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

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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.

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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.[2]

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 United States. 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 more limited. However, additional investment flows to older firms, to 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

[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 United States, 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.

[2] See "The Problem with Tax Credits for Angel Investors," *Bloomberg Businessweek*, August 20, 2010; "Should Angel Investors Get Tax Credits to Invest in Small Businesses?" *Wall Street Journal*, March 18, 2012; "Angel Investment Tax Credit Pricey but Has Defenders," *Minnesota Star Tribune*, October 31, 2015.

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 BDS. 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. We find that the minimum detectable effects 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 a minimum detectable effect 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. Furthermore, 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), Zwick and Mahon (2017), Arefeva et al. (2020), Dechezleprêtre et al. (2020), Edwards and Todtenhaupt (2020), Freedman, Neumark, and Khanna (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 mechanism 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, nonangel 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 nonfinancial 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 the Securities and Exchange Commission (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 nonbeneficiary 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 nonprofessional 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 nonprofessional 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, Korteweg, and Laws (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 nonprofessional 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 therefore depend crucially 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, Lerner, and Schoar (2014), Hellmann and Thiele (2015), Hochberg, Serrano, and Ziedonis (2018), Lerner et al. (2018), Xu (2019), Davis, Morse, and Wang (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)).[3] Especially relevant to our setting is an evaluation of the California Competes Tax Credit (CCTC) by Freedman, Neumark, and Khanna (2021), 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.

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

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, Du, and Hellmann (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)).

The paper is organized as follows. Section I provides an overview of angel investor tax credits. Section II details the data. Section III explains the identification strategy and studies the effects of angel investor tax credits on angel investment and real outcomes. Section IV presents evidence of two mechanisms that explain why angel investment increases yet there is no impact on entrepreneurial activity, and Section V concludes.

I. Angel Investor Tax Credits

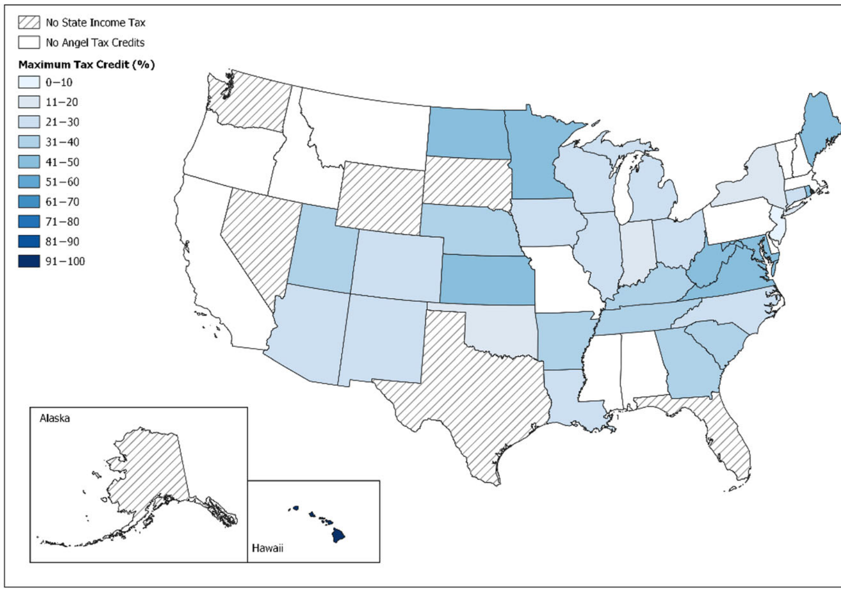
Over the last three decades, 31 states in the United States have introduced and passed legislation to provide accredited angel investors with tax credits. Figure 1, Panel A, provides a map of states with angel tax credit programs, which we abbreviate as "ATC" hereafter. Blue shading indicates the tax credit percentage, with darker shades representing larger tax credits. The figure shows that ATCs are prevalent across the United States. The extent of these programs is particularly notable since they do not occur in the seven states with no income tax (shaded in gray). 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 over the following three decades. Colorado, Maryland, Minnesota, North Dakota, and Ohio passed more than one version of an ATC. Although the pace of adoption has increased in recent years, 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%.[4] 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.[5] Finally, investors often use ATCs, with

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

[5] 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.

Panel A: States with Angel Tax Credit Programs



Panel B: Timing of State Angel Tax Credit Programs

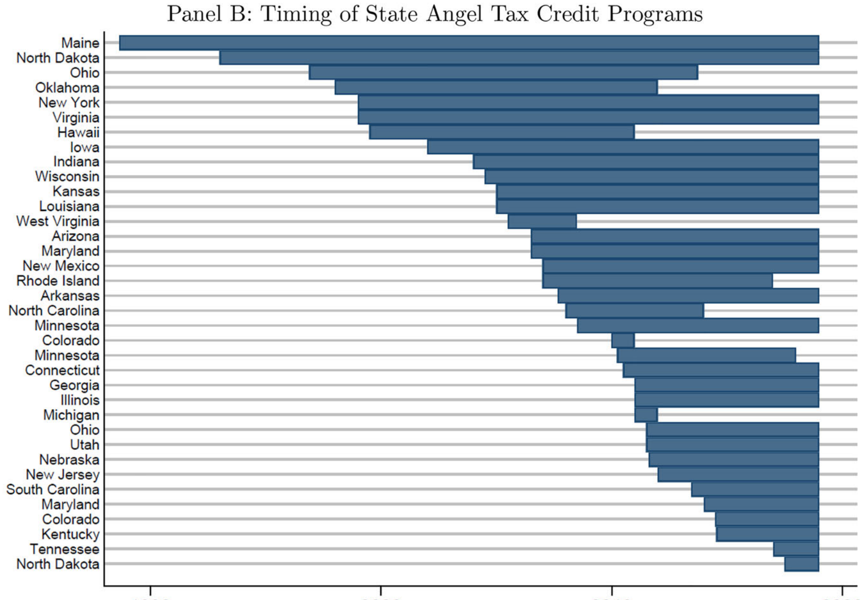


Figure 1. State angel tax credit programs. Panel A provides a map of states that adopted angel tax credit programs from 1988 to 2018. Blue shading indicates the tax credit percentage, with darker shades representing larger tax credits. Slanted lines denote states with no state income tax. Panel B shows the introduction and termination of each program in our sample, starting with the earliest program and ending with the most recent one. (Color figure can be viewed at wileyonlinelibrary.com)

