population of entrepreneurs who are interested in founding companies locally, through peer effects and provision of role models that shift beliefs regarding the probabilities of successful undertaking of entrepreneurial activity in the area, irrespective of whether they participate in the accelerator program. This shift in equilibrium and our finding on the creation of new local investor groups is also consistent with accelerators having the ability to deliver, through encouragement of broader entrepreneurial activity, enough new additional startup formation such that an investor will be willing to incur the fixed costs associated with investing in a new class of financial instruments (early-stage equity investments) or in a new region.[4]

Our results contribute to multiple literatures. First, we contribute to a small but growing literature exploring the role and effects of new institutional players such as accelerator programs in the startup ecosystem. Until now, this research has focused on the effect of treatment on the treated for 'accelerated' startups (Gonzalez-Uribe and Leatherbee, 2016; Fehder, 2018). Many studies of entrepreneurial policies and programs focus on firm-level dependent variables, however existing research suggests that policies which seem "effective" at the individual firm level can have indeterminate or negative impacts on the regional economy (Davis, Haltiwanger, and Schuh, 1998). In contrast to the prior studies, we focus our study on the overall regional effects of such initiatives, and provide evidence suggestive that in the case of accelerators, there may also be positive spillover effects consistent with policy maker motivations.

Second, we contribute to the literature that traces the impact of social factors such as peer effects on geographic clustering of entrepreneurial activity. A similar influence of peer effects has been documented in the formation of behavioral preferences such as risk aversion and trust as well

[4] It is important to note that it does not appear that venture investment trivially follows the opening of an accelerator due to some sort of pre-commitment on the part of investors to invest in the accelerator alumni or in the region. While some accelerators are funded by VC dollars, many are funded initially by their founders, who are typically former successful entrepreneurs. Furthermore, the costs to launch a startup accelerator are in the low hundreds of thousands (around $200-250 thousand), a relatively cheap option purchase for a single investor, let alone a group of investors. Based on our extensive discussion with accelerator founders and VCs who have invested in accelerators, no pre-commitments to invest in accelerator graduates are made by VC investors (this notion was summarily dismissed by every person we spoke to). Rather, investors in a startup accelerator are taking an option on a future stream of projects that (hopefully) have been vetted and improved. Their willingness to exercise those options is another matter entirely.

as the making of financial decisions (Ahern et al., 2014; Shue, 2013). Prior literature has documented a number of social channels for peer influences including co-workers (Nanda and Sorensen, 2010), family (Lindquist et al., 2015), and university alumni ties (Kacperczyk, 2013). Unsurprisingly, therefore, other scholars have found significant spillovers to entrepreneurial peers in terms of fostering new entrepreneurial entry (Giannetti and Simonov, 2009; Markussen and and Røed, 2017). Our findings suggest that even interventions aimed at direct interaction with a small number of startups can have larger spillovers, through the provision of role models and peers that enhance prospective entrepreneurs' subjective estimates of the likelihood of their success.

Lastly, we attempt to bridge programmatic evaluation of accelerators to a broader literature on the regional and geographic context of economic growth through innovation and entrepreneurship. In this particular case, the outcome we explore—growth-oriented innovation-driven entrepreneurial activity—is considered a critical element in the entrepreneurial ecosystem, and has been shown to be tightly tied to economic growth (Decker et al., 2016; Decker et al., 2018; Guzman and Stern, 2018). Researchers have long noted the localization of economic activity, and recent work confirms the clustering phenomenon for entrepreneurship (Glaeser and Kerr, 2009), while also describing the shape and content of these clusters (Delgado, Porter and Stern, 2010). The arrival of startup accelerators provides a new context that provides additional insight into how shifts in a region's institutional support for entrepreneurs can influence its capacity for innovation and entrepreneurial activity more broadly.

The paper proceeds as follows. Section 2 introduces the accelerator model and its relationship to local entrepreneurial activity, and discusses the research to date. Section 3 describes the data. Section 4 presents the methodological approach and results. Section 5 discusses and concludes.

## 2. Seed Accelerators

The formal definition of a startup or seed accelerator, first offered by Cohen and Hochberg (2014), is a fixed-term, cohort-based program, including mentorship and educational components, that culminates in a public pitch event, often referred to as a 'demo-day.' Some accelerator programs, though not all, provide a stipend or small seed investment (a minimum of $28 thousand on average, with a range from $0 to $120 thousand) to their startups, and receive an equity stake in the portfolio company in return, typically 5-7%.[5] Most offer co-working space and other services in addition to mentorship, educational and networking opportunities. Some also offer a larger, guaranteed investment in the startup, in the form of a convertible note, upon graduation. While many accelerators are generalists across industries, others are vertically-focused (healthcare, energy, digital media). Despite the vertical or industry focus, careful examination of the products/services provided by the portfolio companies of accelerators reveals that nearly all accelerator portfolio startups offer some form of software or internet services, though such software may be targeted towards use in a specific industry vertical.[6]

The emergence of accelerators has been facilitated by a significant fall in the costs of experimentation over the last decade (e.g. Ewens, Nanda and Rhodes-Kropf, 2016). The capital requirements to seed a startup software company have fallen dramatically along with the cost of experimentation; where building a software company may have cost $5 million on average 10 years ago, today it can often be accomplished with $500 thousand, and startups can often accomplish with a $50 thousand seed investment what used to take $500 thousand to $1 million. This has allowed accelerators to provide meaningful assistance to startup portfolio companies with a seed investment or stipend as low as $15 thousand, or even without provision of funding.

In practice, accelerator programs are a combination of previously distinct services or functions that were each individually costly for an entrepreneur to find and obtain: value-added mentorship and advisement, co-working/co-location with other startup companies, capital introductions and exposure, network building, and the opportunity to pitch to multiple investors, and in some cases, seed investment. The likely result of these services is a reduction in search costs for the entrepreneur, and possibly an increase in leverage vis-a-vis potential VC investors. Indeed, accelerators often attempt to be an organized version of the “dealmakers” described in Feldman and Zoller (2012), drawing the community together and creating social capital surrounding entrepreneurial efforts.[7]

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[5] Summary statistics obtained from the Seed Accelerator Rankings Project (Gilani and Quann 2011, Hochberg and Kamath 2012 and Hochberg, Cohen, Fehder and Yee 2014), which uses proprietary data collected annually from accelerator programs to assess the relative quality of U.S.-based programs. Additional descriptive statistics on accelerator programs and their participants are detailed in Cohen, et al. (2018).

[6] The Seed Accelerator Rankings Project tracks the identity and focus of the portfolio companies for most established (2 cohorts +) accelerators.

[7] Notably, accelerators differ considerably from existing institutional structures in the entrepreneurial ecosystem, such as incubators. Incubators are primarily real estate ventures, offering startup co-working space at reduced rent. Incubators, unlike accelerators, lack a fixed term, and experience continuous entry and exit of startup groups, which stay resident for much longer periods of time (1-4 years on average versus 3-4 months for an accelerator). Most offer

From the perspective of the VC investors, accelerators serve a dual function as deal sorters and deal aggregators. The accelerator application process screens among a larger population of startups to identify high-potential candidates, and the program aggregates these candidates in a single location, attracting investors who might otherwise find the costs of searching for opportunities in smaller regions too high to justify. Investors often serve as mentors, thus getting an early look at the startups, business plans, team dynamics and progress over the term of the program. The public demo day, or pitch event, allows them to observe multiple companies pitch in a single instance, and since they are already traveling to the region, non-local investors often choose to look at other opportunities in the area as well. The aggregation and sorting function performed by accelerators is thus believed to result in a reduction in search and sorting costs for the VCs when investing in smaller regions.

The existing research on accelerator programs primarily is descriptive or compares accelerated startups to non-accelerated companies. In the descriptive category, Cohen and Hochberg (2014) offer the first formal definition of an accelerator program, distinguishing accelerators from other types of programs that have similar or related goals. Isabelle (2013) presents a comparison of accelerators to incubators, while Miller and Bound (2011) provide descriptions of the accelerator model. Radojevich-Kelley and Hoffman (2012) presents case studies of a small number of early U.S.-based programs. Cohen, et al. (2019) present descriptive evidence on variation in the design of accelerators and their impact on individual participant performance and funding.

A second emerging set of empirical studies compares startup companies that complete accelerator programs to other populations of startups that did not attend accelerator programs. Hallen, Cohen and Bingham (2014) compare accelerated startups that eventually raise venture capital to non-accelerated ventures that eventually raise venture capital. They find that graduating from a top accelerator program is correlated with a shorter time to raising VC, exit by acquisition, and achieving customer traction. Winston-Smith, et al. (2015) compare ventures that have participated in two of the leading accelerators, TechStars and Y Combinator, to similar ventures that do not go through these programs but instead raise angel funding. They find that startups that graduate from these top two programs achieve exit (acquisition or failure) faster than their

fee-based professional services. They do not offer investment or stipends, and their educational and mentorship offerings, if provided, are ad hoc at best. Incubators are primarily thought to shelter vulnerable nascent businesses from the harsh realities of the real world, while accelerators force startups to quickly confront those realities and determine whether the business is viable (Cohen and Hochberg 2014).

matched, angel-funded counterparts, due to both higher acquisition rates and higher failure rates than for angel-funded startups. This increased speed of failure is also found in Yu (2014), utilizing a different sample of accelerated and matched controls.

Other studies examine the impact of particular individual accelerator programs. Fehder (2018) performs a regression discontinuity study of one large program, and finds a significant positive treatment effect for companies that complete that program. Gonzalez-Uribe and Leatherbee (2016) use variation in the provision of services across portfolio companies to identify the impact of certain elements of one particular program, Startup Chile, finding that admission to the program alone did not produce a treatment effect, but access to intensive mentoring and peer effects did have a performance impact.

These early-stage studies are focused on the outcomes for accelerator portfolio companies: in other words, they are interested in the effect of treatment on the treated (do accelerators add value to the companies that attend them). Outcomes, however, are difficult to measure in this setting, and endogeneity issues are rife. Furthermore, if accelerators serve to shift the general equilibrium of the entrepreneurial ecosystem by increasing overall entrepreneurial entry and encouraging the emergence of increased resources for both the treated and the non-treated in a region, studies of this nature will not be able to properly capture the full effects of accelerators. Here, we instead take a complementary approach, examining the regional effects of programs on the quantity of spillover activity in the local entrepreneurial ecosystem outside of the accelerator participants, rather than the treatment effect of the accelerator on the treated startups.

## 3. Data

Our sample is derived from a list of 59 accelerators that were founded in 38 MSAs in the United States between 2005 and 2013, leaving sufficient post-period to measure outcome activity. We create an exhaustive list of accelerators from a number of sources, including thorough web searches and lists compiled through active engagement with the accelerator practitioner community by the Seed Accelerator Ranking Project (http://www.seedrankings.com) (SARP). Our dataset covers 2003 through 2013 and thus includes the entire early development of accelerators as an institution in the United States. The list of the accelerators included in our analysis is included in Table 1. Notably, many accelerators are located in regions that are not typically thought of as hot beds of startup or VC activity. For example, of the ten accelerators launched from 2005 to

2009, only two located in known startup clusters (Silicon Valley and Boston, MA). The remaining eight located in what were, at the time, relatively inactive locations, such as Boulder, CO, Philadelphia and Pittsburgh, PA, Dallas, TX and Providence, RI. Anecdotal evidence from books and interviews with accelerator founders suggests that this pattern emerges precisely because many programs were founded by hometown entrepreneurs who had made their money elsewhere and who wished to return to try and establish a startup cluster in their region.

For each of the accelerators in our list, we code a number of variables. First, we note the founding year as the year in which the first cohort of the accelerator graduated and had a demo day. We exclude accelerators from our analysis if they did not graduate at least two cohorts. Next, we note the MSA region in which the accelerator is located. Third, we note whether the accelerator was ranked in the top fifteen in the 2013 Seed Accelerator Rankings.

For each MSA region in the United States, we create a dichotomous variable that indicates whether a startup accelerator has been established in the region (*TREATED*) and a variable that indicates when the region received its first accelerator (*TREAT YEAR*). We collect a range of outcome and control variables at the MSA-Year level. Table 1 describes each of the variables we collect and their sources. We obtain per capita income and employment at the MSA-year level from the U.S. Census. We obtain an annual count of utility patents issued to entities or individuals in the MSA from the United State Patent and Trademark Office. We obtain an annual count of Science, Technology, Engineering and Mathematics (STEM) graduate students in each MSA and annual university research and development spending in the MSA from the National Science Foundation. Finally, we obtain an annual count of new firms in each MSA from the U.S. Census Business Dynamics Statistics tabulation.

Our analysis contains three outcome variables each obtained from Thomson-Reuter's VentureXpert. First, we measure the number of distinct seed and early stage VC deals that occur each year in each MSA (*NUMBER DEALS*) for companies in "Internet Specific" and "Computer Software" companies. We focus on these company classifications because all but two of the four hundred accelerator portfolio companies that we have records for are classified by VentureXpert in these two categories. We use company name, founder names and location data to match the VentureXpert data to a list of startups in each accelerator generated from the SARP data. Importantly, SARP tracks all name changes and aliases ("DBA") associated with portfolio companies on an ongoing basis, thus easily allowing us to identify accelerator participants in later years even if they have changed names multiple times. Using the SARP list of all startups that attended the accelerators in our sample and their name changes and aliases/DBAs, we are able to exclude investments in these startups from our sample, thus restricting our outcomes measures to non-accelerator participants, a necessary condition to measure spillovers.

Having restricted the outcome data to non-accelerator participants, we further code the total sum of seed and early stage VC dollars invested each year at the MSA level (*FUNDS INVESTED*) in the two classifications, again excluding accelerator portfolio companies. Last, we note the count of distinct investors making investments in each MSA each year (*DISTINCT INVESTORS*). We further break our total count of investors into separate counts of investors whose fund is headquartered more than 100 miles[8] from the startup company (*DISTANT INVESTORS*) and investors whose fund is headquartered less than 100 miles (*NEAR INVESTORS*). Note that these investments are *external* investments by venture capital firms, not the accelerator. We do not include participants in the accelerators themselves in any of our outcome measures.

The resulting sample is a panel with observations at the MSA x Year level. Panel A of Table 3 provides the descriptive statistics for our entire sample across all U.S. MSA regions and all years, segmented by ever-treated or never-treated status. For this comparison, we include all U.S. cities, including known entrepreneurial clusters. Even excluding accelerator graduates, comparing the overall sample means of the never-treated regions to overall means of the treated regions reveals that treated regions exhibit statistically significant higher levels of venture financing activity both in terms of Funds Invested and Number of Deals. Treated regions also exhibit higher levels of other covariates associated with startup formation. In addition, comparison of the change in number of deals across treated and untreated regions over the course of the sample period reveals that treated regions differ significantly from untreated regions not only in terms of level but also growth rate of entrepreneurial financing events. As we next show, however, much of this is driven by the inclusion of known startup-activity clusters, such as Silicon Valley.

Panel B of Table 3 demonstrates the skewness of the distribution of entrepreneurial financing events by dropping the MSAs associated with the San Francisco Bay Area (Silicon Valley) and Boston from the summary statistics for the treated regions. Simply removing these regions from

[8] We calculate this distance as the geodetic distance between the geographic center of the zip codes reported for both the startup company and the investment firm. We chose 100 miles as a distance where a venture capitalist could fly there and back in a day or drive to the startup's office in a day. We obtain similar results when employing smaller radii.

the summary statistics decreases the overall sample means for both Funds Invested and Number of Deals by roughly half in the treated column. The modal number of the funding events across all MSA-years is zero, while a few MSAs have a large number of yearly events. The differences between Panel A and Panel B of Table 3 underline the importance of finding the properly matched treatment and control groups so that our results are not driven by the large apparent differences in the level and growth rate of entrepreneurial financing events in treated and non-treated regions.

## 4. Empirical Analysis

Our research seeks to measure the impact of startup accelerator formation on the venture-backed startup activity in an MSA excluding startups directly "treated" by an accelerator through participation in their program. As discussed above, startup accelerators can serve to increase entrepreneurial entry in a region through peer effects, role models and other social mechanisms. If so, the presence of an accelerator should be associated with an increase in the level of startup activity in a region.

