# Do Venture Capitalists Stifle Competition?

Xuelin Li

Tong Liu

Lucian A. Taylor*

December 6, 2019

**Abstract**

We find that common ownership leads venture capital (VC) firms to stifle competition among startups, but only in limited circumstances. Our evidence is from pharmaceutical startups, where common ownership is widespread: 39% of startups share a VC with a close competitor. After a startup sees a close competitor make progress on a new drug project, the startup is less likely to advance its own project, and less likely to obtain VC funding, if the two startups share a common VC. These anticompetitive effects, however, are concentrated in markets with few competitors, VCs with larger equity stakes, and projects with similar technologies.

**Key words:** Venture capital, common ownership, competition, healthcare

*Li is at the University of Minnesota (Carlson). Liu and Taylor are at the University of Pennsylvania (Wharton). Emails: lixx3811@umn.edu, tongl@wharton.upenn.edu, luket@wharton.upenn.edu. We are grateful for comments from Bruno Biais and seminar participants at Queen's University and the University of Pennsylvania.

# Introduction

There is heated debate about whether common ownership reduces product-market competition. Azar et al. (2018a) and others find that companies compete less when they share a large investor, but other papers, discussed later, reach the opposite conclusion. We argue that the VC setting provides an important litmus test for the common-ownership hypothesis, because conditions among VCs and their startup companies are in many way "ideal" for the hypothesis to hold. The VC setting is also important in itself, as VC-backed companies generate a large share of the innovation in our economy (e.g., Kaplan and Lerner, 2010), and anticompetitive behavior could hinder this innovation. In this paper, we show that common ownership by VCs does stifle competition among startup companies, but only in limited circumstances.

The main prediction we test is that companies in the same product market compete less if they share overlapping investors. Competition erodes profits, so a common investor prefers that its portfolio companies compete less. Several conditions must be met, though, for this prediction to hold. Managers must care about investors' preferences, investors must care about externalities between firms, and investors must be attentive to whether managers' actions have improved portfolio value (Gilje et al., 2019). These conditions are highly likely to hold among VCs and their portfolio companies, because VCs typically own very large equity stakes in startups, they have significant control rights, they are sophisticated, active monitors, and VCs' portfolios are quite concentrated (e.g., Gompers et al., 2019). Also, we document that common ownership by VCs is widespread: 39% of startups in our sample have a close competitor with a shared VC investor. For all these reasons, we might expect the anticompetitive effects of common ownership to be even stronger in the VC setting than, for example, in the setting of passive index funds holding modest stakes in public airline companies (Azar et al., 2018a).

Testing the common ownership hypothesis in the VC setting poses three challenges. First, we cannot study price competition in the product market, as others have done, because many startups are not yet selling their products. We therefore take a different approach. Our ideal experiment would feature two pairs of competing startups. We would randomly assign one pair to share a common VC investor and the other pair to not. We would randomly shock one

"pioneer" startup in each pair to experience some observable form of success, and we would compare the outcomes of the two pairs' "lagging" startups. The common ownership hypothesis predicts that the lagging startup is less likely to succeed in the pair that shares a common VC. The reason is that a shared VC has an incentive to increase the market power of the pioneering startup, which it can do by holding back the lagging startup. A shared VC also wishes to avoid duplicating costs across the startups, which is another motive producing the same behavior.

Second, to apply this ideal experiment to real data, we need to identify startups that compete in the same product market, and we need clear, public signals of startups' success. We overcome this challenge by using project-level data on pharmaceutical startups. Our data cover 1,045 Phase I drug projects conducted by 481 U.S. startups financed by 775 VC firms, from 2005 to 2018. We work at the level of VC firms, so "VC" stands for a VC firm rather than a specific VC fund or partner. Our data partition the pharmaceutical industry into 76 highly detailed product markets, so we can compare, for example, a pair of startups with competing arthritis projects to a pair of startups with competing malaria projects. Regulation by the U.S. Food and Drug Administration (FDA) provides a clear, public signal of success: seeing a drug project progress from Phase I to Phase II clinical trials. Besides having these useful properties, our data cover a sector of the economy that is both highly valuable and important for social welfare.

The third challenge is finding quasi-random variation in whether two startups share a VC. We apply an instrumental variable (IV) approach that exploits the local nature of VC investing. VCs tend to invest in nearby companies in order to reduce the costs of search and monitoring. Our IV for whether two startups share a VC is based on the startups' geographic proximity. The main identification assumption is that geographic proximity affects our dependent variable—the outcome at a lagging startup after a competing pioneer startup makes progress—only through the effect of proximity on whether the two startups share a common VC.

Our first tests examine the probability that an individual Phase I drug project progresses to Phase II after seeing a closely competing project—a pioneer— progress to Phase II. We find that the lagging project is significantly less likely to progress to Phase II if it shares a VC investor in common with the pioneer project. This result is consistent with common ownership leading VCs

to stifle competition. As an extreme example, suppose a VC has two copies of the same project in its portfolio. If the VC sees one make early progress, then the VC has an incentive to kill off the lagging project in order to create a monopolist and avoid duplicating drug development costs. Our result's economic significance is high: the average effect of having a shared VC is comparable to the unconditional rate of progressing to Phase II. Our result holds in ordinary least squares (OLS) regressions as well as IV and bivariate probit regressions. We find our result even upon including fixed effects for time by drug category, which amounts to comparing how two lagging arthritis projects (for example) react differently to seeing a third arthritis project reach Phase II, depending on whether the two lagging projects share a VC with the pioneer. The result is also robust to collapsing our panel to a single observation per drug project, and to using project suspension as our dependent variable.

How does sharing a VC affect drug projects' outcomes? We find evidence consistent with a VC financing mechanism. A VC can hold back a drug project by choosing not to make a follow-on investment in the project's company. We predict that after a VC sees a closely competing startup make progress, the VC is less likely to extend funding to a startup if the VC is invested in both startups. The reason, as before, is that a common VC owner seeks to create market power and avoid duplicating costs. The data strongly support this prediction, with high levels of statistical and economic significance. We find evidence of a VC financing mechanism even if we compare different VCs invested in the same startup and quarter, which effectively controls for a startup's demand for funding. Specifically, we find that relative to other VCs invested in the same startup and quarter, a given VC is less likely to make a follow-on investment if that VC is also invested in a close competitor that has recently made progress. When a common VC abandons a startup, other VCs do not step in to fill the financing hole. When a startup with common ownership experiences a shock, new VCs are significantly less likely to invest in the startup, and the startup's other existing VCs—those without common ownership—do not significantly increase their investment. The net effect is that the startup is less likely to raise financing from *any* VC.

The results above describe average outcomes in our sample. We find that anticompetitive

behavior, however, is limited to a fairly narrow subset of our data. Our project-outcome results hold only in the roughly 25% of our sample containing drug categories without many competing projects or firms. In other words, we find anticompetitive behavior only in product markets that feature low levels of competition. Intuitively, a VC has little incentive to suppress one drug project to boost another if there exist dozens of other competing projects, because there is little hope of creating market power. We also find that the frequency of common ownership is lowest in the less-crowded product markets. An important implication is that common ownership seems to have anticompetitive effects only where common ownership is rare. We also find that anticompetitive behavior is limited to VCs that have a high level of influence over the startup. Specifically, our VC-financing results are significant only for the subsample of VCs that have a relatively large financial stake in the startup. It makes sense that a VC owning a very small stake probably lacks the influence needed to hold back a startup. Finally, we find stronger evidence of anticompetitive behavior if the lagging and pioneering projects share overlapping patent citations, suggesting products must be very close technological substitutes in order to see strong anticompetitive effects. Overall, these results suggest that several conditions must hold in order for common ownership to have anticompetitive effects, which strongly limits VCs' ability to stifle competition.

We address various challenges to our identification strategy. One potential concern is that geographic proximity just captures technological proximity. We show that our results are robust to controlling for a measure of technological proximity. We also explain that this bias, if anything, should work against our findings. Geographic proximity could also reflect easier information sharing, but we again argue this should work against our findings. Geographic proximity could also be picking up the ease of poaching employees from a competing startup, which in theory could explain why a project is more likely to fail after a nearby competing project gains an edge. Inconsistent with that story, however, our result is not weaker when the startup's state strongly enforces employee non-compete agreements.

Important caveats are in order. VCs may not explicitly intend to limit competition when they deny funding to a lagging company. Instead, a VC may simply have limited capital to

invest, and the leading company crowds out the lagging company. Even without an explicit intent to stifle competition, however, the consequence is the same: common ownership by VCs limits competition. Even if VCs do limit competition, it is not necessary bad for social welfare. A social planner might also find it optimal to reduce competition in order to avoid duplicating drugs' very large development costs. Common ownership can also strengthen incentives within startups by creating a form of tournament between a VC's portfolio companies. Finally, this paper only studies common ownership's ex post effects. Common ownership may have ex ante effects that actually increase competition. A VC may be more willing to invest in a pair of startups ex ante knowing that, ex post, the VC can back the leading company and discourage the lagging company.

Theories in which common ownership reduces competition date back at least to Bresnahan and Salop (1986) and Reynolds and Snapp (1986). Three recent papers empirically support this prediction. Azar et al. (2018a) and Azar et al. (2019) find evidence of anticompetitive behavior in the pricing of airline tickets and banking products, respectively. He and Huang (2017) find that common ownership increases market share growth and profitability, consistent with common ownership facilitating product market coordination. These findings are controversial, however. Dennis et al. (2019) and Kennedy et al. (2017) question the airline industry findings of Azar et al. (2018a), prompting a response by Azar et al. (2018b). Gramlich and Grundl (2017) and Lewellen and Lowry (2019) also find that the evidence of anticompetitive effects is not robust to using alternative methods. Kini et al. (2018) find that common ownership can actually increase competition by encouraging investments that have industry spillovers. Gilje et al. (2019) show that decreases in investor attention can weaken the anticompetitive incentives created by common ownership. Lewellen and Lewellen (2017) also challenge whether common ownership creates anticompetitive incentives. Like us, Newham et al. (2018) and Gerakos and Xie (2019) examine how common ownership affects competition in the pharmaceutical industry. Unlike us, they focus on public firms, mature products, non-VC investors, and entry by generic drugs.

We contribute to the common ownership literature by studying the VC industry. VC provides an important litmus test for the common ownership hypothesis, because conditions in VC are

"ideal" for common ownership to have anticompetitive effects. Despite these ideal conditions, we find that anticompetitive behavior is quite limited and depends on the levels of product market competition, investor influence, and product similarity. The VC setting also features a distinct, interesting economic mechanism: Common ownership by VCs affects future market structure by influencing which startups survive. In contrast, existing papers take market structure as given and study how common ownership influences price competition.

Whereas we emphasize the negative effects of sharing a common VC, other papers emphasize positive effects. Startups sharing a common VC are more likely to form alliances (Lindsey, 2008) and share innovation resources (González-Uribe, 2019). Closer to our paper, Eldar et al. (2019) ask whether common ownership by VCs affects startup performance. Our papers complement each other by using very different data and identification strategies. One advantage of Eldar et al. (2019) is that their data span all industries, making it easier to generalize results. An advantage of using only pharmaceutical industry data, however, is that it lets us better identify firms and projects that are close competitors. For example, we assume a malaria drug only competes with other malaria drugs. Eldar et al. (2019) instead use a coarse industry classification that, for instance, combines all pharmaceutical companies into one category, thereby assuming malaria drugs also compete with arthritis and cancer drugs. More important, our papers reach different conclusions. Eldar et al. (2019) find that common ownership benefits startup growth, whereas we find that common ownership— in limited circumstances— leads VCs to suppress a weaker startup in order to benefit a stronger one.

Our results also relate to the killer acquisitions documented by Cunningham et al. (2019). Those authors show that an acquired drug project is less likely to progress if the acquirer has a similar drug project, especially if preexisting competition is weak. Similarly, we find that a drug project is less likely to progress if it shares a VC in common with a similar pioneer drug project, especially if preexisting competition is weak. The distinction between an acquirer and a shared VC is important, though. We study how a financial intermediary, the VC, can affect product market competition, whereas Cunningham et al. (2019) is not about financial intermediaries. Cunningham et al. (2019) instead focus on a company's incentive to acquire a

direct competitor, which is quite different. Acquirers have much more control than VCs, which are typically minority shareholders. Also, VCs presumably do not invest in startups with the intent to kill them.

The rest of the paper is organized as follows. Section 1 describes our data, predictions, and identification strategy. Section 2 contains our empirical results on project outcomes, financing decisions, and limits on anticompetitive behavior. Section 3 discusses robustness and identification challenges. Section 4 concludes.

# 1 Empirical approach

## 1.1 Data and institutional details

Drug development in the U.S. is strictly regulated by the FDA and follows several stages: discovery, and Phase I, II, and III clinical trials. The discovery stage involves pre-clinical research for drug candidates in the laboratory and animal testing for basic safety. Phase I tests the safety and determines dosage of a drug candidate on a small group of humans. Phase II tests efficacy and side effects at a larger scale. Phase III, which involves thousands of participants, tests whether there is a treatment benefit. A project progresses from one phase to the next only if results are positive.[1]

We construct a sample of pharmaceutical projects initiated by U.S.-based companies that are funded by VCs. Detailed information on drug development and clinical trials comes from the Cortellis Life Sciences Healthcare Database. Cortellis obtains its data from public records, such as clinical trial registries, FDA submissions, patent filings, company press releases, and financial filings. We provide additional information on the Cortellis database in the Online Appendix. Cortellis records drug development history at the project level. A project is a sequence of trials for testing the safety and efficacy of a drug targeting a specific indication. An indication is a

[1]More details regarding the drug development process are on the FDA website, https://www.fda.gov/patients/learn-about-drug-and-device-approvals/drug-development-process. Our stage definitions differ somewhat from the FDA's definitions, because Cortellis, our data provider, combines certain FDA stages. For example, Cortellis combines the FDA's "Discovery and Development" and "Preclinical Research" stages into a single "Discovery" stage. Cortellis also combines the FDA's "Phase 3" and "Phase 4" into "Phase III."

specific disease or medical condition. From Cortellis we collect the dates when projects progress through the various clinical phases, and we also collect each project's suspension date (in the event of ultimate failure) or FDA drug approval date (in the event of success). Cortellis also provides detailed information on the company running the project, including the company's name, funding status, organization type, headquarters location, and major shareholders. It also provides detailed information on the portfolio of patents associated with the drug. We also scrape each patent's application date, grant date, and patent citation information from Google Patent, using the patent number from Cortellis.