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.[6] 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 a period that is typically several months. State-level ATCs reduce the state income tax of an investor. For example, suppose that an investor earns $250,000 in a given 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 decrease of 56% relative to her annual state taxes.[7] Unlike capital gains tax credits that require 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.[8] 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 I provides summary statistics on the ATCs. The maximum share of an investment that can be deducted from an investor's tax liability is defined as *Tax credit %*. The mean (median) tax credit percentage is 34% (33%). Programs often have eligibility criteria for both beneficiary companies and investors.

[6] 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.

[7] 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.

[8] See Wisconsin Economic Development Corporation 2013 Qualified New Business Venture Program Report (https://wedc.org/wp-content/uploads/2014/11/2013-QNBV-Report.pdf), Louisiana legislation (http://www.legis.la.gov/Legis/Law.aspx?d=321880), and "Startup investors camp out for Maine tax credit" (https://www.pressherald.com/2019/01/02/startup-investors-camp-out-for-maine-tax-credit).

Table I

Summary Statistics on Angel Tax Credit Programs

This table 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 (1)	Mean (2)	Median (3)
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
Nonrefundable	72%		
No carry forward	11%		
Nontransferrable	72%		

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 IA.I 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. Section III of the

Internet Appendix provides additional details about the predictive regression and examples of frictions in the implementation of ATCs.[9]

II. Data

This section discusses the data we use on angel deals and investors (Section II.A), state-level outcomes (Section II.B), and program applicants and beneficiaries (Section II.C).

A. Angel Deals and Investors

Angel investments are difficult to systematically observe in the United States because, to our knowledge, there are no comprehensive data sets 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 hereafter, and Form D filings available from the SEC.[10] 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)).[11] To identify angel rounds, we drop all financial issuers and focus on the first Form D filing that is not a venture capital (VC) round.[12] We then disambiguate and eliminate duplicates.[13]

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 data set represents one of the most comprehensive sources of angel deals available. Table II 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

[9] The Internet Appendix may be found in the online version of this article.

[10] 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 VC financing. We identify angel investments from these two databases based on round type and investor type. In Crunchbase, we include round types “preseed,” “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,” “preseed,” “crowd,” “angel,” or “accelerator.”

[11] Offerings under Regulation D preempt state securities law. Before March 2008, Form D filings were paper based. We use a Freedom of Information Act request to obtain nonelectronic Form D records from 1992 to 2008.

[12] Specifically, we drop all financial issuers and pooled investment funds. Furthermore, 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.

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

Table II

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 Section II of the Internet Appendix.

	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 nonangel 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

using firm name, address, and founding year. We only use actual, nonimputed employment and employment growth in the year before angel investment (Crane and Decker (2020)).[14] 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.[15] 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 II. Our investment analysis shows that the main results are similar in the full sample and the NETS-matched sample, and then focuses on the NETS-matched sample to study heterogeneity based on the firm characteristics that it provides.

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

B. 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 0 to 5 and jobs created at those firms.[16] 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 North American Industry Classification System (NAICS) 51 (Information) and 54 (Professional, Scientific, and Technical Services), but show robustness to including additional sectors as well as to restricting to these two sectors when

[14] The NETS-matched sample period is 1993 to 2016. We start the sample in 1993 because Form D data are incomplete in 1992. In addition, we require up to two years of preinvestment 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.

[15] Following the programs' most common eligibility restrictions, we define high-tech as the following NAICS codes corresponding to information technology, 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.

[16] Using ages 0 to 5 permits the programs to affect growth at young firms in addition to new entrants. In Table IA.XXI, we use only age zero firms and find stronger results. We use establishments, which are the unit of measurement in BDS, but we refer to them as "firms" because essentially all firms in our data have one establishment.

studying angel investment. Table II presents summary statistics for our main real outcomes, and Section II of the Internet Appendix provides detailed definitions of all variables.

We employ several supplementary data sets 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.[17] This measure was developed by the Startup Cartography project (Fazio, Guzman, and Stern (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 initial public offering (IPO)). Second, we gather data at the state-year level on new high-tech startups from 2000 to 2019. The data are provided by Steppingblocks and based on LinkedIn. Steppingblocks defines a startup as a firm that appears in LinkedIn for the first time in a given year and begins with no more than 20 employees.[18] We also examine innovation using patent applications from the United States Patent and Trade Office (USPTO) and the number of successful startup exits based on CVV data.

C. 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 10 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, although we do not observe all years for all programs. Table IA.III, 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 data sets. First, we match 1,227 firms to financing data. Second, we match 1,350 startups to Steppingblocks LinkedIn data. Steppingblocks provides an employment panel based on comprehensive LinkedIn profiles.

III. Effects of Angel Investor Tax Credits

In this section, we first explain the estimation approach for evaluating state-level effects of ATCs (Section III.A). We then discuss results of this analysis on angel investment (Sections III.B and III.C). Effects on real outcomes are presented in Section III.D.

