Entrepreneurial Spillovers from Corporate R&D

June, 2018

Tania Babina and Sabrina T. Howell*

Abstract

Using U.S. Census data, we show that corporate research and development (R&D) investment increases employee departures to entrepreneurship. We identify a causal effect with changes in federal and state tax incentives. High-tech parents drive the effect, and R&D-induced startups are much more likely to be venture capital-backed than the average employee-founded startup. This is the first evidence of employee startups as a type of R&D spillover. New ideas seem to be the primary channel, though new skills likely also play a role. R&D investment appears to lead to new growth options that are located outside the firm boundary.

*Columbia GSB, NYU Stern. Emails: tania.babina@gsb.columbia.edu; sabrina.howell@nyu.edu.
We are grateful to Manuel Adelino, Pierre Azoulay, Nick Bloom, Patrick Bolton, Alon Brav, Wes Cohen, Ryan Decker, Will Gornall, Bronwyn Hall, Wei Jiang, Bill Kerr, Saul Lach, Josh Lerner, Brian Lucking, Song Ma, Holger Mueller, Ramana Nanda, Stijn Van Nieuwerburgh, Paul Oyer, Tomek Piskorski, John Van Reenen, David Scharfstein, Neng Wang, and seminar participants at the Federal Reserve Board, the IMF, Duke, Toulouse School of Economics, U.S. Census Bureau, and Columbia. We also thank Chris Galvan and Shirley Liu for data assistance. The research in this paper was conducted while the authors were Special Sworn researchers of the U.S. Census Bureau at the Triangle Census Research Data Center. Research results and conclusions expressed are those of the authors and do not necessarily reflect the views of the U.S. Census Bureau. This paper has been screened to ensure that no confidential data are revealed.

1 Introduction

Corporate investment in research and development (R&D) creates new growth options. The optimal boundaries of the firm depend on the nature of the growth options it creates and on limits to contracting with employees (Grossman & Hart 1986, Zingales 2000). As a result, some new growth options may end up outside the firm boundary in startups founded by former employees. Though there are many anecdotes of employee departures from innovative incumbents to entrepreneurship, it is not obvious that R&D will increase employee startups.[1] Instead, R&D might lead the firm to grow internally or become a more interesting workplace, leading to greater employee retention. To our knowledge, this is the first paper to explore startup creation as a type of R&D spillover.

We ask how R&D affects employee departures to entrepreneurship using U.S. Census employer-employee matched panel data.[2] Our baseline model shows that a 100 percent increase in firm R&D leads to a 8.4 percent increase in employee-founded startups. Over the course of the sample, above- relative to below-median R&D changes yield 10,541 additional employee-founded startups, which is 9.7 percent of all employee-founded startups in the data. These R&D-induced startups are much more likely to be venture capital-backed than the average employee-founded startup, suggesting that high-risk, high-reward growth options from R&D are more often reallocated. Gromb & Scharfstein (2002) model how such growth options benefit from the higher-powered incentives of stand-alone entrepreneurial firms (also see Robinson 2008).

The baseline model includes firm, state-year, and industry-year fixed effects, as well as a rich array of time-varying firm characteristics, including total investment and establishment-level wages and employment. The results are robust to several alternative outcome variables, such as the number of startups founded by recently departed employees. Despite these fine controls, the estimate may be biased upwards if an unobserved new technological opportunity leads to both higher parent R&D and more employee-founded startups. Alternatively, the effect of R&D on employee

[1] For example, David Friedberg left Google in 2006 to found WeatherBill (later The Climate Corporation), an agricultural insurance startup. See here and more examples in Section 5.1.3.

[2] For each public firm establishment-year, we follow departing workers and examine whether they are on the founding team of a new firm (top five earners of a firm founded within three years of when R&D is measured).

entrepreneurship may cause the parent to underinvest ex-ante, resulting in downward bias.

To address such concerns, we instrument for R&D using changes in state and federal R&D tax credits, following Bloom, Schankerman & Van Reenen (2013). Changes in tax credits affect the firm's tax price of R&D and thus its incentives to invest in R&D. We provide exhaustive detail on the sources of within-firm variation for both instruments. The federal tax credit is firm-specific for five reasons, most importantly because it depends on firm age, with annual changes for most firms. The state instrument is firm-specific because it is calculated using the time-varying share of the firm's patent inventors located in a given state. The instruments satisfy the relevance condition and are likely to satisfy the exclusion restriction.[3]

The instrumental variables (IV) effect of R&D on employee-founded startups is larger than the main effect and is equally robust. This offers strong evidence that the relationship is causal. The larger instrumented effect could reflect downward bias in the OLS result. Alternatively, the causal effect may be higher among compliers with the instrument. This could occur if employee startup propensity is correlated with adjustable R&D, which may be more general or creative, more often begetting ideas best suited to development outside the firm. It could also be less important to the firm, reducing ex-ante incentives to prevent employee startups that use its output. Either way, the large IV estimate suggests that R&D tax credits stimulate greater R&D-induced employee entrepreneurship. A slightly different interpretation is that the IV estimate represents the marginal effect of R&D (the effect of the "last" R&D dollar), which may generate ideas that are further from the parent's core focus than the "average" dollar of R&D captured by the OLS estimate, explaining why the growth options it creates more often optimally reside outside the firm's boundary. The true economic magnitudes likely lie between the OLS and IV estimates.

The startup creation spillovers that we document could arise from two channels. In what we term the intellectual capital channel, R&D generates new ideas

[3]To satisfy the relevance condition, we present evidence from the literature that the elasticity of R&D spending to tax credits is at least one. To satisfy the exclusion restriction, we show empirically that there is no relationship between the tax credit and employee-founded startups, and present evidence from the legal literature that R&D tax credits are not in general useful to startups.

or technologies that are deployed by employees in new firms. The alternative channel is human capital, where learning-by-doing during the R&D process increases employees' entrepreneurial skills. Both of these channels are likely at play, but the cross-sectional evidence is more consistent with intellectual capital. High-tech parents and those that do broader research have higher effects per dollar of R&D. Further, within the population of employee startups, higher parent R&D is strongly associated with venture capital backing. This associates the effect with new-to-the-world ideas, rather than "Main Street"-type businesses.

The intellectual capital channel is consistent with R&D-induced employee entrepreneurship being a direct avenue for R&D spillovers.[4] R&D spillovers are crucial to economic growth (Marshall 1920, Krugman 1991, Klenow & Rodriguez-Clare 2005). They also seem to be large in magnitude (Jaffe, Trajtenberg & Henderson 1993, Jones & Williams 1998, Griffith, Harrison & Van Reenen 2006, Bloom et al. 2013, and Kerr & Kominers 2015). However, they are difficult to observe, and little is known about their transmission channel. We also do not know much about the identity of spillover recipients; the literature has typically assumed that potential recipients are close in technological or geographic space. Research at the individual level has focused on inventor networks, particularly in academia (Azoulay et al. 2010, Waldinger 2012). We extend this literature by documenting a specific transmission channel for R&D spillovers, and by focusing on R&D inputs and the firm boundary rather that patents.

The loss of the employee and R&D output may be costly to the parent, but several tests suggest that the costs are not extremely large. We expect that if the effect is very costly to the firm, it will be smaller in states that strictly enforce non-compete covenants. Instead, those states exhibit a similar effect as states that weakly enforce non-competes. We also expect that if the effect is very costly, it will be smaller in sectors where intellectual property is easier to protect. Instead, it is equally strong in these sectors. Further, R&D-induced startups likely do not compete in product markets with their parents, because they tend to be in different industries. Industry classifications reflect the firm's market more than its technology, so an

[4] R&D spillovers are benefits from one firm's innovation efforts that accrue to other firms, and which are not embodied in products and services (Griliches 1992).

employee startup may use technology that is related to the parent's but apply it to a different market. Also, parents do not seem to internalize the benefits of R&D-induced startups by investing in or acquiring them. We do not assess the welfare effects of R&D-induced startups, but our results imply that firms underinvest in R&D relative to the social optimum, which would incorporate the social and private benefits of R&D-induced startups. Relative to incumbent firms, new firms have faster productivity and employment growth.[5] Acemoglu, Akcigit, Bloom & Kerr (2013) conclude that R&D subsidies may be misguided because they favor incumbents at the expense of entrants. If entrepreneurial spillovers from corporate R&D were included in their model, the policy implications might be somewhat different.

Our finding is specific to R&D inputs. Patents and patent citations, the standard measures of R&D output, have no effect on employee entrepreneurship. This implies that innovation outputs over which the firm does not establish explicit, contractible ownership generate startups. The distinction between investment and patenting sheds light on the limits of contracting innovation (Holmström 1999, Aghion & Tirole 1994). Because contracting innovation is difficult, the firm may find it optimal instead to permit some R&D-induced startups. In the framework of Grossman & Hart (1986), a new idea is optimally located outside the firm boundary – giving the employee residual rights of control – when it will be too costly for the firm to specify employee responsibilities. Phillips & Zhdanov (2012) and Frésard, Hoberg & Phillips (2017) show how it can be optimal to locate innovation in small, non-vertically integrated firms. There is likely a tradeoff between the agency benefits of locating the idea in a new firm and lower transaction costs in internal capital markets (Gertner, Scharfstein & Stein 1994).

Beyond limits to the parent firm's ability to prevent R&D-induced startups, the firm may sometimes find it optimal to forego diversification by permitting the employee to depart with a new growth option. Rejecting ideas that fit poorly with existing capabilities may often be optimal in light of the negative correlation between

[5] See Kortum & Lerner (2000), Foster, Haltiwanger & Syverson (2008), Haltiwanger, Jarmin & Miranda (2013) Decker et al. (2014), and Glaeser, Kerr & Kerr (2015), among others.

firm performance and increases in diversification (Lang & Stulz 1994, Schoar 2002).[6] Seru (2014) shows that diversification reduces innovation novelty and points out that information asymmetry impedes central monitoring of divisional research activities. A permissive policy towards employee-founded startups could allow the firm to maintain the benefits of focusing on existing products and customers and could dynamically incentivize research employees to maximize effort.

Finally, our results indicate that corporate R&D offers a new source for where ideas for high-growth startups come from (Cohen & Levinthal 1989, Aghion & Jaravel 2015, Babina 2017, Babina, Ouimet & Zarutskie 2018, Guzman & Stern 2017). There is existing evidence that many successful entrepreneurs are former employees of high-tech, large firms (Bhide 2000, Klepper 2001, Gompers, Lerner & Scharfstein 2005).[7] Employee-founded startups have also been an important mechanism in research on agglomeration and innovation clusters, including the seminal papers by Saxenian (1990) and Gilson (1999). More broadly, our paper is related to the literature on knowledge diffusion through labor mobility, including Almeida & Kogut (1999), Kim & Marschke (2005), Matray (2015), and Herkenhoff, Lise, Menzio & Phillips (2018). To our knowledge, no existing work considers the role of parent innovation inputs in employee-founded startups.

2 Data

We use data from five sources: Compustat, Census LBD, Census LEHD, VentureXpert, and the NBER Patent Data Project. This section describes each source of data and explains the key variables we use in analysis. It also discusses concerns with the data

[6]This may be due to profit maximization contingent on firm characteristics (Campa & Kedia 2002, Maksimovic & Phillips 2002, Graham, Lemmon & Wolf 2002), or it may reflect value-destroying behavior, such as inefficient cross-subsidization (Scharfstein & Stein 2000, Rajan, Servaes & Zingales 2000). However, Whited (2001) and Villalonga (2004) argue that measurement error explains some of the discount evidence.

[7]Additional theoretical and empirical work from the management literature on employee-founded startups and spinoffs includes Klepper & Sleeper (2005), Franco & Filson (2006), Klepper (2007), Hellmann (2007), Nanda & Sørensen (2010), Chatterji (2009), Sørensen (2007), Klepper & Thompson (2010), Campbell et al. (2012), Habib, Hege & Mella-Barral (2013), Agrawal, Cockburn, Galasso & Oettl (2014).

and sample.

2.1 Compustat

Our measure of corporate innovation investment is R&D expenditure as reported in 10K filings and provided by Compustat. As R&D expenditure is only available for public firms, they form our universe of firms at hazard of being parents to employee-founded startups. We primarily use log R&D, but also test whether the results are robust to using R&D divided by total assets. We also obtain balance sheet and income statement data about the potential parents from Compustat.

We consider only firms with positive R&D for two reasons. First, firms that report R&D are likely qualitatively different from firms that do not in ways that might affect employee entrepreneurship, despite rigorous controls and fixed effects (Lerner & Seru 2017). Second, our primary specification will be focused on the intensive margin; since we use firm fixed effects, firms with zero R&D provide no variation. However, in a robustness check we include all Compustat firms and find similar results to the main specification.

2.2 Census LBD

We merge Compustat to the restricted-access U.S. Census Bureau's Longitudinal Business Database (LBD) using the internal Census Compustat/LBD crosswalk. The LBD is a panel dataset that tracks all U.S. business establishments with paid employees, providing information on the number of employees and annual payroll. An establishment is any separate physical location operated by a firm with at least one paid employee. The LBD contains a unique firm-level identifier, *firmid*, which longitudinally links establishments that are part of the same firm. For further details about the LBD, see Jarmin & Miranda (2002).

We use LBD data for all 50 states from 1990 to 2011, and track establishments and firms consistently over time. We use data on establishment age, industry, physical location, total employment, payroll, birth, and death. We can therefore identify new employer firms and their future employment growth, payroll, and exit. We define age as the oldest establishment that the firm owns in the first year the firm is observed in the LBD, as in Haltiwanger et al. (2013). A firm birth is defined when all of its establishments are new, preventing us from misclassifying an establishment that changes ownership as a startup. From the point at which it is first observed, firm age rises naturally over the years.

2.3 Census LEHD

A challenge when studying how R&D affects employee departures to entrepreneurship is that we must observe employees and track them from firm to firm. We solve this with the Longitudinal Employer-Household Dynamics (LEHD) program at the U.S. Census Bureau, which provides firm-worker matched data. This permits us to track salaried employees over time and across firms on a quarterly basis. Census builds these data using scrambled social security numbers. In addition to wages, the data contain employees' gender, race, place and date of birth, and citizenship status. Coverage starts in 1990 for several states and increases over time, ending in 2008. We have access to 31 states, shown in Figure 1, in which we observe all employee-founded startups.

The LEHD has been widely used in economic research (e.g. Tate & Yang 2015 and Goldin et al. 2017). In covered states, the LEHD includes over 96 percent of all private-sector jobs and over 96 percent of total wage and salary civilian jobs, so there is no problem with employee self-selection (BLS 1997, Abowd et al. 2009). There is nonetheless some concern about coverage. About 10 percent of workers in year t are not in the LEHD in year $t+3$, though the U.S. Current Population Survey has a similar attrition rate.[8]

The LEHD data we use covers over 60 percent of U.S. employment, with representative industry composition. To establish this, we compare our data to data from the Bureau of Labor Statistics (BLS) Current Employment Statistics (CES) Survey from 1990-2008.[9] We divide state-industry level employment by total state

[8]The CPS tracks workers for a maximum of 16 months. In the CPS data, among private sector employees who are observed 15 months later, about 9 percent drop out from the employment sample. Based on IPUMS-CPS data, available at https://cps.ipums.org/cps/.

