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2013/89/EFE/ACGRE
(Revised version of 2012/47/EFE/ACGRE)

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July 2013

Revised version of 2012/47/EFE/ACGRE

Forthcoming, *Management Science*

We thank Iain Cockburn, Florian Ederer, Joan Farre-Mensa, Josh Lerner, the editorial review team, and audience members at the IFN Conference on Entrepreneurship, Firm Growth and Ownership Change, the NBER productivity lunch, Queen's University Economics of Entrepreneurship and Innovation Conference, Stanford, the Strategic Management Society annual meeting, Temple Fox School, Tilburg Entrepreneurial Finance Conference, and the UCLA Entrepreneurship & Innovation seminar for helpful comments. We also thank Sean Nicholson and Simon Wakeman for providing biotechnology product and alliance data; and Phoebe Chen and especially Andy Wu for excellent research assistance. We acknowledge funding from the Wharton Entrepreneurship and Family Business Research Centre at CERT, the Mack Institute for Innovation Management, the Centre of Excellence for Applied Research and Training Term Fund, and the Wharton-INSEAD Center for Global Research and Education.

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Abstract

We examine how IPOs and acquisitions affect entrepreneurial innovation as measured by patent counts and forward patent citations. We construct a firm-year panel dataset of all venture capital-backed biotechnology firms founded between 1980-2000, tracked yearly through 2006. We address the possibility of unobserved self-selection into exit mode by using coarsened exact matching (CEM), and in two additional ways: (1) comparing firms that filed for an IPO (or announced a merger) with those not completing the transaction for reasons unrelated to innovation, and (2) using an instrumental variables approach. We find that innovation quality is highest under private ownership and lowest under public ownership, with acquisition intermediate between the two. Together with a set of within-exit mode analyses, these results are consistent with the proposition that information confidentiality mechanisms shape innovation outcomes. The results are not explained by inventor-level turnover following exit events or by firms' pre-exit window dressing behavior.

Keywords: Entrepreneurial Exits; Innovation; Information Confidentiality.

1. Introduction

Equity investments in entrepreneurial start-ups are illiquid until an exit (or liquidity) event such as an IPO or acquisition by another entity.[1] As a result, a leading performance measure that researchers in the entrepreneurship literature investigate is the likelihood of an exit event. The main motivation for studying such outcomes is that these events offer liquidity and financial returns to the entrepreneurial founders, their investors, and other shareholders. We know little, however, about the relationship between entrepreneurial exit modes and organizational innovation, particularly when taking into account self-selection. Understanding the link between exits modes and innovation outcomes is important to start-up entrepreneurs and managers at established companies alike. For entrepreneurs, alternate exit mode choices involve tradeoffs in organizational structure, governance, incentives, resources, and degree of information disclosure – all of which can shape innovation outcomes. For industry incumbents, a deeper understanding of the consequences of organizational changes accompanying the going public process and the entrepreneurial acquisition process can be important in assessing the innovation profile of potential competitors.[2] We therefore examine the research question of the relationship between entrepreneurial exit mode and innovation while taking into account the role of (unobserved) entrepreneurial self-selection into exit mode.

To illustrate the phenomenon we study, consider the example of Genentech. Tom Perkins, co-founder of the venture capital firm Kleiner Perkins and Chairman of Genentech's board from 1976 through 1990, reflected on the company's possible sale to Eli Lilly prior to Genentech's 1980 public offering: "We did have some preliminary discussions with Lilly. They made one of the biggest mistakes in business history in that they didn't try to push us very hard to sell the company. I think if Lilly, a year before the public issue, had made an attractive offer we probably would have gone for it. Because there were no precedents to follow; we would have had a good return on the investment. That would be that. But they didn't. So we gave up that idea and decided to pursue the public issue." (Perkins, 2002). On the likely consequences of such a buyout, Genentech's CEO in the early 1990s, Kirk Raab speculated: "Genentech would not be the wonderful place it is today if some large pharmaceutical firm had bought us…and like an amoeba absorbed us, which is what big companies often do." (Raab, 2003).

[1] We use the terms "exit event" and "liquidity event" interchangeably. These refer to the ability of the entrepreneur or venture capitalist (VC) to fully or partially sell their equity stake in a VC-backed start-up firm.

[2] Another motivation for investigating the relationship between entrepreneurial exit modes and innovation outcomes is to better assess the public policy implications of the shifting balance of entrepreneurial exit modes away from initial public offerings and toward mergers and acquisitions. Figure 1 (panel A) plots the ratio of deal (and deal value) from VC-backed M&As to IPOs over the 1992 to 2007 time period. The same data series are plotted for VC-backed biotechnology firms (the industry subject of this study) in Figure 1, panel B. Acquisitions have clearly outstripped IPOs as the modal form of entrepreneurial exit. While assessing the welfare implications of this shift is beyond the scope of this paper, the innovation consequences are a key component to such an analysis.

This anecdote exemplifies a key difficulty in designing a study investigating the innovation consequences of entrepreneurial liquidity mode: the possible issue of self-selection into mode based on unobserved factors. Clearly the gold standard of random assignment of ventures to exit mode is not available. Not only is being in the position to consider a liquidity event (of any sort) not a random occurrence, the choice between exit modes may be importantly influenced by unobserved factors. While we recognize that disentangling the co-mingling of exit mode selection and treatment effects is challenging, we employ three approaches enabled by our panel dataset of the universe of VC-funded US biotechnology start-ups founded between 1980 and 2000. First, we employ a coarsened exact matching (CEM) algorithm to our data to define more closely aligned treatment and control samples. Second, we conduct a quasi-experiment in which we compare the innovation profiles of firms experiencing a given exit event to subsamples of firms which "nearly" experienced the event, but for reasons unrelated to innovation, did not complete the exit process. Finally, we employ an instrumental variables strategy centered on the relative liquidity of alternative exit channels in the biotechnology industry.

Across the range of our comparisons, we find a decline in innovation quality (as measured by patent citations) as a causal effect of both the IPO and M&A treatments, with the IPO effects larger in magnitude. While the quantity of innovations (as measured by patent counts) also declines following an IPO, we find an increase in this measure following an M&A. These results are consistent with an information confidentiality mechanism, in which different levels of information disclosure associated with alternative exit modes influence innovation rates (going public entails the largest information disclosure, while remaining privately-held involves the least, with being acquired in between). We conduct within-exit mode analyses to sharpen our evidence for this mechanism. For firms going public, there is a significant negative interaction on innovation quality between stock market analyst attention and the level of preclinical trial products firms have in their pipeline. For biotechnology firms, the veil of secrecy may be most important during the preclinical phase of drug development, and the interaction with analyst coverage is consistent with an information disclosure mechanism. Furthermore, among acquired firms, we find being acquired by a private rather than a public acquirer (the latter associated with higher information disclosure) results in higher innovation quality among M&As. In addition, our results point to an important role for managerial incentives in M&As: greater technology overlap between the acquiring and acquired firms boosts patent quantity but reduces quality, suggesting that in more competitive settings, once the firm becomes part of another organization, acquired firm managers prefer short-run observable outcomes (patent quantity) at the expense of outcomes which may not be observable until the longer-run (patent quality). Finally, we investigate the extent to which inventor turnover following liquidity events might account for these empirical patterns by constructing an inventor-year panel dataset covering inventor histories both in- and out-of-sample with regard to our focal firms. We find that the

inventor-level turnover effects cannot explain the firm-level patterns, which are instead consistent with information confidentiality mechanisms.

2. Literature

A key pre-condition to the entrepreneurial choice among exit modes is building a significant business to warrant further expansion. Conditional on this, there have been just a few papers to our knowledge that deal with this choice, with these papers suggesting four categories of explanatory factors. In the context of significant VC involvement, a first set of explanations suggests that financing contractual design can influence exit outcomes, as VCs negotiate certain control rights based on their assessment of entrepreneurial quality (e.g., Hellmann, 2006; Cumming, 2008). A second set of explanations centers on industry or market characteristics, such as the industry degree of leverage and concentration, or public equity hotness (e.g., Brau et al., 2003; Bayar & Chemmanur, 2011, 2012). A third set of explanations relates to the role of firm and product market characteristics, such as growth potential, capital constraints, degree of information asymmetry, and complementarity with the potential acquirer (e.g. Poulsen & Stegemoller, 2008; Bayar & Chemmanur, 2011, 2012). Finally, founder characteristics, most notably entrepreneurial preferences for control versus value creation, can play a role. Schwienbacher (2008) argues in a theoretical model that because entrepreneurs value control, which is more likely under an IPO exit, they are driven to be more innovative in order to reduce the likelihood of being acquired.

While there is therefore a limited but growing literature examining the entrepreneurial choice among multiple exit modes, the Schwienbacher paper is the only one to our knowledge that aims to link this choice directly to entrepreneurial innovation. While we do not believe that any empirical study has addressed this topic, there are two mechanisms through which this choice might impact innovation: a first mechanism relates exit mode innovation outcomes to project selection incentives under different ownership regimes; a second mechanism relates to whom information is revealed under varied ownership structures to innovation outcomes. While both relate innovation to the degree of information confidentiality the enterprise is able to retain without disclosing to various parties, we discuss each mechanism and its associated empirical implications separately as each operates in a different way.

Organizational ownership, project selection, and innovation. Under private ownership, the classic agency issues associated with the separation of ownership and corporate control are typically not as severe, as the (concentrated) insiders are also the managers. By contrast, under public ownership, due to the expanded number of possible shareholders, the regulatory requirements associated with going public include regular public disclosures on firm operations. These disclosures may have innovation effects. If managers know that they will have to report project status on a regular basis, they may be incentivized to select projects that are more likely to yield steady progress. Developing important

innovations, however, is a process that not only involves a longer time horizon, but also offers returns with higher variance relative to more certain investment activities. Moreover, innovation often requires experimentation, which may be curtailed if managers know they have to report results on a quarterly basis. As such, for situations in which managers want to incentivize exploratory (rather than exploitative) behavior, private rather than public firm ownership might be optimal (Ferreira, et al., forthcoming). The empirical results of Lerner, et al. (2011) are consistent with these ideas. In that study, the authors use the private equity context to evaluate whether firms' innovation profiles change as a result of being acquired via buyout, finding an overall increase in the innovative output of private equity-acquired firms over the long-term (as a result, going private from a publicly-held status improves innovation outcomes). Therefore, information disclosure to a broad audience under public ownership can negatively impact innovation quantity and quality by reducing the tolerance for failure (Manso, 2011; Ferreira, et al., forthcoming).