An examination of the raw data provides initial indications that such spillover effects may be present. Data on accelerator portfolio company identities obtained from the Seed Accelerator Rankings Project allows us to match funding activity in the region to the companies that completed the accelerator program. As an example, consider the MSA that includes Boulder, CO. TechStars Boulder was founded in 2007. In the period preceding the founding of TechStars, Boulder saw an average of 4.8 seed and early stage software and IT VC deals per year. Post-arrival of TechStars, from 2007-2013, the average number of deals in the Boulder MSA rose to 10.7 deals per year, a 5.9 deal increase. However, during this period, only 2.3 deals per year, on average, involved companies that had graduated from TechStars Boulder. Similarly, consider Cincinnati, OH, home of The Brandery, and accelerator founded in 2010. Pre-arrival of The Brandery, Cincinnati experienced, on average, 0.55 early stage VC deals per year – about one deal every two years. After The Brandery was established, in the period 2010-2013, Cincinnati averaged 4 deals per year—an increase of 3.45 deals per year. However, only 1.45 deals per year on average in this period involved a Brandery graduate startup.

We can perform a similar tabulation for each of the treated MSAs for which we are able to obtain a list of portfolio companies from the Seed Accelerator Ranking Project. Across these MSAs, on average, seed and early stage financing deals of startups that graduated from the accelerator represent only 30.4% of the increase in the annual number of seed and early stage financing deals post-treatment. This hints at a more general effect on startup activity in the region, consistent with the notion that an accelerator program may serve as a catalyst to encourage latent local entrepreneurial entry.

At the same time, conceivably, startup accelerators may be more likely to be founded in regions that have higher levels of startup activity or have experienced (or are about to experience) swift growth in that activity. Thus, from an analysis perspective, our goal is—to the extent possible given the setting and data—to separate the causal impact of startup accelerator formation from the endogenous selection of startup accelerators into "hot" regions for startup activities.

Of course, the founding of an accelerator in a given MSA is potentially a function of variables that are unobserved by the econometrician. While we know from interviews with founders that any number of accelerator programs were established by former entrepreneurs for altruistic reasons such as a desire to support a hometown community or develop an ecosystem in an area that had none, a concern still remains that the regions in which they were established differ in a systematic fashion from regions that do not receive an accelerator.

By restricting our analysis to regions with less-developed entrepreneurial communities, our particular endogeneity concern—namely, that accelerator founders select the region in which to found their accelerator based on variables that are unobservable to the econometrician and bias our estimates upward—is reduced. Anecdotal evidence as well as surveys and interviews conducted by the Seed Accelerator Rankings Project (SARP) and others strongly suggest that accelerator founders in our regions of interest primarily founded accelerators out of altruistic or regional growth oriented objectives. In fact, most accelerator founders in these regions demonstrate deep roots and ties to the region in which they found their accelerator program. In contrast, accelerator founders in regions with active, well-established entrepreneurial ecosystems, such as Silicon Valley and Boston/Cambridge, tend to be founded by outsiders who recount that they came to the region to take advantage of the prior existence and growth of these ecosystems, rather than to grow them.[9]

We lend support to this anecdotal evidence more formally by comparing regions that received accelerators that are in and out of our sample. The Seed Accelerator Rankings Project (SARP)

[9] Consistent with this assumption, each of the top five regions for total yearly venture capital allocations received startup accelerators relatively early in the diffusion of this organizational form (Cambridge, MA and Silicon Valley were the first two locations).

collects information from each of their participating accelerator programs regarding the primary reason for the founding of the accelerator, including the options “ecosystem building,” “regional development,” and “return on invested capital.” Using this data, we find that 91.6% of the accelerators in the regions we include in the matched sample state that they were founded with a primary objective of “ecosystem building” or “regional development.” This contrasts sharply with the objectives stated by founders of accelerators in traditional, established entrepreneurial clusters such as San Francisco or Boston, nearly all of whom state their primary objective as “return on invested capital.”

We then econometrically address the potential for omitted variables bias in multiple ways. First, we create a set of matched control and treatment MSAs using a dynamic hazard rate model; we match MSAs on both the level of and growth trends in VC financing pre-accelerator arrival. Our specification allows for non-linearity in such growth. Second, for each model we run an additional regression with the inclusion of MSA-specific linear time trends, to absorb any remaining linear trend differences. Third, we estimate a triple differences model using early stage investment into semiconductor startups as an untreated industry which adds industry variation within each MSA. Fourth, we employ a synthetic controls methodology. Taken together, these four approaches allow us to examine the robustness of our regression models to different forms of misspecification. Each of these four approaches is discussed in turn below.

### 4.1 Matched Sample

In other contexts, researchers have found that short-term changes, such as a wage dip, can drive a treatment decision, like attending a job-training program (Ashenfelter, 1978; Abadie, 2005). To address such concerns in our context, we match the treated regions to regions without accelerators whose growth dynamics are similar to those of the MSAs in which accelerators are founded in the period prior to arrival of an accelerator to a region. By choosing control regions which match the treated regions on past growth dynamics leading up to the founding of the accelerator, we hope to minimize the chance that our measured impact of the accelerator founding is driven by short term fluctuations in the attractiveness of a region for early stage investors and entrepreneurs. Matching also has the additional advantage of matching the never-treated regions to treated regions in specific event time, such that each matched never-treated region observation has a clear “*PRE*” and “*POST*” event period.

We match our treated MSAs to untreated MSAs that are substantially similar to the treated MSAs in pre-treatment year trends likely to affect the attractiveness of the region for early stage funding. To create our matched sample, we first estimate a dynamic hazard rate model of the form:

$$h(t, msa) = f(\varepsilon_{t,MSA};\ \beta_0 + \beta_t VC_{t,MSA} + \beta_{t-1}\Delta_{t-1,MSA} + \beta_{t-2}\Delta_{t-2,MSA} + \beta_{t-3}\Delta_{t-3,MSA}),$$
(2)

where h(t) is the point hazard of an accelerator being founded in that MSA and in that year. $VC_{t,MSA}$ is the count of seed/early stage venture capital deals in that MSA and in that year. The delta terms ($\Delta_{t-1,MSA}$, $\Delta_{t-2,MSA}$, and $\Delta_{t-3,MSA}$ ) measure the differences in the current number of early stage deals in that MSA to the levels one, two and three years previously respectively, thus capturing possible nonlinearities in the growth prior to accelerator arrival. Thus, our hazard rate model flexibly estimates how both the level and the short-term rate of change in venture-backed startups predict the arrival of an accelerator in a given MSA region. We thus obtain an instantaneous probability, based on current levels of venture-backed startup activity, that an accelerator will choose to locate in a specific MSA in a specific year.

Using the estimates for each region from the dynamic hazard rate model, we then choose a match for each treated region by finding the untreated region with the most similar probability of founding an accelerator in the year of treatment (with replacement) when the treated region is on the common support. As discussed in the introduction, we are specifically focused on understanding the impact of accelerators in regions with less developed startup infrastructure. The use of this matching procedure means that regions such as Silicon Valley and the Boston/Cambridge region, where accelerators are much more likely to be established with the goal of capturing increasing startup activity, and which do not have a natural counterpart in the population of potential control MSAs, will not have control matches and therefore will naturally be excluded from the analysis. Thus, our matching procedure is such that the excluded regions are regions with disproportionately rich entrepreneurial ecosystems, and their exclusion allows us to properly examine the appropriate counterfactual for the research question at hand: the effect of an accelerator's founding on regions looking to create an ecosystem or induce the creation of entrepreneurial activity.

Table 4 explores the differences between the treated and non-treated regions in our matched sample. The matching procedure, which requires matched and treated MSAs to be on the common support, leaves us with 23 treated MSAs that have substantially similar matched MSAs for the estimation. In contrast to the patterns exhibited in Table 3 for the full sample, the differences between the treated and untreated groups in the matched sample are far smaller. Indeed, when we compare the means for each of the variables in Table 4 for the pretreatment period of the treated and untreated MSA regions, we find no significant differences for any of the financing variables, though there remain some statistically significant differences between the two populations for the university R&D funding, firm births, and employment variables. In the subsequent regressions, we are careful to control for these differences by adding these variables as controls. Nevertheless, and importantly, the matching procedure appears to purge these two populations of their differences in both the level and growth rate of entrepreneurial financing events.

In Table 5, we present additional supporting evidence for the use of this matched sample to alleviate endogeneity concerns, using data collected by SARP and through web searches on accelerator founder origins. Column (1) of Table 5 shows the statistics for accelerated regions that had no close matches in untreated regions and were thus excluded from our analysis while column (2) shows those from in our hazard-rate matched sample. In the first row, we compare proportion of accelerator founders who went to high school within 150 miles of where they established their accelerator. In our hazard-rate matched sample, the mean is nearly 50%, while the proportion in the excluded regions are lower by a statistically significant 19.3%. This pattern is repeated in the second row, where we examine distance between a founder's high school and accelerator location as a continuous variable. Accelerator founders in our sample are far more likely to have local connections to the MSA in which they open their accelerator. If these locations are chosen for reasons orthogonal to their immediate economic potential, as suggested by the stated primary objectives of the accelerator population we study, then endogeneity concerns are substantially reduced. In the remainder of the rows of Table 5, we show that accelerators founders outside of our hazard-rate matched sample do seem to choose locations based on the scale and growth of entrepreneurial activity in their chosen locations; lending credence to both the fact that these endogeneity concerns are warranted, but also to the fact that such behavior is largely purged from our matched sample (further exploration of this issue is presented in Appendix C). While this helps to mitigate concerns about specific endogeneity in our sample, we will additionally employ multiple econometric approaches to further lend support to a causal interpretation of our findings.

### 4.2 Differences in Difference Framework

Using a panel data set of US Census MSAs across ten years, we exploit the fact that different accelerators were founded in different years in different MSAs to assess the impact of accelerator foundation through a differences-in-differences (DiD) model. Our preferred specification of our DiD model takes the form:

$$VC_{t,MSA} = \propto_{MSA} + \gamma_t + \beta' X_{t,MSA} + \theta_{MSA} t + \delta POST * TREATED + \varepsilon_{t,MSA} \quad (1)$$

This model controls for time-invariant heterogeneity in the entrepreneurial capacity of different MSA regions with the MSA fixed effect, $\propto_{MSA}$, for national level dynamics with year fixed effects, $\gamma_t$, and for different growth trends in the venture-backed startup activity in each MSA with $\theta_{MSA}$. $POST * TREATED$ is a dichotomous variable that is set to 1 for MSAs with accelerators for all years greater than or equal to the year of the accelerator's first cohort. $X_{t,MSA}$ are time X MSA-specific controls. In this specification, $POST * TREATED$ measures the impact of the founding of an accelerator by comparing treated regions to untreated while controlling for fixed differences in regional levels of startup/venture activity and time period specific shocks that are shared across all regions. In addition, the inclusion of MSA-specific trends means that parameter of interest $\delta$ measures the average deviation from MSA-specific slope term observed after the arrival of an accelerator in an MSA. This means that the identifying variation in this model is focused on changes from average trend that coincide in time with the founding of an accelerator (See Appendix A for further exploration of this assumption). In this model, $VC_{t,MSA}$ are the set of outcome variables generated from the VE data and described in the previous section.

We begin by estimating the baseline specification described in equation (1). We estimate the model using our hazard-rate matched sample, and consider three outcome variables: the number of seed and early stage deals done in the region; the number of distinct seed and early stage VC investors active in the region; and the dollar amount of seed and early stage financing provided in the region. Each model contains an array of control variables and fixed effects for year and MSA. We estimate each of the models twice, adding an MSA-specific linear time trend in the second estimation of each.

Table 6 presents estimates of the difference-in-differences models for our hazard-rate matched sample, with and without MSA-specific linear time trends. The first two columns of Table 6 present our estimates of the baseline model where the outcome variable is the number of early and seed stage deals done in the MSA. The unit of observation is an MSA-year and we are interested primarily in the coefficient loading on the dichotomous variable that indicates whether an accelerator was active in the MSA in that year. Since the number of deals is a count variable, we estimate Poisson models. We report the coefficients in their exponentiated form, also referred to as the incidence rate ratio (or IRR), as it provides an intuitive interpretation as the multiplicative effect of the treatment on the count of the dependent variable in question. Column (1) estimates the baseline model. The coefficient on the *Accelerator Active* (the post x treated) variable of interest is positive and statistically significant at the 1% level; the IRR estimate of 2.292 indicates that the region experiences an increase of 129.2% in the number of early stage venture deals—our proxy for high-growth potential entrepreneurial activity—in the years following the arrival of an accelerator in the MSA.

In column (2), we add the MSA-specific linear time trend to absorb any unobserved variation in the growth rate in venture-backed startup activity in each MSA. Once again we observe a positive and statistically significant coefficient on the variable of interest; the magnitude of the IRR estimate in this case is only slightly lower, suggesting an increase of 102.6% in the number of early stage venture deals in the years following the arrival of an accelerator in the MSA. The unconditional mean of financing events in the matched sample (treated and matched untreated) in the pre-treatment period is 1.75 deals per year. While this baseline level is low, the increase of over 100% in the number of deal represents a significant jump in activity for a region.

Figure 1 graphs the treatment effect for the treated regions by year relative to the control. Year 0 on the graph is the year of the accelerator founding. In the three years prior to the establishment of an accelerator, treated and matched control MSAs look virtually the same in terms of the number of deals done in the region; following the establishment of the accelerator, the number of deals jumps sharply for the accelerated MSAs as compared to the control MSAs. This pattern is evident in both models with and without inclusion of the MSA-specific linear time trend.

In columns (3) and (4) of Table 6, we present estimates from similar models, where the outcome variable employed instead measures the number of distinct seed and early stage venture investors active in the region in a given year. Estimating the model without the MSA-specific linear time trend (column (3)), we find a 98.7% increase in the number of distinct investors in treated MSAs following the arrival of an accelerator. In column (4), we explore whether this effect changes significantly with the inclusion of an MSA-specific linear time trend. The level of the

IRR coefficient and the standard errors remain relatively similar with the inclusion of the linear time trend, addressing concerns that our coefficient estimates are being driven by differences in the growth rates of investors across the treated and untreated MSAs. The estimates in column (4) suggest an increase of 87.9% in the number of distinct seed and early stage investors in the region following the establishment of an accelerator. These increases are relative to the unconditional mean number of 2.66 distinct investors each year in the pre-treatment period.

In similar fashion to Figure 1, Figure 2 graphs the treatment effect for the treated regions by year relative to the control. Once again, in the three years prior to the establishment of an accelerator, treated and matched control MSAs look virtually the same in terms of the number of distinct seed and early stage investors active in the region; following the establishment of the accelerator, the number of distinct investors jumps sharply for the accelerated MSAs as compared to the control MSAs. Again, this pattern is evident in both models with and without inclusion of the MSA-specific linear time trend.

In columns (5) and (6) we repeat these estimations for the outcome variable measuring total dollar amount of seed and early stage software and IT VC investment in the region. We once again observe a significant effect of accelerator establishment on financing activity: with the arrival of an accelerator in the region, the MSA experiences an statistically insignificant estimated increase of 72% (without linear time trend controls) to 165% (with MSA-specific linear time trend controls) in the natural logarithm of total $ seed and early stage capital invested in the region. Figure 3 presents the treatment effect graphically over time; once again, there is no apparent difference between the treated and matched untreated MSAs prior to the arrival of the accelerator, but after accelerator establishment, the treated MSAs experience a jump in total funding relative to the matched controls.

Notably, we observe little in the way of consistent statistically significant coefficients for the control variables included in the models, regardless of whether the models contain an MSA-specific linear time trend or not. The exception is employment; across all but one model in column (2), the parameter estimates suggest a negative relationship between overall local employment levels and our measures of early stage funding activity (in the count models, the reported IRR coefficients are less than one, indicating a negative coefficient on the variable in the actual model estimation, and in the OLS models, the coefficients estimated are negative). Altogether, the models in Table 6 suggest a large and significant association between venture-backed startup activity and the establishment of an accelerator in the region.