We obtain VC investment records from SDC Platinum VentureXpert. We use a fuzzy matching algorithm to link funding records in SDC to company names and locations in Cortellis. For each matched pair, we manually check and ensure the matching accuracy. Companies' eventual outcomes (e.g., IPOs) in VentureXpert are sometimes missing or incorrect, so we update and cross-validate them. Specifically, for the set of companies that go public, we use Jay Ritter's IPO database to supplement missing IPO dates. For the subset of companies that are acquired, we use the SDC M&A records to determine the M&A completion date.

We apply several filters to obtain our final sample. First, we require both the startup and its VC investors to be in the U.S. We apply this filter because of data availability. Fortunately, the U.S. accounts for a very large share of global pharmaceutical R&D.[2] Second, we drop projects initiated before the first quarter of 2005, because Cortellis's coverage of development histories is less reliable before then. Third, we drop projects initiated after the first quarter of 2016 if no progress or suspension is observed. We do so because insufficient time has passed for these projects to reach any outcome. Next, given our focus on VC, we only include quarters when a given project is held within the portfolio of a VC.[3]

We only include quarters when a project is in Phase I trials, for several reasons. For 86% of our sample projects, the startup raises its first VC money when the project is in either the

[2]In 2016, the U.S. accounted for 58% of pharmaceutical R&D among 11 leading countries (ABPI, 2019).

[3]This period lasts from the first quarter when its drug developer receives its initial VC funding until either the company exits successfully (e.g., IPO or being acquired) or is written off. Following González-Uribe (2019), absent any exit, we say a startup is written off five years after its last VC financing round. If that date exceeds 2018Q4, we keep the project in our sample but record its final outcome as missing. Our main results are robust to using other cutoffs (e.g., 3, 7, and 10 years) to define the write-off date.

discovery or Phase I stage. We must therefore look at least as far back as Phase I to capture the effects of VCs. We exclude the discovery stage, because it requires significantly less capital, tends to last a short period of time, and is often not reported to the public or Cortellis. Also, VCs have greater control over the startup during projects' early stages. By the time a project reaches Phase II or III, the startup will often raise financing via an IPO or M&A, which dilutes or removes the VCs' influence. Since we want to study how VCs influence startups, we restrict ourselves to the stage when that influence is most relevant. Finally, Phase I is inherently important. Progressing from Phase I to Phase II increases a drug's probability of FDA approval from 10% to 31% (Bio et al., 2016). Given projects' high attrition rates at each development stage, a major portion of projects alive at any given moment are in Phase I. Like us, Cunningham et al. (2019) also focus on Phase I in their analysis of drug projects' progression through clinical trials.

Our analysis requires identifying drug projects that are close competitors. We do so by partitioning the overall pharmaceutical industry into 76 highly detailed sub-markets. We map Cortellis indications to the second chapter level of International Classification of Diseases ($9^{th}$ Revision, "ICD"), and we refer to each chapter as an ICD category.[4] Examples of large ICD categories include malignant neoplasm of hematopoietic tissue (e.g., bone marrow cancer), arthropathies and related disorders (e.g., arthritis), and anemia. Examples of small ICD categories include urinary system diseases, injury to blood vessels, and Kaposi's sarcoma. Some ICD categories combine indications with unspecified sites, unknown causes, or uncertain effects. The diseases in these "miscellaneous" categories can be quite different from each other, so we drop these categories and the projects in them. After we apply this filter, indications in the same ICD category have a high pathological correlation, so drug projects in the same ICD category are plausibly close substitutes and hence close competitors.

Finally, we drop a project if none of its VC investors are disclosed. These filters together produce a final sample of 59,839 VC-project-quarter observations from 1,045 projects across 76 drug categories. The projects are run by 481 startup companies funded by 775 VC firms. The

[4]The ICD handbook has 156 categories at the second chapter level. Only 118 of these categories appear in our unflitered data. After applying our various filters, the sample includes 76 categories. We do not use the indication categories from Cortellis to define competing projects, because indication names are not standardized and are sometimes too specific or too vague. Sometimes two distinct indication categories in Cortellis refer to highly similar conditions. Therefore, a certain level of indication aggregation is necessary.

sample covers 2005 to 2018.

### 1.2 Predictions and identification strategy

We predict that two drug projects in the same ICD category will compete less if they share a common VC investor. We study one way in which two projects can compete less: The project that shows early promise is allowed to progress to Phase II clinical trials, while the lagging project is suspended or at least discouraged from progressing. A common VC investor has two incentives to reduce competition in this way. By suspending one project, the VC avoids duplicating the large costs involved in taking a drug through clinical trials. Also, a single drug with a monopoly is worth more than two competing drugs, so the common VC has an incentive to "kill off" one of the two competitors.[5] Later, we also study whether the common VC denies funding to the lagging project's company, which is another way to suppress competition.

Our tests seek to approximate two ideal experiments. The first features two pairs of drug projects. The projects within each pair are in the same ICD category, so each pair contains close competitors. One pair is randomly assigned to share a common VC investor, the other not. Within each pair, we shock one project with success, defined as progressing from Phase I to Phase II. We label the shocked project the "pioneer," and we study the outcome of the two pairs' non-shocked project, which we label the "lagging" project. We predict that the lagging project is less likely to progress to Phase II in the pair with the common VC, compared to the lagging project in the pair without the shared VC. More simply, having a common VC makes it more likely that a lagging project gets held back. The null hypothesis is that common ownership is irrelevant, meaning the two lagging projects' outcomes are no different across the two pairs.[6]

One challenge with implementing this experiment is finding a "success" shock that is similar across the two pairs of projects. To mitigate this concern, we also consider a second ideal experiment that compares how two lagging projects respond to exactly the same shock. A

[5]Fulghieri and Sevilir (2009) generate a similar prediction, which is that VCs have an incentive to invest in technologically similar companies, because it lets the VC shift resources from one portfolio company to another in case one company fails. In contrast, our mechanism does not require VCs to shift resources across startups.

[6]Similar to us, Krieger (2017) studies how drug projects react to the failure of a competing project. We add an extra "diff" to the analysis by comparing the reaction between pairs that share a common VC and pairs that do not.

single pioneer project is shocked with success, and we compare the outcomes of multiple lagging projects in the same ICD category. One lagging project is randomly assigned to share a VC with the pioneer, and the other lagging project does not. We predict that the lagging project with a shared VC, compared to the lagging project without a shared VC, is less likely to progress to Phase II.

We can express these experiments in regression form as follows:

$$Progress_{it} = \gamma\, Shocked_{it} + \beta\, Shocked_{it} \times SharedVC_{it} + FEs + Controls + \eta_{it}. \qquad (1)$$

The sample includes all projects $i$ that are in Phase I at the beginning of quarter $t$. $Progress_{it}$ is an indicator for whether project $i$ progresses to Phase II in quarter $t$. $Shocked_{it}$ is an indicator for whether drug project $i$ has experienced a shock, meaning a different project in the same ICD category (but different startup) has progressed to Phase II between project $i$'s initiation quarter and quarter $t-1$. The indicator $SharedVC_{it}$ equals one if project $i$ and the project causing the shock share a common VC in quarter $t$. Note that $SharedVC_{it}$ cannot be included as an extra regressor, because it is only defined when $Shocked_{it} = 1$. Without ICD $\times$ quarter fixed effects ($FEs$), the regression implements the first ideal experiment, in which we test how lagging projects respond to potentially different shocks. If we instead include ICD $\times$ quarter FEs, we isolate variation within a given ICD category and quarter, so we test how multiple lagging projects respond to the same shock. In other words, we implement the second ideal experiment.

The coefficient of interest is $\beta$, which measures how lagging projects' outcomes depend on whether there is a shared VC. We predict $\beta < 0$, meaning a project is less likely to progress to Phase II, after receiving a shock, if there is common ownership.

The main implementation challenge is finding quasi-random variation in $SharedVC$, the indicator of common VC ownership. We exploit the local nature of VC investing. VCs prefer investing in nearby companies to reduce the costs of screening and monitoring. Also, Gompers et al. (2019) show that VCs rely heavily on their networks to source deals, and networks are often local. We use the geographic proximity of two startups to create an instrument for whether

they share a common VC investor. Specifically, we create an instrument for the endogenous interaction term $Shocked_{it} \times SharedVC_{it}$ as follows. For project $i$ and quarter $t$, we collect the set $S_{it}$ of projects $j$ that are in the same ICD category as $i$, and that progressed from Phase I to Phase II between the birth of project $i$ and quarter $t-1$. These projects $j$ are the pioneers that shock project $i$. If there are multiple pioneer projects, we include in $S_{it}$ only those projects from the most recent year before quarter $t-1$. Let $d_{ij}$ denote the distance, in miles, between the Metropolitan Statistical Areas (MSAs) of companies' headquarters for those companies owning projects $i$ and $j$. We scale the distance by a constant of 2,600 miles, which is roughly the air travel distance from Boston to San Francisco. This scaling helps produce a strong first-stage regression. Given the scaled distance $d'_{ij}$, we compute $f\left(d'_{ij}\right) = \exp(-d'_{ij})$ as a proxy for the probability that projects $i$ and $j$ share a common VC. Our IV for the variable $Shocked_{it} \times SharedVC_{it}$ is then

$$Shocked_{it} \times Proximity_{it} = \sum_{j \in S_{it}} \exp(-d'_{ij}). \tag{2}$$

We set the instrument to zero if the set $S_{it}$ is empty, meaning $Shocked_{it} = 0$. We compute a sum in equation (2) because common ownership is more likely if there are multiple nearby pioneer projects. Summation is not driving our results, however; our results are actually stronger if we control for the number of pioneer projects in set $S_{it}$. We show later that geographic proximity strongly explains common ownership, and the first stage of our IV regressions is quite strong. The relevance condition holds for this instrument, in other words.

Our main identifying assumption is that the instrument affects the outcome variable $Progress_{it}$ only through its effect on $Shocked_{it} \times SharedVC_{it}$. Why does this exclusion restriction plausibly hold? Our IV test boils down to checking whether, if the pioneer and lagging projects are geographically near each other, they are more likely to have opposite outcomes. To see this, note that geographic proximity increases the likelihood of a shared VC (our regression's first stage), and we predict that a shared VC makes it more likely that the lagging project fails after the pioneering project succeeds— opposite outcomes. Reverse causation is not an issue here, because the startups' location is determined well before the projects' outcomes. The bigger concern is

omitted variable bias, meaning there exists some omitted variable $W$ that is correlated with both geographic proximity and the projects' achieving opposite outcomes. No such $W$ is apparent to us. For example, $W$ cannot be unobserved similarity between the projects. Nearby companies are more likely to have unobserved similarities, for example, because the scientists and managers have similar backgrounds. Unobserved similarity, however, predicts *similar* project outcomes, whereas we predict *opposite* project outcomes.[7] If anything, this force biases us against finding support for our prediction. Also, $W$ cannot be an unobserved negative correlation between the projects' outcomes. To reduce portfolio risk, VCs may indeed prefer investing in two companies with negatively correlated outcomes, and a negative correlation could explain projects' opposite outcomes. This is exactly why we need an instrument for $SharedVC$. There is no reason why geographic proximity should coincide with negative correlation. If anything, proximity should coincide with a positive correlation, due to unobserved project similarities. Section 3 addresses other possible identification challenges.

Our main regression (1) shares some features in common with a "triple-diff" regression. It examines projects over time (the first diff), depending on whether the project experiences a shock (the second diff), and also depending on whether the projects share a VC (the third diff). Unlike a standard triple-diff regression, though, our regression relies on a continuous instrument to achieve quasi-random assignment in $SharedVC$. Another important difference is that we can only determine whether a pair of projects shares a VC if a shock occurs.

# 2 Empirical results

## 2.1 Summary statistics and frequency of common ownership

Figure 1 shows the geographic distribution of drug projects in our sample. As expected, there are many projects in California, Massachusetts, and New York. Approximately 46% of projects,

[7]For instance, if the two projects' scientists live nearby and have similar training or experience, then the science behind the drug projects is more likely to be similar. If the science is similar, then the outcome of Phase I clinical trials should be similar, because this phase depends solely on the drug's scientific characteristics. Specifically, Phase I involves giving the drug to a small number of healthy volunteers with the goals of assessing safety and understanding how the drug is metabolized. Also, we show later that our conclusions are robust to controlling for a measure of technological similarity.

however, are located outside these states. Many projects are in states with a strong medical research background, such as Illinois, Maryland, North Carolina, Pennsylvania, and Washington.

Table 1 shows summary statistics for our main variables in the full sample as well as "Never Treated" and "Ever Treated" subsamples. The "Never Treated" group contains projects/companies for which $Shocked \times SharedVC$ equals zero across their entire lifespan. Remaining projects/companies are in "Ever Treated."

