Investor Tax Credits and Entrepreneurship: Evidence from U.S. States

Matthew Denes, Sabrina T. Howell, Filippo Mezzanotti, Xinxin Wang, and Ting Xu*

August 1, 2022

Abstract

Angel investor tax credits are used globally to spur high-growth entrepreneurship. Exploiting their staggered implementation in 31 U.S. states, we find that they increase angel investment yet have no significant impact on entrepreneurial activity. Two mechanisms explain these results: Crowding out of alternative financing and low sensitivity of professional investors to tax credits. With a large-scale survey and a stylized model, we show that low responsiveness among professional angels may reflect the fat-tailed return distributions that characterize high-growth startups. The results contrast with evidence that direct subsidies to firms have positive effects, raising concerns about promoting entrepreneurship with investor subsidies.

JEL Classification: E24, G24, H71, L26

Keywords: entrepreneurship, investor tax credit, angel financing, government subsidy

*This paper subsumes two prior working papers: Denes, Wang, and Xu (2019) and Howell and Mezzanotti (2019). None of the authors have any conflicts of interest to disclose. We thank Jim Albertus, Tania Babina, Laurent Bach, Shai Bernstein, Greg Brown, Annamaria Conti, Jesse Davis, Ryan Decker, Mike Ewens, Joan Farre-Mensa, Paolo Fulghieri, Andra Ghent, Juanita Gonzalez-Uribe, Will Gornall, Apoorv Gupta, Arpit Gupta, Jorge Guzman, Thomas Hellmann, Yael Hochberg, Edith Hotchkiss, Yunzhi Hu, Jessica Jeffers, Simone Lenzu, Josh Lerner, Song Ma, David Matsa, Arnobio Morelix, Holger Mueller, Ramana Nanda, Daniel Paravisini, Andrea Passalacqua, David Robinson, Pian Shu, Morten Sorenson, Denis Sosyura, Chester Spatt, Luke Stein, Kairong Xiao, Emmanuel Yimfor, Linghang Zeng, Eric Zwick, and seminar participants at the 3rd Junior Entrepreneurial Finance and Innovation Workshop, 8th HEC Paris Workshop on Entrepreneurship, AFA, ASU Sonoran Winter Finance Conference, Barcelona GSE Summer Forum, Bocconi, Carnegie Mellon University, Duke-UNC Innovation and Entrepreneurship Research Symposium, Finance in the Cloud II, Jackson Hole Finance Group Conference, Kellogg, Kenan Institute Frontiers of Entrepreneurship Conference, MFA, Mid-Atlantic Research Conference in Finance, Northeastern Finance Conference, NBER Conference on Business Taxation in a Federal System, NTU, NYU Stern Corporate Governance Seminar, Red Rock Finance Conference, Southern California PE Conference, Texas A&M, UCLA, UNC Entrepreneurship Working Group, UVA Darden, WFA, and Young Scholar in Finance Consortium for helpful comments. We thank Abhishek Bhardwaj, Grant Goehring, Michael Gropper, Sunwoo Hwang, Nick McMonigle, Danye Wang, and Jun Wong for excellent research assistance. Howell's research on this project was funded by the Kauffman Foundation and the UNC Kenan Institute. Denes: Tepper School of Business, Carnegie Mellon University, e-mail: denesm@andrew.cmu.edu; Howell: NYU Stern & NBER, e-mail: Sabrina.howell@nyu.edu; Mezzanotti: Northwestern Kellogg, e-mail: filippo.mezzanotti@kellogg.northwestern.edu; Wang, Anderson School of Management, UCLA, e-mail: xinxin.wang@anderson.ucla.edu; Xu, Darden School of Business, University of Virginia, e-mail: xut@darden.virginia.edu.

1 Introduction

Fostering high-growth entrepreneurship is crucial for long-term economic success. As a result, governments around the world deploy tools such as grants, loan guarantees, prize competitions, and tax subsidies. This paper studies a popular policy that has been adopted by more than 14 countries around the world and by the majority of U.S. states: angel investor tax credits.[1] These programs offer personal income tax credits equal to a certain percentage of the investment, regardless of the investment outcome. While this tax policy has attracted much attention and debate, we know little about its effects on investors and startups (Shane (2010), Weaver and Cornwall (2012), Coolican (2015)).

Tax subsidies targeting angel investors have several attractive features. First, there is no need for the government to "pick winners," which requires policymakers to be informed about firm quality and could lead to regulatory capture (Lerner (2009)). Tax credits retain market incentives, leaving investors with skin in the game. Second, the administrative burden of tax subsidies is relatively low. Third, angel investor tax credits are a more precise tool than broad cuts to capital gains taxes (Poterba (1989)). However, while tax credit programs offer attractive flexibility, there is no guarantee that investors will respond by increasing financing in the startups that policymakers target.

To assess the effect of angel tax credits, we exploit their staggered introductions and terminations from 1988 to 2018 across 31 states in the U.S. In our baseline analysis, we use a differences-in-differences framework at the state-year level to identify the effect of tax credits. We show that state-level economic, political, fiscal, and entrepreneurial factors do not predict the implementation of angel tax credits, which suggests that program timing is unrelated to local economic conditions. We evaluate the impact of angel tax credit programs using data on angel activity from Crunchbase, VentureXpert, VentureSource, Form D filings, and AngelList. For a subset of states, we also employ data from state governments on the identity of firms and investors who benefit from these tax credit programs.

We find that angel tax credits increase the number of angel investments by approximately 18% and the number of individual angel investors by 32%. This effect is amplified when programs impose fewer restrictions and when the supply of alternative startup capital is

[1] Angels are wealthy individuals who invest in early-stage startups in exchange for equity or convertible debt. Other countries with angel tax credits include Australia, Brazil, Canada, China, England, France, Germany, Ireland, Italy, Japan, Portugal, Singapore, Spain, and Sweden. In the U.S., angel tax credits represent significant portions of state entrepreneurship budgets, and we calculate that they support up to \$13.2 billion of angel investment. On average, investors use 88% of available funding.

more limited. However, additional investment flows to older firms, firms with lower employment growth, and to fewer serial entrepreneurs. Average ex-ante growth characteristics of angel-backed firms in the state also deteriorate after the implementation of angel tax credits. This may be expected if relaxing financial constraints reduces the quality of firms financed at the margin (Evans and Jovanovic (1989)), and does not imply that the investments are not privately or socially valuable. Nonetheless, the declines raise concerns about the ability of angel tax credits to reach high-growth startups and have a significant impact on the local economy.

We next test whether angel tax credits achieve the programs' objectives—as stated in legislation—of high-tech firm entry and job creation using data from the U.S. Census Business Dynamics Statistics. Across many approaches, we consistently find null effects that are statistically insignificant and have economically small confidence intervals. To address the concern that angel tax credits reallocate capital within a state, we show that there are no effects either in regions with the most angel investments or those with limited early-stage capital. Null effects persist across other outcome variables, including LinkedIn-based firm entry and job creation, Delaware-incorporated firms, and patenting activity.

To assess whether the null results reflect a lack of statistical power, we conduct a power analysis to determine the smallest effect that could be statistically rejected, which is referred to as the minimum detectable effect (MDE). We find that the MDEs are small both in absolute terms and relative to a range of plausible expected effects of angel tax credits (i.e., priors), calculated under assumptions about how the increase in angel investments may translate to new firm creation. For example, the estimated effect on the count of young, high-tech firms in our preferred model is -0.3%, compared to an MDE at 80% power of 1.9% and a corresponding prior of 3.3%. These null effects are informative. Abadie (2020) notes that when a policy is expected to be effective and there is sufficient power, null effects are potentially more informative than significant effects.

We also examine whether the null effects could be due to small program scale. We find no effect at the firm level when we compare firms backed by subsidized investors with firms that were certified but failed to have an investor receive a tax credit. Further, we continue to find null results for states with large programs or when we use a dollarized treatment variable. This indicates that the null effects do not reflect small program scale.

The null real effects on state-level firm entry and job creation contrast with the positive effects documented in the literature for other tax credits (e.g., Cummins et al. (1994), Hall and Van Reenen (2000), Dechezleprêtre et al. (2020), Zwick and Mahon (2017), Arefeva et al. (2020), Edwards and Todtenhaupt (2020), Freedman et al. (2021)). These papers study programs that either directly target the operating firm rather than the financial intermediary, or target investment in firms with relatively predictable cash flows. Conversely, angel tax credit programs target financial intermediaries and projects with fat-tailed return distributions. These differences lead us to two mechanisms that together can help explain why angel tax credits increase investment yet have no real effects.

The first is that additional angel investments partially crowd out investment that would have occurred in the absence of the programs. Several pieces of evidence support this channel. First, increased angel investment appears to displace other types of early-stage financing. We show that, following angel tax credits, non-angel early stage investment decreases while total early-stage investment does not change. Second, investments that would have likely occurred regardless of angel tax credits appear to be relabeled as "angel." Relabeling might be more prevalent among insiders who face negligible coordination frictions when investing in their own firms and may invest for non-financial reasons, particularly because tax credit programs do not restrict how firms use subsidized capital. We find that 35% of beneficiary companies have at least one investor who is also a company executive or a family member of an executive. Comparatively, only 8% of angel-backed firms on AngelList had at least one insider investor. Beyond insiders, investors in general may relabel deals that would have happened regardless of the policy as angel investment to receive angel tax credits. We examine SEC Form D filings, which deals often bypass (Ewens and Malenko (2020)) but help to demonstrate a legal equity round in order to obtain tax credits, and show that these filings are more likely for firms with subsidized investors compared to matched non-beneficiary firms. Last, we find that firms with subsidized investors do not perform better than certified firms that failed to have investors receive a tax credit, consistent with crowding out.

The second mechanism emerges from the type of investors who respond to angel tax credits. We start by showing that investors receiving angel tax credits are primarily younger, more local, and less experienced than the average angel investor. The composition of investors also shifts following the introduction of these programs, with a surge of in-state and inexperienced investors and little entry of professional, arms-length angels. We conduct a survey of angel investors to understand why non-professional investors are much more responsive to angel tax credits and receive 1,411 responses. The survey asks angel investors about the importance of nine factors relevant to evaluating early-stage startups. We find that 51% of respondents rate angel tax credits as not at all important (the lowest of five options), which increases to 71% among the most experienced investors. This contrasts with all other factors, which receive much higher importance. For example, 97% of investors rate the management team as very or extremely important. When prompted to explain why credits are unimportant, 57% report that it is because they invest based on whether the startup has the potential to be a home run. In the words of one respondent, "I'm more focused on the big win than offsetting a loss."

To understand why professional investors are less responsive than non-professional investors to tax credits, we build a stylized model by studying the return distributions of early-stage investments. We assume that more professional investors are more likely to access potentially high-growth startups whose returns tend to have a fatter right tail. We show that while angel tax credits increase the probability of investment, this effect declines as the right tail of the return distribution grows fatter. In particular, professional investors are less sensitive to investor tax credits because the marginal benefit of the subsidy—which is a fixed percentage of the investment—decreases as the expected return increases. This suggests that the return distribution of potentially high-growth firms may limit the effectiveness of angel tax credits. The stylized model and survey shed new light on how early-stage investors make decisions (Bernstein et al. (2017), Ewens and Townsend (2020)).

Taken together, these results suggest that U.S. state angel tax credits fail to reach the investor-startup pairs intended by policymakers and can explain why angel tax credits do not produce significant real effects despite sizable program scale. The crowding out mechanism highlights that the increase in angel investment does not appear to translate into an increase in early-stage capital. The investor heterogeneity mechanism suggests that the non-professional investors enter following the introduction of programs and support relatively low-growth and mature firms, limiting the effect on aggregate firm entry and job creation. The impact of investor subsidies may crucially depend on the type of investors responding to the policy (Lee and Persson (2016)).

This paper contributes to the literature on early-stage financing (Robb and Robinson (2012), Kerr et al. (2014a), Hellmann and Thiele (2015), Hochberg et al. (2018), Lerner et al. (2018), Xu (2019), Davis et al. (2020)). In related work, Gonzalez-Uribe and Paravisini (2019) and Lindsey and Stein (2020) look specifically at policies targeting angel investment. Our findings highlight the importance of investor heterogeneity. Inexperienced investors or insiders use tax credits for reasons besides the intended purpose of additional investment in high-growth startups, which is thought to be a challenge facing entrepreneurship policy (Acs et al. (2016), Lerner (2020)). To our knowledge, we are the first to analyze this issue systematically.

More broadly, we contribute to the literature on investment incentives. There is substantial evidence that related policies have positive effects, including capital gains tax relief, accelerated investment depreciation, R&D tax credits, and corporate tax cuts (Cummins et al. (1994), Hall and Van Reenen (2000), Ivković, Poterba and Weisbenner (2005), Dai et al. (2008), Zwick and Mahon (2017), Curtis and Decker (2018), Arefeva et al. (2020), Dechezleprêtre et al. (2020), Edwards and Todtenhaupt (2020)). R&D grant programs have a positive effect on high-tech startups (Lach (2002), Bronzini and Iachini (2014), Howell (2017), Howell and Brown (2019)). Accelerators and new venture competitions are also useful for startups and benefit from public funds (McKenzie (2017), Cohen et al. (2019), Fehder and Hochberg (2019), Howell (2020)).[2] Especially relevant to our setting is Freedman et al. (2021)'s evaluation of the California Competes Tax Credit (CCTC), which provides businesses with tax credits to incentivize job creation. They find large local multipliers from each subsidized job. In contrast to these studies, we present evidence of crowding out of alternative financing.

The above programs are diverse, yet—in addition to being effective—they have a key feature distinguishing them from angel tax credits: Rather than targeting investors or financial intermediaries, they target firms directly. In contrast, the literature on government-backed venture capital, where the investor rather than the firm is subsidized, is more mixed (Cumming and MacIntosh (2006), Brander et al. (2015), Denes (2019)). Despite being attractive to policymakers, the flexibility of tax incentives for investors may also limit their impact. There may be a trade-off between program flexibility and effective targeting, consistent with evidence from public economics that informational and transaction costs to accessing government programs can deter the individuals who the programs wish to target (Bhargava and Manoli (2015), Deshpande and Li (2019), Chetty and Finkelstein (2020)).

2 Angel Investor Tax Credits

Over the last three decades, 31 states in the U.S. have introduced and passed legislation to provide accredited angel investors with tax credits. Figure 1, Panel A, provides a map of

[2] Yagan (2015) is one of very few papers to document a null effect of a tax policy aiming to promote business investment. He finds that the 2003 dividend tax cut had no impact on firm investment or employee compensation, though it did increase dividend payouts.

states with angel tax credit programs, which we abbreviate as "ATC" hereafter. The blue shading indicates the tax credit percentage, with darker shades representing larger tax credits. The figure highlights that ATCs are prevalent across the U.S. The extent of these programs is particularly notable since they do not occur in the seven states with no income tax (shaded in grey). Panel B shows the introduction and termination of these programs. The earliest was Maine's Seed Capital Tax Credit Program, introduced in 1988. A steady progression of states launched programs in the following three decades. Colorado, Maryland, Minnesota, North Dakota, and Ohio passed more than one version of an ATC. Though the pace of adoption has increased recently, the geography is dispersed, and program duration varies from just one year to three decades.

ATCs are economically meaningful. The mean ratio of program expenditures to total angel investment is 23%.[3] Based on an average tax credit percentage of 34%, these tax credits support up to $13.2 billion in angel investment. Furthermore, while the programs are typically small relative to overall state budgets, they often represent a significant portion of funding allocated to supporting entrepreneurship or small businesses.[4] Finally, investors often use ATCs, with an average 88% of funding allocated by state legislatures distributed as tax credits.

Tax credits are available to accredited investors and their pass-through entities.[5] They require both the firm and the investor to be certified by the state ex-ante as eligible for the credit. The investor may apply only after the deal is complete. This requires substantial coordination between the firm and the investor over, typically, a months-long period. State-level ATCs reduce the state income tax of an investor. For example, suppose that an investor earns $250,000 in a particular year and invests $20,000 in a local startup. If the state tax rate is 5% on all income, then the investor pays annual state taxes of $12,500. Assuming that the state introduced an ATC of 35%, the investor can reduce her state taxes by $7,000, which is a decline of 56% relative to her annual state taxes.[6] Unlike capital gains tax credits that require

[3]The mean ratio of program expenditures to seed venture capital is 105%, and the mean ratio of program expenditures to the Small Business Administration's (SBA) 7(a) loan program is 14.3%.

[4]For example, funding for ATC programs in Ohio, Minnesota, and Wisconsin are 19%, 58%, and 86% of annual state funding for high-tech jobs or small businesses, respectively.

[5]We refer to accredited angel investors as angels throughout the paper. An accredited investor is defined as a person who earned income of more than $200,000 ($300,000 with a spouse) or has a net worth over $1 million. Since July 2010, net worth excludes home equity (Lindsey and Stein (2020)). The tax implications might differ for accredited investors compared to pass-through entities. Angel investor tax credits are more likely provided to individuals because most programs include investment caps.

[6]The tax credit available to a particular investor will depend on her state tax liability. Some programs allow transferable and refundable tax credits, which enable out-of-state investors to benefit from tax credits as well.

positive returns, ATCs are not contingent on the startup's outcome. Therefore, ATCs are a fixed subsidy to investors after making an investment.

Policymakers state that they implement ATCs to increase local economic activity, particularly high-tech firm entry and job creation. For example, Wisconsin notes that "the Qualified New Business Venture (QNBV) Program helps companies create high-paying, high-skill jobs throughout Wisconsin." The Louisiana program goals are: "To encourage third parties to invest in early-stage wealth-creating businesses in the state; to expand the economy of the state by enlarging its base of wealth-creating businesses; and to enlarge the number of quality jobs available." The stated goal of Maine's ATC program is "to spur venture capital investment in Maine startups and ultimately create more jobs in the state."[7] Since most programs cite spurring new investment and job creation as their goals, the analysis in subsequent sections focuses on financing outcomes, firm entry, and employment.

Table 1 provides summary statistics on the ATCs. $Tax\,credit\,\%$ is the maximum share of an investment that can be deducted from an investor's tax liability. The mean (median) tax credit percentage is 34% (33%). Programs often have eligibility criteria for both beneficiary companies and investors. They frequently do not allow investors to request cash in lieu of the credit if they do not have local state income tax liability (72%) or to transfer the credit (72%). Other restrictions include firm age caps (31% of programs), employment caps (39%), revenue caps (47%), assets caps (22%), and minimum investment holding periods (50%). Most programs target the high-tech sector, which guides our empirical design. While many programs do not allow participation by owners and their families (61%), the majority of states permit full-time employees, executives, and officers to receive tax credits. Tax credits reduce income tax liability for the current year, but most programs have a carry-forward provision (89%). Table A.1 provides comprehensive details for all programs.

We examine whether economic, political, fiscal, and entrepreneurial factors explain the introduction of ATCs. Consistent with our identification strategy, we find that these factors do not significantly predict the introduction of ATC programs. The lack of predictability is consistent with the presence of considerable frictions in the passage of these programs. Appendix C provides additional details about the predictive regression and examples of frictions in the implementation of ATCs.

[7] See Wisconsin Economic Development Corporation 2013 Qualified New Business Venture Program Report; Louisiana legislation (http://www.legis.la.gov/Legis/Law.aspx?d=321880); "Startup investors camp out for Maine tax credit" (https://www.pressherald.com/2019/01/02/startup-investors-camp-out-for-maine-tax-credit).

3 Data

This section explains the data we use on angel deals and investors (Section 3.1), state-level outcomes (Section 3.2), and program applicants and beneficiaries (Section 3.3).

3.1 Angel Deals and Investors

Angel investments are difficult to systematically observe in the U.S. because to our knowledge there are no comprehensive datasets about them. Much of what is known about the size of the angel market relies on survey estimates (Shane (2009)). To overcome this challenge, we combine data from Crunchbase, Thomson Reuters VentureXpert, and Dow Jones VentureSource, which we refer to collectively as "CVV," and Form D filings available through the U.S. Securities and Exchange Commission (SEC).[8] Form D is a notice of an exempt offering of securities under Regulation D allowing startups to raise capital from accredited investors without registering their securities (Ewens and Farre-Mensa (2020)).[9] To identify angel rounds, we drop all financial issuers and focus on the first Form D filing that is not a VC round.[10] We then disambiguate and eliminate duplicates.[11]

This process generates 206,885 angel investments from 1988 to 2018. While not all angel investments trigger a Form D filing or appear in the databases described above, our dataset represents one of the most comprehensive sources of angel deals available. Table 2 shows that for the full sample there are on average 133.5 angel investments in a state-year.

To observe the characteristics of firms receiving angel investments, we match these data to the National Establishment Time-Series (NETS) database using firm name, address, and founding year. We only use actual, non-imputed employment and employment growth in the

[8] Crunchbase tracks startup financings using crowdsourcing and news aggregation. VentureXpert and VentureSource are commercial databases for investments in startups and mainly capture firms that eventually received venture capital financing. We identify angel investments from these two databases based on round type and investor type. In Crunchbase, we include round types "pre-seed," "seed," "convertible note," "angel," or "equity crowdfunding," and investor types "angel," "micro," "accelerator," or "incubator." In VentureXpert, we keep rounds when the investment firm or fund type is identified as "individual," "angel," or "angel group." In VentureSource, we incorporate round types identified as "seed," "pre-seed," "crowd," "angel," or "accelerator."

[9] Offerings under Regulation D preempt state securities law. Before March 2008, Form D filings were paper based. We use a FOIA request to obtain non-electronic Form D records from 1992 to 2008.

[10] Specifically, we drop all financial issuers and pooled investment funds. Further, we match all first rounds in Form D with VC rounds in CVV based on firm name, location, and round date within three months of each other. We discard rounds that are identified as VC rounds.

[11] We use the following order of VentureXpert, VentureSource, Crunchbase, and Form D filings. We find similar results using different orderings to disambiguate our data.

year before angel investment (Crane and Decker (2020)).[12] For firms in the CVV sample, we observe entrepreneurs' prior founder experience at the time of investment, which proxies for startup growth potential (Hsu (2007), Lafontaine and Shaw (2016)). Since tax credit programs primarily target high-tech sectors, we use detailed information on industries to focus on angel investments in sectors specifically targeted by the policy.[13] In our baseline analysis, we collapse the data to state-year panels of angel investment volume and average deal characteristics in high-tech sectors. Summary statistics for this sample are under "Financing Outcomes" in Table 2. Our investment analysis shows that the main results are similar in the full sample and the NETS-matched sample, then focuses on the NETS-matched sample to study heterogeneity based on the firm characteristics that it provides.

Finally, we also collect data from AngelList to study the effect of ATCs on investor composition. While AngelList is largely self-reported, it is the most comprehensive data available about the identities and locations of investors for angel investments. The drawback of AngelList is that the coverage increases in more recent years. Summary statistics on this sample are at the bottom of Table 2.

3.2 State-level Real Outcomes

The main goal of ATC programs is to enable new business creation and the jobs supported by these new businesses. To evaluate whether these programs achieve their stated objectives, we use data from the Census Business Dynamics Statistics (BDS). We construct measures of high-tech firm entry and job creation. Specifically, we use the count of new high-tech firms aged zero to five and jobs created at those firms.[14] Since the BDS provides only coarse sector-specific data for these state-level variables, we restrict the main analysis to the sectors most aligned with the policy targets of NAICS 51 (Information) and 54 (Professional, Scientific, and

[12]The NETS-matched sample period is 1993 to 2016. We start the sample in 1993 because Form D data is incomplete in 1992. Additionally, we require up to two years of pre-investment data from NETS to measure ex-ante growth characteristics. Given that NETS covers 1990 to 2014, our sample ends in 2016. We do not use sales from NETS because 90% of the sales data are imputed.

[13]Following the programs' most common eligibility restrictions, we define high-tech as the following NAICS codes corresponding to IT, healthcare, and renewable energy: 221110-221120, 3254, 3340-3349, 3353, 3391, 4234, 5112, 5161, 5171-5174, 5179, 5181, 5182, 5414-5417, and 6200-6239. When these NAICS codes are not available, we map them into comparable industry classifications.

[14]Using ages zero to five permits the programs to affect growth at young firms in addition to new entrants. In an unreported robustness test, we use only age zero firms and find stronger results. We use establishments, which are the unit of measurement in BDS, but we term these "firms" because essentially all firms in our data have one establishment.

Technical Services), but show robustness to including additional sectors as well as restricting to these two sectors when studying angel investment. Table 2 contains summary statistics of our main real outcomes, and Appendix B provides detailed definitions of all variables.

We employ several supplementary datasets in robustness tests. First, we use two alternative measures of startup entry. The first is the number of new potentially high-growth firms, measured as the number of Delaware-incorporated firms registered in the state.[15] This measure was developed by the Startup Cartography project (Fazio et al. (2019), Andrews et al. (2020)), which documents that registering as a Delaware corporation is the single strongest predictor of a growth outcome (successful acquisition or IPO). Second, we gather data at the state-year level on new high-tech startups from 2000 to 2019. The data is provided by Steppingblocks and is based on LinkedIn. Steppingblocks defines a startup as being a firm that appears in LinkedIn for the first time in a given year and begins with no more than 20 employees.[16] We also examine innovation using patent applications from the USPTO and the number of successful startup exits based on CVV data.

3.3 Tax Credit Microdata

We obtain data on startups receiving subsidized investment ("beneficiary companies") for 12 states from public records or privately from state officials. Among these, we also received identities of tax credit recipient investors for seven states. We gather data on these investors from LinkedIn. For ten states, we also observe companies that were certified to receive subsidized investment, but for which no investor was awarded a tax credit. We refer to these firms as "failed applicants." The sample period for these data is 2005 to 2018. The data are complete for a given program-year, though we do not observe all years for all programs. Table A.2, Panel A, shows the number of unique companies by state. In total, there are 1,823 beneficiary companies and 1,404 failed applicants. To obtain outcomes for the beneficiary companies and failed applicants, we match them to two datasets. First, we match 1,227 firms to financing data. Second, we match 1,350 startups to Steppingblocks LinkedIn data. Steppingblocks provided an employment panel based on comprehensive LinkedIn profiles.

[15] We are grateful to Jorge Guzman for providing an updated and expanded version of the data.

[16] To confirm that a company is a startup, Steppingblocks checks that the company had no employees at any time prior to the year 2000 (back to 1990). High-tech is defined as a subset of their industry classification. A list is available upon request.

4 Effects of Angel Investor Tax Credits

This section first explains the estimation approach for evaluating state-level effects of ATCs (Section 4.1), and then discusses the results from this analysis on angel investment (Sections 4.2 and 4.3). Effects on real outcomes are presented in Section 4.4.

4.1 Identification Strategy

Our empirical approach is a differences-in-differences design, exploiting the staggered introduction and expiration of 36 ATC programs in 31 states. Specifically, we estimate the following specification:

$$Y_{st} = \alpha_s + \alpha_t + \beta \cdot \mathbb{1}(ATC_{st}) + \gamma' \cdot X_{s,t-1} + \varepsilon_{st}, \tag{1}$$

where $\mathbb{1}(ATC_{st})$ is an indicator variable equaling one if state s has an ATC program in year t. The dependent variable is angel investments or a real outcome. $X_{s,t-1}$ is a vector of state-year controls.[17] The specification includes state (α_s) and time (α_t) fixed effects. Standard errors are clustered by state (Bertrand et al., 2004). The coefficient of interest is β, which captures the marginal effect of ATCs on angel investments and real outcomes. For robustness, we exploit variation in the size of tax credits across programs by replacing $\mathbb{1}(ATC_{st})$ in equation (1) with a continuous variable, $Tax\,credit\,\%_{st}$, which equals the maximum tax credit percentage available in a state-year with an ATC program, and zero otherwise.

A key identifying assumption for our empirical design is that, in the absence of ATCs, there would be parallel trends in states with these programs relative to those without them. To test for parallel trends and study the immediacy of any effects, we estimate the following dynamic differences-in-differences specification:

$$Y_{st} = \alpha_s + \alpha_t + \delta \cdot \mathbb{1}(ATC_{s,\leq t-4}) + \beta' \cdot \sum_{n=-3}^{3} \mathbb{1}(ATC_{s,t+n}) + \theta \cdot \mathbb{1}(ATC_{s,\geq t+4}) + \gamma' \cdot X_{s,t-1} + \varepsilon_{st}, \tag{2}$$

where $\mathbb{1}(ATC_{s,t+n})$ are indicator variables for each year around the tax credit introduction. The year before the start of an angel tax credit is normalized to zero. We group years that are more than four years before or after the policy change ($\mathbb{1}(ATC_{s,\leq t-4})$ and $\mathbb{1}(ATC_{s,\geq t+4})$).

[17] In particular, we include the following state-year controls, which are lagged by one year: Gross State Product (GSP) growth, log of income per capita, log of population, the maximum state personal income tax rate, and the log of the number of young (0-5 years old) high-tech establishments. We find similar results without these controls (see Section 4.3).

4.2 Effect of Angel Tax Credits on Angel Investments

We begin by studying the effect of ATC programs on the number of angel investments in Table 3, Panel A, using equation (1). We estimate this equation using the unrestricted sample (columns 1-2) and the NETS-matched sample (columns 3-4), which we use in the subsequent angel investment analysis since it allows us to more precisely identify targeted firms and observe firm characteristics.[18] Across both samples, we show that angel tax credit programs ($\mathbb{1}(ATC_{st})$) increase angel investments by 17.8% to 19.0% (columns 1 and 3).[19] We also find that a 10-percentage-point rise in the tax credit percentage ($Tax\,credit\,\%_{st}$) increases the number of angel investments by 3.5% to 5.5% (columns 2 and 4). The dynamic differences-in-differences estimates using equation (2) are reported in Figure 2, Panel A. The positive effect is immediate and there are no pre-trends, consistent with the parallel trends assumption. In sum, these estimates indicate that ATCs lead to an economically significant increase in angel activity.

We confirm this result using AngelList data, which include investor identities. In Table A.3, Panel A, we find that ATCs significantly increase the number of angel investments, the number of angel-backed firms, and the number of unique angel investors by 32%, 27%, and 32%, respectively (columns 1, 3, and 5). The interpretations of these estimates are similar using the tax credit percentage. These results imply that the programs induce entry of new angel investors, rather than more deals among existing investors.

Next, we evaluate heterogeneity in Table A.3, Panel B. First, we examine program flexibility and expect a larger effect for more flexible programs. We define *Flex* to measure the presence and strictness of the 17 restrictions in Table 1.[20] We find that a one-standard-deviation increase in program flexibility leads to an additional 12.9% increase in the quantity of angel investments (column 1). When we use the tax credit percentage as the treatment, we find similar and significant results (column 3). These results support a

[18] The unrestricted sample period is 1988-2018. The NETS-matched sample is restricted to a shorter time period of 1993-2016 due to NETS coverage.

[19] When the outcome is a natural logarithm, we report the exponentiated coefficient minus one in the text. The tables contain the raw coefficients.

[20] For each non-binary restriction, we rank programs from least to most strict and assign the highest rank to programs without this restriction. These rank values are normalized to the unit interval. We also construct indicator variables for programs that do not exclude insider investors and for each of the non-refundable, non-transferable, and no carry forward restrictions. To form the *Flex* index, we sum these 17 variables and then standardize the index by subtracting its mean and dividing by its standard deviation prior to interacting it with our treatment variables.

causal interpretation of our main findings and highlight the importance of program design.[21] Second, we study heterogeneity in local venture capital (VC) availability. We construct $VC\,supply_{st}$ as the total VC amount (excluding angel and seed rounds identified in our main sample) divided by the number of young firms (ages 0 to 5 years) in a state-year. ATCs have a weaker effect on angel investment volume in states with an ample supply of VC (columns 2 and 4). This is consistent with angel financing and VC being substitutes (Ersahin et al. (2021), Hellmann et al. (2021)) and with ATC programs being particularly effective when firms face more limited options in raising early-stage capital. It is also consistent with the idea that ATCs might not facilitate investment in potentially high-growth firms, which are more likely to have access to VC.

We explore this question directly by examining the type of firms receiving additional angel financing, focusing on measures of growth potential. We split angel investments flowing to firms with different ex-ante characteristics around the median. In Table 3, Panel B, we show that ATC programs have an insignificant effect on the amount of capital allocated to high-employment firms, but significantly increase the capital invested in low-employment firms (columns 1-2). The results are similar when we look at employment growth (columns 3-4). An important determinant of startup success is founders' prior entrepreneurship experience (Hsu (2007), Lafontaine and Shaw (2016)). We find that ATCs primarily flow to firms founded by fewer serial entrepreneurs (columns 5-6). Last, we show that ATCs direct marginal investments mainly to older firms with above median age at the time of angel financing, while having no significant impact on investments into nascent firms (columns 7-8). We confirm these results by also showing that the average angel-backed firm has lower growth characteristics and fewer serial entrepreneurs after a state implements ATCs (Table A.3, Panel C).

It is possible that the average decline in ex-ante growth characteristics reflects higher risk tolerance or willingness to experiment among investors (Manso (2011), Kerr et al. (2014b)). The results on age are inconsistent with this because marginal investments did not shift to younger firms. To further assess experimentation, we compare the distributions of angel-backed firms' ex-ante growth characteristics in state-years with an ATC program to state-years without a program, conditional on eventually having a program. Figure A.1 shows that, consistent with our regression estimates, the distribution of angel-backed firms shifts to the left towards lower growth characteristics and exit outcomes. This shift occurs across the distribution without a

[21] We also examined individual program restrictions, such as firm size, and did not find significant heterogeneity in these requirements.

change in the dispersion of the tails. Therefore, higher risk tolerance or experimentation are unlikely to explain our findings.

ATCs might be intended by policymakers to support firms in rural areas with relatively lower ex-ante growth characteristics. To explore whether effects differ by geography, we separate each state's angel investments into those that fund firms in top Metropolitan Statistical Areas (MSAs)—defined as having at least 90% of the state's angel deals—and those that fund firms which are outside of these hub regions.[22] Table A.3, Panel D shows that the effect of ATCs on angel investments in top MSAs is similar to our baseline results (columns 1 and 3) and there is no effect outside of top MSAs (columns 2 and 4). This suggests that ATCs primarily support investment in areas that already have substantial angel activity and do not reallocate angel deals to non-hub locations.

Overall, ATCs lead to more angel investment, with additional financing going to firms with relatively low growth potential. This result has two important implications. First, the decline in high-growth investments supports our empirical design. One potential concern about our identification is that states introduce ATCs in response to a boom in local demand. Since we find that marginal investments flow to lower-potential firms, our results are more consistent with ATC programs shifting the supply of angel financing, rather than reflecting changes in demand. Second, our results suggest that the increase in angel activity does not reflect funding of new startups with high-growth potential, and is concentrated in regions that already have substantial angel activity. This raises questions about whether ATCs meaningfully impact the local entrepreneurial ecosystem, a topic that we examine further in Section 4.4.