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

[18] 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 provided in Table IA.II.

A. Identification Strategy

Our empirical approach is a differences-in-differences design that exploits 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 equal to one if state s has an ATC program in year t. The dependent variable is angel investments or a real outcome. The vector $X_{s,t-1}$ contains state-year controls.[19] The specification also includes state (α_s) and time (α_t) fixed effects. Standard errors are clustered by state (Bertrand, Duflo, and Mullainathan (2004)). The coefficient of interest, β, 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:

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

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 ($\mathbb{1}(ATC_{s,\le t-4})$ and $\mathbb{1}(ATC_{s,\ge t+4})$).

B. 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 III, Panel A, using equation (1). We estimate this equation using the unrestricted sample (columns (1) and (2)) and the NETS-matched sample (columns (3) and (4)), which we use in the subsequent angel investment analysis since it allows us to more precisely identify targeted firms and observe firm characteristics.[20] Across both samples, we show that angel

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

[20] The unrestricted sample period is from 1988 to 2018. The NETS-matched sample is restricted to a shorter period, from 1993 to 2016, due to NETS coverage.

Table III

Angel Tax Credits and Angel Investments

Panel A reports 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) and (2) use the unrestricted sample of angel deals from 1988 to 2018. Columns (3) and (4) use the sample of deals that can be matched to NETS from 1993 to 2016. $\mathbb{1}(ATC)$ is an indicator variable equal to 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 preinvestment 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 (1)	Low (2)	High (3)	Low (4)	High (5)	Low (6)	Young (7)	Old (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

tax credit programs ($\mathbb{1}(ATC_{st})$) increase angel investments by 17.8% to 19.0% (columns (1) and (3)).[21] We also find that a 10-percentage-point rise in the tax credit percentage ($Tax\,credit\,\%_{st}$) increases the number of angel investments

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

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 pretrends, 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 IA.IV, 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 IA.IV, Panel B. We first 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 I.[22] 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 causal interpretation of our main findings and highlight the importance of program design.[23] We next study heterogeneity in local 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. We find that ATCs have a weaker effect on angel investment volume in states with an ample supply of VC (columns (2) and (4)). This result is consistent with angel financing and VC being substitutes (Ersahin, Huang, and Khanna (2021), Hellmann, Schure, and Vo (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 may not facilitate investment in potentially high-growth firms, which are more likely to have access to VC.

To explore this question directly, we examine 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 III, 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) and (2)). The results are similar when we look at employment

[22] For each nonbinary 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 nonrefundable, nontransferable, 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.

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

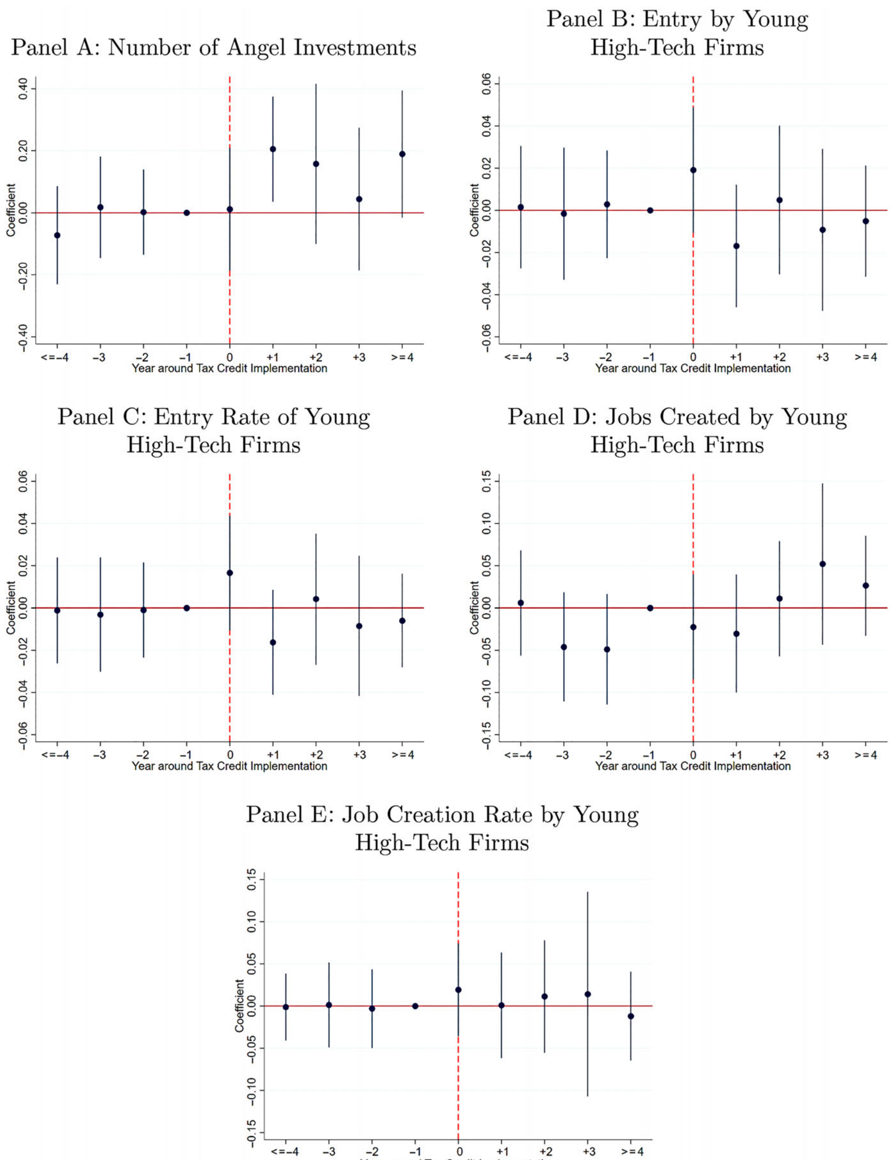


Figure 2. Dynamics effects of angel tax credit introduction. This figure shows the dynamic effects of introducing angel tax credits using equation (2). Dots denote the point estimates of dynamic differences-in-differences coefficients and bars indicate 95% confidence intervals. The year before policy introduction is normalized to zero. Panel A shows the number of angel investments, Panel B examines the entry by young (age 0 to 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, and 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 to 2018. Detailed variable definitions are in Section II of the Internet Appendix. Standard errors are clustered at the state level. (Color figure can be viewed at wileyonlinelibrary.com)