### 4.3 Triple-Differences Model

The outcome variables we measure in the baseline capture seed stage investment activity in the software and IT segments alone. In Table 7, we provide our first falsification test by adding to our models an industry that is less likely to be impacted by accelerators, the semi-conductor industry. Because of the length of the time to market (years versus months to a potential signal of viability) and the differences in the human capital required of founders (Ph.D.s versus software developers), startup accelerators have typically not included semiconductor companies in their portfolios. If accelerators tend to galvanize latent entrepreneurial activity by providing role models, peer effects, and evidence on the existence of ecosystem resources, this effect should be much stronger for startup activity in the industry that accelerators primarily focus on, software and IT. Moreover, since most VC firms, particularly smaller and newer firms, are specialized by industry vertical (Hochberg and Westerfield, 2012; Hochberg et al., 2015), even with the entry of new investors to the region to take advantage of new startup activity, we should expect semi-conductor startup financing activity to be significantly less impacted. Thus, adding financing events from this "non-accelerated" industry to our data on "accelerated" industries within each MSA, we can control for trends across accelerated industries and shared trends within treated MSAs. Given the lack of focus by accelerators on the semiconductor segment, we would expect to see less of an effect of accelerator establishment on entrepreneurial finance activity in that industry.

Table 7 presents the estimates of the triple difference models for our three outcome variables. The coefficient of interest is that on the triple interaction *Treated Region X Treated Industry X Post-Treatment*. We observe significant and positive coefficients in the models for number of deals and number of distinct investors, both when we include MSA-specific linear time trends and when we omit them. Here, in columns (1) and (2), the model estimates suggest that the founding of an accelerator in an MSA produces a statistically significant 328% (280%) increase in the number of early stage software and IT deals in accelerated industries when omitting (including) the linear time trends. In columns (3) and (4), we explore the impact of the arrival of an accelerator on the number of distinct investors while controlling for industry. We find that the arrival of an accelerator is associated with a 195% (167%) increase in the number of investors when omitting (including) MSA-specific linear time trends. Lastly, in columns (5) and (6), we explore the impact of the arrival of accelerators on the total amount of dollars invested. In this set of models, we find no statistically significant differential impact of the arrival of an accelerator on software startups when compared to startups in the semiconductor industry.

Figure 4 graphs the treatment effect for the treated industry in treated industries over time. Also included in the model generating these parameter estimates, but not displayed, are separate terms for treated region and treated industry as well as MSA and year fixed effects. There is no difference in financing patterns pre-accelerator founding between the groups; following the establishment of the accelerator, there is a jump in financing activity for the more-treated industry (software and IT) in the treated region, but not for the less-treated industry (semiconductors). Thus, our estimates of the positive impact of accelerator founding on regional entrepreneurial finance appear to be robust to controlling for whether the industry in question is likely to be more affected.

### 4.4 Synthetic Controls

While the previous analysis uses a dynamic hazard model to find the closest untreated MSA in terms of level and trend in early stage venture-backed startup activity, an alternative approach is to construct a closer counterfactual MSA for each treated region using synthetic control approaches. The synthetic control method (Abadie and Gardeazabal, 2003; Abadie et al., 2010) involves the construction of a convex combination of comparison (untreated) units used as controls, which approximates the characteristics of the unit that is exposed to the treatment. A combination of comparison units often provides a better comparison for the unit exposed to the intervention than any comparison unit alone. This "synthetic control" unit is then used to estimate what would have happened the treatment group if it had not received the treatment.

Unlike difference in differences approaches, the synthetic control method can account for the effects of confounders changing over time, by weighting the control group to better match the treatment group before the intervention. Critically, this means that the synthetic control approach concretely addresses the one remaining source of endogeneity not addressed by our hazard-rate matched difference-in-differences analysis or our triple differences. An additional advantage of the synthetic control method is that it allows researchers to systematically select comparison groups. Synthetic controls can be thought of as an extension of the difference-in-differences model: while the diff-in-diff approach treats unobservables as a cross-sectional constant, the synthetic control approach allows unobservable factors (which may be correlated with covariates $X_{it}$) to vary cross-sectionally.[10] Cavallo, et al. (2013) extend the Abadie et al methodology to account for multiple treated units.

We use the methodology from Cavallo, et al. (2013) and create a synthetic control unit for each of our treated regions, using the never-treated regions as the comparison units. We restrict our treated sample to the programs that were included in our hazard rate sample so as to exclude regions such as San Francisco, Boston, and New York that are not on the common support and thus are not likely to be able to have a synthetic control constructed for them. Appendix B provides a detailed description of the methodology.

The results of our synthetic control analysis begin with Figure 5 where we examine the impact of accelerator founding on the number of early-stage venture capital deals. In keeping with the synthetic control literature, both our parameter estimates and significance tests are displayed graphically. Panel A of Figure 5 displays the average observed level of deals in treatment and synthetic control units in the pre-treatment and post-treatment period. In the pre-period, treatment and control units evolve over time with common trends, but treated units show a marked increase in deal-flow after the arrival of the accelerator. Unlike the pre-trend, post-trend graphs in a differences and differences framework, these graphs display the raw averages across treated and untreated units. Thus, pre-treatment trends do not need to be "flat" but merely similar across treated and control units for the synthetic control estimates to be valid. In the first year after treatment, accelerated regions experienced an average of 1.28 more deals than their synthetic controls, and this difference reaches 2.59 deals in year two. These increases are off of an average of Panel B of figure 5 shows the p-values of each of these estimates, both of which are below 0.05 (0.041 and 0.001 for years 1 and 2 respectively).

Figure 6 presents the results of our synthetic control analysis for the number of distinct investors. Panel A graphically displays the quality of the pre-period match and the estimates for the treatment effect. As in Figure 5, the pre-treatment trends are similar for treatment and controls for our analysis of distinct investors in Figure 6, but a stark difference emerges after the arrival of accelerators in treated regions. In the first year after the arrival of an accelerator, treated regions

[10] This is essentially a factor model since the unobservables can be thought of as factor scores, $\lambda_t$, (which vary through time) weighted by factor loadings, $\mu_i$, (which vary cross-sectionally), both of which are unobservable in a factor analysis.

saw investment activity from 1.8 more distinct investors and this increased to 3.04 more investors in year two. Panel B of Figure 6 shows that these estimates of the impact of an accelerator being founded in a region were statistically significant, with p-values of 0.002 and 0.0001 for years 1 and 2 respectively.

Lastly, we present the results of our synthetic control analysis for logged dollar value of funds invested in a region. Seen graphically, the pre-trends in this treatment graph do not appear as stable as in a difference-in-difference graphical representation (or relative to the previous two synthetic control graphs). However, these synthetic control graphs are not meant to be flat before treatment; rather, they merely show the degree to which control units match treated units before the arrival of the accelerator—which remains the case for this outcome variable, as for the two prior. After the establishment of the accelerator, our synthetic control methods show a 1.51 increase in logged funding in year 1 which corresponds to a 151% increase in logged dollar funding levels in the treated group relative to control. In year 2, we see an increase of 2.29 corresponding to a 229% increase in the logged dollar funding levels of treated regions relative to controls. In Panel B, we display the p-values for our parameter estimates which show that both estimates are statistically significant, at the 0.08 and 0.007 level for years 1 and two respectively.

### 4.5 Shifts in Industry and Stage Composition

Our triple differences models provide evidence consistent with the impact of accelerator founding being isolated to the industries in which we would expect them to be. To add support to this result, we next run an analogous set of regressions that examine the composition of deals in accelerated and non-accelerated MSAs in terms of staging and industry. We would expect the arrival of an accelerator to shift the distribution of deals (at least in the window of our study) towards early stage deals within the treated industry and towards deals within the treated industry because it encourages additional seed and early stage activity in this specific industry.

We estimate a series of fractional logits (Hausman and Leonard, 1997; Papke and Woolridge, 2008) to examine whether there are shifts in deal-flow coinciding with accelerator founding towards the types of investments we expect to be affected by accelerators—early stage investments in software and IT firms. Table 8 explores the impact of accelerator arrival on the proportion of early versus later-stage deals done in an MSA. Columns (1) through (4) demonstrate statistically significant increases in early stage deals in the accelerated industries (Internet Specific and

Computer Software) after the introduction of an accelerator, ranging from 130% to 516% depending upon the measure and specification. Columns (5) through (8) show the opposite: there is no statistically significant relationship between the proportion of early stage deals in any other industry and the arrival of an accelerator.

Table 9 attempts to confirm these results by cutting the data another way and seeing the proportion of deals at each stage are from the accelerated industries. We estimate model similar to those in Table 8, but where the dependent variable is either the proportion of early stage deals in the MSA that are in the Software space (we expect this to increase) or the proportion of late stage deals in the MSA that are in the Software space (at least initially, we do not expect this to be affected). The estimates in the tables confirm our hypotheses: columns (1) through (4) show a significant change in the proportion of Software deals across all early stage, while Columns (5) through (8) confirm that there is no effect in the proportion of software deals in the later stage.

As we have seen, robustness checks using across multiple different econometric approaches and various modeling assumptions provide evidence consistent with a positive effect of accelerators on venture-backed startup activity. The impact of an accelerator's arrival is felt in the expected industries (Software and IT) at the expected deal stage (early deals) but there is no measured impact within a region in other industries or in later deal flow. These results lend credence to the notion that the matching procedure used to create the control group has adequately selected for similar regions to those treated and that the measured statistical relationship between accelerator founding and seed and early stage startup activity is less likely to be accounted for by fundamental unobserved differences between treated and control regions.

### 4.6 Local versus Remote Investors

In Table 10, we build upon our finding that increased venture-backed startup activity is accompanied by an increase in the number of active investment groups in the region by asking whether the increase in the number of distinct active investors is driven by an increase in investors located near the MSA or by investors located at a distance from the MSA. Column (1) of Table 10 provides the estimates for the model measuring the impact of accelerator founding on the count of number of early stage deals where at least one investor that participated in the round was headquartered more than 100 miles away from the headquarters of the startup company (distant investors). The coefficients from this model suggest a statistically significant increase of 146% in the number of deals with at least one distant investor participating in the round after the arrival of an accelerator, and the point estimate remains substantially positive and statistically significant when we add MSA-specific time trends in column (2), where shows a 112% increase in participation from distant investors. In columns (3) and (4), we similarly explore the impact of accelerator founding on the number of deals where the investor syndicate is comprised entirely of investors headquartered within 100 miles of the company headquarters (local investors). Here, we observe a statistically significant 115% increase in the number of deals with entirely local investors, a result that is robust to the inclusion of MSA-specific linear time trends in column (4).

Columns (5) and (6) of Table 10 present estimates of the impact of accelerator founding on the count of distinct distant investors active in the region. The coefficient in the baseline model in column (5) is statistically significant (95% increase in number of investors), and when we add the MSA-specific linear time trends in column (6), the magnitude of the coefficient remains substantially similar (suggesting an 85% increase) and remains statistically significant (although less so than the previous model). In columns (7), when we examine the impact of accelerator founding on the count of number of distinct local investors, we find that the founding of an accelerator leads to a statistically significant increase of 122% in the number of distinct local investors and similar to the previous results, the sign and statistical significance of the results hold when we add MSA-specific linear time trends in column (8). This holds in both specifications with and without the MSA-specific linear time trend.

Finally, we look at the impact of the arrival of an accelerator on the total number of dollars invested in a region by local and distant investors. In column (9) and (10), we explore the impact of accelerator founding on total investment from distant investors, finding no statistically significant relationship. In contrast, column (11) looks at the impact of accelerator founding on the total amount of early stage funding invested by local investors. Here, we find that the arrival of an accelerator is associated with a large and statistically significant increase in the amount of funding provided by local investors, and this result is robust to the inclusion of MSA-specific linear time trends in column (12).

Taken together, the estimates in Table 10 suggest that much of the increase in venture backed startup deal activity in the region following the arrival of an accelerator is financed not necessarily by the entrance of remotely-located investors into the region, but rather from new growth in investment groups local to the region itself. This is consistent with the idea that accelerators may serve as a catalyst for drawing together latent local forces to create an entrepreneurial cluster where it did not exist previously.

## 5. Conclusion

While the proliferation of accelerator programs over the last few years has been rapid, very little has been shown to date regarding their efficacy as institutions and intermediaries in the entrepreneurial ecosystem, including whether they indeed engender the spillover effects looked for by policy makers. With little information to inform decision-making processes, policy makers have struggled to determine how or if these programs should be supported or encouraged. This study provides some initial insights into the spillover effect that accelerator programs can have on the entrepreneurial ecosystem, by exploring their effects on the entrepreneurial financing environment in the local region.

Our findings suggest that accelerators positively impact the regional entrepreneurial ecosystem through spillovers of entrepreneurial activity, consistent with peer effects and provision of social capital to the region. MSAs in which an accelerator is established subsequently exhibit more seed and early stage venture-backed startup activity for non-accelerator graduates.

Certainly, this increase in activity may simply represent a shift of investment dollars and startup activity from other regions into the accelerator's region, possibly to the detriment of the other regions. Even if this is the case, however, if the presence of the accelerator increases activity in local region, this may meet the goals of both the accelerator founders and local policy makers. A second critique is that the companies being funded locally may simply be companies that would otherwise have gone to one of the coasts and been financed there, and now are instead financed in their original home regions. However, again, retaining companies locally is often a primary goal for local policy makers and for accelerator founders.

We note that as the average seed and early stage investment size has fallen in these industries over last 15 years, primarily due to reduction in the cost of experimentation (Ewens, Nanda and Rhodes-Kropf, 2013), angels have begun to emerge as a viable substitute for VC seed and A round investment. While we do not observe angel funding, it is likely that the effects we see for VC investment are also present at the angel level, and may be many times the VC effects.

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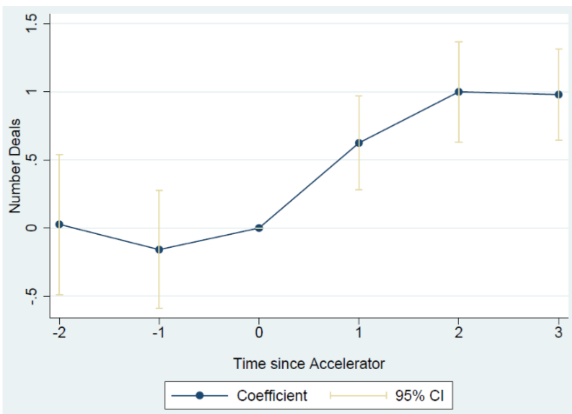
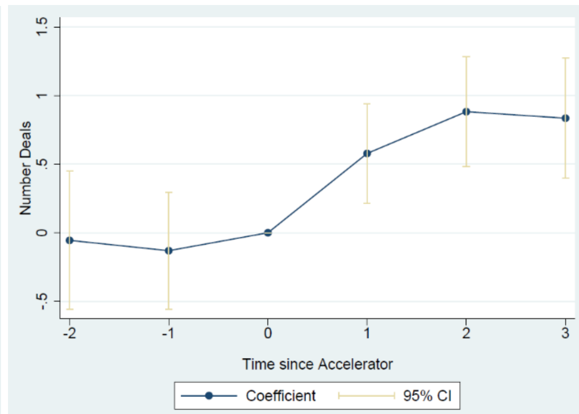
**Figure1. Treatment Effect for Treated Region over Time—Number of Deals**