Panel A shows project-level variables. The full-sample mean of *Shocked* is 0.519, meaning in roughly half of our sample a project has seen a close competitor progress to Phase II. *Progress* is roughly three times higher in the "Never Treated" group, which foreshadows our main result that sharing a VC with a competing, successful project significantly reduces the likelihood of progressing to Phase II.

Panel B contains company-level variables. In a typical quarter, the average startup has 1.66 projects covering 1.42 ICD categories. A typical startup will run several projects over its lifespan, so the total number of projects per startup is much higher than 1.66. The typical startup has 4.8 VC firms invested in it at a given point in time, although there is wide variation in this number.

Next, we show that common ownership by VCs is quite common. To start, Table 2 shows that 93% of startups in a typical quarter have a close competitor, defined as another startup developing at least one drug in the same ICD category at the same time. Having a close competitor is necessary for common ownership to have any anticompetitive effects. We find that a sizeable 39% of our startups have a close competitor that shares at least one VC in common, in a typical quarter. At first glance, this rate of common ownership seems high given that our sample contains 775 distinct VC firms. The rate is high for a few reasons. First, the average startup has 4.8 VCs, so there is a decent chance that at least one VC is shared with a competitor's (multiple) VCs. Also, Table 2 shows that VCs specialize in specific ICD categories. While a VC could potentially invest across all 76 drug categories, the typical VC holds stakes in just 2.5 categories in the typical quarter, making it more likely that the VC holds multiple companies in the category. Put differently, while there are 775 VCs in our full sample, the

typical ICD category has only 16.6 VCs invested in it at once, increasing the chances of VC overlap. Finally, some ICD categories contain many startups, making it easier to find at least one other competing startup that shares a VC. Supporting this idea, Table 2 shows that the rate of common ownership is more than twice as high in the drug categories that contain many projects compared to categories containing few projects. Common ownership, in other words, is concentrated in drug categories that are already quite competitive.

### 2.2 Evidence from project outcomes

Our main results begin in Table 3, which contains results from the panel regression in equation (1). The dependent variable in columns 1, 4, and 6 is $Progress_{it}$, the indicator for whether project $i$ progresses to Phase II in quarter $t$. Within each project, $Progress$ equals zero until the project's last observation, which is either one or zero (if the project is suspended or we reach the end of the sample). We include fixed effects (FEs) for the startup company, ICD category, and year-quarter to control for unobserved heterogeneity.[8] We exclude ICD×quarter FEs, so these regressions approximate the first ideal experiment discussed above. We include three controls: the age of the project ($Age$), number of Phase I projects being developed by the startup company in the given quarter ($NProjects$), and number of VC firms owning a stake in the startup company in the given quarter ($NVCs$). We find that $Age$ is a particularly important control: older projects are more likely to progress to Phase II. Standard errors are computed by two-way clustering at the ICD category and startup levels.

The first column contains OLS estimates. This regression does not have a causal interpretation, but it provides a useful description of the data. Since the dependent and independent variables of interest are indicators, the slope coefficients can be interpreted as average changes in probability. The OLS results point in the predicted direction: sharing a common VC is associated with a 0.016 lower probability that, if a similar project makes progress, the project progresses to Phase II during the quarter. The unconditional average of $Progress$ is also 0.016,

[8]The ICD category FEs are useful because it may be more difficult to reach Phase II for certain diseases. The quarter FEs controls for aggregate changes in the FDA process, conditions in the macroeconomy or pharmaceutical industry, etc. The startup company FEs are useful, because the typical startup company runs several projects over its lifespan, and some companies may be systematically better at getting projects through Phase I. We therefore focus on variation in projects within companies.

so the estimated coefficient is economically large. It is also highly statistically significant, with a $t-$statistic of $-3.4$. The insignificant slope on *Shocked* indicates that, absent a shared VC, seeing a related project make progress makes it neither more nor less likely that the project itself makes progress.

Our first IV test is in columns 2–4. Since the focal regressor, $Shocked \times SharedVC$, is a binary variable, two stage least squares (2SLS) is generally imprecise and results in coefficients with large magnitudes. Wooldridge (2010) and Angrist and Pischke (2008) instead recommend a Probit-2SLS procedure.[9] In the first step (column 2), we estimate a probit regression of the endogenous regressor ($Shocked \times SharedVC$) on the instrument ($Shocked \times Proximity$) and exogenous controls.[10] The probit model produces predicted probabilities $\widehat{Shocked \times SharedVC}$, which we use as the instrument in the first stage of the 2SLS procedure (column 3). Probit models do not easily accommodate a large number of FEs, so we only introduce the FEs in the first stage of the 2SLS procedure.

As expected, in column 2 we see a strong, positive relation between $Shocked \times Proximity$ and $Shocked \times SharedVC$, consistent with VCs preferring to invest in nearby companies. The $t$-statistic is 4.8 in column 2 and declines to 2.5 in column 3's first-stage regression. The first-stage $F-$statistic, shown at the bottom of the table, rejects the null of a weak instrument.[11] The IV estimate in column 4 echoes the OLS results. Consistent with our prediction, after seeing a similar "pioneer" project progress to Phase II, a project is less likely to progress to Phase II if it shares a VC with the pioneer project. This result is consistent with common ownership leading VCs to suppress competition among similar projects. Statistical significance decreases from column 1 to 4, but economic significance is much larger in column 4. Jiang (2017) shows that it is is common for IV estimates to be much larger than their OLS counterparts. As discussed above, we expect the IV estimate to exceed the OLS estimate, because unobserved similarities between the pioneering and lagging project tend to make the projects' outcomes similar, biasing

[9] See, for example, page 191 in Angrist and Pischke (2008). This approach has been used recently by Dinc and Erel (2013), Saretto and Tookes (2013), and Ewens and Marx (2018).

[10] We omit *Shocked* from column 2 because its slope is not identified in the probit model. If $Shocked = 0$, then $Shocked \times SharedVC$ is zero with probability one, so the probit model wants to assign *Shocked* a slope of positive or negative infinity.

[11] To test for weak instruments, we report the Kleibergen-Paap Wald statistic, which is the robust, multivariate analogue of the Stock and Yogo (2005) first-stage $F-$statistic.

the OLS slope toward zero.

Columns 5 and 6 show that our result is robust to using a bivariate probit model instead of 2SLS. Bivariate probit models are suited to IV settings like ours, in which the dependent and endogenous independent variables are both binary. Similar to 2SLS, in the bivariate probit model our main identifying assumption is that the instrument, $Shocked \times Proximity$, is uncorrelated with the error term in the latent index that determines $Progress$.[12] The bivariate probit estimates again support our prediction. After a similar project experiences success, a lagging project is less likely to progress if it shares a VC with the pioneering project. Having a shared VC reduces the probability of progressing by 0.048, which is again economically large. The coefficient is significantly different from zero at the 10% level but not the 5% level. An advantage of the bivariate probit model over 2SLS is that it recognizes that our dependent and independent variables are binary. A disadvantage of the bivariate probit model is that it imposes a strong distributional assumption on the data, namely, that error terms are normally distributed. A bigger disadvantage is that bivariate probit does not easily accommodate the large number of FEs in our 2SLS specification, so we omit all FEs in columns 5 and 6. Despite these shortcomings, we continue to report bivariate probit results alongside 2SLS results when possible.

Table 4 approximates our second ideal experiment, which asks how two projects respond to the same shock. We now include ICD×quarter FEs in order to isolate, for example, variation across different Phase I malaria drug projects in a given quarter. If a malaria project progresses to Phase II in quarter $t-1$, then all remaining Phase I malaria projects in quarter $t$ would have $Shocked = 1$. (Note the project that progresses to Phase II during quarter $t-1$ is no longer in our sample during quarter $t$.) The variable $Shocked \times SharedVC$ would therefore only pick up variation in $SharedVC$ across remaining Phase I malaria projects in quarter $t$, and the regression would test how those remaining projects respond differently to the same shock depending on whether they share a VC with the shocked project. Note that the slope on $Shocked$ is still identified in our regression, because $Shocked$ can vary within ICD×quarter. This variation results from projects initiating in different quarters, and $Shocked$ measuring whether

[12] Bivariate probit models are discussed by Angrist and Pischke (2008), pages 197–205, and Wooldridge (2010), pages 594–599.

each project experiences any shock since its initiation, not necessarily the previous quarter. Since *Shocked* can still vary somewhat within ICD×quarter, we continue to control for *Shocked*, and we acknowledge that we only approximate the second ideal experiment.[13] Our bivariate probit tests omit fixed effects and are therefore identical to those in Table 3, so we omit them here.

The second experiment produces results similar to the first. Most important, the slope coefficients on $Shocked \times SharedVC$ are significantly negative, consistent with our prediction. Statistical and economic significance in Table 4 are lower for the OLS results and higher for the 2SLS results, in part because the first stage regression is stronger in Table 4, column 3.

To summarize, results on project-level outcomes are consistent with our prediction that common ownership leads VCs to reduce competition between competing projects. Specifically, we find that common ownership makes it less likely that a lagging project progresses to Phase II after a competing project achieves success. This result goes through whether we look at how projects respond to different shocks (Table 3) or respond to the same shock (Table 4). The result holds in OLS, IV, and bivariate probit specifications.

### 2.3 A VC financing mechanism

How can a VC limit competition between two projects or firms within its portfolio? We investigate a mechanism related to VC financing. Specifically, by extending financing to one company but not the other, the VC can potentially limit competition between them. Startups typically have negative cash flows, so they will eventually shut down if they cannot raise external finance. By starving one company of funding, a VC can potentially increase the other company's market power.

For this mechanism to work, a startup that loses funding from one VC cannot easily raise

[13] To get closer to the ideal experiment, we could redefine variables so that they look back over a constant rather than a project-specific time window. *Shock* would then be constant within ICD×quarter, so its slope would not be identified. That alternative timing convention is poorly suited to our setting, for two reasons. Many projects are quite young, so their time window needs to be quite short to avoid looking at periods before the project was even initiated. At the same time, we want to allow other projects to have a long window, because it can take considerable time for a project to progress to Phase II or be suspended. For example, after a VC decides to starve a startup of cash, it could take a long time for that company to run out of cash and finally suspend its projects. Our main specification allows this flexibility in timing. For robustness, we repeat this analysis after redefining variables to look back over a fixed time window. Results are in the Online Appendix. Some of our results become stronger, others weaker.

funding from another source, including other VC firms. VCs' private information makes this condition likely to hold, at least in some situations. A startup will typically raise multiple rounds of financing from one or more VC firms. After investing in and working with a startup, a VC gains private information about the company. A VC's choice to stop funding a startup can send a negative signal, leading other VCs to either avoid funding the startup or offer unfavorable deal terms. This condition will not always hold, though. In some situations, a startup can easily find alternative funding, so this mechanism should not work.[14] In the next subsection, we explore heterogeneity in the strength of this mechanism, and we show that our results are stronger where we expect them to be stronger.

To test this mechanism, we consider a variation on our two ideal experiments. As before, we consider two pairs of similar drug projects, and one project (the "pioneer") within each pair progresses to Phase II. In the ideal experiment, we would randomly assign one pair of projects to share a VC and the other pair to not share a VC. Whereas we previously studied whether the lagging projects progress to Phase II, we now study whether the startup companies running those lagging projects receive VC funding. We predict that, if there is a shared VC, then that VC is less likely to fund the lagging project's company. We can express this experiment via the following regression:

$$ExtendFunds_{ijt} = \gamma\, Shocked_{ijt} + \beta\, Shocked_{ijt} \times SharedVC_{ijt} + FEs + Controls_{ijt} + \eta_{ij}, \quad (3)$$

where $ExtendFunds_{ijt}$ equals one if VC $j$ extends funding to startup $i$ in quarter $t$. To create a company-level rather than project-level variable, we redefine $Shocked_{ijt}$ to equal one if at least one project in startup $i$ has seen another project (a "pioneer") in the same ICD category (but different startup) progress to Phase II between the quarter when VC $j$ invests in startup $i$ and quarter $t-1$. $SharedVC_{ijt}$ equals one if VC $j$ holds a stake in both startup $i$ and at least one of the startups owning the pioneering projects that produced $Shocked_{ijt} = 1$. To create a

[14] Two other papers study signaling in VC financing decisions. Khanna and Mathews (2016) show theoretically that a VC can send a positive signal about a startup to third parties (not other VCs) by investing at a high price. Ewens et al. (2016) show empirically that "inside" VC financing rounds predict worse startup outcomes, which is inconsistent with VCs using their private information to hold up portfolio companies.

company-level version of our instrument, $Shocked_{ijt} \times Proximity_{ijt}$, we modify equation (2) so that we sum our proximity measure over all the pioneering projects that shock any of company $i$'s projects. We also include controls for the number of startups and ICD categories in the VC's portfolio, the size of and time elapsed since the startup's previous financing round, the number of VC firms invested in the startup, and the number of projects owned by the startup. We include VC firm FEs (because some VCs invest more frequently than others), startup FEs (because some startups raise more VC rounds than others for unrelated reasons), and year-quarter FEs (to soak up aggregate unobservables). We predict $\beta < 0$, meaning a VC is less likely to extend funding to a startup, after the startup sees a competitor make progress, if the VC is invested in both companies.

Support for this prediction is in Table 5. The OLS slope coefficient on $Shocked \times SharedVC$ is indeed negative and statistically significant at the 5% level. The coefficient's magnitude, $-0.022$, is quite large relative to the 0.064 unconditional mean of $ExtendFunds$. Column 3 shows a very strong first-stage regression, and column 4's 2SLS slope on $Shocked \times SharedVC$ is negative, large in magnitude, and statistically significant at the 1% confidence level. Results from the bivariate probit model also strongly the prediction. Overall, these results support the idea that VCs withhold funding from a startup if a commonly owned close competitor has made progress.