growth (columns (3) and (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) and (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 in nascent firms (columns (7) and (8)). We confirm these results by showing that the average angel-backed firm has lower growth characteristics and fewer serial entrepreneurs after a state implements ATCs (Table IA.IV, 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, Nanda, and Rhodes-Kropf (2014)). The results on age are inconsistent with this interpretation 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 an ATC program, conditional on eventually having a program. Figure IA.1 shows that, consistent with our regression estimates, the distribution of angel-backed firms shifts to the left toward lower growth characteristics and exit outcomes. This shift occurs across the distribution without a 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 outside of these hub regions.[24] Table IA.IV, 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 nonhub 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

[24] 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.

activity. This finding raises questions about whether ATCs meaningfully impact the local entrepreneurial ecosystem, a topic that we examine further in Section III.D.

C. 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.[25] Table IA.V 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 estimates are identified using less data.

We impose sample restrictions in Table IA.VI, 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)). We again find similar estimates.[26] Third, we show that 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 IA.VI, Panel B, shows that the effects using only NAICS 51 and 54 sectors are again similar in terms of magnitude and significance.

We employ alternative specifications in Table IA.VII. 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) and (2)) and are robust to focusing on state-years with positive investments (Panel B, columns (3) and (4)).[27] 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) and (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) and (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,

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

[26] This addresses the 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 IV.A.2 for a discussion of this possibility).

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

columns (1) and (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) and (4)).

Table IA.VII, 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, two caveats should be noted for these estimates. First, many angel deals do not report round amount. Second, the round amount can include both investment by angels and coinvestment by VCs in the same round.

D. 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 III.D.1), we interpret the results by deriving a prior for the expected effect and calculating the statistical power of our empirical models (Section III.D.2). In addition, we evaluate the role of program scale (Section III.D.3) and discuss robustness tests (Section III.D.4).

D.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 I), we estimate their effects on firm entry and job creation. We use data from the Census BDS to measure the count of young (0 to 5 years old) high-tech firms and new jobs created by these firms.[28] We construct these variables 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 III.B, the effect of ATCs on investments is concentrated in these areas. Focusing on these areas can improve precision in detecting real effects.

Table IV 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.[29] For each outcome, we report results for counts (columns (1) and (2)) and rates (columns (3) and (4)).[30] We use rates because this measure adjusts for

[28] The BDS data only allow us to measure the entry of establishments, rather than firms. However, 99% of high-tech firms 0 to 5 years old are single-establishment firms in our data.

[29] 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 III.D.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%.

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

Table IV

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) and (2) and firm entry rate in columns (3) and (4). Young firms are defined as age 0 to 5. In Panel B, the dependent variable is the log number of new jobs created by young, high-tech firms in columns (1) and (2) and job creation rate by these firms in columns (3) and (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 Section II of the Internet Appendix for variable definitions and in Section IV of the Internet Appendix for power calculations. The sample period is 1988 to 2018. $\mathbb{1}(ATC)$ is an indicator equal to 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

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 models in Table IV, we consistently find small estimates that are not significantly different from zero. The confidence intervals are also economically small. For 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 preexisting negative trend in entrepreneurial activity. The dynamic differences-in-differences, reported in Figure 2, Panels B to E, show no pretrends.[31] 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.

D.2. Interpretation: Statistical Power

In this section, we assess whether our tests have sufficient power to detect real effects.[32] 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 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.[33] A more detailed explanation of the MDE calculation is in Section IV of the Internet Appendix.

[31] Figure IA.2 provides the plots for top MSAs.

[32] 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).

[33] Abadie (2020) highlights that when the power is above 50%, statistically insignificant effects can be more informative than significant effects. Shapiro, Hitsch, and Tuchman (2021) assess the statistical power of their analyses using the 50% threshold. The field experiment literature typically uses 80% as a threshold for high-powered analysis (Chow, Wang, and Shao (2007), Sakpal (2010), Mumford (2012), Black et al. (2019), Isakov, Lo, and Montazerhodjat (2019)).