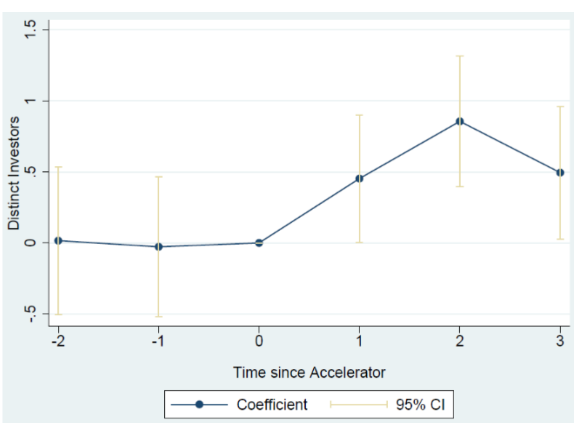



Without MSA-specific linear time trend

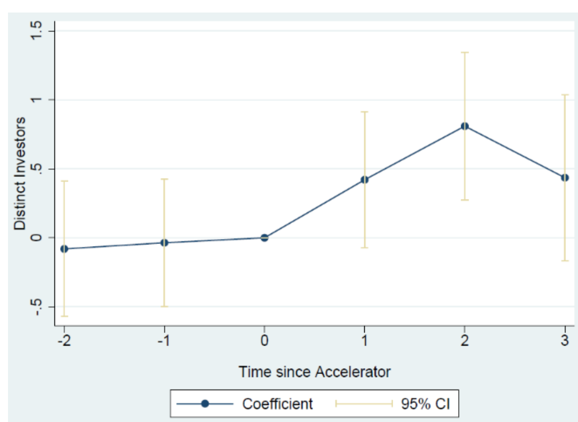



With MSA-specific linear time trend

**Figure 2. Treatment Effect for Treated Region over Time—Number of Distinct Investors**

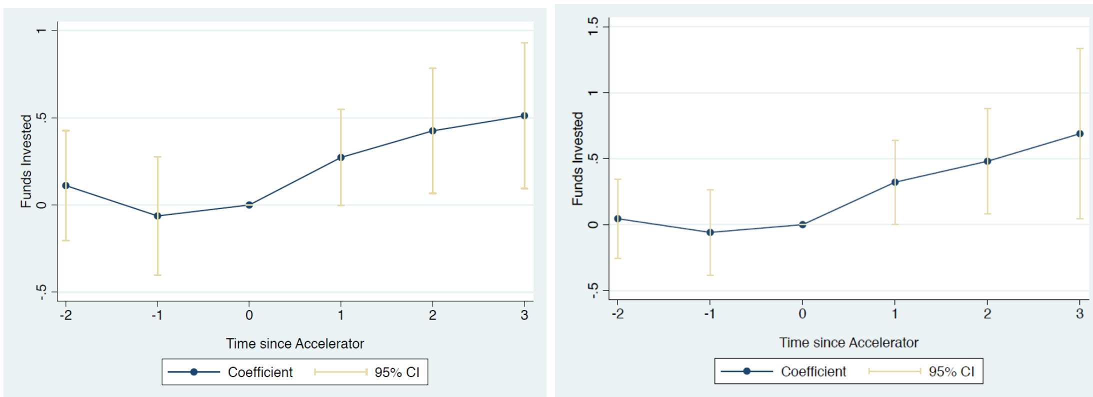



Without MSA-specific linear time trend



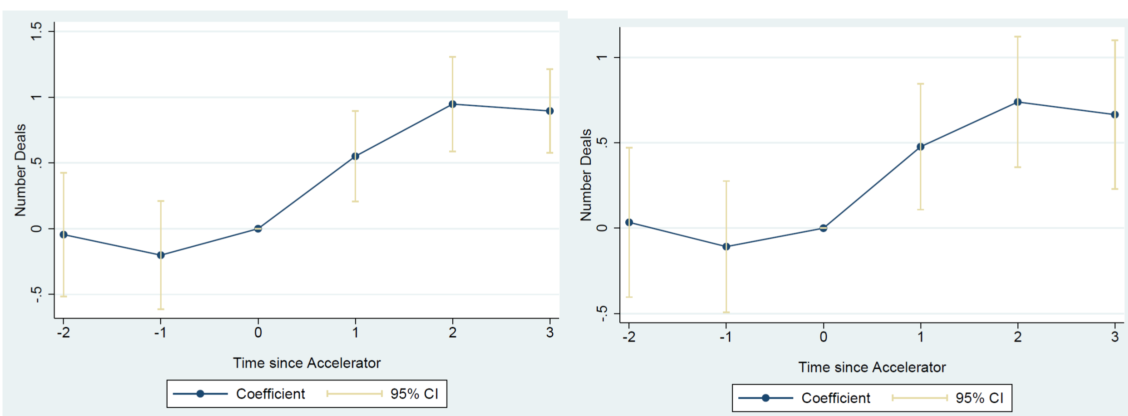

With MSA-specific linear time trend

## Figure 3. Treatment Effect in Treated Region over Time—Total $ Funding




Without MSA-specific linear time trend With MSA-specific linear time trend

## Figure 4. Treatment Effect for Treated Industry in Treated Region over Time—Number of Deals (Triple-Diff)

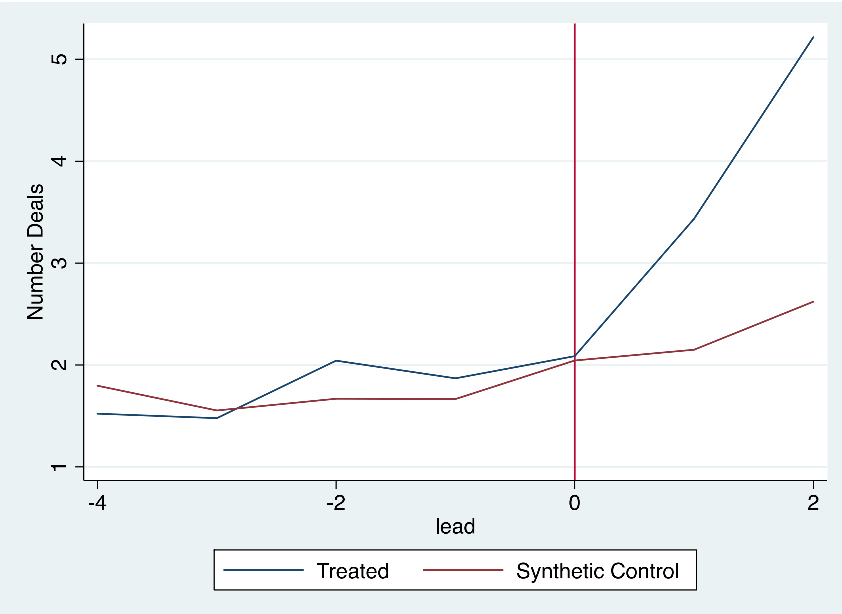



Without MSA-specific linear time trend With MSA-specific linear time trend

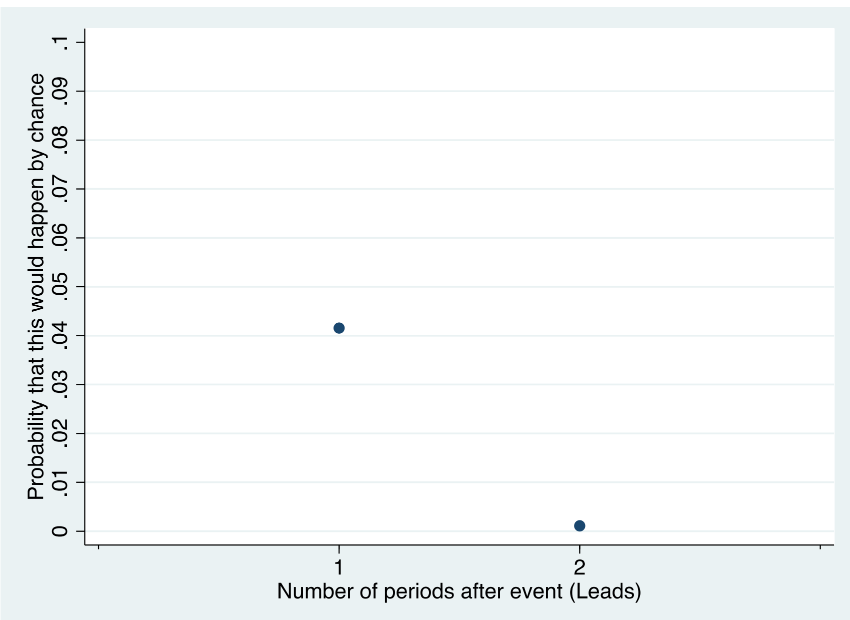
Note: Figure 4 shows the parameter estimates of a model for a series of dummy variables where treated region is interacted with treated industry and years before/since treatment dummies. Also included in these regressions, but not displayed, are separate dummies for treated region, treated industry, and year and MSA fixed effects.

**Figure 5: Synthetic Control Results for Number of Deals**

**Panel A: Synthetic Control Estimates of Impact of Accelerator Founding on Number of Deals**



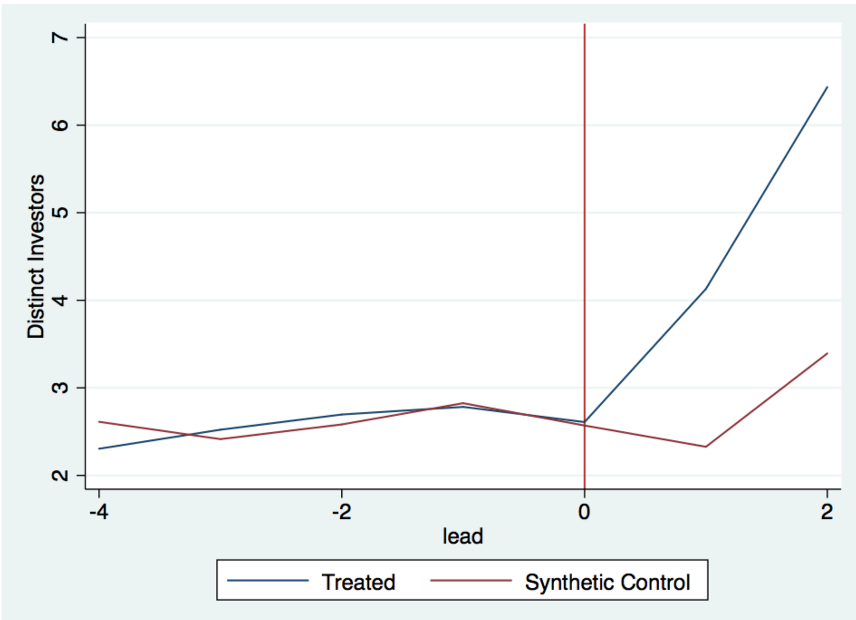

**Panel B: Lead Specific Significance Level (P-Values) for Number of Deals**



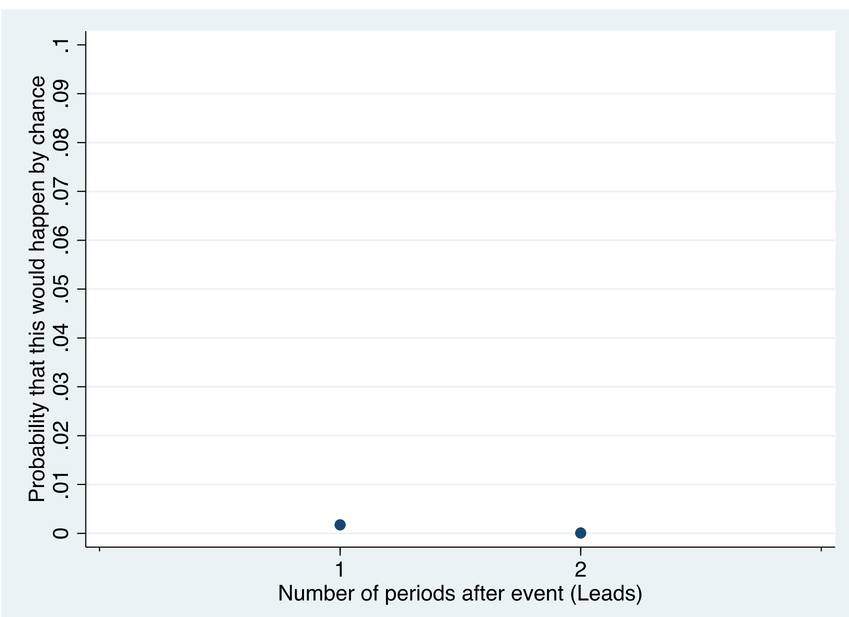

**Note:** Panel A of displays the average observed level of deals in treatment and synthetic control units in the pre-treatment and post-treatment period. Unlike trend graphs in a Dif-in-Diff framework, the pre-treatment trends do not need to be "flat" but merely similar across treated and control units for the synthetic control estimates to be valid. Panel B displays p-values for each post-treatment lag.

**Figure 6: Synthetic Control Results for Number of Investors**

**Panel A: Synthetic Control Estimates of Impact of Accelerator Founding on Number of Investors**



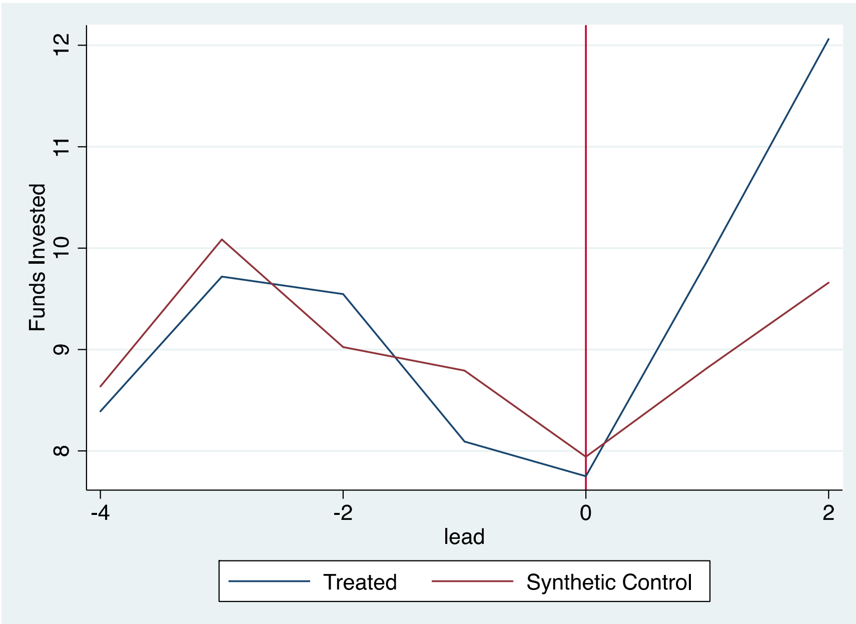

**Panel B: Lead Specific Significance Level (P-Values) for Number of Distinct Investors**



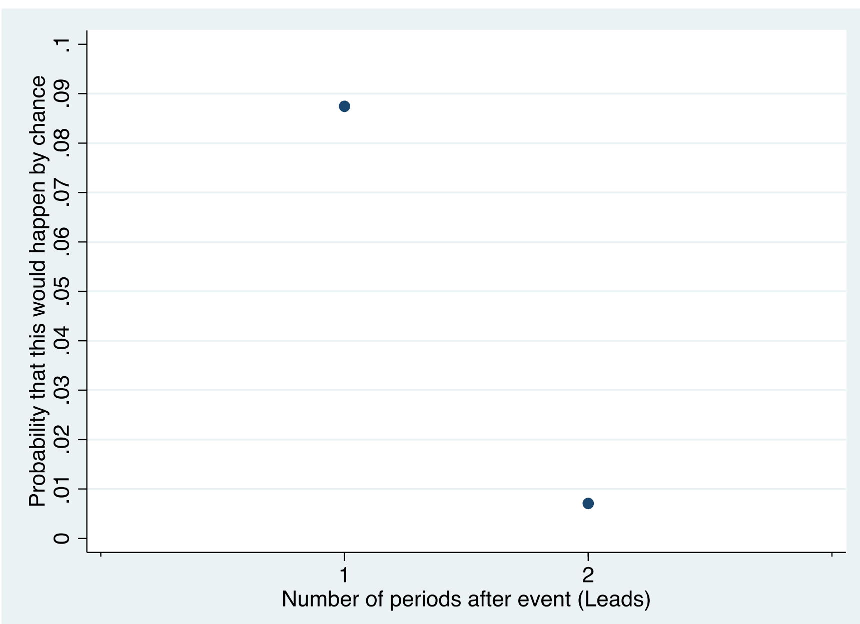

**Note:** Panel A of displays the average observed level of deals in treatment and synthetic control units in the pre-treatment and post-treatment period. Unlike trend graphs in a Dif-in-Diff framework, the pre-treatment trends do not need to be "flat" but merely similar across treated and control units for the synthetic control estimates to be valid. Panel B displays p-values for each post-treatment lag.