4.3 Robustness of Effect on Angel Investments

We conduct several robustness tests of the effect of ATCs on angel investments. First, we test whether the staggered nature of our differences-in-differences context biases the results by employing both the Callaway and Sant'Anna (2021) and Sun and Abraham (2021) estimators.[23] Table A.4 shows that the results are robust to using these estimators, with the magnitudes for the NETS-matched sample (columns 2 and 4) nearly the same as the baseline result. In some specifications, the coefficients become slightly less precise, which is expected given that these

[22] We measure a state's angel investment in the year before ATCs were implemented for treated states and in 2005 for control states. The results are not sensitive to alternatively using two or three years before implementation.

[23] These two papers propose alternative estimation methods to deal with the bias that may arise for two-way fixed effects regression when there are treatment effect heterogeneity and dynamic treatment effects.

estimates are identified using less data.

We impose sample restrictions in Table A.5, Panel A. First, we limit our sample to 2001 to 2016, when our data have better coverage of angel investments. The effect on angel investment volume in this period is similar to the main sample (column 1). Second, we separately estimate our results for the CVV sample (column 2) and the Form D sample (column 3), and again find similar estimates.[24] Third, the finding is robust to dropping angel investments from VentureXpert and VentureSource, which tend to capture angel-backed firms that eventually received institutional capital (column 4). Fourth, we show that the result is similar when we exclude California and Massachusetts, the largest innovation hubs, from the sample (column 5). Last, we estimate our results using the same sectors available in the BDS data for the real effects analysis. Table A.5, Panel B, shows the effects using only NAICS 51 and 54, are again similar in terms of magnitude and significance.

We employ alternative specifications in Table A.6. The estimates are similar without controls (Panel A). The results are also not driven by states switching from zero to positive investments (Panel B, columns 1-2) and are robust to focusing on state-years with positive investments (Panel B, columns 3-4).[25] The results continue to hold when we scale the number of angel investments by the number of young firms in a state-year (Panel C, columns 1-2), and when we transform the number of angel investments using the inverse hyperbolic sine (IHS) function, which unlike the log-transform, is defined for zero (Panel C, columns 3-4). We also show that our results are robust to using dollarized treatment variables that incorporate program size, specifically the log of a state's aggregate annual tax credit cap (Panel D, columns 1-2) and the log of maximum supported investment, which is defined as the annual tax credit cap divided by the tax credit percentage (Panel D, columns 3-4).

Last, Table A.6, Panel E, evaluates the effect of ATCs on angel deal size. We find that ATCs increase the average angel round amount by 23.5% to 25.1%. However, there are two caveats for these estimates. First, many angel deals do not report round amount. Second, the round amount can include both the investment by angels and co-investment by VCs in the same round.

[24]This addresses a concern that the Form D data might capture some investments by other types of investors or that tax credits may induce some investors to file a Form D (see Section 5.1.2 for a discussion of this possibility).

[25]In our sample, only 9.7% of state-years have no angel investments.

4.4 Angel Tax Credits and Real Effects

States introduce ATCs primarily to stimulate the local economy and entrepreneurial ecosystem. This section evaluates whether ATCs achieved these real effects. After estimating the main effects (Section 4.4.1), we interpret the results by deriving a prior for the expected effect and calculating the statistical power of our empirical models (Section 4.4.2). Additionally, we evaluate the role of program scale (Section 4.4.3) and discuss robustness tests (Section 4.4.4).

4.4.1 Effect of Angel Tax Credits on Real Outcomes

Since the stated goal of ATC programs is mainly to spur new firms and jobs (see Section 2), we estimate their effects on firm entry and job creation. We use data from the Census BDS to measure the count of young (0-5 years old) high-tech firms and new jobs created by these firms.[26] We construct these variables either for top MSAs within a state that account for at least 90% of angel investment ("top MSAs") or at the state level. The motivation for the former approach is that within each state there are innovation centers where both angel investment overall and beneficiary firms (i.e., firms supported by investors receiving tax credits) are concentrated. Indeed, the top MSAs contain more than 80% of beneficiary firms and, as shown in Section 4.2, the effect of ATCs on investments is concentrated in these areas. Focusing on these areas can improve precision in detecting real effects.

Table 4 presents the estimates for the effect of ATCs on real outcomes from 1988 to 2018 using equation (1). Panels A and B show the results for firm entry and job creation, respectively. In each case, the outcome is log-transformed.[27] For each outcome, we present results for counts (columns 1-2) and rates (columns 3-4).[28] We use rates because this measure adjusts for differences in the size of the entrepreneurial ecosystem across states, and therefore may improve the precision of our tests. Columns with odd numbers are based on top MSAs and those with even numbers are statewide.

Across all the models in Table 4, we consistently find small estimates that are not significantly different from zero. The confidence intervals are also economically small. For

[26]The BDS data only allow us to measure the entry of establishments rather than firms. However, 99% of high-tech firms zero to five years old are single-establishment firms in our data.

[27]Since the outcomes are never zero, we do not add one before taking the log. The log makes effect sizes more comparable across outcomes, which is particularly useful in the power analysis in Section 4.4.2. For interpretability, we also scale *Tax Credit %* in this section by the average tax credit in state-years that have programs. This average is 35.5%.

[28]Firm entry rates are calculated as establishment entry divided by the average establishments in t and $t-1$ (Decker et al. (2020)). We similarly construct job creation rates.

example, the estimated effect on the count of young, high-tech firms in column 1 of Panel A is -2.0% and the upper bound of the 95% confidence interval is 2.3%. The null effects are not driven by ATCs reversing a pre-existing negative trend in entrepreneurial activity. The dynamic differences-in-differences, reported in Figure 2, Panels B-E, show no pre-trends. The estimates remain statistically and economically insignificant for several years following the introduction of ATCs.

These near-zero estimates and small confidence intervals could indicate null effects of ATCs on real outcomes. Alternatively, they could reflect insufficient statistical power or programs being too small to generate measurable effects. In the following two sections, we consider these possibilities.

4.4.2 Interpretation: Statistical Power

In this section, we assess whether our tests have sufficient power to detect real effects.[29] We conduct a power analysis that provides the smallest effect that could be rejected by our tests with reasonable certainty, which we refer to as the minimum detectable effect (MDE). The MDE is useful in two ways. First, it provides an upper bound on the true effect of angel tax credits, as any effect larger than the MDE should likely be detected by our tests and yield a significant result. Second, readers or policymakers can compare the MDE with their expected effect of ATCs based on their assumptions, which we refer to as the prior. For reference, at the end of this section, we provide calculations of priors for the effect on real outcomes using a range of plausible assumptions.

To calculate the minimum detectable effect (MDE), we follow Black et al. (2019) in using a simulation method that calculates how often our empirical model can detect a statistically significant effect of ATCs on outcome Y when we induce an effect size M in the simulated data. For each effect size M, we generate 1,000 random sets of ATC programs in our data and impose a treatment effect of M on the outcome. The power at M is the fraction of the 1,000 simulations with a positive, statistically significant effect of the policy. Following convention, we define significance as a p-value of less than 0.1 and show robustness to a 0.05 threshold. Finally, we identify the MDE as the effect size that we can reject with 80% power. This power threshold is conservative and in line with conventions in the field experiment literature.[30] A

[29]Statistical power is the probability of rejecting the null hypothesis of no effect when the null is false (i.e., one minus the probability of Type II error).

[30]Abadie (2020) highlights that when the power is above 50%, statistically insignificant effects can be more informative than significant ones. Shapiro et al. (2021) assess the statistical power of their analyses

more detailed explanation of the MDE calculation is in Appendix D.

Figure 3 plots the estimated power at a wide range of effect sizes for each of the real outcomes at the state level, providing a transparent assessment of the power of our results.[31] This analysis confirms that our empirical models can detect relatively small changes in a state's entrepreneurial activity. For example, if a 3% effect on young, high-tech firm entry exists at the state level, we should be able to detect it almost 100% of the time (Figure 3, Panel A). Even for a prior of only 1.9%, we would still detect an effect at the power threshold of 80%. More generally, these figures can be used to independently assess the ability of our tests to detect any level of expected effect of ATCs.

The bottom of Table 4 reports the MDEs at 80% power for our main outcomes. The upper bound of the 95% confidence intervals for the estimates on firm entry at the MSA and state level are 2.3% (column 1) and 1.5% (column 2), respectively, which are beneath the MDEs at 80% power. This pattern generally holds for rates (columns 3-4 of Panel A) and for job creation measured using both counts and rates (Panel B). We also show that our ability to detect an effect is even larger when we consider the joint power across multiple outcomes (see Appendix D).[32]

To facilitate the assessment of the power, we calculate priors for the expected real effects of ATCs given their effect on angel investments, which we compare with the MDEs. While the priors rely on assumptions about how additional angel deals translate to new firms and job creation, they are nonetheless useful as a benchmark. For the effect on new firm count, we construct the prior as the number of new angel-backed firms induced by ATCs as a share of all young, high-tech firms. Since we include only a firm's first deal in our analysis of angel investments, we assume that the estimated effect on angel investments of 18% corresponds to an equal number of new firms, or a one-for-one pass-through of new angel deals. We follow a similar approach to construct the priors for rates and job creation. Table A.10 reports the main priors along with alternatives that relax various assumptions. A comprehensive explanation of the prior construction is in Appendix E.

Comparing the baseline prior effects in the first row of Table A.10 with MDEs at 80% power, we find that, for all specifications, the priors are larger than the MDEs. For example,

using the 50% threshold. The field experiment literature typically uses 80% as a threshold for high-powered analysis (Chow et al. (2007), Sakpal (2010), Mumford (2012), Black et al. (2019), Isakov et al. (2019)).

[31] Appendix Figure A.3 repeats the plots for top MSAs. In the figures, the vertical line denotes 80% power.

[32] The results are similar using a 5% significance level (Table A.7), without controls (Table A.8), or using the continuous treatment $Tax\,credit\,\%_{st}$ (Table A.9).

the prior for the count of young, high-tech firms is 5.9% in top MSAs and 3.3% statewide, while the corresponding MDEs at 80% power are 4% and 1.9%, respectively (columns 1-2 of Table 4, Panel A). As mentioned above, these priors are calculated based on particular assumptions. We relax these assumptions in the other rows of Table A.10 and find qualitatively similar results, with most having power above or close to 80% at the prior.[33] In sum, given the estimated increase in angel investments, our tests have sufficient power to detect the real effects of ATCs.

4.4.3 Interpretation: Program Sizing

This section examines whether null real effects reflect small programs. We start by studying program heterogeneity by size in case larger programs have a significant real effect. Table A.11 restricts the sample of treated states to those with an above-median annual budget.[34] Table A.12 exploits variation in the program budgets by using the annual tax credit cap in a state-year or the maximum aggregate investment supported by the credit (i.e., annual tax credit cap divided by tax credit percentage) as alternative treatment variables. In both tables, we continue to find statistically and economically insignificant real effects.

We next evaluate the effect of ATCs on startups by comparing firms financed by subsidized investors ("beneficiary companies") to firms that were certified but failed to have an investor receive a tax credit ("failed applicants"). This approach allows us to detect an effect at the firm level, irrespective of the aggregate size of these programs. Failed applicants represent a useful comparison group because they are in the same state and were interested in the tax credit. However, failed applicants are likely to be of relatively lower quality because they either failed to raise angel financing or applied after the state ran out of funding for the tax credits. If there is bias in comparing these groups, it should be in the direction of beneficiary companies performing better. Table A.2, Panel B, provides summary statistics on beneficiary companies and failed applicants.

We estimate the following equation:

$$Y_{i,t+k} = \alpha_{jt} + \alpha_{st} + \beta \cdot \mathbb{1}(Tax\,Credit_{it}) + \theta Y_{i,t} + \varepsilon_{i,t+k}, \tag{3}$$

where the dependent variable $Y_{i,t+k}$ is the outcome for startup i in year $t + k$. Year t is the year that the startup either first had an investor receive a tax credit or applied for an investor

[33] In Appendix E, we discuss several implicit assumptions that could lead us to underestimate these priors.

[34] These large programs have an average annual budget of \$13.7 million and can support up to \$40.3 million of angel financing per year based on the average tax credit percentage.

to receive a tax credit for the first time. $\mathbb{1}(Tax\,Credit_{it})$ is an indicator variable for startup i having an investor receive a tax credit in year t. The specification includes sector-year (α_{jt}) and state-year (α_{st}) fixed effects. Standard errors are clustered by state-year.[35]

Table 5 reports estimates of equation (3). We find that receiving subsidized angel investment does not impact raising venture capital within two years of t (column 1) or the probability of a successful exit based on an IPO or acquisition (column 2). We also examine measures of firm-level employment using LinkedIn data from Steppingblocks. We construct indicators for the firm having at least 25 employees (columns 3-4) and employment greater than the 75th percentile within the sample (columns 5-6) measured in the second and third years after the tax credit. We find no differences in future employment between beneficiary firms and failed applicants. Table A.13 shows that this is robust to using a matching estimator comparing beneficiary companies to similar control firms in nearby states without tax credit programs. In unreported results, we also find similar results using NETS rather than LinkedIn. Overall, tax credits did not affect recipient firms, which is consistent with the aggregate results and suggests that program size does not explain the null real effects.

4.4.4 Robustness of Effect on Real Outcomes

We conduct a wide range of robustness tests. First, we employ the Callaway and Sant'Anna (2021) and Sun and Abraham (2021) estimators in Table A.14. As for angel investments, the results are robust, with no evidence of positive effects. For one outcome at one level of aggregation (top MSAs), one coefficient is significant and negative. This would be expected by chance under a null effect given the number of models.

Second, we use the continuous treatment variable, $Tax\,credit\,\%_{st}$, in Table A.9 and again find no effect of ATCs on firm entry and job creation. In Table A.15, Panel A, we show that the results are similar using levels rather than logs. Next, we assess whether ATCs produce real effects in areas that do not typically foster entrepreneurial activity. We focus on those regions outside of top MSAs ("non-top MSAs") and examine the impact of ATCs on firm entry and job creation. Table A.16 shows that we continue to find no statistically and economically significant effects in these regions. This suggests that ATCs do not increase real activity outside of the top MSAs.

We consider several alternative measures of real outcomes in Table A.17. We construct

[35] We cluster by state-year because there are limited clusters by state. The results are similar with other approaches, including robust standard errors.

similar variables at the state-year level for firm entry and job creation using LinkedIn data (see Section 3.2 for details). In Panel A, we find economically small and statistically insignificant effects of ATCs on new startups (columns 1-2), new high-tech startups (columns 3-4), employment at new startups (columns 5-6), and employment at new high-tech startups (columns 7-8). In Panel B, we use data from the Startup Cartography Project on the number of high-quality startups (columns 1-2) and the number of new Delaware-incorporated firms (column 3-4), which proxies for high-quality firms. We also examine successful exits in the form of IPOs and large acquisitions (columns 5-6) and the number of patent applications (columns 7-8). We find that there is no effect of ATCs on these alternative outcomes and obtain similar results using levels rather than logs in Table A.15, Panel B.

The null effects on the real outcomes persist with alternative sectors to define "high-tech." In Table A.18, we use all two-digit sectors that have any 4-digit subsector included in the angel analysis.[36] The coefficients are qualitatively similar to those estimated in our main analysis, with just one model significant at the 10% level (Panel B, column 6). This is in fact less than what we would expect by chance.

In sum, we do not find evidence that ATCs significantly impact state-level entrepreneurial activity based on a variety of real outcomes relevant to policymakers' goals of stimulating high-growth, high-tech new firms.[37] It is important to note that this does not rule out the possibility of any effect; there may be positive effects along dimensions that we cannot measure. However, null effects are especially informative when the prior is that a policy will be effective and they become more informative than a significant effect when there is sufficient power (Abadie (2020)). A positive prior is reasonable since the literature has found that other tax credits have positive effects (Cummins et al. (1994), Hall and Van Reenen (2000), Zwick and Mahon (2017), Arefeva et al. (2020), Dechezleprêtre et al. (2020), Edwards and Todtenhaupt (2020), Freedman et al. (2021)). These papers either study programs directly targeting the operating firm rather than the financial intermediary, or programs targeting investment in firms with relatively predictable cash flows. Below, we present mechanisms for our results that follow from two distinctive features of ATC programs, namely that they target financial intermediaries and projects with fat-tailed return distributions.

[36]Panel A uses sectors 31-33, 51, and 54. Panel B uses 22, 31-33, 42, 51, 54, and 62. The sample in Panel B includes all of utilities (22), wholesale (42), and healthcare (62), and thus is not especially relevant to the angel policies, but we include these models for completeness.

[37]As DellaVigna and Linos (2020) discuss, reporting null results reduces publication bias in policy evaluation towards effective policies.

5 Mechanisms

Thus far, we have shown that despite increasing angel investments, ATCs have no measurable real effects, a finding that does not reflect program size or limited statistical power. In this section, we present evidence for two mechanisms. First, the increase in angel investment in part reflects crowding out, where additional funding displaces funding from other sources that would have occurred in the absence of the ATCs. We document a decline in non-angel early-stage investment after ATCs (Section 5.1.1) and of relabeling investment as "angel" in order to access the ATCs (Section 5.1.2). Second, to the degree ATCs do increase investment, they have little impact on the professional, sophisticated angels who typically fund high-growth startups that could generate large benefits for the local economy. Instead, the increase in angel investment is mostly driven by local, inexperienced investors without entrepreneurial backgrounds (Section 5.2.1). Based on a survey of angel investors and a theoretical model, we argue that the nature of returns for early-stage firms combined with the tax credit being a fixed percentage of investment can explain the limited response from professional investors (Sections 5.2.2 and 5.2.3).

Together, these two channels can explain our main results. The crowding out channel suggests that the observed increase in angel investment does not translate entirely to increased access to financing for firms. The investor heterogeneity channel explains why subsidized firms are relatively low-growth and mature, and are therefore unlikely to significantly drive aggregate firm entry and job creation.

5.1 Crowding Out

5.1.1 Angel Tax Credit and Alternative Finance

Our firm-level analysis (see Section 4.4.3) points us in the direction of crowding out. Above, we showed that beneficiary firms (firms with investors who receive a tax credit) do not perform better than firms that applied but ultimately did not have an investor receive the tax credit. This is consistent with crowding out because it implies that—conditional on applying—receiving subsidized investment does not alleviate constraints; failed firms raise subsequent VC and succeed at the same rates as beneficiary firms. This logic follows the practice of identifying crowding out as occurring when government funds displace private capital, observable when a subsidy program has no effect on its targeted outcome (Knight

(2002), Andreoni and Payne (2003), Howell (2017), Moretti et al. (2019)).

One way that crowding out could occur is if ATCs increase angel investment by displacing other sources of early-stage investment. The tax credits might crowd out sources such as early-stage VC and accelerator funding, for either supply- or demand-side reasons. On the supply side, some investors may participate in both angel (including angel groups) and early-VC rounds, leading to a substitution between the two if these investors are constrained. There may also be competition between different early-stage investors in both financing and product markets, such that an increase in angel investment reduces the returns to other early-stage investors. This would be consistent with the theories of Inderst and Mueller (2009) and Khanna and Mathews (2022), as well as the empirical evidence of substitution between angel and VC investment in Ersahin et al. (2021) and Hellmann et al. (2021). On the demand side, a limited supply of projects or a limited size of each project could lead to inelastic financing demand. Also, entrepreneurs may not want to raise more money than they need to limit dilutution of their equity due to early-stage investment (Bergemann and Hege (1998)).

To test for various forms of crowding out in the startup financing market, such as between different types of investors, between angel-backed and non-angel backed firms, and between subsidized and unsubsidized angel-backed firms, we examine all early-stage financing for young firms. We estimate equation (1) with measures of early-stage financing as the outcome variables.[38] We use dollar measures because we expect crowding out to manifest via dollar rather than deal substitution since the deal types have dramatically different sizes. That is, a dollar of angel would crowd out a dollar of VC. Table 6, Panel A, reports the results. First, we find an insignificant, slightly negative effect on total early-stage investment at the state-year level (column 1).[39] Meanwhile, there is a negative effect on non-angel investment (column 2) and an offsetting positive effect on angel investment (column 3). Non-angel investors are commonly early-stage VCs. As a result, the share of angel investment increases by 7.5 percentage points (column 4) from a mean of 42%. This suggests that ATCs did not affect aggregate early-stage financing while angel's share of the total increased, consistent with crowding out.

[38]We include all early-stage rounds in CVV and Form D data. Specifically, we define early-stage rounds as the first two rounds in VentureXpert, round types "1st," "seed," "angel," "crowdfunding," and "accelerator" in VentureSource, founding types "pre-seed," "seed," "grant," "angel," "convertible debt," "equity crowdfunding," "product crowdfunding," and "series A" in Crunchbase, and the first two rounds of financing in Form D data.

[39]The pre-ATC share of angel investments among early-stage investments is substantial, at 41% on average and 34% at the median.

In Panel B, we examine the effect of ATCs on total early-stage financing received at the firm level. The sample includes all firms receiving early-stage financing, which form the basis for the state-year panel in Panel A. All columns include state, year, and age fixed effects. The even columns are augmented with controls. Additionally, the specifications in columns 3-4 are weighted by the inverse of the number of firms in a state to mitigate the influence of hub states. Across all specifications, we find no effect of ATCs on early-stage financing for a firm. The effects are statistically and economically small. Overall, these results suggest that subsidized angel financing may crowd out alternative early-stage financing, limiting the degree to which the policy increases firms' overall access to finance.

5.1.2 Insider Investors and Relabeling

In addition to crowding out across investment stages, crowding out could also occur within investors via relabeling, where investments that would have occurred regardless of ATCs are identified as "angel investments" to obtain the subsidy. While proving relabeling is extremely challenging, in this section, we narrow our focus to those investors who receive tax credits and provide evidence consistent with relabeling being important in the data.

We first examine corporate insiders, a special class of investors who are in a particularly advantageous position to benefit from ATCs. Insiders face relatively low information or coordination frictions when investing in their own companies and claiming tax credits. Insiders may invest for tax arbitrage reasons (Slemrod and Yitzhaki (2002), Korinek and Stiglitz (2009)), potentially even making "investments" that are subsequently paid out as dividends. They may also relabel pre-existing corporate transactions as "angel investments." Angel investment among insiders induced by the ATCs is more likely to represent crowding out, in the sense that any new capital from insiders would likely have been deployed regardless within the beneficiary firm. Lee and Persson (2016) also argue that insider investment in the form of friends and family financing is not a perfect substitute for external formal sources of capital, and is less likely than other sources to lead to firm growth.

We assess the prevalence of insider investors among tax credit recipients. Our data include 628 unique firms and 3,560 investors from five states.[40] We identify an investor as an insider

[40] These states are Ohio, New Jersey, Maryland, New Mexico, and Kentucky. They are reasonably representative of states that employ ATCs, including some high-tech clusters (New Jersey and Maryland), rural areas (Kentucky and New Mexico), and the Rust Belt (Ohio). Some states explicitly permit the investor to be employed at the company (Table A.1). Ohio, New Jersey, Kentucky and Maryland do not exclude executives, but do exclude owners with above a certain threshold of pre-investment ownership stake, ranging

if the person is an executive on a Form D filing, listed as an employee on LinkedIn, or shares a last name with an executive. Further details are in Appendix F. In Table 7, we find that 35% of firms have at least one investor who is an executive or family member of an executive. The share is 24% or higher in all states except Kentucky. As a benchmark, only 8% of startups in AngelList have at least one investor who is also employed at the company in which they are investing. At the investor level, 14% of subsidized investors are executives of the invested company or their family members. The corresponding benchmark in AngelList is only 2%.

Beyond insiders, investors more broadly may relabel transactions that would have happened regardless of the program as "angel investments" in order to receive the tax credits. Such relabeling could increase the rate of Form D filings because this document can serve as evidence that a legal equity round occurred, which is needed to access the tax credit.[41] Relabeled investments would appear in our sample as an angel investment when they might not have otherwise. To explore this, we compare the Form D filing rate across beneficiary firms and matched non-beneficiary firms that also received angel financing. We focus on Form Ds filed within three years of the tax credit because some states have a minimum holding period. We match each beneficiary firm with up to five similar control firms from nearby states without ATCs through a nearest neighbor matching procedure.[42] Table 7, Panel B, reports the results. Rows 2 to 5 show that beneficiary firms and control firms have similar ex-ante characteristics, indicating a proper matching procedure. However, the likelihood of a beneficiary firm filing a Form D is 64.4%, while the chance of filing for control firms is only 32%. This difference is both statistically and economically significant. Consistent with relabeling, beneficiary firms are significantly more likely to file a Form D than control firms, whose investors are not required to submit proof of a legal equity round.

Finally, we expect that insider and non-professional investors are more responsive to increased incentives to file a Form D because they are more likely to engage in informal

from 5% for Ohio to 80% for New Jersey. New Mexico excludes executives but has no limits for owners, families, or employees.

[41] While a Form D is often theoretically required to exempt an equity investment from SEC registration, many startups do not file, often to avoid the accompanying disclosure. Ewens and Malenko (2020) show that for more than 20% of VC-backed startups, no Form D is ever filed. Details are in Disappearing Form D. While there could be penalties for failing to file a Form D, they appear to be rarely enforced. Additionally, U.S. courts and the SEC have ruled that failing to file a Form D does not cause a startup to lose its security exemption status (SEC Rules). The effect of ATCs on angel investments is similar when we restrict to using only deals from CVV (Table A.5, Panel A).

[42] We restrict the control firms to be located in a different state but the same Census division, belong to the same industry, have a similar age, and have a similar amount of previous financing relative to the year of the treatment firm's first tax credit using nearest neighbor matching.

transactions and may not have other financing documentation such as stock purchase and equity rights agreements. Consistent with this, we find that the gap in Form D filing rates between beneficiary and control firms is much higher when the deal contains insider investors. In Table A.19, we split the sample by whether a firm has an insider investor. We find that treated firms are 53 percentage points more likely to file Form Ds than control firms when insider investors are present, while this difference is only 30 percentage points when no investors are insiders. This is consistent with the marginal benefit of filing being much higher for insiders than for professional investors when they need to qualify for tax credits.

In sum, additional angel investment following ATCs appears to in part reflect relabeling, where informal transactions that would have happened regardless are formalized as "angel" deals via Form D filings. This form of crowding out can help reconcile the increase in angel investment with the null real effects. However, it likely does not explain the *entire* increase in angel investment. For example, it does not explain the increase in investment amount per deal as shown in Table A.6, Panel E, because it concerns the extensive margin decision of whether to report an investment or not. Additionally, Table A.20 shows that the angel results are similar in states that exclude insiders from receiving tax credits. Nevertheless, together with the other sources of crowding out, this direct form helps explain why we would see large increases in reported angel investment with no commensurate effects on economic activity.

5.2 How Investors Make Decisions

This section explores who responds to angel tax credits and then seeks to explain why. The success of ATCs might depend on *which* investors take up the subsidy. A commonly cited goal of ATCs is to attract professional angel investors who would otherwise not invest in local firms. If instead the response is concentrated among non-professional investors, the effectiveness of these programs may be limited.

5.2.1 Investor Heterogeneity in Tax Credit Use and Responsiveness

We first examine heterogeneity among ATC recipients and then assess how ATCs affect investor composition. For seven states, we obtain data on the identities of subsidized investors and connect them with LinkedIn information on investor characteristics. Table 8 reports the statistics for the 5,637 individuals who received tax credits, which excludes a small number of fund recipients. We find that 87% of the subsidized investors are male and

95% are white, consistent with the findings in Ewens and Townsend (2020) that angel investors are overwhelmingly white males.[43] The average age is 42 years, which is younger than the average age of 58 among angel investors in Huang et al. (2017). Subsidized investors also appear to be relatively non-professional. Just 0.7% identify on LinkedIn as professional investors and only 6.2% have prior entrepreneurial experience. In contrast, Huang et al. (2017) find that 55% of angels have entrepreneurial experience, and that these investors tend to finance more companies, take a more active role in their portfolio companies, and earn higher returns. The majority of tax credit recipients in our data are corporate executives (82%). The next largest groups are doctors (7.3%) and lawyers (4.1%).

Most subsidized investors are located in the same state as the tax credit program (79%). This is partly by design as many programs restrict investors to be in-state, which may limit the ability of the programs to attract sophisticated investors. In-state investors are less likely to come from entrepreneurial hubs, because the major hubs of California and Massachusetts do not have tax credit programs. Overall, we find that the average angel investor who receives tax credits is younger, more local, and less entrepreneurial than the typical angel investor.

To quantify the relative importance of different types of investors in explaining the increase in angel investment, we use AngelList data to examine the effect of ATCs on the composition of investors. In particular, we consider the following four characteristics of non-professional investors: in-state, less than five years of investing experience, no prior successful exit, and no prior founder experience. These measures are consistent with Huang et al. (2017), who find that professional angels tend to have prior entrepreneurial experience and are active in making investments. We verify in Table A.21 that these measures of non-professional investors are negatively correlated with better startup exit outcomes.

Table 9, Panel A, reports the estimates of equation (1) using investment-level data.[44] The dependent variables are indicators for the investor in a deal having a particular characteristic. Observations are weighted by the inverse of the number of deals in a state, which gives each state an equal weight and accounts for the overrepresentation of hub states. In column 1, we find that ATCs increase the likelihood of being an in-state investor by 7.5 percentage points.

[43] We coded the ethnicity or race using pictures. We also coded individuals as Hispanic who our web researchers identified as "white" but who had names among the top 20 Hispanic names in the U.S. (See Name List).

[44] The sample starts from 2003, a period when AngelList data has reasonable coverage. We find similar results when we restrict the sample to start in 2010 in order to mitigate a potential concern about backfilled data. We use investment-level, rather than investor-level, data because investor characteristics are defined relative to the location and timing of a particular deal.

This is a 15% increase relative to the sample mean in Table 2. The probabilities of a deal having investors with limited experienced, no successful exit, and no founder experience also increase by 4.1, 7.3, and 6.9 percentage points, respectively (columns 2-4). In Panel B, we examine whether the shift to non-professional investors reflects variation in investor entry, rather than reallocation across deals. Here, the dependent variables are the log number of investors making investments in a given state-year who are in a particular category. ATCs increase in-state angel investors by 33% and, to a lesser extent, out-of-state investors by 21% (columns 1-2). They increase inexperienced investors by 32%, but have a small and insignificant effect on experienced investors (columns 3-4). We observe a similar pattern for exit and founder experience (columns 5-8).

Overall, local, inexperienced angel investors drive the increase in angel investments described in Section 4.2, while professional, arms-length angels are relatively unresponsive to the tax incentive. ATCs do not simply affect the investment decisions among existing investors, but affect who is investing, leading to a larger share of non-professional investors. This shift helps to explain why marginal investments flow to lower-growth firms. If non-professional investors have less access to high-quality deals or lower screening ability, they may invest in projects that have a limited impact on firm and local economic growth, helping to explain the null real effects. Non-professional investors may also be more likely to invest for non-pecuniary reasons (Huang et al. (2017)) or may have close connections with the firm, making them better positioned to utilize ATCs to minimize their tax obligations.

5.2.2 Survey of Angel Investors

To understand how different investors make decisions, we conduct a large-scale survey of investors. The objective of the survey is threefold. First, it validates whether and how ATCs affect investment decisions in practice. Second, it explores how these effects differ across professional and non-professional investors. Lastly, it sheds light on why professional investors do not respond to ATCs. We contribute to the literature using surveys to study management practices (Bloom and Van Reenen (2010)), institutional investors (McCahery et al. (2016)), venture capitalists (Gompers et al. (2020)), and private equity investors (Gompers et al. (2016), Bernstein et al. (2019)). To the best of our knowledge, this survey is the first to elicit novel information about investment approaches among a wide swathe of angel investors.

We develop the sample of investors to survey from two sources described in Section 3: state-provided lists of ATC recipients and all investors on AngelList as of early 2020 who had made at least one investment. We sent each investor an email containing a personalized survey link. This email and the complete survey are in Appendix G. In total, we emailed just over 12,000 individuals and obtained 1,411 responses, out of which 1,384 are complete, representing a response rate of 11.6%, which is in line with other recent investor surveys.[45] Among respondents, about 11% are from the state ATC recipient data and the remainder are from AngelList. Details on respondents and selection are in Table A.22.[46]

The survey yields four central insights. First, investors report that they do not consider ATCs to be important when evaluating investments. Figure 4, Panel A, provides responses about the importance of nine factors (randomly sorted for each investor). ATCs are not at all important for 51% of respondents, and are very or extremely important for only 7%. This contrasts starkly with the other eight factors. For example, 97% rate the management team as very or extremely important, and 0% rate the team as not at all important, consistent with Bernstein et al. (2017). Only 2% rate valuation and gut reaction as not at all important, while over 50% rate these factors as very or extremely important.

Second, professional investors find ATCs less useful than other investors and tax credit recipients, who are relatively less professional (see Section 5.2.1). The top figure of Figure 4, Panel B, validates the survey by showing that 76% of tax credit recipients view ATCs as at least slightly important, compared to 49% of all respondents. Among respondents who identify as professional investors, 64% rate ATCs as not at all important. For investors in the top decile by number of deals, 71% rate credits as not at all important. We also estimate the relationship between the importance of ATCs for an investor and the probability that she is a professional investor. Table 10, Panel A, finds that there is a significant negative association between how important investors rate ATCs and a variety of proxies for investor sophistication and experience (columns 1-3). For example, being a professional investor reduces ATC importance

[45]We obtained approval from the NYU IRB for this survey. Twenty-seven responses are either incomplete or cannot be matched back to our investor data due to response from a different email address. Our response rate is in line with the previous literature conducting other large-scale surveys. Gompers et al. (2020) survey VC investors and obtain a response rate of 8.3%, Bernstein et al. (2019) obtain a response rate of 10.3% from PE investors, Graham and Harvey (2001) obtain a response rate of 8.9% from CFOs, and Da Rin and Phalippou (2017) obtain a response rate of 14.4% from private equity LPs. Our absolute number of responses is also high relative to other surveys of private equity investors. For example, Gompers et al. (2016) survey 79 buyout investors and Gompers et al. (2020) survey 885 VC investors.