[9]According to the BLS, employment data comes from a voluntary state level stratified sample of firms that is adjusted for population using monthly state unemployment insurance records.

employment across all states in our sample. We do this for each year, and then average across years. We compare this to the analogous figure for states out of our sample. The result is shown in Appendix Table 1. For example, in the 1990-2001 period, Manufacturing represents 15.4 percent of employment in our sample states, and 15.8 percent of employment in other states. In the 2002-08 period, Professional and Business Services represents 12.3 percent of employment in our sample states, and 12.8 percent of employment in other states. A second calculation considers the share of people employed in an industry in our sample states versus the other states. The results are in Appendix Table 2. The share of employment for each industry is quite similar to the overall share of employment we observe. For example, between 2002 and 2008, we observe 60.9 percent of total employment, 57.5 percent of Information employment, 59.9 percent of Professional and Business Services, and 60.6 percent of Manufacturing.

The LEHD connects quarterly earnings from the state Unemployment Insurance programs to the Quarterly Census of Employment and Wages Program. Abowd et al. (2009) describe the construction of this data in detail. Workers' employers are identified with State Employer Identification Numbers (SEIN, the state equivalent to EIN). We link firms in the LEHD to those in the LBD using the *firmid* and SEIN identifiers in the first quarter of each year. The linkage is accomplished with the aid of an internal Census bridge file. We drop SEINs with less than ten employees, as they tend to have noisy reporting.[10] This yields an annual panel of SEINs of the LBD firms, in which employees are observed as of the first quarter of each year. For ease of exposition, we term SEINs "establishments."

2.3.1 Identifying Employee-founded Startups

The final sample consists of an annual panel of public firm establishments in 31 states between 1990–2005. Using the establishment as the primary unit of analysis permits fine controls such as establishment average wage, and establishment-specific industry-year and state-year fixed effects. We follow startup creation from 1990 to 2008. To identify employee-founded startups, we begin by observing worker identities

[10] We obtain similar results if we drop those with less than five or less than 15 employees.

at public firm establishments in the first quarter of year t, and the quantity of R&D investment in year year $t-1$. We denote an establishment e. Using longitudinally consistent individual identifiers available in the LEHD data, we follow the establishment e's employees one, two, and three years after year t.

The LEHD data do not designate the founder(s) of a new firm. We proxy for an individual being on the founding team using the highest earners at a young firm. While this is an imperfect measure of entrepreneurship, we believe that it is the best available in administrative data. It is supported by the evidence in Kerr & Kerr (2017) that a firm's top three initial earners usually include the firm's owners. It is also in line with prior research focusing on the executive team, including Gompers et al. (2005). As Azoulay et al. (2017) point out, the W-2 data that is the basis for the LEHD must be filed for all employees, including owners who actively manage the business. Note also that the law requires managers to pay themselves reasonable wage compensation.[11]

Founders may not pay themselves the highest wage if they seek to attract high-skill employees. Therefore, using only the highest earner is unlikely to capture all founders. Our definition captures both founders and the early employees who are important to the startup's initial success. Our primary definition of an employee-founded startup is a firm founded between t and $t+3$ in which any of the parent firm establishment's employees at year t is among the top five earners as of $t+3$. To arrive at our primary outcome variable – an establishment's rate of employee departures to new firms – we divide the number of founders by e's total number of employees in year t. We use the establishment rate of employee entrepreneurship because we are most interested in the individual's decision to leave; the results will be informative about labor mobility. In an alternative specification, we also show the effect on the number of employee-founded startups.

There are four other outcomes for e's year t employees. First, they may remain at the firm. Second, they may be employed at a different firm that existed before year t (other incumbents). Third, they may be employed at an institution with unknown age (because some LEHD employers are non-profits, government entities, or non-employer firms not covered by the LBD, which is used to determine employer age). Finally, the

[11]See https://www.irs.gov/uac/Wage-Compensation-for-S-Corporation-Officers.

employee may no longer be observed in the data because he/she left the work force, no longer earns a wage, or otherwise fails to be covered by the LEHD (see Section 2.3 for representativeness). We use these outcomes in robustness tests, for example to test whether R&D also leads to greater labor mobility to other incumbent firms. A final concern is that parent R&D is correlated with worker mobility to or from uncovered state. If this is the case, then R&D should correlate with the fraction of workers who drop out of sample. We find that this is not the case (Table 3).

2.4 Venture capital and patent data

We use a linking between ThomsonOne VentureXpert and the Census Business Register to identify venture capital-backed startups from the bridge constructed by Puri & Zarutskie (2012). We use patent data from the NBER Patent Data Project, which includes patent and citation variables through 2006. The NBER data include Compustat identifiers. We use patent data to construct the instrument for R&D.

We employ several annual patent-based variables at both the firm and industry level. These are the number of patent classes a firm or industry patents in, the number of patents, the number of forward and backward citations, and the average, maximum, and median patent generality and originality. Generality is higher (closer to one than zero) when forward citations are in many classes, and originality is higher when backward citations are in many classes. In analysis, we use indicators for having an above median value for each patent variable within a year.

2.5 Summary statistics

Table 1 panels 1-3 show summary statistics at the parent firm-year, parent establishment-year, and employee-founded startup levels, respectively. We show the mean for indicator variables, as well as the quasi-median and the standard deviation for continuous variables.[12] Our main dependent variable, employee entrepreneurship,

[12]Since Census disclosure procedures prohibit disclosure of percentile value, we approximate median with a quasi-median, which is estimated using a 99 percent weight on observations within the interquartile range and a 1 percent weight on the remaining observations. The number of observations and all estimates in the tables are rounded according to the Census disclosure requirements.

is measured at the establishment-year level (panel 2). These are the set of establishments of public firms with positive R&D and at least 10 employees, between 1990 and 2005 (recall that the sample goes through 2008, but we allow three years to follow workers). On average, 1.3 percent of an establishment's employees separate and are identified as entrepreneurs three years later. Similarly, using the LBD/LEHD matched data Kerr et al. (2015) find that 1.7 percent of workers transition to entrepreneurship over a four-year period.

Panel 3 of Table 1 present summary statistics of the 108,000 employee-founded startups identified in the LBD. When we observe a former public firm employee first working at an employee-founded startup (three years after R&D is measured), the startup is on average 1.6 years old and have 14.6 employees. Two percent of employee-founded startups ever receive venture capital funding. This is much higher than estimates of the rate of venture capital backing among the whole population of firm founders. Puri & Zarutskie (2012) find, also using Census data, that just 0.11 percent of new firms receive venture capital. Startups founded by recent employees of public firms with positive R&D are thus around eighteen times more likely to receive venture capital than the average firm.

3 Empirical Approach

The primary estimation strategy, a tightly controlled fixed effects regression, is introduced in Section 3.1. In Section 3.2, we explain our instrumental variables strategy.

3.1 Reduced Form Relationship

We estimate variants on Equation 1, where e denotes an establishment, f a firm, and t the year. As described in Section 2.3.1, the dependent variable is the percent of e_t's employees who are among the top five earners at startups as of $t + 3$.

$$\begin{aligned} \text{Employee entrepreneurship}_{efist+3} &= \beta \ln\left(\text{R\&D}_{f,t-1}\right) \qquad (1) \\ &+ \text{Firm FE}_f + \text{Industry-year FE}_{e,t} + \text{State-year FE}_{e,t} \\ &+ \text{Controls}_{f,t} + \text{Controls}_{e,t} + \varepsilon_{e,f,t} \end{aligned}$$

We employ firm fixed effects to control for time-invariant differences across firms. We expect omitted variables to be correlated within the firm, so we cluster standard errors by firm. Industry-year fixed effects (using SIC three-digit codes) control for changes in investment opportunities, and also subsume industry and year effects. We also use SIC four-digit industry code fixed effects in some specifications (not interacted with year). State-year fixed effects control for regional shocks, which may affect investment opportunities at incumbents as well as entrepreneurship.

Time-varying establishment and firm controls address other concerns. First, we control for establishment size, in case, for example, smaller establishments have more focused or autonomous cultures and thus lead to more employee entrepreneurship. Second, we control for the establishment's average wage, which serves two purposes. First, R&D may be associated with increases in wages. Second, employee entrepreneurship could be driven by higher paid workers rather than R&D. We also include the following firm-level controls, which might correlate with R&D and employee entrepreneurship: return on assets, sales growth, Tobin's Q, asset tangibility (measures as PPE investment divided by total assets), size (log total assets), cash holdings, age, and diversification (indicator for firm having establishments in more than one SIC 3-digit industry).

3.2 Instrument for R&D

The central challenge to Equation 1 is that an unobserved demand shock or new technological opportunity, not captured by our granular industry-year fixed effects, may jointly engender parent R&D and employee entrepreneurship. This is a version of the Manski (1993) reflection problem. The ideal experiment would randomly allocate R&D to firms and observe whether firms assigned to more R&D have more employees that leave to found their own firms. This is infeasible, so we use the best available instrument for R&D expenditure: changes in the tax price of R&D, induced by state and federal R&D tax credits, following Bloom et al. (2013).

This section first describes the motivation for the instrument (Section 3.2.1), and then addresses the expected direction of endogeneity (Section 3.2.2). In Section 3.2.3, we briefly explain the two tax prices of R&D that we use. Appendix Section 6 contains exhaustive details about the federal tax credit and its calculation, the state tax credits, and concerns with instrument validity. While imperfect, we show that the instrumental variables strategy is well-suited to our context and is likely to satisfy the exclusion restriction.

3.2.1 Instrument motivation

We use two instruments: federal tax credit changes and state tax credit changes. These have been shown to be important drivers of corporate R&D expenditure. First, the federal R&D tax credit has a strong effect on corporate R&D in the short and long term. The elasticity is at least one, such that an extra dollar of federal R&D tax credits stimulates roughly a dollar of additional R&D expenditure (or much more, in some studies). This evidence includes Hall (1993), McCutchen (1993), Mamuneas & Nadiri (1996), Hall & Van Reenen (2000), Billings et al. (2001), Bloom et al. (2002), Klassen et al. (2004), and Clausen (2009). The relative sensitivity to the R&D tax credit may reflect the fact that firms tend to finance R&D out of free cash flows (Brown & Petersen 2011).

Second, state R&D tax credits increase R&D within the affected state, as shown by Paff (2005), Wu (2008) and Wilson (2009), among others. The most conservative finding is in Wilson (2009), where a one percentage point increase in the state tax credit rate increases R&D by 1.7 percent in the short term and 3-4 percent in the longer term. However, Wilson (2009) also finds that the tax credits cause a reallocation of R&D activity geographically. Since large, multi-state firms are responsible for most R&D expenditure, and they may shift R&D across states in response to the tax credits while our independent variable is firm-wide R&D, we expect the state instrument to be generally weaker than the federal one.

3.2.2 Expected Direction of Endogeneity

There are two major sources of endogeneity that may bias the ordinary least squares (OLS) estimates: 1) technology shocks to the firm's industry would bias the estimates upwards, and 2) the firm's inability to fully capture the benefits of R&D would bias the estimates downwards. An example of the first source is a scientific discovery at a university that creates new opportunities for the firm's industry. This may increase both firm R&D and entrepreneurship rates. The second source of endogeneity stems from firms' investment being correlated with their ability to capture the its benefits. This point is widely used to justify government subsidy of corporate R&D (Feldman & Kelley 2006, Howell 2017). The presence of an effect of R&D on employee-founded startups represents benefits that the parent firm is not capturing. This second source implies that if a firm's R&D expenditure could be randomly increased, a larger fraction of the additional R&D output would be developed outside of the firm's boundaries in startups founded by former employees.

In our setting, do we expect that endogeneity biases the OLS result upwards or downwards? While it is possible to tell stories going both ways, we believe it is more likely that endogeneity biases the OLS result down. Two facts suggest that positive bias due to technology shocks is unlikely. First, adding industry-year fixed effects to specifications with firm fixed effects does not attenuate the estimates. Second, an opportunity shock in a given sector should lead to both more R&D and more startup formation in that sector. We find that the R&D-induced employee-founded startup's line of business tends to be unrelated to the parent's. While it is more difficult to test for the negative bias due to appropriation concerns, the instruments proposed in the following sections will address this concern.

3.2.3 Summaries of the tax credits

Changes in tax credits affect firm incentives to invest in R&D, because they change the firm-specific tax price of R&D (i.e., the user cost of R&D capital). The tax credits are not deductions. Instead, they reduce the firm's corporate income tax liability by the value of the credit. The first instrument is the federal tax price of R&D, which we denote ρ^F_{ft}. The Appendix contains a detailed description of the calculation, which draws from Hall (1993). We explain in the Appendix that the federal tax credit value depends on the firm's qualified research expenditures and, crucially, a fixed base R&D spending. The credit is firm-specific for five reasons, including because it depends on firm age (more specifically, years since the firm's first positive R&D investment), with annual changes for most firms. We find substantial within-industry variation in the tax price of R&D, as well as the necessary variation within firm over time.

The state instrument, also described in more detail in the Appendix, requires two objects: the state tax price component of the R&D user cost of capital, and a measure of the share of a firm's R&D that occurs in a given state. First, we use the state tax price of R&D in Wilson (2009). He incorporated state level corporate income taxes, depreciation allowances, and R&D tax credits into this tax price component, which we call ρ^S_{st}. These credits vary across states and time. To build the second object, θ_{fst}, we follow Bloom et al. (2013). θ_{fst} is a proxy for a firm's R&D share in a given state-year calculated using the share of the firm's patent inventors located in state s. The firm's state-level tax price is then $\rho^S_{ft} = \sum_s \theta_{fst}\rho^S_{st}$.

3.2.4 First stage estimation

Having constructed firm-level federal and state tax prices of R&D (ρ^F_{ft} and ρ^S_{ft} , respectively), we estimate the following first stage regression:

$$\begin{aligned}\ln(R\&D_{ft}) = \beta_1 \ln\left(\rho^S_{ft}\right) + \beta_2 \ln\left(\rho^F_{ft}\right) + \text{Firm FE}_f + \text{Industry-year FE}_{it} \\ + \text{State-year FE}_{st} + \text{Controls}_{ft} + \varepsilon_{eft}\end{aligned} \tag{2}$$

We cluster standard errors by firm. The results are in Table 3. We show all of the specifications that we will show in the instrumented results table. The instruments are strong, yielding F-statistics of about 25, well above the rule-of-thumb cutoff of ten. The partial R^2 of the two instruments ranges from 2.2 to 3.2 percent, which captures a reasonable amount of variation in R&D (Jiang 2015). The federal instrument is stronger than the state instrument, which in part reflects the fact that the state instrument is identified by firms with patents. As we show below, our main result is not driven by firms with patents, but rather by firms in high-tech sub-sectors.

Note that Bloom et al. (2013) use only firm and year fixed effects. This is equivalent to column 1. In Column 2, we add firm time-varying controls, which reduce the magnitude of the effects somewhat but do not affect their statistical significance. We show a variety of specifications; our preferred specification, with SIC 3-digit industry-year and state-year fixed effects, along with firm time-varying controls and firm fixed effects, is in column 5. The results are also robust to using SIC 4-digit industry fixed effects (column 6).

3.2.5 Concerns with the instrument

There are five potential concerns with the instrument, which we describe in detail in the Appendix. Here, we summarize the two more important ones. First, the exclusion restriction is that tax credits cannot affect employee entrepreneurship. We show empirically that there is no relation between the state tax credits and startup creation. Also, Curtis & Decker (2018) show in a border-county differences-in-differences model that R&D tax credits have no effect on startup formation. More generally, the legal literature has argued that R&D tax credits are not very useful to startups because they usually do not have taxable income (Bankman & Gilson 1999).[13]

The second concern is that changes in state-level R&D tax credits may lead firms to reallocate R&D (or misreport it such that it appears reallocated). Any such reallocation should reduce the power of the instrument. This leads us to expect that the federal instrument will have more power than the state instrument, which is indeed what we find. In sum, R&D tax credits offer the best available source of variation driving corporate R&D, which is plausibly unrelated to technological or demand shocks that could jointly give rise to parent R&D and employee entrepreneurship.