## Figure 7: Synthetic Control Results for Funds Invested

### Panel A: Synthetic Control Estimates of Impact of Accelerator Founding on Funds Invested




### Panel B: Lead Specific Significance Level (P-Values) for Funds Invested




**Note:** Panel A of displays the average observed level of deals in treatment and synthetic control units in the pre-treatment and post-treatment period. Unlike trend graphs in a Dif-in-Diff framework, the pre-treatment trends do not need to be "flat" but merely similar across treated and control units for the synthetic control estimates to be valid. Panel B displays p-values for each post-treatment lag.

**Table 1: List of Data Sources for MSA Level Data**

| VARIABLE | DESCRIPTION | SOURCE |
|---|---|---|
| Funds Invested | Logged Yearly Sum of Early Stage VC Dollars in MSA | VentureXpert |
| Number Deals | Yearly Count of Early Stage VC Financing Events by MSA | VentureXpert |
| Distinct Investors | Yearly Count of Early Stage Investors in MSA | VentureXpert |
| Patent Count | Yearly Count of Utility Patents in MSA | USPTO |
| # STEM Grad. Students | Yearly Count of STEM Graduate Students by State | NSF |
| Firm Births | Yearly Count of New Firms by MSA | US Census BDS |
| University R&D Spending | Yearly Sum of University R&D Spending in MSA | NSF |
| Per Capita Income | Per Capita Income at MSA Level | US Census |
| Employment | Employment at the MSA Level | US Census |

**Table 2: U.S.-Based Accelerator Programs Founded 2007-2012**

| | Accelerator Name | First Class Year | MSA | | Accelerator Name | First Class Year | MSA |
|---|---|---|---|---|---|---|---|
| 1 | Y Combinator | 2005 | San Jose-Sunnyvale-Santa Clara, CA | 31 | Dreamit Ventures - NYC | 2011 | New York-North. NJ-Long Island-PA |
| 2 | Techstars - Boulder | 2007 | Boulder, CO | 32 | gener8tor -- Milwaukee | 2012 | Milwaukee-Waukesha-West Allis, WI |
| 3 | Dreamit Ventures - Philadelphia | 2008 | Philadelphia-Camden-Wilmington | 33 | Hatch | 2012 | Virginia Beach-Norfolk-Newport News, VA-NC |
| 4 | AlphaLab | 2008 | Pittsburgh, PA | 34 | Blueprint Health | 2012 | New York-North. NJ-Long Island-PA |
| 5 | Tech Wildcatters | 2009 | Dallas-Fort Worth-Arlington, TX | 35 | StartFast Venture Accel. | 2012 | Syracuse, NY |
| 6 | Techstars - Boston | 2009 | Boston-Cambridge-Quincy, MA-NH | 36 | Accelerate Baltimore | 2012 | Baltimore-Towson, MD |
| 7 | Capital Factory | 2009 | Austin-Round Rock-San Marcos, TX | 37 | Telluride Venture Accel. | 2012 | Telluride, CO |
| 8 | First Growth Venture Network | 2009 | New York-North. NJ-Long Island-PA | 38 | Alchemist Accelerator | 2012 | San Jose-Sunnyvale-Santa Clara, CA |
| 9 | Betaspring | 2009 | Providence-New Bedford-Fall River | 39 | LaunchHouse | 2012 | Cleveland-Elyria-Mentor, OH |
| 10 | Launchpad LA | 2009 | Los Angeles-Long Beach-Santa Ana, CA | 40 | MindTheBridge | 2012 | San Francisco-Oakland-Fremont, CA |
| 11 | AngelPad | 2010 | San Francisco-Oakland-Fremont, CA | 41 | Techstars - Cloud | 2012 | San Antonio-New Braunfels, TX |
| 12 | Brandery | 2010 | Cincinnati-Middletown, OH-KY-IN | 42 | Healthbox -- Chicago | 2012 | Chicago-Joliet-Naperville, IL-IN-WI |
| 13 | BoomStartup | 2010 | Salt Lake City, UT | 43 | StartEngine | 2012 | Los Angeles-Long Beach-Santa Ana, CA |
| 14 | JumpStart Foundry | 2010 | Nashville-Davidson-Murfreesboro-Franklin, TN | 44 | SURGE Accelerator | 2012 | Houston-Sugar Land-Baytown, TX |
| 15 | Techstars - Chicago | 2010 | Chicago-Joliet-Naperville, IL-IN-WI | 45 | Triangle Startup Factory | 2012 | Durham-Chapel Hill, NC |
| 16 | Portland Incubator Experiment | 2010 | Portland-Vancouver-Hillsboro, OR-WA | 46 | Rock Health -- Boston | 2012 | Boston-Cambridge-Quincy, MA-NH |
| 17 | NYC Seed Start | 2010 | New York-North. NJ-Long Island-PA | 47 | MuckerLab | 2012 | Los Angeles-Long Beach-Santa Ana, CA |
| 18 | 500 Startups | 2010 | San Jose-Sunnyvale-Santa Clara, CA | 48 | The Iron Yard | 2012 | Greenville-Mauldin-Easley, SC |
| 19 | Techstars - Seattle | 2010 | Seattle-Tacoma-Bellevue, WA | 49 | Bizdom - Detroit | 2012 | Detroit-Warren-Livonia, MI |
| 20 | Entrepreneurs Roundtable | 2011 | New York-North. NJ-Long Island-PA | 50 | InnoSpring | 2012 | San Jose-Sunnyvale-Santa Clara, CA |
| 21 | FinTech Innovation Lab | 2011 | New York-North. NJ-Long Island-PA | 51 | NY Digital Health Accel. | 2012 | New York-North. NJ-Long Island-PA |
| 22 | NewMe | 2011 | San Jose-Sunnyvale-Santa Clara, CA | 52 | Co.Lab Accelerator | 2012 | Chattanooga, TN-GA |
| 23 | Portland Seed Fund | 2011 | Portland-Vancouver-Hillsboro, OR-WA | 53 | Tandem | 2012 | San Francisco-Oakland-Fremont, CA |
| 24 | Techstars - NYC | 2011 | New York-North. NJ-Long Island-PA | 54 | Blue Startups | 2012 | Honolulu, HI |
| 25 | Imagine K12 | 2011 | San Jose-Sunnyvale-Santa Clara, CA | 55 | TechLaunch | 2012 | New York-North. NJ-Long Island-PA |
| 26 | Seed Hatchery | 2011 | Memphis, TN-MS-AR | 56 | ARK Challenge | 2012 | Fayetteville-Springdale-Rogers, AR-MO |
| 27 | Rock Health -- San Francisco | 2011 | San Francisco-Oakland-Fremont, CA | 57 | Gener8tor -- Madison | 2012 | Madison, WI |
| 28 | Amplify.LA | 2011 | Los Angeles-Long Beach-Santa Ana, CA | 58 | Impact Engine | 2012 | Chicago-Joliet-Naperville, IL-IN-WI |
| 29 | Start Engine | 2011 | Los Angeles-Long Beach-Santa Ana, CA | 59 | Healthbox -- Boston | 2012 | Boston-Cambridge-Quincy, MA-NH |
| 30 | Capital Innovators | 2012 | St. Louis, MO-IL | | | | |

## Table 3. Summary Statistics – Full Sample

**Panel A: Summary Stats at the Year by MSA Level for Full Data**

| | Never-Treated | Ever-Treated | Treated, Pre-Treat | Treated, Post-Treat | Total |
|---|---|---|---|---|---|
| Funds Invested | 1.44 | 11.52 | 9.64 | 14.28 | 2.49 |
| | (4.43) | (7.76) | (7.93) | (6.62) | (5.76) |
| Number Deals | 0.34 | 18.42 | 8.97 | 32.33 | 2.22 |
| | (1.92) | (43.75) | (27.45) | (57.57) | (15.23) |
| Change in Number Deals (t-2) | 0.05 | 5.22 | 2.52 | 9.20 | 0.59 |
| | (1.22) | (18.11) | (11.64) | (24.24) | (6.14) |
| Change in Number Deals (t-3) | 0.07 | 7.60 | 3.76 | 13.24 | 0.85 |
| | (1.27) | (22.50) | (15.47) | (29.15) | (7.69) |
| Distinct Investors | 0.50 | 22.36 | 10.59 | 39.68 | 2.77 |
| | (2.47) | (50.38) | (29.50) | (67.16) | (17.68) |
| Patent Count | 0.11 | 1.35 | 0.82 | 2.13 | 0.24 |
| | (0.27) | (1.87) | (1.11) | (2.42) | (0.76) |
| # STEM Grad. Students | 20.55 | 22.19 | 19.12 | 26.71 | 20.72 |
| | (19.51) | (20.18) | (17.18) | (23.27) | (19.59) |
| Firm Births | 1.06 | 7.64 | 6.48 | 9.34 | 1.74 |
| | (2.16) | (11.04) | (9.67) | (12.64) | (4.56) |
| University R&D Spending | 0.07 | 0.70 | 0.54 | 0.94 | 0.14 |
| | (0.18) | (0.73) | (0.63) | (0.81) | (0.35) |
| Per Capita Income | 34.78 | 42.34 | 40.24 | 45.43 | 35.57 |
| | (5.99) | (9.31) | (9.67) | (7.81) | (6.81) |
| Employment | 0.25 | 1.77 | 1.47 | 2.22 | 0.41 |
| | (0.43) | (2.15) | (1.84) | (2.48) | (0.93) |

mean coefficients; sd in parentheses

**Panel B: Summary Stats at the Year by MSA Level excluding SF Bay Area and Boston**

| | Never-Treated | Ever-Treated | Treated, Pre-Treat | Treated, Post-Treat | Total |
|---|---|---|---|---|---|
| Funds Invested | 1.44 | 10.80 | 9.23 | 13.37 | 2.35 |
| | (4.43) | (7.66) | (7.81) | (6.69) | (5.56) |
| Number Deals | 0.34 | 9.19 | 4.54 | 16.81 | 1.20 |
| | (1.92) | (22.41) | (11.83) | (31.77) | (7.64) |
| Change in Number Deals (t-2) | 0.05 | 2.84 | 1.41 | 5.19 | 0.32 |
| | (1.22) | (9.46) | (6.43) | (12.67) | (3.26) |

| | | | | | |
|---|---|---|---|---|---|
| Change in Number Deals (t-3) | 0.07 | 4.10 | 1.99 | 7.57 | 0.46 |
| | (1.27) | (12.44) | (8.76) | (16.30) | (4.21) |
| Distinct Investors | 0.50 | 11.42 | 5.78 | 20.68 | 1.55 |
| | (2.47) | (26.94) | (13.51) | (38.57) | (9.26) |
| Patent Count | 0.11 | 0.98 | 0.70 | 1.43 | 0.19 |
| | (0.27) | (1.25) | (0.95) | (1.54) | (0.53) |
| # STEM Grad. Students | 20.55 | 19.79 | 18.13 | 22.50 | 20.48 |
| | (19.51) | (16.38) | (15.14) | (17.98) | (19.23) |
| Firm Births | 1.06 | 7.49 | 6.23 | 9.56 | 1.68 |
| | (2.16) | (11.45) | (9.78) | (13.56) | (4.52) |
| University R&D Spending | 0.07 | 0.63 | 0.50 | 0.86 | 0.13 |
| | (0.18) | (0.71) | (0.60) | (0.81) | (0.32) |
| Per Capita Income | 34.78 | 41.06 | 39.59 | 43.48 | 35.39 |
| | (5.99) | (8.50) | (9.24) | (6.47) | (6.54) |
| Employment | 0.25 | 1.73 | 1.41 | 2.24 | 0.39 |
| | (0.43) | (2.22) | (1.86) | (2.65) | (0.91) |

mean coefficients; sd in parentheses

**Table 4: Summary Statistics at the MSA-Year Level for Hazard-Rate Matched Sample**

| | Treated, Pre-treatment | Matched, Pre-treatment | Diff. | Treated, Post-treatment | Matched, Post-treatment | Diff. | Total |
|---|---|---|---|---|---|---|---|
| Funds Invested | 7.79 | 6.95 | 0.84 | 11.34 | 8.70 | 2.64** | 8.51 |
| | (7.62) | (7.78) | (0.88) | (7.12) | (7.92) | (1.33) | (7.74) |
| Number Deals | 1.50 | 1.91 | -0.32 | 5.46 | 2.49 | 2.97*** | 2.59 |
| | (2.40) | (3.28) | (0.31) | (6.20) | (3.30) | (0.89) | (4.07) |
| Change in Number Deals (t-2) | 0.12 | 0.12 | 0.00 | 2.70 | 0.42 | 2.27*** | 0.68 |
| | (2.11) | (2.14) | (0.26) | (4.15) | (2.67) | (0.63) | (2.89) |
| Change in Number Deals (t-3) | 0.21 | 0.17 | 0.03 | 3.38 | 0.56 | 2.82*** | 0.88 |
| | (2.20) | (2.57) | (0.31) | (4.72) | (2.67) | (0.69) | (3.26) |
| Distinct Investors | 2.36 | 3.16 | -0.80 | 5.94 | 4.14 | 1.81* | 3.51 |
| | (3.20) | (5.25) | (0.50) | (5.83) | (5.65) | (1.07) | (5.05) |
| Patent Count | 0.56 | 0.48 | 0.08 | 0.83 | 0.76 | 0.07 | 0.62 |
| | (0.62) | (0.63) | (0.07) | (0.73) | (0.89) | (0.14) | (0.71) |
| # STEM Grad. Students | 19.39 | 20.96 | -1.57 | 20.29 | 28.26 | -7.96 | 21.54 |
| | (14.60) | (18.25) | (1.74) | (14.41) | (23.42) | (3.38) | (17.67) |
| Firm Births | 4.88 | 3.42 | 1.46** | 5.52 | 3.55 | 1.97 | 4.33 |
| | (3.98) | (4.43) | (0.44) | (4.54) | (4.08) | (0.77) | (4.32) |
| University R&D Spending | 0.49 | 0.22 | 0.27*** | 0.66 | 0.38 | 0.27 | 0.42 |
| | (0.54) | (0.35) | (0.04) | (0.58) | (0.46) | (0.09) | (0.51) |
| Per Capita Income | 38.50 | 37.69 | 0.81 | 41.78 | 40.92 | 0.86 | 39.30 |
| | (4.28) | (5.31) | (0.54) | (3.94) | (4.57) | (0.75) | (4.87) |
| Employment | 1.22 | 0.75 | 0.47*** | 1.52 | 0.85 | 0.67 | 1.07 |
| | (0.91) | (0.87) | (0.09) | (1.13) | (0.89) | (0.18) | (0.98) |

**Note:** This table provides summary statistics of regions before and after treatment for the hazard-rate matched sample. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. Standard deviations are displayed in parentheses for columns 1 and 2. Standard errors in parentheses for column 3.

**Table 5: Differences between Accelerated Regions in and out of Hazard-Rate Matched Subsample**

| | Not In Subsample | In Hazard-Rate Subsample | Difference |
|---|---|---|---|
| Local Founder | 0.286 | 0.479 | $-0.193^{***}$ |
| | (0.45) | (0.54) | (0.08) |
| Migration Distance | 3,148.03 | 929.92 | $2{,}218.11^{***}$ |
| | (3,973.76) | (1,757.50) | (598.48) |
| Log Local Investments | 19.07 | 9.63 | $9.443^{***}$ |
| | (3.31) | (7.61) | (.899) |
| Number Deals | 138.54 | 3.94 | $134.60^{***}$ |
| | (109.55) | (4.68) | (15.74) |
| Change in Number Deals | 41.35 | 2.06 | $39.29^{***}$ |
| (t-2) | (50.46) | (3.39) | (7.27) |
| Change in Number Deals | 53.87 | 1.73 | $52.14^{***}$ |
| (t-3) | (53.22) | (3.56) | (7.70) |
| Distinct Investors | 155.68 | 4.81 | $150.87^{***}$ |
| | (110.57) | (5.56) | (15.87) |

Note: This table provides summary statistics of regions that have received accelerators tabulated by whether or not they are included in the hazard-rate matched sample. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. Standard deviations are displayed in parentheses for columns 1 and 2. Standard errors in parentheses for column 3.