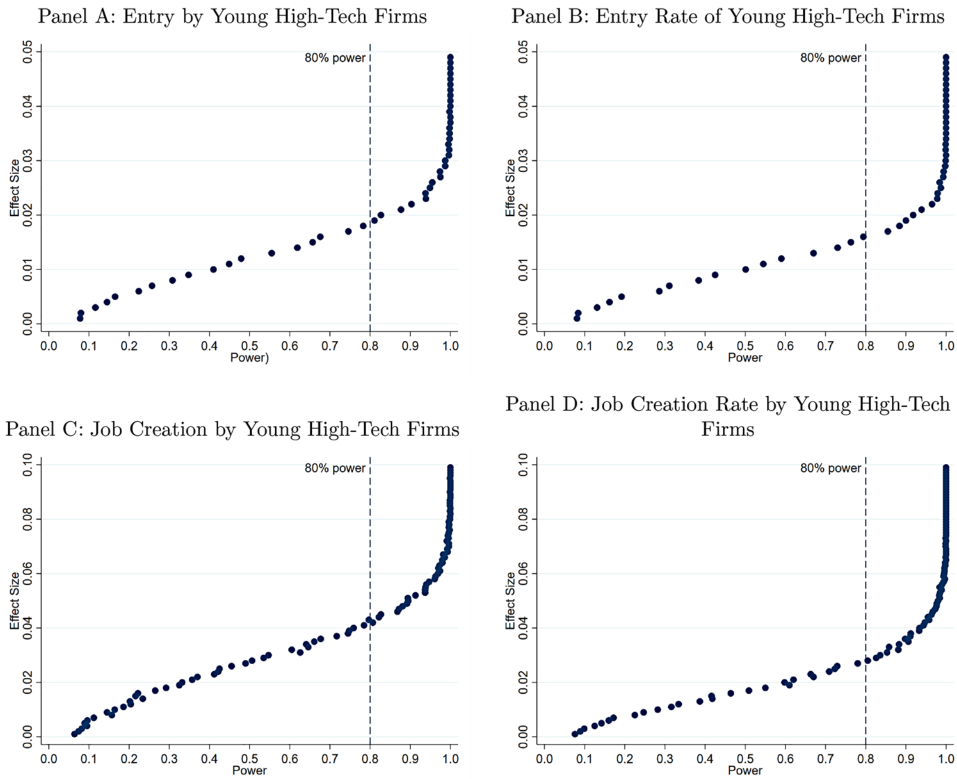


Figure 3. Power and prior for the effect of angel tax credits on real outcomes. This figure shows the relationship between the estimated power of our differences-in-differences model and the minimum detectable effect (MDE) for the four main real outcomes considered in Table IV at the statewide level. Power is computed using the simulation method detailed in Section IV of the Internet Appendix and represents the likelihood that our test detects a significant effect of angel tax credits (at the 10% significance level) when we induce an effect equal to MDE in the data. Each dot represents the MDE for a given power. Solid horizontal lines denote our prior effect (see Section V of the Internet Appendix for the calculation) and dotted lines denote 80% power. (Color figure can be viewed at wileyonlinelibrary.com)

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.[34] 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.

[34] Figure IA.3 repeats the plots for top MSAs. In each panel, the vertical line denotes 80% power.

The bottom of Table IV 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 levels 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) and (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 Section IV of the Internet Appendix).[35]

To facilitate 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 IA.XI reports the main priors along with alternatives that relax various assumptions. A comprehensive explanation of the prior construction is in Section V of the Internet Appendix.

Comparing the baseline prior effects in the first row of Table IA.XI with MDEs at 80% power, we find that, for all specifications, the priors are larger than the MDEs. For example, 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) and (2) of Table IV, Panel A). As mentioned above, these priors are calculated based on particular assumptions. We relax these assumptions in the other rows of Table IA.XI and find qualitatively similar results, with most having power above or close to 80% at the prior.[36] In sum, given the estimated increase in angel investments, our tests have sufficient power to detect the real effects of ATCs.

D.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 IA.XII restricts the sample of treated states to those with an above-median annual budget.[37] Table IA.XIII exploits variation in the program budgets by using the annual tax credit cap in a state-year or

[35] The results are similar using a 5% significance level (Table IA.VIII), without controls (Table IA.IV), or using the continuous treatment *Tax credit* $\%_{st}$ (Table IA.X).

[36] In Section V of the Internet Appendix, we discuss several implicit assumptions that could lead us to underestimate these priors.

[37] 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.

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 IA.III, Panel B, provides summary statistics on beneficiary companies and failed applicants.

We estimate the following equation:

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

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 to receive a tax credit for the first time. We define $\mathbb{1}(TaxCredit_{it})$ as an indicator variable equal to one if startup i had 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.[38]

Table V reports estimates of equation (3). We find that receiving subsidized angel investment does not impact raising VC 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) and (4)) and employment greater than the 75$^{\text{th}}$ percentile within the sample (columns (5) and (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 IA.XIV shows that this result is robust to using a matching estimator that compares beneficiary companies to similar control firms in nearby states without tax credit programs. In Table IA.XV, 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.

[38] We cluster by state-year because there are limited clusters by state. The results are similar when we use other approaches, such as robust standard errors.

Table V

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. $\mathbb{1}(TaxCredit_{it})$ is an indicator variable for startup i having an investor that receives 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 equal to one if a startup experienced a successful exit (via acquisition or IPO). The dependent variables in columns (3) and (4) are indicator variables equal to one if a startup had more than 25 employees two or three years after it applied to be certified for angel investors to receive a tax credit. This is repeated in columns (5) and (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 * denote significance at the 1%, 5%, and 10% level, respectively.

			Empl > 25		Empl > 75th Pctile	
	Raised VC	Exit	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

D.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 IA.XVI. 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 IA.X and again find no effect of ATCs on firm entry and job creation. In Table IA.XVII, 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 ("nontop MSAs") and examine the impact of ATCs on firm entry and job creation. Table IA.XVIII shows that we continue to find no statistically and economically significant effects in these regions. This finding suggests that ATCs do not increase real activity outside of the top MSAs.

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

The null effects on real outcomes persist when we use alternative sectors to define high-tech. In Table IA.XX, we use all two-digit sectors that have any four-digit subsector included in the angel analysis.[39] 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 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.[40] 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 shows 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, Neumark, and Khanna (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.

[39] Panel A uses sectors 31–33, 51, and 54. Panel B uses sectors 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.