[13]The presence of carry-forwards may make the credits somewhat useful to some startups, but our evidence in the Appendix suggests any effect is not large enough to be measurable.

4 Results

This section first explains our main results (Section 4.1). We present the instrument result in Section 4.2. In Section 4.3, we consider alternative measures of entrepreneurship and R&D. Alternative explanations for the main effect, such as firm restructuring, are addressed in Section 4.4. Reverse causation is examined in Section 4.5.

4.1 Main results

We present the main results from estimating Equation 1 in Table 2. Our preferred specification in column 5 includes firm, industry-year, and state-year fixed effects. The coefficient of 0.109 implies that a 100 percent increase in R&D is associated with a 8.4 percent increase in employee entrepreneurship, relative to sample mean of 1.3 percent.[14] The main result is remarkably robust to a wide array of alternative controls and fixed effects as shown across the eight models in panels 1 and 2. For example, the result is robust to using SIC 4-digit industry fixed effects (panel 1 column 4 and panel 2 column 1).

Our baseline set of firm-level controls are reported in Panel 1. We do not report them in further results because we are strictly limited by the Census Bureau in the number of coefficients we may disclose. The controls are at the firm level, except for employment and payroll which are at the establishment level. The only control with any predictive power is employment; employee entrepreneurship is negatively associated with the establishment's number of employees, consistent with the finding in Elfenbein et al. (2010). Some controls are denoted with a lag $(t-1)$ and others are not. This is because firm-level controls are measured when R&D is measured (last quarter of year $t-1$), but establishment-level variables are measured when the employee snapshot is taken (first quarter of year t). We use alternative controls in panel 2 columns 2 and 3. Column 2 employs establishment employee-level controls. Establishments with a higher share of white workers or foreign-born workers are associated with more

[14] As R&D is in log units, the coefficient means that a 1 percent increase in R&D increases employee entrepreneurship by .109/100.

employee entrepreneurship. Note that the results do not attenuate with wage controls, so the effect is not driven by an increase in employee wages.

We include measures of patenting activity as controls in Table 2 panel 2 column 3. These variables are measured at the firm level. Neither the number of patents nor the two citation measures predict employee entrepreneurship. However, patenting in more classes is associated with employee entrepreneurship. These controls do not attenuate the main estimate. Therefore, innovation inputs rather than patenting outputs yield employee entrepreneurship. It is natural that patenting does not explain the effect because it emerges precisely from those R&D products over which the firm does not establish explicit, contractible ownership.

This distinction between R&D and patenting sheds light on the limits of contracting on innovation. In a world of perfect information, the parent firm could patent all the good ideas that emerge from R&D, sell those they do not wish to develop in-house, and contract with the employee ex-ante such that he will not wish to depart to start his own firm. It is well known that innovation effort is difficult to contract (Holmström 1999, Aghion & Tirole 1994). Prior literature on innovation and worker mobility, including Gompers et al. (2005) and Matray (2015), has focused on contractible innovation (patents). This may make it optimal for the firm not to try to write such contracts and instead to be permissive towards some amount of R&D-induced employee departures to entrepreneurship. However, relative to an optimal contracting scenario, this permissiveness likely comes at the expense of some foregone R&D.

4.2 IV Result

The results from the instrumented second stage are in Table 4. (The first stage results are described in Section 3.2.4 and are in Table 3.) We repeat the specifications from Table 2. The coefficients in all models are statistically significant, and they are larger than the OLS results. Our preferred specification, in column 5, is about five times the OLS estimate. The larger instrumented effect indicates that the subset of R&D expenditure affected by the tax credits leads to greater employee entrepreneurship than the average increase in R&D. This could reflect endogeneity that biases the OLS result downward, as discussed in Section 3.2.2 above. However, the local average treatment effect for the complier subset may also be larger than the population average treatment effect. As Angrist & Imbens (1995) and Jiang (2015) explain, this can lead an IV strategy to produce larger effects than the true effect, even if the exclusion restriction is satisfied. That is, compliers with the instrument (in our case, the change in the tax price of R&D), may be those firms with a higher causal effect of R&D on employee entrepreneurship.

There are three possible explanations for such a phenomenon. First, there may be a correlation between propensity to generate employee-founded startups and adjustable R&D. That is, firms whose R&D is more sensitive to its tax price may also be doing the sort of R&D that leads to more employee entrepreneurship. Adjustable R&D may tend to be more general or inventive, and thus more often yield new ideas best suited to development outside the firm. It is not obvious why adjustable R&D would be more inventive, but we cannot rule out this possibility.

A second, more plausible explanation is that adjustable R&D is less crucial to the firm. The loss of the innovation output to employee-founded startups would then be less costly, implying lower ex-ante incentives to prevent employee entrepreneurship. That is, if the managers making R&D investment decisions are rational and have some information about the expected treatment effect, then costly employee entrepreneurship should lead them to increase R&D less in response to the tax price shock than a firm for which employee entrepreneurship is less costly. If R&D-induced startups are less costly to compliers with the treatment (the tax shock), we expect the IV estimate to exceed the OLS estimate. To the degree the effect represents a R&D spillover, this interpretation is relevant to policy: the large IV estimate suggests that R&D tax credits stimulate greater R&D-induced employee entrepreneurship.

The third possibility is that the IV estimate represents the marginal effect of R&D, which is higher than the average effect. Note that OLS estimates the effect of an additional dollar of average R&D. The IV strategy, which uses additional R&D tax subsidies to approximate increased R&D expenditure on the margin, better captures the effect on employee entrepreneurship of the "last" R&D dollar. This marginal R&D is likely farther from the parent's core focus than average R&D, which may make it either less costly to lose or harder to protect.

If endogeneity biases the OLS result down, or if we capture the marginal effect of R&D better in the IV, then the IV estimate better approximates the true effect. Conversely, if the IV strategy isolates those firms whose cost of R&D-induced employee entrepreneurship is especially low, or for which adjustable R&D is otherwise correlated to employee entrepreneurship, then the LATE in the IV is biased upward, and we should assume that OLS yields a better approximation of the true effect. The true economic magnitudes likely lie between the OLS and IV estimates.

It may initially seem inconsistent that the state instrument uses patent locations to proxy for the location of R&D, but R&D and not patenting drives the effect (Table 2 panel 2 column 3). However, R&D is mostly conducted in laboratories, where some R&D activity yields patents and some does not. The firms responsible for the IV result are patenting in general, but changes in their patenting do not predict employee departures to entrepreneurship. Finally, it is also worth noting that the IV effect persists using only the federal instrument.

4.3 Alternative Startup and R&D Measures

We consider alternative measures of employee entrepreneurship in Table 5. Panel 1 column 1 considers only startups founded within one year (by year $t+1$). We continue to find a positive, significant coefficient using this more immediate measure. In the next two columns, we demonstrate why our primary dependent variable (employee entrepreneurship rate_{t+3}) limits measuring entrepreneurship to three years after the employee snapshot is taken at the parent firm. In panel 1 columns 2-3, the dependent variable classifies employees as entrepreneurs if they depart to a firm that is no more than 1 years old and are among the top five earners at that new firm. The dependent variable is the fraction of an establishment's workers as of first quarter of year zero who are entrepreneurs as of the first quarter of either year two or three. The effect remains positive but becomes insignificant by year three; that is, R&D-induced departures to entrepreneurship occur in the first two years after the increase in R&D.

As a robustness check of our main result, we replicate our main dependent variable using two instead of three years. We continue to find a significant effect (panel

2 column 1). Our primary dependent variable took a snapshot of the workers in year $t+3$. We turn to a different, "flow" measure of employee entrepreneurship in panel 2 column 2. Here entrepreneurs are defined as departed employees who are among the top five earners at a one-year-old employee-founded startup in year $t+1$, at a two-year-old employee-founded startup in year $t+2$, or at a three-year-old employee-founded startup in year $t+3$. That is, we consider cumulative departures. The coefficient in this specification is also positive and significant at the .01 level.

We then examine whether the results are driven by team exits, in which multiple employees depart together to a new firm. In this case, the number of employee-founded startups should be less than the number of employees departing to start new firms. The dependent variable in panel 2 column 3 is the number of an establishment's employee-founded startups. We continue to observe a significant effect, indicating that team exits do not explain the main results.

Our results are robust to alternative measures of R&D, shown in Table 6. When the independent variable is an indicator for the parent firm having had an above median change in last year's R&D, the effect is .089, significant at the .01 level (column 1). This implies that moving from the bottom to the top half of R&D changes increases the rate of employee entrepreneurship by seven percent. We can use this figure to calculate in a back-of-the-envelope manner that above-median relative to below-median R&D changes lead to a total of 10,541 additional employee-founded startups over the sample period.[15] This is 9.7 percent of all employee-founded startups in the data (108,000).

The effect is stronger when the independent variable is an indicator for the firm being the top 10 percentiles of R&D change (columns 3 and 4). It implies that moving from the bottom 90 percentiles to the top 10 percentiles increases the employee entrepreneurship rate by 12 percent. The effect turns negative when the bottom 10 percentiles of R&D change are employed (columns 5 and 6). We also find that the effect is robust to using R&D divided by total assets (columns 7-8). This confirms that the effect is not an artifact of small changes in R&D.

[15]The calculation is as follows. As there are 329 employees in an establishment-year on average, the coefficient implies an increase of 0.29 employee-founded startups per establishment-year, which we multiply by the 36,000 establishment-years to arrive at 10,541 new firms.

4.4 Restructuring and employee turnover

R&D may lead to restructuring, in which many employees depart the firm. This could be an omitted variable causing the correlation between R&D and employee entrepreneurship. Our evidence is inconsistent with this alternative hypothesis. Appendix Table 1 column 1 shows that R&D in year t has no effect on the percent of employees who remain with the parent by year $t+3$. Similarly, columns 2, 3, and 4 show that R&D has no effect on the percent of employees who move to another incumbent firm, drop out of the LEHD sample, or move to organizations whose age is unknown.

A second possible source of endogeneity is that when a firm undertakes R&D, it may hire new research employees, who are inherently more likely to start their own ventures than the average worker. In this case, workers with relatively short tenures would drive the effect. In fact, we find that the effect of R&D on employee entrepreneurship is positive and significant among employees with above-median tenure, suggesting that workers hired specifically for the new R&D project do not drive the effect.[16]

Finally, it would be concerning if our effect were driven by employees who are unlikely to be engaged in R&D activities or who are unlikely to start their own ventures. Unfortunately, we do not observe worker occupations. However, the effect is driven by workers with above median age.[17] This is consistent with the peak age for entering any type of entrepreneurship, high-tech entrepreneurship, and VC-backed or high-growth entrepreneurship being at least 40 (Jones 2010, Ozkal 2016, Azoulay et al. 2017).

4.5 Reverse causation

If employee entrepreneurship predicts R&D, it would raise concerns about whether the effect of R&D on employee entrepreneurship is causal. To test this, we project current-year R&D (in year t) on past employee entrepreneurship in Appendix Table 2. In column 1, we include one year of employee entrepreneurship, from year $t-2$ to year

[16] Regressions are unreported due to disclosure limitations.
[17] Regressions are unreported due to disclosure limitations but are available upon request.

$t-1$. In columns 2 and 3, we include two years ($t-3$ to $t-1$) and three years ($t-4$ to $t-1$), respectively. In all cases, the coefficient is insignificant. This provides strong evidence for causality of our main effect, beyond the instrumental variables approach. In particular, it allays the primary endogeneity concern, which is that a technological opportunity jointly causes R&D and employee entrepreneurship. The very nature of a startup is to be adaptable and responsive to new opportunities. We would thus expect startup founding to respond to the new opportunity faster than corporate R&D. In contrast, we find that the employee entrepreneurship occurs after the R&D.

5 Mechanism

This section considers two ways that corporate R&D might increase employee entrepreneurship. One is intellectual capital, in which an employee takes R&D-generated new ideas or technologies to his new firm. We consider cross-sectional evidence about this channel in Section 5.1. The other channel is human capital, or entrepreneurial skills that make employees more likely to launch their own ventures. This is considered in Section 5.2. Both channels are likely at play, and we do not affirmatively identify a specific mechanism.

5.1 Intellectual capital

We expect that the intellectual capital channel will imply that R&D induced employee entrepreneurship is associated with (a) the "research" part of R&D; (b) new-to-the-world ideas; and (c) R&D generating some ideas that are too far afield for the firm to benefit from. These are examined in Sections 5.1.1, 5.1.2, and 5.1.3, respectively.

5.1.1 "Research" part of R&D

We expect that the intellectual capital channel will be more associated with the "research" part of R&D, rather than the "development" part. Indeed, we find that high-tech establishments and more firms that do more general-purpose innovation are responsible for the effect of R&D on employee-founded startups. First, Table 7 shows that high-tech establishments drive our result. We interact R&D with a parent firm-level cross-sectional variable. An establishment is "high-tech" if its four-digit SIC code corresponds to high-tech manufacturing or R&D.[18] The effect is 0.083 larger for high-tech establishments than non-high-tech establishments. The fact that the result is driven by high tech establishments implies it is coming from the R&D-intensive establishments. This helps to address the limitation that we do not have information about where firms perform R&D. The effect for non-high-tech establishments (the independent coefficient on R&D) is small and insignificant, indicating that despite having positive R&D, non-high-tech establishments do not generate R&D-induced startups. This is consistent with Franco & Filson (2006)'s prediction that more technologically advanced firms are more likely to produce employee-founded startups.

Second, the effect is driven by firms with patents that are more valuable because they are more general-purpose; used by a wider array of fields (Hall & Trajtenberg 2004). We interact R&D with an indicator for the firm having above-median patent generality, which means that future cites of its patents are from a wider array of patent classes. The effect is significantly higher for these firms (Table 7 column 4). Also, recall that firms that patent in more classes tend to have higher employee entrepreneurship rates (Table 2 panel 2 column 5). Thus, it seems that firms doing more basic and broad research have more employee-founded startups per dollar of R&D.

It is important to note that patenting does not drive our results, and there is no significant interaction between parent R&D and the number of patents or patent citations. R&D investment is an input, producing innovation in a highly uncertain, serendipitous manner. Patents represent outputs that the firm has chosen to appropriate and sufficiently values the intellectual property right conferred by patents to make the necessary disclosure worthwhile. To our knowledge, we present the first evidence that R&D inputs lead to employee entrepreneurship.

[18]We identify high-tech SIC codes as 3200-3299, 3500-3599, 3700-3899, and 8732-8734.

5.1.2 New-to-the-world ideas

The intellectual capital channel should yield employee-founded startups with new-to-the-world ideas, rather than “Main street” type businesses. If R&D stimulates restaurants or plumbing companies, it seems unlikely that ideas and inventions created by the R&D investment are the mechanism. We find that within the population of employee-founded startups, more parent R&D is associated with high-tech and venture capital-backed employee-founded startups. We examine in Table 8 whether parent R&D is associated with certain startup characteristics.

Venture capital-backed startups are widely thought to be strongly associated with new-to-the-world ideas. Gornall & Strebulaev (2015) show that among U.S. public companies, those with venture capital are responsible for 44 percent of research and development expenditure, and Kaplan & Lerner (2010) show that over 60 percent of IPO issuers have venture capital backing. The dependent variable in Table 8 panel 1 column 1 is one if the employee-founded startup receives venture capital, which is the case for two percent of employee-founded startups (recall from Section 2.5 that this is high relative to the rate in the overall population of new firms). The coefficient on R&D is 0.007, significant at the .01 level. This implies that a 100 percent increase in R&D leads to a 35 percent increase in the chances that an employee-founded startup is venture capital-backed. Among parent firm observables, R&D is by far the strongest predictor of venture capital-backed employee-founded startups; while some other variables have predictive power, it is much weaker.