[46]In Appendix Table A.22, Panel C, we find no evidence of selection on key variables related to ATCs, including residing in a state with an ATC or living in the hub states of California and Massachusetts. However, investors with more deals are more likely to respond and investors who are company insiders are less likely to respond. In addition, ATC recipients are less likely to respond. While these relationships are not large in magnitude, they point towards respondents being somewhat more experienced investors.

by 0.38, which is a 21% decrease relative to the sample mean. This offers further evidence that professional, arms-length angels are relatively unresponsive to the tax incentive.

Third, we explore why angels do not view ATCs as important. We ask investors who rate ATCs as unimportant to select one of five options to explain their answer. The majority (57%) report that ATCs are unimportant because they invest based on whether the startup has the potential to be a home run or not (Figure 4, Panel C). We refer to this as the “Home Run” approach, which characterizes investing in potentially high-growth, early-stage companies. Responses to the open-ended question are consistent with this view. For example, respondents wrote that “If the deal is bad a tax credit will not make it good” and “If I believe in the business model/technology then a tax credit is largely irrelevant. Conversely, if I don’t believe in the model then the tax credit is also irrelevant.” This approach does not imply that investors leave money on the table, but rather that ATCs do not change their selection of startups ex-ante. We formalize why professional investors may follow this investing approach in Section 5.2.3.

In Table 10, Panel A, we also see that a focus on financial metrics – the opposite of the “Home Run” approach – predicts ATC importance (column 4). In Panel B, we correlate reasons for ATC unimportance with the investor’s deal volume. More professional investors with above-median deal volume are more likely to cite the “Home Run” approach and coordination frictions as reasons for ATCs being unimportant.

Fourth, the survey highlights frictions that could help to explain our results, beyond investment styles. Specifically, administrative costs, coordination frictions with startups, and lack of information about the ATCs appear to play a role in reducing the use of ATCs among arms-length, professional investors. Of the investors rating ATCs as unimportant, 11% report that the reason is coordination costs (Figure 4, Panel C). Coordination costs are likely to be higher for professional arm-length investors as they typically do not have close ties with the startups before investing, face a fast-paced deal cycle, or have higher opportunity costs of their time. Consistent with this, we find that professional investors are more likely to report coordination frictions (Table 10, Panel B, column 2).[47]

In sum, tax credits are not important for our sample of investors, especially for professional investors, and this unimportance appears to reflect a “Home Run” investing strategy. This does not imply that investors leave money on the table. For instance, investors using a “Home

[47]We also ask whether an investor used ATCs and, if not, why. Figure 4, Panel D, shows that 15% do not use ATCs because of coordination costs, and 60% are unaware the programs exist; indeed, even among investors whose states have a program, 19% report that ATCs are not available and 60% do not know about their availability, indicating information barriers.

Run" approach may take up the tax credit ex-post if the coordination or administrative costs are not too high, even if the credit does not change their selection of startups ex-ante.

5.2.3 Stylized Model

Professional investors appear to be less responsive to tax credits than non-professional investors based on the investor heterogeneity and survey results. Further, survey respondents suggest that a "Home Run" investing approach might explain why professional investors do not respond to the tax credits. We use a simple model to explore why this might occur. The model seeks to understand the role of return distributions, though it does not fully characterize how ATCs affect investment decisions. The full model and proofs are in Appendix H. A brief summary is presented below.

We study an investor who decides to invest in a startup if and only if the expected return is higher than a hurdle rate, which captures the opportunity cost of other projects and any coordination or effort cost. We follow Othman (2019) and Malenko et al. (2020) by assuming that startup investment returns follow a Pareto distribution, with shape parameter α_j. Our choice of the Pareto distribution is motivated by the well-documented fact that startup returns exhibit a heavy-right tail and extreme skewness (Scherer and Harhoff (2000), Kerr et al. (2014b), Ewens et al. (2018)).[48] We assume that α_j is an investor-specific parameter governing the pool of projects that the investor can access.[49]

Sophisticated, professional investors have access to projects with higher expected returns and higher uncertainty, which means a lower α_j. A low α_j captures the "Home Run" investing approach. These opportunities might be available to professional investors focusing on early-stage, high-growth, and high-risk startups with very fat-tailed return distributions. A high α_j characterizes firms with more traditional business models that have lower risk profiles, which tend to be accessed by non-professional investors. The model also allows firms to differ in terms of observable quality.

In this setting, we study how an investor tax credit affects the ex-ante probability of

[48] Hall and Woodward (2010) and Kerr et al. (2014b) document that most startups fail completely while a few generate enormous returns. Malenko et al. (2020) further show that such skewness is much higher for seed-stage investments than for later-stage ones. Practitioners also embrace the idea that early-stage startup returns follow a power law (Pareto) distribution (Thiel and Masters (2014), Wilson (2015)). Additionally, the Pareto distribution allows us to capture limited liability facing investors as the distribution is bounded below.

[49] We assume that projects have bounded expected returns with $\alpha_j > 1$. We consider the extreme case of $\alpha_j \leq 1$ in Appendix H.

investing in a startup and how sensitivity to the tax credit differs across investor types (i.e., α_j). Intuitively, the tax credit increases the expected return to the investor, raising the chances of reaching her hurdle rate. The key insight of the model is that this effect declines as α_j decreases and the right tail of the distribution grows fatter. As α_j decreases, the expected return increases and the marginal benefit of the tax credit decreases, leading to lower sensitivity. This follows from the fact that the tax credit subsidy does not vary with investment returns. Instead, it is fixed at the time of investment. For example, the tax credit is the same if it supports an investment in a new coffee shop or a new high-tech company with high-growth potential. Given the different return profiles of the two firms, the ATC is less likely to be pivotal (i.e., change the decision to invest) for investing in the tech company than investing in the coffee shop.

This result is visualized in Figure 5, which plots the investment probability as a function of the tax credit rate and shows how the relationship depends on α_j. The chances of investment increase in the tax credit rate, but this relationship is flatter when α_j is smaller, indicating lower sensitivity. As α_j converges to 1, the slope converges to zero.[50] This stylized model helps us to interpret the survey finding that ATCs do not impact the decisions for investors following a "Home Run" approach. Conditional on access to projects with fat-tailed outcome distributions, tax credits are not useful at the margin because they represent fixed subsidies. When investing in more traditional firms with limited upside potential but also limited risk, the tax credits are more effective. This helps explain the larger sensitivity for non-professional investors documented in both the survey and our investor composition analysis.

More broadly, the model highlights that fat-tailed return distributions have important implications for the role of entry prices and thus for the effectiveness of early-stage investor subsidies. When the potential gains are very high (α_j is low), the entry price for early-stage investments is largely irrelevant for the extensive margin decision to invest in a startup.[51] The predictions above align well with observations from practitioners such as Charles Birnbaum, a partner at Bessemer Venture Partners, who noted "your entry price matters when you think

[50] Appendix H provides a numerical example of this relationship based on a calibrated value of α_j.

[51] It is important to note that our analysis is positive as opposed to normative. We do not imply that angel investors *should* assume that their returns follow the distribution described above, and therefore largely ignore the entry price. Also, the model does not imply that the tax credit is always an ineffective policy tool; conversely, it may effectively increase investments in subsistence-type companies. A key feature of the tax credit is that the size of the subsidy does not scale up with the quality of the company. As we show in Appendix H, other policies – like capital gains exemptions – may work better in this setting.

there's a ceiling [on the startup's exit valuation]."[52]

6 Conclusion

There is substantial government interest in supporting startups, and investor incentives are a particularly appealing option. As the global angel market rapidly expands, more jurisdictions are proposing implementing these programs. For example, Senator Christopher Murphy recently proposed legislation to establish a federal angel investor tax credit in the U.S.[53] Yet there has been no systematic evidence on the effectiveness of these policies.

This paper offers the first analysis of U.S. angel tax credits. We find that angel tax credits significantly increase state-level angel investment. This increase is connected to a decline in the ex-ante growth characteristics of marginal startups funded by angels. Yet when we turn to real outcomes that policymakers focus on, such as new business creation or young firm employment, we find no significant impacts. The lack of any real effect is not driven by these programs being too small or limited statistical power. Rather, two mechanisms together help to explain these seemingly puzzling results. First, the investment that increases due to the policy – generating the positive causal effects that we observe on angel investment – partially crowds out investment that would have happened in the absence of the policy. Second, the types of investors who respond tend to be local and non-professional; the additional companies that they finance tend to be low-growth and relatively old, muting potential effects on firm entry and job creation.

We then ask why professional investors who tend to fund high-risk, high-growth startups do not respond to the angel tax credits. A survey documents that investors view tax credits as unimportant to their investment decisions. The more professional and experienced an investor is, the higher the chance she will find them unimportant. The survey also suggests that professional investors find the ATCs unimportant because they take a "Home Run" investment approach. Using a stylized model, we show that the low sensitivity of professional investors to the tax credit may stem from the fat-tailed distribution of early-stage investment returns. These findings shed new light on how angel investors make decisions. They are likely related to the importance of non-monetary factors such as certification and advice that angel investors provide, as opposed to capital constraints being the primary scarce factor. This is a promising

[52]See Birnbaum Podcast.
[53]See Senate Bill.

topic for future research.

Our findings raise questions about the ability of investor tax credits to stimulate entrepreneurial activity. Angel tax credits, relative to direct programs such as grants, have the attractive feature of being more market-based tools that do not require the government to identify which companies deserve subsidies. However, this flexibility presents problems of its own as the targeted investors may not be sensitive to the policy. Our results highlight the importance of program design and investor type. Targeting investors who can identify and monitor high-growth startups is an important element of government programs focused on subsidizing capital for high-growth entrepreneurship.

Finally, angel tax credits likely represent a regressive tax policy. The credits accrue to rich people given the income and wealth requirements to become an accredited angel investor. If the credits had large job creation effects, there might be an argument for "trickle down" benefits to poorer people. However, since we find no effects on job creation and instead find evidence of crowding out, it seems likely that the programs lead to transfers from less wealthy to more wealthy taxpayers, creating potentially large opportunity costs from alternative uses of these public funds.

REFERENCES

Abadie, Alberto, "Statistical Nonsignificance in Empirical Economics," *American Economic Review: Insights*, 2020, *2* (2), 193–208.

Acs, Zoltan, Thomas Åstebro, David Audretsch, and David T Robinson, "Public Policy to Promote Entrepreneurship: A Call to Arms," *Small Business Economics*, 2016, *47* (1), 35–51.

Andreoni, James and A Abigail Payne, "Do Government Grants to Private Charities Crowd Out Giving or Fund-Raising?," *American Economic Review*, 2003, *93* (3), 792–812.

Andrews, Raymond J, Catherine Fazio, Jorge Guzman, Yupeng Liu, and Scott Stern, "The Startup Cartography Project," *Working paper*, 2020.

Arefeva, Alina, Morris A Davis, Andra C Ghent, and Minseon Park, "Who Benefits from Place-Based Policies? Job Growth from Opportunity Zones," *Workng paper*, 2020.

Bergemann, Dirk and Ulrich Hege, "Venture capital financing, moral hazard, and learning," *Journal of Banking & Finance*, 1998, *22* (6-8), 703–735.

Bernstein, Shai, Arthur Korteweg, and Kevin Laws, "Attracting Early-Stage Investors: Evidence from a Randomized Field Experiment," *Journal of Finance*, 2017, *72* (2), 509–538.

__ , Josh Lerner, and Filippo Mezzanotti, "Private Equity and Financial Fragility during the Crisis," *Review of Financial Studies*, 2019, *32* (4), 1309–1373.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan, "How Much Should We Trust Differences-In-Differences Estimates?," *Quarterly Journal of Economics*, 2004, *119* (1), 249–275.

Bhargava, Saurabh and Dayanand Manoli, "Psychological Frictions and the Incomplete Take-up of Social Benefits: Evidence from an IRS Field Experiment," *American Economic Review*, 2015, *105* (11), 3489–3529.

Black, Bernard, Alex Hollingsworth, Leticia Nunes, and Kosali Simon, "Simulated Power Analyses for Observational Studies: An Application to the Affordable Care Act Medicaid Expansion," *Working paper*, 2019.

Bloom, Nicholas and John Van Reenen, "New Approaches to Surveying Organizations," *American Economic Review*, 2010, *100* (2), 105–09.

Brander, James A, Qianqian Du, and Thomas Hellmann, "The Effects of Government-Sponsored Venture Capital: International Evidence," *Review of Finance*, 2015, *19* (2), 571–618.

Bronzini, Raffaello and Eleonora Iachini, "Are Incentives for R&D Effective? Evidence from a Regression Discontinuity Approach," *American Economic Journal: Economic Policy*, 2014, *6* (4), 100–134.

Callaway, Brantly and Pedro HC Sant'Anna, "Difference-in-differences with multiple time periods," *Journal of Econometrics*, 2021, *225* (2), 200–230.

Chetty, Raj and Amy Finkelstein, "Public Economics," *NBER Reporter*, 2020, *1*, 1–6.

Chow, Shein-Chung, Hansheng Wang, and Jun Shao, *Sample Size Calculations in Clinical Research*, Chapman and Hall/CRC, 2007.

Cohen, Susan, Daniel C Fehder, Yael V Hochberg, and Fiona Murray, "The Design of Startup Accelerators," *Research Policy*, 2019, *48* (7), 1781–1797.

Coolican, Patrick J., "Angel Investment Tax Credit Pricey but Has Defenders," *Minnesota Star Tribune*, 2015.

Crane, Leland and Ryan Decker, "Research with Private Sector Business Microdata: The Case of Nets/D&B," *Working paper*, 2020.

Cumming, Douglas J and Jeffrey G MacIntosh, "Crowding Out Private Equity: Canadian Evidence," *Journal of Business Venturing*, 2006, *21* (5), 569–609.

Cummins, Jason G, Kevin A Hassett, R Glenn Hubbard, Robert E Hall, and Ricardo J Caballero, "A Reconsideration of Investment Behavior Using Tax Reforms as Natural Experiments," *Brookings Papers on Economic Activity*, 1994, *1994* (2), 1–74.

Curtis, E Mark and Ryan Decker, "Entrepreneurship and State Taxation," *Working paper*, 2018.

Da Rin, Marco and Ludovic Phalippou, "The importance of size in private equity: Evidence from a survey of limited partners," *Journal of Financial Intermediation*, 2017, *31*, 64–76.

Dai, Zhonglan, Edward Maydew, Douglas A Shackelford, and Harold H Zhang, "Capital Gains Taxes and Asset Prices: Capitalization or Lock-In?," *Journal of Finance*, 2008, *63* (2), 709–742.

Davis, Jesse, Adair Morse, and Xinxin Wang, "The Leveraging of Silicon Valley," *Working paper*, 2020.

Dechezleprêtre, Antoine, Elias Einiö, Ralf Martin, Kieu-Trang Nguyen, and John Van Reenen, "Do Tax Incentives for Research Increase Firm Innovation? An RD Design for R&D," *Working paper*, 2020.

Decker, Ryan A, John Haltiwanger, Ron S Jarmin, and Javier Miranda, "Changing Business Dynamism and Productivity: Shocks versus Responsiveness," *American Economic Review*, 2020, *110* (12), 3952–90.

DellaVigna, Stefano and Elizabeth Linos, "RCTs to Scale: Comprehensive Evidence from Two Nudge Units," *Working paper*, 2020.

Denes, Matthew, "When Do Firms Risk Shift? Evidence from Venture Capital," *Working paper*, 2019.

Deshpande, Manasi and Yue Li, "Who Is Screened Out? Application Costs and the Targeting of Disability Programs," *American Economic Journal: Economic Policy*, 2019, *11* (4), 213–48.

Edwards, Alexander and Maximilian Todtenhaupt, "Capital Gains Taxation and Funding for Start-Ups," *Journal of Financial Economics*, 2020, *138* (2), 549–571.

Ersahin, Nuri, Ruidi Huang, and Naveen Khanna, "Competition, Reputation, and Venture Capital Investment," *Working paper*, 2021.

Evans, David S and Boyan Jovanovic, "An Estimated Model of Entrepreneurial Choice under Liquidity Constraints," *Journal of Political Economy*, 1989, *97* (4), 808–827.

Ewens, Michael and Joan Farre-Mensa, "The Deregulation of the Private Equity Markets and the Decline in IPOs," *Review of Financial Studies*, 2020, *33* (12), 5463–5509.

_ and Nadya Malenko, "Board Dynamics over the Startup Life Cycle," *Working paper*, 2020.

_ and Richard R Townsend, "Are Early Stage Investors Biased against Women?," *Journal of Financial Economics*, 2020, *135* (3), 653–677.

_ , Ramana Nanda, and Matthew Rhodes-Kropf, "Cost of Experimentation and the Evolution of Venture Capital," *Journal of Financial Economics*, 2018, *128* (3), 422–442.

Fazio, Catherine, Jorge Guzman, and Scott Stern, "The Impact of State-Level R&D Tax Credits on the Quantity and Quality of Entrepreneurship," *Working paper*, 2019.

Fehder, Daniel C and Yael V Hochberg, "Spillover Effects of Startup Accelerator Programs: Evidence from Venture-Backed Startup Activity," *Working paper*, 2019.

Freedman, Matthew, David Neumark, and Shantanu Khanna, "Combining Rules and Discretion in Economic Development Policy: Evidence on the Impacts of the California Competes Tax Credit," *Working paper*, 2021.

Gompers, Paul A, Will Gornall, Steven N Kaplan, and Ilya A Strebulaev, "How Do Venture Capitalists Make Decisions?," *Journal of Financial Economics*, 2020, *135* (1), 169–190.

Gompers, Paul, Steven N Kaplan, and Vladimir Mukharlyamov, "What Do Private Equity Firms Say They Do?," *Journal of Financial Economics*, 2016, *121* (3), 449–476.

Gonzalez-Uribe, Juanita and Daniel Paravisini, "How Sensitive is Young Firm Investment to the Cost of Outside Equity?," *Working paper*, 2019.

Graham, John R and Campbell R Harvey, "The Theory and Practice of Corporate Finance: Evidence from the Field," *Journal of Financial Economics*, 2001, *60* (2-3), 187–243.

Hall, Bronwyn and John Van Reenen, "How Effective Are Fiscal Incentives for R&D? A Review of the Evidence," *Research Policy*, 2000, *29* (4-5), 449–469.

Hall, Robert E and Susan E Woodward, "The Burden of the Nondiversifiable Risk of Entrepreneurship," *American Economic Review*, 2010, *100* (3), 1163–94.

Hellmann, Thomas and Veikko Thiele, "Friends or Foes? The Interrelationship between Angel and Venture Capital Markets," *Journal of Financial Economics*, 2015, *115* (3), 639–653.

— **, Paul Schure, and Dan H Vo**, "Angels and venture capitalists: substitutes or complements?," *Journal of Financial Economics*, 2021, *141* (2), 454–478.

Hochberg, Yael V, Carlos J Serrano, and Rosemarie H Ziedonis, "Patent Collateral, Investor Commitment, and the Market for Venture Lending," *Journal of Financial Economics*, 2018, *130* (1), 74–94.

Howell, Sabrina T, "Financing Innovation: Evidence from R&D Grants," *American Economic Review*, 2017, *107* (4), 1136–64.

— , "Reducing Information Frictions in Venture Capital: The Role of New Venture Competitions," *Journal of Financial Economics*, 2020, *136* (3), 676–694.

— **and J David Brown**, "Do Cash Windfalls Affect Wages? Evidence from R&D Grants to Small Firms," *Working paper*, 2019.

Hsu, David H, "Experienced Entrepreneurial Founders, Organizational Capital, and Venture Capital Funding," *Research Policy*, 2007, *36* (5), 722–741.

Huang, Laura, Andy Wu, Min Ju Lee, Jiayi Bao, M Hudson, and E Bolle, "The American Angel," *Whitepaper Angel Capital Association*, 2017.

Inderst, Roman and Holger M Mueller, "Early-stage financing and firm growth in new industries," *Journal of Financial Economics*, 2009, *93* (2), 276–291.

Isakov, Leah, Andrew W Lo, and Vahid Montazerhodjat, "Is the FDA Too Conservative or Too Aggressive?: A Bayesian Decision Analysis of Clinical Trial Design," *Journal of Econometrics*, 2019, *211* (1), 117–136.

Ivković, Zoran, James Poterba, and Scott Weisbenner, "Tax-Motivated Trading by Individual Investors," *American Economic Review*, 2005, *95* (5), 1605–1630.

Kerr, William R, Josh Lerner, and Antoinette Schoar, "The Consequences of Entrepreneurial Finance: Evidence from Angel Financings," *Review of Financial Studies*, 2014, *27* (1), 20–55.

— **, Ramana Nanda, and Matthew Rhodes-Kropf**, "Entrepreneurship as Experimentation," *Journal of Economic Perspectives*, 2014, *28* (3), 25–48.

Khanna, Naveen and Richmond D Mathews, "Skill versus reliability in venture capital," *Journal of Financial Economics*, 2022, *145* (2), 41–63.

Knight, Brian, "Endogenous Federal Grants and Crowd-Out of State Government Spending: Theory and Evidence from the Federal Highway Aid Program," *American Economic Review*, 2002, *92* (1), 71–92.

Korinek, Anton and Joseph E Stiglitz, "Dividend Taxation and Intertemporal Tax Arbitrage," *Journal of Public Economics*, 2009, *93* (1-2), 142–159.

Lach, Saul, "Do R&D Subsidies Stimulate or Displace Private R&D? Evidence from Israel," *Journal of Industrial Economics*, 2002, *50* (4), 369–390.

Lafontaine, Francine and Kathryn Shaw, "Serial entrepreneurship: Learning by doing?," *Journal of Labor Economics*, 2016, *34* (S2), S217–S254.

Lee, Samuel and Petra Persson, "Financing from Family and Friends," *Review of Financial Studies*, 2016, *29* (9), 2341–2386.

Lerner, Josh, *Boulevard of Broken Dreams: Why Public Efforts to Boost Entrepreneurship and Venture Capital Have Failed–and What to Do about It*, Princeton University Press, 2009.

__ , "Government incentives for entrepreneurship," 2020.

__ , Antoinette Schoar, Stanislav Sokolinski, and Karen Wilson, "The Globalization of Angel Investments: Evidence across Countries," *Journal of Financial Economics*, 2018, *127* (1), 1–20.

Lindsey, Laura and Luke CD Stein, "Angels, Entrepreneurship, and Employment Dynamics: Evidence from Investor Accreditation Rules," *Working paper*, 2020.

Malenko, Andrey, Ramana Nanda, Matthew Rhodes-Kropf, and Savitar Sundaresan, "Investment Committee Voting and the Financing of Innovation," *Working paper*, 2020.

Manso, Gustavo, "Motivating Innovation," *Journal of Finance*, 2011, *66* (5), 1823–1860.

McCahery, Joseph A, Zacharias Sautner, and Laura T Starks, "Behind the Scenes: The Corporate Governance Preferences of Institutional Investors," *Journal of Finance*, 2016, *71* (6), 2905–2932.

McKenzie, David, "Identifying and Spurring High-Growth Entrepreneurship: Experimental Evidence from a Business Plan Competition," *American Economic Review*, 2017, *107* (8), 2278–2307.

Moretti, Enrico, Claudia Steinwender, and John Van Reenen, "The Intellectual Spoils of War? Defense R&D, Productivity and International Spillovers," *Working paper*, 2019.

Mumford, Jeanette A, "A Power Calculation Guide for fMRI Studies," *Social Cognitive and Affective Neuroscience*, 2012, *7* (6), 738–742.

Othman, Abraham, "Startup Growth and Venture Returns," *AngelList*, 2019.

Poterba, James M, "Capital Gains Tax Policy toward Entrepreneurship," *National Tax Journal*, 1989, *42* (3), 375–389.

Robb, Alicia and David Robinson, "The Capital Structure Decisions of Startup Firms," *Review of Financial Studies*, 2012, *1* (1), 1–27.

Sakpal, Tushar, "Sample Size Estimation in Clinical Trial," *Perspectives in Clinical Research*, 2010, *1* (2), 67–67.

Scherer, Frederic M and Dietmar Harhoff, "Technology Policy for a World of Skew-Distributed Outcomes," *Research Policy*, 2000, *29* (4-5), 559–566.

Shane, Scott, *Fool's Gold?: The Truth behind Angel Investing in America*, Oxford University Press, 2009.

— , "The Problem with Tax Credits for Angel Investors," *Bloomberg Businessweek*, 2010.

Shapiro, Bradley, Günter J Hitsch, and Anna Tuchman, "TV Advertising Effectiveness and Profitability: Generalizable Results from 288 Brands," *Econometrica*, 2021.

Slemrod, Joel and Shlomo Yitzhaki, "Tax Avoidance, Evasion, and Administration," in "Handbook of Public Economics," Vol. 3, Elsevier, 2002, pp. 1423–1470.

Sun, Liyang and Sarah Abraham, "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects," *Journal of Econometrics*, 2021, *225* (2), 175–199.

Thiel, Peter A and Blake Masters, *Zero to One: Notes on Startups, or How to Build the Future*, Currency, 2014.

Weaver, David and Jeff Cornwall, "Should Angel Investors Get Tax Credits to Invest in Small Businesses?," *Wall Street Journal*, 2012.

Wilson, Fred, "Power Law and the Long Tail," *AVC*, 2015. Available at https://avc.com/2015/11/power-law-and-the-long-tail.

Xu, Ting, "Learning from the Crowd: The Feedback Value of Crowdfunding," *Available at SSRN 2637699*, 2019.

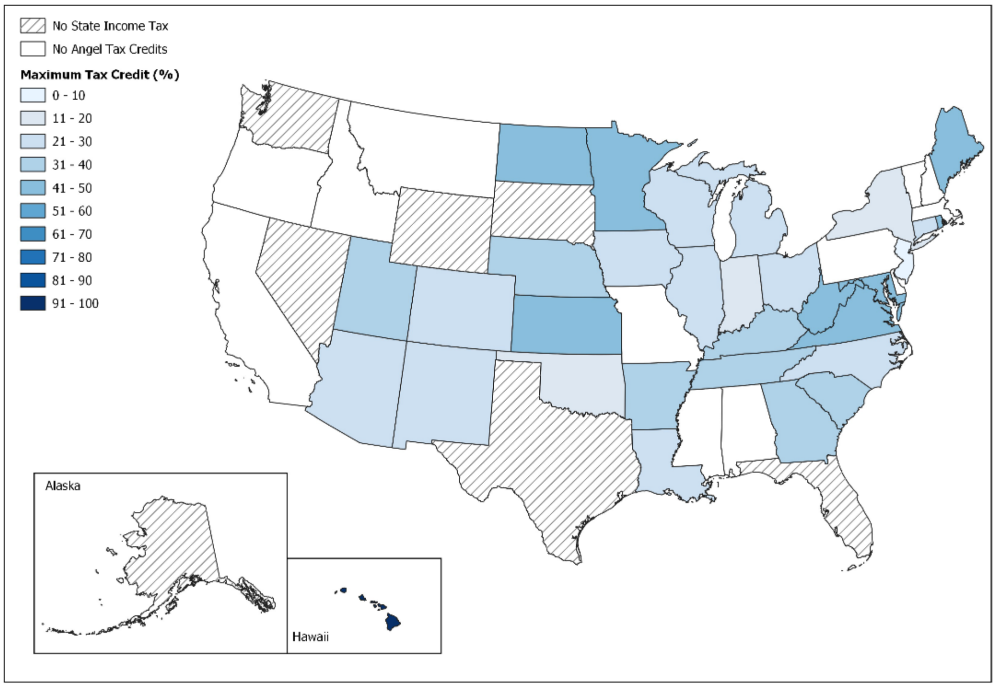
Yagan, Danny, "Capital Tax Reform and the Real Economy: The Effects of the 2003 Dividend Tax Cut," *American Economic Review*, 2015, *105* (12), 3531–63.

— **and James Mahon**, "Tax Policy and Heterogeneous Investment Behavior," *American Economic Review*, 2017, *107* (1), 217–48.

Figure 1: State Angel Tax Credit Programs

Panel A provides a map of states that have adopted angel tax credit programs from 1988 to 2018. The blue shading indicates the tax credit percentage, with darker shades representing larger tax credits. The slanted lines denote states with no state income tax. Panel B shows the introduction and termination of each program in our sample, starting with the earliest program and ending with the most recent one.

Panel A. States with Angel Tax Credit Programs



Panel B. Timing of State Angel Tax Credit Programs

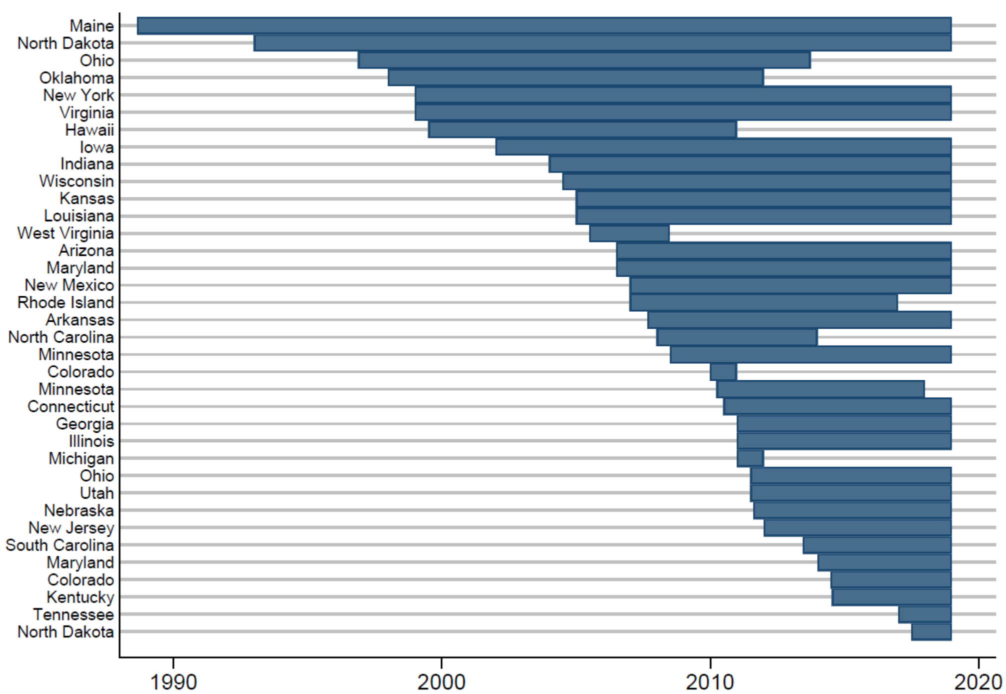


Figure 2: Dynamic Effects of Angel Tax Credit Introduction

This figure shows the dynamic effects of introducing angel tax credits using equation (2). The dots denote the point estimates of dynamic differences-in-differences coefficients and the bars indicate 95% confidence intervals. The year before policy introduction is normalized to zero. Panel A shows the number of angel investments; Panel B examines the entry by young (age 0-5) high-tech firms in a state; Panel C shows the entry rate among young high-tech firms; Panel D examines the number of new jobs created by young high-tech firms; Panel E looks at the job creation rate among young high-tech firms. All outcome variables are log transformed and are defined at the state-year level. The sample period is 1988-2018. Detailed variable definitions are in Appendix B. Standard errors are clustered at the state level.

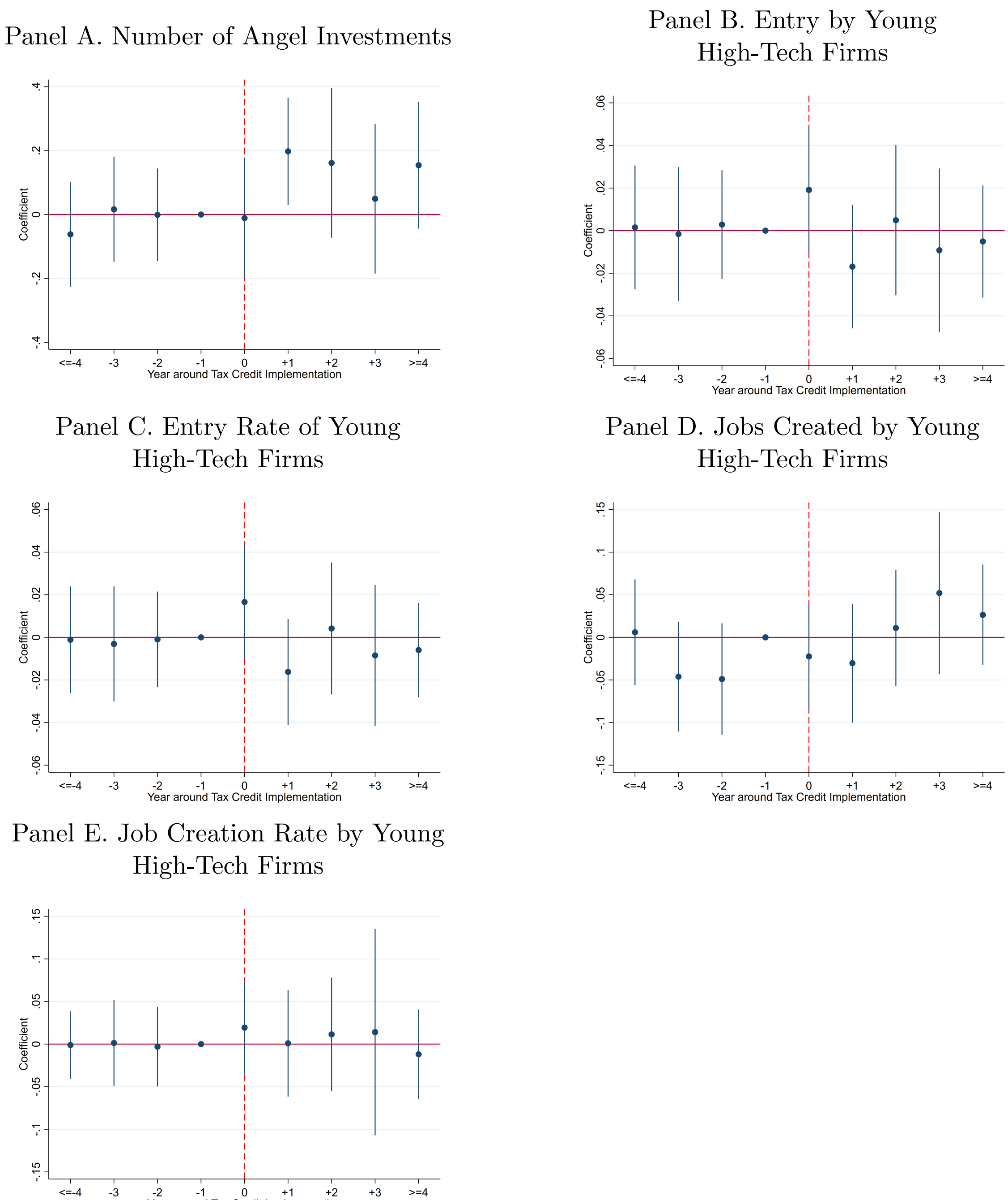
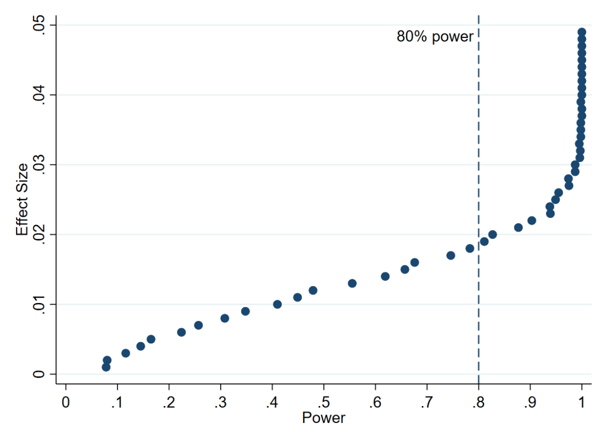


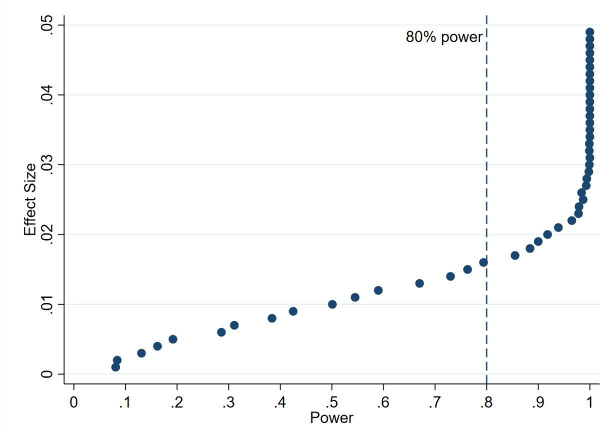
Figure 3: Power and Prior for the Effect of Angel Tax Credits on Real Outcomes

This figure shows the relationship between the estimated power of our differences-in-differences model and the minimum detectable effect (MDE) for the four main real outcomes considered in Table 4 at the statewide level. Power is computed using the simulation method detailed in Appendix D and represents the likelihood that our test detects a significant effect of angel tax credits (at 10% significance) when we induce an effect equal to MDE in the data. Each dot represents the MDE for a given power. The solid horizontal line denotes our prior effect (see Appendix E for calculation), and the dotted line denotes 80% power.