With regard to acquisitions, while in concept there are synergies of personnel and organizations that should benefit the acquisition target (the entrepreneurial firm), the act of merging, typically into a larger organization, can impose costs that might dampen innovation. Seru (forthcoming) argues that as a division within a conglomerate, the acquired firm may have skewed managerial incentives to oversell the true prospects of a given technology in an effort to acquire more resources for the business unit (or to target projects with near-term as opposed to longer-term payoffs). The result is that managers in the conglomerate are less willing to fund innovative projects in the first place, as they are not able to assess the true quality of projects.

We would therefore expect the following ordering of innovation outcomes associated with project selection incentives resulting from varied ownership structures: privately-held would be ahead of the other two exit modes of publicly-held and acquisition with regard to innovation quality. There is evidence in the literature consistent with this ordering, but little or no within-industry evidence taking into account the full spectrum of entrepreneurial liquidity options, while also addressing issues of self-selection into exit mode. There have, however, been a few efforts to order innovation outcomes by ownership structure on a pairwise basis (publicly-held versus privately-held and acquisition versus privately-held) while taking into account possible selection effects. For example, research contemporaneous with our study suggests that firms pursuing an IPO realize a decline in the quality of their innovations, largely due to skilled inventor departures and post-IPO productivity decreases (Bernstein, 2012). However, the same study finds that more entrenched managers experience a smaller decline in innovation productivity. The Bernstein study complements our own by evaluating a multi-industry context, with a focus solely on the IPO mode of exit (and so is unable to assess how acquisitions fit in comparatively). In addition, in a study using the medical device industry as the empirical context, Wu (2012) finds similar post-IPO effects as

Bernstein with respect to innovation quality (a decrease in patent impact following an IPO), but at the same time finds an increase in the quantity of patents after an IPO (in contrast with Bernstein, who finds no effect of the IPO treatment on this same metric). Likewise, for acquisitions, Seru (forthcoming) finds lower patent grants and forward citations following acquisition as compared to exogenously uncompleted acquisitions, especially for firms with active internal capital markets.[3] Of course, studies comparing only one liquidity mode to private ownership cannot estimate the relative ordering of expected outcomes among a broader set of alternatives in a causal way, which is our objective in this paper.

Together, these studies point to the likely importance of factors determining within-event heterogeneity, as well as the need to examine multiple dimensions of innovative outcomes – e.g., patent quantity and quality. Overall, according to this first information confidentiality mechanism of project selection, private ownership appears to dominate IPOs and acquisitions with regard to innovation output, though the latter two exit modes are not clearly ordered among themselves in this regard.

Organizational ownership, information disclosure, and innovation. Under private-ownership, details of a product or service innovation can more likely remain hidden from potential competitors. For acquired firms, such information is only disclosed to a small set of outsiders who are evaluating the firm as a suitable acquisition target.[4] By contrast, publicly-held organizations must routinely make public disclosures, which can provide important information to organizational outsiders. Consider the following quotes regarding the biotechnology industry: "The biotech industry is fiercely competitive and disclosure costs are generally high because most companies develop only a few products, and the entrance of a competitor poses a serious survival threat." (Guo et al., 2004: 320). Furthermore, as Tom Perkins, the Kleiner Perkins co-founder and former Genentech Chairman noted in regard to possible disclosure of contractual details with Eli Lilly around the time of the company's IPO (Perkins, 2002), "We figured that our competitors would try to ferret out the details of those contracts. They were literally inked out in the SEC files." Entrepreneurs therefore sacrifice the opportunity to operate "under the radar" with respect to announcing their offerings, in exchange for liquidity and other benefits of a public offering. Nevertheless, the decision to go public likely involves a tradeoff between early liquidity and the risks of information disclosure to product market competitors, as the theoretical models of Bhattacharya & Ritter (1983), Maksimovic & Pichler (2001) and Spiegel & Tookes (2007) suggest.

[3] According to this "dark side" explanation of internal capital markets of conglomerates, however, it would seem that business unit managers (including those acquired) would have incentives to over-represent their innovation potential as measured by innovation quantity, even if doing so may be at the cost of developing higher quality inventions. Seru does not find this effect, though we do in our empirics.

[4] If the acquirer is publicly-held, however, the transaction could receive more scrutiny by antitrust authorities and/or shareholders of the acquiring firm (in which case there would be more information disclosed to a broader audience). We exploit this within-acquisition event heterogeneity in our empirics to sharpen our empirical evidence beyond across exit mode innovation ordering for this information confidentiality mechanism.

In an empirical analysis of U.S. manufacturing firms, Chemmanur et al. (2012) build on these models, finding that product market characteristics can drive firms' choice of exit mode in ways that are consistent with predictions based on the relative degree of expected information confidentiality under alternate ownership structures (private, M&A and public). The Chemmanur et al. study examines a cross-industry sample of manufacturing firms and finds evidence for a greater decrease in total factor productivity (TFP) following an IPO as compared to an acquisition, consistent with the mechanism of information confidentiality. This study provides complementary insights to ours, with our study in the context of entrepreneurial biotechnology firms differing in its emphasis on innovative output, as compared to production and product market characteristics.

A small, related literature is the connection between corporate governance and innovation outcomes. With private-ownership, in addition to innate entrepreneurial preferences or benefits associated with control, less distributed control rights allow entrepreneurs to retain relative autonomy in making decisions in the face of differences of opinion with outsiders (Boot et al., 2006). The net impact of concentrated versus more distributed ownership (as would be the case with public-ownership) on innovation, however, is theoretically ambiguous as it depends on the relative productivity differences associated with more versus less concentrated corporate governance. Typically the corporate board of directors expands in the ramp-up to an IPO (Baker & Gompers, 2003). Unfortunately there is little literature on the direct impact of expanded boards or of tighter corporate governance more generally on innovation. While earlier literature found a negative relationship between anti-takeover provisions and innovation investments (e.g., Meulbroek, et al. 1990), a recent study (O'Connor & Rafferty, 2012) finds no relation between broad measures of corporate governance and innovation levels once simultaneity is taken into account in their empirical models.

Taken together, this second information confidentiality mechanism, focusing on to whom information is disclosed under different ownership structures, predicts acquisitions as middling in innovation performance, with better outcomes than going public and worse outcomes relative to remaining private.

3. Methodology

Overview. Examining the causal implications of alternate exit mode choices requires a methodology that takes into account possible self-selection of firms into particular modes based on unobserved factors. In addition to our aim of drawing causal inferences on the effects of exit mode treatments, we also seek to frame our results in the context of the prior literature. As discussed in the previous section, there are two streams of work related to information confidentiality that are potentially helpful in understanding the potential mechanisms at work: altered project selection, and disclosures to

external parties, both of which operate through managerial channels. While these two mechanisms are conceptually distinct, they yield similar predictions with regard to the relationship between ownership structure and innovation patterns. As a result, we will not be able to untangle the mechanisms empirically, especially since the first mechanism of project selection from a choice set of alternatives is unobservable to us. Nevertheless, the two mechanisms of information confidentiality imply an ordering of innovation outcomes across ownership modes and some empirical patterns within exit mode. Our analyses are accordingly structured to test the empirical salience of the two information confidentiality mechanisms. To the degree that our effects might be alternatively explained solely through inventor-level changes (rather than managerial level effects associated with project selection incentives or direct information disclosure), however, we supplement our firm-level analyses with inventor-level analyses, examining the role of inventor movements and inventor productivity around exit events.

Sample. We sample the universe of VC-funded biotechnology firms founded between 1980 and 2000, identifying these firms using the VentureXpert database. We focus on start-ups receiving venture capital funding because the quality screen of VC involvement (Kortum & Lerner, 2000) offers a desirable dimension of homogeneity among firms in the sample, with liquidity needs arising from the venture capital cycle (Gompers & Lerner, 2004; Inderst & Muller, 2004) creating pressures to pursue exit opportunities. A second desirable dimension of homogeneity is the use of biotechnology as the industry context. The importance of patenting to the appropriation and valuation of innovations is particularly important in biotechnology relative to other sectors (e.g., Levin et al., 1987). A single industry context enables us to obtain relevant measures of the value and importance of innovations, an objective that would be significantly more challenging in a multi-industry setting. We focus on firms founded in the 21-year period between 1980 and 2000 to ensure that our results are generalizable across a range of initial industry conditions, as well as to ensure that we can observe firm outcomes for a sufficiently long period of time post-founding. The sample consists of the 476 U.S.-based firms in the human biotechnology industry (SIC codes 2833-2836) founded during these years.

The primary dataset is structured as an unbalanced firm-year panel, with observations for each firm starting with the year of founding. Since the most recent founding year is 2000, and the data are collected through 2006, we observe each firm for a minimum of seven years, except in cases where the firm is dissolved prior to 2006.[5] Our dataset thus includes observations at the firm-year level for each year in which the firm is in operation, including those years following an exit event (which can be either an IPO or an M&A). We do not, however, include observations for those years after which a firm ceases to exist as a consequence of a dissolution event. Left-censoring is not an issue since we observe firms

[5] The average lifespan of a venture fund during this timeframe is eight to ten years and so VC-backed firms in this industry thus have strong incentives to pursue an exit event within five to seven years post-founding.

beginning with their date of founding. The final observation year of 2006 is chosen in accordance with our use of forward citations as one of our two measures of innovative output (described in more detail below), for which we utilize a 4-year post-application observation window. In addition to the firm-year panel we assemble an inventor-year panel dataset (described in more detail below) to understand the role of individual inventors in influencing our results.