## Table 6: Fixed Effects Models on Hazard-Rate Matched Subsample

Panel A: Without Portfolio Firms

| | (1) Number Deals | (2) Number Deals | (3) Distinct Investors | (4) Distinct Investors | (5) Funds Invested | (6) Funds Invested |
|---|---|---|---|---|---|---|
| Accelerator Active | 2.292$^{***}$ | 2.026$^{***}$ | 1.987$^{***}$ | 1.879$^{***}$ | 1.721 | 2.651$^{*}$ |
| | (0.337) | (0.298) | (0.347) | (0.408) | (1.216) | (1.525) |
| Patent Count | 0.758 | 2.083$^{***}$ | 0.715$^{**}$ | 1.397 | -4.208$^{*}$ | -3.296 |
| | (0.143) | (0.497) | (0.118) | (0.548) | (2.194) | (2.442) |
| # STEM Grad. Students | 1.042$^{*}$ | 1.118$^{***}$ | 1.045 | 1.087 | -0.200 | -0.158 |
| | (0.022) | (0.037) | (0.036) | (0.069) | (0.194) | (0.308) |
| Firm Births | 0.992 | 0.977 | 1.079 | 1.059 | 0.350 | 0.981 |
| | (0.080) | (0.089) | (0.083) | (0.089) | (0.495) | (0.762) |
| University R&D Spending | 2.133 | 2.728$^{***}$ | 2.719$^{***}$ | 3.499$^{*}$ | 7.521$^{***}$ | 3.498 |
| | (1.155) | (0.973) | (0.995) | (2.539) | (2.257) | (4.339) |
| Per Capita Income | 0.861$^{***}$ | 0.893 | 0.855$^{***}$ | 0.889 | -0.739$^{***}$ | -0.854 |
| | (0.047) | (0.084) | (0.038) | (0.086) | (0.272) | (0.543) |
| Employment | 0.073$^{***}$ | 0.236 | 0.061$^{***}$ | 0.042$^{***}$ | -8.164$^{**}$ | -22.699$^{**}$ |
| | (0.045) | (0.278) | (0.031) | (0.048) | (3.247) | (8.912) |
| Observations | 451 | 451 | 451 | 451 | 451 | 451 |
| R-squared | | | | | 0.107 | 0.210 |
| log-likelihood | -525.443 | -473.091 | -712.560 | -642.751 | -1356.850 | -1321.873 |
| MSA Fixed Effects | YES | YES | YES | YES | YES | YES |
| Year Fixed Effects | YES | YES | YES | YES | YES | YES |
| MSA-Specific Linear Trend | NO | YES | NO | YES | NO | YES |

### Panel B: With Portfolio Firms

| | (1) Number Deals | (2) Number Deals | (3) Distinct Investors | (4) Distinct Investors | (5) Funds Invested | (6) Funds Invested |
|---|---|---|---|---|---|---|
| Accelerator Active | 2.374$^{***}$ | 2.043$^{***}$ | 1.986$^{***}$ | 1.856$^{***}$ | 2.960$^{**}$ | 3.899$^{**}$ |
| | (0.309) | (0.267) | (0.333) | (0.403) | (1.340) | (1.651) |
| Patent Count | 0.678$^{*}$ | 1.700$^{**}$ | 0.647$^{***}$ | 1.213 | -5.114$^{**}$ | -4.743 |
| | (0.139) | (0.421) | (0.104) | (0.465) | (2.443) | (2.854) |
| # STEM Grad. Students | 1.015 | 1.110$^{***}$ | 1.036 | 1.086 | 0.040 | -0.116 |
| | (0.024) | (0.037) | (0.034) | (0.067) | (0.206) | (0.350) |
| Firm Births | 0.977 | 0.985 | 1.088 | 1.052 | 0.682 | 1.370 |
| | (0.079) | (0.088) | (0.079) | (0.093) | (0.552) | (0.903) |

| | | | | | | |
|---|---|---|---|---|---|---|
| University R&D Spending | 1.841 | 2.563** | 2.488** | 3.468 | 7.843*** | 3.759 |
| | (1.138) | (1.002) | (0.964) | (2.642) | (2.509) | (5.509) |
| Per Capita Income | 0.881** | 0.895 | 0.860*** | 0.894 | -0.863*** | -0.817 |
| | (0.048) | (0.085) | (0.037) | (0.089) | (0.284) | (0.581) |
| Employment | 0.075*** | 0.225 | 0.057*** | 0.068** | -12.093*** | -31.075** |
| | (0.045) | (0.218) | (0.028) | (0.078) | (3.078) | (12.457) |
| Observations | 451 | 451 | 451 | 451 | 451 | 451 |
| R-squared | | | | | 0.107 | 0.210 |
| log-likelihood | -538.902 | -486.561 | -724.843 | -662.893 | -1357.225 | -1329.485 |
| MSA Fixed Effects | YES | YES | YES | YES | YES | YES |
| Year Fixed Effects | YES | YES | YES | YES | YES | YES |
| MSA-Specific Linear Trend | NO | YES | NO | YES | NO | YES |

Note: In Panel A, we explore the impact of the arrival of an accelerator on investment activity outside the accelerator while Panel B includes each accelerator's portfolio firms. Across both panels, we run the same models. Models (1) - (4) are QML Poisson fixed effects models with exponentiated coefficients — incident rate ratios (IRR)— so that a coefficient of 1 represents no effect and a 2 represents a 100% increase. Z-Statistic and p-value calculations are made based on untransformed models. Models (5) - (6) are log linear models where the dependent variable is logged dollars. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. Standard errors in parentheses. All standard errors are clustered at the MSA level.

## Table 7: Triple Diff Models on Hazard-Rate Matched Sub-Sample

| | (1)<br>Number<br>Deals | (2)<br>Number<br>Deals | (3)<br>Distinct<br>Investors | (4)<br>Distinct<br>Investors | (5)<br>Funds<br>Invested | (6)<br>Funds<br>Invested |
|---|---|---|---|---|---|---|
| Treated Region X Treated Industry X Post-Treatment | 4.286$^{***}$ | 3.807$^{***}$ | 2.949$^{**}$ | 2.677$^{*}$ | 0.346 | -0.373 |
| | (1.911) | (1.655) | (1.483) | (1.347) | (3.362) | (3.578) |
| Treated Industry X Post-Treatment | 1.056 | 1.218 | 1.285 | 1.441$^{*}$ | 2.927 | 3.762$^{*}$ |
| | (0.191) | (0.233) | (0.231) | (0.279) | (1.929) | (2.146) |
| Post-Treatment X Treated Region | 0.545 | 0.491$^{*}$ | 0.604 | 0.560 | -0.675 | -1.268 |
| | (0.204) | (0.190) | (0.269) | (0.264) | (0.970) | (1.132) |
| Patent Count | 0.717$^{*}$ | 1.917$^{***}$ | 0.647$^{**}$ | 1.348 | -2.213$^{**}$ | 2.827 |
| | (0.127) | (0.452) | (0.113) | (0.426) | (0.893) | (2.642) |
| # STEM Grad. Students | 1.003 | 1.079$^{***}$ | 0.985 | 1.048 | -0.286 | 0.112 |
| | (0.023) | (0.030) | (0.032) | (0.049) | (0.177) | (0.272) |
| Firm Births | 0.925 | 0.944 | 0.990 | 1.026 | 0.100 | 0.561 |
| | (0.076) | (0.101) | (0.074) | (0.098) | (0.741) | (0.892) |
| University R&D Spending | 1.859$^{*}$ | 1.375 | 1.971$^{***}$ | 1.248 | 1.296 | -2.346 |
| | (0.678) | (0.627) | (0.464) | (0.921) | (1.672) | (3.122) |
| Per Capita Income | 0.870$^{***}$ | 0.860$^{*}$ | 0.880$^{***}$ | 0.870 | -0.097 | -0.397 |
| | (0.045) | (0.077) | (0.039) | (0.089) | (0.171) | (0.456) |
| Employment | 0.113$^{***}$ | 0.309 | 0.131$^{***}$ | 0.076$^{**}$ | -3.428 | -4.544 |
| | (0.065) | (0.417) | (0.071) | (0.084) | (5.538) | (13.404) |
| Observations | 902 | 902 | 902 | 902 | 902 | 902 |
| R-squared | | | | | 0.141 | 0.192 |
| | -823.758 | -766.611 | -1216.297 | -1134.202 | -2998.288 | -2970.559 |
| MSA Fixed Effects | YES | YES | YES | YES | YES | YES |
| Year Fixed Effects | YES | YES | YES | YES | YES | YES |
| MSA-Specific Linear Trend | NO | YES | NO | YES | NO | YES |

Note: Because the semiconductor industry is a less-treated industry than software/IT, we exploit this within-MSA variation in a triple differences specification. By including an additional industry within our estimation, we can now control for within-MSA effects. Models (1) - (4) are QML Poisson fixed effects models with exponentiated coefficients — incident rate ratios (IRR)— so that a coefficient of 1 represents no effect and a 2 represents a 100% increase. Z-Statistic and p-value calculations are made based on untransformed models. Models (5) - (6) are log linear models where the dependent variable is logged dollars. All standard errors are clustered at the MSA level. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. Standard errors in parentheses.

## Table 8: Fractional Logit for Early Stage Funding as Proportion of Total Funding

| | (1) Proportion Early Funding $ | (2) Proportion Early Funding $ | (3) Proportion Early Funding (# Deals) | (4) Proportion Early Funding (# Deals) | (5) Proportion Early Funding $ | (6) Proportion Early Funding $ | (7) Proportion Early Funding (# Deals) | (8) Proportion Early Funding (# Deals) |
|---|---|---|---|---|---|---|---|---|
| Industry Segment | Software/IT | Software/IT | Software/IT | Software/IT | All Other | All Other | All Other | All Other |
| Accelerator Active | 2.308* | 4.803** | 2.869** | 6.166** | 0.353 | -0.492 | 0.782 | 0.046 |
| | (1.107) | (3.509) | (1.306) | (4.427) | (0.540) | (0.829) | (0.543) | (0.859) |
| Patent Count | 0.126** | 0.521 | 0.130** | 0.408 | -0.989 | 0.164 | -1.044 | 0.371 |
| | (0.130) | (0.673) | (0.131) | (0.669) | (0.769) | (1.502) | (0.800) | (1.377) |
| # STEM Grad. Students | 1.110 | 1.168 | 1.090 | 1.066 | 0.188 | -0.050 | 0.236 | -0.037 |
| | (0.097) | (0.149) | (0.094) | (0.140) | (0.176) | (0.213) | (0.189) | (0.201) |
| Firm Births | 1.385 | 1.421 | 1.554 | 1.569 | 0.145 | -0.021 | 0.385 | 0.157 |
| | (0.383) | (0.686) | (0.462) | (0.779) | (0.241) | (0.535) | (0.263) | (0.511) |
| University R&D Spending | 31.791* | 4.331 | 116.117** | 212.307 | 1.084* | 0.971 | 2.107** | 1.706 |
| | (62.264) | (12.353) | (229.505) | (880.998) | (0.612) | (1.188) | (0.832) | (1.458) |
| Per Capita Income | 0.731*** | 0.848 | 0.694*** | 0.867 | -0.296*** | -0.272 | -0.301*** | -0.127 |
| | (0.089) | (0.207) | (0.089) | (0.206) | (0.104) | (0.241) | (0.109) | (0.231) |
| Employment | 0.007*** | 0.000 | 0.010*** | 0.000* | -3.639 | -6.568 | -4.206 | -7.563 |
| | (0.013) | (0.001) | (0.016) | (0.000) | (2.781) | (10.278) | (2.809) | (9.998) |
| Observations | 484 | 484 | 484 | 484 | 484 | 484 | 484 | 484 |
| log-likelihood | -202.154 | -179.783 | -192.588 | -171.073 | -194.280 | -162.946 | -183.530 | -153.372 |
| MSA Fixed Effects | YES | YES | YES | YES | YES | YES | YES | YES |
| Year Fixed Effects | YES | YES | YES | YES | YES | YES | YES | YES |
| MSA-Specific Linear Trend | NO | YES | NO | YES | NO | YES | NO | YES |

Note: In this table, we confirm that the accelerator effect is localized to early stage deals in accelerated industries by measuring the composition of venture capital investment along the two dimensions. In models 11-1 through 11-4, we use fractional logit models to examine how the arrival of an accelerator in a region impacts the percentage of early stage deals funded (dollars invested) relative to later stage deals (total invested dollars) in accelerated industries. In models 11-5 through 11-8, we then examine that relationship across all other industries receiving VC investments.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

**Table 9: Fractional Logit for Software/IT Funding as Proportion of Total Funding**

| | (1) Proportion Accel Funding $ | (2) Proportion Accel Funding $ | (3) Proportion Accel Funding (# Deals) | (4) Proportion Accel Funding (# Deals) | (5) Proportion Accel Funding $ | (6) Proportion Accel Funding $ | (7) Proportion Accel Funding (# Deals) | (8) Proportion Accel Funding (# Deals) |
|---|---|---|---|---|---|---|---|---|
| Stage | Early | Early | Early | Early | Later | Later | Later | Later |
| Accelerator Active | 1.786* | 2.035 | 2.243*** | 2.859** | 0.435 | -0.260 | 0.581 | 0.023 |
| | (0.542) | (1.016) | (0.622) | (1.272) | (0.700) | (0.878) | (0.681) | (0.863) |
| Patent Count | 0.440 | 1.557 | 1.027 | 3.158 | 0.535 | 1.317 | 0.698 | 2.058 |
| | (0.242) | (1.241) | (0.517) | (2.492) | (0.949) | (1.746) | (1.003) | (1.555) |
| # STEM Grad. Students | 0.989 | 1.088 | 1.034 | 1.097 | -0.241** | -0.385* | -0.190* | -0.253 |
| | (0.087) | (0.170) | (0.098) | (0.142) | (0.106) | (0.226) | (0.106) | (0.239) |
| Firm Births | 1.267 | 1.003 | 1.081 | 0.955 | -0.075 | 0.167 | 0.083 | 0.354 |
| | (0.235) | (0.321) | (0.175) | (0.252) | (0.322) | (0.544) | (0.287) | (0.426) |
| University R&D Spending | 10.205*** | 9.369 | 10.190*** | 11.967 | 1.541 | 7.568** | 1.922 | 7.334** |
| | (5.718) | (13.754) | (7.795) | (22.381) | (1.434) | (3.198) | (1.588) | (3.023) |
| Per Capita Income | 0.831** | 0.876 | 0.839** | 0.843 | -0.228 | -0.614* | -0.192 | -0.531 |
| | (0.061) | (0.157) | (0.062) | (0.129) | (0.144) | (0.352) | (0.146) | (0.361) |
| Employment | 0.617 | 16.614 | 0.353 | 0.897 | -1.874 | -13.242*** | -2.894 | -14.210*** |
| | (0.542) | (55.205) | (0.384) | (3.003) | (1.839) | (4.352) | (1.855) | (3.506) |
| Observations | 484 | 484 | 484 | 484 | 484 | 484 | 484 | 484 |
| log-likelihood | -196.279 | -181.973 | -189.020 | -174.034 | -133.980 | -105.823 | -133.055 | -105.343 |
| MSA Fixed Effects | YES | YES | YES | YES | YES | YES | YES | YES |
| Year Fixed Effects | YES | YES | YES | YES | YES | YES | YES | YES |
| MSA-Specific Linear Trend | NO | YES | NO | YES | NO | YES | NO | YES |

Note: These fractional logit models help assess whether changes in the distribution of venture capital match our predictions for the expected impact of the arrival of an accelerator. The dependent variable is the proportion of total funding (early vs. later) as noted in the stage row.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