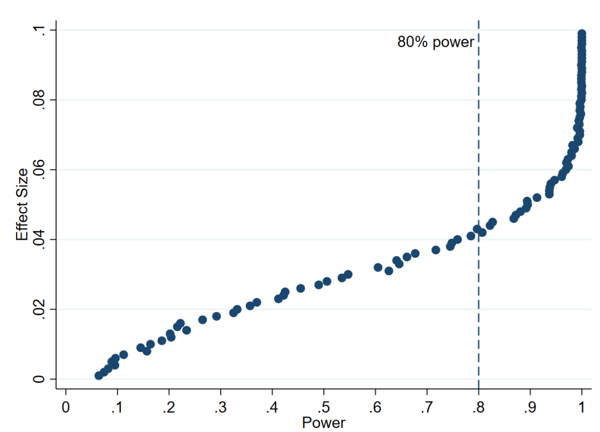
Panel A. Entry by Young High-Tech Firms



Panel B. Entry Rate of Young High-Tech Firms



Panel C. Job Creation by Young High-Tech Firms



Panel D. Job Creation Rate by Young High-Tech Firms

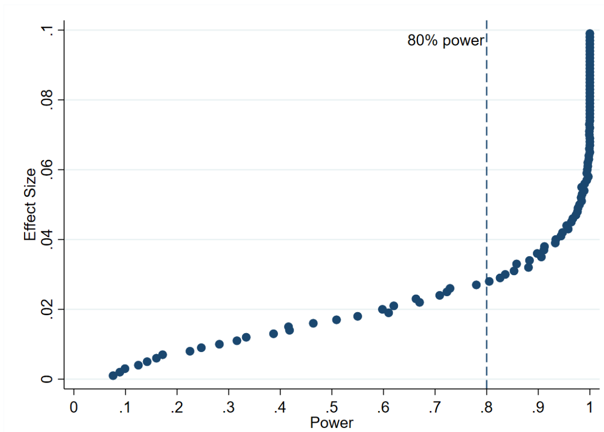
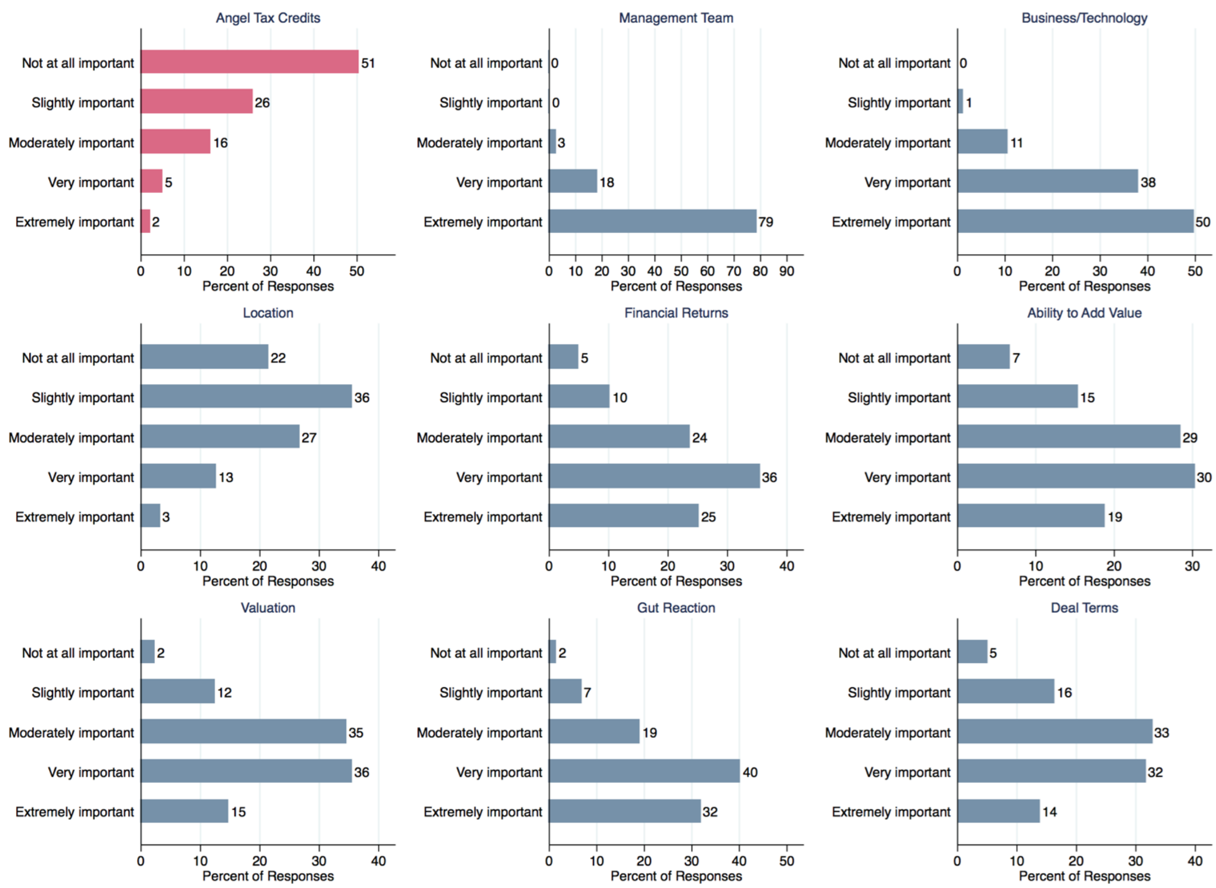


Figure 4: Survey Results

Panel A. Distribution of Responses to Factor Importance Question

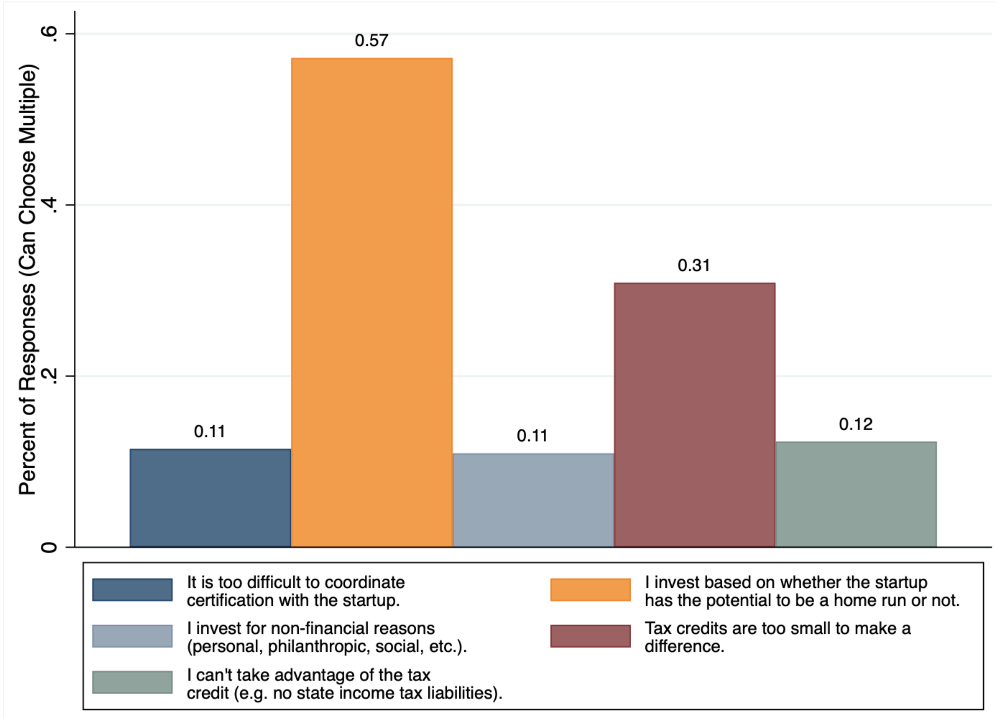


These graphs show the distribution of responses to question 1 in the survey for each of the nine investment factors. Respondents could only choose one importance level for each factor. The order in which the factors were presented was randomized across survey participants. N=1,364.

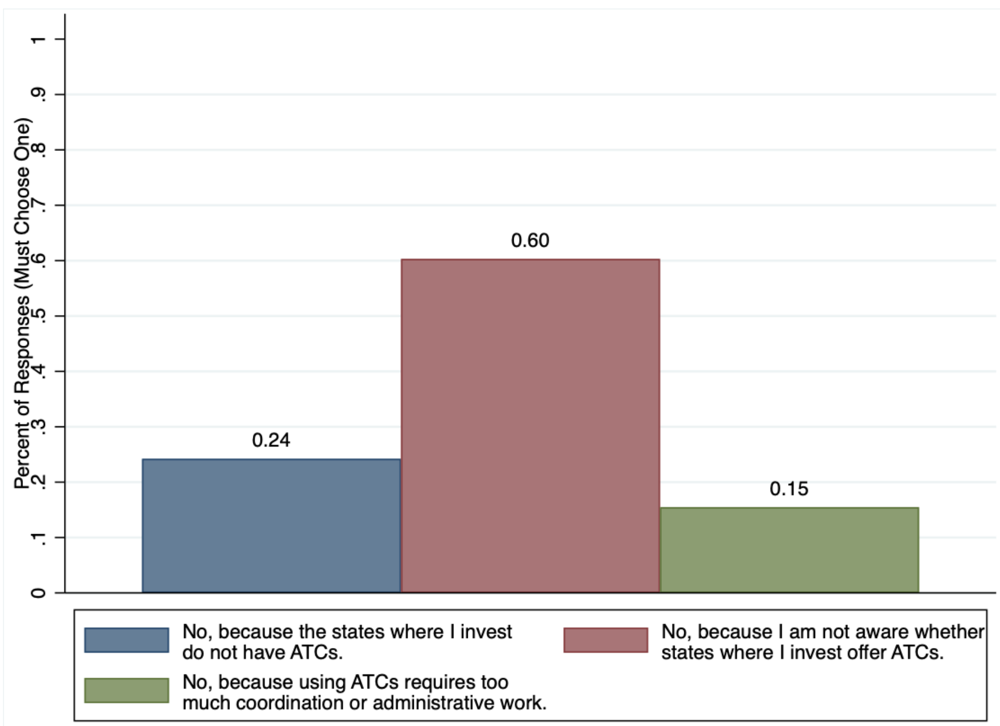
Panel B. Distribution of Responses to Importance of Angel Tax Credits by Respondent Type

These graphs show the distribution of responses to the question of whether angel tax credits are important to the decision to invest in a startup. Each graph presents a different sample. The top graph shows the subset of respondents who were either angel tax credit recipients from our state-provided data, or who reported having used an angel tax credit in the survey (N=268). The second graph shows the subset of respondents from AngelList data who reported having never used an angel tax credit in the survey (N=1,028). The third graph shows the subset of respondents from AngelList data who identify as professional investors (N=241). The bottom graph shows the subset of respondents from AngelList data whose number of deals are in the top 10% among all AngelList responders (N=84). For this graph, no respondents answered "Very important." Respondents could only choose one importance level. The order in which the factors were presented was randomized across survey participants.

Panel C. Why Angel Tax Credits Are Unimportant (If Rated as Unimportant)



Panel D. Distribution of Responses to Why Have Not Used Angel Tax Credits



Panel C shows the distribution of responses to the question of why angel tax credits are unimportant (N=948) to the decision to invest in a startup. Respondents were prompted to answer the question of why the credits are unimportant if they rated them as not at all or slightly important. Panel D shows the distribution of responses to the question of why an investor has not used angel tax credits, conditional on not using them (N=1,028). For this question, respondents could only choose one option.

Figure 5: Model Prediction: Investment Probability and Investor Tax Credit Rate

This figure plots investment probability against tax credit rate τ and shows how the relationship varies with the shape of the return distribution α. We consider cases where α is equal to 1, 1.5, 2, 5, and 10. A lower α represents a Pareto distribution with a fatter tail. We assume cost of capital $k = 10\%$ and $C = 1$. Appendix H details the investment probability function and the associated parameters.

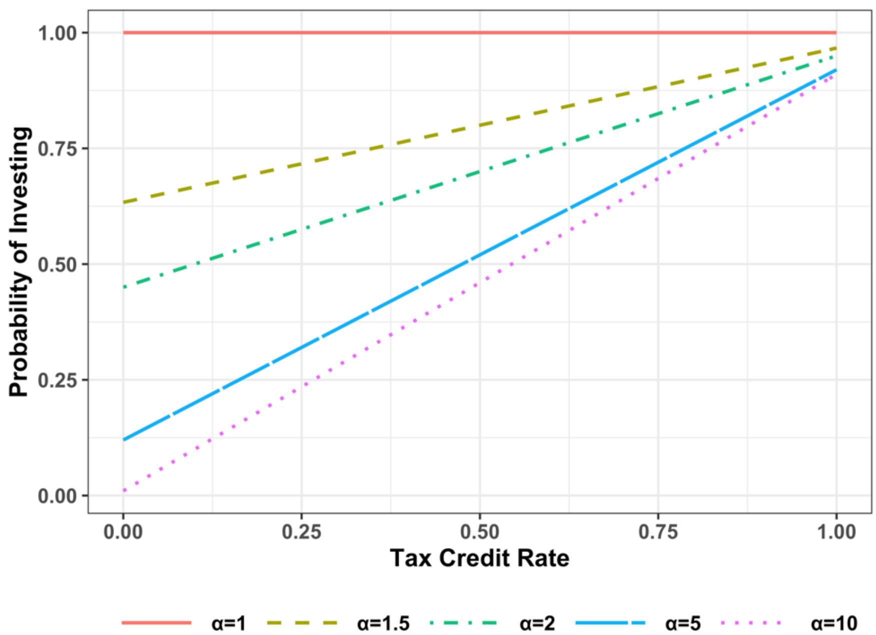


Table 1: Summary Statistics on Angel Tax Credit Programs

Table 1 presents the program parameters for the 36 angel tax credit programs in our sample. Column 1 reports the percentage of programs that have a particular restriction in place. Columns 2 and 3 report the mean and median values of these restrictions.

	% with restriction	Mean	Median
Tax credit %		34%	33%
		Company restrictions	
Age cap	31%	7.1	6.0
Employment cap	39%	64.6	50.0
Revenue cap ($ million)	47%	5.4	5.0
Asset cap ($ million)	22%	11.5	7.5
Prior total external financing cap ($ million)	19%	5.7	4.0
		Investment and investor restrictions	
Minimum investment per investor ($)	36%	19,231	25,000
Minimum holding period (years)	50%	3.2	3.0
Ownership cap before investment	64%	35%	30%
Exclude owners and their families	61%		
Exclude full-time employees	22%		
Exclude executives and officers	33%		
		Tax credit restrictions	
State tax credit allocation per year ($ million)	86%	9.0	5.0
Maximum tax credit per company per year ($ million)	42%	0.81	0.6
Maximum tax credit per investor per year ($ million)	78%	0.21	0.11
Non-refundable	72%		
No carry forward	11%		
Non-transferrable	72%		

Table 2: Summary Statistics

This table reports summary statistics for the state-year level variables used in our analyses and investor-level characteristics. All variables are defined in Appendix B.

	N	Mean	Std. dev.	p5	p50	p95
	Treatment variables					
$\mathbb{1}$(ATC)	1,200	0.25	0.43	0.00	0.00	1.00
Tax credit %	1,200	0.09	0.18	0.00	0.00	0.50
Ln(agg. TC cap)	1,168	3.36	6.31	0.00	0.00	15.65
Ln(agg. supported investment)	1,168	3.60	6.77	0.00	0.00	16.59
	Real outcomes					
Entry by young HT firms (statewide)	1,550	1,475	1,994	135	833	5,706
Entry by young HT firms (top MSAs)	1,550	1,122	1,740	50	531	5,447
Jobs created by young HT firms (statewide)	1,550	11,330	17,196	810	5,831	42,277
Jobs created by young HT firms (top MSAs)	1,550	8,940	15,430	329	3,877	37,291
Entry rate of young HT firms (statewide)	1,550	0.271	0.027	0.225	0.272	0.314
Entry rate of young HT firms (top MSAs)	1,550	0.272	0.032	0.218	0.273	0.319
Jobs creation rate by young HT firms (statewide)	1,550	0.356	0.059	0.270	0.352	0.453
Jobs creation rate by young HT firms (top MSAs)	1,550	0.360	0.070	0.259	0.354	0.465
	Financing outcomes					
Number of angel investments (unrestricted sample)	1,550	133.5	330.1	1.0	40.0	465.0
Number of angel investments (NETS-matched sample)	1,200	24.2	65.9	0.0	8.0	78.0
Aggregate early-stage financing amount	1,200	1,394	5,847	0	185	5,362
Aggregate non-angel financing amount	1,200	1,058	5,399	0	87	3,861
Aggregate angel financing amount	1,200	336	1,049	0	38	1,319
Angel share among early-stage financing	1,200	0.42	0.32	0.02	0.33	1.00
	Investor characteristics on AngelList					
In-state	89,146	0.51	0.50	0.00	1.00	1.00
Inexperienced	89,146	0.73	0.45	0.00	1.00	1.00
Had no exit	89,146	0.41	0.49	0.00	0.00	1.00
No founder experience	89,146	0.81	0.39	0.00	1.00	1.00

Table 3: Angel Tax Credits and Angel Investments

Panel A reports the differences-in-differences estimates for the effect of angel tax credit programs on the log number of angel investments in the high-tech sector (IT, biotech, and renewable energy). Columns 1 to 2 use the unrestricted sample of angel deals from 1988 to 2018. Columns 3 to 4 use the sample of deals that can be matched to NETS from 1993 to 2016. $\mathbb{1}(ATC)$ is an indicator variable equaling one if a state has an angel tax credit program in that year. *Tax Credit %* is a continuous variable equal to the maximum tax credit percentage available in a state-year with an angel tax credit program and is zero in state-years without a program. Panel B splits the angel volume in the NETS-matched sample by different pre-investment startup characteristics at the median (employment, employment growth, fraction of serial entrepreneurs on founding team, and age). Control variables are defined in equation (1). Each observation is a state-year. All specifications include state and year fixed effects. Standard errors are reported in parentheses and clustered by state. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A: Angel Investments

	Ln(no. of angel investments)			
	(1)	(2)	(3)	(4)
$\mathbb{1}$(ATC)	0.164***		0.174**	
	(0.058)		(0.073)	
Tax Credit %		0.348***		0.535***
		(0.128)		(0.169)
Sample	Unrestricted		NETS-matched	
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	1,550	1,550	1,200	1,200
Adjusted R^2	0.954	0.954	0.912	0.912

Panel B: Angel Investments by Ex-Ante Growth Characteristics

	Employment		Growth		Serial Entrep.		Age	
	High	Low	High	Low	High	Low	Young	Old
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}$(ATC)	-0.001	0.249***	0.081	0.186**	-0.003	0.186*	0.091	0.197***
	(0.073)	(0.082)	(0.065)	(0.082)	(0.105)	(0.098)	(0.066)	(0.067)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,200	1,200	1,200	1,200	1,200	1,200	1,200	1,200
Adjusted R^2	0.880	0.872	0.892	0.860	0.733	0.874	0.899	0.833

Table 4: Angel Tax Credits and Real Effects

This table provides the differences-in-differences estimates of the effect of angel tax credit programs on firm entry and job creation from BDS. In Panel A, the dependent variable is the log number of young, high-tech firms in columns 1-2 and firm entry rate in columns 3- 4. Young firms are defined as age 0-5. In Panel B, the dependent variable is the log number of new jobs created by young, high-tech firms in columns 1-2 and job creation rate by these firms in columns 3-4. The odd columns construct these variables using only data from the top MSAs, which are defined as the largest MSAs by angel volume that account for at least 90% of angel deals in the year before the tax credit implementation. The even columns use statewide data. MDE for 80% power is the minimum detectable effect (MDE) for 80% power. Details are in Appendix B for variable definitions and in Appendix D for power calculations. The sample period is 1988 to 2018. $\mathbb{1}(ATC)$ is an indicator equaling one if a state has an angel tax credit program in that year. Control variables are defined in equation (1). Each observation is a state-year. All specifications include state and year fixed effects. Standard errors are reported in parentheses and clustered by state. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A: Firm Entry

	Counts		Rates	
	(1)	(2)	(3)	(4)
$\mathbb{1}$(ATC)	-0.020	-0.003	-0.010	-0.002
	(0.022)	(0.009)	(0.012)	(0.008)
Year and State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Geography	Top MSAs	State	Top MSAs	State
MDE for 80% Power	0.040	0.019	0.022	0.017
Observations	1,550	1,550	1,550	1,550
Adjusted R^2	0.993	0.996	0.496	0.620

Panel B: Job Creation

	Counts		Rates	
	(1)	(2)	(3)	(4)
$\mathbb{1}$(ATC)	0.006	0.018	0.012	0.001
	(0.026)	(0.019)	(0.016)	(0.018)
Year and State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Geography	Top MSAs	State	Top MSAs	State
MDE for 80% Power	0.059	0.044	0.031	0.028
Observations	1,550	1,550	1,550	1,550
Adjusted R^2	0.981	0.983	0.221	0.283

Table 5: Angel Tax Credits and Firm-Level Outcomes

This table reports the effect of receiving a tax credit on firm-level outcomes, using the sample of firms that applied to be certified for angel investors to receive a tax credit. $\mathbb{1}(Tax\,Credit_{it})$ is an indicator variable for startup i having an investor receive a tax credit in year t. The dependent variable in column 1 is an indicator variable denoting that a startup received VC financing within two years after applying to be certified for angel investors to receive a tax credit. The dependent variable in column 2 is an indicator variable equaling one if a startup experienced a successful exit (via acquisition or IPO). The dependent variables in columns 3-4 are indicator variables equaling one if a startup had more than 25 employees either two or three years after it applied to be certified for angel investors to receive a tax credit. This is repeated in columns 5-6 except using the 75th percentile employment among firms in the sample. Employment data are from the Steppingblocks LinkedIn panel. $\mathbb{1}(Finance\,pre\text{-}TC\,Yr)$ is an indicator variable for whether a firm received any other external financing before its investors received a tax credit. All specifications include sector-year and state-year fixed effects. Standard errors are clustered at the state-year level. ***, **, and * denotes significance at the 1%, 5%, and 10% level, respectively.

	Raised VC	Exit	Empl > 25		Empl > 75th Pctile	
			2 Yrs Post-TC	3 Yrs Post-TC	2 Yrs Post-TC	3 Yrs Post-TC
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}$(Tax Credit)	-0.009	-0.005	0.006	0.010	0.018	0.015
	(0.016)	(0.009)	(0.006)	(0.007)	(0.014)	(0.013)
$\mathbb{1}$(Finance pre-TC Yr)	0.174***	0.086***	0.029***	0.040***	0.003	-0.005
	(0.027)	(0.015)	(0.010)	(0.011)	(0.014)	(0.017)
$\mathbb{1}$(Empl > 25 in TC Yr)			0.811***	0.780***		
			(0.040)	(0.039)		
$\mathbb{1}$(Empl > 75th Pctile in TC Yr)					0.753***	0.739***
					(0.030)	(0.033)
Sector-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,218	3,218	3,218	3,218	3,218	3,218
Adjusted R^2	0.260	0.047	0.480	0.421	0.612	0.575

Table 6: Crowding Out

This table examines whether angel tax credit programs crowd out alternative early-stage financing. Panel A examines the effect of angel tax credits on aggregate early-stage financing received by young high-tech firms at the state-year level. The dependent variables are aggregate early-stage financing, non-angel financing, angel financing, and the fraction of angel financing in a state-year. All financing amounts are log transformed. Early-stage financing are all early rounds (see Section 5.1 for detailed definition) identified in CVV and Form D data, including angel rounds. Panel B examines the effect of angel tax credits on total early-stage financing received by firms at the firm-level. Columns 1-2 are unweighted and columns 3-4 weight each observation by one over the number of firms in each state. The sample period is 1993 to 2016. Standard errors are clustered at the state level. ***, **, and * denotes significance at the 1%, 5%, and 10% level, respectively.

Panel A: Aggregate Financing at State-Year Level

	Ln(early-stage) (1)	Ln(non-angel) (2)	Ln(angel) (3)	Angel share (4)
$\mathbb{1}$(ATC)	-0.068	-0.326*	0.268*	0.075**
	(0.118)	(0.178)	(0.142)	(0.029)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	1,200	1,200	1,200	1,200
Adjusted R^2	0.853	0.706	0.813	0.247

Panel B: Total Early-Stage Financing at Firm Level

	Ln(early-stage)			
	(1)	(2)	(3)	(4)
$\mathbb{1}$(ATC)	0.005	-0.001	-0.006	0.002
	(0.038)	(0.045)	(0.048)	(0.046)
Weighted	No	No	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Observations	38,487	38,487	38,487	38,487
Adjusted R^2	0.098	0.099	0.088	0.094

Table 7: Relabeling

Panel A reports summary statistics for tax credit recipients who are insider investors, defined as angel investors who also serve as executives or managers at the firm for which they receive angel tax credits, as well as their family members. For company-level statistics, the unit of observation is a unique tax credit beneficiary company for which we observe an investor-company link. For investor-level statistics, the unit of observation is a unique investor for which we observe an investor-company link. Panel B compares the Form D filing rate by beneficiary firms (treated) and matched non-beneficiary firms (control). The panel also compares covariates across the two samples. Each treated firm is matched to up to five similar control firms through a nearest neighbor matching procedure. To match with a treated firm, the control firm(s) must also have received angel financing, be located in a different state but the same Census division, belong to the same sector, have a similar age (within two years), and have a similar amount of previous financing relative to the year of the treatment firm's first tax credit.

Panel A: Tax Credit Take-up by Insiders

	N	Fraction
	Company level	
≥1 investor is executive or has family member who is executive	628	0.35
among Kentucky companies	77	0.04
among Maryland companies	81	0.38
among New Jersey companies	63	0.24
among New Mexico companies	61	0.26
among Ohio companies	346	0.44
≥1 investor is an executive	628	0.33
	Investor level	
Investor is executive or has family who is executive	3,560	0.14
Investor is executive	3,560	0.11

Panel B: Form D Filing Rate by Beneficiary and Matched Non-Beneficiary Firms

	Got Tax Credit			No Tax Credit			t-Test	
	Mean	SD	Obs	Mean	SD	Obs	t-value	p-value
Filed Form D	0.644	0.479	517	0.320	0.467	3,129	−14.56	0.000
Year Founded	2009.5	4.137	517	2009.3	3.803	3,129	−1.282	0.200
Total Financing	10.15	27.25	517	8.106	23.28	3,129	−1.035	0.301
Average Emp	6.450	10.55	517	7.811	88.61	3,129	0.386	0.700
Average Sales	777,256	3,390,227	517	663,931	3,015,808	3,129	−0.661	0.508

Table 8: Characteristics of Investors Receiving Tax Credits

This table describes the characteristics of investors who received angel tax credits. We gather information from LinkedIn on angel investors from seven states that publicly release the names of individual investors who received angel tax credits. Corporate Executive is an investor who lists their current occupation as President, Vice President, Partner, Principal, Managing Director, or Chief Officer other than CEO. Gender and race are identified from pictures. An individual's approximate age is derived from adding 22 years to the difference between the individual's college graduation year and the median year of investment in the sample, which is 2013.

	N	Fraction		N	Fraction
Number of investor-tax credit pairs	8,218		Profession	3,286	
			Corp. Exec.		0.82
Number of unique investors	5,637		Doctor		0.073
Illinois		0.14	Entrepreneur		0.062
Kentucky		0.05	Lawyer		0.041
Maryland		0.16	Investor		0.007
Minnesota		0.39	Other		0.003
New Jersey		0.09			
New Mexico		0.03	Race	4,446	
Ohio		0.14	White		0.95
			South Asian		0.03
Location is in state	4,694	0.79	East Asian		0.02
			Black		0.007
Male	4,702	0.87	Hispanic		0.002
			Middle Eastern		0.001
	N	Mean			
Age	2,363	41.9			

Table 9: Which Investors Respond to Angel Tax Credits?

This table examines changes in investor composition due to angel tax credit programs. Panel A reports the differences-in-differences estimates for the effects of angel tax credits on investor characteristics using AngelList. Each observation is an investor-startup pair (i.e., investment) and is weighted by one over the number of observations in each state. The dependent variables are indicators if an investor was in-state, inexperienced (less than 5 years of deal experience), had no prior exit, or had no prior founder experience. All specifications include CBSA and year fixed effects. Panel B reports the differences-in-differences estimates for the effects of angel tax credits on the entry of investors using AngelList. Each observation is a state-year. The dependent variable is the log number of investors in each category (in-state, out-of-state, inexperienced, experienced, had no prior exit, had exit, no prior founder experience, had founder experience) who invested in a state-year. All specifications include state and year fixed effects. $\mathbb{1}(ATC)$ is an indicator equaling one if a state has an angel tax credit program in that year. Control variables are defined in equation (1). The sample period is 2003 to 2017 in both panels. Standard errors are reported in parentheses and clustered by state. ***, **, and * denotes significance at the 1%, 5%, and 10% level, respectively.

Panel A: Investor Characteristics at the Investment Level

	In-state	Inexperienced	Had no exit	No founder experience
	(1)	(2)	(3)	(4)
$\mathbb{1}$(ATC)	0.075**	0.041**	0.073**	0.069**
	(0.031)	(0.018)	(0.027)	(0.032)
Year FE	Yes	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	89,146	89,146	89,146	89,146
Adjusted R^2	0.202	0.102	0.176	0.109

Panel B: Investor Entry at the State-Year Level

	In-state	Out-of-state	Inexperienced	Experienced	Had no exit	Had exit	No founder experience	Has founder experience
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}$(ATC)	0.284**	0.194*	0.275**	0.103	0.277***	0.150	0.262**	0.134
	(0.119)	(0.099)	(0.106)	(0.112)	(0.095)	(0.114)	(0.101)	(0.131)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	735	735	735	735	735	735	735	735
Adjusted R^2	0.867	0.848	0.863	0.821	0.854	0.846	0.867	0.803

Table 10: Survey Analysis

This table examines investors' perception of the importance of angel tax credits based on survey data. In Panel A, the dependent variable is *ATC importance*, a score that takes a value of 1 to 5 (1 being "not at all important" and 5 being "extremely important"). Column 1 examines whether a respondent has done an above median number of angel deals since January 2018. Column 2 focuses on investor experience measured by matching respondents to AngelList data. Column 3 examines investor profession. Column 4 examines surveyed importance of other investment factors. The first four independent variables describe any past experience using AngelList data. The remaining variables are based on the survey. Panel B examines how deal experience correlates with the reasons a respondent perceives angel tax credits as unimportant. All regressions include state fixed effects. Standard errors are clustered by state. ***, **, and * denotes significance at the 1%, 5%, and 10% level, respectively.