The large negative slope on $Duration$ in Table 5 deserves comment. $Duration_{ijt}$ is the log of one plus the number of years since VC $j$ last funded startup $i$. VCs—especially early-stage ones—will often fund a company for a few rounds, then stop participating in rounds once the round sizes get too large. The negative slope on $Duration$ likely reflects that, once a VC has stopped funding a company, the VC is unlikely to start funding it again.

One potential concern with the previous test is that a startups' demand for funding, an omitted variable, may differ depending on whether there is a shared VC or on geographic considerations. Next, we slightly change the previous regression to control for a startup's demand for funding. We do so by adding startup $\times$ quarter fixed effects, which isolate variation across VC investors within the same startup-quarter. For example, suppose startup $i$ has two existing VC investors, $A$ and $B$, and startup $i$ sees a competing startup make progress. VC $A$ is invested

in both startups, but VC $B$ is only invested in startup $i$. Our regression tests whether VC $A$ is less likely than $B$ to extend funding to startup $i$ in the given quarter. The common ownership hypothesis predicts that $A$ is indeed less likely to extend funding, meaning the slope on $Shocked \times SharedVC$ is again negative. The results in Table 6 support this prediction. The slope on $Shocked \times SharedVC$ is significantly negative in both the OLS and probit-2SLS specifications. (Our bivariate probit tests omit fixed effects and are therefore identical to those in Table 5, so we omit them here.) The OLS slope, $-0.024$, is slightly larger in magnitude compared to the previous table, but the 2SLS slope is smaller.

One potential challenge to the financing mechanism is that other VCs could step in and fill the hole left by a VC who abandons the startup. Table 7 shows this not to be the case. When there is a shock to a startup with common ownership, the startup is significantly less likely to raise money from *any* VC (column 1), and the total dollar amount raised from VCs is significantly lower (column 2). These results indicate that new VCs, or existing VCs without common ownership, do not fill the hole left by the existing VC with common ownership. In fact, column 3 shows that new VCs are significantly less likely to invest in a startup when there is a shock to a startup with common ownership. One interpretation is that it sends a negative (but noisy) signal about the startup when an existing VC with common ownership abandons the startup, and this negative signal deters new VCs from investing in the startup. Column 4 tests whether the existing non-common VCs offset the effects of the common VC. The regressor of interest is $Shocked \times SharedVC \times NonCommonVC$, an indicator for whether startup $i$ is shocked by a pioneer, at least one of startup $i$'s VCs is shared with the pioneer, and VC $j$ is not the shared VC. We find no significant relation between this regressor and $ExtendFunds$, an indicator for whether VC $j$ extends funds to startup $i$ in quarter $t$. This result implies that existing non-common VCs do not significantly offset the common VC's reduction in investment.

To summarize, the previous three tables are consistent with a VC financing mechanism by which common ownership affects project outcomes. An existing VC with common ownership is less likely to fund a startup that is lagging behind a (commonly owned) competitor. Making matters worse, new VCs are deterred when the existing VC abandons the firm. Other existing

VCs do offset these effects. By starving the lagging company of capital, the VC with common ownership makes it less likely that the lagging company's projects survive to compete with those of the leading company.

Beyond financing choices, there may be additional mechanisms by which VCs limit competition between their portfolio companies. For example, VCs could intervene using their board seats, voting rights, option to remove the CEO, and many other control rights. These mechanisms are not mutually exclusive. Our earlier results on project outcomes reflect these mechanisms' combined effects.

### 2.4 Limits on anticompetitive behavior

The results above come from our full sample. In this subsection, we show that our results are concentrated in a small subset of our data, implying that anticompetitive behavior by VCs is quite limited.

Several conditions must be met in order for common ownership to lead VCs to stifle competition. Suppressing a competitor must significantly improve the surviving startup's market power. A VC must have the power to suppress a startup, either through direct control or by starving the startup of funding. Next, we form proxies for these conditions, we split our full sample into subsamples using each proxy, and we show that our main results are stronger where we expect them to be stronger.

First, we examine whether suppressing a competitor significantly improves a startup's market power. For market power to be significantly increased, the two competing startups must be quite similar, and their product space cannot already be highly competitive. For example, suppressing a competing drug project does not grant much extra market power if the competitor is not a close substitute, or if there are already dozens of other competitors.

To proxy for the number of competitors, we first use the number of drug projects in the same ICD category, including projects in phases I–III. Some ICD categories are much more crowded than others. For instance, in 2018Q4 the meningococcal infection category had only 7 active drug projects and 5 unique startups working on those projects, whereas the malignant

neoplasm of bladder category had 72 drug projects belonging to 67 startups. We assign each ICD category to one of two subsamples depending on whether the average number of drug projects per quarter in that category is above or below 108. We choose this cutoff so that the number of project-quarter observations is roughly equal across the two subsamples. The more-crowded subsample contains 14 ICD categories with an average of 225 projects (Phases I–III) per ICD category and quarter. The less-crowded subsample contains 92 ICD categories with an average of 27 projects. We then estimate our main regression (1) by OLS in each subsample. We do not estimate our IV regressions in subsamples, because the smaller sample sizes result in first-stage regressions that are weak in some subsamples, and we do not want differences across subsamples to be driven by differences in first-stage strength.

We expect our results to be stronger in the subsample of less-crowded ICD categories, where opportunities to increase market power are greater. The results in Table 8, columns 1–2, support this prediction. The slope on $Shocked \times SharedVC$ is significantly negative ($t = -2.9$) in the subsample of less-crowded ICD categories, and it is both smaller in magnitude and statistically insignificant ($t = -1.3$) in the subsample of more-crowded ICD categories. Columns 3–4 of 8 show a variation on this exercise in which we form subsamples based on the number of startups (rather than projects) per ICD category. This distinction is potentially important, because a single startup could have many projects in a drug category and yet coordinate them as a monopolist. Results are similar. Slopes on $Shocked \times SharedVC$ are significantly different from zero in the less-crowded drug categories ($t = -3.3$), and they are statistically insignificant ($t = -1.5$) in the more-crowded categories.

As yet another variation on this exercise, we estimate regression (1) in four rather than two subsamples based on the number of projects in the ICD category. Again, we form subsamples so that they contain roughly equal numbers of project-quarter observations. For example, the first subsample contains 53 ICD categories and 186 drug projects, and the fourth subsample contains 4 ICD categories and 178 drug projects. Figure 2 compares the estimated slope coefficients on $Shocked \times SharedVC$ across the four subsamples. The coefficient is largest in magnitude and highly statistically significant in the least-crowded subsample. Coefficients are smaller in

magnitude and statistically insignificant in the three other subsamples.

To recap this set of results, we find that common ownership reduces competition only in the smallest, least-competitive product markets. This result makes sense, because a VC has little incentive to stifle one competing project if there remain dozens of other competitors. Figure 2 suggests that the anticompetitive effects of common ownership are limited to only 25% of our sample. Furthermore, we know that the frequency of common ownership is lowest exactly in these small, least-competitive product markets (recall Table 2). The overall effects of common ownership arguably depend on the product of our main regression slopes and the frequency of common ownership. The regression slopes measure how a change in common ownership (from zero to one) affects outcomes. The overall effects of common ownership also depend on how frequent it is. The fact that we find significant regression slopes only in the subsample with a low frequency of common ownership implies that the anticompetitive effects of common ownership are quite limited.

Next, we check whether anticompetitive behavior is limited to scenarios in which the two commonly owned projects are technologically similar and hence close competitors. Our proxy for technological similarity is based on the similarity of patent citations. $Shocked \times SharedCite$ equals one if the shocked company's drug shares at least one outgoing citation in common with the progressing drug. We interact that term with $SharedVC$, creating a triple-interaction term, and we include it in our project-outcome and VC-funding regressions.[15] Results are in Table 9. To provide a reference point, columns 1 and 3 recap our previous OLS analysis from Tables 3 and 5 while adding the new control variable, $Shocked \times SharedCite$. We still find significantly negative slopes on $Shocked \times SharedVC$. Next, in columns 2 and 4, we add the triple interaction term $Shocked \times SharedVC \times SharedCite$. We predict a negative coefficient on this term, meaning the negative relation between the dependent variable and $Shocked \times SharedVC$ is even more negative if the projects' technologies are more similar. The estimated coefficients are indeed negative. When the dependent variable is project-level success ($Progress$, column 2), the slope on the triple-interaction term is not statistically significant. It

[15]Whereas the previous analysis compares regression results across ICD subsamples, this analysis focuses on a triple interaction term. The reason is that *SharedCite* is only defined when a shock occurs, hence we cannot form subsamples based on *SharedCite*.

is very large in magnitude, though, implying the effects of $Shocked \times SharedVC$ are roughly three times larger ([0.013+0.020]/0.013]) if $SharedCite = 1$. The slope on $Shocked \times SharedVC$ is still significantly negative in column 2, meaning we find evidence of anticompetitive effects even when projects do not have overlapping citations. When the dependent variable is company-level VC funding ($ExtendFunds$, column 4), the slope on the triple-interaction term is both large in magnitude and statistical significance. Now, the slope on $Shocked \times SharedVC$ is small and statistically insignificant, meaning we find no evidence of anticompetitive VC funding behavior if the projects' patents do not overlap. Overall, these results are consistent with common VC owners having a stronger incentive to suppress competition when projects are more technologically similar and hence closer competitors.

Our next tests focus on whether it is even feasible for a common VC to suppress competition. In order for a VC to suppress a lagging company, the VC must either have strong control rights over that company, or the VC's decision to withhold funding must send a strong negative signal about the company. We argue that both of those conditions relate to how much money the VC has invested in the startup. If a VC has invested more, the VC likely has stronger control rights, and it also likely has better knowledge of the company, which allows the VC to send a more-negative signal by withholding funding. We create two proxies for the VC's financial commitment to a startup. The first is an indicator for whether the VC is a "high-commitment VC," meaning the total amount invested by the VC is above the median across all the VCs that have invested in the given startup. The second is an indicator for whether the VC is the "lead VC," meaning the VC's total amount invested is the highest across all the given startup's VCs. We then estimate our VC-funding regression (equation 3) in subsamples formed using these proxies. We predict that the slope on $Shocked \times SharedVC$ is more negative, meaning anticompetitive effects are stronger, in the subsamples with greater VC investment in the startup. The results in Table 10 support this prediction. The coefficients on $Shocked \times SharedVC$ are large in magnitude and highly statistically significant in the high-commitment and lead-VC subsamples, whereas the coefficients are close to zero and statistically insignificant in other subsamples. These results suggest it is often infeasible for a VC to suppress competition by, for example, starving a startup

of funding. Note also that the high-commitment and lead-VC subsamples have significantly fewer observations, again implying that our evidence of anticompetitive behavior is limited to a small portion of our sample.

## 3 Robustness and identification challenges

Our main project-level tests take the form of panel regressions. Similar results obtain even if we collapse the panel into a single observation per project. We redefine $Progress_i$ to be an indicator for whether project $i$ ultimately progresses to Phase II. $Progress_i$ equals zero if the project is eventually suspended or remains "in progress" indefinitely. We redefine $Shock_i$ to be an indicator for whether project $i$ receives a shock at any time between the project's birth and the end of its Phase I. It is infeasible to include an ICD×quarter FEs, so we instead include FEs for the project's initial quarter, meaning these results are more comparable to Table 3 than Table 4. The results, shown in Table 11, are quite similar to those in the full panel regressions. Whether we estimate by OLS, 2SLS, or bivariate probit, we find a significantly negative loading of $Progress$ on $Shocked \times SharedVC$, consistent with our prediction. The slope coefficients are considerably larger in magnitude than in the panel regressions, which makes sense given that this test aggregates all of a project's quarters. The OLS estimate, for example, implies a sizeable 0.216 lower probability of ultimately progressing to Phase II if there is a shock and a shared VC.

We also find support for our project-level predictions if we replace the dependent variable $Progress_{it}$ with its opposite, $Suspend_{it}$, an indicator for whether project $i$ is suspended in quarter $t$. We focus on $Progress$ in our main tests because it is difficult to accurately measure suspensions. We define $Suspend_{it}$ in two steps. First, we check whether Cortellis records a project as being explicitly discontinued, withdrawn, or out-licensed in a given quarter. Second, there are many projects that are never officially discarded and yet continue to be listed in the drug portfolio without further trial updates. We assume these "zombie projects" are suspended three years after the first quarter when Cortellis designates them as "no development reported." For zombie projects without such designation, we assume they are suspended five years after

project initiation. We predict a positive relation between $Suspend$ and $Shocked \times SharedVC$, meaning a project is more likely to be suspended if a close competitor with a shared VC makes progress. The results in Table 12 support this prediction. Whether we estimate by OLS or IV, the slopes on $Shocked \times SharedVC$ are significantly positive, consistent with the negative slopes we found when using dependent variable $Progress$ in Tables 3 and 4. Economic significance is even higher with $Suspend$ in place of $Progress$. Statistical significance remains high with $Suspend$, but not as high as in our main results with $Progress$. In sum, our main conclusions are robust to using this alternative dependent variable.

Next, we address three potential identification challenges. The first challenge is that our instrument, based on geographic proximity, may simply be picking up the effects of information sharing. Some types of information are more likely to flow between startups that are nearby. This concern, however, is not likely to hold for the type of information we study, namely, whether a project progresses from Phase I to II. Such events are widely publicized via clinical trial registries, FDA submissions, patent filings, and company press releases. Even if this information were shared privately between nearby startups, the bias would likely work against our main result. If drug A experiences success, that sends a positive signal about the scientific underpinnings of nearby competing drug B, making it *more* likely that drug B progresses to Phase II.[16] We instead find that progress by A is *less* likely to predict progress by B if the two companies are nearby, consistent with anticompetitive effects by common VC owners.