We utilize several archival sources to assemble our datasets. For exit events this includes news article searches from Factiva, combined with data from Thomson One Banker, Zephyr, and SEC filings. For measures of innovation we draw on the IQSS Patent Network database (see Lai et al., 2011 for a description), which incorporates the U.S. Patent and Trademark Office (USPTO) data on all patents applied for since 1975. This allows us to construct patent-based measures of innovation output at the firm-year level, and in addition, to identify unique inventors associated with these patents, thereby enabling the construction of inventor career histories. We also collect data on firms' VC funding histories, strategic alliances, product pipelines, as well as (for post-IPO firms) coverage from stock market analysts. These data draw respectively on the following sources: VentureXpert, Deloitte Recap RDNA, PharmaProjects and Inteleos, and I/B/E/S. Finally, to construct an instrument for the level of "heat" in the IPO market relative to the M&A market, we collect data on IPO and M&A market volume from multiple sources, including Jay Ritter's IPO data website[6] and SDC.

Empirical strategy. Our main empirical strategy employs the "coarsened exact matching" (CEM) procedure (Iacus et al., 2011, 2012) to construct treatment and control samples that are balanced on pre-treatment covariates (discussed in more detail below). We use the matched control group to run, for example, difference-in-differences estimates of the treatment effect of alternate exit modes.[7] We employ two additional empirical strategies on the CEM-matched data to mitigate any additional concerns of bias due to unobserved pre-treatment characteristics: (1) a quasi-experiment based on "near" exit events – those which were started but not completed for exogenous reasons; and (2) an instrument variables strategy to address the possible endogenous selection of IPO vs. M&A liquidity events. To better understand the mechanisms driving our results, we then conduct within-exit mode analyses, along with an analysis at the inventor-year level. We first describe the construction of the various measures in our firm-year dataset, including innovation outcomes, exit events, firm characteristics, and an instrument for the IPO vs. M&A choice. We then discuss the CEM process we employ to generate the matched control samples. We end the section by detailing how we construct our inventor-year dataset.

[6] This data, updated through 2012, uses the methodology in Ibbotson, Sindelar and Ritter (1994), with the most recent version found here: http://bear.warrington.ufl.edu/ritter/ipoisr.htm

[7] Recent examples of studies employing the CEM technique to construct matched control samples include Azoulay et al. (2010) and Singh and Agrawal (2011).

Innovation outcomes. We begin with our measures of innovation, for which we utilize patent data. To identify all patents associated with the firms in our sample we first extract from the IQSS Patent Network database (Lai et al., 2011) all patents applied for between 1975 and 2010 where the "assignee" name matches our focal firms' current or former name(s). To ensure that we are comprehensive in our data collection process we conduct the search using an algorithm that matches various permutations of the company name (e.g., we would code patents from "Amgen" and "Amgen Inc." as being associated with the same firm). The patent numbers we collect for our focal firms enable us to collect a range of other patent-based characteristics including forward citations and patent classes. Our sample of 476 firms includes 15,439 patents and 45,789 forward citations associated with these patents.

Identifying patents for firms undergoing an M&A exit raises the issue that post-M&A patent applications associated with inventions of the acquired firm may be made with the acquirer listed as the assignee. As a consequence, it may be difficult to track the innovation outcomes of firms after an acquisition, unless the acquired firm operates as an independent entity, with future patents accruing to the subsidiary rather than to the parent. We use an inventor-matching algorithm to address this issue. We first assemble a database of inventors associated with pre-acquisition patents applied for by the focal (acquired) firm. We then search patent applications where the acquirer is the assignee during the post-acquisition period, and consider patents from this set of inventors as having originated from the acquired firm. Thus, the list of patents for a focal firm in our sample undergoing an M&A includes those patents associated directly with the acquired firm before and after the acquisition, as well the subset of the acquiring firm's patents that were invented by the acquired entity (i.e., the focal firm) after the acquisition.[8]

We utilize two measures of patent-based innovation output: patent applications and forward citations. These two characteristics of firm-level output proxy for the quantity and quality of innovation, respectively. Prior work (Trajtenberg, 1990) suggests, moreover, that forward citations in particular have a strong correlation with economic value. We define the firm-year variables *patent applications stock* as the number of patent applications applied for by the firm up to and including the firm-year, and *forward patent citations 4 years stock* as the number of patent citations within a four-year post-issue window to patents applied for (and subsequently granted) by the focal firm up to and including the firm-year.[9,10] We measure both through 2006 (the forward citations window constraints our final observation year).

[8] We verify that all of our results are robust to excluding all acquirers who assign any post-M&A patents to the corporate parent or other entity. We thank an anonymous reviewer for suggesting this robustness test.

[9] We also examine the robustness of our results to using our forward citation measure less self-citations (the two versions of the variable are pairwise correlated at 92%). Removing self-citations strengthens the results, and so we report the more conservative full forward citations in our empirical tables.

Exit events. We observe variation in the modes by which entrepreneurs and their stakeholders achieve exit. From the time of founding, each firm can undergo multiple exit or "near-exit" events (those for which the process was begun, but never consummated). For M&A events we are concerned specifically with situations in which the focal firm is the target in the acquisition (thereby creating a liquidity event for the founders and investors). We conduct an exhaustive archival search using news articles from Factiva, triangulated with Thomson One Banker, Zephyr, and SEC filings, to identify realized exit events for our focal firms (from founding through 2006). We utilize in our specifications a set of indicator variables for sub-samples of firms that underwent an IPO or M&A, as well as indicator variables for the 3-year period of time following the IPO or M&A. These latter variables, *focal, post-IPO (1,3)* and *focal, post-M&A (1,3),* allow us to obtain difference-in-differences estimates of the IPO and M&A effects on our CEM matched sample, as we discuss in detail in Section 4.[11]

In addition to identifying realized exit events from our archival data search, we also identify those exit events that were "withdrawn" in the sense that the exit process started but was never taken to completion. For IPOs, a withdrawn event represents situations in which the firm filed for an IPO but subsequently did not go public due to exogenous market conditions. Withdrawn M&A events represent similar situations in which a deal was announced but never consummated. These two sets of events enable us to conduct a quasi-experiment to identify the treatment effect of exits (IPO or M&A) using sub-samples that pool realized-exit and near-exit events (IPO/near-IPO in one case and M&A/near-M&A in the other). An assumption of this approach is that a firm's withdrawal from a previously planned exit event is uncorrelated with its innovation capacity and with other firm-level characteristics. For withdrawn IPO events we verify through news articles that the withdrawal is a function of unstable or volatile market conditions, factors exogenous to our model specifications. For withdrawn M&A events we similarly verify that withdrawals are due to shareholder objections or to regulatory oversight.[12] Furthermore, we regress the likelihood of (IPO or M&A) withdrawal on our innovation variables and our full set of (time-lagged) firm characteristics (described later), and find all effects to be insignificant. This increases our confidence that exit withdrawals are not systematically related to either innovation or firm characteristics.

Our firm-year dataset is structured to account for the fact that a firm can undergo multiple "near" and "realized" exit events throughout its lifetime. We code the full history of such events, and can therefore observe situations where, for example, the firm experiences a withdrawn IPO or M&A event,

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[10] We use the stock versions of these variables as we believe these have a more natural interpretation given our difference-in-differences approach with firm fixed effects and firm age controls (as discussed later); our results are, however, robust to alternatively using the flow measures.

[11] We also researched the incidence of publicly-held firms being taken private (as in the Lerner, et al. 2011 study). Among our sample companies, we did not find a single such case.

[12] Although we were able to confirm that the reasons for M&A withdrawal were externally-driven, we cannot determine whether the decision to withdraw came from the target or from the acquirer.

and subsequently exits via one of these modes. Similarly, we can observe situations in which one mode of exit (e.g., an IPO) is followed by another (an M&A). One additional category of firm-level outcomes is the complete dissolution, or liquidation, of a firm in our sample. Such situations differ importantly from our two modes of exit (IPO and M&A) in that the firm ceases to exist as a going concern and can thus no longer continue its innovation output. For firms that are dissolved, we use the year of dissolution as the final observation year for the firm in our firm-year panel dataset.

In addition to the indicator variables for different exit modes and the 3-year post-exit windows, we utilize two additional exit event-related measures that are specific to the sub-sample of acquired firms. First, we create an indicator variable for whether the acquiring entity is private (the *private* dummy). Second, following Jaffe (1986), we define *technology overlap* as the angular separation between the primary U.S. patent class vectors of the acquiring and acquired (focal) firms. Each vector has a dimension of 987, and is indexed by unique patent classes; a given value within a vector represents the proportion of the firm's stock of patents (applied for prior to and until the date of acquisition) assigned to the patent class associated with the index for that value. The *technology overlap* measure is the angular dot product of the two vectors: a value of 1 represents vectors with perfect overlap, while a value of 0 represents orthogonal patent class vectors. We interact both the *private* dummy and the *technology overlap* measure with the *focal, post-M&A (1,3)* indicator variable to examine the role of particular organizational mechanisms in influencing innovation output within the M&A mode of exit.

Firm characteristics. We employ a set of firm-level controls to account for any residual time-varying unobserved heterogeneity in our models (we utilize firm fixed effects in most specifications). To account for firm-level quality and life cycle considerations we use *firm age*, which is the age of the firm since founding, along with *VC inflows stock*, which measures the cumulative amount of VC funding received by the firm through the current firm-year (collected using VentureXpert). In addition, we use the Deloitte Recap RDNA database to collect data on the cumulative stock of strategic alliances a firm has entered up to the current firm-year, *strategic alliance stock*, a further measure of firm quality (e.g., Stuart, Hoang & Hybels, 1999).[13]

In addition to age, VC funding and strategic alliances, we use a firm's product portfolio as a final firm-level characteristic. In the empirical context of biotechnology, a relevant metric for product development is the stage of an individual drug compound in the FDA approval process. To construct our two product-related measures, we utilize the Inteleos and PharmaProjects databases to compile the number of products each firm has at different stages of development in a given firm-year. We track the trajectory of an individual drug compound over time by combining Inteleos, for which we have data for

[13] Firms' strategic alliance stock is correlated with VC inflows stock at the 66% level, and so in the empirical tables, we only use the latter variable, though the results are robust to using the former variable instead.

years 1990-2001, and PharmaProjects (which we use to collect 2002-2006 data, matching these with drug compounds identified in Inteleos).[14] We measure the number of products in a given firm-year at four stages of the FDA approval process: pre-clinical, stage 1, stage 2 and stage 3. Our measure of early-stage innovations, *preclinical products*, enables us to test the conditions under which information disclosure may be most significant. In addition, as an aggregate measure of a firm's product portfolio value in a given firm-year (which we use as a control variables), we construct a measure, *weighted products*, which weights the number of products based on their stage, putting arbitrary values of 1, 2, 5, and 10, respectively, on the four development stages, reflecting the relative degree of economic value of the firm's portfolio based on the likelihood of eventual product commercialization (our results are similar with un-weighted counts of firm product portfolios).