Panel A. ATC Importance

	ATC importance			
	(1)	(2)	(3)	(4)
Above median no. of deals since 2018	-0.229***			
	(0.041)			
Has exit (AL)		-0.199***		
		(0.039)		
Has founder exper (AL)		-0.118*		
		(0.061)		
Has invested as insider (AL)		0.103**		
		(0.049)		
Top school (AL)		-0.138***		
		(0.033)		
Corp Executive			-0.144	
			(0.110)	
Entrepreneur			-0.193*	
			(0.105)	
Investor			-0.375***	
			(0.136)	
Team importance				-0.103**
				(0.040)
Business importance				0.127***
				(0.034)
Location importance				0.055*
				(0.031)
Financial return importance				0.117***
				(0.020)
Add value importance				0.041**
				(0.017)
Valuation importance				0.001
				(0.031)
Gut reaction importance				-0.02
				(0.021)
Deal terms importance				0.141***
				(0.029)
State FE	Yes	Yes	Yes	Yes
Observations	1,202	1,199	1,242	1,331
Adjusted R^2	0.126	0.048	0.121	0.170

Panel B. Reasons for ATC Unimportance

	Home run (1)	Coordination (2)	Non-financial (3)	Too small (4)	Cannot use (5)
Above median no. of deals since 2018	0.046**	0.051**	0.006	-0.021	0.003
	(0.020)	(0.024)	(0.016)	(0.021)	(0.021)
State FE	Yes	Yes	Yes	Yes	Yes
Observations	1,202	1,202	1,202	1,202	1,202
Adjusted R^2	0.018	0.025	0.007	0.018	0.090

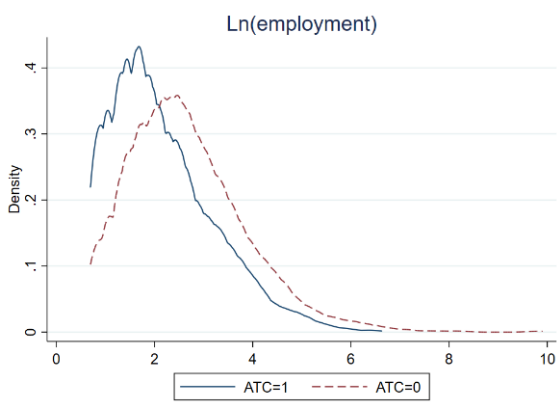
A Appendix: Additional Figures and Tables

(For Online Publication)

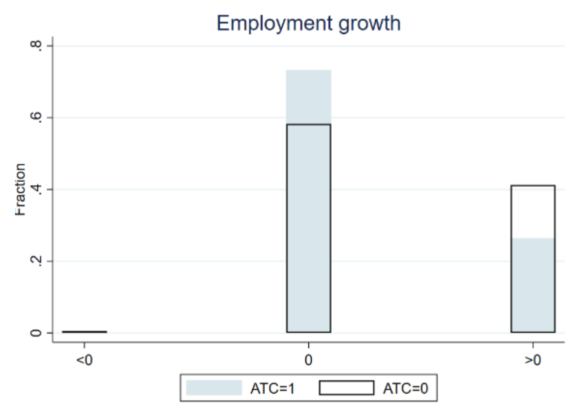
Figure A.1: Distributions of Ex-Ante Growth Characteristics

Panel A (B) compares the distributions of ex-ante employment (employment growth) of angel-backed firms in state-years with an angel tax credit program to state-years without a program, restricting to states that ever had an angel tax credit program. Employment and employment growth are measured in the year before angel investment. In Panel A, the solid line (dotted line) represents the estimated kernel density for firms that received angel investments in state-years with (without) an angel tax credit program. Panel B shows the histogram where employment growth is discretized into negative growth, zero growth, and positive growth. Panel C compares the histograms of exit outcomes by angel-backed firms in state-years with an angel tax credit program to those in state-years without a program, restricting to states that ever had an angel tax credit program. The blue bars (empty bars) represent the fraction of angel-backed firms achieving each exit outcome by the end of 2018 and that received angel investments in state-years with (without) an angel tax credit program from 1985 to 2016. Panel D compares the distribution of the log of the exit multiple for angel-backed firms that have achieved M&A or IPO by the end of 2018 and received angel investments in state-years with (without) an angel tax credit program from 1985 to 2016. Exit multiple is defined as total enterprise value at exit divided by total invested capital. All variables are defined in Appendix B.

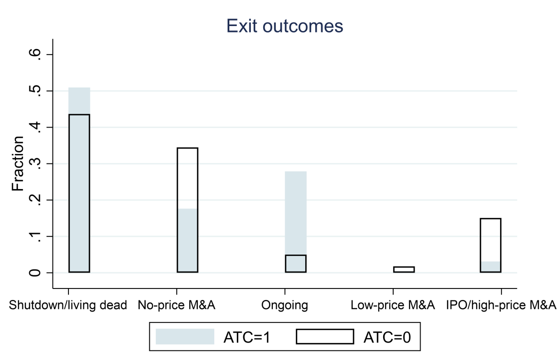
Panel A



Panel B



Panel C



Panel D

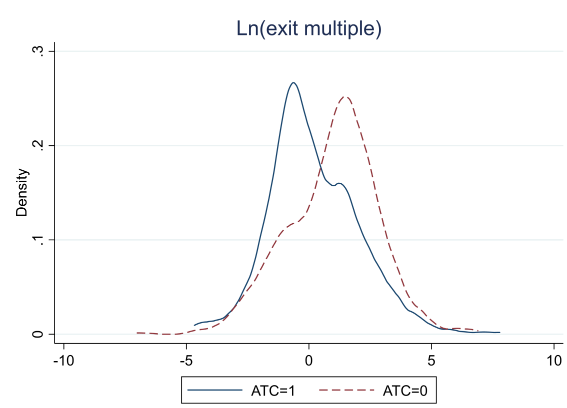
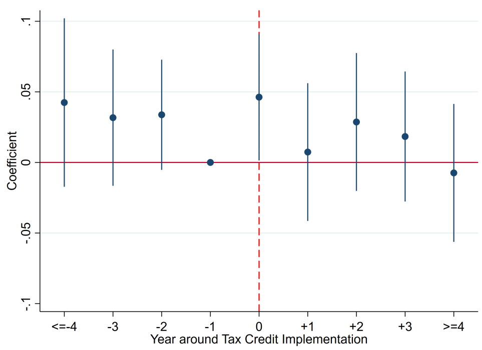


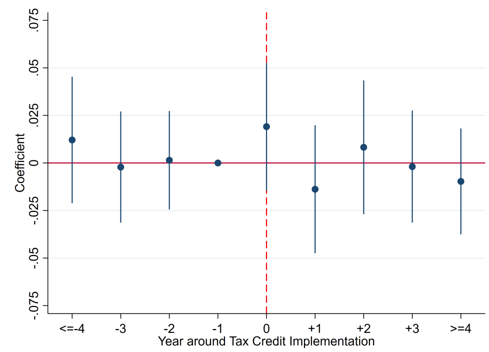
Figure A.2: Dynamic Effects of Angel Tax Credit Introduction on Real Aggregate Outcomes in Top-MSAs

This figure shows the dynamic effects of angel tax credit introduction on real aggregate outcomes in top MSAs using equation (2). Top MSAs are the largest MSAs by angel volume that account for at least 90% of angel deals in the year before tax credit implementation. The year before policy introduction is normalized to zero. Panel A examines the entry of young (age 0-5) high-tech firms; Panel B shows the entry rate among young high-tech firms; Panel C examines the number of new jobs created by young high-tech firms; and Panel D looks at the job creation rate among young high-tech firms. All outcome variables are log transformed and are defined at the state-year level. Detailed variable definitions are in Appendix B. Standard errors are clustered at the state level. 95% confidence intervals are shown.

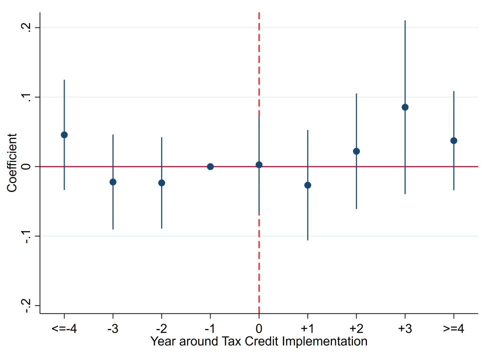
Panel A. Entry by Young High-Tech Firms



Panel B. Entry Rate of Young High-Tech Firms



Panel C. Jobs Created by Young High-Tech Firms



Panel D. Job Creation Rate by Young High-Tech Firms

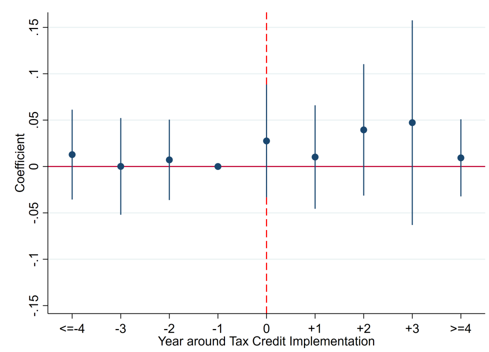
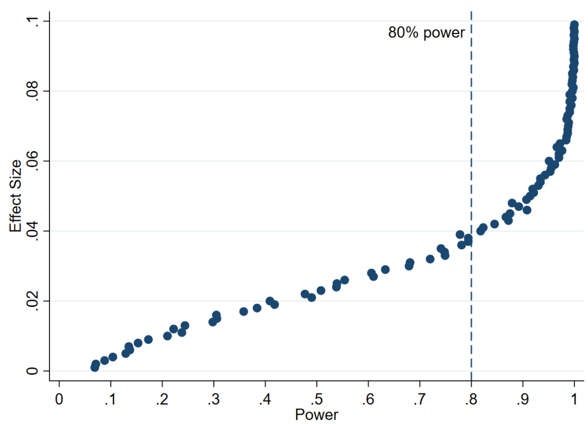


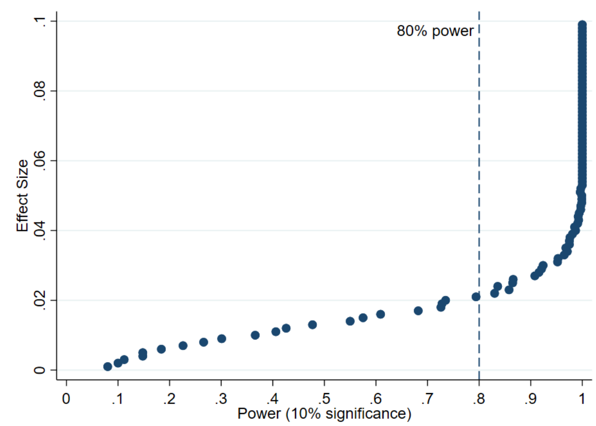
Figure A.3: Power and Prior for the Effect of Angel Tax Credits on Real Aggregate Outcomes in Top MSAs

This figure shows the relationship between estimated power and minimum detectable effect (MDE) for the four real outcomes considered in Table 4 defined at the top MSA level. Top MSAs are the largest MSAs by angel volume that account for at least 90% of angel deals in the year before tax credit implementation. Power is computed using the simulation method detailed in Appendix D and represents the likelihood that our test detects a significant effect of angel tax credits (at 10% significance) when we induce an effect equal to MDE in the data. Each dot represents the MDE for a given power. The solid horizontal line denotes our prior effect (see Appendix E for calculation), and the dotted line denotes 80% power.

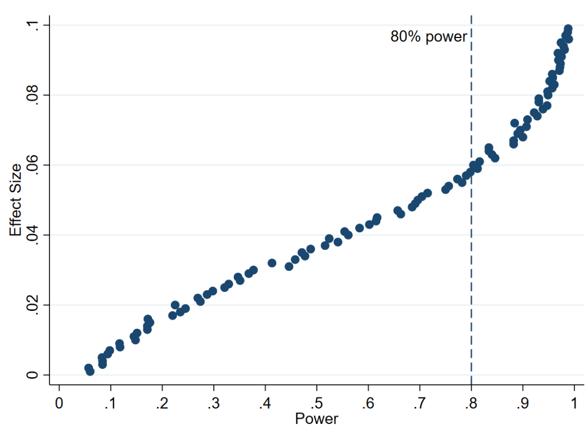
Panel A. Entry by Young High-Tech Firms



Panel B. Entry Rate of Young High-Tech Firms



Panel C. Job Creation by Young High-Tech Firms



Panel D. Job Creation Rate by Young High-Tech Firms

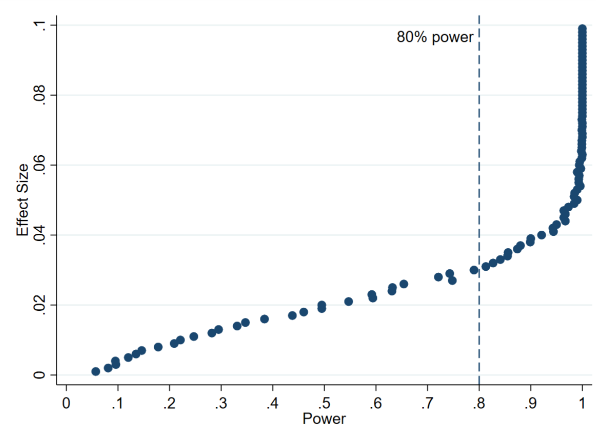
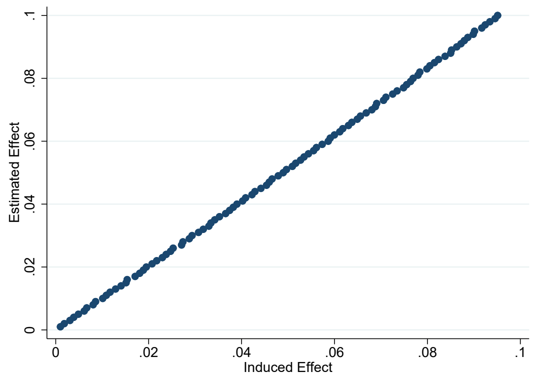


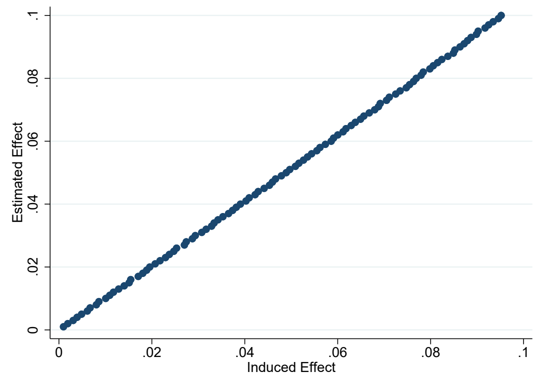
Figure A.4: Validity Check for Power Analysis

This figure plots the average estimated coefficient against the true effect size imposed in power simulation for the four main state-level real outcomes considered in Table 4.

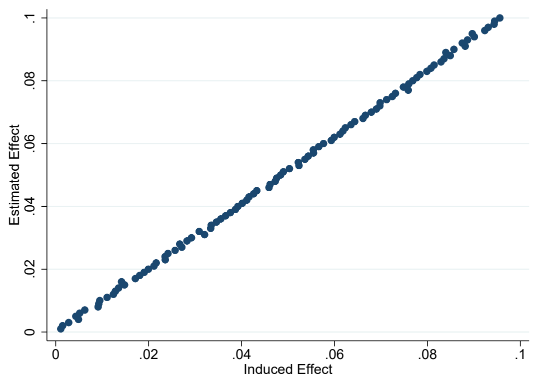
Panel A. Entry by Young High-Tech Firms



Panel B. Entry Rate of Young High-Tech Firms



Panel C. Job Creation by Young High-Tech Firms



Panel D. Job Creation Rate by Young High-Tech Firms

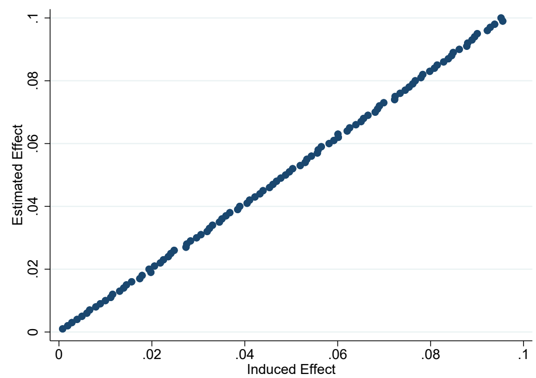


Table A.1: Tax Credit Program Details

This table lists the 36 angel tax credit programs in the U.S. from 1988 to 2018. For each program, it provides the state, program name, effective period, and tax credit percentage. It also details program-level company, investment, investor, and tax credit restrictions. We include the latest value for any restrictions that vary over a program's life.

State	Program	Effective year	Expiration year	Individuals or groups qualify for tax credit	Max tax credit percentage
Arizona	Angel Investment Program	2006	2021	Both	0.3–0.35
Arkansas	Equity Investment Incentive Program	2007	2019	Both	0.333
Colorado	Innovation Investment Tax Credit	2010	2010	Both	0.15
	Advanced Industry Investment Tax Credit	2014	2022	Both	0.25–0.3
Connecticut	Angel Investor Tax Credit Program	2010	2019	Both	0.25
Georgia	Angel Investment Tax Credit	2011	2018	Individuals	0.35
Hawaii	High Technology Business Investment Tax Credit	1999	2010	Both	0.1–1.0
Illinois	Angel Investment Credit Program	2011	2021	Both	0.25
Indiana	Venture Capital Investment Tax Credit Program	2004	2020	Both	0.2–0.25
Iowa	Innovation Fund Tax Credit	2002	ongoing	Both	0.2
Kansas	Angel Investor Tax Credit	2005	2021	Both	0.5
Kentucky	Angel Investment Act Tax Credit	2015	ongoing	Individuals	0.4–0.5
Louisiana	Angel Investor Tax Credit	2005	2021	Individuals	0.25
Maine	Seed Capital Tax Credit Program	1989	ongoing	Both	0.3–0.6
Maryland	Biotechnology Investment Incentive Tax Credit	2007	ongoing	Both	0.5
	Cybersecurity Investment Tax Credit	2014	2023	Both	0.33–0.5
Michigan	Small Business Investment Tax Credit	2011	2011	Groups	0.25
Minnesota	Angel Tax Credit	2010	2017	Both	0.25
	Seed Capital investment Credit	2019		Both	0.45
Nebraska	Angel Investment Tax Credit	2011	2022	Both	0.35–0.4
New Jersey	Angel Investor Tax Credit Program	2013	ongoing	Both	0.1
New Mexico	Angel Investment Credit	2007	2025	Individuals	0.25
New York	Qualified Emerging Technology Company Tax Credits	2000	ongoing	Both	0.1–0.2
North Carolina	Qualified Business Tax Credit Program	2008	2013	Both	0.25
North Dakota	Seed Capital Investment Tax Credit	1993	ongoing	Both	0.45
	Angel Investor Investment Credit	2017	ongoing	Both	0.35
Ohio	Ohio Technology Investment Tax Credit	1996	2013	Both	0.25–0.3
	InvestOhio	2011	ongoing	Both	0.1
Oklahoma	Small Business Capital Companies Tax Credit	1998	2011	Both	0.2
Rhode Island	Innovation Tax Credit	2007	2016	Both	0.5
South Carolina	High Growth Small Business Job Creation Act	2013	2019	Individuals	0.35
Tennessee	Angel Tax Credit	2017	ongoing	Individuals	0.33–0.5
Utah	Life Science and Technology Tax Credits	2011		Both	0.35
Virginia	Qualified Equity and Subordinated Debt Investments Credit	1999	ongoing	Individuals	0.5
West Virginia	High-Growth Business Investment Tax Credit	2005	2008	Both	0.5
Wisconsin	Qualified New Business Venture Program	2005	ongoing	Both	0.25

State	Program	Asset cap ($ million)	Revenue cap ($ million)	Employment cap	Age cap (years)
Arizona	Angel Investment Program	Assets < 2 if before 2012; Assets < 10 otherwise			
Arkansas	Equity Investment Incentive Program				
Colorado	Innovation Investment Tax Credit	Assets < 5	2		5
	Advanced Industry Investment Tax Credit		5		5
Connecticut	Angel Investor Tax Credit Program		1	25	7
Georgia	Angel Investment Tax Credit		0.5	20	3
Hawaii	High Technology Business Investment Tax Credit				
Illinois	Angel Investment Credit Program			100	10
Indiana	Venture Capital Investment Tax Credit Program		10		
Iowa	Innovation Fund Tax Credit	Net worth < 3 before 2005; Net worth < 10 otherwise			3 before 2009; 6 otherwise
Kansas	Angel Investor Tax Credit		5		5
Kentucky	Angel Investment Act Tax Credit	Net worth < 10		100	
Louisiana	Angel Investor Tax Credit	Net worth < 2	10	50	
Maine	Seed Capital Tax Credit Program		3 before 2014; 5 otherwise		
Maryland	Biotechnology Investment Incentive Tax Credit			50	10
	Cybersecurity Investment Tax Credit			50	
Michigan	Small Business Investment Tax Credit	Pre-investment valuation < 10		100	10 years if business uses MI university research; 5 otherwise
Minnesota	Angel Tax Credit			25	20 if med tech or pharma; 10 otherwise
	Seed Capital investment Credit				
Nebraska	Angel Investment Tax Credit			25	
New Jersey	Angel Investor Tax Credit Program			225	
New Mexico	Angel Investment Credit		5	100	
New York	Qualified Emerging Technology Company Tax Credits		10		
North Carolina	Qualified Business Tax Credit Program		5		
North Dakota	Seed Capital Investment Tax Credit		10		
	Angel Investor Investment Credit		10		
Ohio	Ohio Technology Investment Tax Credit	Net book value < 2.5	2.5		
	InvestOhio	Assets < 50	10		
Oklahoma	Small Business Capital Companies Tax Credit	Net worth < 1			
Rhode Island	Innovation Tax Credit		1		
South Carolina	High Growth Small Business Job Creation Act				5
Tennessee	Angel Tax Credit		3	25	5
Utah	Life Science and Technology Tax Credits				
Virginia	Qualified Equity and Subordinated Debt Investments Credit		3		
West Virginia	High-Growth Business Investment Tax Credit		20		
Wisconsin	Qualified New Business Venture Program			100	10

State	Program	Min. investment per investor ($)	Min. holding period (year)	Ownership cap before investment	Exclude existing owners and their families	Exclude full-time employees	Exclude executives and officers	Reporting req. for investor's firm	Previous external financing cap ($ million)	Registration req. for business
Arkansas	Equity Investment Incentive Program							N		Y
Arizona	Angel Investment Program	25,000	1	30%	Y			N	2 in total inv	Y
Colorado	Innovation Investment Tax Credit	25,000		30%	Y			N		Y
	Advanced Industry Investment Tax Credit	10,000		30%	Y			N	10 in inv, debt, equity	N
Connecticut	Angel Investor Tax Credit Program	25,000		50%	Y			N	2 in angel financing	Y
Georgia	Angel Investment Tax Credit		2					N	1 in equity or debt inv	Y
Hawaii	High Technology Business Investment Tax Credit		5							
Illinois	Angel Investment Credit Program	10,000	3	50%	Y			Y	10 in PE, 4 TC inv	Y
Indiana	Venture Capital Investment Tax Credit Program			50%	Y			N		Y
Iowa	Innovation Fund Tax Credit		3 if before 2014; none if after	70%	Y			N		N
Kansas	Angel Investor Tax Credit					Y	Y	Y		N
Kentucky	Angel Investment Act Tax Credit	10,000		20%	Y	Y		Y	1 in TC angel inv	Y
Louisiana	Angel Investor Tax Credit		3	50%	Y		Y	N		Y
Maine	Seed Capital Tax Credit Program		4	50%	Y		Y	Y		N
Maryland	Biotechnology Investment Incentive Tax Credit	25,000	2	25%	Y			N		Y
	Cybersecurity Investment Tax Credit	25,000	2	25%	Y			N		Y
Michigan	Small Business Investment Tax Credit	20,000	3		Y		Y	Y		Y
Minnesota	Angel Tax Credit	10,000	3	20%	Y		Y	Y	4 in PE	Y
	Seed Capital investment Credit			50%	Y			Y		Y
Nebraska	Angel Investment Tax Credit	25,000	3	50%	Y		Y	Y		Y
New Jersey	Angel Investor Tax Credit Program			80%	Y			N		N
New Mexico	Angel Investment Credit					Y	Y	N		N
New York	Qualified Emerging Technology Company Tax Credits		4	10%	Y			N		N
North Carolina	Qualified Business Tax Credit Program		1	10%	Y	Y	Y	N		Y
North Dakota	Seed Capital Investment Tax Credit		3	50%	Y			N		Y
	Angel Investor Investment Credit		3					N		N
Ohio	Ohio Technology Investment Tax Credit		3	5%	Y	Y		N		Y
	InvestOhio		2-5					N		Y
Oklahoma	Small Business Capital Companies Tax Credit							Y		N
Rhode Island	Innovation Tax Credit									Y
South Carolina	High Growth Small Business Job Creation Act		2					N		Y
Tennessee	Angel Tax Credit	15,000						Y		Y
Utah	Life Science and Technology Tax Credits	25,000	3	30%	Y			N		N
Virginia	Qualified Equity and Subordinated Debt Investments Credit		3		Y	Y	Y	N	3 in equity or debt inv	Y
West Virginia	High-Growth Business Investment Tax Credit		5	5%	Y		Y	N		N
Wisconsin	Qualified New Business Venture Program		3	20%	Y			N	10 in PE	Y

State	Program	Aggregate tax credit cap ($ million)	Max tax credit per company ($)	Max tax credit per investor ($)	Max tax credit amount per investor per business per year ($)	"First come first served" policy	Refundable	Transferrable	Carry over	Carry forward (year)	Total angel inv. in state during eff. year ($ million)	State funding as share of total angel inv. in state
Arizona	Angel Investment Program	2.5	600,000	250,000		Y	N	N	Y	3	4.2	0.60
Arkansas	Equity Investment Incentive Program	6.25					N	Y	Y	9	0	≥ 1
Colorado	Innovation Investment Tax Credit	0.75		20,000		Y	N	N	Y	5	44.62	0.02
	Advanced Industry Investment Tax Credit	0.75		50,000		Y	N	N	Y	5	143.59	0.01
Connecticut	Angel Investor Tax Credit Program	3	500,000	250,000		Y	N	N	Y	5	33.04	0.09
Georgia	Angel Investment Tax Credit	5-10		50,000		N	N	N	Y	5	28.97	0.35
Hawaii	High Technology Business Investment Tax Credit			700,000			Y	Y	Y	Unlimited	12.41	
Illinois	Angel Investment Credit Program	10	1,000,000		500,000	Y	N	N	Y	5	49.87	0.20
Indiana	Venture Capital Investment Tax Credit Program	12.5		1,000,000		N	N	Y after 2012; N before 2012	Y	5	0	≥ 1
Iowa	Innovation Fund Tax Credit	2-4	500,000	100,000	50,000	Y	Y	Y	Y	3-5	8.33	≥ 1
Kansas	Angel Investor Tax Credit	6		250,000	50,000	Y	N	Y	Y	Unlimited	0	≥ 1
Kentucky	Angel Investment Act Tax Credit	3		200,000		Y	N	Y	Y	15	9.55	0.31
Louisiana	Angel Investor Tax Credit	3.6		362,880	181,440	Y		Y			6.51	≥ 1
Maine	Seed Capital Tax Credit Program	Lifetime cap 30 before 2014; 5 otherwise	5,000,000		500,000	Y	Y	N	Y	15	3.07	≥ 1
Maryland	Biotechnology Investment Incentive Tax Credit	6-12		250,000		Y	Y	N			75.32	0.16
	Cybersecurity Investment Tax Credit	2-4	250,000 to 500,000			Y	Y	N	N			
Michigan	Small Business Investment Tax Credit	9	1,000,000	250,000	250,000		N		Y	5	24.81	0.36
Minnesota	Angel Tax Credit	15		125,000			Y	N	Y		33.7	0.45
	Seed Capital investment Credit			112,500			N	N	Y	4		
Nebraska	Angel Investment Tax Credit	3-4		300,000		Y	Y	N	N		13.27	0.30
New Jersey	Angel Investor Tax Credit Program	25			500,000	Y	Y	N	Y for corporate; N for individuals		46.17	0.54
New Mexico	Angel Investment Credit	2			62,500	Y	N	N	Y	5 years if after 2015; 3 years if before 2015	7.2	0.28
New York	Qualified Emerging Technology Company Tax Credits			150,000			Y		Y	Unlimited	279.57	
North Carolina	Qualified Business Tax Credit Program	7.5			50,000	N		N	Y	5	15.82	0.47

State	Program	Aggregate tax credit cap ($ million)	Max tax credit per company ($)	Max tax credit per investor ($)	Max tax credit amount per investor per business per year ($)	"First come first served" policy	Refundable	Transferrable	Carry over	Carry forward (year)	Total angel inv. in state during eff. year ($ million)	State funding as share of total angel inv. in state
North Dakota	Seed Capital Investment Tax Credit	3.5	225,000	112,500		Y	N	N	Y	4	0	≥ 1
	Angel Investor Investment Credit			45,000			N	N	Y	5		
Ohio	Ohio Technology Investment Tax Credit	45		62,500		Y	N	N	Y	15	0	≥ 1
	InvestOhio	50		500,000		Y	N	N	Y	7	46.66	≥ 1
Oklahoma	Small Business Capital Companies Tax Credit						N	N	Y	3 years if after 2006; 10 years if before 2006	0	
Rhode Island	Innovation Tax Credit	0.5			100,000		N	N	Y	3	6.18	0.08
South Carolina	High Growth Small Business Job Creation Act	5		100,000			N	Y	Y	10	11.2	0.45
Tennessee	Angel Tax Credit	4		50,000		Y	N	N	Y	5	34.68	0.12
Utah	Life Science and Technology Tax Credits						N		N			
Virginia	Qualified Equity and Subordinated Debt Investments Credit	5		50,000		N	N	N	Y	15	35	0.14
West Virginia	High-Growth Business Investment Tax Credit	1	500,000	50,000		Y	N	N	Y	4	0	≥ 1
Wisconsin	Qualified New Business Venture Program	30	2,000,000				N	Y for early stage, seed investment credit, N for angel investor tax credit	Y	15	1.08	≥ 1

Table A.2: Tax Credit Applicant Summary Statistics

This table presents summary statistics on companies that applied to be eligible for an investor tax credit, some of which had an investor receiving a credit ("beneficiary companies") and some of which did not ("failed applicants"). Panel A tabulates these two groups by state. Panel B compares the characteristics between the two groups. Employment data are from the Steppingblocks LinkedIn panel. All variables are defined in Appendix B.