A related concern is that nearby competitors have opposite outcomes not because of common ownership, but because success by one drug project makes its competitors optimally "give up" earlier because they will not be first to market. In this story, proximity is just picking up greater information sharing or technological similarity. This story is implausible for Phase I drug projects, for two reasons. First, as discussed above, a competitor's success sends a positive signal, not a negative signal, about the lagging project's scientific fundamentals, making the lagging project less likely to give up. Second, Phase I projects are far from reaching the market, so there is still a good chance that the lagging project can catch up to the pioneer, making it

[16]Supporting this logic, Krieger (2017) finds that a drug project is more likely to be terminated after a close competitor terminates its own project, implying that technological learning dominates competition effects.

suboptimal to give up earlier.

We also address this concern empirically in two ways. First, if this concern were valid, then even in the absence of a shared VC, we should see a negative relation between *Progress* and success by a close competitor (*Shocked*). We do not see this in the data. Instead, column 1 of Tables 3 and 4 shows a positive slope of *Progress* on *Shocked*.

Second, to address the concern that our proximity instrument is just picking up the effects of technological similarity, we control for overlapping patent citations. Specifically, we repeat our main panel regressions while controlling for $Shocked \times SharedCite$, an indicator for whether the shocked company's drug shares at least one citation in common with the progressing drug. In columns 1 and 3 of Table 9, we see that OLS regressions still produce a significantly negative slope on $Shocked \times SharedVC$ if we control for $Shocked \times SharedCite$, for both project and funding outcomes. IV results for project (funding) outcomes are in Tables 13 (14). Column 1 in both tables shows that sharing a similar technology is positively related to sharing a VC, which makes sense if VCs specialize in certain technologies. Even after controlling for $Shocked \times SharedCite$, we still find that our proximity instrument is strongly related to sharing a VC. More important, our main results in columns 3 and 5 continue to go through very strongly with the additional control: $Shocked \times SharedVC$ is still negatively related to both *Progress* and *ExtendFunds*. Also interesting, *Progress* is positively related to $Shocked \times SharedCite$, consistent with the argument above that seeing a close competitor make progress, even absent a shared VC, is a positive signal, not a negative signal. Overall, these results indicate that our instrument is not simply picking up the omitted effects of technological similarity.

Third, our geographic proximity instrument could be picking up the ease of poaching employees from a competing startup. For example, if startup A experiences success, it could poach employees from a nearby and competing startup B, causing B to fail. This story could in theory explain why nearby startups are more likely to have opposite outcomes. We offer two counterarguments. First, there is an opposing force. Success by A sends a positive signal about B (explained above), making startup B and its employees less willing to separate from each other, which makes it harder for A to poach B's employees. Second, if the competing story were

true, then our results should be weaker in states that strongly enforce employee non-compete agreements, because poaching is less feasible there. The evidence in Table 15 does not support that prediction, however. We create a dummy variable for whether non-compete agreements are strongly enforced in startup $i$'s state in quarter $t$. We then interact that dummy variable with $Shocked \times SharedVC$ in our main project-outcome regressions. If the competing story were true, we should find a significantly positive slope on the triple-interaction term in columns 1 and 2. Instead, we find coefficients near zero, with $t-$statistics below 1. Results are similar if we examine project suspensions in columns 3 and 4. These results indicate that employee poaching is not spuriously generating our main results.

# 4 Conclusions

The VC setting provides an important litmus test for the common-ownership hypothesis, as conditions are in many ways "ideal" for the hypothesis to hold. We find that common ownership by VCs does stifle competition, but only in limited circumstances. Our evidence comes from detailed project-level data on pharmaceutical startups. Common ownership by VCs is widespread in our sample: 39% of startups have a close competitor with a shared VC investor. Using geographic proximity to instrument for common ownership, we show that after a startup sees a close competitor make progress on a drug project, the startup is less likely to advance its own project—and less likely obtain VC funding— if the two companies share a common VC. This fact is consistent with shared VCs having an incentive to hold back the weaker project in order to grant more market power to the surviving project. However, we find that this anticompetitive behavior is quite limited. It is concentrated in product markets with few competitors, VCs owning large equity stakes, and projects that share similar technologies.

## References


ABPI (2019). Worldwide pharmaceutical company R&D expenditure by country.

Angrist, J. D. and Pischke, J.-S. (2008). *Mostly harmless econometrics: An empiricist's companion*. Princeton university press.

Azar, J., Raina, S., and Schmalz, M. C. (2019). Ultimate ownership and bank competition. *Working Paper*.

Azar, J., Schmalz, M. C., and Tecu, I. (2018a). Anticompetitive effects of common ownership. *Journal of Finance*, 73(4):1513–1565.

Azar, J., Schmalz, M. C., and Tecu, I. (2018b). Reply to "common ownership does not have anti-competitive effects in the airline industry". *Working paper*.

Bio, Biomedtracker, and Amplion (2016). Clinical development success rates 2006-2015.

Bresnahan, T. F. and Salop, S. C. (1986). Quantifying the competitive effects of production joint ventures. *International Journal of Industrial Organization*, 4(2):155–175.

Cunningham, C., Ederer, F., and Ma, S. (2019). Killer acquisitions. *Working Paper*.

Dennis, P. J., Gerardi, K., and Schenone, C. (2019). Common ownership does not have anti-competitive effects in the airline industry. *Working Paper*.

Dinc, S. I. and Erel, I. (2013). Economic nationalism in mergers and acquisitions. *Journal of Finance*, 68(6):2471–2514.

Eldar, O., Grennan, J., and Waldock, K. (2019). Common ownership and startup growth. *Working paper*.

Ewens, M. and Marx, M. (2018). Founder replacement and startup performance. *Review of Financial Studies*, 31(4):1532–1565.

Ewens, M., Rhodes-Kropf, M., and Strebulaev, I. A. (2016). Insider financing and venture capital returns. *Working Paper*.

Fulghieri, P. and Sevilir, M. (2009). Size and focus of a venture capitalist's portfolio. *Review of Financial Studies*, 22(11):4643–4680.

Gerakos, J. J. and Xie, J. (2019). Institutional horizontal shareholdings and generic entry in the pharmaceutical industry. *Working Paper*.

Gilje, E. P., Gormley, T. A., and Levit, D. (2019). Who's paying attention? measuring common ownership and its impact on managerial incentives. *Journal of Financial Economics*, Forthcoming.

Gompers, P. A., Gornall, W., Kaplan, S. N., and Strebulaev, I. A. (2019). How do venture capitalists make decisions? *Journal of Financial Economics*, Forthcoming.

González-Uribe, J. (2019). Exchanges of innovation resources inside venture capital portfolios. *Journal of Financial Economics*, Forthcoming.

Gramlich, J. and Grundl, S. (2017). Testing for competitive effects of common ownership. *Working Paper*.

He, J. J. and Huang, J. (2017). Product market competition in a world of cross-ownership: Evidence from institutional blockholdings. *Review of Financial Studies*, 30(8):2674–2718.

Jeffers, J. (2019). The impact of restricting labor mobility on corporate investment and entrepreneurship. *Working Paper*.

Jiang, W. (2017). Have instrumental variables brought us closer to the truth. *Review of Corporate Finance Studies*, 6(2):127–140.

Kaplan, S. N. and Lerner, J. (2010). It ain't broke: The past, present, and future of venture capital. *Journal of Applied Corporate Finance*, 22(2):36–47.

Kennedy, P., O'Brien, D. P., Song, M., and Waeher, K. (2017). The competitive effects of common ownership: Economic foundations and empirical evidence. *Working paper*.

Khanna, N. and Mathews, R. D. (2016). Posturing and holdup in innovation. *Review of Financial Studies*, 29(9):2419–2454.

Kini, O., Lee, S., and Shen, M. (2018). Common institutional ownership and product market threats. *Working paper*.

Krieger, J. L. (2017). Trials and terminations: Learning from competitors' R&D failures. *Working Paper*.

Lewellen, J. and Lewellen, K. (2017). Institutional investors and corporate governance: The incentive to increase value. *Working paper*.

Lewellen, K. and Lowry, M. (2019). Does common ownership really increase firm coordination? *Working paper*.

Lindsey, L. (2008). Blurring firm boundaries: The role of venture capital in strategic alliances. *Journal of Finance*, 63(3):1137–1168.

Newham, M., Seldeslachts, J., and nol, A. B.-E. (2018). Common ownership and market entry: Evidence from the pharmaceutical industry. *Working Paper*.

Reynolds, R. J. and Snapp, B. R. (1986). The competitive effects of partial equity interests and joint ventures. *International Journal of Industrial Organization*, 4(2):141–153.

Saretto, A. and Tookes, H. E. (2013). Corporate leverage, debt maturity, and credit supply: The role of credit default swaps. *Review of Financial Studies*, 26(5):1190–1247.

Stock, J. H. and Yogo, M. (2005). Testing for weak instruments in linear IV regressions. In Andrews, D. W. and Stock, J. H., editors, *Identification and inference for econometric models: Essays in honor of Thomas Rothenberg*, pages 80–108. Cambridge University Press, New York.

Wooldridge, J. M. (2010). *Econometric analysis of cross section and panel data*. MIT press.

Figure 1: Geographic Distribution of Drug Projects

This figure shows the number of drug projects in our sample in each state.

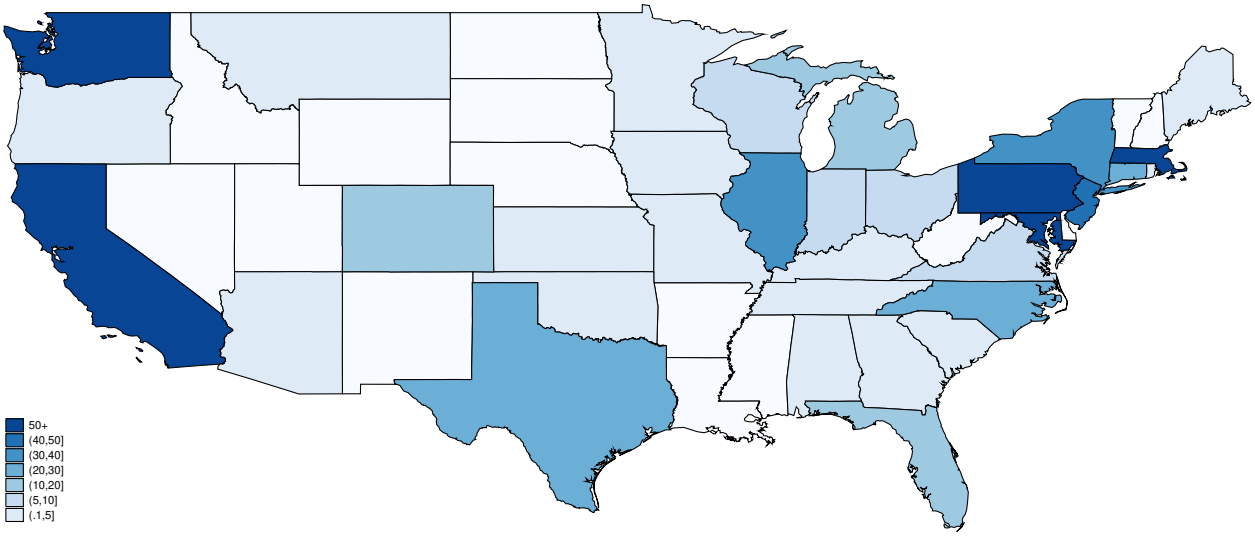

Figure 2: Comparing More- and Less-Crowded Drug Categories

This figure plots the estimated slope coefficients on $Shocked \times SharedVC$, from regression (1), estimated by OLS independently in four subsamples. The subsamples, noted on the horizontal axis, are based on the average number of drug projects per quarter in each ICD category. We first rank ICD categories by their average project count per quarter. We choose subsample breakpoints so the four subsamples contain roughly equal numbers of project×quarter observations. The smallest subsample contains 53 ICD categories with 186 drug projects; the medium-small one includes 12 ICD categories with 191 drug projects; the medium-large one has 7 ICD categories with 224 drug projects; and the largest one has 4 ICD categories with 178 drug projects. The dependent variable is $Progress$, which equals one if a project progresses to Phase II in a given quarter. As in Table 3, regressions include startup, ICD, and year-quarter FEs; we control for $Age$, $NProjects$, and $NVCs$; and standard errors are double-clustered at the ICD category and startup company levels. Vertical bars indicate 95% confidence intervals.