Finally, for the sub-sample of firms that undergo an IPO, we collect from I/B/E/S a measure of stock market analyst coverage, *analyst reports*, which measures the total number of analyst reports published about the firm in the firm-year. Prior studies have discussed the role that analysts play in influencing both information availability and incentive structures, which can influence innovation (Chemmanur, et al., 2012; Ferreira, et al., forthcoming; He & Tian, 2013). We thus use this variable, which measures the degree of scrutiny on the firm by outside parties, to examine the information confidentiality mechanism in our sample of firms that have gone public.

Instrumental variable. As discussed previously, one component of our strategy for addressing the possibility of unobserved self-selection into exit mode involves instrumenting for the endogenous selection between the IPO and M&A modes of exit. We utilize as our instrument the relative level of "heat" in one market as compared to the other within the biotechnology industry. Prior literature has typically used volume-based measures of IPO market heat. Yung, et al. (2008), for example, define market heat in two ways: first, by comparing the four-quarter moving average to the historical quarterly volume; and second, by examining IPO market underpricing relative to the historical average. While other studies of IPO market heat utilize variants of this approach, the commonality is using volume-based measures (e.g., Helwege and Liang, 2004). For M&As, "merger waves" are an analogous concept to "hot markets" in IPOs (e.g., Harford, 2005), and in this case transaction volume is similarly used as the key metric. We thus focus on volume in the IPO and M&A markets as this offers an approach to measuring market heat that is common to both markets. We build on the methodology used in Yung et al. (2008) to develop our metric for relative IPO market attractiveness. Using IPO volume data from Jay Ritter's website, and M&A volume data from SDC, we first identify the number of quarters in each firm-year

[14] We compile product pipeline data only for firms founded post-1989 due to time period coverage limitations associated with these two data sources. However, since our unit of analysis is an individual drug compound as it moves through the FDA approval process, we are able to track product portfolios post-M&A as well.

where the four-quarter moving average of biotechnology IPO (or M&A) volume is 25% above the quarterly average from the prior five years. We then construct a measure of IPO relative to M&A market heat (*IPO vs. M&A biotechnology industry liquidity*) by taking the ratio of the IPO measure to the M&A measure. When we discuss our results using this instrumental variable (IV) in the next section, we will also discuss how the IV is both correlated with the possibly endogenous variable but satisfies the exclusion restriction by being unrelated to firm-level innovation outcomes.

Coarsened exact matching (CEM) procedure. Table 1 summarizes the definitions and descriptive statistics of the measures used in our analyses. In Table 2 we show that the CEM procedure helps balance the pre-event sub-samples, which we use as the basis for our main difference-in-differences specifications. As Iacus, et al. (2011) note, CEM is part of a general class of methods termed "monotonic imbalance bounding" (MIB), which has beneficial statistical properties as compared to prior "equal percent bias reducing" (EPRB) models (Rubin, 1976), of which propensity score matching and Mahalanobis distance are examples.[15] MIB generalizes the EPRB class, eliminating many of the assumptions required for unbiased estimates of treatment effects, and outperforming EPRB in most situations, including those specifically designed to meet the EPRB assumptions (Iacus et al., 2011, 2012). A key difference in practice lies in the sequence of data pre-processing: whereas methods such as propensity score matching (PSM) require determining ex ante the size of the matched control sample, then ensuring balance ex post, CEM performs the balancing ex ante (Iacus et al., 2012). CEM entails "coarsening" a set of observed covariates, performing exact matching on the coarsened data, "pruning" observations so that strata have at least one treatment and one control unit, then running estimations using the original (but pruned) uncoarsened data (Blackwell et al., 2009).

A key goal of any matching process is to ensure that the treated and control groups are "balanced" in the sense that their covariates have (approximately) equal distributional characteristics. Table 2 shows the outcome of our application of the CEM process. We focus on four key pre-treatment observables, *age*, *VC inflow stock*, *strategic alliance stock*, and *weighted products*, creating separate treatment and control samples post-CEM for the IPO and M&A treatments. The four variables we use to balance the treatment and control samples represent observable quality dimensions that we expect would be correlated with the IPO and M&A treatments. As the "pre-CEM" column shows, the IPO and M&A treatment and control sub-samples are significantly different (at the 5% level) across the board for the full set of covariates. These differences are reduced, however, post-CEM, with none of the treatment-control differences significant at higher than 5%, suggesting balance in the two sets of samples.

[15] In summarizing a series of analytical and numerical tests of the CEM method, Iacus, et al. (2011, p. 359) note: "[CEM] … generates matching solutions that are better balanced and estimates of the causal quantity of interest that have lower root mean square error than methods under the older existing class, such as based on propensity scores, Mahalanobis distance, nearest neighbors, and optimal matching."

[Insert Tables 1 & 2 here]

Inventor-year dataset. Finally, while our primary aim is to empirically assess the role of information confidentiality in the relationship between entrepreneurial exits and innovation, an alternative to such mechanisms based in human resource turnover might instead be a primary driver of innovation patterns. For example, Stuart and Sorenson (2003) suggest that IPO and M&A liquidity events are organizationally disruptive for the focal enterprise, and link the geographic distribution of new firm foundings to the regional pattern of such entrepreneurial liquidity events. They find support for an employee spinoff mechanism behind the empirical pattern. More generally, in acquisitions, there may be personnel adjustment costs that can result from changes in corporate culture and/or from turnover in personnel composition. Similarly for employees holding stock options, IPOs could loosen the bonds of employment for personnel not subject to lock-up restrictions. We therefore wish to assess the degree to which our firm-year results are wholly explained by inventor-level turnover. If they are, the information confidentiality mechanisms, which operate at the managerial policy level rather than at the inventor level, may be less important in explaining the firm-level empirical patterns. We therefore construct an inventor-year dataset by identifying all inventors associated with patents of our focal firm sample, and constructing full inventor histories for each of these individuals.

These inventor histories include patenting activities both within and outside our focal firms,[16] with the resulting inventor-year dataset consisting of 12,769 inventors associated with 15,439 focal firm patents, each observed on average for 11.3 years (the total number of patents within and outside the focal firm associated with these inventors is 57,803). We define the variables *change in* (mean: 0.46; s.d.: 0.50) and *change out* (mean: 0.02; s.d.: 0.15) as indicators for whether a given inventor either joined or departed a focal firm in a given year. For inventors joining a focal firm in our sample, we set the variable *change in* to equal 1 in the first year in which the inventor applies for a patent in the focal firm. A departure, captured by *change out*, is identified when an inventor who has patented in one of our focal firms is observed to subsequently patent outside this same focal firm. This variable is equal to 1 in the year the inventor patents in the "new" firm. We additionally define the variable *years since first invention* at the inventor-year level to reflect the length of the inventor's career to date. Finally, we create patent outcome measures similar to the firm-year measures discussed previously (*patent applications stock* and *forward patent citations 4 years stock*), except that these are specific to the inventor and defined at the inventor-year level.

[16] We track inventor histories starting from 1975 to ensure that we capture a sufficient window of history for inventors prior to their joining the focal firm.

4. Empirical Results

Post-event versus pre-event comparisons. We begin our analysis in Table 3 with a simple regression analysis of the innovation patterns for firms that experienced an IPO or an acquisition, comparing the post- as compared to the pre-event innovation profiles. This analysis does not confine the sample to observations matched via CEM, as we initially want to describe the innovation patterns comparing post- versus pre-events for the sample of firms undergoing each event. In subsequent analyses, we will adopt methods to address possible selection issues associated with firms of different characteristics choosing liquidity modes. We examine two innovation outcomes throughout our empirics, *patent applications stock* and *forward patent citations 4 years stock*, with the former measure corresponding to innovation quantity and the latter a proxy for innovation quality. We take the log value of these outcome variables and run firm fixed effects OLS regressions on our firm-year sample. Negative binomial count models (of unlogged outcomes) yield similar estimates for the specifications that converge in estimation. For the sake of consistency throughout the tables, we report OLS results.

We first compare the innovation profiles of the 202 firms in our sample undergoing an IPO in the first four columns of Table 3. The first two columns report the effect of being in the post-IPO period, with the first column including no controls beyond the firm fixed effects and the second adding to the model a variety of (logged) time-varying firm controls: *age*, *VC inflows stock*, and *weighted products*. *VC inflows stock* proxies for differential firm resource inputs, while *age* and *weighted products* aim to control for possible innovation rate differences across the firm and product life cycle. Chemmanur, et al. (2010), for example, find that IPOs occur at the peak of firms' productivity cycle. The key independent variable, *focal, post-event (1,3)* variable is negative and significant in both specifications, with the estimate in (3-2) suggesting a 36 percent decline in patent applications in the three years post-IPO. The analogous specifications for the forward patent citation outcome are contained in the next two columns of Table 3. The only difference is that we normalize these forward patent citations regressions by including the log of *patent applications stock* as a regressor (a structure we adopt throughout our empirical specifications when we analyze this outcome variable). Dropping this normalization does not alter the statistical significance of the estimates, though the independent variable of interest is typically estimated with a larger coefficient. The key independent variable, *focal, post-event (1,3)* variable is positive but only significantly so in specification (3-4), with the estimate suggesting a five percent increase in forward patent citations stock within four years of patent application in the three years post-IPO.