Panel A: Unique Tax Credit Applicants by State

	Received Tax Credit	No Tax Credit
AZ	144	145
CO	109	25
CT	100	70
KS	199	63
KY	60	101
MD	87	0
MN	338	205
NJ	69	6
NM	72	0
OH	374	537
SC	65	136
WI	206	116
Total	1,823	1,404

Panel B: Summary Statistics

	Received Tax Credit	No Tax Credit	p-value
Tax credit (TC) amount ($ thou)	32.00	0.00	0.00
$\mathbb{1}$(Finance pre-TC)	0.37	0.12	0.00
$\mathbb{1}$(Raised VC 2 yrs post-TC)	0.26	0.16	0.00
$\mathbb{1}$(Exit)	0.066	0.037	0.00
Empl in TC Yr	4.33	3.55	0.09
$\mathbb{1}$(Empl $>$ 25 in TC Yr)	0.037	0.031	0.34
Emp. 2yrs post-TC	5.95	4.23	0.00
$\mathbb{1}$(Emp. $>$ 25 2 yrs Post-TC)	0.058	0.036	0.00

Table A.3: Angel Tax Credits and Angel Investments: Additional Analysis

Panel A reports the differences-in-differences estimates for the effect of angel tax credits on the amount of angel activities based on AngelList. The dependent variables are the log number of angel investments, number of unique invested companies, and number of unique investors in a state-year, respectively. Investments, companies, and investors are assigned to state-years based on the invested companies' locations. $\mathbb{1}(ATC)$ is an indicator variable equaling one if a state has an angel tax credit program in that year. *Tax credit %* is a continuous variable equal to the maximum tax credit percentage available in a state-year with an angel tax credit program and is zero otherwise. Panel B examines heterogeneity in the effect of angel tax credit programs on the log number of angel investments in the high-tech sector. *Flex* is an index ranging from 0 to 17 that measures the presence and flexibility of the 17 program restrictions in Table 1. Higher values of the index represent more flexible programs. *VC Supply* is state-year-level aggregate VC investment amount (excluding angel and seed rounds identified in our main sample) scaled by the total number of young firms (ages 0 to 5) in that state-year. Both *Flex* and *VC Supply* are standardized by subtracting the sample mean and dividing by the standard deviation. Panel C examines the effect of angel tax credits on the state-year average ex-ante characteristics of angel-backed firms in the high-tech sector (IT, biotech, and renewable energy). Dependent variables in columns 1 to 4 are based on firms that have non-imputed employment numbers from NETS, in columns 5 and 6 are the average fraction of serial entrepreneurs, and in columns 7 and 8 are average age at the time of investment. Panel D restricts to angel investments in top MSAs or non-top MSAs within each state. Top MSAs are those that account for at least 90% of angel deals in the year before tax credit implementation. The sample period is 2003 to 2017 in Panel A and 1993 to 2016 in Panels B, C, and D. Standard errors are reported in parentheses and clustered by state. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A: ATC and Angel Activities on AngelList

	Ln(no. of investments)		Ln(no. of companies)		Ln(no. of investors)	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}$(ATC)	0.281**		0.239***		0.274**	
	(0.109)		(0.086)		(0.103)	
Tax Credit %		0.818***		0.648***		0.782***
		(0.200)		(0.167)		(0.190)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	735	735	735	735	735	735
Adjusted R^2	0.872	0.873	0.899	0.899	0.870	0.871

Panel B: Heterogeneity

	(1)	(2)	(3)	(4)
$\mathbb{1}$(ATC)	0.160**	0.171**		
	(0.068)	(0.066)		
$\mathbb{1}$(ATC) × Flex	0.121*			
	(0.064)			
$\mathbb{1}$(ATC) × VC Supply		-0.173***		
		(0.050)		
Tax Credit %			0.393***	0.374**
			(0.141)	(0.155)
Tax Credit % × Flex			0.351***	
			(0.084)	
Tax Credit % × VC Supply				-0.272***
				(0.068)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	1,200	1,200	1,200	1,200
Adjusted R^2	0.913	0.913	0.914	0.914

Panel C: ATC and Ex-ante Growth Characteristics of Angel-Backed Firms

	Ln(employment)		Employment growth		Serial entrepreneurs		Age at angel round	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}$(ATC)	-0.178**		-0.106**		-0.016*		0.462*	
	(0.087)		(0.042)		(0.009)		(0.231)	
Tax credit %		-0.452**		-0.254***		-0.042**		0.966**
		(0.177)		(0.087)		(0.017)		(0.468)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,200	1,200	1,200	1,200	957	957	1,084	1,084
Adjusted R^2	0.183	0.183	0.075	0.075	0.126	0.126	0.273	0.272

Panel D: Angel Investment Volume in Top and Non-Top MSAs

	(1)	(2)	(3)	(4)
$\mathbb{1}$(ATC)	0.189**	0.100		
	(0.090)	(0.093)		
Tax credit %			0.510***	0.254
			(0.172)	(0.203)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Geography	Top MSAs	Non-Top MSAs	Top MSAs	Non-Top MSAs
Observations	1,200	1,200	1,200	1,200
Adjusted R^2	0.936	0.812	0.936	0.812

Table A.4: Angel Tax Credits and Angel Investments: Staggered Adjustment Estimators

This table reports the differences-in-differences estimates using staggered adjustment estimators for the effect of angel tax credit programs on the log number of angel investments in the high-tech sector (IT, biotech, and renewable energy). Columns 1 to 2 use the Sun and Abraham (2021) estimator, and columns 3-4 use the Callaway and Sant'Anna (2021) estimator. Columns 1 and 3 use the unrestricted sample of angel deals from 1988 to 2018, and Columns 2 and 4 use the sample of deals that can be matched to NETS from 1993 to 2016. $\mathbb{1}(ATC)$ is an indicator variable equaling one if a state has an angel tax credit program in that year. We do not consider *Tax Credit %* since these estimators cannot be used for continuous treatment effects. Control variables are defined in equation (1). Each observation is a state-year. All specifications include state and year fixed effects. Standard errors are reported in parentheses and clustered by state. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	Ln(no. of angel investments)			
	Sun and Abraham (2021):		Callaway and Sant'Anna (2021):	
	(1)	(2)	(3)	(4)
$\mathbb{1}$(ATC)	0.120*	0.170**	0.120*	0.158*
	(0.073)	(0.082)	(0.069)	(0.082)
Sample	Unrestricted	NETS-matched	Unrestricted	NETS-matched
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Observations	1,550	1,200	1,550	1,200

Table A.5: Angel Tax Credits and Angel Investments: Alternative Samples

This table provides the differences-in-differences estimates of the effect of angel tax credits on the log number of angel investments in the high-tech sector for various alternatives or subsamples. Panel A focuses on angel investments measured from the following subsamples: post-2000, CVV deals, Form D deals, Form D and Crunchbase deals, and dropping deals in California and Massachusetts. Panel B focuses on all angel deals in the NAICS 51 (Information) and 54 (Professional, Scientific, and Technical Services) sectors. Control variables are defined in equation (1). $\mathbb{1}(ATC)$ is an indicator variable equaling one if a state has an angel tax credit program in that year. *Tax credit %* is a continuous variable equal to the maximum tax credit percentage available in a state-year with an angel tax credit program. Each observation is a state-year. Standard errors are reported in parentheses and clustered by state. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A: ATC and Angel Volume: Subsamples

Sample:	Post-2000 (1)	CVV (2)	FormD (3)	Drop VX&VS (4)	Drop CA&MA (5)
$\mathbb{1}$(ATC)	0.172**	0.168*	0.208***	0.167**	0.169**
	(0.066)	(0.099)	(0.071)	(0.070)	(0.074)
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Observations	800	1,200	1,200	1,200	1,152
Adjusted R^2	0.912	0.888	0.870	0.884	0.888

Panel B: ATC and Angel Volume: NAICS 51 and 54

	(1)	(2)	(3)	(4)
$\mathbb{1}$(ATC)	0.149**	0.155**		
	(0.073)	(0.072)		
Tax credit %			0.462***	0.469***
			(0.162)	(0.161)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Observations	1200	1200	1200	1200
Adjusted R^2	0.892	0.893	0.893	0.894

Table A.6: Angel Tax Credits and Angel Investments: Alternative Specifications

This table reports the differences-in-differences estimates for the effect of angel tax credits on angel investment volume in the high-tech sector (IT, biotech, and renewable energy) using alternative specifications. Panel A demonstrates the robustness of Table 3, Panel A, to specifications without controls. In Panel B, the dependent variable is an indicator variable equaling one if a state-year has at least one angel investment (columns 1-2) or the log number of angel investments in state-years with at least one angel deal (columns 3-4). In Panel C, the dependent variable is the number of angel investments scaled by the lagged number of young firms (age 0-5) (columns 1-2) or the inverse hyperbolic sine of the number of angel investments (columns 3-4). Panel D examines alternative treatment variables. *Ln(agg. TC cap)* is the log of annual tax credit cap in a state-year. *Ln(agg. supported investment)* is the log of the maximum aggregate investment supported by angel tax credits (annual tax credit cap divided by *Tax credit %*). Both variables are set to zero in state-years without angel tax credits. NY and OK do not have a tax credit cap and are dropped from the sample. Panel E evaluates round size and the sample contains all state-years with at least one angel deal for which we observe round amount. The dependent variable is the average log round amount (million \$) for angel financing in a state-year. $\mathbb{1}(ATC)$ is an indicator variable equaling one if a state has an angel tax credit program in that year. *Tax credit %* is a continuous variable equal to the maximum tax credit percentage available in a state-year with an angel tax credit program and zero otherwise. The sample period is 1993 to 2016 unless specified above. Control variables are defined in equation (1). Each observation is a state-year. All specifications include state and year fixed effects. Standard errors are reported in parentheses and clustered by state. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A: No Controls

	Ln(no. of angel investments)			
	(1)	(2)	(3)	(4)
$\mathbb{1}$(ATC)	0.144**		0.174**	
	(0.067)		(0.075)	
Tax credit %		0.327**		0.540***
		(0.147)		(0.170)
Sample	Unrestricted		NETS-matched	
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Observations	1,550	1,550	1,200	1,200
Adjusted R^2	0.950	0.950	0.911	0.912

Panel B. Extensive Margin

	Has Deals		Ln(no. of angel investments)	
	(1)	(2)	(3)	(4)
$\mathbb{1}$(ATC)	0.022	0.028	0.150**	0.136**
	(0.025)	(0.025)	(0.070)	(0.066)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Observations	1,200	1,200	1,084	1,084
Adjusted R^2	0.376	0.377	0.912	0.914

Panel C. Other Transformations of Angel Investments

	Scaled by No. of Young Firms		Inverse Hyperbolic Sine	
	(1)	(2)	(3)	(4)
𝟙(ATC)	0.092**	0.077**	0.212**	0.213**
	(0.039)	(0.034)	(0.089)	(0.087)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Observations	1,200	1,200	1,200	1,200
Adjusted R^2	0.727	0.732	0.899	0.900

Panel D. Alternative Treatments

	Ln(no. of angel investments)			
	(1)	(2)	(3)	(4)
Ln(agg. TC cap)	0.011**	0.012**		
	(0.005)	(0.005)		
Ln(agg. supported investment)			0.011**	0.011**
			(0.004)	(0.004)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Observations	1,152	1,152	1,152	1,152
Adjusted R^2	0.910	0.910	0.910	0.910

Panel E: Angel Round Size

	Ln(round amount)			
	(1)	(2)	(3)	(4)
𝟙(ATC)	0.224**	0.211*		
	(0.106)	(0.107)		
Tax credit %			0.614**	0.554**
			(0.263)	(0.268)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Observations	863	863	863	863
Adjusted R^2	0.225	0.225	0.226	0.225

Table A.7: Angel Tax Credits and Real Effects: Power Based on 5% Significance

This table shows robustness of Table 4 to power calculations based on 5% statistical significance. Panel A (B) reports estimates of the effect of angel tax credits on entry (job creation) by young high-tech firms. All dependent variables are defined the same as those in Table 4. MDE for 80% power is the minimum detectable effect (MDE) for 80% power. Details are in Appendix B for variable definitions and in Appendix D for power calculations. The sample period is 1988 to 2018. $\mathbb{1}(ATC)$ is an indicator equaling one if a state has an angel tax credit program in that year. Control variables are defined in equation (1). Each observation is a state-year. All specifications include state and year fixed effects. Standard errors are reported in parentheses and clustered by state. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A: Firm Entry

	Counts		Rates	
	(1)	(2)	(3)	(4)
$\mathbb{1}$(ATC)	-0.020	-0.003	-0.010	-0.002
	(0.022)	(0.009)	(0.012)	(0.008)
Year and State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Geography	Top MSAs	State	Top MSAs	State
MDE for 80% Power	0.043	0.022	0.025	0.018
Observations	1,550	1,550	1,550	1,550
Adjusted R^2	0.993	0.996	0.496	0.620

Panel B: Job Creation

	Counts		Rates	
	(1)	(2)	(3)	(4)
$\mathbb{1}$(ATC)	0.006	0.018	0.012	0.001
	(0.026)	(0.019)	(0.016)	(0.018)
Year and State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Geography	Top MSAs	State	Top MSAs	State
MDE for 80% Power	0.067	0.048	0.037	0.031
Observations	1,550	1,550	1,550	1,550
Adjusted R^2	0.981	0.983	0.221	0.283

Table A.8: Angel Tax Credits and Real Effects: Without Control Variables

This table shows robustness of Table 4 to specifications without control variables. Panel A (B) reports estimates of the effect of angel tax credits on entry (job creation) by young high-tech firms. All dependent variables are defined the same as those in Table 4. MDE for 80% power is the minimum detectable effect (MDE) for 80% power. Details are in Appendix B for variable definitions and in Appendix D for power calculations. The sample period is 1988 to 2018. $\mathbb{1}(ATC)$ is an indicator equaling one if a state has an angel tax credit program in that year. Control variables are defined in equation (1). Each observation is a state-year. All specifications include state and year fixed effects. Standard errors are reported in parentheses and clustered by state. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A: Firm Entry

	Counts		Rates	
	(1)	(2)	(3)	(4)
$\mathbb{1}$(ATC)	-0.050	-0.031	-0.010	-0.001
	(0.043)	(0.036)	(0.013)	(0.008)
Year and State FE	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Geography	Top MSAs	State	Top MSAs	State
MDE for 80% Power	0.088	0.077	0.022	0.017
Observations	1,550	1,550	1,550	1,550
Adjusted R^2	0.983	0.982	0.495	0.611

Panel B: Job Creation

	Counts		Rates	
	(1)	(2)	(3)	(4)
$\mathbb{1}$(ATC)	-0.018	-0.006	0.013	0.003
	(0.040)	(0.033)	(0.015)	(0.019)
Year and State FE	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Geography	Top MSAs	State	Top MSAs	State
MDE for 80% Power	0.087	0.075	0.030	0.029
Observations	1,550	1,550	1,550	1,550
Adjusted R^2	0.974	0.974	0.221	0.274

Table A.9: Angel Tax Credits and Real Effects: Variation in Tax Credit

This table shows robustness of Table 4 to variation in the tax credit percentage. Panel A (B) reports estimates of the effect of angel tax credits on entry (job creation) by young high-tech firms. All dependent variables are defined the same as those in Table 4. MDE for 80% power is the minimum detectable effect (MDE) for 80% power. Details are in Appendix B for variable definitions and in Appendix D for power calculations. The sample period is 1988 to 2018. *Tax Credit %* is a continuous variable representing the maximum tax credit percentage available, scaled by the average tax credit percentage for ease of interpretation relative to the prior. Control variables are defined in equation (1). Each observation is a state-year. All specifications include state and year fixed effects. Standard errors are reported in parentheses and clustered by state. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A: Firm Entry

	Counts		Rates	
	(1)	(2)	(3)	(4)
Tax Credit %	-0.028	-0.003	-0.010	-0.002
	(0.020)	(0.007)	(0.010)	(0.006)
Year and State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Geography	Top MSAs	State	Top MSAs	State
MDE for 80% Power	0.034	0.017	0.021	0.015
Observations	1,550	1,550	1,550	1,550
Adjusted R^2	0.993	0.996	0.496	0.620

Panel B: Job Creation

	Counts		Rates	
	(1)	(2)	(3)	(4)
Tax Credit %	0.005	0.025	0.015	0.012
	(0.022)	(0.016)	(0.014)	(0.011)
Year and State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Geography	Top MSAs	State	Top MSAs	State
MDE for 80% Power	0.053	0.041	0.030	0.027
Observations	1,550	1,550	1,550	1,550
Adjusted R^2	0.981	0.983	0.221	0.284

Table A.10: Prior Estimates

This table provides the prior estimates for the effect of angel tax credit programs on firm entry and job creation in the BDS. In Panel A, the prior is for the log number of young, high-tech firms in columns 1-2 and firm entry rate in columns 3- 4. Young firms are defined as age 0-5. In Panel B, the prior is for the log number of new jobs created by young, high-tech firms in columns 1-2 and job creation rate by these firms in columns 3-4. The odd columns construct these variables using only data from the top MSAs, which are defined as the largest MSAs by angel volume that account for at least 90% of angel deals in the year before the tax credit implementation. The even columns use statewide data. The power (based on 10% significance) of our test at each prior is reported in the parentheses. Details are in Appendix B for variable definitions and in Appendix E for prior calculations.

Panel A: Firm Entry

	Counts		Rates	
	(1)	(2)	(3)	(4)
Baseline	0.059	0.033	0.042	0.019
Power at Prior	(96.2%)	(99.5%)	(99.0%)	(90.0%)
Translation of Deals to Entry	0.036	0.020	0.025	0.012
Power at Prior	(78.1%)	(82.7)	(86.5%)	(59.0%)
Using Lowest Estimated Coefficient	0.042	0.024	0.030	0.014
Power at Prior	(84.5%)	(93.8%)	(92.4%)	(73.0%)
Geography	Top MSAs	State	Top MSAs	State

Panel B: Job Creation

	Counts		Rates	
	(1)	(2)	(3)	(4)
Baseline	0.083	0.051	0.076	0.045
Power at Prior	(96.1%)	(89.4%)	(99.0%)	(96.4%)
Translation of Deals to Jobs	0.066	0.041	0.060	0.036
Power at Prior	(88.2%)	(78.5%)	(99.0%)	(89.8%)
Using Lowest Estimated Coefficient	0.059	0.037	0.054	0.032
Power at Prior	(81.2%)	(71.7%)	(99.0%)	(88.1%)
Geography	Top MSAs	State	Top MSAs	State

Table A.11: Angel Tax Credits and Real Effects: Large Programs

This table provides the estimates of the effect of angel tax credits on entry (job creation) by young high-tech firms for large programs. Panel A (B) reports estimates on entry (job creation) by young high-tech firms. All dependent variables are defined the same as those in Table 4. The sample is restricted to states with large angel tax credit programs, defined as programs with above median annual budget, as well as states without any program. The sample period is 1988 to 2018. $\mathbb{1}(ATC)$ is an indicator equaling one if a state has an angel tax credit program in that year. Control variables are defined in equation (1). Each observation is a state-year. All specifications include state and year fixed effects. Standard errors are reported in parentheses and clustered by state. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A: Firm Entry

	Counts		Rates	
	(1)	(2)	(3)	(4)
$\mathbb{1}$(ATC)	-0.021	-0.003	-0.015	-0.002
	(0.025)	(0.011)	(0.014)	(0.010)
Year and State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Geography	Top MSAs	State	Top MSAs	State
Observations	992	992	992	992
Adjusted R^2	0.994	0.996	0.459	0.582

Panel B: Job Creation

	Counts		Rates	
	(1)	(2)	(3)	(4)
$\mathbb{1}$(ATC)	-0.002	0.008	-0.001	0.007
	(0.027)	(0.020)	(0.020)	(0.013)
Year and State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Geography	Top MSAs	State	Top MSAs	State
Observations	992	992	992	992
Adjusted R^2	0.986	0.987	0.225	0.300

Table A.12: Angel Tax Credits and Real Effects: Alternative Treatment Variables

This table shows robustness of Table 4 to alternative treatment variables. Panel A (B) reports estimates of the effect of angel tax credits on entry (job creation) by young high-tech firms. All dependent variables are defined the same as those in Table 4. The sample period is 1988 to 2018. *Ln(agg. TC cap)* is the log of annual tax credit cap in a state-year. *Ln(agg. supported investment)* is the log of maximum aggregate investment supported by angel tax credit (i.e., annual tax credit cap divided by tax credit percentage). Both variables are set to zero in state-years without angel tax credits. NY and OK do not have tax credit cap and are dropped from the sample. Control variables are defined in equation (1). Each observation is a state-year. All specifications include state and year fixed effects. Standard errors are reported in parentheses and clustered by state. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A: Firm Entry

	Counts				Rates			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ln(agg. TC cap)	-0.002	-0.000			-0.001	-0.000		
	(0.002)	(0.001)			(0.001)	(0.001)		
Ln(agg. supported investment)			-0.002	-0.000			-0.001	-0.000
			(0.002)	(0.001)			(0.001)	(0.001)
Year and State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geography	Top MSAs	State	Top MSAs	State	Top MSAs	State	Top MSAs	State
Observations	1,488	1,488	1,488	1,488	1,488	1,488	1,488	1,488
Adjusted R^2	0.993	0.995	0.993	0.995	0.495	0.620	0.495	0.620

Panel B: Job Creation

	Counts				Rates			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ln(agg. TC cap)	0.000	0.001			0.000	-0.001		
	(0.002)	(0.001)			(0.001)	(0.001)		
Ln(agg. supported investment)			0.000	0.001			0.000	-0.001
			(0.002)	(0.001)			(0.001)	(0.001)
Year and State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geography	Top MSAs	State	Top MSAs	State	Top MSAs	State	Top MSAs	State
Observations	1,488	1,488	1,488	1,488	1,488	1,488	1,488	1,488
Adjusted R^2	0.980	0.983	0.980	0.983	0.217	0.281	0.217	0.281

Table A.13: Different-State-Matched Employment and Exit Outcomes

This table shows the nearest-neighbor matching estimates for Table 5. Instead of comparing beneficiary firms to failed applicants, we compare them to control firms in nearby states without tax credit programs. We match each beneficiary startup with up to five similar control startups through a nearest neighbor matching procedure. To match with a startup in the treatment group, startup(s) in the control group must be located in a different state but in the same Census division, belong to the same sector, have a similar age (within 2 years), and have a similar amount of previous financing relative to the year of the treatment startup's first tax credit. The dependent variables are defined within two years following the tax credit year, except for Exit. The dependent variables are indicators that are equal to one if the employment is above ten workers, twenty-five workers, the top quartile in the sample, or if the firm experienced a successful exit. We control for sector-by-year and the firm-level controls discussed in the paper. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	Empl > 10 2 Yrs Post-TC (1)	Empl > 25 2 Yrs Post-TC (2)	Empl > 75th Pctile 2 Yrs Post-TC (3)	Exit (4)
$\mathbb{1}$(Tax Credit)	-0.001	-0.014	0.019	-0.017
	(0.016)	(0.009)	(0.015)	(0.015)
Sector-Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	2,511	2,511	2,511	4,115
Adjusted R^2	0.507	0.446	0.420	0.056

Table A.14: Angel Tax Credits and Real Effects: Staggered Adjustment Estimators

This table reports the differences-in-differences estimates using staggered adjustment estimators for the effect of angel tax credit programs on firm entry (Panel A) and job creation (Panel B) from BDS. Columns 1 to 4 use the Sun and Abraham (2021) estimator and columns 5-8 use the Callaway and Sant'Anna (2021) estimator. Odd columns use the top MSA restriction while even columns use state-level data. $\mathbb{1}(ATC)$ is an indicator variable equaling one if a state has an angel tax credit program in that year. Control variables are defined in equation (1). Each observation is a state-year. All specifications include state and year fixed effects. Standard errors are reported in parentheses and clustered by state. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A: Firm Entry

	Sun and Abraham (2021):				Callaway and Sant'Anna (2021):			
	Counts		Rates		Counts		Rates	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}$(ATC)	-0.083**	-0.009	-0.021	0.009	-0.084**	-0.010	-0.023	0.006
	(0.042)	(0.032)	(0.018)	(0.017)	(0.041)	(0.031)	(0.017)	(0.016)
Year and State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geography	Top MSAs	State	Top MSAs	State	Top MSAs	State	Top MSAs	State
Observations	1,550	1,550	1,550	1,550	1,550	1,550	1,550	1,550

Panel B: Job Creation

	Sun and Abraham (2021):				Callaway and Sant'Anna (2021):			
	Counts		Rates		Counts		Rates	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}$(ATC)	-0.062	-0.007	-0.060	0.009	-0.057	-0.001	-0.059	0.006
	(0.052)	(0.045)	(0.038)	(0.017)	(0.052)	(0.025)	(0.037)	(0.016)
Year and State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geography	Top MSAs	State	Top MSAs	State	Top MSAs	State	Top MSAs	State
Observations	1,550	1,550	1,550	1,550	1,550	1,550	1,550	1,550

Table A.15: Angel Tax Credits and Real Effects: Outcomes in Levels Rather than Logs

This table reports differences-in-differences estimates for the effect of angel tax credits on real outcomes in levels rather than in logs. The main BDS outcomes are in Panel A. Key alternative outcomes are in Panel B. The sample period is 1988 to 2018. $\mathbb{1}(ATC)$ is an indicator equaling one if a state has an angel tax credit program in that year. Control variables are defined in equation (1). Each observation is a state-year. All specifications include state and year fixed effects. Confidence intervals are reported in parentheses and clustered by state. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A: Main Outcomes from BDS

	Counts		Rates	
	Entry	Jobs	Entry	Jobs
	(1)	(2)	(3)	(4)
$\mathbb{1}$(ATC)	82.340	-44.246	0.007	-0.003
	(123.571)	(48.588)	(0.006)	(0.003)
Year and State FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	1,550	1,550	1,550	1,550
Adjusted R^2	0.965	0.978	0.182	0.500
Outcome Mean	4095.324	1010.351	0.360	0.272

Panel B: Alternative Outcomes

	LinkedIn-Based Variables					
	New Startups	New HT Startups	New Startup Emps	New HT Startup Emps	New DE Corps	Patent Apps
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}$(ATC)	-42.278	-152.721	-239.029	-71.813	-47.664	-403.656
	(28.716)	(96.171)	(166.466)	(52.708)	(47.041)	(330.632)
Year and State FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	950	950	950	950	1,514	1,500
Adjusted R^2	0.954	0.975	0.954	0.918	0.762	0.835
Outcome Mean	353.099	1771.788	2176.135	455.535	181.359	2627.604

Table A.16: Angel Tax Credits and Real Effects: Non-Top MSAs

This table reports estimates of the effect of angel tax credits on entry (job creation) by young high-tech firms in non-top MSAs. Young firms are defined as age 0-5. Non-Top MSAs are the bottom MSAs by angel volume that together account for less than 10% of state angel deals in the year before tax credit implementation. The dependent variables in columns 1 and 3 are counts (e.g., number of firms) and those in columns 3 and 4 are rates. The sample period is 1988 to 2018. $\mathbb{1}(ATC)$ is an indicator equaling one if a state has an angel tax credit program in that year. Control variables are defined in equation (1). Each observation is a state-year. All specifications include state and year fixed effects. Standard errors are reported in parentheses and clustered by state. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	Firm Entry		Job Creation	
	Counts	Rates	Counts	Rates
	(1)	(2)	(3)	(4)
$\mathbb{1}$(ATC)	0.006	0.000	0.013	-0.023
	(0.021)	(0.003)	(0.052)	(0.015)
Year and State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	1,550	1,550	1,550	1,550
Adjusted R^2	0.965	0.782	0.898	0.645

Table A.17: Angel Tax Credits and Real Effects: Alternative Outcome Variables

This table reports differences-in-differences estimates for the effect of angel tax credits on alternative state-year level outcomes. Panel A reports estimates using outcomes based on Steppingblocks LinkedIn data. The dependent variables are log counts. Panel B reports estimates using outcomes from the Startup Cartography project, high-value acquisitions and IPOs (based on CVV), and patents (based on USPTO data). The sample period is 2000 to 2018 for Panel A and 1988-2018 for Panel B. $\mathbb{1}(ATC)$ is an indicator equaling one if a state has an angel tax credit program in that year. Control variables are defined in equation (1). Each observation is a state-year. All specifications include state and year fixed effects. Confidence intervals are reported in parentheses and clustered by state. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A: LinkedIn-Based Outcomes

	New Startups		New High-Tech Startups		New Startup Emps		New High-Tech Startup Emps	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}$(ATC)	-0.012	-0.014	-0.007	-0.012	-0.007	-0.010	-0.005	-0.011
	(0.020)	(0.016)	(0.027)	(0.023)	(0.023)	(0.018)	(0.032)	(0.026)
Year and State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	950	950	950	950	950	950	950	950
Adjusted R^2	0.994	0.996	0.985	0.987	0.993	0.995	0.982	0.985
Outcome Mean	6.891	6.891	5.170	5.170	7.072	7.072	5.384	5.384

Panel B: Startup Cartography, Exits, Patents

	Log Quality Firms		Log New DE Corps		Any Good Exit		Log Patent Apps	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}$(ATC)	-0.039	-0.007	-0.071	-0.031	-0.031	-0.038	-0.082	-0.033
	(0.069)	(0.052)	(0.116)	(0.100)	(0.034)	(0.034)	(0.054)	(0.045)
Year and State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1,406	1,406	1,423	1,423	1,550	1,550	1,500	1,500
Adjusted R^2	0.945	0.957	0.927	0.936	0.523	0.524	0.977	0.984
Outcome Mean	1.842	1.842	3.927	3.927	0.439	0.439	7.069	7.069

Table A.18: Angel Tax Credits and Real Effects: Expanded Sectors

This table reports differences-in-differences estimates for the effect of angel tax credits on alternative definitions of high-tech sectors. Panel A provides estimates using outcomes based on sectors 31-33, 51, and 54 in BDS. Panel B reports estimates using outcomes based on sectors 22, 31-33, 42, 51, 54, and 62 in BDS. The dependent variables in columns 1, 2, 5, and 6 are counts (e.g., number of firms) and those in columns 3, 4, 7 and 8 are rates. $\mathbb{1}(ATC)$ is an indicator equaling one if a state has an angel tax credit program in that year. Control variables are defined in equation (1). Each observation is a state-year. All specifications include state and year fixed effects. Confidence intervals are reported in parentheses and clustered by state. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A: Sectors 31-33, 51, and 54

	Firm Entry				Job Creation			
	Counts		Rates		Counts		Rates	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}$(ATC)	-0.025	-0.005	-0.009	-0.001	-0.010	0.013	0.014	0.004
	(0.023)	(0.012)	(0.011)	(0.007)	(0.029)	(0.026)	(0.013)	(0.017)
Year and State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geography	MSA	State	MSA	State	MSA	State	MSA	State
Observations	1,550	1,550	1,550	1,550	1,550	1,550	1,550	1,550
Adjusted R^2	0.993	0.996	0.520	0.676	0.983	0.984	0.253	0.307

Panel B: Sectors 22, 31-33, 42, 51, 54, and 62

	Firm Entry				Job Creation			
	Counts		Rates		Counts		Rates	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}$(ATC)	-0.028	-0.012	-0.008	-0.007	0.012	0.033*	0.004	-0.000
	(0.024)	(0.017)	(0.010)	(0.008)	(0.026)	(0.016)	(0.013)	(0.012)
Year and State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geography	MSA	State	MSA	State	MSA	State	MSA	State
Observations	1,550	1,550	1,550	1,550	1,550	1,550	1,550	1,550
Adjusted R^2	0.994	0.995	0.561	0.678	0.988	0.990	0.338	0.435

Table A.19: Form D Filing Rate and Insider Investors

This table examines how the difference in Form D filing rate between beneficiary firms and matched control firms depends on the presence of insider investors. The table builds on Table 7, Panel B and splits the sample by whether a firm has an insider investor. We restrict to the five states (KY, MD, NJ, NM, OH) for which we can identify whether a beneficiary investor is an insider. For control firms ("No Tax Credit"), we identify whether there is at least an insider investor using investor information from AngelList.

	Form D Filing Rate			
	Got Tax Credit	No Tax Credit	Difference	t-Test p-value
All firms	0.783	0.445	0.338	0.000
(obs)	(60)	(281)		
Has insider investor	0.917	0.387	0.530	0.001
(obs)	(12)	(31)		
No insider investor	0.750	0.452	0.298	0.000
(obs)	(48)	(250)		

Table A.20: ATC and Angel Volume: States that Exclude Insiders

This table provides the differences-in-differences estimates of the effect of angel tax credits on the log number of angel investments in the high-tech sector for a subsample that excludes states with programs that allow insider participation. Control variables are defined in equation (1). 1(ATC) is an indicator variable equaling one if a state has an angel tax credit program in that year. Tax credit % is a continuous variable equal to the maximum tax credit percentage available in a state-year with an angel tax credit program. Each observation is a state-year. Standard errors are reported in parentheses and clustered by state. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	Ln(no. of angel investments)			
	(1)	(2)	(3)	(4)
$\mathbb{1}$(ATC)	0.215***	0.221***		
	(0.078)	(0.077)		
Tax credit %			0.643***	0.652***
			(0.231)	(0.231)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Observations	984	984	984	984
Adjusted R^2	0.923	0.923	0.923	0.923

Table A.21: Investor Characteristics and Startup Exit Outcomes

This table reports the relationship between investor characteristics and the exit outcomes of the invested startups based on AngelList data. In columns 1 to 4, the dependent variable is an indicator equal to one if the startup achieved exit through an IPO or M&A. In columns 5 to 8, the dependent variable is an indicator equal to one if the startup achieved exit through an IPO. Independent variables are defined the same as the dependent variables in Panel A of Table 9. The sample period is 2003 to 2017. All specifications include company state-year fixed effects and investor state-year fixed effects. Standard errors are reported in parentheses and clustered by state. ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

	Exit Through IPO or M&A				Exit Through IPO			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
In-State	-0.011**				-0.008***			
	(0.004)				(0.002)			
No Exit		-0.281***				-0.030***		
		(0.020)				(0.006)		
Inexperienced			-0.022***				-0.011***	
			(0.002)				(0.002)	
No Founder Exp.				0.000				-0.002***
				(0.002)				(0.000)
Company State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Investor State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	79,258	79,258	79,258	79,258	79,258	79,258	79,258	79,258
Adjusted R^2	0.115	0.232	0.116	0.115	0.093	0.103	0.094	0.092

Table A.22: Survey Summary Statistics

Panels A and B present the summary statistics for our survey analysis. Panel A shows sample sizes for investors who we emailed and those who responded. Panel B presents the summary statistics for the variables used in our regressions. Panel C examines sample selection. Column 1 examines the roles of ATC usage, availability, and locations in California or Massachusetts. Column 2 additionally examines investor experience measured from AngelList. Standard errors are clustered by state. ***, **, and * denotes significance at the 1%, 5%, and 10% level, respectively.

Panel A: Samples

	Tax credit recipient	AngelList Investors	Total
Emailed	2,508	9,566	12,074
Responded	158	1,226	1,384

Panel B: Summary Statistics for Regression Variables

Variable	N	Mean	Std. dev.
ATC importance	1,361	1.82	1.02
Above median no. of deals since 2018	1,215	0.47	0.50
Has exit (AL)	1,228	0.30	0.46
Has founder experience (AL)	1,228	0.60	0.49
Has invested as insider (AL)	1,228	0.32	0.47
Top school (AL)	1,228	0.31	0.46
Corp Executive	1,250	0.24	0.43
Entrepreneur	1,250	0.37	0.48
Investor	1,250	0.22	0.41
Team importance	1,363	4.75	0.52
Business importance	1,364	4.36	0.73
Location importance	1,360	2.41	1.06
Financial return importance	1,365	3.66	1.11
Add value importance	1,364	3.39	1.16
Valuation importance	1,362	3.48	0.97
Gut reaction importance	1,363	3.94	0.96
Deal terms importance	1,362	3.33	1.06
ATC unimportance Coordination	1,361	0.08	0.27
ATC unimportance Home run	1,361	0.40	0.49
ATC unimportance Non-financial	1,361	0.08	0.26
ATC unimportance Too small	1,361	0.22	0.41
ATC unimportance Cannot use	1,361	0.09	0.28
Tax credit recipient	1,384	0.11	0.32
State has ATC	1,384	0.41	0.49
CA or MA	1,384	0.42	0.49
Above median deal experience (AL)	1,228	0.44	0.50

Panel C: Sample Selection: Who Responds?

	Responded	
	(1)	(2)
Tax credit recipient	-0.074***	0.114*
	(0.012)	(0.063)
State has ATC	0.012	0.010
	(0.013)	(0.012)
CA or MA	-0.001	-0.009
	(0.011)	(0.010)
Above median deal experience (AL)		0.066***
		(0.006)
Has exit (AL)		-0.015*
		(0.009)
Has founder experience (AL)		0.015**
		(0.007)
Has invested as insider (AL)		-0.031***
		(0.006)
Top school (AL)		0.020*
		(0.010)
Observations	12,073	9,572
Adjusted R^2	0.007	0.010

Table A.23: Predictive Regressions

This table examines whether a state's economic, political, fiscal, or entrepreneurial conditions predict the adoption of angel tax credit programs for the sample period 1985 to 2018. The dependent variable is an indicator equal to one ($\mathbb{1}(ATC)$) if a state has adopted an angel tax credit program in that year (columns 1 to 4) or a continuous variable (*Tax credit %*) equal to the maximum tax credit percentage available in state-years with an angel tax credit program and zero otherwise (columns 5 to 8). State-years after a state adopts a program are excluded from the sample. All independent variables are lagged by one year relative to the dependent variable and are defined in Appendix B. Each column includes year fixed effects, while the even-numbered columns also include state fixed effects. Standard errors are reported in parentheses and clustered by state. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	$\mathbb{1}$(ATC)				Tax credit %			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
GSP growth	-0.051	0.056	-0.042	0.047	0.002	0.024	0.013	0.033
	(0.112)	(0.135)	(0.135)	(0.145)	(0.039)	(0.045)	(0.046)	(0.048)
Ln(Income per capita)	-0.003	0.013	-0.002	0.011	-0.000	-0.004	-0.002	-0.004
	(0.027)	(0.066)	(0.027)	(0.066)	(0.010)	(0.022)	(0.011)	(0.022)
Ln(Population)	0.000	-0.118	0.002	-0.126*	-0.001	-0.041	-0.001	-0.045
	(0.005)	(0.072)	(0.008)	(0.075)	(0.002)	(0.026)	(0.003)	(0.028)
Unemployment rate	-0.002	0.005	-0.001	0.006	-0.001	0.001	-0.001	0.001
	(0.003)	(0.005)	(0.003)	(0.005)	(0.001)	(0.002)	(0.001)	(0.002)
Democratic control	0.002	-0.008	0.001	-0.008	-0.001	-0.006	-0.001	-0.006
	(0.010)	(0.013)	(0.010)	(0.013)	(0.003)	(0.005)	(0.003)	(0.005)
Republican control	-0.009	-0.016	-0.009	-0.015	-0.003	-0.005	-0.003	-0.005
	(0.009)	(0.014)	(0.010)	(0.014)	(0.003)	(0.005)	(0.003)	(0.005)
Revenue/GSP	-0.133	-0.171	-0.129	-0.188	-0.049	-0.060	-0.040	-0.060
	(0.222)	(0.275)	(0.227)	(0.273)	(0.086)	(0.104)	(0.088)	(0.105)
Expenditure/GSP	0.131	-0.355	0.085	-0.273	0.064	-0.164	0.055	-0.140
	(0.276)	(0.440)	(0.281)	(0.461)	(0.098)	(0.151)	(0.099)	(0.158)
Debt/GSP	-0.023	0.480	-0.010	0.460	-0.028	0.132	-0.035	0.126
	(0.099)	(0.299)	(0.101)	(0.319)	(0.032)	(0.101)	(0.033)	(0.108)
Has income tax	0.032**	0.032	0.027	0.036	0.011**	0.006	0.009*	0.008
	(0.016)	(0.035)	(0.016)	(0.035)	(0.005)	(0.012)	(0.005)	(0.012)
Max income tax	-0.001	-0.016**	-0.001	-0.015**	-0.000	-0.005**	-0.000	-0.005*
	(0.003)	(0.007)	(0.003)	(0.007)	(0.001)	(0.002)	(0.001)	(0.003)
Capital gain tax	0.000	0.003	0.001	0.003	-0.000	0.001	-0.000	0.001
	(0.003)	(0.005)	(0.003)	(0.005)	(0.001)	(0.002)	(0.001)	(0.002)
Neighbor ATC	0.015	0.012	0.015	0.011	0.004	0.004	0.005	0.004
	(0.013)	(0.015)	(0.013)	(0.015)	(0.005)	(0.005)	(0.004)	(0.005)
Establishment entry rate			-0.016	0.329			0.019	0.112
			(0.227)	(0.345)			(0.079)	(0.112)
Establishment exit rate			-0.247	-0.292			-0.112	-0.083
			(0.224)	(0.385)			(0.083)	(0.144)
Net job creation rate			-0.034	-0.066			-0.062	-0.080
			(0.242)	(0.273)			(0.086)	(0.098)
Venture capital volume			-0.001	0.004			0.000	0.002
			(0.004)	(0.005)			(0.001)	(0.002)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1,343	1,343	1,343	1,343	1,343	1,343	1,343	1,343
Adjusted R^2	0.022	0.038	0.02	0.036	0.017	0.04	0.015	0.039

Table A.24: Summary Statistics for Additional State-Year- and Firm-Level Variables

This table presents further summary statistics for variables used in the paper. Panel A presents summary statistics at the state-year level for our control variables and the other real aggregate outcomes examined in Table A.17. Panel B presents firm-level summary statistics for startup ex-ante characteristics in the NETS-matched sample, as well as total early-stage financing examined in Panel B of Table 6. All variables are defined in Appendix B.