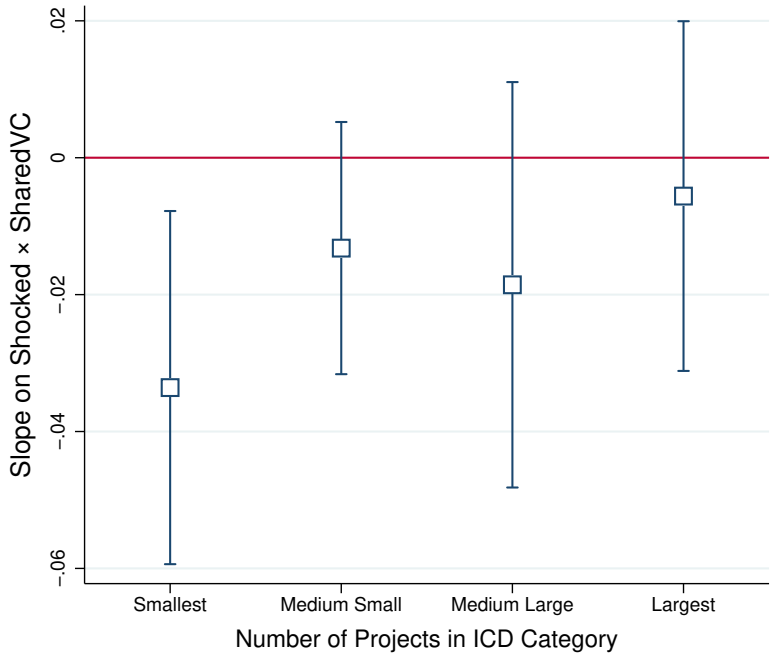

Table 1: Summary Statistics

This table contains summary statistics for our sample of Phase I drug projects in U.S. VC-backed startups. The unit of observation is the project×quarter in Panel A, and the startup company×quarter in Panel B. We show summary statistics for the full sample and the subsamples of observations that are "Never Treated" versus "Ever Treated." The last column reports the difference between those subsamples. Projects/startup companies are categorized as "Never Treated" if the variable $Shocked \times SharedVC$ is equal to zero across its lifespan, and "Ever Treated'" otherwise. *Shocked* is an indicator for whether a different project (at a different startup) in the same ICD category progresses from phase I to II between the given project's initiation quarter and the previous quarter. *Progress* is an indicator for whether a project progresses to Phase II in a given quarter. *Age* is the log of one plus the number of quarters since the project's initiation. $Number\,of\,Projects$ is the number of projects being developed within the startup company at all clinical stages in the given quarter. $Number\,of\,ICD\,Categories$ is the number of distinct ICD categories covered by those projects. $Number\,of\,VCs$ is the number of VC firms invested in the startup. $Size\,of\,Last\,VC\,Round$ is the log dollar amount that the startup raised in its most recent VC financing round. $*$, $**$, and $***$ denote statistical significance at the 10%, 5%, and 1% level, respectively, two-way clustering at ICD category and startup company levels.

| | Full Sample | | | | | | Never Treated | Ever Treated | |
|---|---|---|---|---|---|---|---|---|---|
| *Variable* | *Mean* | *Std Dev* | *P25* | *Median* | *P75* | *Obs* | *Mean* | *Mean* | *Difference* |
| **Panel A: Project-Level Variables** | | | | | | | | | |
| *Shocked* | 0.519 | 0.500 | 0.000 | 1.000 | 1.000 | 12,782 | 0.472 | 0.790 | −0.318*** |
| *Progress* | 0.017 | 0.131 | 0.000 | 0.000 | 0.000 | 12,782 | 0.019 | 0.006 | 0.013*** |
| *Age* | 2.308 | 0.872 | 1.792 | 2.485 | 2.944 | 12,782 | 2.287 | 2.432 | −0.145** |
| **Panel B: Company-Level Variables** | | | | | | | | | |
| *Number of Projects* | 1.662 | 1.383 | 1.000 | 1.000 | 2.000 | 7,924 | 1.614 | 2.383 | −0.769** |
| *Number of ICD Categories* | 1.416 | 0.891 | 1.000 | 1.000 | 2.000 | 7,924 | 1.391 | 1.791 | −0.400 |
| *Number of VCs* | 4.790 | 4.021 | 2.000 | 4.000 | 6.000 | 7,924 | 4.813 | 4.451 | 0.362 |
| *Size of Last VC Round* | 9.024 | 1.513 | 8.256 | 9.393 | 10.123 | 6,862 | 9.034 | 8.871 | 0.163 |

Table 2: Frequency of Common Ownership by VCs

Row one reports the percent of startup-quarter observations in which the startup has a close competitor. Startup $i$ is defined as having a close competitor in a quarter if there is another startup in the same quarter developing a drug in Phase I in the same ICD category as one of $i$'s Phase I drugs. Row two reports the fraction of startup-quarter observation in which the startup has a close competitor, and that close competitor shares at least one VC in common with the startup in question. Row three reports the average number of distinct VCs that own stakes in Phase I drug projects in a given ICD category, averaging across drug categories and quarters. Row four reports the average number of distinct ICD categories in which a typical VC owns a stake, averaging across VCs and quarters. We say a VC owns a stake in an ICD category if the VC owns a stake in a startup with a Phase I project in that ICD category. We report results for the full sample as well as in the subsamples of more- and less-crowded drug categories. We say an ICD category is less crowded if the average number of Phase I projects in the category is less than 108; we choose this cutoff so the number of project-quarters is roughly equal across the two subsamples. To compute the subsample statistics, we only include project-quarter observations (and their associated startup identifier) that are in the given subsample. A given startup-quarter can therefore appear in both subsamples if it has projects in both subsamples.

| | Full Sample | Less-Crowded Drug Categories | More-Crowded Drug Categories |
|---|---|---|---|
| Percent of startups with a close competitor | 93.3% | 84.8% | 99.7% |
| Percent of startups with a close competitor held by same VC | 39.3% | 22.7% | 51.2% |
| Average number of VCs per drug category | 16.6 | 10.9 | 38.2 |
| Average number of drug categories per VC | 2.51 | 1.98 | 1.74 |

Table 3: Project Outcomes in Panel Regressions, First Experiment

This table contains estimates of regression (1). The unit of observation is the project by quarter. The sample contains all project-quarters when each project was in Phase I clinical trials. Dependent variables are indicated in the column titles. $Progress_{it}$ is an indicator for whether project $i$ progresses to Phase II in quarter $t$. $Shocked_{it}$ is an indicator for whether another project (at a different startup) in the same ICD category as project $i$ progresses from phase I to II between project $i$'s initiation quarter and quarter $t-1$. $SharedVC_{it}$ is an indicator for whether that progressing project shares a VC with project $i$ in quarter $t$. $Age_{it}$ is the log of one plus the number of quarters since the project's $i$ initiation. $NProjects_{it}$ is the number of Phase I projects being developed in project's $i$'s company during quarter $t$. $NVCs_{it}$ is the number of VC firms that own a stake in project $i$'s startup company in quarter $t$. Columns 1 reports results from OLS regressions. Columns 2 reports results from estimating a probit model with dependent variable $Shocked \times SharedVC$ and independent variable $Shocked \times Proximity$, which is defined in equation (2) and captures the distance between project $i$ and the project causing the shock. The probit model's predicted probabilities, denoted $\widehat{Shocked \times SharedVC}$, form the instrument in the first-stage regression (column 3). The $1st$ stage F-stat is the Kleibergen-Paap Wald $F$ statistic, whose $p$-value is in parentheses. Standard errors are computed by two-way clustering at the ICD category and startup company levels in the OLS and 2SLS regressions. Columns 5 and 6 report results from the bivariate probit method, with standard errors clustered by ICD category. $t$-statistics are in parentheses. Fixed effects are noted in the bottom row. ***,**, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | ***OLS*** | ***Probit*** | ***1st Stage*** | ***2SLS*** | **Biprobit** | **Biprobit** |
| | *Progress* | *Shocked×SharedVC* | *Shocked×SharedVC* | *Progress* | *Shocked×SharedVC* | *Progress* |
| $Shocked \times SharedVC$ | -0.016*** | | | -0.111** | | -0.048* |
| | (-3.363) | | | (-2.460) | | (-1.683) |
| $\widehat{Shocked \times SharedVC}$ | | | 0.459** | | | |
| | | | (2.521) | | | |
| $Shocked \times Proximity$ | | 0.836*** | | | 0.826*** | |
| | | (4.753) | | | (4.574) | |
| $Shocked$ | 0.003 | | 0.074*** | 0.014** | | -0.006 |
| | (1.033) | | (2.652) | (2.172) | | (-0.060) |
| $Age$ | 0.010*** | 0.471*** | -0.001 | 0.012*** | 0.483*** | -0.012 |
| | (3.602) | (5.084) | (-0.096) | (4.457) | (5.103) | (-0.195) |
| $NProjects$ | 0.002** | -0.007 | 0.005 | 0.002** | -0.007 | -0.024 |
| | (2.035) | (-0.355) | (1.496) | (2.168) | (-0.379) | (-1.428) |
| $NVCs$ | -0.002 | 0.088*** | -0.005 | -0.001 | 0.088*** | 0.026** |
| | (-0.786) | (6.566) | (-0.830) | (-0.680) | (6.557) | (2.285) |
| $1st$ *stage F-stat* | | | 6.35 | | | |
| | | | (0.014) | | | |
| *Startup FE* | Yes | No | Yes | Yes | No | No |
| *Yr-Qtr FE* | Yes | No | Yes | Yes | No | No |
| *ICD FE* | Yes | No | Yes | Yes | No | No |
| *N* | 12,769 | 12,782 | 12,769 | 12,769 | 12,782 | 12,782 |
| *Adj.* $R^2$ | 0.075 | | 0.559 | | | |

Table 4: Project Outcomes in Panel Regressions, Second Experiment

This table is the same as the previous, except we include ICD×quarter FEs. These FEs replace the ICD FEs and quarter FEs in the previous table.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | ***OLS*** | ***Probit*** | 1*st* ***Stage*** | ***2SLS*** |
| | *Progress* | *Shocked*× *SharedVC* | *Shocked*× *SharedVC* | *Progress* |
| $Shocked \times SharedVC$ | -0.013** | | | -0.180*** |
| | (-2.095) | | | (-3.167) |
| $Shocked \times \widehat{SharedVC}$ | | | 0.552*** | |
| | | | (3.074) | |
| $Shocked \times Proximity$ | | 0.836*** | | |
| | | (4.753) | | |
| $Shocked$ | 0.014** | | 0.049* | 0.029*** |
| | (2.510) | | (1.752) | (3.622) |
| $Age$ | 0.004 | 0.471*** | 0.004 | 0.008** |
| | (1.089) | (5.084) | (0.451) | (2.003) |
| $NProjects$ | 0.002** | -0.007 | 0.002 | 0.002* |
| | (2.067) | (-0.355) | (0.719) | (1.915) |
| $NVCs$ | -0.003 | 0.088*** | -0.006 | -0.002 |
| | (-1.346) | (6.566) | (-0.858) | (-0.953) |
| 1*st stage F-stat* | | | 9.45 | |
| | | | (0.003) | |
| *Startup FE* | Yes | No | Yes | Yes |
| *ICD × Qtr. FE* | Yes | No | Yes | Yes |
| *N* | 11,782 | 12,782 | 11,782 | 11,782 |
| *Adj* $R^2$ | 0.074 | | 0.593 | |

Table 5: Funding Outcomes

This table reports results from regression (3). The sample includes all pairs of VC firms $j$ that invest in startups $i$. For each pair $\{i, j\}$, we include quarters $t$ when the startup has at least one project in Phase I trials and the quarter is between $j$'s first investment in $i$ and $i$'s exit (e.g., IPO, trade sale). The dependent variable, $ExtendFunds_{ijt}$, is an indicator for whether VC $j$ invests in startup $i$ in quarter $t$. $Shocked_{it}$ equals one if at least one project in startup $i$ has seen another project (a "pioneer") in the same ICD category (but different startup) progress to Phase II between the former project's inception and quarter $t-1$. $Shocked \times SharedVC_{ijt}$ equals one if VC $j$ holds a stake in both startup $i$ and at least one of the startups owning the pioneering projects that produced $Shocked_{it} = 1$. $SelfProgress_{ijt}$ equals 1 if startup $i$ has progressed at least one drug project to the next stage of clinical trials between the quarter when VC $j$ first invested in $i$ and quarter $t-1$; we include this variable as a proxy for startup performance. $NCats_{jt}$ is the number of ICD categories covered by all of VC $j$'s portfolio companies during quarter $t$; this variable measures the VC's diversification. $Duration_{ijt}$ is the log of one plus the number of years since startup $i$ last received funding from VC $j$. $NProjects_{it}$ is the number of projects under development at startup $i$ in quarter $t$. $NVCs_{it}$ is the number of VC firms that own a stake in startup $i$ at the beginning of quarter $t$. $PortfolioSize_{jt}$ is the number of startups in VC $j$'s portfolio in quarter $t$. $PrevRoundSize_{it}$ is the log dollar amount that startup $i$ raised in its most recent VC financing round before quarter $t$. Column (1) contains results from OLS. Columns (2) to (4) contain results from a probit-2SLS specification similar to those in previous tables. Columns (5) and (6) report results from the bivariate probit model. Standard errors are clustered at the startup company level. We report $t-$statistics in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | **OLS** | **Probit** | 1*st* **Stage** | **2SLS** | **Biprobit** | **Biprobit** |
| | *ExtendFunds* | *Shocked*× *SharedVC* | *Shocked*× *SharedVC* | *ExtendFunds* | *Shocked*× *SharedVC* | *ExtendFunds* |
| $Shocked \times SharedVC$ | -0.022** | | | -0.564*** | | -0.144*** |
| | (-1.981) | | | (-3.633) | | (-8.349) |
| $Shocked \times \widehat{SharedVC}$ | | | 0.736*** | | | |
| | | | (5.611) | | | |
| $Shocked \times Proximity$ | | 0.414*** | | | 0.421*** | |
| | | (5.257) | | | (5.580) | |
| $Shocked$ | 0.069*** | | 0.006 | 0.082*** | | 0.445*** |
| | (6.898) | | (0.841) | (7.334) | | (7.639) |
| $SelfProgress$ | 0.023** | 0.343* | -0.028** | 0.014 | 0.369** | 0.310*** |
| | (2.230) | (1.940) | (-2.023) | (1.026) | (2.084) | (5.446) |
| $NCats$ | -0.003 | -0.008 | 0.008** | 0.001 | -0.009 | 0.001 |
| | (-1.494) | (-0.301) | (2.449) | (0.439) | (-0.344) | (0.125) |
| $NProjects$ | 0.002 | 0.071 | -0.008 | 0.004 | 0.072 | 0.030 |
| | (0.426) | (1.199) | (-1.052) | (0.721) | (1.231) | (1.130) |
| $NVCs$ | 0.011*** | 0.004 | -0.000 | 0.011*** | 0.004 | 0.001 |
| | (4.177) | (0.262) | (-0.121) | (4.118) | (0.298) | (0.129) |
| $PortfolioSize$ | 0.001 | 0.095*** | -0.014*** | -0.000 | 0.099*** | 0.040*** |
| | (0.650) | (2.990) | (-3.320) | (-0.181) | (3.141) | (3.111) |
| $Duration$ | -0.122*** | 0.209*** | -0.015** | -0.126*** | 0.214*** | -0.737*** |
| | (-20.941) | (3.078) | (-2.568) | (-18.652) | (3.279) | (-19.497) |
| $PrevRoundSize$ | -0.009** | -0.014 | -0.008 | -0.013** | -0.023 | -0.042** |
| | (-2.409) | (-0.276) | (-1.470) | (-2.452) | (-0.435) | (-2.570) |
| 1*st stage F-stat* | | | 31.48 | | | |
| | | | (0.000) | | | |
| *VC Firm FE* | Yes | No | Yes | Yes | No | No |
| *Startup FE* | Yes | No | Yes | Yes | No | No |
| *Yr-Qtr FE* | Yes | No | Yes | Yes | No | No |
| *N* | 34,045 | 32,731 | 32,710 | 32,710 | 32,731 | 32731 |
| *Adj.* $R^2$ | 0.181 | | 0.361 | | | |