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

IV. 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 is driven in part by 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 nonangel early-stage investment after ATCs (Section IV.A.1) and relabeling of investment as "angel" in order to access the ATCs (Section IV.A.2). Second, to the extent that 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 driven mostly by local, inexperienced investors without entrepreneurial backgrounds (Section IV.B.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 IV.B.2 and IV.B.3).

Taken 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 therefore are unlikely to significantly drive aggregate firm entry and job creation.

A. Crowding Out

A.1. Angel Tax Credit and Alternative Finance

Our firm-level analysis (see Section III.D.3) points in the direction of crowding out. Above, we show that beneficiary firms (firms with investors who receive a tax credit) do not perform better than firms with investors who applied but ultimately did not receive a 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, Steinwender, and Van Reenen (2019)).

One way 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, Huang, and Khanna (2021) and Hellmann, Schure, and Vo (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 dilution 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 nonangel-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.[41] 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 funding would crowd out a dollar of VC funding. Table VI, 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)).[42] We further find there is a negative effect on nonangel investment (column (2)) and an offsetting positive effect on angel investment (column (3)). Nonangel 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 angels' share of the total increased, consistent with crowding out.

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. In addition, the specifications in columns (3) and (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.

[41] 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.

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

Table VI
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, nonangel 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 IV.A for a 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) and (2) are unweighted and columns (3) and (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 * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A: Aggregate Financing at State-Year Level

	Ln(Early-Stage) (1)	Ln(Nonangel) (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

A.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 relabeling is extremely difficult to prove, 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 preexisting 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.[43] We identify an investor as an insider 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 Section VI of the Internet Appendix. In Table VII, 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.[44] Relabeled investments would appear in our sample as an angel investment when they might not have

[43] 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 IA.I). Ohio, New Jersey, Kentucky, and Maryland do not exclude executives, but do exclude owners with a preinvestment ownership stake above a certain threshold, ranging from 5% for Ohio to 80% for New Jersey. New Mexico excludes executives but has no limits for owners, families, or employees.

[44] 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 (https://techcrunch.com/2018/11/07/the-disappearing-form-d). While there could be penalties for failing to file a Form D, they appear to be rarely enforced. In addition, 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 (https://www.sec.gov/divisions/corpfin/guidance/securitiesactrules-interps.htm)). The effect of ATCs on angel investments is similar when we restrict to deals only from CVV (Table IA.VI, Panel A).

Table VII
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 nonbeneficiary 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 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 Lvel	
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 Nonbeneficiary 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

otherwise. To explore this, we compare the Form D filing rate across beneficiary firms and matched nonbeneficiary 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.[45] Table VII, Panel B, reports the results.

[45] We restrict control firms to be located in a different state but belong to the same Census division and the same industry, have 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.

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 insider and nonprofessional investors to be more responsive to increased incentives to file a Form D because they are more likely to engage in informal transactions and may not have other financing documentation such as stock purchase and equity rights agreements. Consistent with this view, 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 IA.XXII, 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 result 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 reflect in part 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 IA.VII, Panel E because it concerns the extensive-margin decision of whether to report an investment. In addition, Table IA.XXIII 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.

B. 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 the response is instead concentrated among nonprofessional investors, the effectiveness of these programs may be limited.

B.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

Table VIII
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 Chief Executive Officer. Gender and race are identified from pictures. An individual's approximate age is derived by 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			

identities of subsidized investors and connect them with LinkedIn information on investor characteristics. Table VIII 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.[46] 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 nonprofessional. 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 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%).

[46] We coded 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 United States (see Name List (https://names.mongabay.com/data/hispanic.html)).

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 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 nonprofessional 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. In Table IA.XXIV, we verify that these measures of nonprofessional investors are negatively correlated with better startup exit outcomes.

Table IX, Panel A, reports the estimates of equation (1) using investment-level data.[47] 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. This is a 15% increase relative to the sample mean in Table II. 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) to (4)). In Panel B, we examine whether the shift to nonprofessional 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) and (2)). They increase inexperienced investors by 32%, but have a small and insignificant effect on experienced investors (columns (3) and (4)). We observe a similar pattern for exit and founder experience (columns (5) to (8)).

Overall, local, inexperienced angel investors drive the increase in angel investments described in Section III.B, while professional, arms-length angels are relatively unresponsive to the tax incentive. ATCs affect not only the investment decisions of existing investors, but also who is investing, leading to a larger share of nonprofessional investors. This shift helps explain why

[47] The sample starts in 2003, when AngelList data began to have reasonable coverage. We find similar results when we restrict the sample to start in 2010 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.

Table IX

Which Investors Respond to Angel Tax Credits?

This table examines changes in investor composition due to angel tax credit programs. Panel A reports 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 (fewer than five 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 equal to 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 * denote 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

(*Continued*)

Table IX—*Continued*

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

marginal investments flow to lower-growth firms. If nonprofessional 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 explain the null real effects. Nonprofessional investors may also be more likely to invest for nonpecuniary 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.

B.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 nonprofessional investors. Last, 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, Sautner, and Starks (2016)), venture capitalists (Gompers et al. (2020)), and private equity investors (Gompers, Kaplan, and Mukharlyamov (2016), Bernstein, Lerner, and Mezzanotti (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 II: state-provided lists of ATC recipients and all investors on AngelList as of early 2020 who made at least one investment. We sent each investor an email containing a personalized survey link. This email and the complete survey are reproduced in Section VII of the Internet Appendix. In total, we emailed just over 12,000 individuals and obtained 1,411 responses, of which 1,384 are complete, representing a response rate of 11.6%, which is in line with other recent investor surveys.[48] 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 IA.XXV.[49]

[48] We obtained approval from the NYU Institutional Review Board 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 previous literature conducting other large-scale surveys. Gompers et al. (2020) survey VC investors and obtain a response rate of 8.3%, Bernstein, Lerner, and Mezzanotti (2019) obtain a response rate of 10.3% from private equity investors, Graham and Harvey (2001) obtain a response rate of 8.9% from chief financial officers, 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, Kaplan, and Mukharlyamov (2016) survey 79 buyout investors and Gompers et al. (2020) survey 885 VC investors.