## Table 10: Fixed Effects Models for Near and Distant Investors

| | (1) Number Deals, Far | (2) Number Deals, Far | (3) Number Deals, Near | (4) Number Deals, Near | (5) Distinct Investors, Far | (6) Distinct Investors, Far | (7) Distinct Investors, Near | (8) Distinct Investors, Near | (9) Funds Invested, Far | (10) Funds Invested, Far | (11) Funds Invested, Near | (12) Funds Invested, Near |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Accelerator Active | 2.458$^{***}$ | 2.116$^{***}$ | 2.157$^{***}$ | 1.854$^{***}$ | 1.949$^{***}$ | 1.848$^{*}$ | 2.220$^{***}$ | 1.806$^{**}$ | 2.081 | 2.529 | 2.352$^{**}$ | 2.543$^{**}$ |
| | (0.615) | (0.601) | (0.398) | (0.361) | (0.443) | (0.617) | (0.450) | (0.431) | (1.551) | (2.173) | (1.053) | (1.192) |
| Patent Count | 0.835 | 1.879$^{*}$ | 0.563 | 2.418 | 0.797 | 1.380 | 0.647$^{*}$ | 1.516 | -4.314$^{*}$ | -6.081$^{**}$ | -6.641$^{***}$ | 0.077 |
| | (0.134) | (0.638) | (0.239) | (1.392) | (0.155) | (0.692) | (0.155) | (0.809) | (2.288) | (2.751) | (1.731) | (2.709) |
| # STEM Grad. Students | 1.056 | 1.118$^{**}$ | 1.025 | 1.201$^{**}$ | 1.044 | 1.091 | 1.055 | 1.133$^{*}$ | -0.382 | -0.519 | -0.064 | 0.690$^{**}$ |
| | (0.039) | (0.062) | (0.068) | (0.086) | (0.053) | (0.101) | (0.053) | (0.074) | (0.282) | (0.414) | (0.252) | (0.325) |
| Firm Births | 1.052 | 1.108 | 0.897 | 0.978 | 0.987 | 0.987 | 1.119 | 1.157 | 0.635 | 1.411 | -0.286 | -0.045 |
| | (0.102) | (0.139) | (0.086) | (0.126) | (0.089) | (0.114) | (0.105) | (0.156) | (0.528) | (0.884) | (0.600) | (0.906) |
| University R&D Spending | 2.098$^{**}$ | 3.199$^{**}$ | 1.507 | 1.251 | 2.866$^{**}$ | 4.503 | 2.425$^{**}$ | 2.248 | 7.444$^{***}$ | 6.229 | 5.393$^{***}$ | 0.340 |
| | (0.667) | (1.551) | (1.108) | (0.898) | (1.174) | (4.207) | (0.880) | (1.745) | (2.712) | (4.684) | (1.789) | (2.804) |
| Per Capita Income | 0.906 | 0.858 | 0.775$^{***}$ | 0.779$^{***}$ | 0.922 | 0.860 | 0.764$^{***}$ | 0.949 | -0.427 | -1.174$^{**}$ | -0.730$^{***}$ | 0.156 |
| | (0.056) | (0.104) | (0.055) | (0.073) | (0.051) | (0.091) | (0.037) | (0.092) | (0.298) | (0.580) | (0.223) | (0.270) |
| Employment | 0.043$^{***}$ | 0.019$^{*}$ | 0.261 | 1.329 | 0.062$^{***}$ | 0.026$^{*}$ | 0.059$^{***}$ | 0.055$^{*}$ | -10.152$^{**}$ | -24.012 | 2.341 | 5.280 |
| | (0.026) | (0.041) | (0.316) | (2.223) | (0.041) | (0.053) | (0.047) | (0.092) | (4.198) | (15.169) | (4.181) | (10.255) |
| Observations | 429 | 429 | 341 | 341 | 429 | 429 | 352 | 352 | 451 | 451 | 451 | 451 |
| R-squared | | | | | | | | | 0.089 | 0.223 | 0.093 | 0.232 |
| log-likelihood | -386.853 | -352.427 | -327.411 | -288.439 | -488.267 | -442.753 | -373.995 | -343.014 | -1374.429 | -1338.523 | -1313.149 | -1275.544 |
| MSA Fixed Effects | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Year Fixed Effects | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| MSA-Specific Linear Trend | NO | YES | NO | YES | NO | YES | NO | YES | NO | YES | NO | YES |

Note: The regressions in this table build upon Table 6 by exploring one source of treatment heterogeneity: the distance between the investor's headquarters and their early-stage investments. We define a Near Deal as any investment made within 100 miles of the investor's headquarters and define a Far Deal as any investment made farther than 100 miles. Models (1) - (9) are QML Poisson fixed effects models with exponentiated coefficients — incident rate ratios (IRR)— so that a coefficient of 1 represents no effect and a 2 represents a 100% increase. Z-Statistic and p-value calculations are made based on untransformed models. Models (10) - (12) are log linear models where the dependent variable is logged dollars. All standard errors are clustered at the MSA level. .$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. Standard errors in parentheses.

**Table 11: Fixed Effects Models for Angel Groups Near**

| | (1) Number Deals | (2) Number Deals | (3) Distinct Investors | (4) Distinct Investors | (5) Funds Invested | (6) Funds Invested |
|---|---|---|---|---|---|---|
| Accelerator Active X No Angel Group | 4.403*** (1.642) | 4.827** (3.033) | 2.945** (1.421) | 3.314* (2.022) | 0.880 (2.087) | 1.591 (2.581) |
| Accelerator Active X Angel Group | 2.260*** (0.333) | 1.990*** (0.295) | 1.973*** (0.346) | 1.830*** (0.405) | 1.916 (1.237) | 2.982* (1.644) |
| Patent Count | 0.762 (0.144) | 2.094*** (0.504) | 0.717** (0.118) | 1.218 (0.468) | -4.266* (2.212) | -3.402 (2.444) |
| # STEM Grad. Students | 1.038* (0.022) | 1.118*** (0.037) | 1.043 (0.036) | 1.086 (0.067) | -0.176 (0.205) | -0.164 (0.306) |
| Firm Births | 0.989 (0.079) | 0.978 (0.089) | 1.077 (0.082) | 1.053 (0.093) | 0.385 (0.497) | 0.978 (0.759) |
| University R&D Spending | 2.174 (1.170) | 2.776*** (1.000) | 2.744*** (0.998) | 3.500 (2.670) | 7.353*** (2.232) | 3.366 (4.310) |
| Per Capita Income | 0.862*** (0.047) | 0.893 (0.084) | 0.855*** (0.038) | 0.894 (0.090) | -0.749*** (0.274) | -0.853 (0.543) |
| Employment | 0.077*** (0.048) | 0.231 (0.274) | 0.062*** (0.032) | 0.067** (0.077) | -8.464** (3.341) | -22.598** (8.861) |
| Observations | 451 | 451 | 451 | 451 | 451 | 451 |
| R-squared | | | | | 0.088 | 0.219 |
| log-likelihood | -524.762 | -472.770 | -712.253 | -642.709 | -1356.696 | -1321.700 |
| MSA Fixed Effects | YES | YES | YES | YES | YES | YES |
| Year Fixed Effects | YES | YES | YES | YES | YES | YES |
| MSA-Specific Linear Trend | NO | YES | NO | YES | NO | YES |

Note: This table explores the impact of prior existing formal angel groups on the relationship between accelerator founding and early stage venture investment. We define formal angel group as group with regularly schedule angel meetings that are published on the web or in angel directories.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. All standard errors are clustered at the MSA level

| | (1) Number Deals | (2) Number Deals | (3) Distinct Investors | (4) Distinct Investors | (5) Funds Invested | (6) Funds Invested |
|---|---|---|---|---|---|---|
| Highly Rated Accelerator | 3.035*** | 1.914*** | 2.253*** | 1.868** | 3.956*** | 3.117 |
| | (0.568) | (0.280) | (0.421) | (0.541) | (1.418) | (2.060) |
| Unrated Accelerator | 1.771** | 2.239*** | 1.712** | 1.840* | 1.791 | 4.605** |
| | (0.401) | (0.484) | (0.409) | (0.577) | (1.624) | (2.213) |
| Patent Count | 0.655** | 1.723** | 0.639*** | 1.211 | -5.185** | -4.678 |
| | (0.117) | (0.414) | (0.096) | (0.452) | (2.405) | (2.847) |
| # STEM Grad. Students | 1.026 | 1.109*** | 1.037 | 1.086 | 0.031 | -0.129 |
| | (0.022) | (0.038) | (0.036) | (0.067) | (0.211) | (0.354) |
| Firm Births | 1.006 | 0.993 | 1.100 | 1.051 | 0.746 | 1.444 |
| | (0.075) | (0.087) | (0.079) | (0.088) | (0.536) | (0.952) |
| University R&D Spending | 2.017 | 2.547** | 2.565*** | 3.470 | 7.605*** | 3.897 |
| | (0.925) | (0.991) | (0.855) | (2.634) | (2.429) | (5.534) |
| Per Capita Income | 0.863*** | 0.897 | 0.854*** | 0.894 | -0.876*** | -0.813 |
| | (0.046) | (0.085) | (0.036) | (0.089) | (0.280) | (0.581) |
| Employment | 0.047*** | 0.193* | 0.047*** | 0.069** | -13.326*** | -32.590** |
| | (0.029) | (0.190) | (0.024) | (0.077) | (3.003) | (13.869) |
| Observations | 451 | 451 | 451 | 451 | 451 | 451 |
| R-squared | | | | | 0.195 | 0.282 |
| log-likelihood | -534.116 | -486.453 | -723.156 | -662.892 | -1514.608 | -1488.938 |
| MSA Fixed Effects | YES | YES | YES | YES | YES | YES |
| Year Fixed Effects | YES | YES | YES | YES | YES | YES |
| MSA-Specific Linear Trend | NO | YES | NO | YES | NO | YES |

Note: In this table, we focus on variation at the accelerator level and examine how accelerator quality drives aggregate levels of early-stage financing in that region. To do so, we build upon the empirical framework in Table 6 (and described in section 3, by allowing a separate parameter estimate, $\delta_j$, for each of the two dichotomous accelerator-level variables we measure (i.e. high/low quality accelerators) .$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. Standard errors in parentheses.

## Appendix A: Moving Founding Years by Arbitrary Amounts

In this section, we provide evidence that our preferred models in Table 6 with MSA-specific linear time trends measure changes in the overall trend in funding dynamic for a region rather than merely measure changes in level. To do so, we conduct a series of placebo checks by artificially varying the years in which an accelerator arrives in a region by various leading and lagging amounts and then rerunning model 6-2 (in Table A1), model 6-4 (in Table A2) and model 6-6 (in Table A3) with these leads and lags. In each of the tables, our estimates of the impact of an accelerator founding are only statistically significant within a one-year window of the actual founding of the accelerator (except for one result in model A2-6). Taken together, these results suggest that our empirical framework is able to control for

### Table A1: Sensitivity Analysis for Number of Deals (Model 6-2 in Table 6)

| | (1) Number Deals | (2) Number Deals | (3) Number Deals | (4) Number Deals | (5) Number Deals | (6) Number Deals | (7) Number Deals |
|---|---|---|---|---|---|---|---|
| Lag or Lead | Original | -1 Year | -2 Year | -3 Year | +1 Year | +2 Year | +3 Year |
| Accelerator Active | 2.043*** | 1.400** | 0.995 | 0.994 | 1.642*** | 0.835 | 0.829 |
| | (0.267) | (0.232) | (0.173) | (0.139) | (0.247) | (0.130) | (0.105) |
| Patent Count | 1.700** | 1.492 | 1.381 | 1.382 | 1.507 | 1.431 | 1.400 |
| | (0.421) | (0.429) | (0.410) | (0.397) | (0.408) | (0.409) | (0.400) |
| # STEM Grad. Students | 1.110*** | 1.089** | 1.083** | 1.083** | 1.089** | 1.091** | 1.088** |
| | (0.037) | (0.040) | (0.039) | (0.038) | (0.037) | (0.039) | (0.038) |
| Firm Births | 0.985 | 0.952 | 0.962 | 0.962 | 0.984 | 0.958 | 0.963 |
| | (0.088) | (0.077) | (0.080) | (0.079) | (0.086) | (0.079) | (0.081) |
| University R&D Spending | 2.563** | 2.183** | 1.947* | 1.948** | 2.460*** | 1.823* | 2.022** |
| | (1.002) | (0.758) | (0.670) | (0.656) | (0.784) | (0.637) | (0.685) |
| Per Capita Income | 0.895 | 0.924 | 0.928 | 0.928 | 0.901 | 0.939 | 0.932 |
| | (0.085) | (0.090) | (0.093) | (0.093) | (0.085) | (0.099) | (0.095) |
| Employment | 0.225 | 0.365 | 0.378 | 0.378 | 0.242 | 0.384 | 0.383 |
| | (0.218) | (0.381) | (0.450) | (0.443) | (0.233) | (0.445) | (0.425) |
| Observations | 451 | 451 | 451 | 451 | 451 | 451 | 451 |
| log-likelihood | -486.561 | -494.635 | -496.776 | -496.776 | -491.456 | -496.297 | -496.159 |
| MSA Fixed Effects | YES | YES | YES | YES | YES | YES | YES |
| Year Fixed Effects | YES | YES | YES | YES | YES | YES | YES |
| MSA-Specific Linear Trend | YES | YES | YES | YES | YES | YES | YES |

**Table A2:**

| | (1) Distinct Investors | (2) Distinct Investors | (3) Distinct Investors | (4) Distinct Investors | (5) Distinct Investors | (6) Distinct Investors | (7) Distinct Investors |
|---|---|---|---|---|---|---|---|
| Lag or Lead | | -1 Year | -2 Year | -3 Year | +1 Year | +2 Year | +3 Year |
| Accelerator Active | 1.856*** | 1.284 | 0.997 | 0.955 | 1.611*** | 0.741* | 0.868 |
| | (0.403) | (0.255) | (0.204) | (0.170) | (0.274) | (0.118) | (0.183) |
| Patent Count | 1.213 | 1.081 | 1.031 | 1.033 | 1.152 | 1.045 | 1.035 |
| | (0.465) | (0.423) | (0.408) | (0.408) | (0.455) | (0.397) | (0.396) |
| # STEM Grad. Students | 1.086 | 1.076 | 1.073 | 1.073 | 1.074 | 1.083 | 1.076 |
| | (0.067) | (0.067) | (0.067) | (0.066) | (0.066) | (0.067) | (0.066) |
| Firm Births | 1.052 | 1.016 | 1.022 | 1.023 | 1.050 | 1.014 | 1.023 |
| | (0.093) | (0.090) | (0.093) | (0.091) | (0.092) | (0.092) | (0.091) |
| University R&D Spending | 3.468 | 3.153 | 2.941 | 2.962 | 3.490* | 2.724 | 2.984 |
| | (2.642) | (2.383) | (2.175) | (2.191) | (2.623) | (2.005) | (2.225) |
| Per Capita Income | 0.894 | 0.907 | 0.911 | 0.910 | 0.901 | 0.921 | 0.912 |
| | (0.089) | (0.094) | (0.094) | (0.094) | (0.090) | (0.099) | (0.096) |
| Employment | 0.068** | 0.130 | 0.144 | 0.142 | 0.078* | 0.154 | 0.144 |
| | (0.078) | (0.164) | (0.195) | (0.194) | (0.102) | (0.195) | (0.185) |
| Observations | 451 | 451 | 451 | 451 | 451 | 451 | 451 |
| log-likelihood | -662.893 | -670.850 | -672.342 | -672.295 | -666.436 | -670.675 | -671.938 |
| MSA Fixed Effects | YES | YES | YES | YES | YES | YES | YES |
| Year Fixed Effects | YES | YES | YES | YES | YES | YES | YES |
| MSA-Specific Linear Trend | YES | YES | YES | YES | YES | YES | YES |

## Table A3: Sensitivity Analysis for Funds Invested (Model 6-6 in Table 6)

| | (1) Funds Invested | (2) Funds Invested | (3) Funds Invested | (4) Funds Invested | (5) Funds Invested | (6) Funds Invested | (7) Funds Investe |
|---|---|---|---|---|---|---|---|
| Lag or Lead | | -1 Year | -2 Year | -3 Year | +1 Year | +2 Year | +3 Yea |
| Accelerator Active | 0.336$^{**}$ | 0.017 | -0.240 | -0.194 | 0.333$^{**}$ | 0.207 | 0.021 |
| | (0.153) | (0.142) | (0.149) | (0.178) | (0.149) | (0.186) | (0.129 |
| Patent Count | 0.000 | -0.069 | -0.084 | -0.071 | -0.014 | -0.083 | -0.072 |
| | (0.261) | (0.262) | (0.264) | (0.260) | (0.271) | (0.260) | (0.261 |
| # STEM Grad. Students | -0.018 | -0.020 | -0.020 | -0.020 | -0.023 | -0.027 | -0.020 |
| | (0.034) | (0.034) | (0.034) | (0.033) | (0.035) | (0.036) | (0.034 |
| Firm Births | 0.061 | 0.047 | 0.058 | 0.050 | 0.060 | 0.050 | 0.047 |
| | (0.092) | (0.096) | (0.097) | (0.094) | (0.094) | (0.096) | (0.095 |
| University R&D Spending | 0.228 | 0.147 | 0.148 | 0.172 | 0.239 | 0.182 | 0.141 |
| | (0.493) | (0.479) | (0.481) | (0.484) | (0.498) | (0.487) | (0.481 |
| Per Capita Income | -0.095$^{*}$ | -0.092 | -0.097$^{*}$ | -0.093 | -0.094$^{*}$ | -0.095$^{*}$ | -0.092 |
| | (0.057) | (0.058) | (0.056) | (0.057) | (0.057) | (0.057) | (0.058 |
| Employment | -2.730$^{***}$ | -2.408$^{**}$ | -2.314$^{**}$ | -2.375$^{**}$ | -2.603$^{**}$ | -2.394$^{**}$ | -2.395$^{*}$ |
| | (1.024) | (1.015) | (1.061) | (1.009) | (1.058) | (1.063) | (1.039 |
| Observations | 451 | 451 | 451 | 451 | 451 | 451 | 451 |
| log-likelihood | -1443.305 | -1450.968 | -1446.987 | -1448.493 | -1444.235 | -1449.297 | -1450.97 |
| MSA Fixed Effects | YES | YES | YES | YES | YES | YES | YES |
| Year Fixed Effects | YES | YES | YES | YES | YES | YES | YES |
| MSA-Specific Linear Trend | YES | YES | YES | YES | YES | YES | YES |

## Appendix B: Synthetic Control Methodology (Cavallo et al. 2013)

We use the multiple comparative case study methodology outlined in Cavallo et al. (2013). As a firsdt step in understanding the methodological approach, consider the case where we wish to evaluate the effect of treatment on a single MSA (we will later aggregate the MSA-specific effects into an average effect).