In Table 8 panel 2 column 3, the dependent variable is one if the employee-founded startup is in a high-tech industry, and zero if it is not. Parent R&D is strongly associated with employee-founded startups being high-tech. In column 4, we show that R&D induces employee-founded startups with higher wages than the average employee-founded startup. In unreported analysis, we do not find that R&D induces employee-founded startups with more initial employees. Thus R&D seems to induce employee-founded startups with high-skill labor, but that do not start at a larger than average size. The last outcome is the rate of exit, which we view as a proxy for risk. We assume the vast majority of exits are firm failures, but a small minority may be acquisitions, which could be a very successful exit. In column 5, the dependent variable is one if the startup exits within five years (starting from year $t+3$, where t is the year in which we measure R&D). We find a positive, significant effect of R&D. In sum, relative to the average employee-founded startup, those induced by R&D are more likely to be high-tech, high-impact, and high-risk.

5.1.3 R&D generates some ideas that are too far afield for the firm to benefit from

In the intellectual capital channel, the employee appropriates a new idea generated by his employer's R&D investment. If R&D-induced startups are simply replicating the parents' business models, then they likely do not have a new idea. Instead, we find that employee-founded startups tend to be in different industries from parents, and more parent R&D makes it less likely that the employee-founded startup is in same industry as the parent. This suggests that the startup has a new idea and is not replicating the parent's business.

In column 1 of Table 8 panel 2, the dependent variable is one if the employee-founded startup is in the same 2-digit SIC classification as its parent (examples of 2-digit industries are "Business Services" and "Coal Mining"). The coefficient is negative and significant; more parent R&D reduces the chances that the startup is in the same industry as its parent. Only 16.8 percent of employee-founded startups are in their parent's 2-digit industry. We find a similar result when we use NAICS codes rather than SIC codes.

It may initially seem counter-intuitive that R&D leads employees to found firms in different industries, particularly since many employee-founded startups more broadly are spin-offs, imitating the parent's business. However, consider three examples of employee-founded startups. First, in 1894, Henry Ford left Thomas Edison's Illuminating Company to launch his own venture. Two years later, he produced the first Ford Quadricycle with the help of a local angel investor (Glaeser 2011). Edison would be SIC 49 (Electric, Gas and Sanitary Services), while Ford is in SIC 37 (Transportation Equipment). Yet Ford relied on mechanical and electrical engineering advances made at Edison. Second, in the 1990s, Michael Rosenfelt worked for the computer memory company Micron Electronics (now Micron

Technology), where he helped to revitalize its PC business. He left in 1999 to found Powered Inc., an online education company.[19] Micron Technology is in SIC 36 (Electronic and other Electrical Equipment), while Powered, Inc, the online education company, would likely be in either SIC code 73 (Business Services, the location of most Internet companies), or SIC code 82 (Educational Services). Powered was built on marketing innovations at Micron. Finally, David Friedberg and Siraj Khaliq left Google in 2006 to start WeatherBill (later The Climate Corporation), an agricultural insurance startup ultimately acquired by Monsanto.[20] Google's parent company Alphabet is in SIC 73 (Business Services), while WeatherBill, the insurance company, would be in SIC code 63 (Insurance Carriers). WeatherBill employed artificial intelligence insights from Google to better price insurance. In all three examples, an R&D-intensive parent spawned a new firm in a different 2-digit SIC code sector, but where the underlying intellectual capital bears some relation to the parent's. These examples highlight how SIC assignments reflect the firm's market more than its technology. It seems likely that R&D-induced startups often employ technology related to the parent's, but apply it to a different market.

Thus, our effect is driven by high-tech parents and high-tech employee-founded startups, while the R&D-induced startups tend to be in different industries from their parents. The intellectual capital channel can reconcile these facts. The parent firm R&D creates growth options far from its core focus, which the employee can deploy in a new firm. The intellectual capital mechanism also fits well with our interpretation of the IV results. The ideas generated by R&D that wind up in departing employees' startups are much more likely to come from the last dollar of R&D than the first. In this light, the IV strategy yields an effect that isolates the driving mechanism: marginal R&D generates ideas, some of which spill over into startups founded by employees.

[19]Powered, Inc raised $8.5 million in VC and served clients such as Bloomberg.com Inc. It was acquired by Sprinklr, and internet company, and continues to exist as a standalone subsidiary. See here, here, and here.

[20]See here.

5.2 Human capital

In the human capital channel, R&D induces employee learning, which makes the employee more productive as an entrepreneur. In this channel, R&D leads to employee entrepreneurship because it generates new skills, not new ideas. This channel likely plays a role. However, three pieces of cross sectional evidence suggest that it may not be the primary driver.

First, in a human capital channel, we expect R&D-induced startups to come from small parents. This is because small firm employees tend to have a broader scope of work (Stuart & Ding 2006, Sørensen 2007). Instead, large firms – defined as having above-median total assets within a given year – drive the effect (Table 7 column 2). Also note that the independent indicator for being a large firm has a negative coefficient that is slightly more than twice the positive coefficient on the interaction with R&D. While R&D-induced employee entrepreneurship is driven by large firms, on average small firms tend to have more employee-founded startups. This is consistent with Elfenbein et al. (2010), who find using survey data on scientists that entrepreneurs are more likely to emerge from small firms. Second, we might also expect that there is more opportunity for entrepreneurial learning at young firms. However, when we interact R&D with an indicator for the firm being old (above median age), we find no effect, shown in Table 7 column 4.

Third, we would expect that capital expenditure would have a similar effect on employee entrepreneurship if the channel were skills, because new capital investment seems likely to create similar project management skills as R&D projects. Instead, Table 2 panel 1 shows that there is no effect of total investment or PPE investment on employee entrepreneurship. In sum, while it is most likely that both human and intellectual capital explain why R&D leads employees to start their own firms, the data are most consistent with the intellectual capital channel being dominant.

5.3 Costs to the Parent Firm

The intellectual capital channel suggests that the effect of R&D on employee entrepreneurship may be a new avenue for R&D spillovers. Note that these spillovers are both social and private in nature; the private value is to the entrepreneur and other equity holders, and the social value comes from new jobs created or unpriced benefits from commercializing a new idea. These spillover benefits likely coexist with costs to the parent of losing the employee and the idea. Without observing why employees leave, we cannot calculate the magnitude of these costs. For example, one reason an employee might leave is that there is a hold-up problem; leaving is the best way for the innovator to limit the risk of a hold up by the former employer. With perfect information, the parent firm could patent all the good ideas that emerge from R&D, sell those it does not wish to develop in-house, and contract with the employee ex-ante so that he will not depart to start his own firm. However, in practice information frictions make it difficult to contract on innovation (Holmström 1999, Aghion & Tirole 1994).

We consider several indicators for high costs to the parent. First, if the R&D effect on employee entrepreneurship is very costly to the parent, it should be attenuated in states that enforce non-compete covenants. Non-competes restrict employees from working for a competing firm within the state for a specified period of time. It has been found that non-compete enforcement reduces local R&D spillovers (Belenzon & Schankerman 2013, Matray 2015), and reduce within-state inventor mobility (Marx et al. 2015). The main result persists in states that enforce non-competes, and there is no significant effect on an interaction between R&D and an indicator for being in a weak enforcement state.

Second, if the effect is very costly to the parent, it should be attenuated when intellectual property is easier to protect, which likely makes it easier to contract on innovation effort. We do not find that the effect varies with a measure of industry patentability. Third, costly employee-founded startups may compete in product markets with the parent. Instead, we found that employee-founded startups tend to be in different industries from parents. This suggests that many of the R&D-generated ideas that employees take to new firms are not highly valued by the parent firm; the ideas are too far afield for the parent to desire to keep them in-house.

Finally, we make a revealed preference argument. By virtue of observing the persistent phenomenon of R&D-induced employee entrepreneurship, the parent either chose not to develop the idea in house or chose not to take ex-ante steps to prevent the employee-founded startup. These steps could include increasing the employee's compensation to retain him, or even not conducting R&D at all. It is possible that the parent does not possess the option to prevent the employee-founded startup. For example, the employee may fear expropriation and not disclose ideas, or he may be able to steal an idea that the firm deems valuable.[21] In the case of such contracting frictions, the parent firm should predict the loss of some innovative employees. It might price this cost into their compensation ex-ante. Regardless of these considerations, by virtue of observing employee entrepreneurship, any costs of preventing it must exceed the benefits.

5.4 Parent Firm Benefits

At the other extreme, the extent of spillover could be mitigated if the parent firm internalizes the employee-founded startup's benefits. If R&D-induced startups are spinoffs of the parent firm that the parent firm either partially owns or subsequently acquires, then the parent internalizes, or captures, some of the employee-founded startup's private benefits. Full internalization would imply no ex-ante underinvestment relative to the social optimum. Note, however, that the ex-post split between the parent and the employee-founded startup of the surplus from the new idea is not relevant from a social welfare perspective.

Two pieces of cross-sectional evidence make internalization unlikely. First, we expect parent-supported spinoffs to start at a larger scale than a typical bootstrapped startup. We find no relation between initial employee-founded startup size and parent R&D. That is, in specifications similar to those in Table 8, we find no effect of parent R&D on initial employee-founded startup employment. Second, spinoffs or parent

[21]The low costs of information and resource sharing (including teamwork) are reasons the firm exists in the first place. Giving employees the right incentives to innovate – that is, high-powered incentives – would make it impossible to manage the larger R&D process. For example, the firm will find it difficult to identify ex-post exactly who is responsible for the innovation, and individuals will have incentives to hoard information. Note that the contracting challenges arise in large part from the inalienability of human labor (no slavery). This relates to the property rights literature associated with Grossman & Hart (1986).

reorganization would be expected to at least in some cases maintain the same establishment. Startups are defined in our data as firms with no prior activity at any of their establishments.

To provide more concrete evidence, we directly assess the possibility that parents internalize employee-founded startups' benefits using an out-of-sample test based on the underlying data in Gompers et al. (2005). They connected all venture capital-backed startup executives in the VentureOne database between 1986 and 1999 to their prior employers.[22] We hypothesize that this data should provide an upper bound on possible internalized employee-founded startups; since these startups by definition received external investment, they are more likely than the average employee-founded startup to have received investment from their former employer. We begin with 13,612 entrepreneur-parent pairs. The entrepreneurs are founders of 6,499 unique employee-founded startups. There are 9,152 unique parents. In most cases employee-founded startups have multiple parents (that is, there are multiple executives with prior jobs). We linked all of the employee-founded startup parents to VentureXpert acquisition and investment data. We successfully matched 4,786 unique employee-founded startups to at least one investor or acquirer, a match rate of 74 percent. There are 20,478 unique startup-investor pairs.[23]

A merge of these investors and acquirers to the parents yields 266 unique startups where the parent matches an investor or acquirer, out of 4,786 startups that we matched to VentureXpert, or 5.6 percent.[24] Of these, 192 are investment deals, and 74 are acquisitions. There are 208 unique parents that are matched to investors/acquirers. Note that some parents have multiple employee-founded startups, such as IBM and

[22]This time period overlaps with our primary Census data (1990 to 2005).

[23]Note that the underlying dataset, from Dow Jones Venture Source, is of venture capital-backed startups. In theory, if we used VentureSource, we should match 100 percent to initial investors. However, as Kaplan & Lerner (2016) and Maats et al. (2011) explain, VentureXpert's coverage is much better than Venture Source (more than 40 percent more investments). VentureXpert also has superior acquisition data, and Venture Source's data quality has declined over time. We are most interested in whether parents ultimately invested in (and especially acquired) employee-founded startups, so VentureXpert seems like the optimal data set to use. If there is any bias, it should be the case that the employee-founded startups that do not match have lower rates of subsequent investment and acquisition, since the commercial databases often backfill based on exit events.

[24]We matched on the company's first word, which yielded 275 matches. This enables successful matches such as "Xerox Venture Capital" to "Xerox." We then manually removed obviously wrong matches, erring on the side of leaving the match to be conservative in ambiguous cases.

Highland Capital Partners, so the parent and startup numbers do not match. Some parents that invested in or acquired their employee-founded startups are corporates, including Seagate, Xerox, Monsanto, Johnson & Johnson, and Microsoft. Others are asset managers, including Accel Partners, Softbank, and Equus Capital. Still others are non-corporates, including Boston University. We identified 41 parent firms that are clearly venture funds or other asset managers. This leaves 167 parents that are plausibly corporates, though this is generous as we retained financial services companies such as Goldman Sachs.

To interpret this exercise, we return to the total parent population. Of the 9,152 unique parents in the original Gompers et al. (2005) data, just 2.3 percent (208) invest in or acquire their employee-founded startups. This small percentage is evidence that parents do not usually internalize employee-founded startups by investing in or acquiring them. One concern may be that perhaps many corporate parents are not covered as investors or acquirers in VentureXpert. We can match 2,617 of the parents to investors or acquirers in VentureXpert. The most conservative framing of our results, then, restricts the parent population to firms that ever invested in or acquired a startup in VentureXpert. In this case, 7.9 percent of parents (208 out of 2,617) invest in or acquire their employee-founded startups. This extreme upper bound is still small and confirms that it is unlikely that parents generally internalize the benefits of their employee-founded startups.

The parent could also appropriate the employee-founded startup's benefits through technology licensing deals. We cannot assess this possibility with our data, but we think it unlikely that the parent can fully internalize the employee-founded startup's social benefits through such arms-length contracts.

Consistent with the out-of-sample test, within our data we find no effect on employee entrepreneurship of the interaction between R&D and the parent having a corporate venture capital program. These results are consistent with Ma (2016), who finds that public firms launch corporate venture capital programs when internal innovation is poor, invest in startups in their own industries, and invest in geographically distant startups. That is, corporate venture capital is a way to outsource innovation. This is the opposite of the corporate environment that yields

R&D-induced employee entrepreneurship. Instead, when corporate R&D increases at innovative firms, it seems to serendipitously produce "extra" growth options, and employee entrepreneurship is an unintended consequence.

6 Conclusion

This paper shows that some growth options generated by a firm's R&D process are reallocated from large incumbents to startups. The importance of innovation to growth is well-known, but it is difficult to observe in practice (Jones & Williams 1998). Further, much of the literature on innovation has focused on innovation outputs, namely patents, and on the effect of demand-side policies on targeted firms (e.g. Howell 2017). This paper takes a more supply side approach by focusing on an important and likely unintended consequence of R&D inputs. Specifically, we show that corporate R&D investment leads to employee entrepreneurship, in which employees depart to launch their own firms. We do this both in tightly controlled fixed effects regressions and in an instrumental variables approach, where we instrument for R&D using federal and state R&D tax credits. For the parent firm, R&D-induced startups yield no obvious contractual benefits, nor is the phenomenon observably costly. Our evidence is consistent with corporate R&D being a new channel for R&D spillovers, as well as a new source of high-tech startups.

Our results have two policy implications. First, the employee entrepreneurship effect of R&D implies greater corporate underinvestment in R&D relative to the social optimum than previously thought. Second, the presence of R&D spillovers are one motivation for offering firms tax credits that lower their cost of R&D investment. The employee entrepreneurship effect of R&D is much larger in the instrumental variables model than in the fixed effects regression. This suggests, albeit in a partial equilibrium sense, that R&D tax credits are effective in that they lead to greater R&D-induced employee entrepreneurship.