[49] In Table IA.XXV, 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

Panel A: Distribution of Responses to Factor Importance Question

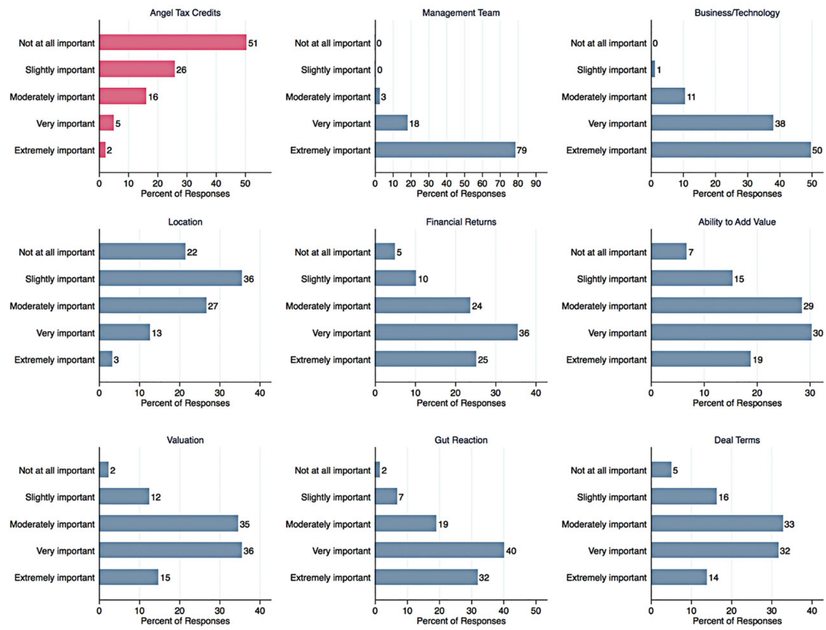


Figure 4. Survey results. The graphs in Panel A show the distribution of responses to question 1 in the survey for each of the nine investment factors. Respondents could only choose one importance level for each factor. The order in which the factors were presented was randomized across survey participants. $N = 1,364$. The graphs in Panel B 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 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 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. (Color figure can be viewed at wileyonlinelibrary.com)

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

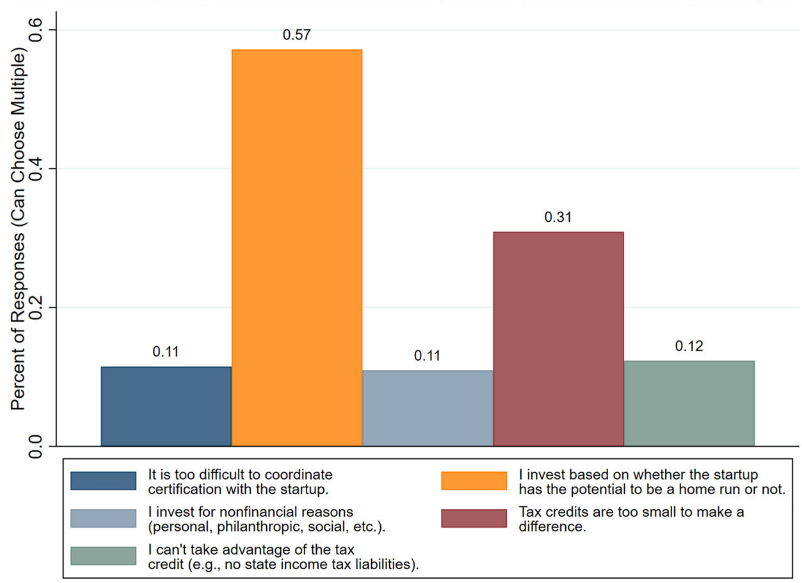
respond. While these relationships are not large in magnitude, they point toward respondents being somewhat more experienced investors.

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

Figure 4. *Continued*

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, Korteweg, and Laws (2017). Only 2% rate valuation and gut

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

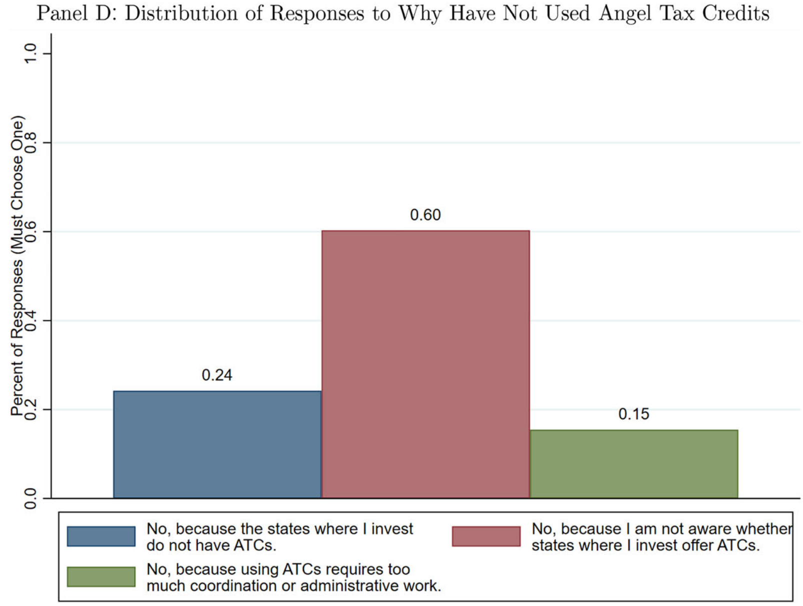


Figure 4. *Continued*

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 IV.B.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 X, Panel A, finds a significant negative association between how important investors rate ATCs and a variety of proxies for investor sophistication and experience (columns (1) to (3)). For example, being a professional investor reduces ATC importance by 0.38, which is a 21% decrease relative to the sample mean. This finding provides 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 (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 IV.B.3.