The final four columns of the table report analogous specifications for the 180 firms undergoing an M&A, comparing post- with pre-M&A innovation rates. With the full slate of controls, we find that the post-M&A (1,3) window is associated with a 22 percent increase in patent applications and a seven percent decrease in forward patent citations (both estimates are statistically significant at the one percent

level). These estimates have not taken into consideration the possible self-selection into exit mode based on unobservables, however. We therefore employ several strategies including CEM matching, an instrumental variables analysis, and a comparison of actual versus "near" liquidity events to better understand the relationship between exit modes and innovation patterns.

[Insert Table 3 here]

Coarsened exact matching (CEM) estimates. In Table 4, we use the CEM technique, balanced on the log values of *age*, *VC inflows stock*, *alliance stock*, and *weighted products* to define an IPO treatment and control sample (we omit the alliance stock variable as a regressor in our models because it significantly reduces our sample size and because it is significantly correlated with our VC inflows variable). We also test the robustness of our results to using CEM matching on pre-event stocks (as of the year prior to the event) of the dependent variables. We find that our key results hold, though we report our results without matching on the stocks of pre-event outcomes, as they are more conservative. The first three columns of Table 4 examine the outcome variable log *patent applications stock*. Each OLS specification contains our full set of firm controls, event year fixed effects, and firm fixed effects. The specifications differ on the sample analyzed. We start with the entire CEM-balanced sample employing 328 firms. The difference-in-differences estimate, *focal, post-IPO (1,3)*, after controlling for the *focal IPO sample*, is negative and significant, with an implied 40 percent drop in patent applications post-IPO (the comparison group is therefore firms which were either private or experienced an M&A). The next two columns restrict the sample successively by first removing firms which remained privately-held over the duration of the study window (reducing the sample size to 200 firms and 1,872 firm-years, with the comparison group as firms undergoing an M&A) and then examining just the subsample of firms experiencing both an IPO and an M&A (yielding 79 firms and 817 firm-year observations). In both cases, *focal, post-IPO (1,3)* is negative and significant at the one percent level, though the estimated effect drops to 35 and 28 percent, respectively. These estimates are in line with the estimates produced from the simple post- versus pre-IPO analysis of Table 3. We also note that the CEM balancing procedure seems successful, as the coefficient on the focal event sample in these and subsequent specifications is not different than zero, suggesting no pre-event differences in trends in the comparison groups. A final note is that in (4-3), since the sample contains firms undergoing both liquidity events (almost always in the order of IPO followed by M&A), we can also estimate a *focal, post-M&A (1,3)* variable. That estimated coefficient is not different than zero.

The final three columns of Table 4 examine the forward patent citations outcome, following a parallel model structure and subsample comparison as the first group of analyses in this table. Here, we find a reversal of the empirical patterns produced by a simple post- versus pre-IPO comparison. Recall that in that analysis, we found a positive and significant effect of citations post-IPO. Using the CEM-

balancing procedure, we instead find a negative and significant effect at the one percent level across the various samples. Using the entire sample, we find a 19 percent drop. Under the logic that firms remaining private for the entire study period may be qualitatively different (in unobservables) compared to firms achieving liquidity, and so should be left aside in the analysis, we estimate a 15 percent drop in forward citations. Finally, restricting the sample to firms undergoing both events produces a 24 percent estimated decline in forward citations post-IPO as compared to a 12 percent (and statistically significant) decline post-M&A (the two coefficients are statistically different from each other). Therefore using a CEM-balanced sample of IPO treatment versus control, we find that IPOs are associated with both worse innovation quantity and quality.

In Table 5, we report a similar table but for M&A treatment and control samples using CEM balancing. We follow an analogous structure as in Table 4 with regard to model specification and sample comparisons. For patent applications, our results are similar to what we find in the post- versus pre-M&A sample: a positive and significant effect.[17] However, across the range of samples used in this table, our estimated effects here are 25 to 50 percent of the economic size of the prior analysis, which did not account for selection. On the other hand, our analysis of forward patent citations yields both similar statistical and economic significance as the simple post versus pre-M&A analysis: a negative and significant decline in forward patent citations. In addition, the negative and significant effect of the post-IPO window for (5-3) and (5-6) associated with patent applications and forward citations, respectively, is consistent with the results from Table 4 (the former coefficient is statistically different and of opposite sign than the *focal, post-M&A (1,3)* coefficient in the same specification; the latter coefficient is statistically lower than its corresponding *focal, post-M&A (1,3)* coefficient). Finally, note that the focal M&A sample dummy is also not statistically different than zero in all the specifications in Table 5, again implying a successful CEM-balancing procedure.

[Insert Tables 4 & 5 here]

Endogenous choice of IPO versus M&A. One concern with the CEM-balanced estimates presented in the prior two tables is that the matching procedure is only as good as the observables upon which we could possibly balance the treated and control samples. As a result, there could still be

[17] At the suggestion of an anonymous reviewer, we investigate the importance of an alternative "window dressing" mechanism in the pre-event period in which the focal firm files many patent applications to attract the relevant audience. This alternative holds only for patent applications rather than forward patent citations, as only the former is contemporaneously observed by the audience (we also checked for any pre-event spikes in forward patent citations – we did not find any). Recall that our findings on patent applications are declines in the time window following IPO but an increase in the time window following the average M&A. Therefore this alternative explanation only applies to our post-IPO patent application results. To evaluate it, we include dummies for various time window dummies prior to the event year (in addition to the post-IPO time dummy – and so the interpretation of the time window variables is relative to the event year). We find either no pre-event spike for a majority of the pre-event windows, or for a few of the windows, a small (relative to the post-IPO window) and *negative* coefficient. We therefore conclude that pre-event window dressing is unlikely to explain the empirical patterns.

unobserved selection issues associated with those estimates. We therefore employ two additional empirical strategies to estimate our effects, both of which use CEM matching as the first step to sample construction. In our first strategy, we conduct extensive research into the firms within our original sample that nearly completed a liquidity event, but for reasons unrelated to innovation did not complete the event. We compare actual versus "near" IPO events, post-CEM matching, in the first two columns of Table 6 for both of our outcome variables (in unreported analyses, we find that the balance between the treatment and control samples for the actual versus near IPOs and M&As reported in Table 2 is maintained). We include in each specification our full set of time-varying firm controls and report firm fixed effects OLS models. Our results are consistent with the CEM analyses in Table 4, in which we find negative and significant difference-in-differences post-IPO time window effects for patent quantity and quality. In the third and fourth columns of Table 6, we conduct an analogous examination using actual versus near acquisitions. Again, our results echo our findings from Table 5, with a positive and significant post-M&A window effect on patent applications, but a negative and significant coefficient for the same window on forward patent citations. For both pairs of actual versus near event analyses, we regressed the likelihood of withdrawal on our innovation variables and our full set of firm characteristics (with time lags) and found all regressors insignificant (available on request from the authors). This lends support to our quasi-experimental strategy in that withdrawn events are not systematically related to innovation or firm characteristics in a regression framework.

Our second empirical strategy to address selection of liquidity mode based on unobservables adds an instrumental variables strategy to an IPO-treatment CEM-balanced sample. For this analysis, we confine the sample to firms experiencing either an IPO or M&A liquidity event and instrument for the potentially endogenous variable, *IPO year indicator*. We do so by constructing a variable, *IPO vs. M&A biotechnology industry liquidity*. As noted above, this variable is defined at the biotechnology industry level and is a measure of the comparative deal volume of each liquidity mode over a rolling time window. The higher the value of *IPO vs. M&A biotechnology industry liquidity*, the "hotter" is the IPO market relative to the M&A market for biotechnology transactions. As a result, all else equal, the higher the instrumental variable (IV) the more likely a given firm will choose an IPO as a result of the comparative "money chasing deals" IPO environment. This logic is borne out when we regress *IPO year indicator* on *IPO vs. M&A biotechnology industry liquidity* and our slate of firm controls. The resulting coefficient is positive and statistically significant at the one percent level. This is the first stage regression in both specifications (6-5) and (6-6) in which we run two stage least squares (2SLS) regressions. The F-statistic for our first stage regression is 22.7, strongly suggesting that our IV is not weak. Durbin and Wu-Hausman tests (with values of 19 and 15) reject the null hypothesis that *IPO year indicator* is exogenous.

In addition, the requirement that the IV is uncorrelated with firm innovation outcomes is likely satisfied in our case. The IV is a measure of industry-level relative liquidity, while our ultimate outcome variables are at the firm level. Furthermore, the IV is a measure of relative liquidity of exit mode rather than a measure of differences in factor inputs that might be correlated with firm-level innovation outcomes. Finally, it is not only notoriously difficult to predict the degree to which a financing channel will be "hot" (e.g., Lowry, 2003), but also the relative degree to which one market will be more active than another. This suggests that it will be very difficult or not possible for entrepreneurs with (possibly unobserved) innovation expectations to correctly anticipate relatively "hot" financing modes. While our instrumental variable allows us to meet the order condition for identification, there is no direct statistical test of the exclusion restriction. Using this empirical framework, our results on innovation quantity and quality are consistent with the estimates we obtained from using CEM-matching alone (Table 4) and CEM-matching coupled with actual versus near IPOs (first two columns of Table 6). Furthermore, the 2SLS results are robust to omitting the CEM-balancing scheme (which has the effect of nearly tripling the number of usable firm-year observations).

The results thus far are consistent with the information confidentiality mechanism in that innovation outcomes are worse post-IPO relative to post-M&A, and seem to be best under private ownership. This pattern holds for patent applications (comparing (6-1) to (6-3) and (6-5)) and for forward patent citations (comparing (6-2) to (6-4) and (6-6)), even after addressing the role of possible self-selection into liquidity mode. Information confidentiality is best preserved under private ownership and is partially compromised under an acquisition (information is spread to the acquirer or candidate acquirers). IPOs represent the structure with the most information revelation to the most number of outsiders among the ownership structures, consistent with the predictions of the information confidentiality mechanisms. We now examine situations *within* liquidity mode in which the information confidentiality effects are likely to be more or less severe to provide another dimension of empirical evidence for this mechanism as it might connect to firm-level innovation outcomes.