The literature on R&D spillovers has focused on their ability to explain industrial agglomeration, or the spatial concentration of firms (Glaeser & Kerr 2009, Greenstone, Hornbeck & Moretti 2010). A final contribution of our paper is to offer another channel for the link between industrial agglomeration and R&D spillovers, which is often attributed in part to the importance of tacit information (Audretsch & Feldman 1996, Glaeser 1999, Duranton & Puga 2001). R&D spillovers have been found to be quite local and to decline with distance (e.g. Jaffe et al. 1993, Belenzon & Schankerman 2013, Kantor & Whalley 2014). A remarkable 88 percent of employee-founded startups in our data are located in the same state as the parent. Since employee-founded startups tend not to be in the same industry as their parents, our data suggest another reason for the connection between spillovers and agglomeration, more along the lines found in Ellison, Glaeser & Kerr (2010): moving may be privately costly to the nascent entrepreneur, or he may have relevant networks in the location of his former firm.

References

Abowd, J. M., Stephens, B. E., Vilhuber, L., Andersson, F., McKinney, K. L., Roemer, M. & Woodcock, S. (2009), The lehd infrastructure files and the creation of the quarterly workforce indicators, *in* 'Producer dynamics: New evidence from micro data', University of Chicago Press, pp. 149–230.

Acemoglu, D., Akcigit, U., Bloom, N. & Kerr, W. (2013), 'Innovation, reallocation and growth', *NBER Working Paper No. 18993* .

Adelino, M., Ma, S. & Robinson, D. (2017), 'Firm age, investment opportunities, and job creation', *The Journal of Finance* .

Aghion, P. & Jaravel, X. (2015), 'Knowledge spillovers, innovation and growth', *The Economic Journal* **125**(583), 533–573.

Aghion, P. & Tirole, J. (1994), 'The management of innovation', *The Quarterly Journal of Economics* **109**(4), 1185–1209.

Agrawal, A., Cockburn, I., Galasso, A. & Oettl, A. (2014), 'Why are some regions more innovative than others? the role of small firms in the presence of large labs', *Journal of Urban Economics* **81**, 149–165.

Almeida, P. & Kogut, B. (1999), 'Localization of knowledge and the mobility of engineers in regional networks', *Management science* **45**(7), 905–917.

Angrist, J. D. & Imbens, G. W. (1995), 'Two-stage least squares estimation of average causal effects in models with variable treatment intensity', *Journal of the American statistical Association* **90**(430), 431–442.

Audretsch, D. B. & Feldman, M. P. (1996), 'R&d spillovers and the geography of innovation and production', *The American economic review* **86**(3), 630–640.

Azoulay, P., Jones, B., Kim, J. D. & Miranda, J. (2017), 'Age and high-growth entrepreneurship'. Working Paper.

Azoulay, P., Zivin, J. G. & Wang, J. (2010), 'Superstar extinction', *Quarterly Journal of Economics* **25**.

Babina, T. (2017), 'Destructive creation at work: How financial distress spurs entrepreneurship'. Working Paper.

Babina, T., Ouimet, P. P. & Zarutskie, R. (2018), 'Going entrepreneurial? ipos and new firm creation'. Working Paper.

Bankman, J. & Gilson, R. J. (1999), 'Why start-ups?', *Stanford Law Review* **51**, 289, 293–95.

Belenzon, S. & Schankerman, M. (2013), 'Spreading the word: Geography, policy, and knowledge spillovers', *Review of Economics and Statistics* **95**(3), 884–903.

Bhide, A. V. (2000), *The Origin and Evolution of New Businesses.*, Oxford University Press.

Billings, A., Glazunov, S. & Houston, M. (2001), 'The role of taxes in corporate research and development spending', *R&D Management* **31**(4), 465–477.

Bloom, N., Griffith, R. & Van Reenen, J. (2002), 'Do r&d tax credits work? evidence from a panel of countries 1979–1997', *Journal of Public Economics* **85**(1), 1–31.

Bloom, N., Schankerman, M. & Van Reenen, J. (2013), 'Identifying technology spillovers and product market rivalry', *Econometrica* **81**(4), 1347–1393.

BLS (1997), *BLS Handbook of Methods*, Bureau of Labor Statistics, Washington DC. U.S. Bureau of Labor Statistics, Division of Information Services, Washington D.C.

Brown, J. R. & Petersen, B. C. (2011), 'Cash holdings and r&d smoothing', *Journal of Corporate Finance* **17**(3), 694–709.

Campa, J. M. & Kedia, S. (2002), 'Explaining the diversification discount', *The journal of finance* **57**(4), 1731–1762.

Campbell, B. A., Ganco, M., Franco, A. M. & Agarwal, R. (2012), 'Who leaves, where to, and why worry? employee mobility, entrepreneurship and effects on source firm performance', *Strategic Management Journal* **33**(1), 65–87.

Chatterji, A. K. (2009), 'Spawned with a silver spoon? entrepreneurial performance and innovation in the medical device industry', *Strategic Management Journal* **30**(2), 185–206.

Chirinko, R. S. & Wilson, D. J. (2008), 'State investment tax incentives: A zero-sum game?', *Journal of Public Economics* **92**(12), 2362–2384.

Chirinko, R. S. & Wilson, D. J. (2011), 'Tax competition among us states: racing to the bottom or riding on a seesaw?', *Working Paper* .

Clausen, T. H. (2009), 'Do subsidies have positive impacts on r&d and innovation activities at the firm level?', *Structural Change and Economic Dynamics* **20**(4), 239–253.

Cohen, W. M. & Levinthal, D. A. (1989), 'Innovation and learning: the two faces of r & d', *The economic journal* **99**(397), 569–596.

Curtis, E. M. & Decker, R. (2018), Entrepreneurship and state taxation, Technical report, Board of Governors of the Federal Reserve System (US).

Decker, R., Haltiwanger, J., Jarmin, R. & Miranda, J. (2014), 'The role of entrepreneurship in us job creation and economic dynamism', *The Journal of Economic Perspectives* **28**(3), 3–24.

Duranton, G. & Puga, D. (2001), 'Nursery cities: Urban diversity, process innovation, and the life cycle of products', *American Economic Review* pp. 1454–1477.

Elfenbein, D. W., Hamilton, B. H. & Zenger, T. R. (2010), 'The small firm effect and the entrepreneurial spawning of scientists and engineers', *Management Science* **56**(4), 659–681.

Ellison, G., Glaeser, E. L. & Kerr, W. R. (2010), 'What causes industry agglomeration? evidence from coagglomeration patterns', *The American Economic Review* **100**(3), 1195–1213.

Feldman, M. P. & Kelley, M. R. (2006), 'The ex ante assessment of knowledge spillovers: Government r&d policy, economic incentives and private firm behavior', *Research Policy* **35**(10), 1509–1521.

Foster, L., Haltiwanger, J. & Syverson, C. (2008), 'Reallocation, firm turnover, and efficiency: Selection on productivity or profitability?', *American Economic Review* **98**(1), 394–425.

Franco, A. M. & Filson, D. (2006), 'Spin-outs: knowledge diffusion through employee mobility', *The RAND Journal of Economics* **37**(4), 841–860.

Frésard, L., Hoberg, G. & Phillips, G. (2017), 'Vertical acquisitions, integration and the boundaries of the firm'.

Gertner, R. H., Scharfstein, D. S. & Stein, J. C. (1994), 'Internal versus external capital markets', *Quarterly Journal of Economics* **109**.

Gilson, R. J. (1999), 'The legal infrastructure of high technology industrial districts: Silicon valley, route 128, and covenants not to compete', *NYUl Rev.* **74**, 575.

Glaeser, E. (2011), *Triumph of the city: How urban spaces make us human*, Penguin Press. p. 47.

Glaeser, E. L. (1999), 'Learning in cities', *Journal of urban Economics* **46**(2), 254–277.

Glaeser, E. L., Kerr, S. P. & Kerr, W. R. (2015), 'Entrepreneurship and urban growth: An empirical assessment with historical mines', *Review of Economics and Statistics* **97**(2), 498–520.

Glaeser, E. L. & Kerr, W. R. (2009), 'Local industrial conditions and entrepreneurship: how much of the spatial distribution can we explain?', *Journal of Economics & Management Strategy* **18**(3), 623–663.

Goldin, C., Kerr, S. P., Olivetti, C., Barth, E. et al. (2017), 'The expanding gender earnings gap: evidence from the lehd-2000 census', *American Economic Review* **107**(5), 110–14.

Gompers, P., Lerner, J. & Scharfstein, D. (2005), 'Entrepreneurial spawning: Public corporations and the genesis of new ventures, 1986 to 1999', *The Journal of Finance* **60**(2), 577–614.

Gornall, W. & Strebulaev, I. A. (2015), 'The economic impact of venture capital: Evidence from public companies', *Working Paper* .

Graham, J. R., Lemmon, M. L. & Wolf, J. G. (2002), 'Does corporate diversification destroy value?', *The Journal of Finance* **57**(2), 695–720.

Greenstone, M., Hornbeck, R. & Moretti, E. (2010), 'Identifying agglomeration spillovers: Evidence from winners and losers of large plant openings', *Journal of Political Economy* **118**(3), 536–598.

Griffith, R., Harrison, R. & Van Reenen, J. (2006), 'How special is the special relationship? using the impact of us r&d spillovers on uk firms as a test of technology sourcing', *American Economic Review* **96**(5), 1859–1875.

Griliches, Z. (1992), 'The search for r&d spillovers', *The Scandinavian Journal of Economics* pp. S29–S47.

Gromb, D. & Scharfstein, D. (2002), Entrepreneurship in equilibrium, Technical report, National bureau of economic research.

Grossman, S. J. & Hart, O. D. (1986), 'The costs and benefits of ownership: A theory of vertical and lateral integration', *Journal of Political Economy* **94**(691), 693–95.

Guzman, J. & Stern, S. (2017), *Nowcasting and Placecasting Entrepreneurial Quality and Performance*, Vol. 75, University of Chicago Press.

Habib, M. A., Hege, U. & Mella-Barral, P. (2013), 'Entrepreneurial spawning and firm characteristics', *Management Science* **59**(12), 2790–2804.

Hall, B. H. (1993), 'R&d tax policy during the 1980s: success or failure?', *Tax policy and the economy* **7**, 1–35.

Hall, B. H. & Trajtenberg, M. (2004), Uncovering gpts with patent data, Technical report, National Bureau of Economic Research.

Hall, B. & Van Reenen, J. (2000), 'How effective are fiscal incentives for r&d? a review of the evidence', *Research Policy* **29**(4), 449–469.

Haltiwanger, J., Jarmin, R. S. & Miranda, J. (2013), 'Who creates jobs? small versus large versus young', *Review of Economics and Statistics* **95**(2), 347–361.

Hellmann, T. (2007), 'When do employees become entrepreneurs?', *Management science* **53**(6), 919–933.

Herkenhoff, K., Lise, J., Menzio, G. & Phillips, G. (2018), 'Worker mobility and the diffusion of knowledge'.

Holmström, B. (1999), 'Managerial incentive problems: A dynamic perspective', *The review of Economic studies* **66**(1), 169–182.

Howell, S. T. (2017), 'Financing innovation: evidence from r&d grants', *American Economic Review* **107**(4), 1136–64.

Jaffe, A. B., Trajtenberg, M. & Henderson, R. (1993), 'Geographic localization of knowledge spillovers as evidenced by patent citations', *the Quarterly journal of Economics* pp. 577–598.

Jarmin, R. S. & Miranda, J. (2002), 'The longitudinal business database'.

Jiang, W. (2015), 'Have instrumental variables brought us closer to the truth', *The Review of Corporate Finance Studies* .

Jones, B. F. (2010), 'Age and great invention', *The Review of Economics and Statistics* **92**(1), 1–14.

Jones, C. I. & Williams, J. C. (1998), 'Measuring the social return to r&d', *Quarterly Journal of Economics* pp. 1119–1135.

Kantor, S. & Whalley, A. (2014), 'Knowledge spillovers from research universities: evidence from endowment value shocks', *Review of Economics and Statistics* **96**(1), 171–188.

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

Kaplan, S. N. & Lerner, J. (2016), Venture capital data: Opportunities and challenges, *in* 'Measuring Entrepreneurial Businesses: Current Knowledge and Challenges', University of Chicago Press.

Kerr, S., Kerr, W. R. & Nanda, R. (2015), House money and entrepreneurship, Technical report, National Bureau of Economic Research.

Kerr, S. P. & Kerr, W. R. (2017), 'Immigrant entrepreneurship', *Measuring Entrepreneurial Businesses: Current Knowledge and Challenges* p. 187.

Kerr, W. R. & Kominers, S. D. (2015), 'Agglomerative forces and cluster shapes', *Review of Economics and Statistics* **97**(4), 877–899.

Kim, J. & Marschke, G. (2005), 'Labor mobility of scientists, technological diffusion, and the firm's patenting decision', *RAND Journal of Economics* pp. 298–317.

Klassen, K. J., Pittman, J. A., Reed, M. P. & Fortin, S. (2004), 'A cross-national comparison of r&d expenditure decisions: Tax incentives and financial constraints', *Contemporary Accounting Research* **21**(3), 639–680.

Klenow, P. J. & Rodriguez-Clare, A. (2005), 'Externalities and growth', *Handbook of economic growth* **1**, 817–861.

Klepper, S. (2001), 'Employee startups in high-tech industries', *Industrial and corporate change* **10**(3), 639–674.

Klepper, S. (2007), ‘Disagreements, spinoffs, and the evolution of detroit as the capital of the us automobile industry’, *Management Science* **53**(4), 616–631.

Klepper, S. & Sleeper, S. (2005), ‘Entry by spinoffs’, *Management science* **51**(8), 1291–1306.

Klepper, S. & Thompson, P. (2010), ‘Disagreements and intra-industry spinoffs’, *International Journal of Industrial Organization* **28**(5), 526–538.

Kortum, S. & Lerner, J. (2000), ‘Assessing the contribution of venture capital to innovation’, *The Rand Journal of Economics* **31**(4), 674.

Krugman, P. R. (1991), ‘Geography and trade’.

Lang, L. H. & Stulz, R. M. (1994), ‘Tobin’s q, corporate diversification, and firm performance’, *Journal of political economy* **102**(6), 1248–1280.

Lerner, J. & Seru, A. (2017), The use and misuse of patent data: Issues for corporate finance and beyond, Technical report, National Bureau of Economic Research.

Ma, S. (2016), ‘The life cycle of corporate venture capital’. Working Paper.

Maats, F., Metrick, A., Yasuda, A., Hinkes, B. & Vershovski, S. (2011), ‘On the consistency and reliability of venture capital databases’, *Unpublished working paper* .

Maksimovic, V. & Phillips, G. (2002), ‘Do conglomerate firms allocate resources inefficiently across industries? theory and evidence’, *The Journal of Finance* **57**(2), 721–767.

Mamuneas, T. P. & Nadiri, M. I. (1996), ‘Public r&d policies and cost behavior of the us manufacturing industries’, *Journal of Public Economics* **63**(1), 57–81.

Manski, C. F. (1993), ‘Identification of endogenous social effects: The reflection problem’, *The Review of Economic Studies* **60**(3), 531–542.

Marshall, A. (1920), *Principles of economics*, MacMillan, London.

Marx, M., Singh, J. & Fleming, L. (2015), ‘Regional disadvantage? employee non-compete agreements and brain drain’, *Research Policy* **44**(2), 394–404.

Matray, A. (2015), ‘The local innovation spillovers of listed firms’, *Working Paper* .

McCutchen, W. W. (1993), ‘Estimating the impact of the r&d tax credit on strategic groups in the pharmaceutical industry’, *Research Policy* **22**(4), 337–351.

Miller, C. R. & Richard, B. (2010), ‘The policy diffusion of the state r&d investment tax credit’, *State and Local Government Review* **42**(1), 22–35.

Nanda, R. & Sørensen, J. B. (2010), ‘Workplace peers and entrepreneurship’, *Management Science* **56**(7), 1116–1126.