In Table X, 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 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

Table X

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 = "not at all important" and 5 = "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 why a respondent perceives angel tax credits to be unimportant. All regressions include state fixed effects. Standard errors are clustered by state. ***, **, and * denote 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

(*Continued*)

Table X—*Continued*

Panel B. Reasons for ATC Unimportance					
	Home Run (1)	Coordination (2)	Nonfinancial (3)	Too Small (4)	Cannot Use (5)
Above median no. of deals since 2018	0.046** (0.020)	0.051** (0.024)	0.006 (0.016)	−0.021 (0.021)	0.003 (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

investors are more likely to report coordination frictions (Table X, Panel B, column (2)).[50]

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 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.

B.3. Stylized Model

Professional investors appear to be less responsive to tax credits than nonprofessional investors based on the investor heterogeneity and survey results. Furthermore, 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, although it does not fully characterize how ATCs affect investment decisions. The full model and proofs are in Section VIII of the Internet Appendix. 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, Nanda, and Rhodes-Kropf (2014), Ewens, Nanda, and Rhodes-Kropf (2018)).[51]

[50] 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.

[51] Hall and Woodward (2010) and Kerr, Nanda, and Rhodes-Kropf (2014) document that most startups fail completely while a few generate enormous returns. Malenko et al. (2020) further

We assume that α_j is an investor-specific parameter governing the pool of projects that the investor can access.[52]

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 nonprofessional 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 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 one, the slope converges to zero.[53] This stylized model helps us interpret the survey finding that ATCs do not impact the decisions of 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 nonprofessional investors documented in both the survey and our investor composition analysis.

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) and see Power Law and the Long Tail (https://avc.com/2015/11/power-law-and-the-long-tail)). In addition, the Pareto distribution allows us to capture limited liability facing investors as the distribution is bounded below.

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

[53] Section VIII of the Internet Appendix provides a numerical example of this relationship based on a calibrated value of α_j.

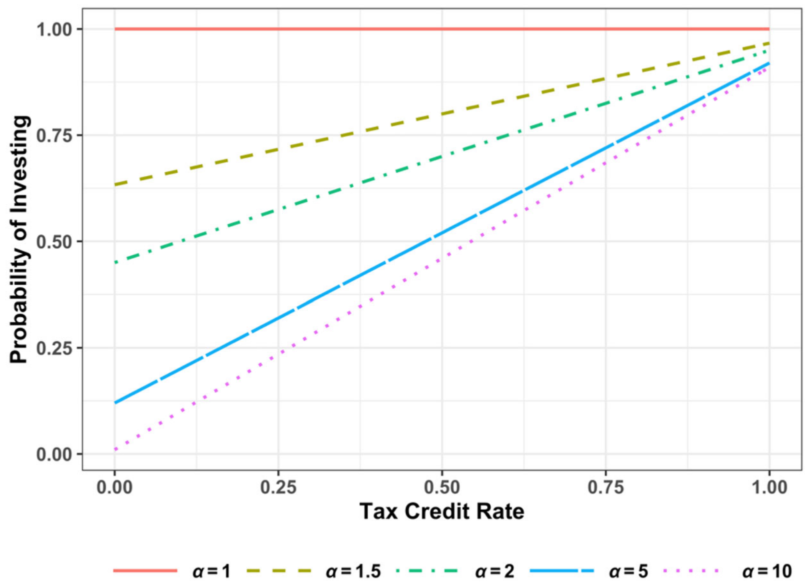


Figure 5. Model prediction: Investment probability and investor tax credit rate. This figure plots investment probability against tax credit rate τ and shows how the relationship varies with the shape of the return distribution α. We consider cases in which α is equal to 1, 1.5, 2, 5, and 10. A lower α represents a Pareto distribution with a fatter tail. We assume cost of capital $k = 10\%$ and $C = 1$. Section VIII of the Internet Appendix details the investment probability function and the associated parameters. (Color figure can be viewed at wileyonlinelibrary.com)

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.[54] The predictions above align well with observations from practitioners such as Charles Birnbaum, a partner at Bessemer Venture Partners, who noted that "your entry price matters when you think there's a ceiling [on the startup's exit valuation]."[55]

[54] 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 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 Section VIII of the Internet Appendix, other policies such as capital gains exemptions may work better in this setting.

[55] See Birnbaum Podcast (https://podcasts.apple.com/us/podcast/bessemer-venture-partners-charles-birnbaum-fintech/id1042827113?i=1000514179070).

V. 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 United States.[56] 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, 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 nonprofessional, and the additional companies that they finance tend to be low growth and relatively old, muting potential effects on firm entry and job creation.

We next 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 nonmonetary factors such as certification and advice that angel investors provide, as opposed to capital constraints being the primary scarce factor. This is a promising 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.

[56] See Senate Bill (https://www.congress.gov/bill/114th-congress/senate-bill/973).

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.

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Appendix S1: Internet Appendix.
Replication Code.