[Insert Table 6 here]

Within-event heterogeneity. We begin by examining heterogeneous within-IPO effects. While all IPOs in the U.S. necessitate regulatory compliance with the Securities and Exchange Commission with regard to information disclosure, we believe that the negative effect of information confidentiality on innovation outcomes may be most salient under two concurrent conditions: namely, when the focal biotechnology firm has many early-stage projects (as proxied by the number of preclinical products), and at the same time the firm itself receives considerable scrutiny (leading to increased information flows to outsiders) by stock analysts. Stock analysts therefore work in the opposite direction as information confidentiality, exposing information and firm analysis to the outside. In the first two columns of Table 7,

we analyze the interaction effect of log *analyst reports* and log *preclinical products* on our two innovation outcomes. While we do not find a significant patent applications effect, we do find a negative and statistically significant effect of this interaction on our measure of innovation quality. While the direct effect of analyst coverage is positive on innovation, the interaction effect suggests that for a given level of preclinical products, the marginal impact of increasing analyst attention as measured by analyst reports by one standard deviation results in a decrease of 2.2 percent in forward patent citations. This effect is consistent with other research highlighting the risks of information disclosure in the early stages of the biotechnology product development process: "At an early stage in the product development cycle, the firm's lead time over potential competitors is short, and managers may accordingly view the risk of adverse competitor action as high and therefore be reluctant to disclose extensive proprietary information." (Guo, et al., 2004: 326).

To probe the within-IPO sample for possible evidence of organizational governance effects, we collected information on whether the executive officers (including the chief executive officer) of the firm at the time of IPO were also founders of the firm. While there could be varied reasons for observing such instances, we examine whether there are consequences for innovation depending on such executive officer status. On the one hand, we might conjecture incentive alignment because founders typically possess a large share of equity, even at the time of IPO. On the other hand, the literature has reported founder control tendencies (e.g., Boot et al., 2006; Schwienbacher, 2008), and so the net effect is theoretically ambiguous. We define two variables to capture the phenomenon: (1) an indicator variable for whether the CEO at the time of IPO is also a founder, and (2) the percentage of executive officers at the time of IPO who were founders. In both cases (when interacted with the post-IPO time window), we find no significant effect on forward patent citations, though we do find a significant positive effect using the CEO variable on patent applications (results available on request).

Similarly, we examine heterogeneity within the M&A sample with an eye to testing the information confidentiality mechanism. First, we conjecture that there might be differential information disclosure effects associated with acquisition by a public versus private acquirer. Since such an acquisition happens only once, to estimate the effect, we interact an indicator for *private acquirer* with the key difference-in-differences variable *focal, post-M&A (1,3)* in our OLS panel firm fixed effects framework. While we do not find an effect of this interaction on patent applications, the effect is positive and significant for forward patent citations. This suggests that relative to the post-M&A window of public acquirers, biotechnology targets acquired by private entities receive a nearly eight percent boost in innovation quality. Naturally, private acquirers retain more information confidentiality relative to public acquirers.

Taken together, these two empirical patterns of within-event heterogeneity provide additional evidence consistent with the information confidentiality mechanism. With regard to M&As, the Seru (forthcoming) and related theories suggest an additional within-M&A pattern. Recall that this theory relates business unit manager incentives for innovation in the context of a competitive internal capital and labor market of a conglomerate (which the acquired innovator joins in the case of an acquisition). Due to such competition at least in the short run, individual managers may have the incentive to over-represent their unit's innovation prospects. We empirically examine acquisitions that differ in the degree to which such incentives may play out by measuring the degree of technological overlap between acquirer and target. We do so by constructing the *tech overlap* measure at the time of acquisition, which follows the Jaffe (1986) method of comparing patent classifications of the entire portfolio of patents between the acquirer and the acquired firms. High values of *tech overlap* suggest more similar technical alignment between the parties, and this is a situation in which the incentives for business unit managers are more likely to be competitive. We interact *tech overlap* with the *focal, post-M&A (1,3)* variable in the final two columns of Table 7. Consistent with the Seru project selection mechanism on internal incentives, we find that the interaction effect is significantly positively correlated with patent applications but significantly negatively correlated with forward patent citations. This suggests that in such competitive settings, managers are incentivized to display outcomes that are observable in the near term (a 3.7 percent increase in patent applications for a one standard deviation increase in *tech overlap*) while sacrificing quality, which is only apparent over the longer run (a 7.7 percent decrease in forward patent citations for the same *tech overlap* increase).

[Insert Table 7 here]

Event window result robustness. Throughout the analyses thus far, we have mainly employed a one to three year post-event window in assessing our results. In Table 8, we report results that vary this event window. Each cell in the table represents a different regression using all the same non-window right hand side regressors as the specification stated in the third row of the table, with only the estimated coefficient associated with the relevant time window variable reported. For ease of comparison, we repeat the estimates using the (1,3) window under the different estimation strategies. We then show the results of the same models, but replace the (1,3) window with (1,4), (1,5) and (1,10) time windows. The results are quite robust to these alternative time windows, and the longer time windows suggest that the effects we report are not necessarily transitory – but rather are more consistent with a regime shift (as would hold under the information confidentiality mechanisms).

[Insert Table 8 here]

Inventor-level analysis. To examine the extent to which these firm-year patterns are driven by inventor-level effects, we rebuild our entire database at the inventor-year level (rather than the firm-year

level) and construct inventor career histories for the focal inventors who have invented in our focal set of firms. As described in Section 3, we construct the inventor histories backward (to 1975 when the electronic patent records are first available) and forward (to 2006, when the inventor database has been disambiguated) in time.

We explore two sets of outcomes at the inventor level, patent applications and forward patent citations. Note that while these mirror the outcomes we examine at the firm level, the results in this table should be interpreted at the inventor-year level of analysis. We wish to evaluate these outcomes for inventors as they transition into an IPO or M&A ownership regime. In particular, we want to assess three dimensions of inventor-level impact: average inventor productivity within firms in the time window following the liquidity event, average productivity of inventors being hired into firms after the liquidity event, and average productivity of inventors departing the firm after a given liquidity event.

To estimate the first dimension, we examine the *focal, post-event (1,3)* variable, as in our firm-year analysis. This captures the change in inventor innovation productivity for the focal sample in the time window after the event. To estimate the second and third dimensions, we interact *focal, post-event (1,3)* with either *inventor change out* or *inventor change in*, respectively. After preparing the inventor-year data in the manner described in Section 3, we use a CEM algorithm to define a treatment and control sample. We match based on log *years since first invention* (a proxy for inventor age), log *firm age*, and log *VC inflows stock*. In each specification in Table 9, we include firm and event year fixed effects, as well as controls for *firm age*, *VC inflow stock*, and *years since first invention*. In addition, when we analyze the outcome variable log *forward patent citations 4 years stock*, we include an additional regressor as before, log *patent applications stock*. All models are estimated via OLS.

The first four columns of Table 9 examine post-IPO inventor effects by analyzing each of the two innovation outcome variables using two different samples, first the CEM sample defining an IPO treatment versus control sample, and second, an actual versus near IPO sample following the CEM process (the former subsample is "treated"). Across the two estimating techniques, we find a fairly consistent set of results. First, patent applications decline while forward patent citations increase on average for inventors in the firm post-IPO. Second, following an IPO, the inventors departing the firm are the ones underperforming with regard to patent applications (though departing inventors did not differ with regard to forward patent citations). Finally, following an IPO, inventors joining the newly-public firm underperform with respect to both patent applications and patent citations. Taken together, these analyses suggest that the firm-level drop in innovation quantity and quality, when evaluated from the dimension of inventor-level productivity, results both from factors related to changes in innovative productivity of the technical staff within firms undergoing an IPO, and also from the quality of inventors attracted to and departing from the firm in the post-event window. In the case of forward citations post-

IPO, the negative effect of the quality of inventors entering the firm overwhelms the positive productivity effect experienced by the scientist-inventors at the firm. While the net result of the inventor analysis is consistent with Bernstein (2012), that study finds that inventors entering the firm produce higher quality innovations, while the quality of those staying declines. The difference may be partially due to the cross-industry setting used in that analysis. The results of inventor fixed effects models are similar to what we have reported, though because such fixed effects models cannot also accommodate the CEM weighting, we do not formally report those models.

The final four columns of Table 9 reports similar model specifications, again employing the same two sampling strategies, except analyzing post-M&A innovation effects. Here, we find much less with regard to changes in inventor productivity in the time window post-M&A. There is no effect in the post-M&A window of inventor productivity changes (in contrast to Seru's [forthcoming] finding of a drop in post-M&A inventor productivity). Similarly, there is no difference in the quality of inventors departing the firm post-M&A. There is mixed evidence suggesting that inventors hired into the newly-merged company are slightly worse as measured by forward patent citations (but only using the actual versus near-M&A comparison). Taken as a whole, these results suggest that inventor-level effects are not driving the overall firm-level patterns (the same conclusion applies to inventor fixed effects models, which we do not formally report for the same reason as before). As a consequence, there is little support for the alternative explanation that inventor-level turnover as a result of the entrepreneurial exit events (both IPO and M&A) explains the firm-level empirical patterns.

[Insert Table 9 here]

5. Conclusion

We examine the impact of entrepreneurial exit mode on innovation outcomes, as measured by patent quantity and quality. We construct a firm-year panel dataset of all venture capital funded biotechnology firms founded between 1980 and 2000, tracking these firms through the end of 2006, to evaluate the innovation implications of entrepreneurial firms' choice among a menu of alternative exit mode options. Our empirical methods address the challenge of inference based on self-selection effects by controlling for firm-level qualities using Coarsened Exact Matching (CEM) alone, and variously coupled with a quasi-experiment utilizing both exit event and "near-exit" event observations, as well as an instrumental variable approach which instruments for the exit event using the relative "hotness" of different exit mode channels within the biotechnology industry. We find that innovation quality is highest under private ownership and lowest under public ownership, with acquisition intermediate between the two. Within the IPO sample, innovation quality suffers when firms simultaneously have more analyst attention and more preclinical stage products. Within the M&A sample, innovation quality is bolstered by

private (as compared to public) acquirers, and when there is less technology overlap between the acquirer and target. The collection of across and within exit mode results is consistent with information confidentiality mechanisms in which project selection and information disclosure stratified by ownership structure impact firm innovation outcomes. Moreover, these patterns are not driven entirely by inventor-level turnover behavior, nor are they explained by pre-event window dressing behavior.