Ozkal, D. (2016), ‘Millennials can’t keep up with boomer entrepreneurs’. Kauffman Foundation.

Paff, L. A. (2005), ‘State-level r&d tax credits: A firm-level analysis’, *The BE Journal of Economic Analysis & Policy* **5**(1).

Phillips, G. M. & Zhdanov, A. (2012), ‘R&d and the incentives from merger and acquisition activity’, *The Review of Financial Studies* **26**(1), 34–78.

Puri, M. & Zarutskie, R. (2012), ‘On the life cycle dynamics of venture-capital-and non-venture-capital-financed firms’, *The Journal of Finance* **67**(6), 2247–2293.

Rajan, R., Servaes, H. & Zingales, L. (2000), ‘The cost of diversity: The diversification discount and inefficient investment’, *The journal of Finance* **55**(1), 35–80.

Robinson, D. T. (2008), ‘Strategic alliances and the boundaries of the firm’, *Review of Financial Studies* **21**(2), 649–681.

Saxenian, A. (1990), ‘Regional networks and the resurgence of silicon valley’, *California management review* **33**(1), 89–112.

Scharfstein, D. S. & Stein, J. C. (2000), ‘The dark side of internal capital markets: Divisional rent-seeking and inefficient investment’, *The Journal of Finance* **55**(6), 2537–2564.

Schoar, A. (2002), ‘Effects of corporate diversification on productivity’, *The Journal of Finance* **57**(6), 2379–2403.

Seru, A. (2014), ‘Firm boundaries matter: Evidence from conglomerates and r&d activity’, *Journal of Financial Economics* **111**(2), 381–405.

Sørensen, J. B. (2007), ‘Bureaucracy and entrepreneurship: Workplace effects on entrepreneurial entry’, *Administrative Science Quarterly* **52**(3), 387–412.

Stuart, T. E. & Ding, W. W. (2006), ‘When do scientists become entrepreneurs? the social structural antecedents of commercial activity in the academic life sciences’, *American Journal of Sociology* **112**(1), 97–144.

Tate, G. & Yang, L. (2015), ‘The bright side of corporate diversification: Evidence from internal labor markets’, *The review of financial studies* **28**(8), 2203–2249.

Villalonga, B. (2004), ‘Diversification discount or premium? new evidence from the business information tracking series’, *The Journal of Finance* **59**(2), 479–506.

Waldinger, F. (2012), ‘Peer effects in science: Evidence from the dismissal of scientists in nazi germany’, *The Review of Economic Studies* **79**(2), 838–861.

Whited, T. M. (2001), ‘Is it inefficient investment that causes the diversification discount?’, *The Journal of Finance* **56**(5), 1667–1691.

Wilson, D. J. (2009), ‘Beggar thy neighbor? the in-state, out-of-state, and aggregate effects of r&d tax credits’, *The Review of Economics and Statistic* .

Wilson, D. J. et al. (2005), 'The rise and spread of state r&d tax credits', *FRBSF Economic Letter* .

Wu, Y. (2008), 'State r&d tax credits and high-technology establishments', *Economic Development Quarterly* **22**(2), 136–148.

Zingales, L. (2000), 'In search of new foundations', *The journal of Finance* **55**(4), 1623–1653.

Table 1: Summary Statistics

Panel 1: Firm-year level variables

	Mean	Quasi-median	Standard deviation
Made corporate VC investments$_t$	0.038		
Had $\geq$ 1 patent$_{t-10,t}$	0.601		
Diversified$_t$	0.789		
R&D/Total Assets$_{t-1}$	0.085	0.052	0.102
Log R&D$_{t-1}$	2.53	2.45	2.25
Tobin's Q$_{t-1}$	2.12	1.65	1.59
Age$_t$	20.03	21.03	6.18
Total Assets$_{t-1}$ ('000s)	3,483	529	12,630
Employment$_{t-1}$	6,107	1,987	12,690

Panel 2: Establishment-year level variables

	Mean	Quasi-median	Standard deviation
Weak non-compete enforcement (state)	0.613		
In high-tech industry	0.641		
Employee Entrepreneurship$_{t+3}$	1.31	0.82	2.43
# employee-founded startups$_{t+3}$	1.15	0.78	1.91
Stayers$_{t+3}$	47.77	52.30	25.98
Movers to old firms$_{t+3}$	26.29	22.51	18.10
Depart LEHD coverage$_{t+3}$	12.39	11.11	7.78
Movers to firms of unknown age$_{t+3}$	9.73	6.65	12.28
Average worker quarterly wage$_t$	17.53	15.50	10.56
Average worker age$_t$(years)	40.08	40.27	4.76
Average employee tenure$_t$(years)	2.69	2.40	1.88
Share employees female$_t$	0.333	0.313	0.192
Share employees white$_t$	0.795	0.835	0.171
Share employees foreign$_t$	0.062	0.031	0.098
Number employees$_t$	329	122	1,698

Note: Panel 1 shows summary statistics at the firm-year level (10,500 observations), and Panel 2 at the establishment-year level (36,000 observations). We do not show the median or standard deviation for indicators. Since Census disclosure procedures prohibit disclosure of percentile value, we approximate median with a quasi-median, which is estimated using a 99% weight on observations within the interquartile range and a 1% weight on the remaining observations. R&D, assets, and wages are in real 2014 dollars.

Panel 3: Employee-founded startup level variables

	Mean	Quasi-median	Standard deviation
	(1)	(2)	(3)
Same industry (SIC2) as parent	0.168		
Same state as parent	0.876		
High-tech industry	0.494		
Ever received VC	0.020		
Employee female	0.331		
Employee white	0.799		
Employee foreign	0.077		
Employee born in state	0.475		
Employee-founded startup employment$_{t+3}$	14.59	5.92	39.48
Employee-founded startup age$_{t+3}$	1.59	1.99	1.01
Employee-founded startup payroll$_{t+3}$ ('000s)	511	126	1,603
Employee age$_t$	35.16	34.64	10.94
Employee education	13.66	14.36	2.49
Employee tenure (years)$_t$	2.07	1.58	2.25
Employee wages (at parent firm)$_t$	57.80	39.12	71.70
Employee wages (at employee-founded startup)$_{t+3}$	51.84	33.60	60.99

Note: Panel 3 shows summary statistics at the Employee-founded startup level. All variables are indicators and have 108,000 observations. Variables through "Employee born in state" are indicators, and the rest are continuous. "Employee" refers to individuals who left the parent firm to join the startup's founding team. Payroll and wages are in thousands of real 2014 dollars.

Table 2: Effect of R&D on Employee Entrepreneurship

Panel 1

Dependent variable: Employee Entrepreneurship$_{t+3}$

	(1)	(2)	(3)	(4)	(5)
Log R&D$_{t-1}$	0.096**	0.105**	0.106**	0.099*	0.109*
	(0.045)	(0.050)	(0.051)	(0.052)	(0.060)
Log employment$_t$			-0.181***	-0.174***	-0.179***
			(0.019)	(0.018)	(0.019)
Log payroll$_t$			-0.057	-0.082	-0.033
			(0.054)	(0.056)	(0.054)
Firm age$_t$			-0.033	-0.021	-0.003
			(0.033)	(0.028)	(0.030)
Firm diversified$_t$			-0.130	-0.135	-0.141
			(0.095)	(0.095)	(0.100)
Sales growth$_{t-1}$			0.130	0.124	0.129
			(0.090)	(0.091)	(0.099)
EBITDA$_{t-1}$			0.127	0.155	-0.112
			(0.260)	(0.261)	(0.294)
Investment/Total assets$_{t-1}$			0.811	0.731	0.508
			(0.543)	(0.553)	(0.617)
Log Tobin's Q$_{t-1}$			0.032	0.027	0.044
			(0.067)	(0.067)	(0.077)
Log Total Assets$_{t-1}$			-0.033	-0.054	-0.001
			(0.069)	(0.070)	(0.066)
PPE investment/Total assets$_{t-1}$			-0.058	-0.050	-0.063
			(0.385)	(0.393)	(0.424)
Cash$_{t-1}$			-0.502	-0.506	-0.521
			(0.307)	(0.315)	(0.320)
Debt$_{t-1}$			0.052	0.069	0.187
			(0.220)	(0.225)	(0.203)
Controls		Yes			
Year FE	Yes	Yes	Yes	Yes	
Firm FE	Yes	Yes	Yes	Yes	Yes
State FE			Yes	Yes	
Industry (SIC3) FE			Yes		
Industry (SIC4) FE				Yes	
Industry (SIC3)-year FE					Yes
State-year FE					Yes
N	36,000	36,000	36,000	36,000	36,000
Adj. R^2	0.156	0.167	0.176	0.184	0.180

Panel 2

Dependent variable: Employee Entrepreneurship$_{t+3}$

	(1)	(2)	(3)
Log R&D$_{t-1}$	0.102**	0.104**	0.101**
	(0.052)	(0.051)	(0.051)
Average employee age$_t$		-0.036***	
		(0.007)	
Share employees female$_t$		-0.084	
		(0.165)	
Share employees white$_t$		0.713***	
		(0.169)	
Share employees foreign$_t$		0.508**	
		(0.251)	
Average employee education$_t$		-0.055	
		(0.043)	
Average employee tenure$_t$		-0.023*	
		(0.013)	
Average employee experience$_t$		0.004	
		(0.017)	
Log patent classes			0.227*
			(0.120)
Log patents			-0.137
			(0.091)
Log forward citations			-0.006
			(0.022)
Log backward citations			-0.005
			(0.038)
Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
State FE		Yes	Yes
Industry (SIC3) FE		Yes	Yes
Industry (SIC4) FE	Yes		
N	36,000	36,000	36,000
Adj. R^2	0.181	0.179	0.176

Note: This table shows the effect of corporate R&D on employee entrepreneurship. The sample is an establishment-year panel of public firms. The dependent variable is the fraction of an establishment's workers as of first quarter of year zero who are entrepreneurs as of 1st quarter of year 3. An entrepreneur is defined as a person at a firm no more than 3 years old who is among the top 5 earners at that new firm. In panel 2, controls are the same as in panel 1. Standard errors are clustered by firm. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels.

Table 3: First Stage IV Results

Dependent variable: Log R&D$_{t-1}$

	(1)	(2)	(3)	(4)	(5)	(6)
Federal R&D tax price	-2.020***	-1.504***	-1.504***	-1.470***	-1.363***	-1.424***
	(0.295)	(0.231)	(0.231)	(0.225)	(0.168)	(0.199)
State R&D tax price	-1.158*	-0.950**	-0.956**	-0.978**	-0.303	-0.947**
	(0.691)	(0.476)	(0.476)	(0.471)	(0.375)	(0.420)
Controls		Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes		Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE			Yes	Yes		Yes
Industry (SIC3) FE				Yes		
Industry (SIC3)-year FE					Yes	
State-year FE					Yes	
Industry (SIC4) FE						Yes
N	36,000	36,000	36,000	36,000	36,000	36,000
R^2 (partial for the IV instruments)	0.032	0.027	0.026	0.026	0.022	0.025
F-test (instruments)	24.70	22.23	22.25	22.37	34.11	27.64

Note: This table shows the first stage of the instrumental variables analysis (Table 4). The sample is an establishment-year panel of public firms. We predict parent firm R&D using firm-level federal and state tax prices of R&D, which are partially determined by tax credits that change across time, states, and depending on firm age. The federal R&D tax price is the log firm-level tax price of R&D, based on the federal tax credit, and following Hall (1993) and Bloom et al. (2013). The state R&D tax price is the log state-level tax price of R&D, following Bloom et al. (2013). See Section 3.2 and Appendix Section 1 for details. Establishment controls are size and average wage. Firm controls are return on assets, sales growth, Tobin's Q, asset tangibility (PPE investment/total assets), size (log total assets), cash holdings, age, and diversified (establishments in more than one SIC 3-digit industry). Standard errors are clustered by firm. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels.

Table 4: Second Stage IV Results: Effect of R&D on Employee Entrepreneurship

Dependent variable: Employee Entrepreneurship$_{t+3}$

	(1)	(2)	(3)	(4)	(5)	(6)
Instrumented log R&D$_{t-1}$	0.577***	0.719***	0.659**	0.648**	0.587*	0.598**
	(0.207)	(0.274)	(0.271)	(0.270)	(0.317)	(0.276)
Controls		Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes		Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE				Yes		Yes
Industry (SIC3) FE			Yes	Yes		
Industry (SIC3)-year FE					Yes	
State-year FE					Yes	
Industry (SIC4) FE						Yes
N	36,000	36,000	36,000	36,000	36,000	36,000

Note: This table shows the effect of instrumented R&D on employee entrepreneurship. The sample is an establishment-year panel of public firms. The first stage predicting R&D is shown in Table 3. The dependent variable is the fraction of an establishment's workers as of first quarter of year 0 who are entrepreneurs as of 1st quarter of year 3. An entrepreneur is defined as a person at a firm no more than 3 years old who is among the top 5 earners at that new firm. We do not display controls because we are limited by Census in the number of coefficients we may disclose. Establishment controls are size and average wage. Firm controls are return on assets, sales growth, Tobin's Q, asset tangibility (PPE investment/total assets), size (log total assets), cash holdings, age, and diversified (establishments in more than one SIC 3-digit industry). Standard errors are clustered by firm. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels.

Table 5: Effect of R&D on Alternative Measures of Employee Entrepreneurship

Panel 1

Dependent variable:	Rate of employee departure to founding teams of... 1-yr old startups$_{t+1}$	1-yr old startups$_{t+2}$	1-yr old startups$_{t+3}$
	(1)	(2)	(3)
Log R&D$_{t-1}$	0.055**	0.057*	0.036
	(0.025)	(0.033)	(0.032)
Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Industry (SIC3) FE	Yes	Yes	Yes
N	36,000	36,000	36,000
Adj. R^2	0.090	0.097	0.106

Panel 2

Dependent variable:	Employee entrepreneurship to 1-or 2-yr old startups$_{t+2}$	Flow employee entrepreneurship$_{t+3}$	Number of employee-founded startups$_{t+3}$
	(1)	(2)	(3)
Log R&D$_{t-1}$	0.076*	0.89***	0.067*
	(0.042)	(0.070)	(0.037)
Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Industry (SIC3) FE	Yes	Yes	Yes
N	36,000	36,000	36,000
Adj. R^2	0.131	0.209	0.154

Note: This table shows the effect of R&D on alternative measures of employee entrepreneurship. The sample is an establishment-year panel of public firms. For a detailed description of the dependent variables, see Section 4.3. Controls are the same as in Table 2 Panel 1. Standard errors are clustered by firm. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels.

Table 6: Effect of Alternative Measures of R&D on Employee Entrepreneurship

Dependent variable: Employee Entrepreneurship$_{t+3}$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Above median Δ R&D$_{t-1}$	0.089***	0.078**						
	(0.033)	(0.032)						
Top 10 pct Δ R&D$_{t-1}$			0.132**	0.157**				
			(0.067)	(0.070)				
Bottom 10 pct Δ R&D$_{t-1}$					-0.105**	-0.114*		
					(0.053)	(0.060)		
R&D/Total Assets$_{t-1}$							1.020**	0.887*
							(0.495)	(0.529)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes		Yes		Yes		Yes	
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry (SIC3) FE	Yes		Yes		Yes		Yes	
State FE	Yes		Yes		Yes		Yes	
Year-industry FE		Yes		Yes		Yes		Yes
Year-state FE		Yes		Yes		Yes		Yes
N	36,000	36,000	36,000	36,000	36,000	36,000	36,000	36,000
Adj. R^2	0.176	0.180	0.176	0.180	0.176	0.180	0.175	0.180

Note: This table shows the effect of alternative measures of R&D on employee entrepreneurship. The sample is an establishment-year panel of public firms. Change (Δ) in R&D is defined as: $\frac{R\&D_{t-1}-R\&D_{t-2}}{.5\cdot(R\&D_{t-1}+R\&D_{t-2})}$. Top 10 pct Δ R&D$_{t-1}$ is 1 if the firm had a change in R&D that is in the top 10 percentiles in that year, and 0 if in the bottom 90 percentiles. Bottom 10 pct Δ R&D$_{t-1}$ is defined analogously. The dependent variable is the fraction of an establishment's workers as of first quarter of year zero who are entrepreneurs as of 1st quarter of year 3. An entrepreneur is defined as a person at a firm no more than 3 years old who is among the top 5 earners at that new firm. Controls are the same as in Table 2 Panel 1. Standard errors are clustered by firm. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels.