Panel A: Additional State-Year-Level Summary Statistics

Variable	N	Mean	Std. dev.	p5	p50	p95
	Controls					
GSP growth	1,550	1.05	0.03	0.99	1.05	1.10
Ln(Income per capita)	1,550	10.29	0.38	9.65	10.33	10.86
Ln(Population)	1,550	15.07	1.01	13.36	15.18	16.76
Max income tax rate	1,550	5.19	3.03	0.00	5.89	9.28
Ln(no. of young HT estab.)	1,550	8.27	1.06	6.55	8.31	10.13
	Other real outcomes					
Linkedin_new startups	950	1,772	2,420	197	975	6,854
Linkedin_new HT startups	950	353	543	27	189	1,307
Linkedin_new startup employment	950	2,176	3,086	224	1,200	8,458
Linkedin_new HT startup employment	950	456	748	32	229	1,621
Startup Quality	1,406	12.6	25.2	0.2	4.9	52.0
New DE corps	1,423	175	405	2	45	645
Any good exit	1,700	0.44	0.50	0.00	0.00	1.00
Patent applications	1,650	2,487	4,606	74	1,033	7,931

Panel B: Firm-Level Summary Statistics

Variable	N	Mean	Std. dev.	p5	p50	p95
	Angel-backed firms					
Employment	17,444	32.18	190.00	1.00	10.00	101.00
Employment growth	16,442	1.11	0.58	1.00	1.00	1.59
Fraction of serial entrepreneurs	25,460	0.09	0.19	0.00	0.00	0.50
Age at angel round	29,143	2.84	4.17	0.00	1.00	11.00
	All firms with early-stage financing					
Total early-stage financing	38,487	14.37	798.43	0.05	2.00	26.50

B Appendix: Variable Definitions

Variable Name	Definition
$\mathbb{1}(ATC_{st})$	Indicator variable equaling one if a state has an angel tax credit program in that year.
$Tax\,credit\,\%_{st}$	Continuous variable equal to the maximum tax credit percentage available for state-years with an angel tax credit program and zero otherwise.
Ln(agg. TC cap)	Log of annual tax credit cap for state-years with an angel tax credit program,. The variable is set to zero for state-years without a program.
Ln(agg. supported investment)	Log of annual tax credit cap divided by tax credit percentage for state-years with an angel tax credit program. The variable is set to zero for state-years without a program.
Entry by young HT firms – statewide (top MSAs)	Number of young (age 0-5) high-tech firms in a state-year (in top MSAs for a state-year). Top MSAs are the largest MSAs in a state that together account for at least 90% of angel volume in the year before ATC introduction (in year 2005 for states without ATC). Source: BDS.
Jobs created by young HT firms – statewide (top MSAs)	Number of new jobs created by young (age 0-5), high-tech firms in a state-year (in top MSAs for a state-year). Source: BDS.
Entry rate of young HT firms – statewide (top MSAs)	Number of young (age 0-5) high-tech firms in a state-year (in top MSAs for a state-year) divided by the average total number of young high-tech firms in that state (in top MSAs of that state) in the current and the previous year. Source: BDS.
Job creation rate by young HT firms – statewide (top MSAs)	Number of new jobs created by young (age 0-5) high-tech firms in a state-year (in top MSAs of a state-year) divided by the average total number of jobs created by young high-tech firms in that state (in top MSAs of that state) in the current and the previous year. Source: BDS.
Number of angel investments (unrestricted sample)	Total number of angel deals in a state-year in the CVV and Form D data.
Number of angel investments (NETS-matched sample)	Total number of angel deals in a state-year in the CVV and Form D data that can be matched to firms in NETS based on firm name and location.
Aggregate early-stage financing amount	Aggregate early-stage financing amount in a state-year (in millions). Source: CVV and Form D.
Aggregate non-angel financing amount	Aggregate non-angel early-stage financing amount in a state-year (in millions). Source: CVV and Form D.
Aggregate angel financing amount	Aggregate angel financing amount in a state-year (in millions). Source: CVV and Form D.

Variable Name	Definition
Angel share among early-stage financing	Aggregate angel financing amount as a fraction of all early-stage financing amount in a state-year (in millions). Source: CVV and Form D.
Early-stage financing amount	Total early-stage financing amount received by a firm (in millions). Source: CVV and Form D.
Pre-investment employment	Number of employees in the year prior to receiving angel investment. Source: Non-imputed NETS.
Pre-investment employment growth	The percentage change in firm employment from year t-2 to t-1. Source: Non-imputed NETS.
Fraction of serial entrepreneurs	Fraction of founding team members who have prior entrepreneurship experience at the time of angel investment. Source: CVV.
GSP growth	Gross State Product (GSP) growth rate at the state-year level. Source: BEA.
Log income per capita	Log of income per capita at the state-year level. Source: BEA.
Log population	Log of population at the state-year level. Source: BEA.
Max income tax rate	Maximum state personal income tax rate at the state-year level. Source: NBER.
Unemployment rate	State unemployment rate in a given year in percentage points. Source: BEA.
Log young HT establishments	Log number of young (age 0-5) high-tech establishments at the state-year level. Source: BDS.
Democratic control	Indicator variable for whether a state (both the legislative and executive branches) is controlled by Democrats. Source: NCSL.
Republication control	Indicator variable for whether a state (both the legislative and executive branches) is controlled by Republicans. Source: NCSL.
Revenue/GSP	Ratio of revenue to Gross State Product at the state-year level. Source: Annual Survey of State and Local Government Finances.
Expenditure/GSP	Ratio of expenditure to Gross State Product at the state-year level. Source: Annual Survey of State and Local Government Finances.
Debt/GSP	Ratio of debt to Gross State Product at the state-year level. Source: Annual Survey of State and Local Government Finances.
Has income tax	Indicator variable equal to one if a state has personal income tax in a given year. Source: NBER.
Capital gains tax rate	State long-term capital gains tax rate. Source: NBER.
Neighbor ATC	Indicator variable equaling one if a state has a least one neighboring state with an active angel tax credit program.
Establishment entry rate	Number of new establishments divided by all establishments averaged between the current and previous year. Source: BDS
Establishment exit rate	Number of establishments that exit the next year divided by all establishments averaged between the current and previous year. Source: BDS
Net job creation rate	Number of net jobs created (jobs created minus jobs destroyed) divided by all jobs averaged between the current and previous year. Source: BDS

Variable Name	Definition
Venture capital volume	Log of aggregate VC investment amount (in millions) in a state-year. Source: VentureXpert
$\mathbb{1}(Tax\,Credit_{it})$	Indicator variable for startup i having an investor receive a tax credit in year t. Source: State programs.
Raised VC	Indicator variable for whether a firm received any VC financing within two years after its investors received angel tax credit. Source: State programs and CVV.
Exit	Indicator variable equaling one if a startup has an IPO or high-valued M&A, defined as the sale price being at least 1.25 times the total invested capital. Source: State programs and CVV.
Emp. >25 2 yrs (3 yrs) post-TC	Indicator variable for whether a firm had more than 25 employees within two years (three years) after its investors received angel tax credit. Source: State programs and Steppingblock Linkedin data.
Emp. > p75 2 yrs (3 yrs) post-TC	Indicator variable for whether a firm's employment count was above the 75th percentile within two years (three years) after its investors received angel tax credit. Source: State programs and Steppingblock Linkedin data.
Emp. >25 in TC yr	Indicator variable for whether a firm had more than 25 employees in the year its investors received angel tax credit. Source: State programs and Steppingblock Linkedin data.
Emp. >p75 in TC yr	Indicator variable for whether a firm's employment count was above the 75th percentile within our sample in the year its investors received angel tax credit. Source: State programs and Steppingblock Linkedin data.
Finance pre-TC	Indicator variable for whether a firm received any other external financing before its investors received angel tax credit. Source: CVV
In-state	An indicator equal to one if an investor is located in a different state than the investment company's state. Source: AngelList.
Inexperienced	An indicator equal to one if an investor has five years or less of deal experience as of the time of the focal investment. Source: AngelList.
Had no exit	An indicator equal to one if an investor has no prior successful exit as of the time of the focal investment. Source: AngelList.
No founder experience	An indicator equal to one if an investor has no prior founder experience as of the time of the focal investment. Source: AngelList.
Number of in-state investors	Number of investors investing in same-state startups in each startup state-year. Source: AngelList.
Number of out-of-state investors	Number of out-of-state investors in each startup state-year. Source: AngelList.
Number of inexperienced investors	Number of investors with less than five years of investment experience in each startup state-year. Source: AngelList.
Number of experienced investors	Number of investors with more than five years of investment experience in each startup state-year. Source: AngelList.

Variable Name	Definition
Number of investors with no exits	Number of investors with no prior successful exit in each startup state-year. Source: AngelList.
Number of investors with exits	Number of investors with prior successful exits in each startup state-year. Source: AngelList.
Number of investors with no founder exp.	Number of investors with no prior founder experience in each startup state-year. Source: AngelList.
Number of investors with founder exp.	Number of investors with prior founder experience in each startup state-year. Source: AngelList.
ATC importance	An index variable that takes values of 1 to 5 (1 being "not at all important" and 5 being "extremely important") on the perceived importance of angel tax credits for investment decision. Source: Survey.
Above median no. of deals since 2018	Indicator variable equaling one if the number of self-reported deals made by the investor since 2018 is above the median in our sample. Source: Survey.
Has exit (AL)	Indicator variable equaling one if the investor has had at least one exit (IPO or M&A) in the past. Source: Survey matched to AngelList.
Has founder experience (AL)	Indicator variable equaling one if the investor has prior founder experience. Source: Survey matched to AngelList.
Has invested as insider (AL)	Indicator variable equaling one if the investor has invested in a startup as an insider. Source: Survey matched to AngelList.
Top school (AL)	Indicator variable equaling one if the investor holds a degree from one of the Wall Street Journal Top 50 Universities or Wall Street Journal Top 50 MBA Programs. Source: Survey matched to AngelList.
Corp executive	Respondent's self-identified primary profession is a corporate executive. Source: Survey.
Entrepreneur	Respondent's self-identified primary profession is an entrepreneur. Source: Survey.
Investor	Respondent's self-identified primary profession is an investor. Source: Survey.
Team importance	An index variable that takes values of 1 to 5 (1 being "not at all important" and 5 being "extremely important") on the perceived importance of startup's management team for investment decision. Source: Survey.
Business importance	An index variable that takes values of 1 to 5 (1 being "not at all important" and 5 being "extremely important") on the perceived importance of business model for investment decision. Source: Survey.
Location importance	An index variable that takes values of 1 to 5 (1 being "not at all important" and 5 being "extremely important") on the perceived importance of startup's location for investment decision. Source: Survey.
Financial return importance	An index variable that takes values of 1 to 5 (1 being "not at all important" and 5 being "extremely important") on the perceived importance of expected financial return for investment decision. Source: Survey.

Variable Name	Definition
Add value importance	An index variable that takes values of 1 to 5 (1 being "not at all important" and 5 being "extremely important") on the perceived importance of the investor's ability to add value to the startup for investment decision. Source: Survey.
Valuation importance	An index variable that takes values of 1 to 5 (1 being "not at all important" and 5 being "extremely important") on the perceived importance of valuation for investment decision. Source: Survey.
Gut reaction importance	An index variable that takes values of 1 to 5 (1 being "not at all important" and 5 being "extremely important") on the perceived importance of gut reaction for investment decision. Source: Survey.
Deal terms importance	An index variable that takes values of 1 to 5 (1 being "not at all important" and 5 being "extremely important") on the perceived importance of deal terms for investment decision. Source: Survey.
ATC unimportance Home Run	The respondent believed angel tax credit is unimportant because the investor invests based on whether the startup has the potential to be a "Home Run" or not. Source: Survey.
ATC unimportance Coordination	The respondent believed angel tax credit is unimportant because it is too difficult to coordinate certification with the startup. Source: Survey.
ATC unimportance Non-financial	The respondent believed angel tax credit is unimportant because the investor invests for non-financial reasons (personal, philanthropic, social, etc.). Source: Survey.
ATC unimportance Too small	The respondent believed angel tax credit is unimportant because the investor thinks tax credits are too small to make a difference. Source: Survey.
ATC unimportance Cannot use	The respondent believed angel tax credit is unimportant because the investor cannot take advantage of the tax credit (e.g. no state income tax liabilities). Source: Survey.
Tax credit recipient	Indicator for whether the respondent has received state angel tax credit in the past. Source: Survey and state programs.
State has ATC	Indicator for whether the respondent's state ever had an angel tax credit program. Source: Survey.
CA or MA	Indicator for whether the respondent resides in California or Massachusetts. Source: Survey.
Above median deal experience (AL)	Indicator variable equaling one if the number of deals in AngelList data made by the investor is above the median in our sample. Source: Survey.

Variable Name	Definition
Program flexibility	An index ranging from 0 to 17 and is constructed based on the restrictions in Table 1. For each non-binary restriction, we rank programs from least to most strict and assign the highest rank to programs without this restriction. These rank values are then normalized to the unit interval by dividing all values by the maximum value. We also construct indicator variables for programs that do not exclude insider investors and for each of the non-refundable, non-transferable, and no carry forward restrictions. To form the Program flexibility index, we sum these 17 variables and then standardize the index by subtracting its mean and dividing by its standard deviation prior to interacting it with our treatment variables.
VC supply	State-year level aggregate venture capital investment amount (excluding angel and seed rounds identified in our main sample) scaled by the total number of young firms (of age 0-5) in that state-year. This variable is standardized by subtracting its mean and dividing by its standard deviation. Source: VentureXpert and BDS.
New (high-tech) startups (LinkedIn)	Number of new startups (in high-tech sectors) created in a state-year based on aggregate LinkedIn data. Source: Steppingblocks.
New (high-tech) startup employees (LinkedIn)	Number of new employees hired by startups (in high-tech sectors) in a state-year based on aggregate LinkedIn data. Source: Steppingblocks.
Quality firms	Log of one plus the number of high-potential firms founded in each state-year, where high potential is predicted by firm characteristics at founding. This corresponds to the Regional Entrepreneurship Cohort Potential Index (RECPI) in Fazio, Guzman, and Stern (2019). Source: Startup Cartography Project.
New DE corps	Number of Delaware incorporated new businesses in a state-year. Source: Startup Cartography Project.
Any good exit	Dummy equal to one if the state-year has any angel-backed firm that later had a successful exit, defined as an IPO or high-valued M&A (at least 1.25 times the total invested capital). Source: CVV.
Patent applications	Log of one plus state-year count of patent applications for eventually granted patents. Source: USPTO.

C Appendix: Predictors of Angel Tax Credit Program Implementation

Angel tax credit programs have often been touted as "relatively simple and cost-effective for states" (Kousky and Tuomi (2015)) and proponents argue that they promote job creation, innovation, and economic growth.[54] In light of this, states may introduce angel tax credit programs in times of local economic stagnation, which could pose a threat to our identification strategy. We assess whether economic, political, fiscal, and entrepreneurial factors explain the introduction of angel tax credit programs using a predictive regression. The outcome, $\mathbb{1}(ATC)$, is an indicator variable equaling one if a state introduces an angel tax credit program in a given year. We include year fixed effects and omit the years after a program starts. Appendix B defines the state-level variables included in each specification.

Table A.23 presents the results. In column 1, we find that lagged state economic, political, and fiscal measures do not significantly predict the introduction of angel tax credit programs, except for the state income tax indicator. Column 3 incorporates entrepreneurship variables, which include establishment entry and exit rates, net job creation rate, and venture capital volume. We find that these variables do not have significant predictive power. When we include state fixed effects (even columns), there is an economically small relation between the maximum state personal income tax rate and $\mathbb{1}(ATC)$. We obtain similar estimates when we use *Tax credit %* as an outcome (columns 5 to 8). Overall, state economic, political, fiscal, and entrepreneurial conditions do not seem to drive the passage of angel tax credit programs.

The lack of predictability is consistent with the presence of considerable frictions in the passage and implementation of these programs. Several states passed legislation for angel tax credits after years of failed initiatives and amid persistent lobbying efforts.[55] Some states discussed introducing these programs, but never proposed a law (e.g., Idaho and Montana). Other state legislatures proposed bills, but did not pass them (e.g., Mississippi and Pennsylvania). Even if a state legislature passed a program, several states failed to implement the program due to lack of funding or resistance after its passage (e.g., Delaware, Massachusetts, Michigan, and Missouri).[56]

D Appendix: Power Analysis of Aggregate Real Effects

In this section, we discuss our simulation method to estimate the power for our real effects analysis. The power of a test is the probability of rejecting the null hypothesis when the null is false (i.e., one minus the probability of Type II error). In the context of our analysis, calculating the power is especially important because of the lack of statistical significance for

[54] Tuomi and Boxer (2015) conduct case studies of two angel tax credit programs in the U.S. (Maryland and Wisconsin) and find suggestive evidence that these programs generate benefits that outweigh the costs.

[55] Local businesses and trade associations advocated for angel investor tax credits in Kentucky for many years, which were eventually adopted in 2014 (Campbell (2014)). In New Jersey, Governor Chris Christie signed legislation for angel investor tax credits in 2013, despite vetoing the bill two years earlier (Linhorst (2013)).

[56] For example, the Missouri House of Representatives passed legislation in 2014, but it did not advance because of a controversial amendment barring companies that do stem cell research (Moxley (2014)).

the estimated effect of ATC on real outcomes. A statistical null effect could mean that the effect is too small to be detected by a particular statistical model. Therefore, we interpret a null effect together with the minimum detectable effect (MDE), which is the smallest effect size that could be reasonably rejected by the model. Furthermore, the MDE can be compared with the prior for the expected effect size of the policy to evaluate our ability to detect real effects.

We use a simulation approach to calculate the power of our baseline model, following Black et al. (2019).[57] Recall that that the baseline model is a staggered differences-in-differences design employing the following specification:

$$Y_{st} = \alpha_s + \alpha_t + \beta \cdot \mathbb{1}(ATC_{st}) + \gamma' \cdot X_{s,t-1} + \varepsilon_{st}, \tag{4}$$

The simulation method estimates this model many times to generate a probability of detecting a statistically significant effect of size M when this effect has been artificially imposed on the data. (The effect size of M means that the policy increases the outcome by $M\%$.)

Specifically, our estimation includes the following steps:

1. Choose an effect size of M.
2. For each M, we conduct 1,000 simulations:
    (a) For each simulation, we randomly assign a fictitious treatment variable $\mathbb{1}(ATC^*_{st})$ that has the same distribution as our actual treatment $\mathbb{1}(ATC_{st})$. In particular, we maintain the duration for each of the 36 programs, but we randomly assign them to states with random starting years. Because the power depends crucially on the amount of variation in the treatment, matching the number and duration of treatments will ensure that our simulation does not impose more variation than in our actual data and thus overestimate the power.
    (b) Based on the fictitious treatment, we impose a treatment effect of M in the data.[58]

[57] The key challenge with estimating the power of a staggered differences-in-differences model is that it is hard to specify a closed-form formula for the power without strong assumptions. For instance, Burlig et al. (2020) develop a closed-form formula to estimate the power in a simple differences-in-differences setting. However, their model is characterized by several features that do not fit our setting. First, they assume that the treatment occurs at the same time for all treated units. In our setting, the treatment is staggered since different states introduce the tax credit at different times. Second, their model assumes that the treatment happens only once, and that it does not reverse. In our setting, some states terminated their tax credit programs and, in a few cases, reintroduced them. Third, the model does not allow for controls. While the addition of controls does not substantially impact our analysis, the simulated approach allows us to examine the change in power when they are included. If we make several simplifying assumptions to apply the power formula in Burlig et al. (2020) to our setting, we find that the estimates are generally similar to those from the simulated method.

[58] Since our main outcomes are log transformed count and rate variables, our treatment effect can be interpreted as a percentage change in the outcome when the policy is introduced. In particular, we induce the treatment effect by adding an M increase to the observed outcome in the years of the simulated policy. As such, an outcome variable $ln(Y_{st})$ will take the value of $ln(Y_{st}(1 + M))$ when $\mathbb{1}(ATC^*_{st}) = 1$ and the original

(c) We use the new data with the imposed treatment effect and fictitious treatment to estimate our main regression in equation (4). We store the estimates from this simulated model.

3. For each M, we calculate the fraction of the 1,000 simulations that detect a positive effect with at least a particular statistical significance. This metric is the estimated power of our model for an effect size M, as it directly tells us the probability of detecting a statistically significant effect when the policy actually has an effect of M.

4. We repeat this procedure for a wide set of plausible treatment effects M. In particular, we consider each M between 0.1% and 10% in increments of 0.1%.

We also use a similar procedure to estimate the power for our continuous differences-in-differences model, which replaces $\mathbb{1}(ATC_{st})$ with $Tax\,credit\,\%_{scaled}$. To facilitate interpretation and comparison with the discrete differences-in-differences model, we scale $Tax\,credit\,\%$ by its sample average, 0.355. Thus, M represents the effect size of an ATC program with a 35.5% tax credit percentage. Other estimation steps are the same as above, except in 2(a), we randomly assign the 36 programs retaining their tax credit percentage in addition to the program duration.

There are several advantages to the simulation method. First, relative to the closed-form power analysis that can only be derived for a few simple empirical models, the simulated method can flexibly accommodate more complex models and calculate the power without simplifying assumptions. In our case, the simulation allows for staggered treatments, the inclusion of controls, and a generalized differences-in-differences with continuous treatment. Second, because the simulated method uses actual data, it can account for the underlying data structure, including serial correlation within a treatment unit.

Third, the simulation method can be easily extended to compute the *joint power* of our tests across multiple outcomes. The joint power is the probability that an effect is detected for at least one outcome, given that the policy affected multiple outcomes. Examining the joint power is useful because it reduces the probability of a false negative when we expect the policy to impact multiple dimensions of entrepreneurship. In fact, the joint power is always (weakly) stronger than the power of testing an individual outcome because each additional outcome brings new information about the effectiveness of the policy. Since the simulation method uses actual data, it has the advantage of not requiring assumptions about the ex-ante correlation between outcomes. We compute our joint power as the likelihood that we fail to find a significant effect across both firm entry and job creation, given that ATCs have an effect equal to our prior for each outcome (see Table A.10 for prior effects). Using this approach, we find that our joint power across firm entry count and job creation count is 99.6%, and the

value of $ln(Y_{st})$ when $\mathbb{1}(ATC^{*}_{st}) = 0$. Following the simulation, we check that the estimated treatment effect based on this fictitious treatment is indeed M. In other words, if we induce a treatment effect of the policy $M = 3\%$, our average estimate of the treatment effect across all simulations is almost always 3%.

joint power across firm entry rate and job creation rate is 96.8%. The joint power across all these four outcomes is 99.7%.[59]

Lastly, the simulation method allows for a validity check by comparing our estimated effect of the simulated policy β_M with the imposed effect M. Although each realization of β_M could be different due to randomness in the data, they should on average converge to M if the simulation is correctly specified. Figure A.4 plots the average β_M across 1,000 simulations against M for the four outcomes examined in Table 4. The scatter plot lies almost exactly on the 45 degree line, indicating that our simulation is correctly specified.

E Appendix: Calculating the Prior Mean Effect

This section details how we calculate the expected effect of ATC on real outcomes based on the assumption that the increase in angel investments in Section 4.2 translates into new entrepreneurial activity (i.e., no crowding out as in Section 5.1). We refer to this expected effect as the prior and use it to provide a benchmark when assessing the statistical power of our results (see Appendix D). We start by describing our baseline approach, and then provide a few variations from this baseline to demonstrate its robustness to different assumptions.

E.1 Prior for Count Variables

We first explain the prior calculation for the number of new high-tech firms in a state, which is denoted as E. Since our analysis of angel investments only includes a firm's first deal, we assume that the estimated effect on angel investments of 18% (Table 3, Panel A, column 1) corresponds to an equal number of new firms.[60] We multiply this estimate by the number of angel deals (A) in the year before ATC was implemented in treated states.[61] This is the expected number of new firms created as a result of ATCs if the estimated effect on angel deals transmitted one-for-one to new firm creation (i.e., no crowding out). We then divide by the number of new, high-tech firms in the year before the policy, E_{t-1}. Finally, we average across states with ATCs (s). The formula for the prior effect on E induced by the policy in year t is:

$$p\left(\mu_E\right) = \frac{1}{S}\sum_{s=1}^{S}\frac{0.18\bar{A}_{t-1}}{E_{s,t-1}}. \tag{5}$$

[59]These are based on baseline prior effects for statewide outcomes. The joint power is similar for top-MSA outcomes.

[60]We abstract away from the difference between establishments and firms since 99% of young, high-tech firms are single-establishment firms.

[61]Since 18% is the average effect of ATCs on angel investment, we use the average number of angel investments in the year before ATC was implemented in treated states. The results are not sensitive to using two or three years before ATC implementation.

We adjust this approach to create a prior consistent with the alternative continuous treatment variable, which is the maximum tax credit percentage available in a state-year with an ATC program ($Tax\, credit\,\%$). We replace 0.18 in equation (5) with the estimated effect based on the continuous treatment variable (Table 3, Panel A, column 2). Since the average ($Tax\, credit\,\%$) is 35.5%, the estimate translates to a 13% effect on the number of new firms ($\exp(0.348 \cdot 0.355) - 1 = 0.13$) for an average tax credit percentage. For ease of comparing the prior with the coefficients in Table 4, we scale $Tax\, credit\,\%$ by the average tax credit.

To calculate the prior for new jobs (J) created by firms E, we use the median number of jobs at angel-backed firms from Table A.24, Panel B (i.e., 10 jobs per firm) to compute the number of jobs created by ATC-induced firms. The prior formula then becomes:

$$p\left(\mu_J\right) = \frac{1}{S} \sum_{s=1}^{S} \frac{10 \cdot 0.18 \bar{A}_{t-1}}{J_{s,t-1}}. \tag{6}$$

Finally, we adapt both equations for top MSAs by re-estimating A, E, and J for top MSAs within each state.

E.2 Prior for Rate Variables

In addition to using the counts of firm entry and job creation, we also present the effects of the programs on the rates of these variables. One advantage of using rates is to make policy effects more comparable across states with very different levels of entrepreneurial activity (e.g., North Dakota vs. New York). Since the New York economy is more dynamic, the natural fluctuations in count variables will be much higher in New York than in North Dakota in the absence of an ATC program. Importantly, this will not be fully addressed by state fixed effects or size controls. This is one reason the macroeconomic literature uses, for example, GDP growth rates across areas rather than GDP changes.[63]

Calculating the prior for rates requires us to modify the formula. In particular, we need to account for the fact that ATCs affect both the numerator of the rate (i.e., flow count such as new jobs created by young high-tech firms) and the denominator (i.e., stock count such as total jobs by young high-tech firms). We also account for natural attrition of angel-backed firms created by ATCs as the programs go on.[64] As above, we focus on the firm entry rate, which can then be easily extended to the job creation rate. First, we define the following objects:

[62]This simple averaging is consistent with the differences-in-differences regressions in Table 3, which treat each state equally rather than weighting them by size.

[63]See Dallas Fed on Growth .

[64]These adjustments push the priors for rates closer to zero, leading to a more conservative prior relative to no denominator or attrition adjustment.

- F_t, S_t, and R_t are the flow count, stock, and rate of firm entry in year t, respectively, in the counterfactual scenario of no ATC. In our context, flow count is the number of new high-tech firms created in a state-year, stock is the total number of young high-tech firms in that state-year, and rate is firm entry rate among young high-tech firms.

- Following Census definition, $R_t = \frac{F_t}{0.5(S_{t-1}+S_t)}$. That is, all rate variables are flow count divided by the stock averaged between year t and year t-1 (Decker et al. (2020)).

- $F_t = A_t + B_t$, where A_t is the number of angel-backed firms created in year t in the absence of ATCs, and B_t is the number of non-angel-backed firms created in year t. For simplicity and based on what we observe in data, we assume that the counterfactual angel share—the fraction of new firms that are angel-backed in the absence of ATCs—is largely stable over time. That is, $\frac{A_1}{A_1+B_2} = \frac{A_2}{A_1+B_2} = ... = \frac{A_n}{A_n+B_n}$.

- δ_h is the survival rate of angel-backed firms from birth to age h. δ_h can be thought of as the "depreciation rates" when accumulating angel-backed firms created by ATCs each year into a stock of young high-tech firms. By definition, $\delta_0 = 1$. We obtain other δ_h values from firm survival rates available from the Bureau of Labor Statistics' (BLS) Business Employment Dynamics

- $p(F_t)$ is the prior for the effect of ATCs on flow count in year t. $p(R_t)$ is the prior for the effect on the rate variable in year t.

Let $R_t|\,(atc = 0)$ denote the counterfactual value of the rate variable when there are no ATCs and $R_t|\,(atc = 0)$ denote the value when there is a program. Based on our estimated 18% effect of ATC on the number of angel deals, we have $A_t|\,(atc = 1) = 1.18 \times A_t|\,(atc = 0)$. For an ATC that lasts n years, it is straightforward to show that:

$$R_1|\,(atc = 0) = \frac{A_1 + B_1}{0.5(S_1 + S_0)};\ R_1|\,(atc = 1) = \frac{1.18A_1 + B_1}{0.5(S_1 + S_0 + 0.18\delta_0 A_1)}$$

$$R_2|\,(atc = 0) = \frac{A_2 + B_2}{0.5(S_2 + S_1)};\ R_2|\,(atc = 1) = \frac{1.18A_2 + B_2}{0.5(S_2 + S_1 + 0.18((\delta_0 + \delta_1)A_1 + \delta_0 A_2))}$$

...

$$R_n|(atc = 0) = \frac{A_n + B_n}{0.5(S_n + S_{n-1})};\ R_n|(atc = 1) = \frac{1.18A_n + B_n}{0.5(S_n + S_{n-1} + 0.18\sum_{t=1}^{n-1}(\delta_{n-t-1} + \delta_{n-t})A_t + 0.18A_n)}.$$

The prior for flow count is:

$$p(F_1) = \frac{F_1|\,(atc = 1)}{F_1|\,(atc = 0)} - 1 = \frac{0.18A_1}{A_1 + B_1}.$$

Based on the assumption of a stable counterfactual angel share, we have $p(F_1) = p(F_2) = ... = p(F_n)$.

The prior for the rate variable in year n is:

$$p(R_n) = \frac{R_n|\,(atc = 1)}{R_n|\,(atc = 0)} - 1 = \frac{(1.18A_n + B_n)(S_n + S_{n-1})}{(S_n + S_{n-1} + 0.18\sum_{t=1}^{n-1}(\delta_{n-t-1} + \delta_{n-t})A_t + 0.18A_n)(A_n + B_n)} - 1$$
$$= (Prior(F_n) + 1) \times \frac{S_n + S_{n-1}}{S_n + S_{n-1} + 0.18\sum_{t=1}^{n-1}(\delta_{n-t-1} + \delta_{n-t})A_t + 0.18A_n} - 1.$$

Therefore, we have the following formula mapping the prior for flow count (which is already computed) to the prior for rates:

$$\frac{p(F_n) + 1}{p(R_n) + 1} = 1 + \frac{0.18\sum_{t=1}^{n-1}(\delta_{n-t-1} + \delta_{n-t})A_t + 0.18A_n}{S_n + S_{n-1}}$$

We compute the prior for each year a program is in place, and then average these priors over the program duration.

We modify the above formula to compute the prior for job creation rate. Specifically, we replace the stock of young high-tech firms, S_n, with the stock of jobs by young high-tech firms, J_n. We also multiply the number of new angel-backed firms by the median number of jobs per angel-backed firm. Hence we have:

$$\frac{p(F_n) + 1}{p(R_n) + 1} = 1 + \frac{10 \cdot (0.18\sum_{t=1}^{n-1}(\delta_{n-t-1} + \delta_{n-t})A_t + 0.18A_n)}{J_n + J_{n-1}}$$

E.3 Alternative Assumptions

We extend our analysis and show that the priors remain reasonably large under alternative assumptions.

We begin by relaxing the assumption that all angel deals translate 1:1 into firm creation to provide a lower bound for our prior. We assume that only angel deals for firms less than or equal to one year old translate 1:1 into new firms since these angel financings occur concurrently with (and hence are pivotal to) startup founding. For existing firms greater than one year old, we assume that angel financing only affects their growth and not entry. In our data, 60.6% of angel deals went to firms less than or equal to one year old. This adjustment decreases the calculated prior for firm entry from 0.033 to 0.02, which is still above the 80% MDE of 0.019. As we show in Table A.10, Panel A, the power for the adjusted prior (row "Translation of Deals to Entry") is at or above 80% for nearly all outcomes.