Three caveats to the analysis are important to note. First, we rely on patent data for our measures of innovation. If private companies are less likely to pursue the patenting channel for invention appropriability relative to public firms, our estimates may be biased. However, the stylized fact is that patents are a primary means of appropriation in the biotechnology industry. Furthermore, Hsu and Ziedonis (2013) report an important patent signaling value to financial input providers for private firms in the semiconductor industry, which is particularly salient in the earliest stages of venture development. Second, we examine only a single industry, biotechnology, and so result generalizability will be left for future research. Finally, we rely on public sources to code M&A withdrawal announcements for our quasi-experimental analysis, and these sources could be incomplete. Nevertheless, we conclude from the collection of evidence that entrepreneurial firms' exit modes affect subsequent innovation outcomes.

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Table 1
Descriptive statistics and variable definitions
(Firm-year unit of analysis)

VARIABLE	DEFINITION	MEAN	STD. DEV.
Dependent variables			
Patent applications stock	Stock of patent applications to firm *i* in year *t*	17.17	57.04
Forward patent citations 4 years stock	Forward patent citations to firm *i*'s stock of patents within 4 years of patents granted in year *t*	55.01	156.45
Independent variables			
Event and time variables			
Focal IPO sample	Dummy = 1 only for all firm-years (pre- and post-event) associated with a firm undergoing an IPO	0.48	0.50
Focal, post-IPO window	Dummy = 1 for the time window 1 to 3 years (inclusive) post the IPO event	0.08	0.27
IPO year indicator	Dummy = 1 only for the year in which a firm undertook an IPO	0.04	0.20
Focal M&A sample	Dummy = 1 only for all firm-years (pre- and post-event) associated with a firm undergoing an M&A	0.40	0.49
Focal, post-M&A window	Dummy = 1 for the time window 1 to 3 years (inclusive) post the M&A event	0.07	0.26
Focal, post-M&A window, private acquirer	Interaction term for the time window 1 to 3 years (inclusive) post the M&A event if acquired by a privately-held entity (indicator variable)	0.04	0.20
Focal, post-M&A window, technology overlap	Interaction term for the time window 1 to 3 years (inclusive) post the M&A event with a normalized angular separation between vectors of primary patent classes of acquired and acquiring firms (see text; formula follows Jaffe (1986))	0.10	0.26
Biotechnology firm characteristics			
Age	Age in years of the focal firm as of year *t*	8.42	6.12
VC inflows stock	Cumulative VC inflows invested in the focal firm to year *t* (in $M)	16.39	27.87
Strategic alliance stock	Cumulative number of strategic alliances the focal firm had entered into as of year *t* as reported by Recap	10.39	17.91
Weighted products§	Aggregate measure of focal firm's product portfolio in year *t* created by weighting the number of products along the FDA approval process: pre-clinical (weighted 1), stage 1 (2), stage 2 (5), and stage 3 (10).	75.54	143.38
Preclinical products§	Number of preclinical products in a firm-year.	1.05	3.39
Analyst reports	For firms going public, number of analyst reports issued on focal firm in year *t*	61.25	128.94
Instrumental variable			
IPO vs. M&A biotechnology industry liquidity	Ratio of number of quarters in a focal year in which the deal volume of IPOs in the biotechnology industry exceeded by 25% the rolling average over the prior 5-year window to the same count for M&As.	0.56	0.63

§ denotes data compiled only for firms founded post-1989.

Table 2
Firm characteristics before and after coarsened exact matching (CEM) procedure

	Pre-CEM		**Post-CEM**	
	IPO sample	*Control sample*	*IPO*	*Control sample*
L Age	2.04 (0.83)	1.90** (0.83)	2.19 (0.60)	2.20 (0.68)
L VC inflow stock	2.10 (1.53)	1.55** (1.40)	2.16 (1.55)	2.00 (1.49)
L strategic alliance stock	2.11 (1.18)	1.10** (1.03)	2.10 (0.79)	2.25 (0.77)
L Weighted products	1.22 (2.15)	0.91** (1.68)	0.59 (1.61)	0.44 (1.40)
	M&A sample	*Control sample*	*M&A*	*Control sample*
L Age	2.00 (0.83)	1.94** (0.83)	2.47 (0.47)	2.50 (0.43)
L VC inflow stock	2.05 (1.48)	1.66** (1.48)	2.44 (1.34)	2.37 (1.33)
L strategic alliance stock	1.81 (1.23)	1.58** (1.21)	2.23 (0.96)	2.25 (0.77)
L Weighted products	1.12 (1.86)	1.02** (1.97)	1.03 (1.76)	0.99 (1.93)

The mean and standard deviation (in parentheses) are reported. The natural logarithm of a variable, X, is denoted L X. ** indicates difference is significant at the 5% or higher level compared to the "treated" sample. The CEM procedure involves matching on the log values of age, VC inflow stock, alliance stock and weighted products.

Table 3
Post- vs. pre-event innovation comparisons (firm-year level of analysis)
OLS regression coefficients reported

	Post- vs. pre-IPO innovation comparisons				*Post- vs. pre-M&A innovation comparisons*			
Dependent variable	*L patent applications stock*		*L forward patent citations 4 years stock*		*L patent applications stock*		*L forward patent citations 4 years stock*	
	(3-1)	**(3-2)**	**(3-3)**	**(3-4)**	**(3-5)**	**(3-6)**	**(3-7)**	**(3-8)**
Focal, post-event (1,3)	-1.069*** (0.054)	-0.361*** (0.032)	0.022 (0.022)	0.049** (0.023)	0.508*** (0.051)	0.223*** (0.032)	-0.066*** (0.020)	-0.073*** (0.021)
Firm-level controls	No	Yes	No	Yes	No	Yes	No	Yes
Event year FE	No	Yes	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	2.339*** (0.021)	-0.308*** (0.034)	0.276*** (0.018)	0.219*** (0.024)	1.759*** (0.021)	-0.320*** (0.033)	0.278*** (0.015)	0.264*** (0.022)
# observations (firms)	3498 (202)	3498 (202)	3498 (202)	3498 (202)	2934 (180)	2934 (180)	2934 (180)	2934 (180)

*, ** or *** indicates statistical significance at 10%, 5%, and 1%. Firm-level controls include *L age*, *L VC inflows stock,* and *L weighted products. L patent applications stock* is also a control for (3-3), (3-4), (3-7) and (3-8). Note: the samples are not CEM matched because the comparisons simply reflect innovation rates post vs. pre-event for the sample of firms undergoing each event.

Table 4
IPO treatment vs. control sample (post-CEM) innovation comparisons (firm-year level of analysis)
OLS regression coefficients reported

Dependent variable	*L patent applications stock*			*L forward patent citations 4 years stock*		
Sample	All	Removing firms remaining private	Firms experiencing both an IPO and M&A	All	Removing firms remaining private	Firms experiencing both an IPO and M&A
	(4-1)	**(4-2)**	**(4-3)**	**(4-4)**	**(4-5)**	**(4-6)**
Focal, post-IPO (1,3)	-0.399*** (0.038)	-0.352*** (0.039)	-0.279*** (0.050)	-0.190*** (0.027)	-0.155*** (0.029)	-0.243*** (0.038)
Focal IPO sample	1.076 (2.257)	-1.391 (1.872)		1.710 (1.588)	0.112 (1.356)	
Focal, post-M&A (1,3)			0.035 (0.055)			-0.119*** (0.041)
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Event year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-0.778 (2.207)	-1.190 (0.540)	-1.762*** (0.585)	0.063 (1.555)	0.066 (1.308)	-0.437 (0.443)
N observations (firms)	2702 (328)	1872 (200)	817 (79)	2702 (328)	1872 (200)	817 (79)

*, ** or *** indicates statistical significance at 10%, 5%, and 1%. Firm-level controls include *L age*, *L VC inflows stock,* and *L weighted products. L patent applications stock* is also a control for 4-4, 4-5, and 4-6 only.

Table 5
M&A treatment vs. control sample (post-CEM) innovation comparisons (firm-year level of analysis)
OLS regression coefficients reported

Dependent variable	*L patent applications stock*			*L forward patent citations 4 years stock*		
Sample	All	Removing firms remaining private	Firms experiencing both a M&A and an IPO	All	Removing firms remaining private	Firms experiencing both a M&A and an IPO
	(5-1)	**(5-2)**	**(5-3)**	**(5-4)**	**(5-5)**	**(5-6)**
Focal, post-M&A (1,3)	0.171*** (0.022)	0.177*** (0.022)	0.106*** (0.027)	-0.037*** (0.013)	-0.036*** (0.014)	-0.078*** (0.018)
Focal M&A sample	0.693 (3.198)	0.522 (3.222)	0.011 (0.189)	1.431 (1.932)	1.430 (1.956)	-1.432 (2.082)
Focal, post-IPO (1,3)			-0.504*** (0.047)			-0.083*** (0.033)
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Event year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-0.290 (2.262)	0.372 (2.280)	-0.188 (2.235)	0.009 (1.366)	0.361 (1.383)	0.153 (1.472)
N observations (firms)	4711 (396)	3681 (260)	1369 (96)	4711 (396)	3681 (260)	1369 (96)

*, ** or *** indicates statistical significance at 10%, 5%, and 1%. Firm-level controls include *L age*, *L VC inflows stock,* and *L weighted products. L patent applications stock* is also a control for 5-4, 5-5, and 5-6 only.