Table 6: Funding Outcomes, Controlling for Demand

This table is the same as the previous, except we include startup × quarter FEs in order to control for startups' demand for funding. Control variables measured at the startup×quarter level are omitted.

| | (1) **OLS** *ExtendFunds* | (2) **Probit** | (3) 1*st* **Stage** | (4) **2SLS** *ExtendFunds* |
|---|---|---|---|---|
| $Shocked \times SharedVC$ | -0.024*** | | | -0.353*** |
| | (-2.910) | | | (-3.165) |
| $Shocked \times \widehat{SharedVC}$ | | | 0.978*** | |
| | | | (5.865) | |
| $Shocked \times Proximity$ | | 0.448*** | | |
| | | (7.638) | | |
| $Shocked$ | 0.266*** | | 0.045** | 0.289*** |
| | (11.181) | | (2.087) | (11.310) |
| $SelfProgress$ | 0.086*** | 0.438*** | -0.061 | 0.080** |
| | (4.277) | (3.011) | (-0.928) | (2.460) |
| $NCats$ | -0.003* | 0.004 | 0.006* | -0.001 |
| | (-1.871) | (0.162) | (1.849) | (-0.637) |
| $PortfolioSize$ | 0.001 | 0.088*** | -0.016*** | 0.001 |
| | (0.705) | (2.655) | (-3.733) | (0.544) |
| $Duration$ | -0.172*** | 0.190*** | -0.031*** | -0.177*** |
| | (-19.982) | (3.321) | (-3.206) | (-19.789) |
| 1*st stage F-stat* | | | 34.40 | |
| | | | (0.000) | |
| *VC Firm FE* | Yes | No | Yes | Yes |
| *Startup× Qtr. FE* | Yes | No | Yes | Yes |
| *N* | 36,002 | 36,282 | 34,659 | 34,659 |
| *Adj.* $R^2$ | 0.459 | | 0.330 | |

Table 7: Aggregate Funding Outcomes for Lagging Firms

The unit of observation in columns (1)–(3) is the startup ($i$) by quarter ($t$). The dependent variable in column (1) is $ExtendFundAgg$, an indicator for whether any VC invests in startup $i$ in quarter $t$. The dependent variable in column (2) is $AmountRaised$, the percentage increase in cumulative VC funding during quarter $t$ for startup $i$. Specifically, $AmountRaised$ equals $\log(1+NewFunds_{it}/CumFunds_{i,t-1})$, where $NewFunds_{it}$ is the dollar amount raised by startup $i$ from all VCs in quarter $t$, and $CumFunds_{i,t-1}$ is the cumulative amount of VC funding raised by firm $i$ through the end of quarter $t-1$. The dependent variable in column (3) is $NewInvestors$, an indicator for whether startup $i$ raises funding from any new VC during quarter $t$. In columns (1)–(3), $Shocked_{it}$ equals one if at least one project in startup $i$ has seen another project (a "pioneer") in the same ICD category (but different startup) progress to Phase II between the former project's inception and quarter $t-1$, and $Shocked \times SharedVC_{it}$ equals one if there is any VC that holds a stake in startup $i$ and at least one of the startups owning the pioneering projects that produced $Shocked_{it} = 1$. Control variables $SelfProgress_{it}$, $Duration_{it}$, $NProjects_{it}$, $NVCs_{it}$, and $PrevRoundSize_{it}$ are defined as in Table 5, except we collapse them to the startup-quarter level in columns (1)–(3). Column (4) resembles the specification of column (1) in Table 5, which is at VC-startup-quarter level. The dependent variable is $Extendfunds_{ijt}$, an indicator for whether VC $j$ invests in startup $i$ in quarter $t$. One distinction is that our independent variable $Shocked \times SharedVC_{it}$ is collapsed to the startup-quarter level, as in columns (1)–(3). Additionally, we interact our focal variables with $NonCommonVC_{ijt}$, a dummy for whether VC $j$ is not a common owner of a pioneering project, if there is any, competing with startup $i$ in quarter $t$. We estimate all models by OLS. Standard errors are clustered at the startup company level. Fixed effects are noted in the bottom row. ***,**, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | $ExtendFundsAgg$ | $AmountRaised$ | $NewInvestors$ | $ExtendFunds$ |
| $Shocked \times SharedVC$ | -0.090*** | -0.079*** | -0.031** | -0.057*** |
| | (-3.500) | (-4.878) | (-2.099) | (-2.822) |
| $Shocked \times SharedVC \times NonCommonVC$ | | | | 0.016 |
| | | | | (0.772) |
| $Shocked$ | 0.120*** | 0.012 | -0.015 | 0.077*** |
| | (6.985) | (0.873) | (-1.427) | (3.721) |
| $Shocked \times NonCommonVC$ | | | | -0.004 |
| | | | | (-0.214) |
| $SelfProgress$ | 0.028 | 0.022 | -0.035** | 0.023** |
| | (1.287) | (1.172) | (-2.343) | (2.268) |
| $NProjects$ | 0.014* | -0.001 | 0.002 | 0.002 |
| | (1.855) | (-0.142) | (0.389) | (0.396) |
| $NVCs$ | 0.025*** | 0.040*** | 0.020*** | 0.013*** |
| | (3.943) | (4.463) | (4.647) | (4.996) |
| $Duration$ | -0.231*** | -0.040*** | -0.043*** | -0.122*** |
| | (-24.620) | (-4.620) | (-7.872) | (-20.875) |
| $PrevRoundSize$ | -0.011* | | 0.014*** | -0.009** |
| | (-1.745) | | (4.191) | (-2.516) |
| $\log(CumFunds_{i,t-1})$ | | -0.277*** | | |
| | | (-4.388) | | |
| $PortfolioSize$ | | | | -0.001 |
| | | | | (-0.990) |
| *VC Firm FE* | No | No | No | Yes |
| *Startup FE* | Yes | Yes | Yes | Yes |
| *Yr-Qtr FE* | Yes | Yes | Yes | Yes |
| *N* | 7,704 | 7,640 | 7,704 | 34,045 |
| *Adj.* $R^2$ | 0.371 | 0.290 | 0.079 | 0.182 |

Table 8: Anticompetitive Effects and Competition Intensity

This table contains results from subsamples based on the number of drug projects as well as the number of startup firms in the same ICD category. Specifically, in columns (1) and (2) we assign each ICD category to one of two subsamples depending on whether the average number of drug projects (in Phases I–III) per quarter is above or below 108. We choose this cutoff so the number of project-quarter observations is roughly equal across subsamples. In columns (3) and (4), we create subsamples in the same way but replace number of projects by number of distinct startups that have at least one active drug project in the ICD category. We estimate OLS regressions within each subsample. In columns 1–2 we estimate (1) with dependent variable $Progress$; remaining details are the same as in Table 3. In columns 3–4 we estimate regression (3) with dependent variable $ExtendFunds$; remaining details are the same as in Table 5.

| | (1) **Low Number of Projects** | (2) **High Number of Projects** | (3) **Low Number of Startups** | (4) **High Number of Startups** |
|---|---|---|---|---|
| $Shocked \times SharedVC$ | -0.019*** | -0.010 | -0.023*** | -0.011 |
| | (-2.857) | (-1.294) | (-3.260) | (-1.464) |
| $Shocked$ | 0.006 | 0.004 | 0.004 | 0.005 |
| | (1.129) | (0.789) | (0.763) | (1.042) |
| $Age$ | 0.020*** | 0.008** | 0.018*** | 0.010** |
| | (4.433) | (2.468) | (3.457) | (3.128) |
| $NProjects$ | 0.001** | 0.002 | 0.001 | 0.002* |
| | (2.573) | (1.467) | (1.626) | (1.847) |
| $NVCs$ | 0.001 | -0.003 | 0.001 | -0.004 |
| | (0.315) | (-1.050) | (0.508) | (-1.194) |
| *Startup FE* | Yes | Yes | Yes | Yes |
| *Yr-Qtr. FE* | Yes | Yes | Yes | Yes |
| *ICD FE* | Yes | Yes | Yes | Yes |
| *N* | 6,050 | 6,716 | 5,983 | 6,783 |
| *Adj.* $R^2$ | 0.092 | 0.072 | 0.083 | 0.079 |

Table 9: Anticompetitive Effects and Technological Similarity

This table reports how the results from regression (1) and (3) vary depending on how technologically similar the lagging project and the pioneering project are. Columns (1)–(2) report the project level results and columns (3)–(4) report the VC-funding results. The proxy for technological similarity is based on patent citations, which we collect from the Google Patents database. $Shocked \times SharedCite$ equals one if the pioneering drug project and the shocked drug project have at least one outgoing citation in common. $Shocked \times SharedVC \times SharedCite$ equals one if the shocked company's drug shares at least one citation in common with at least one progressing drug of a different company, and both those companies share a common VC. To provide a reference point, Columns (1) and (3) estimate the regressions without a triple-interaction term. Remaining details and variable definitions are the same as in Table 3 and 5.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | *Progress* | *Progress* | *ExtendFunds* | *ExtendFunds* |
| $Shocked \times SharedVC$ | -0.017*** | -0.013** | -0.022* | -0.008 |
| | (-3.483) | (-2.245) | (-1.945) | (-0.758) |
| $Shocked \times SharedVC \times SharedCite$ | | -0.020 | | -0.084*** |
| | | (-1.465) | | (-3.634) |
| $Shocked$ | 0.004 | 0.003 | 0.069*** | 0.068*** |
| | (1.029) | (0.998) | (6.925) | (6.920) |
| $Shocked \times SharedCite$ | 0.010 | 0.014 | -0.010 | -0.006 |
| | (0.919) | (1.085) | (-0.545) | (-0.349) |
| $NProjects$ | 0.002** | 0.002** | 0.002 | 0.002 |
| | (2.076) | (2.127) | (0.461) | (0.438) |
| $NVCs$ | -0.001 | -0.001 | 0.011*** | 0.011*** |
| | (-0.534) | (-0.639) | (4.171) | (4.145) |
| $Age$ | 0.010*** | 0.010*** | | |
| | (3.431) | (3.396) | | |
| $SelfProgress$ | | | 0.024** | 0.024** |
| | | | (2.260) | (2.264) |
| $NCats$ | | | -0.003 | -0.003 |
| | | | (-1.487) | (-1.513) |
| $PortfolioSize$ | | | 0.001 | 0.001 |
| | | | (0.631) | (0.588) |
| $Duration$ | | | -0.122*** | -0.122*** |
| | | | (-20.992) | (-21.171) |
| $PrevRoundSize$ | | | -0.009** | -0.009** |
| | | | (-2.407) | (-2.483) |
| *VC Firm FE* | No | No | Yes | Yes |
| *Startup FE* | Yes | Yes | Yes | Yes |
| *Yr-Qtr. FE* | Yes | Yes | Yes | Yes |
| *ICD FE* | Yes | Yes | No | No |
| *N* | 12,765 | 12,765 | 34,045 | 34,045 |
| *Adj.* $R^2$ | 0.074 | 0.074 | 0.181 | 0.181 |

Table 10: Anticompetitive Effects and VC Influence

This table presents results from estimating our VC-funding regression (equation 3) in subsamples based on the VC's financial commitment to the startup. The dependent variable is an indicator for whether VC $j$ extends funding to startup $i$ in quarter $t$. Columns 1 and 2 compare results across low- versus high-commitment VCs, where a high-commitment VC is defined as one whose total investment in the startup to date is above the median across all the startup's VCs. Columns 3 and 4 compare results across subsamples of non-lead and lead VCs, where a lead VC is defined as the VC whose total amount invested to date is the highest across all the startup's VCs. This table reports OLS estimates. Remaining details and variable definitions are the same as in Table 5.