We similarly adjust our prior for job creation. We continue to assume that angel deals for new firms create jobs through the extensive margin of firm entry. However, angel deals for existing firms create jobs only through the intensive margin of 40% employment growth (Lerner et al. (2018)). Using this information combined with the average employment of new and existing angel firms in our data, we estimate the new increase in jobs, which is about 79.5% of our original estimate.[65] We then use this number to recalculate our priors, which are reported in Panel B of Table A.10 (row "Translation of Deals to Jobs"). For instance, the adjusted prior for statewide job creation count is 0.041, which is just around the 80% power MDE. The adjusted priors for other job creation outcomes are all above the 80% power MDEs.

Last, we consider how our prior estimates would change if we use the smallest estimate for the effect of ATCs on angel investments across our results. There are only two estimates smaller than the baseline coefficient: excluding controls (0.144 in column 1 of Table A.6, Panel A) and adjusting for staggered treatments (0.120 in Table A.14). We provide the adjusted prior estimates in Table A.10 (row "Using the Lowest Estimated Coefficient"). We continue to find that the power for the adjusted priors is above or close to 80% for all outcomes.

While we do not formally quantify these cases, we highlight that there are several implicit assumptions in our model that are likely to lead us to underestimate our priors. First, to the extent that our data does not capture the universe of all angel deals, we are undercounting the numerator in our priors, implying that the true priors are likely to be higher than estimated. Second, the 18% used in the prior calculation is based on the extensive margin result on angel deal count. Incorporating the intensive margin effect on deal amount would likely increase the prior, especially the prior on job creation. Third, there could be an ex-ante effect on entry and job creation through expectations, which are not incorporated into our estimate. That is, knowing that future growth will be constrained due to lack of angel financing, entrepreneurs could be less likely to enter ex ante. A large literature has documented such ex-ante effects with bank financing. For example, Black and Strahan (2002) and Kerr and Nanda (2008) find that U.S. interstate banking deregulations led to a 6% to 11% increase in new firm entry; Core (2020) finds that government subsidies for bank loans in Italy increased new firm entry by 16% in innovative sectors. These effects can be viewed as a lower bound on the ex-ante effect of angel financing on entry, as angel financing tends to happen closer to firms' founding than bank financing.

[65]Specifically, in our data, the median employment for new firms is 10 while it is 12 for existing firms. Using the above parameters, the number of jobs created per angel deal is: 60.6%*10 + 39.4%*12*40% = 7.95, or 79.5% relative to the original assumption of 10 jobs per deal.

F Appendix: Identifying Insiders

In Section 5, we describe how a substantial share of angels using the tax credit are actually insiders of the beneficiary firms. In this appendix, we present some of the methods we have used to identify insiders. As mentioned in the paper, we conduct this analysis in the five states where we observe the identities of tax credit beneficiary companies, the names of investors who were awarded tax credits, and the link between these two pieces of information (Ohio, New Jersey, Maryland, New Mexico, and Kentucky). These five states are reasonably representative of states that employ angel tax credits, including some high-tech clusters (New Jersey and Maryland), as well as rural areas (Kentucky and New Mexico), and the Rust Belt (Ohio). There are 628 unique companies in this group and 3,560 investors.

We identify insiders in three ways. First, we check whether any of the investors are executives in the company using data from LinkedIn. Among investors for whom we observe LinkedIn employment histories, 20% identify as employed at the company they invested in during the time period in which they received the tax credit, of which almost half are the CEO.

Second, we repeat the same procedure using the listed executives in Form D. We can find Form D filings in the year of the tax credit for 186 of the companies, and we matched executive officers from Form D to investors in the tax credit data. A company must list its executive officers and board members in its Form D. We match our companies to SEC Form Ds available on https://disclosurequest.com, which are those after 2010 when the Form Ds are available in HTML (rather than PDF). Of the 628 unique companies, we are able to match 186 firms. We use the Form D filed in the year of the tax credit. There are 407 unique executive officers on these Form Ds, and of them, there are 38 with the same full name as an investor who received a tax credit, and an additional 24 with the same last name as an investor. Of the 186 matched companies, 39 have at least one investor who is an executive or family member of an executive. The share of investors implicated is small, as the companies that match tend to have a large number of investors.

Lastly, we also check for investors who are potential family members of any of the executives. We first identify the 61 companies that have at least three investors with the same last name. For these investors, we search websites to identify if they or a family member were an executive. Based on this process, 61 percent of these 61 companies are identified as having an insider investor.

The methods used are inherently imperfect. However, we think that the errors are likely to be false negatives (i.e., fail to identify an investor as an insider when she is actually an insider) rather than false positives (i.e., incorrectly identify an insider). As a result, we consider our estimates to be a lower bound for the presence of insiders in the beneficiary group. We refer to the paper for more details on the results.

G Angel Investor Survey

Survey Request Email

Re: Angel Investor Finance Professor Research

[redacted]
to Sabrina

Jun 10, 2020, 9:18 PM (11 hours ago)

Done - my pleasure.

On Wed, Jun 10, 2020 at 3:18 PM Sabrina Howell <showell@stern.nyu.edu> wrote:

> [redacted]
>
> I'm Sabrina Howell, a Professor of Finance at the NYU Stern School of Business, and I'm studying factors that are important to angel investors' decisions to invest in startups. This is joint work with professors at Northwestern, Carnegie Mellon, UNC, and UVA.
>
> We would really appreciate it if you could complete this brief survey:
> https://nyu.qualtrics.com/jfe/form/SV_9Ex6zwrbjQXOZFz?Q_DL=eQSLGCUu54q6MA3_9Ex6zwrbjQXOZFz_MLRP_4ON2oKGVA11cCC9&Q_CHL=gl
> I promise it will only take 3 minutes! Your response will be anonymous, and we will only report aggregated results in our research paper.
>
> I hope you and your family are doing OK during these difficult times.
>
> Thanks a lot for your time,
> Sabrina
>
> ________________________________
>
> Sabrina T. Howell
> Assistant Professor of Finance
> NYU Stern School of Business
> Phone: 212-998-0719
> Email: sabrina.howell@nyu.edu
> Website: www.sabrina-howell.com

Complete Survey

Which of the following factors do you consider to be the most important in affecting your decision about **whether or not** to invest in a startup?

	Not at all important (1)	Slightly Important (2)	Moderately important (3)	Very important (4)	Extremely important (5)
Quality of the startup's management team (1)	○	○	○	○	○
Quality of the startup's technology or business model (2)	○	○	○	○	○
Location of the startup (3)	○	○	○	○	○
Expected financial returns (based on NPV/IRR/Multiple) (4)	○	○	○	○	○
My ability to add value to the startup and its alignment with my expertise (5)	○	○	○	○	○
My ability to benefit from a state-level angel investor income tax credit after investing (6)	○	○	○	○	○
Valuation (overall worth of the startup) (7)	○	○	○	○	○
My gut reaction after seeing the business plan or meeting the management (8)	○	○	○	○	○
Terms of the investment (e.g. board control, future participation rights) (9)	○	○	○	○	○

(*Display this question if answer to previous question "My ability to benefit from a state-level angel investor income tax credit after investing" <= 2*)
In the previous question, you rated your ability to benefit from a state-level angel investor income tax credit after investing as relatively unimportant. Why? (Select all that apply)

- ☐ It is too difficult to coordinate certification with the startup. (1)
- ☐ I invest based on whether the startup has the potential to be a "home run" or not. (6)
- ☐ I invest for non-financial reasons (personal, philanthropic, social, etc.). (2)
- ☐ Tax credits are too small to make a difference. (3)
- ☐ I cannot take advantage of the tax credit (e.g. no state income tax liabilities). (4)
- ☐ Other (please describe). (5) ____

Page Break

(*Display this question if answer to previous question "My ability to benefit from a state-level angel investor income tax credit after investing" >= 3*)

In the previous question, you rated your ability to benefit from a state-level angel investor income tax credit after investing as relatively important. Why? (Select all that apply)

- ☐ It helps to certify the startup's high quality. (1)
- ☐ It might make the investment financially viable (i.e., change NPV from negative to positive). (2)
- ☐ I wouldn't calculate the effect on the NPV, but it would make the investment more appealing financially. (3)
- ☐ It would enable me to invest in additional startups. (4)
- ☐ Other (please describe). (5) ____

Have you ever received a state-level angel investor income tax credit? Choose one.

- ◯ No, because the states where I invest do not have angel investor tax credits. (1)
- ◯ No, because I am not aware whether the states where I invest offer angel investor tax credits. (2)
- ◯ No, because making use of angel investor tax credits requires too much coordination or administrative work. (3)
- ◯ Yes. (4)

What is your opinion of state-level angel investor income tax credits? Do you think they attract new investments into startups?

____

____

Page Break

Which of the following best describes your main approach to investing in a startup? Choose one.

- ◯ After conducting financial analysis, I invest when the expected return is above a certain threshold. (1)
- ◯ I focus on whether the startup is likely to experience dramatic growth over the next couple of years. (2)
- ◯ I focus on whether the startup has a strong team, high quality technology, and/or good business model. (3)
- ◯ I invest for non-financial reasons (personal, philanthropic, social). (4)
- ◯ None of the above (please describe). (5) ____

Page Break

To close our survey, we would like to ask you for some background information.

How many investments in startups have you made since January 2018?

____

What is your average investment amount in a startup financing round (a rough estimate is fine)?

____

What is your main profession?

- ○ Corporate Executive (4)
- ○ Doctor (5)
- ○ Entrepreneur (6)
- ○ Lawyer (7)
- ○ Investor (8)
- ○ Other (please describe) (9) ____

Are you a member of an angel investment group?

- ○ Yes (1)
- ○ No (2)

End of Block: Default Question Block

H Appendix: Model Details and Extension

This section offers a simple framework for understanding how angel tax credits affect an investor's extensive margin decision about whether or not to finance a company. We first set up the theoretical environment (Section H.1). Then we discuss the distribution of returns (Section H.2) and derive the optimal response to angel tax credits for investors in this model (Section H.3). In an extension, we also derive predictions for capital gains tax credits, an alternative policy that governments often use that have similar goals as angel tax credit programs (Section H.4). We conclude by comparing the predictions of angel tax credits with capital gains tax credits (Section H.5).

H.1 Model Setup

Consider an investor j who is deciding whether or not to invest an amount I in a startup i. The startup has an after-tax return of r_i, so the investor will receive $I(1 + r_i)$. If the government introduces an investor tax credit along the lines of the angel tax credit programs that we study, the investor will also receive τI, where $\tau \in [0, 1]$ is the tax credit percentage.[66] Then the expected return for the investor is:

$$E(R_i) = E(1 + r_i + \tau). \tag{7}$$

For simplicity, we assume that the investor is risk neutral and makes decisions following a simple hurdle rate rule: she invests only if the expected return is above $(1 + k)$, where $k > 0$. We can interpret k as the investor's cost of capital, or the expected return from alternative investments of similar systematic risk. Given this decision rule, the investor would invest only if:

$$E(1 + r_i + \tau) > 1 + k. \tag{8}$$

We assume that $1 + r_i$ follows a Pareto distribution, with a scale parameter (i.e., minimum) $c_i > 0$, and a shape parameter α_j. The Pareto distribution is frequently used to model startup returns (Othman (2019), Malenko et al. (2020)), which exhibit a heavy-right tail and extreme skewness (Scherer and Harhoff (2000), Kerr et al. (2014), Ewens et al. (2018)). Hall and Woodward (2010) and Kerr et al. (2014) document that most startups fail completely while a few generate enormous returns. Malenko et al. (2020) further show that such skewness is much higher for seed-stage investments than for later-stage ones. Practitioners also embrace the idea that early-stage VC returns follow a power law (Pareto) distribution (Thiel and Masters (2014), Wilson (2015)).[67]

The Pareto distribution has two additional useful properties. First, it is bounded below by c_i, which implies limited liability: the investor cannot lose more money than she invested. Second, modifying the parameter α changes the shape of the distribution, such that a smaller α makes the distribution fatter-tailed. This allows us to characterize different types of investment returns and compare optimal responses by investors. Additionally, we assume that α_j is an investor-specific parameter governing the pool of projects that she can access. In contrast, the parameter c_i is a project-specific parameter observable to the investor: projects with higher c_i have higher returns, conditional on the shape parameter. For now, we only consider the case where $\alpha_j > 1$, i.e., projects with a bounded expected return. We consider the special case of $\alpha_j \leq 1$ in Section H.3.1.

[66] For simplicity, returns are normalized to abstract from the roles of the discount rate and duration differences. This does not affect the qualitative predictions of the model.

[67] The Pareto distribution assumption should not be interpreted in a normative way. In other words, we are not suggesting that investors should make decisions using this distributional assumption. Rather, we model returns using a Pareto distribution because it proxies well the way investors behave in practice.

The expected value of investments from the Pareto distribution is:

$$E(1 + r_i) = \frac{\alpha_j c_i}{\alpha_j - 1} \quad . \tag{9}$$

The shape parameter, α_j, determines the thickness of the tail. As a result, a lower α (as long as $\alpha > 1$) increases both the expected returns and the variance of the returns. We also assume that the discount rate k does not vary with α_j and c_i. This implies that changes in the return distribution parameters reflect changes in idiosyncratic project risk rather than systematic risk.

H.2 Distribution of Investors and Projects

As described above, there is a spectrum of investors defined by α_j. Professional investors have access to projects with higher expected returns and higher uncertainty, which means a lower α_j. Non-professional investors have access to more traditional projects with lower mean and variance, implying a higher α_j.

Within a pool of projects with the same α_j, project quality is defined by the scale parameter c, which follows a uniform distribution $c_i \sim U(0, C]$, $C > 0$, and represents the minimum return. Projects with higher c_i have higher returns on average. Since c_i is known ex-ante when investors make decisions, it is not a random variable. Instead, investors observe a cross-section of projects with various c_i.[68]

The final assumption is the following participation constraint (under the assumption that $\alpha_j > 1$):

$$1 + k \leq \frac{C\alpha_j}{\alpha_j - 1}. \tag{10}$$

This ensures that an investor's cost of capital is not prohibitively high, in which case even the best possible project's return would not exceed it. In other words, this assumption simply ensures that an investor will always have a non-zero probability of investing in a company before observing the quality c_i.[69]

H.3 Effect of Investor Tax Credits on Investment Decisions

We now examine how angel tax credits influence investment decisions. As above, we assume that $\alpha_j > 1$ and consider the complementary case in Section H.3.1.

[68] Our results do not depend on the specific distributional assumption for c_i. An advantage of the uniform distribution is that it allows us to set $c_i > 0$, which is required for the Pareto distribution. It also simplifies the expression for the probability of an investment.

[69] This assumption can be microfounded by assuming that the investor must incur a fixed cost to participate in this market.

It follows from equations (9) and (10) that the investor invests if and only if:

$$E(1+r_i) = \frac{\alpha_j c_i}{\alpha_j - 1} > 1 + k - \tau. \tag{11}$$

That is,

$$c_i > c^* = \left(1 - \frac{1}{\alpha_j}\right)(1 + k - \tau). \tag{12}$$

This means investors will only invest if the project has $c_i > c^*$. Note that c^* is always positive since $k > 0$ and $\tau < 1$. Since $c_i \sim U(0, C]$, we can identify the ex-ante probability P of investing, which is before doing due diligence and observing c_i. Alternatively, P can be thought of as the share of investments that an investor will fund conditional on the pool she has access to, defined by α_j. In particular:

$$P = 1 - \frac{c^*}{C} = 1 - C^{-1}\left(1 - \frac{1}{\alpha_j}\right)(1 + k - \tau). \tag{13}$$

The participation constraint $1 + k \leq \frac{C\alpha_j}{\alpha_j - 1}$ implies that $c^* \leq (1 - \frac{1}{\alpha_j})(1 + k) \leq \frac{C\alpha_j}{\alpha_j - 1}$. Also, because $\alpha_j > 1$ and $\tau < 1$, $c^* > 0$. Hence, $c^* \in (0, C]$ and thus $P \in [0, 1)$. The sensitivity of investment to the tax credit is then:

$$\frac{\partial P}{\partial \tau} = C^{-1}\left(1 - \frac{1}{\alpha_j}\right). \tag{14}$$

Given these relationships, it is straightforward to prove the following:

- **Lemma 1**: *A higher investor tax credit increases the ex-ante probability of investing for all investors with $\alpha_j > 1$.*

  Proof: Because $C > 0$ and $\alpha_j > 1$, $\frac{\partial P}{\partial \tau} = C^{-1}\left(1 - \frac{1}{\alpha_j}\right) > 0$. Hence, the probability of investing strictly increases with the tax credit percentage τ.

- **Lemma 2**: *Conditional on $\alpha_j > 1$, the investment decision is less sensitive to investor tax credits when α_j is smaller (i.e., projects that have a fatter right tail). Since professional investors have smaller α_j, they are less sensitive to investor tax credits than non-professional investors.*

  Proof: We examine the sensitivity of investor tax credits to changes in α_j by taking the partial derivative of $\frac{\partial P}{\partial \tau}$ with respect to α_j: $\frac{\partial^2 P}{\partial \tau \partial \alpha_j} = \frac{1}{C\alpha_j^2} > 0$. This implies that the investor tax credit sensitivity is higher when α_j is larger—i.e., for non-professional investors.

These two predictions are visualized in Figure 5, which plots the investment probability as a function of the tax credit rate and shows how the relationship depends on α_j. The chances of investment increase in the tax credit rate, but this relationship is flatter when α_j is smaller, indicating lower sensitivity. As α_j converges to 1, the slope converges to zero.

We present a simple numerical example to illustrate the model's mechanics.[70] We then consider the impact of an average tax credit (30%) for investors facing different return distributions. To start, we consider an average investor facing α=1.42, which is calibrated to the average early-stage investment returns from Othman (2019). In this case, introducing the tax credits would increase the investment probability from 0.675 to 0.763, a 13% increase. Next, we consider a 20% swing from this average α: we can interpret the group with a higher α (α=1.136) as professional investors, while those facing a lower α (α=1.704) as non-professionals. For a professional investor, introducing tax credits would increase her investment probability by only 4%, from 0.868 to 0.904. For a non-professional investor with access to less skewed returns, the tax credits would increase her investment probability by 23%, from 0.546 to 0.670. Hence, professional investors have a much weaker response to tax credits than non-professional investors (4% vs 23%). Although this is not a full calibration, it shows that the response can indeed vary greatly when returns become more or less skewed than the average distribution.

H.3.1 Special Case: $\alpha_j \leq 1$

When $\alpha_j \leq 1$, the expected return from the Pareto distribution is infinite ($E(1 + r_i) = \infty$) with an extremely fat right tail. These projects correspond to the "high-risk, high-growth" startups that typically raise financing from the most sophisticated angels and VCs. In this case, at any level of c_i, returns in the right tail are overwhelmingly dominant within a portfolio of investments. Therefore, the investor always decides to invest. In turn, this means that an intensive margin benefit (τI) at the time of the investment is irrelevant to the decision about whether or not to invest in a high-potential project.

Under this scenario, we have:

- **Lemma 3**: *The most sophisticated investors who face projects with $\alpha_j \leq 1$ are completely insensitive to investor tax credits.*

  Proof: When $\alpha_j \leq 1$, $E(1 + r_i) = \infty$. Thus the investment rule $E(1 + r_i) > 1 + k - \tau$ becomes $\infty > 1+k-\tau$. Then the probability of investing is $P = Prob(\infty > 1+k-\tau) \equiv 1$, hence $\frac{\partial P}{\partial \tau} = 0$. Therefore, investor tax credits do not affect the investor's decision.

If the investor's expected return is infinite, it is invariant to the entry price. Furthermore, this case highlights how sensitivity can be arbitrarily close to zero if investors *believe* that they are selecting deals from a distribution with an extremely fat tail (i.e., as α converges to 1).

H.3.2 Supporting Evidence

The predictions above align well with observations from practitioners. Anecdotal evidence suggests that professional early-stage investors understand the logic that returns from the

[70] We assume that $C = 1$ and the cost of capital $k = 10\%$, consistent with Figure 5.

startups they are considering are drawn from a distribution with an extremely fat right tail, and therefore approach angel investing without a first-order focus on the intensive margin price per share. For example, Charles Birnbaum, a partner at Bessemer Venture Partners, said that "your entry price matters when you think there's a ceiling [on the startup's exit valuation]."[71] Crowdwise, an early-stage investing advice website, explains that:

> "[In] angel investing there is a common adage that many investors follow that says to 'swing for the fences'...you need to be aiming for the big, long-tail returns of power law investing...If during its seed round Uber had been valued at $10M or $20M instead of $5.5M, would it have been wise for those initial investors to pass because it was over-valued?...At that order of magnitude return in the thousands, the key is being involved at all vs. getting in at the right price" (Belley (2018)).

Peter Thiel, known for his angel investment in Facebook among other successful startups, has the following directive based on a power law argument: "This implies two very strange rules for VCs. First, only invest in companies that have the potential to return the value of the entire fund... This leads to rule number two: because rule number one is so restrictive, there can't be any other rules." (Thiel and Masters (2014)). Finally, Fred Wilson, the founding partner of Union Square Ventures has written that startup outcomes follow "a classic power law curve, with the best investment in each fund towering over the rest...The goal is not to maximize the VC's returns from a failed investment. Because it doesn't matter to the fund economics one bit." (Wilson (2015)). While some of these arguments concern VC investors, they are magnified at earlier stages when professional angels fund the types of startups that are at hazard of subsequently raising VC.

To our knowledge, we are the first to point out that fat-tailed return distributions have important implications for the role of entry prices and thus for the effectiveness of early-stage investor subsidies. When the potential gains are very high (α_j is low) or potentially infinite ($\alpha_j \leq 1$), the entry price for early-stage investments is largely unimportant.

H.4 Effect of Capital Gains Tax Credits on Investment Decisions

Consider an alternative scenario with a capital gains tax credit. In contrast to the investor tax credit, which is state independent, investors benefit from a capital gains tax credit only when there is a positive return ($r_i > 0$). Rather than being fixed at the time of investment, the subsidy scales up as returns increase. In fact, this tax credit offers an amount equal to a fixed fraction of the capital gains, i.e., $\tau \times I r_i$. In the U.S., investors are allowed to exclude 50% to 100% of capital gains from federal taxation of Qualified Small Business Stock (QSBS). For this program, τ can be interpreted as the capital gains tax rate times the exclusion percentage.

[71] See Birnbaum Podcast.

The expected return for an investor with a capital gains tax credit is:

$$E(R_i) = E(1 + r_i) + E(\tau r_i \mid r_i > 0)Prob(r_i > 0). \tag{15}$$

As before, we assume that $1 + r_i$ follows a Pareto distribution with a scale parameter $c_i > 0$ and a shape parameter α. Note that the conditional Pareto distribution with parameters (α_j, c_i) is also a Pareto distribution with the same α_j and a scale parameter equal to the conditioning threshold $(1 + r_i \mid 1 + r_i > 1 \sim Pareto(\alpha_j, 1))$. However, when the threshold is smaller than c_i (i.e., $c_i > 1$), the random variable will always be above the threshold, and thus $E(\tau r_i \mid r_i > 0)Prob(r_i > 0)$ simplifies to $E(\tau r_i)$ for this case. Therefore, we consider the case of $c_i \leq 1$ and $c_i > 1$ separately.[72]

H.4.1 Case $c_i \leq 1$

When $c_i \leq 1$, we have $E(r_i + 1 \mid r_i > 0) = \frac{\alpha_j}{\alpha_j - 1}$, and therefore $E(r_i \mid r_i > 0) = \frac{1}{\alpha_j - 1}$. Further, the CDF of the Pareto distribution is $Prob(1 + r_i \leq x) = 1 - (\frac{c_i}{x})^{\alpha_j}$, which implies $Prob(r_i > 0) = c_i^{\alpha_j}$. This leads to the following investment rule if $\alpha_j > 1$:

$$E(R) = \frac{\alpha_j c_i}{\alpha_j - 1} + \frac{\tau}{\alpha_j - 1} c_i^{\alpha_j} > 1 + k. \tag{16}$$

The investor invests if and only if:

$$F(c, \tau) = \frac{\tau c_i^{\alpha_j}}{\alpha_j - 1} + \frac{\alpha_j c_i}{\alpha_j - 1} - (1 + k) > 0. \tag{17}$$

This implies $c_i > c^*$, where $F(c^*, \tau) = 0$. The fraction of projects that will receive investment is then $P = 1 - \frac{c^*}{C}$.[73] Differentiating the implicit function, the sensitivity of the investment likelihood to the capital gains tax credit is:

$$\frac{\partial P}{\partial \tau} = -C^{-1}\frac{\partial c^*}{\partial \tau} = C^{-1}\frac{\partial F/\partial \tau}{\partial F/\partial c^*} = \frac{C^{-1} c_i^{\alpha_j}}{\tau \alpha_j c_i^{\alpha_j - 1} + \alpha_j}. \tag{18}$$

H.4.2 Case $c_i > 1$

When $c_i > 1$, $Prob(r_i > 0) = 1$, and $E(\tau r_i \mid r_i > 0)Prob(r_i > 0) = E(\tau r_i) = \tau(\frac{\alpha_j c_i}{\alpha_j - 1} - 1)$. Hence, the investment rule under $\alpha_j > 1$ is:

$$E(R) = \frac{\alpha_j c_i}{\alpha_j - 1} + \tau(\frac{\alpha_j c_i}{\alpha_j - 1} - 1) > 1 + k. \tag{19}$$

[72] We only consider the case when $\alpha_j > 1$. When $\alpha_j \leq 1$, investors will be completely insensitive to a capital gains tax credit, as the cost of passing an investment with unbounded expected mean is infinite.

[73] Note that to define P, we need to assume that the function $F(c^*, \tau)$ is continuous in c^*. However, this property follows from the continuity and monotonocity of $E(\tau r_i \mid r_i > 0)$.

The investor invests if and only if:

$$F(c,\tau) = \frac{\alpha_j c_i}{\alpha_j - 1}(1+\tau) - \tau - (1+k) > 0. \tag{20}$$

Again, the fraction of projects that will receive investment is $P = 1 - \frac{c^*}{C}$, where $F(c^*, \tau) = 0$. The sensitivity of the investment likelihood to the capital gains tax credit is:

$$\frac{\partial P}{\partial \tau} = -C^{-1}\frac{\partial c^*}{\partial \tau} = C^{-1}\frac{\partial F/\partial \tau}{\partial F/\partial c^*} = C^{-1}\frac{\alpha_j(c_i - 1) + 1}{\alpha_j(1+\tau)}. \tag{21}$$

H.4.3 Lemmas

We derive the following predictions using the sensitivities derived in 4.1 and 4.2:

- **Lemma 4**: *A higher capital gains tax credit increases investment by all investors with $\alpha_j > 1$.*

  Proof: When $c_i \leq 1, \frac{\partial P}{\partial \tau} = \frac{C^{-1} c_i^{\alpha_j}}{\tau \alpha_j c_i^{\alpha_j - 1} + \alpha_j} > 0$. When $c_i > 1, \frac{\partial P}{\partial \tau} = C^{-1}\frac{\alpha_j(c_i - 1)+1}{\alpha_j(1+\tau)} > 0$. Thus, $\frac{\partial P}{\partial \tau}$ is always positive regardless of the value of c_i.

- **Lemma 5**: *When $\alpha_j > 1$, the sensitivity of the investment decision to a capital gains tax credit decreases with α_j, making professional investors more sensitive to a capital gains tax credit than non-professional investors.*

  Proof: When $c_i \leq 1$,

  $$sign\left(\frac{\partial^2 P}{\partial \tau \partial \alpha_j}\right) = sign\left(\alpha_j \ln c_i - \tau c_i^{\alpha_j - 1} - 1\right). \tag{22}$$

  Since $\ln c_i \leq 0$ when $c_i \leq 1$, $\alpha_j \ln c_i - \tau c_i^{\alpha_j - 1} - 1 < 0$. When $c_i > 1$,

  $$sign\left(\frac{\partial^2 P}{\partial \tau \partial \alpha_j}\right) = sign\left(-1 - \tau\right). \tag{23}$$

  Thus, in both cases, we have $\frac{\partial^2 P}{\partial \tau \partial \alpha_j} < 0$, implying that investors with lower α_j (i.e., professional investors) are more sensitive to a capital gains tax credit.

H.5 Discussion

Our model shows that angel tax credits affect investment decisions less for investors financing startups whose returns are distributed with a fatter right tail. When the Pareto shape parameter α is greater than one, we find that angel tax credits increase the ex-ante probability of investing in a company, but this effect declines as the right tail of the distribution grows fatter (i.e., α decreases). This simple model provides a framework for interpreting our empirical analysis, which finds that professional investors funding potentially high-growth startups are less sensitive to angel tax credits than non-professional investors.

In an extension, we also show that a capital gains tax credit affects investors differently. Both tax credits increase the ex-ante investment likelihood. However, the effect of the capital gains tax credit on investment decisions increases as α declines. This subsidy – the opportunity to avoid capital gains taxes – increases with the potential return. In contrast, the subsidy for angel tax credits is fixed once the investment is made and therefore it becomes relatively less important as investors have greater access to startups with a fatter right tail.

An important caveat is that our model focuses on the sensitivity of investor decisions to tax incentives, and not on investor or social welfare. Just because investors are not responsive to a change in tax policy does not mean that they are indifferent. Clearly, an individual investor is better off with tax incentives than without them (all else equal), even if she does not change her behavior because of the incentive. A promising avenue for future empirical research is to explicitly compare the effects of capital gains and investor tax credits.

Finally, the model assumes that investors are not financially constrained. It is useful to discuss how the presence of a tax credit affects the funding constraints of some investors. It is reasonable to think that at some level and for some subset of investors, arbitrarily increasing the tax credit amount should increase investment. However, we do not think budget constraints have a first-order effect in our setting for three reasons.

First, the tax credit does not immediately relax financial constraints because it is claimed against future tax liabilities. There is a delay between the time of the investment and the time at which the tax credit can be "cashed out," and in some cases this delay can be substantial. Of course, under an efficient markets hypothesis investors could borrow against the tax credit value. In reality, there are frictions to such dynamic transfers.

Second, in the angel investment market, it is unclear that the main constraint investors face is their budget. Instead, constraints in (a) the supply of high-quality companies; and (b) their time available to monitor and advise startups in which they invest may play a more important role. In practice, many angels decide they will make a certain number of investments each year (for example, two or three) and then search for startups with "home run" potential. In this approach to investing, budget constraints are not central.

Third, one piece of evidence for unresponsiveness among angels comes from the survey, where we abstract from financial constraint considerations. We ask investors to rate various factors based on their importance in determining decisions about whether or not to invest in a company. In this setting, we find that angel tax credits are rated as not important, both in absolute and in relative terms. This is consistent with, at best, a secondary role for financial constraints.

REFERENCES

Belley, Brian, "Power Law Investing in Crowdfunding," *Crowdwise*, 2018.

Black, Bernard, Alex Hollingsworth, Leticia Nunes, and Kosali Simon, "Simulated Power Analyses for Observational Studies: An Application to the Affordable Care Act Medicaid Expansion," Technical Report 2019.

Black, Sandra E and Philip E Strahan, "Entrepreneurship and bank credit availability," *The Journal of Finance*, 2002, *57* (6), 2807–2833.

Burlig, Fiona, Louis Preonas, and Matt Woerman, "Panel Data and Experimental Design," *Journal of Development Economics*, 2020, *144*, 102458.

Callaway, Brantly and Pedro HC Sant'Anna, "Difference-in-differences with multiple time periods," *Journal of Econometrics*, 2021, *225* (2), 200–230.

Campbell, J., "Legislation Could Impact Economy," 2014. Available at https://insurancenewsnet.com/oarticle/Legislation-could-impact-economy-a-485988 (accessed June 29, 2020).

Core, Fabrizio, "Lend Me a Hand-Bank Market Power and Firm Creation in Innovative Industries," *Available at SSRN 3733765*, 2020.

Decker, Ryan A, John Haltiwanger, Ron S Jarmin, and Javier Miranda, "Changing Business Dynamism and Productivity: Shocks versus Responsiveness," *American Economic Review*, 2020, *110* (12), 3952–90.

Ewens, Michael, Ramana Nanda, and Matthew Rhodes-Kropf, "Cost of Experimentation and the Evolution of Venture Capital," *Journal of Financial Economics*, 2018, *128* (3), 422–442.

Hall, Robert E and Susan E Woodward, "The Burden of the Nondiversifiable Risk of Entrepreneurship," *American Economic Review*, 2010, *100* (3), 1163–94.

Kerr, William and Ramana Nanda, "Democratizing entry: banking deregulations, financing constraints, and entrepreneurship," 2008.

Kerr, William R, Ramana Nanda, and Matthew Rhodes-Kropf, "Entrepreneurship as Experimentation," *Journal of Economic Perspectives*, 2014, *28* (3), 25–48.

Kousky, K. and K. Tuomi, "What Do the Reports Say about Job Creation?," 2015.

Lerner, Josh, Antoinette Schoar, Stanislav Sokolinski, and Karen Wilson, "The globalization of angel investments: Evidence across countries," *Journal of Financial Economics*, 2018, *127* (1), 1–20.

Linhorst, M., "Bill Offering Breaks for 'Angel Investors' Signed," 2013.

Malenko, Andrey, Ramana Nanda, Matthew Rhodes-Kropf, and Savitar Sundaresan, "Investment Committee Voting and the Financing of Innovation," Technical Report, Working paper 2020.

Moxley, E., "Why Missouri Doesn't Offer Angel Tax Credits to Startup Investors, October 21, 2014," 2014.

Othman, Abraham, "Startup Growth and Venture Returns," *AngelList*, 2019.

Scherer, Frederic M and Dietmar Harhoff, "Technology Policy for a World of Skew-Distributed Outcomes," *Research Policy*, 2000, *29* (4-5), 559–566.

Sun, Liyang and Sarah Abraham, "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects," *Journal of Econometrics*, 2021, *225* (2), 175–199.

Thiel, Peter A and Blake Masters, *Zero to One: Notes on Startups, or How to Build the Future*, Currency, 2014.

Tuomi, Krista and Barbara Boxer, "The Costs and Benefits of Early-Stage Business Tax Credits: A Case Study of Two Us States," *Venture Capital*, 2015, *17* (3), 263–270.

Wilson, Fred, "Power Law and the Long Tail," *AVC*, 2015. Available at https://avc.com/2015/11/power-law-and-the-long-tail.