Assume we observe $N + 1$ MSAs. Without loss of generality, let the first MSA be that which is exposed to the treatment (accelerator arrival), and retain the remaining $N$ MSAs as potential controls. In the synthetic control approach, it is assumed that the treated unit is uninterruptedly exposed to treatment after an initial intervention. Here, we consider the establishment of the first accelerator in the MSA as the initiation of the intervention period, which includes the years following the initial treatment.

Following Abadie et al (2010), let $Y_{it}^C$ be the number of deals (investors, dollars) that would be observed for country $i$ at time $t$ in the absence of the arrival of an accelerator, for MSAs $i = 1,\ldots,N + 1$, and $t = 1,\ldots,T$. Let $T_0$ be the number of periods before the disaster, with $1 \leq T_0 < T$. Let $Y_{it}^A$ be the outcome that would be observed for MSA $i$ at time $t$ if MSA $i$ is treated with an accelerator beginning in period $T_0 + 1$ to $T$. To the extent that the arrival of an accelerator is unpredictable, it has no effect on the outcome variable prior to its arrival, i.e. for $t \in \{\mathbf{1},\ldots,\boldsymbol{T_0}\}$ and all i $\in \{\mathbf{1},\ldots,\boldsymbol{N}\}$, $Y_{it}^C = Y_{it}^A$.

Let $\alpha_{it} = Y_{it}^A$ - $Y_{it}^C$ be the effect of an accelerator on MSA $i$ at time $t$ when MSA $i$ receives an accelerator, for periods $T_0 + 1$, $T_0 + 2,\ldots T$. This effect may potentially vary over time. Denote $D_{it}$ as an indicator that takes the value of 1 if MSA $i$ receives an accelerator at time $t$, and zero otherwise. Then the number of deals (investors, dollars) for MSA $i$ at time $t$ is:

$$\boldsymbol{Y_{it} = Y_{it}^A + \alpha_{it} D_{it}}$$

Because only one MSA (MSA 1) is exposed to the accelerator and only after period $T_0$, we have that

$$D_{it} = \begin{cases} 1, & if\ i = 1\ and\ t > T_0 \\ 0, & otherwise \end{cases}$$

The parameters of interest are $(\alpha_{1,T0+1},\ldots,\alpha_{1,T})$, the lead-specific causal effect of the arrival of an accelerator on the outcome of interest. For $t < T_0$,

$$\boldsymbol{\alpha_{1t} = Y_{1t}^A - Y_{1t}^C = Y_{1t} - Y_{1t}^C}$$

Note that $Y_{1t}^A$ is observed. To estimate $\alpha_{1t}$, we therefore only need to come up with an estimate for $Y_{1t}^C$.

Now consider an ($N$ x 1) vector of weights $W = (w_2, \ldots, w_{N+1})'$ such that $w_j \geq 0$ for $n = 2, \ldots, N+1$ and $w_2 + w_3 + \ldots + w_{N+1} = 1$. Each value of the vector $W$ represents a potential synthetic control—a particular weighted average of control MSAs.

Let $Zi$ be an ($r$ x 1) vector of observed predictors for the number of deals (investors, dollars) per annum in the MSA (not treated with an accelerator). Suppose that there exists a set of weights $(w^*_2, \ldots, w^*_{N+1})$ that satisfy $\sum_{n=2}^{N+1} w_j^* = 1$ such that

$$\sum_{n=2}^{N+1} w_n^* Y_{n1} = Y_{1,1}$$

$$\vdots$$

$$\sum_{n=2}^{N+1} w_n^* Y_{nT0} = Y_{1,T0}$$

$$\sum_{n=2}^{N+1} w_n^* Z_n = Z_1$$

Abadie et al. (2010) then propose the estimator below as an estimator of $\alpha_{1,t}$:

$$\widehat{\alpha}_{1t} = Y_{1t} - \sum_{n=2}^{N+1} w_n^* Y_{nt}$$

If no set of weights exists such that the equation above hold exactly in the data. When this is the case, the synthetic control observations are selected so that they hold approximately.

Cavallo et al (2013) discuss the computational details for estimating these parameters. We follow their approach. We match each MSA with its synthetic counterpart using the path of deals (investors, dollars) per annum. Therefore, the estimated MSA-specific effect of an accelerator is measured as the difference in the actual and counterfactual evolution of the number of deals (investors, dollars).

Of course, our goal is not to do inference for a single MSA, but to be able to make a statement about the average effect estimated across the MSA-specific comparative case studies of each accelerated MSA. Note that the size of the effect for a given MSA will depend on the level of deals (investors, dollars). The same increase in deals (investors, dollars) would be more important in MSAs with little prior activity.

Thus, we need to normalize the estimates before pooling the MSA-specific results to come up with the average effect of an accelerator. We normalize by setting the outcome variable (deals, dollars, investors) to be equal to one in the treatment year.

Recall that our lead estimates of the effect of an accelerator on the treated MSA of interest are denoted by $(\widehat{\boldsymbol{\alpha}}_{\mathbf{1},\boldsymbol{T_0}+\mathbf{1}},\ldots,\widehat{\boldsymbol{\alpha}}_{\mathbf{1},\boldsymbol{T}})$ for leads 1, 2,…, $T$ - $T_0$. We wish to take the average across $G$ accelerated MSAs of interest. Then the estimated average effect for $G$ accelerators is given by

$$\overline{\boldsymbol{\alpha}} = (\overline{\boldsymbol{\alpha}}_{\boldsymbol{T_0}+\mathbf{1}},\ldots,\overline{\boldsymbol{\alpha}}_{\boldsymbol{T}}) = \frac{\mathbf{1}}{\boldsymbol{G}}\sum_{\boldsymbol{g}=\mathbf{1}}^{\boldsymbol{G}}(\widehat{\boldsymbol{\alpha}}_{\boldsymbol{g},\boldsymbol{T_0}+\mathbf{1}},\ldots,\widehat{\boldsymbol{\alpha}}_{\boldsymbol{g},\boldsymbol{T}}).$$

To evaluate statistical significance of the estimates, we follow Cavallo et al, Abadi and Gardeazabal (2003) and Abadie et al (2010) and use exact inference techniques, similar to permutation tests. These methods allow valid inference regardless of the number of available control MSAs and the number of available pre-accelerator periods.

As in classical permutation tests, we apply the synthetic control method to every potential control in our sample. This allows us to assess whether the effect estimated by the synthetic control for the MSA affected by the accelerator is large relative to the effect estimated for an MSA chosen at random (which was not exposed to an accelerator). This inferential exercise is exact in the sense that regardless of the number of available comparison MSAs and time periods, it is always possible to calculate the exact distribution of the estimated effect of the placebos. More generally, this inferential exercise examines whether the estimated effect of an accelerator's arrival is large relative to the distribution of the effects estimated for the MSAs not exposed to an accelerator. More formally, assume that we are doing inference about point estimates at every lead $l$ (every year after the accelerator arrives). We can then compute a lead-specific significance level ($p$-value) for the estimated accelerator impact as:

$$\boldsymbol{p} - \text{value}_{\boldsymbol{l}} = \boldsymbol{Pr}\left(\widehat{\boldsymbol{\alpha}}_{\mathbf{1},\boldsymbol{l}}^{\boldsymbol{PL}} < \widehat{\boldsymbol{\alpha}}_{\mathbf{1},\boldsymbol{l}}\right) = \frac{\sum_{\boldsymbol{n}=2}^{\boldsymbol{N}+\mathbf{1}}\left(\widehat{\boldsymbol{\alpha}}_{\mathbf{1},\boldsymbol{l}}^{\boldsymbol{PL}(\boldsymbol{n})} < \widehat{\boldsymbol{\alpha}}_{\mathbf{1},\boldsymbol{l}}\right)}{\text{\# of control MSAs}} = \frac{\sum_{\boldsymbol{n}=2}^{\boldsymbol{N}+\mathbf{1}}\left(\widehat{\boldsymbol{\alpha}}_{\mathbf{1},\boldsymbol{l}}^{\boldsymbol{PL}(\boldsymbol{n})} < \widehat{\boldsymbol{\alpha}}_{\mathbf{1},\boldsymbol{l}}\right)}{\boldsymbol{N}}$$

where $\widehat{\boldsymbol{\alpha}}_{\mathbf{1},\boldsymbol{l}}^{\boldsymbol{PL}(\boldsymbol{n})}$ is the lead $l$-specific effect of an accelerator when control MSA $n$ is assigned a placebo (PL) accelerator at the same time as MSA 1. $\widehat{\boldsymbol{\alpha}}_{\mathbf{1},\boldsymbol{l}}^{\boldsymbol{PL}(\boldsymbol{n})}$ is computed following the same computational procedure as for the estimate $\widehat{\boldsymbol{\alpha}}_{\mathbf{1},\boldsymbol{l}}$. By computing $\widehat{\boldsymbol{\alpha}}_{\mathbf{1},\boldsymbol{l}}^{\boldsymbol{PL}(\boldsymbol{n})}$ for every MSA $n$ in the control pool for MSA 1, we can characterize the distribution of placebo effects and assess how the estimate $\widehat{\boldsymbol{\alpha}}_{\mathbf{1},\boldsymbol{l}}$ ranks in that distribution.

To conduct valid inference for $\overline{\boldsymbol{\alpha}}$ we need to account for the fact that the average smooths out some noise. We then construct a distribution of average placebo effects according to the following steps:

1. For each disaster g of interest, we compute all the placebo effects using the available controls $n_g = 2,\ldots, N_g + 1$ corresponding to disaster g.
2. At each lead, we compute every possible placebo average effect by picking a single placebo estimate corresponding to each disaster g and then taking the average across the $G$ placebos. There are many possible placebo averages:

$$\boldsymbol{M_{\overline{PL}}} = \textbf{Number of possible placebo averages} = \prod_{g=1}^{G} \boldsymbol{J_g}$$

   We index all these possible placebo averages by $np = 1,\ldots, \boldsymbol{M_{\overline{PL}}}$. This number grows very quickly in $G$ and the typical $N_g$.
3. We rank the actual lead-specific average accelerator effect $\overline{\boldsymbol{\alpha_l}}$ in the distribution of $\boldsymbol{M_{\overline{PL}}}$ average placebo effects (this involves $\boldsymbol{M_{\overline{PL}}}$ comparisons.)
4. Finally, we compute the lead $l$ specific p-value for the average as:

$$\text{p-value}_{\boldsymbol{l}} = \Pr\left(\frac{\mathbf{1}}{\boldsymbol{G}}\sum_{\boldsymbol{g=1}}^{\boldsymbol{G}} \widehat{\boldsymbol{\alpha}}_{\boldsymbol{gl}}^{\boldsymbol{PL}} < \overline{\boldsymbol{\alpha}}_{\boldsymbol{l}}\right)$$

## Appendix C: Location Decisions of Accelerator Founders

In our work with startup accelerators, we have collected substantial qualitative evidence that suggests that most startup accelerators are founded not in order to take advantage of a region's growing entrepreneurial activity, but rather to catalyze activity in a region in which it is lacking. We lend support to this anecdotal evidence more formally through a series of regressions that measure the statistical differences between startup accelerators in and out of our hazard-rate matched sample.

Column 1 of Table C1 shows that the accelerators in our matched sample have founders whose high school was 1,668 miles closer to the location of their startup accelerator versus the founders of accelerators in regions such as New York, San Francisco or Boston, which were unmatched because they were off the common support.[11] Column (2) uses the full distribution of data (both matched and unmatched accelerators) to look at how the logged total level of early-stage investments in a region at the time of an accelerator's founding is related to the migration distance of accelerator founders. The estimates suggest a strong relationship between "local" accelerator founders and low levels of entrepreneurial financing before accelerator founding. This is consistent with the anecdotal evidence from surveys and interviews that suggests that these local accelerator founders who make up our matched sample are driven by regional growth objectives rather than pecuniary gains; that is, they are focused on making a difference in a particular region (their home town) rather than on choosing the fiscally optimal region to found their accelerator. The magnitude of this relationship is economically significant: a one percent increase in funding in a region is associated with a 107 mile increase in the migration distance of the founder from their high school region. In column (3), we additionally include a time variable that measures time since the first accelerator (Y Combinator) was founded, and we observe a similar relationship. In column (4), we create a new binary variable (LOCAL FOUNDER), which is set to 1 if a founder created an accelerator within a 300 mile distance from their high school. Again, we find a statistically significant relationship, with a ten percent increase in local funding in the year of an accelerator's founding associated with a ten percentage point

[11] To put this in perspective, this is roughly the driving distance from Los Angeles to Houston or from Miami to Boston.

decrease in the likelihood that an accelerator founder is a local. Taken together, the estimates in Table C1 reduce the concern that the founders in our matched sample, who are primarily local, are coming specifically to the regions in which they found accelerators because these regions have a larger funding ecosystem or are experiencing growth in funding activity.

**Table C1: Relationship between Accelerator Founder Background and Location Choice**

| | (1) Migration Distance | (2) Migration Distance | (3) Migration Distance | (4) Local Founder |
|---|---|---|---|---|
| In Hazard-Rate Sample | -1,688.67*** | | | |
| | (529.49) | | | |
| Log Local Investments | | 107.39*** | 110.86*** | -0.01** |
| | | (34.44) | (34.83) | (0.01) |
| Years since 2005 | | | 101.81 | |
| | | | (143.92) | |
| Observations | 178 | 178 | 178 | 178 |

Note: This table measures how the entrepreneurial activity of a region is related to the types of people who found accelerators there. In models 1-3, our dependent variable is migration distance, the distance between an accelerator founder's high school and their accelerator. In model 4, we run a limited dependent variable OLS model where the dependent variable, Local Founder, is set to 1 if the distance between an accelerator founder's high school and accelerator is less than 300 miles. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.