Table 7: Parent Variation in Effect of R&D on Employee Entrepreneurship

Dependent variable: Employee Entrepreneurship$_{t+3}$

	(1)	(2)	(3)	(4)
Log R&D$_{t-1}$	0.048	0.016	0.035	0.099*
	(0.057)	(0.062)	(0.066)	(0.052)
Log R&D$_{t-1}$·High Tech	0.083***			
	(0.029)			
High Tech	1.378***			
	(0.351)			
Log R&D$_{t-1}$·Large		0.130**		
		(0.056)		
Large		-0.333**		
		(0.149)		
Log R&D$_{t-1}$·Old			0.098	
			(0.067)	
Old			-0.315	
			(0.491)	
Log R&D$_{t-1}$·High patent generality				0.027*
				(0.016)
High patent generality				-0.093
				(0.076)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Industry (SIC3) FE	Yes	Yes	Yes	Yes
N	36,000	36,000	36,000	36,000
Adj. R^2	0.176	0.176	0.176	0.176

Note: This table shows how the effect of corporate R&D on employee entrepreneurship varies by parent firm characteristics. The sample is an establishment-year panel of public firms. High Tech is 1 if the parent establishment is in a high-tech industry, and 0 if not. Large is 1 if the parent has above-median total assets (calculated at the firm-year level), and 0 if below-median. Old is 1 if the parent is of above-median age (calculated at the firm-year level), and 0 if below-median. High patent generality is 1 if the parent has above-median patent generality (calculated at the industry-year level), and 0 if below-median. The dependent variable is the fraction of an establishment's workers as of first quarter of year zero who are entrepreneurs as of 1st quarter of year 3. An entrepreneur is defined as a person at a firm no more than 3 years old who is among the top 5 earners at that new firm. Controls are the same as in Table 2 panel 1. Standard errors are clustered by firm. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels.

Table 8: Effect of R&D on Employee Entrepreneurship by Employee-founded Startup Characteristics

Panel 1: What predicts venture capital-backed employee-founded startups?

Dependent variable: Employee-founded startup ever received VC

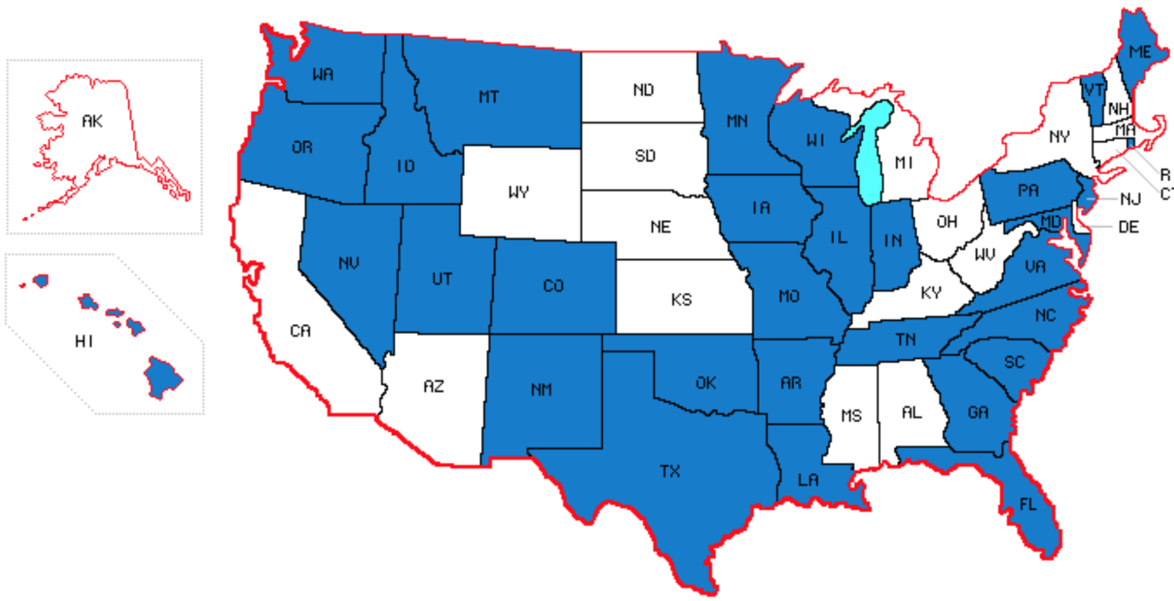
	(1)
Log R&D$_{t-1}$	0.007***
	(0.001)
Employee age$_t$	0.001**
	(0.000)
Employee age$^2{}_t$	-0.000**
	(0.000)
Employee female	-0.013***
	(0.002)
Employee white	0.003**
	(0.001)
Employee foreign	-0.002
	(0.004)
Employee born in state	-0.007***
	(0.001)
Employee education	0.001***
	(0.000)
Employee experience$_t$	-0.000
	(0.001)
Employee tenure$_t$	-0.000
	(0.000)
Employee log earnings$_t$	0.008***
	(0.002)
Employee-founded startup age$_{t+3}$	0.007***
	(0.001)
Employee-founded startup initial employment	0.008***
	(0.002)
Continued...	
...Continued	
Establishment Log Employment$_t$	0.001
	(0.001)
Establishment average employee wage$_t$	0.012***
	(0.003)
Firm Age$_t$	-0.002***
	(0.001)
Firm Diversified	-0.003
	(0.006)
Firm Sales growth$_{t-1}$	0.004
	(0.006)
Firm EBITDA$_{t-1}$	-0.008
	(0.016)
Firm Investment/Total$_{t-1}$	-0.013
	(0.041)
Firm Log Tobin's Q$_{t-1}$	0.002
	(0.004)
Firm Log Total Assets$_{t-1}$	-0.006***
	(0.002)
Firm PPE Investment/Total Assets$_{t-1}$	-0.004
	(0.012)
Firm Cash$_{t-1}$	0.076***
	(0.015)
Firm Debt$_{t-1}$	0.009
	(0.007)
Year-state FE	Yes
Year-industry (SIC3) FE	Yes
N	108,000
Adj. R^2	0.079

Panel 2: Other employee-founded startup characteristics

Dependent variable:	Employee-founded startup... in same industry (SIC2) as parent establishment	in same state as parent establishment	in high-tech industry	log wages$_{t+3}$	exit$_{t+5}$
	(1)	(2)	(3)	(4)	(5)
Log R&D$_{t-1}$	-0.007**	0.002	0.009***	0.028***	0.007**
	(0.003)	(0.002)	(0.004)	(0.006)	(0.003)
Controls	Yes	Yes	Yes	Yes	Yes
Year-state FE	Yes	Yes	Yes	Yes	Yes
Year-industry (SIC3) FE	Yes	Yes	Yes	Yes	Yes
N	108,000	108,000	108,000	108,000	108000
Adj. R^2	0.206	0.053	0.102	0.318	0.083

Note: This table shows the effect of R&D on types of employee entrepreneurship. The sample is at the employee-founded startup level. Based on the main variable used in Table 2, we identify whether the new firm associated with the departing employee has a given characteristic. The dependent variable in panel 1 column 1 is 1 if the employee-founded startup ever received VC backing (either before or after the employee-founded startup is identified in year $t + 3$), and 0 if not. The "Employee..." controls in panel 1 column 1 refer to the employee who left the parent to found a new firm. The dependent variable in panel 2 column 1 (2) (3) is 1 if the employee-founded startup is in the same 2-digit SIC code as the parent establishment (is in the same state as the parent establishment) (is in a high-tech industry), and 0 if not. The dependent variable in panel 2 column 4 is the departing employee entrepreneur's log wages at the new firm in the 1st quarter of year 3. An entrepreneur is defined as a person at a firm no more than 3 years old who is among the top 5 earners at that new firm. The dependent variable in panel 2 column 5 is 1 if the employee-founded startup exited (failed, though a small minority may be acquisitions) by year 5, and 0 if not. Controls are the same as in Table 8 Panel 1, except that we include the indicator for being VC-backed as an additional control in panel 2 columns 1-5. Standard errors are clustered by parent firm. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels.

Figure 1: Map of States with LEHD (Employee-founded Startup) Data



Note: This figure shows the 31 LEHD states that we have access to. We observe all employee-founded startups located in these states.

Appendix

(for online publication)

Instrumental variables calculation and discussion

A.1. The Federal R&D tax credit

The first instrument is the federal tax price of R&D, which we denote ${\rho_{ft}}^F$. Implemented in 1981, the federal "Research and Experimentation" (R&E) tax credit permits firms to reduce their corporate income tax liability by the value of the credit. The credit was extremely complex to calculate (leading to a substantial simplification in 2009), and has changed over time. In the early 2000s, the total value of the federal credits was about $5 billion per year (Wilson et al. 2005).

In this description, we focus on the calculation of the credit between 1990 and 2005, which is the sample period for which we need to predict public firm R&D.[25] The general formula for the R&E tax credit is as follows, for tax year t and firm f:

$$R\&E\ Tax\ Credit\ Value_{tf} = 20\% \cdot [QRE_{tf} - Base_{tf}] + 20\% \cdot [Basic\ Research_{tf}] \quad (3)$$

The last element, basic research expenditures, must be paid to a qualified organization, which is either a research university or tax-exempt scientific organizations. The other, more complex type of research costs are qualified research expenditures (QRE). These must occur within the U.S., and have three categories: salaries and wages, supplies, and contract research. The law is quite specific about what counts and what does not count as QRE. For example, QRE must be technological in nature and relate to new or improved function, performance, reliability, or quality. Among other excluded types, research after commercial production of a component, survey research, and social science research do not count.[26]

[25] The calculation was quite different before 1989. In practice, we draw heavily from code originally written for Hall (1993).

[26] The complete legal text is here: https://www.law.cornell.edu/uscode/text/26/41.

The "base" amount is by far the most complicated element. It is constructed using the following equation:

$$Base_{tf} = Fixed\ Base\ \%_{tf} \cdot Sales_t$$

The complexity lies in the fixed base percentage, which varies by a firm's "startup" status. This term, which is used in the legislation and in Hall (1993), refers to the number of years since the firm's first instance of QRE. It is calculated as follows (firm index omitted for simplicity):

$$Fixed\ Base\ \% = \begin{cases} \max\left[\frac{\sum_{t=1984}^{1988}\frac{QRE_t}{Sales_t}}{5},\ 0.16\right] & if\ QRE_{1983} > 0\ \&\ Sales_{1983} > 0 \\ 0.03 & if\ QRE_{t-6} \in \{0, \emptyset\} \\ \frac{1}{6}\left[\frac{\sum_{t=-2}^{-1}\frac{QRE_t}{Sales_t}}{2}\right] & if\ QRE_{t-7} \in \{0, \emptyset\}\ \&\ QRE_{t-6} > 0 \\ \frac{1}{3}\left[\frac{\sum_{t=-2}^{-1}\frac{QRE_t}{Sales_t}}{2}\right] & if\ QRE_{t-8} \in \{0, \emptyset\}\ \&\ QRE_{t-7} > 0 \\ \frac{1}{2}\left[\frac{\sum_{t=-3}^{-1}\frac{QRE_t}{Sales_t}}{3}\right] & if\ QRE_{t-9} \in \{0, \emptyset\}\ \&\ QRE_{t-8} > 0 \\ \frac{2}{3}\left[\frac{\sum_{t=-4}^{-1}\frac{QRE_t}{Sales_t}}{4}\right] & if\ QRE_{t-10} \in \{0, \emptyset\}\ \&\ QRE_{t-9} > 0 \\ \frac{5}{6}\left[\frac{\sum_{t=-5}^{-1}\frac{QRE_t}{Sales_t}}{5}\right] & if\ QRE_{t-11} \in \{0, \emptyset\}\ \&\ QRE_{t-10} > 0 \\ \min\left[\frac{QRE_t}{Sales_t}\right]_{t-6}^{t-1} & if\ QRE_{t-x} \in \{0, \emptyset\}\ \&\ QRE_{t-x-1} > 0\ \forall\ \text{x} \geq 12 \end{cases}$$

In words, the first row is interpreted in the following way. For firms that had positive QRE and sales in 1983, the fixed base percentage is the maximum of 16% and the average of R&D intensity over the five years between 1984 and 1988. All the subsequent rows in the above equation pertain to what the law terms “startups.” For example, for the first five taxable years after the first year in which a firm has positive QRE, the fixed base is 3%. In the 6th such year, it is one-sixth the average of the R&D intensity over the previous two years. The following rows are similarly calculated. Starting in the eleventh such year, firm may choose the percentage from any of the prior fifth through tenth years.

A few other details bear mention. The expense deduction for R&D is recaptured, reducing the effective credit rate from 20% to about 13.5%. Also, in the fiscal year 1995-6, the credit lapsed entirely. Additionally, when the credit value is larger that taxable profits, it can be carried forward for ten years. Finally, between 1990 and 1996, the only option was the R&E tax credit. Starting in 1996, firms could elect the alternative incremental credit (AIC), in lieu of the R&E tax credit. This has 3 tiers depending on R&D intensity (QRE relative to sales); if intensity is 1-1.5% (1.5-2%) (>2%), the AIC rate is 2.65% (3.2%) (3.75%), respectively. These rates have varied over time; they were lower in the late 1990s, and have increased in recent years.

The credit is firm-specific for a number of reasons. First, it depends on firm age, with annual changes for most firms. Second, the “base” amount of R&D is calculated using a firm’s past R&D and current-year sales. Third, the base amount of the tax credit is the difference between realized R&D and the base. Fourth, there is a lower implicit value of the credit among tax exhausted firms because the value of the carry forward must be discounted. Finally, the lapse in 1995-96 generates additional within-firm variation, only for firms with R&D expenditures that year.

The R&E tax credit (denoted ERC_t) is in practice considerably more complicated to calculate than Equation 3, and follows Equation 7 in Hall (1992) and underlying equations not shown in her paper; these are available in Stata code on request. Calculating ERC_t begins with the tax credit rate (constant across firms), and multiplies by a categorical variable derived from QRE. This is then deducted from corporate tax liability. Then, a 3-year carry-back and a 15-year carry-forward are added in cases of no taxable income this year. Once this tax credit is arrived at, the tax price of R&D is calculated following Equation 6 in Hall (1992). This is:

$$\rho_{ft}^{F} = \rho_t^{R}\left[1 - T_t\left(1+r\right)^{-Jt}\tau\right] - \eta ERC_t \tag{4}$$

Here, ρ_t^R is an R&D deflator divided by a GDP deflator, or the "price" of R&D investment in the absence of taxes, T_t is an indicator for whether the firm has taxable income in the current year, J_t is the number of years until loss carry-forwards will be exhausted, τ_t is the corporate tax rate, and η_t is QRE. If ${\rho_{ft}}^F = 1$, then the firm should not treat R&D differently than other expenditure. If ${\rho_{ft}}^F < 1$, R&D is less expensive than other expenditure because of the tax credit.