| | (1) **Low-Commitment VC** | (2) **High-Commitment VC** | (3) **Non-lead VC** | (4) **Lead VC** |
|---|---|---|---|---|
| $Shocked \times SharedVC$ | -0.007 | -0.052** | -0.001 | -0.105*** |
| | (-0.533) | (-2.386) | (-0.091) | (-3.142) |
| $Shocked$ | 0.053*** | 0.049*** | 0.055*** | 0.089*** |
| | (8.588) | (4.832) | (9.023) | (6.911) |
| $SelfProgress$ | 0.030*** | -0.022 | 0.015** | 0.026 |
| | (5.055) | (-1.523) | (2.258) | (1.436) |
| $NCats$ | -0.000 | -0.005** | -0.001 | -0.003 |
| | (-0.044) | (-1.974) | (-0.922) | (-0.965) |
| $NProjects$ | 0.002 | -0.000 | 0.001 | 0.007 |
| | (1.378) | (-0.010) | (0.505) | (0.973) |
| $NVCs$ | 0.011*** | 0.016*** | 0.011*** | 0.017** |
| | (6.368) | (4.671) | (6.482) | (2.222) |
| $PortfolioSize$ | 0.001 | 0.004 | -0.001 | 0.007** |
| | (0.564) | (1.350) | (-0.589) | (2.195) |
| $Duration$ | -0.072*** | -0.183*** | -0.100*** | -0.175*** |
| | (-13.383) | (-22.192) | (-18.992) | (-14.162) |
| $PrevRoundSize$ | -0.008* | -0.008 | -0.008** | -0.011 |
| | (-1.918) | (-1.205) | (-2.434) | (-1.106) |
| *VC Firm FE* | Yes | Yes | Yes | Yes |
| *Startup FE* | Yes | Yes | Yes | Yes |
| *Qtr FE* | Yes | Yes | Yes | Yes |
| *N* | 22,115 | 11,920 | 25,915 | 8,112 |
| *Adj.* $R^2$ | 0.122 | 0.237 | 0.140 | 0.233 |

Table 11: Project Outcomes in Cross-Sectional Regressions

This table reports results from a cross-sectional version of regression (1), with one observation per project. The dependent variable $Progress_i$ measures whether project $i$ ultimately progresses to Phase II. $Shocked_i$ is an indicator for whether another project in the same ICD category as project $i$ progresses to Phase II between project $i$'s initiation quarter and end of Phase I. $SharedVC_i$ is an indicator for whether project $i$ shares a VC with the project causing the shock. Column (1) reports results from OLS regressions. Columns (2) to (4) report results from probit-2SLS. Columns (5) and (6) contain results from the other simultaneous probit model. All other details are the same as in Table 3.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | **OLS** *Progress* | **Probit** *Shocked*× *SharedVC* | **1st Stage** *Shocked*× *SharedVC* | **2SLS** *Progress* | **Biprobit** *Shocked*× *SharedVC* | **Biprobit** *Progress* |
| $Shocked \times SharedVC$ | -0.216** | | | -0.629** | | -0.343** |
| | (-2.565) | | | (-2.215) | | (-2.392) |
| $\widehat{Shocked \times SharedVC}$ | | | 0.913*** | | | |
| | | | (3.035) | | | |
| $Shocked \times Proximity$ | | 1.040*** | | | 1.043*** | |
| | | (4.669) | | | (4.684) | |
| $Shocked$ | -0.211*** | | 0.145*** | -0.131* | | -0.396*** |
| | (-4.729) | | (3.407) | (-1.825) | | (-3.321) |
| 1*st stage F-stat* | | | 9.21 | | | |
| | | | (0.003) | | | |
| *Startup FE* | Yes | No | Yes | Yes | No | No |
| *ICD FE* | Yes | No | Yes | Yes | No | No |
| *Initial Qtr FE* | Yes | No | Yes | Yes | No | No |
| *N* | 800 | 1,045 | 797 | 797 | 1,045 | 1,045 |
| *Adj* $R^2$ | 0.295 | | 0.415 | | | |

Table 12: Project Suspensions

This table contains estimates of regression (1), replacing $Progress_{it}$ with $Suspend_{it}$, an indicator for whether project $i$ is suspended in quarter $t$. The sample contains all project-quarters when each project was in Phase I clinical trials. Dependent variables are indicated in the column titles. Columns 1 and 2 report OLS results, and columns 3 and 4 report probit-2SLS results. The probit and first stage are omitted as they are identical to Tables 3 and 4. Standard errors are computed by two-way clustering at the ICD category and startup company levels. $t$-statistics are in parentheses. Fixed effects are noted in the bottom row. ***,**, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | ***OLS*** | ***OLS*** | ***2SLS*** | ***2SLS*** |
| | *Suspend* | *Suspend* | *Suspend* | *Suspend* |
| $Shocked \times SharedVC$ | 0.022*** | 0.027*** | 0.218* | 0.187** |
| | (2.791) | (3.042) | (1.982) | (2.245) |
| $Shocked$ | 0.008 | 0.010 | -0.013 | -0.004 |
| | (1.291) | (1.001) | (-1.020) | (-0.277) |
| $Age$ | 0.016*** | 0.016*** | 0.013*** | 0.013*** |
| | (6.531) | (6.012) | (4.038) | (3.895) |
| $NProjects$ | 0.001 | 0.000 | 0.000 | -0.000 |
| | (1.243) | (0.174) | (0.346) | (-0.036) |
| $NVCs$ | -0.002 | -0.002 | -0.002 | -0.003 |
| | (-1.210) | (-0.937) | (-0.881) | (-1.062) |
| *Startup FE* | Yes | Yes | Yes | Yes |
| *Yr-Qtr. FE* | Yes | Yes | Yes | Yes |
| *ICD FE* | Yes | No | Yes | No |
| *ICD × Qtr. FE* | No | Yes | No | Yes |
| *N* | 12,769 | 11,782 | 12,769 | 11,782 |
| *Adj.* $R^2$ | 0.049 | 0.068 | | |

Table 13: Project Outcomes, Controlling for Technological Similarity

This table replicates columns 2–6 in Table 3, except we include $Shocked \times SharedCite$ in order to control for the similarity of technologies between the shocked project and the progressing project. All other details are the same as in Table 3.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | ***Probit*** *Shocked× SharedVC* | ***1st Stage*** *Shocked× SharedVC* | ***2SLS*** *Progress* | **Biprobit** *Shocked× SharedVC* | **Biprobit** *Progress* |
| $Shocked \times SharedVC$ | | | -0.134** | | -0.056** |
| | | | (-2.116) | | (-1.982) |
| $Shocked \times \widehat{SharedVC}$ | | 0.402** | | | |
| | | (2.514) | | | |
| $Shocked \times Proximity$ | 0.790*** | | | 0.778*** | |
| | (4.606) | | | (4.404) | |
| $Shocked \times SharedCite$ | 0.947*** | 0.029 | 0.025 | 0.950*** | 0.489** |
| | (3.459) | (0.390) | (1.480) | (3.464) | (2.354) |
| $Shocked$ | | 0.076*** | 0.014** | | -0.029 |
| | | (2.824) | (2.003) | | (-0.325) |
| $Age$ | 0.422*** | 0.002 | 0.012*** | 0.435*** | -0.013 |
| | (4.661) | (0.167) | (4.501) | (4.685) | (-0.254) |
| $NProjects$ | -0.005 | 0.005 | 0.002** | -0.005 | -0.024 |
| | (-0.254) | (1.543) | (2.190) | (-0.274) | (-1.382) |
| $NVCs$ | 0.096*** | -0.002 | -0.001 | 0.096*** | 0.030** |
| | (6.874) | (-0.350) | (-0.387) | (6.868) | (2.395) |
| $1st\ stage\ F\text{-}stat$ | | 6.32 | | | |
| | | (0.014) | | | |
| *Startup FE* | No | Yes | Yes | No | No |
| *Yr-Qtr. FE* | No | Yes | Yes | No | No |
| *ICD FE* | No | Yes | Yes | No | No |
| *N* | 12,782 | 12,769 | 12,769 | 12,782 | 12,782 |
| *Adj.* $R^2$ | | 0.563 | | | |

Table 14: Funding Outcomes, Controlling for Technological Similarity

This table replicates columns 2–6 in Table 5, except we include $Shocked \times SharedCite$ in order to control for the similarity of technologies between the shocked startup and the startup owning the progressing project. All other details are the same as in Table 5.

| | (1) **Probit** *Shocked× SharedVC* | (2) 1*st* **Stage** *Shocked× SharedVC* | (3) **2SLS** *ExtendFunds* | (4) **Biprobit** *Shocked× SharedVC* | (5) **Biprobit** *ExtendFunds* |
|---|---|---|---|---|---|
| $Shocked \times SharedVC$ | | | -0.391*** | | -0.141*** |
| | | | (-3.377) | | (-8.504) |
| $Shocked \times \widehat{SharedVC}$ | | 0.817*** | | | |
| | | (6.432) | | | |
| $Shocked \times Proximity$ | 0.362*** | | | 0.370*** | |
| | (4.222) | | | (4.548) | |
| $Shocked \times SharedCite$ | 0.739*** | -0.035 | 0.011 | 0.735*** | -0.006 |
| | (3.805) | (-1.183) | (0.587) | (3.752) | (-0.057) |
| $Shocked$ | | 0.008 | 0.080*** | | 0.450*** |
| | | (1.122) | (7.479) | | (7.651) |
| $SelfProgress$ | 0.261 | -0.025* | 0.014 | 0.285 | 0.316*** |
| | (1.492) | (-1.718) | (1.123) | (1.643) | (5.656) |
| $NCats$ | -0.016 | 0.008** | -0.000 | -0.017 | -0.000 |
| | (-0.529) | (2.555) | (-0.040) | (-0.571) | (-0.013) |
| $NProjects$ | 0.054 | -0.007 | 0.003 | 0.055 | 0.027 |
| | (1.086) | (-1.079) | (0.673) | (1.123) | (1.092) |
| $NVCs$ | 0.054 | -0.007 | 0.003 | 0.055 | 0.027 |
| | (1.086) | (-1.079) | (0.673) | (1.123) | (1.092) |
| $PortfolioSize$ | 0.104*** | -0.015*** | 0.000 | 0.107*** | 0.042*** |
| | (3.029) | (-3.573) | (0.012) | (3.184) | (3.160) |
| $Duration$ | 0.159*** | -0.013** | -0.125*** | 0.164*** | -0.744*** |
| | (2.669) | (-2.238) | (-19.934) | (2.891) | (-20.005) |
| $PrevRoundSize$ | -0.005 | -0.008 | -0.011** | -0.012 | -0.042*** |
| | (-0.107) | (-1.599) | (-2.451) | (-0.263) | (-2.618) |
| 1*st stage F-stat* | | 41.37 | | | |
| | | (0.000) | | | |
| *VC Firm FE* | No | Yes | Yes | No | No |
| *Startup FE* | No | Yes | Yes | No | No |
| *Yr-Qtr. FE* | No | Yes | Yes | No | No |
| *N* | 35,217 | 35,198 | 35,198 | 35,217 | 35,217 |
| *Adj.* $R^2$ | | 0.373 | | | |

Table 15: Project Outcomes and Enforcement of Non-Compete Agreements

This table examines whether treatment effects on project outcomes differ across states with high versus low enforceability of employee non-compete agreements. Columns 1 and 2 repeat our main OLS regressions of project outcomes, from column 1 of Tables 3 and 4, except we include interactions terms $Shocked \times SharedVC \times Noncompete$ and $Shocked \times Noncompete$. $Noncompete_{it}$ is an indicator for whether startup $i$'s state in year $t$ strongly enforces non-compete agreements. We obtains states' current non-compete statutes from Beck Reed Riden LLP (`http://www.beckreedriden.com/50-state-noncompete-chart-2/`). We record states' historical statute changes following Ewens and Marx (2018) and Jeffers (2019). We then use the U.S. Department of Treasury's report (`https://www.treasury.gov/resource-center/economic-policy/Documents/UST%20Non-competes%20Report.pdf`) to classify the enforceability of non-compete agreements into 5 levels: not enforced, undecided, red pencil, blue pencil, and reformation. We classify blue pencil and reformation as having a high degree of enforceability. Columns 3 and 4 replace *Progress* with *Suspend* as the dependent variable; these columns are comparable to Table 12. Standard errors are computed by two-way clustering at the ICD category and startup company levels. Fixed effects are noted in the bottom row. ***,**, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1) *Progress* | (2) *Progress* | (3) *Suspend* | (4) *Suspend* |
|---|---|---|---|---|
| $Shocked \times SharedVC$ | -0.017** | -0.018** | 0.024*** | 0.031*** |
| | (-2.577) | (-2.045) | (2.924) | (3.055) |
| $Shocked \times SharedVC \times Noncompete$ | 0.002 | 0.010 | -0.002 | -0.007 |
| | (0.236) | (0.742) | (-0.139) | (-0.468) |
| $Shocked$ | 0.004 | 0.016** | 0.001 | 0.002 |
| | (0.624) | (2.060) | (0.137) | (0.186) |
| $Shocked \times Noncompete$ | -0.000 | -0.003 | 0.011* | 0.013* |
| | (-0.020) | (-0.366) | (1.667) | (1.908) |
| $Age$ | 0.010*** | 0.004 | 0.016*** | 0.016*** |
| | (3.605) | (1.113) | (6.492) | (5.909) |
| $NProjects$ | 0.002** | 0.002** | 0.001 | 0.000 |
| | (2.030) | (2.026) | (1.268) | (0.223) |
| $NVCs$ | -0.002 | -0.003 | -0.002 | -0.002 |
| | (-0.797) | (-1.361) | (-1.205) | (-0.912) |
| *Startup FE* | Yes | No | Yes | Yes |
| *Yr-Qtr FE* | Yes | No | Yes | No |
| *ICD FE* | Yes | No | Yes | No |
| *ICD × Qtr. FE* | No | Yes | No | Yes |
| *N* | 12,769 | 12,782 | 12,769 | 12,782 |
| *Adj.* $R^2$ | 0.075 | 0.074 | 0.049 | 0.068 |