In practice, we find substantial within-industry variation in ${\rho_{ft}}^F$, especially in manufacturing and services. The median tax price is well below 1 on average, so that R&D is cheaper than other spending. Within industries, the distributions have negative skew (i.e., a longer right tail). We also ensure that relevant current year variables, including R&D, do not have strong explanatory power over the tax price of R&D. Within firms, we find small positive correlations (all less than 0.1) between ${\rho_{ft}}^F$ and employment, assets, and R&D. In regressions, we verify substantial firm-level variation in the tax price of R&D. Firms in high tech areas such as pharmaceuticals and electronics, tend to have the most variation.

A.2 State R&D tax credits

State R&D tax credits have been generally modeled on the federal one. The first state R&D tax credit was implemented in 1982 by Minnesota; by the end of our sample period, forty states had some sort of R&D tax credit. The calculation of the base amount, and the definition of qualified R&D, can vary across states (Wilson et al. 2005). According to Miller & Richard (2010), manufacturing-intensive states, and those with one-party political control, are more likely to pass R&D tax credits. They argue that the tax credits primarily support incumbent R&D-conducting firms. To the best of our knowledge, the state credits are not refundable during the sample period.

The state instrument requires two objects: the state tax price component of the R&D user cost of capital, and a measure of the share of a firm's R&D that occurs in a given state. For both, we follow Bloom et al. (2013). First, we use the state tax price of R&D in Wilson (2009). He incorporated state level corporate income taxes, depreciation allowances, and R&D tax credits into this tax price component, which we call ρ^S_{st}.[27] These credits vary across states and time. They allow a firm to offset its state-level corporate tax liabilities, and they are calculated by weighting total firm profits according to the location of the firm's sales, employment, and property. Thus firms with R&D activities in the state will likely both have tax liability and R&D tax credit eligibility there.

The second object, θ_{fst}, is a proxy for a firm's R&D share in a given state-year. It is the 10-year moving average of the share of the firm's patent inventors located in state s.[28] The firm's state-level tax price is then $\rho^S_{ft} = \sum_s \theta_{fst}\rho^S_{st}$.

A.3 Concerns

There are five potential concerns. Most importantly, the exclusion restriction is that tax credits cannot affect employee entrepreneurship. In a rigorous border-county differences-in-differences model, Curtis & Decker (2018) show that R&D tax credits have no effect on startup formation. We also show empirically that there is no relation between the state tax credits and state-level startup creation, or the federal tax credit and national startup creation. We do this using two data sources, each of which have limitations. The first is the Business Dynamics Statistics (BDS), which contains firm entry by state for our entire sample period, but does not have state-industry data.[29] The second is the Quarterly Workforce Indicators (QWI), a publicly available dataset derived from the LEHD. While the QWI has state-industry level data, its coverage is poor in the early years of our data, with counties being added over time.[30]

At the state level we regress either the log number of new firms or the change in firm entry rates year to year on the tax price of R&D, as well as state and year

[27]Specifically, it is roughly: $\frac{1-(tax\ credits+depr.\ allowances)}{1-tax\ rate}$.
[28]The data is from NBER patent data, available at https://sites.google.com/site/patentdataproject/Home/downloads.
[29]This public version of the LBD is available at https://www.census.gov/ces/dataproducts/bds/data_firm.html.
[30]We used a transformed version of the data used in Adelino et al. (2017), courtesy of Song Ma.

fixed effects. The results with BDS data are in Table 5. We cluster errors by state. Regardless of the fixed effects or standard error assumptions, we find that the tax credits have no correlation with startup entry (panel 1). Using the QWI sample, our dependent variable is either the logged new jobs created in new firms in the past two years, or the change in the number of new jobs created in new firms in the past two years. We consider only R&D-intensive industries.[31] Again, regardless of whether we use year and/or state fixed effects, and regardless of the standard error assumptions, we find no effect of the tax price of R&D on these measures. This is in Table 5 Panel 2.

At the federal level, we regress either the log number of new firms or the change in firm entry rates on the statutory federal R&D tax credit. This is, of course, very different from the firm-specific tax price of R&D that is calculated per the description in Section A1.1. This reflects baseline changes in the rate, which is then applied to a firm's specific situation. There are very few observations, and we do not use robust standard errors. The results, in Table 5 Panel 3, again show no correlation.

More generally, the legal literature has argued that R&D tax credits are not useful to startups, as they have no or little taxable income against which to offset losses from failed R&D efforts (Bankman & Gilson 1999).[32] Perhaps in response to this, a few states have recently made their R&D tax credits transferable, so that firms without revenue can potentially derive value from them. However, these policies occurred after the end of our sample period.

The second concern is that changes in state-level R&D tax credits may lead firms to reallocate R&D (or misreport it such that it appears reallocated). For studies evaluating how a state-level R&D tax credit affects national R&D, this is a central concern. In our case, however, such reallocation will simply reduce the power of the instrument. As long as the combined instruments have adequate power, some degree of reallocation should not bias our findings. It does lead us to expect that the federal

[31] NAICS codes 31-33, 51, and 54.

[32] Bankman & Gilson (1999) note that "the U.S. tax code subsidizes R&D by existing successful companies by allowing losses from failed attempts at innovation to offset otherwise taxable income from other activities. Since startups have no other income against which their losses from a particular project may be set off, the government in effect gives established companies with a stable source of income an R&D tax subsidy that is not available to a startup entity."

instrument will have more power than the state instrument, which is indeed what we find. This is because it should have a larger effect on firms that only operate in the affected state, but most firms with positive R&D operate in multiple states.

The third concern is that the tax credits may not be large enough to affect R&D. The above sections pointed to substantial literature finding R&D responses to R&D tax credits that are large in economic magnitude and quite robust, especially for the federal instrument. The literature examining the state instrument finds large within-state elasticities, but also finds evidence of reallocation across states.

The fourth concern is that changes to the R&D tax credits may be anticipated by firms, which may then behave strategically to maximize their value. The federal tax credit formula is exceedingly complicated, as explained above, and it seems implausible that firms will optimize on all of the variables (especially firm age) in order to maximize the tax credit value. Strategic behavior around state tax credit changes would require firms in one state to response by moving states. The tax credits are not large enough to merit such a response from many firms. For firms in multiple states, reallocation across states should attenuate the effect of the instrument. Beyond these points, note that the goal is to predict changes in R&D. Suppose that firms choose to conduct less R&D in the years immediately preceding the tax credit change and more after in order to maximize the tax credit benefit. This does not obviously bias our main result, which is that changes to R&D affect employee entrepreneurship.

Finally, the fifth concern is that state decisions to adopt R&D tax credits could be endogenous, reflecting recent declines in R&D. Bloom et al. (2013) consider this possibility at length, and show that the results are robust to lagging the tax credit instruments one and two periods. They also point out that cross-sectional variation in the state R&D tax credit rates is very large relative to the average rate within states, and also large relative to the secular increase in the tax credit generosity that has occurred over time. Finally, Chirinko & Wilson (2008), Chirinko & Wilson (2011), and Bloom et al. (2013) show that the level and timing of R&D tax credit adoption is uncorrelated with local economic observables like state R&D expenditure or per capita GDP, once year and state fixed effects are included.

In sum, we believe that R&D tax credits offer the best available source of variation driving corporate R&D that is plausibly unrelated to technological opportunities that could jointly give rise to parent R&D and employee entrepreneurship.

Table 1: Sample Composition by Industry

Panel 1

1990 -2001

Industry	In Sample	Out Sample
Construction	4.8%	4.1%
Finance, Insurance, and Real Estate	5.6%	6.3%
Manufacturing	15.4%	15.8%
Mining	0.6%	0.4%
Services	27.9%	28.8%
Total Government	16.4%	17.2%
Trade	23.7%	22.6%
Transportation and Public Utilities	5.5%	4.8%

Panel 2

2002-2008

Industry	In Sample	Out Sample
Construction	5.6%	4.8%
Educational Services	1.9%	2.4%
Financial Activities	5.9%	6.3%
Government	16.3%	17.0%
Health Care and Social Assistance	10.9%	11.6%
Information	2.2%	2.5%
Leisure and Hospitality	9.9%	9.1%
Manufacturing	10.6%	10.7%
Mining and Logging	0.6%	0.3%
Other Services	4.0%	3.9%
Professional and Business Services	12.3%	12.8%
Retail Trade	11.6%	11.1%
Transportation and Warehousing	3.4%	2.8%
Utilities	0.4%	0.4%
Wholesale Trade	4.4%	4.2%

Note: This table compares the data in our sample (from 31 states) to national data from the Bureau of Labor Statistics (BLS) Current Employment Statistics (CES) Survey from 1990-2008. This is done separately for the pre-2002 and post-2002 periods because before 2002, the BLS used SIC codes, while after 2002, it used NAICS codes. Panel 1 shows the pre-2002 industries, and Panel 2 the post-2002 industries. We divide state-industry level employment by total state employment across all states in our sample. We do this for each year, and then average across years. We compare this to the analogous figure for states that are not in our sample (right column).

Table 2: Industry Composition by Sample

Panel 1

1990 - 2001

Industry	In Sample	Out Sample
Construction	63.7%	36.3%
Finance, Insurance, and Real Estate	57.4%	42.6%
Manufacturing	59.4%	40.6%
Mining	69.4%	30.6%
Services	59.2%	40.8%
Total Government	58.8%	41.2%
Trade	61.1%	38.9%
Transportation and Public Utilities	63.2%	36.8%
Total Observations	60.0%	40.0%

Panel 2

2002-2008

Industry	In Sample	Out Sample
Construction	64.5%	35.5%
Educational Services	55.0%	45.0%
Financial Activities	59.6%	40.4%
Government	59.9%	40.1%
Health Care and Social Assistance	59.5%	40.5%
Information	57.5%	42.5%
Leisure and Hospitality	62.7%	37.3%
Manufacturing	60.6%	39.4%
Mining and Logging	71.9%	28.1%
Other Services	61.6%	38.4%
Professional and Business Services	59.9%	40.1%
Retail Trade	61.8%	38.2%
Transportation and Warehousing	65.3%	34.7%
Utilities	59.6%	40.4%
Wholesale Trade	62.3%	37.7%
Total Observations	62.3%	37.7%

Note: This table compares the data in our sample (from 31 states) to national data from the Bureau of Labor Statistics (BLS) Current Employment Statistics (CES) Survey from 1990-2008. This is done separately for the pre-2002 and post-2002 periods because before 2002, the BLS used SIC codes, while after 2002, it used NAICS codes. Panel 1 shows the pre-2002 industries, and Panel 2 the post-2002 industries. Each percent is the share of people employed in an industry in our sample states (left column) versus the other states (right column).

Table 3: Effect of R&D on Non-entrepreneurial Employee Outcomes

Dependent variable:	Stayers_{t+1}	Movers to old firms_{t+3}	Depart LEHD coverage_{t+3}	Movers to firms of unknown age_{t+3}
	(1)	(2)	(3)	(4)
Log $\text{R\&D}_{t-1}$	-1.133	0.485	-0.004	0.506
	(0.715)	(0.608)	(0.133)	(0.452)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Industry (SIC3) FE	Yes	Yes	Yes	Yes
N	36,000	36,000	36,000	36,000
Adj. R^2	0.385	0.356	0.222	0.207

Note: This table shows the effect of R&D on alternative employee outcomes. The sample is an establishment-year panel of public firms. In column 1, the dependent variable is the fraction of an establishment's workers in the 1st quarter of year zero who remain at the firm in the 1st quarter of year 3. In column 2, the dependent variable is the fraction of an establishment's workers in the 1st quarter of year zero who move to a firm that is more than 3 years old by the 1st quarter of year 3. In column 3, the dependent variable is the fraction of an establishment's workers in the 1st quarter of year zero who drop out of the employment sample by the 1st quarter of year 3 (note they may have moved to an uncovered state). In column 4, the dependent variable is the fraction of an establishment's workers in the 1st quarter of year zero who move to an organization whose age is unknown by the 1st quarter of year 3. Controls are the same as in Table 2 Panel 1. Standard errors are clustered by firm. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels.

Table 4: Reverse Causality Test (Effect of Employee Entrepreneurship on R&D)

Dependent variable: Log R&D_t

	(1)	(2)	(3)
One-year employee entrepreneurship$_{t-2,\ t-1}$	0.008		
	(0.005)		
Two-year employee entrepreneurship$_{t-3,\ t-1}$		0.001	
		(0.006)	
Three-year employee entrepreneurship$_{t-4,\ t-1}$			-0.005
			(0.003)
Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Industry (SIC3) FE	Yes	Yes	Yes
N	36,000	36,000	36,000
Adj. R^2	0.879	0.879	0.879

Note: This table shows that current employee entrepreneurship does not predict corporate R&D. The sample is an establishment-year panel of public firms. The independent variables are lagged variations on our main employee entrepreneurship rate measures used as the dependent variable in Tables 2 and 4. The one-year employee entrepreneurship$_{t-1}$ rate is the fraction of an establishment's workers as of first quarter of year $t-1$ who are entrepreneurs as of 1st quarter of year t, which is the year that R&D is measured (the dependent variable). The two-year employee entrepreneurship$_{t-2}$ rate is the fraction of an establishment's workers as of first quarter of year $t-2$ who are entrepreneurs as of 1st quarter of year t. The three-year employee entrepreneurship$_{t-3}$ rate is the fraction of an establishment's workers as of first quarter of year $t-3$ who are entrepreneurs as of 1st quarter of year t. An entrepreneur is defined as a person at a firm no more than 3 years old who is among the top 5 earners at that new firm. Controls are the same as in Table 2 Panel 1. Standard errors are clustered by firm. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels.

Table 5: Relationship between state tax price of R&D and state startup formation

Panel 1: Quarterly Workforce Indicator (LEHD) data

Dependent variable	2-year employment growth		Log 2-year employment growth		Change in 2-year employment growth	
	(1)	(2)	(3)	(4)	(5)	(6)
State tax price of R&D	-20068	4754	-.74	.33	-117	-6.5
	(21295)	(9035)	(.59)	(.36)	(7912)	(57677)
State f.e.	Y	Y	Y	Y	Y	Y
Year f.e.	Y	N	Y	N	Y	N
N	449	449	449	449	448	447
R^2	.21	.2	.44	.43	.11	.11

Panel 2: Business Dynamics Statistics Data

Dependent variable	2-year employment growth		Log 2-year employment growth		Change in 2-year employment growth	
	(1)	(2)	(3)	(4)	(5)	(6)
State tax price of R&D	-1650	-493	-.11	.036	188	-583
	(3570)	(756)	(.37)	(.084)	(1619)	(981)
State f.e.	Y	Y	Y	Y	Y	Y
Year f.e.	Y	N	Y	N	Y	N
N	1530	1530	1530	1530	1529	1529
R^2	0.1585	0.0016	.24	0.0012	0.0204	0.0005

Note: This table shows estimates of the relationship between last year's state tax price of R&D (from Wilson), and employment growth at new firms. Panel 1 uses data from the QWI, courtesy of Song Ma. Firms are limited to R&D-intensive (high tech) sectors. Panel 2 uses data from the BDS, where all firms are used as the data do not include industry information. Errors are clustered at the state *** indicates p-value<.01.

Panel 3:

Data source:	Quarterly Workforce Indicator (LEHD) data		Business Dynamics Statistics Data	
Dependent variable	Log 2-year employment growth	Change in 2-year employment growth	Log 2-year employment growth	Change in 2-year employment growth
	(1)	(2)	(3)	(4)
Federal R&D credit	4.4	-39912	-.19	-377227
	(7.3)	(885697)	(.16)	(274243)
N	16	15	30	37
R^2	.026	.00016	.05	.051

Note: This panel shows estimates of the relationship between last year's federal tax price of R&D, and employment growth at new firms. *** indicates p-value$<$.01.