Table 6
Endogenous choice of IPO vs. M&A innovation comparisons (firm-year level of analysis)
"Near" vs. actual events & instrumental variable analyses (Post-CEM)
OLS regression coefficients reported

Estimation method & sample	*OLS analysis of "near" vs. actual IPOs, Post-CEM*		*OLS analysis of "near" vs. actual M&As, Post-CEM*		*2SLS IV analysis on firms undergoing either an IPO or and M&A, Post-CEM balancing (IPO treatment)*	
Dependent Variable	*L patent applications stock*	*L forward patent citations 4 years stock*	*L patent applications stock*	*L forward patent citations 4 years stock*	*L patent applications stock (2SLS)*	*L forward patent citations 4 years stock (2SLS)*
	(6-1)	**(6-2)**	**(6-3)**	**(6-4)**	**(6-5)**	**(6-6)**
Focal, post-IPO (1,3)	-0.320*** (0.041)	-0.151*** (0.030)			-0.409*** (0.058)	-0.150*** (0.043)
Focal, post-M&A (1,3)			0.180*** (0.021)	-0.050** (0.014)		
Focal event sample	-0.429 (0.910)	0.045 (0.457)	0.457 (2.182)	-0.985 (1.449)	-0.145 (1.033)	0.152 (0.751)
IPO year indicator (instrumented)					1.157** (0.527)	-0.069 (0.384)
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Event year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-2.373*** (0.749)	0.807* (0.435)	-0.457*** (0.124)	0.985*** (0.169)	-1.000 (0.700)	0.809 (0.510)
# observations (firms)	1612 (168)	1612 (168)	2154 (175)	2154 (175)	1049 (179)	1049 (179)

*, ** or *** indicates statistical significance at 10%, 5%, and 1%. Firm-level controls include *L age*, *L VC inflows stock,* and *L weighted products. L patent applications stock* is also a control for 6-2, 6-4, and 6-7 only. For (6-5) and (6-6), the first stage logit regression of the endogenous variable, *IPO year indicator*, on the instrumental variable, *IPO vs. M&A biotechnology industry liquidity*, yields a positive and significant coefficient of 0.036 with a standard error of 0.013 ($p < 0.01$). The F statistic of the first stage is 22.7, suggesting that the instrument is not weak.

Table 7
Within-event heterogeneity (firm-year level of analysis, Post-CEM matching)
OLS regression coefficients reported

	Within IPO sample		*Within M&A sample*			
	L patent applications stock	*L forward patent citations 4 years stock*	*L patent applications stock*	*L forward patent citations 4 years stock*	*L patent applications stock*	*L forward patent citations 4 years stock*
	(7-1)	**(7-2)**	**(7-3)**	**(7-4)**	**(7-5)**	**(7-6)**
L analyst reports	0.060*** (0.013)	0.033*** (0.009)				
L preclinical products	-0.134 (0.238)	-0.022 (0.162)				
*L analyst reports * L preclinical products*	0.046 (0.060)	-0.084** (0.041)				
Focal, post-M&A (1,3)			0.191*** (0.024)	-0.062*** (0.016)	0.043 (0.054)	0.137*** (0.039)
Focal, post-M&A (1,3), private acquirer			-0.051 (0.052)	0.075** (0.034)		
Focal, post-M&A (1,3), tech overlap					0.143** (0.075)	-0.298*** (0.054)
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Event year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-0.545 (0.551)	1.956*** (0.375)	0.123 (2.185)	1.410 (1.437)	0.821 (2.060)	0.607 (1.496)
# observations (firms)	891 (129)	891 (129)	2089 (170)	2089 (170)	1593 (119)	1593 (119)

*, ** or *** indicates statistical significance at 10%, 5%, and 1%. Firm-level controls include *L age* and *L VC inflows stocks. L weighted products* is included in specifications 7-3 through 7-6. *L patent applications stock* is also a control for 7-2, 7-4, and 7-6 only.

Table 8
Exit event window robustness regressions (firm-year level of analysis)

Dependent variable	*L patent applications stock*			*L forward patent citations 4 years stock*		
Comparison	CEM, IPO treatment	CEM, M&A treatment	*2SLS IV on IPO or M&A, post-CEM (IPO treatment)*	CEM, IPO treatment	CEM, M&A treatment	*2SLS IV on IPO or M&A, post-CEM (IPO treatment)*
Non-window RHS same as:	(4-1)	(5-1)	(6-6)	(4-4)	(5-4)	(6-7)
	(8-1)	**(8-2)**	**(8-3)**	**(8-4)**	**(8-5)**	**(8-6)**
Focal, post-IPO (1,3)	-0.399*** (0.038)		-0.406*** (0.068)	-0.190*** (0.027)		-0.179*** (0.050)
Focal, post- IPO (1,4)	-0.479*** (0.037)		-0.427*** (0.066)	-0.202*** (0.027)		-0.234*** (0.049)
Focal, post- IPO (1,5)	-0.499*** (0.037)		-0.405*** (0.068)	-0.207*** (0.028)		-0.263*** (0.050)
Focal, post- IPO (1,10)	-0.645*** (0.044)		-0.474*** (0.080)	-0.245*** (0.033)		-0.310*** (0.058)
Focal, post-M&A (1,3)		0.171*** (0.022)			-0.037*** (0.013)	
Focal, post- M&A (1,4)		0.187*** (0.021)			-0.037*** (0.013)	
Focal, post- M&A (1,5)		0.200*** (0.021)			-0.051*** (0.013)	
Focal, post- M&A (1,10)		0.259*** (0.024)			-0.018 (0.015)	

Values are regression coefficients (standard errors). *, ** or *** indicates statistical significance at 10%, 5%, and 1%. Each cell represents a different (full) regression equation with only the focal time window changed relative to the specification listed in the third row in the table.

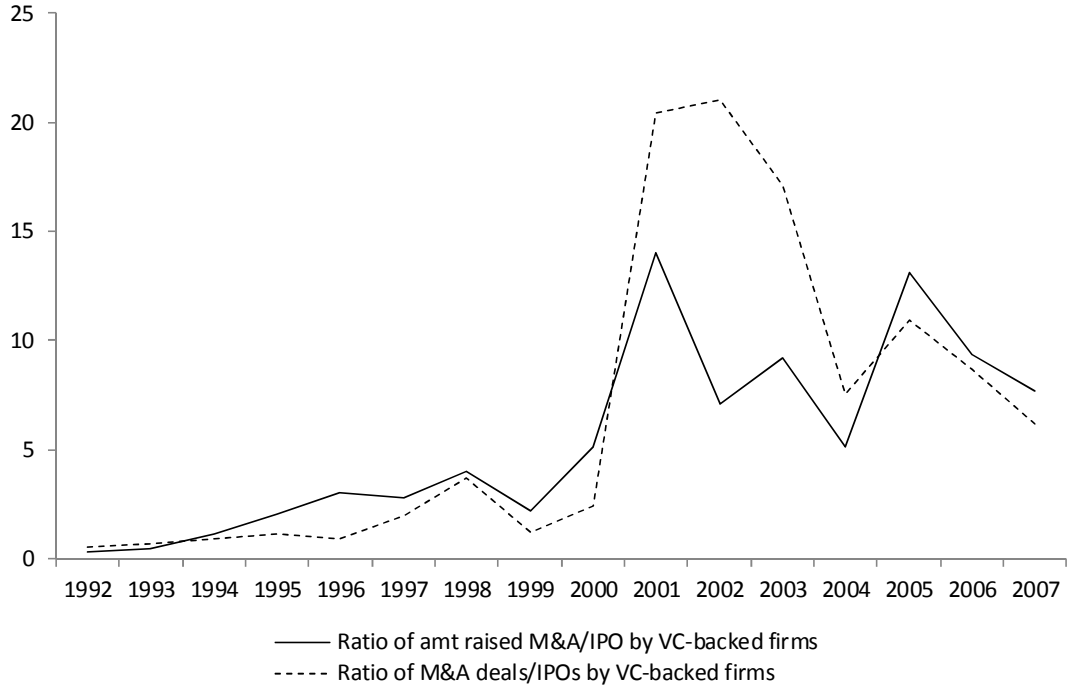
Table 9
Inventor level analyses (inventor-year level of analysis)

	Post-IPO effects				*Post-M&A effects*			
Dependent Variable	*L patent applications stock*		*L forward patent citations 4 years stock*		*L patent applications stock*		*L forward patent citations 4 years stock*	
Sample	CEM, IPO treatment	Actual vs. near IPO, Post CEM	CEM, IPO treatment	Actual vs. near IPO, Post CEM	CEM, M&A treatment	Actual vs. near M&A, Post CEM	CEM, M&A treatment	Actual vs. near M&A, Post CEM
	(9-1)	**(9-2)**	**(9-3)**	**(9-4)**	**(9-5)**	**(9-6)**	**(9-7)**	**(9-8)**
Focal, post-event (1,3)	-0.050***	-0.051***	0.036**	0.030**	-0.002	0.009	-0.003	0.004
	(0.020)	(0.019)	(0.016)	(0.015)	(0.022)	(0.024)	(0.018)	(0.014)
*Focal, post-event (1,3) * inventor change out*	-0.157***	-0.189***	0.010	0.002	0.073	0.072	-0.044	-0.059
	(0.063)	(0.062)	(0.051)	(0.049)	(0.081)	(0.091)	(0.064)	(0.054)
*Focal, post-event (1,3) * inventor change in*	-0.044**	-0.059***	-0.068***	-0.070***	0.041*	0.034	-0.018	-0.036**
	(0.021)	(0.020)	(0.017)	(0.016)	(0.023)	(0.026)	(0.018)	(0.015)
Focal event sample	-0.998	-1.655***	0.770	0.257	-2.225***	-0.498	-4.188***	-1.876***
	(1.010)	(0.443)	(0.805)	(0.351)	(0.758)	(0.312)	(0.600)	(0.184)
Inventor change out	-0.035**	-0.007	0.058***	0.064***	-0.034***	-0.050*	0.019*	0.042**
	(0.016)	(0.019)	(0.013)	(0.015)	(0.014)	(0.029)	(0.011)	(0.017)
Inventor change in	-0.081***	-0.056***	-0.003	-0.001	-0.041***	-0.057**	-0.025***	0.005
	(0.008)	(0.008)	(0.005)	(0.006)	(0.007)	(0.013)	(0.005)	(0.035)
Firm & inventor controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-1.675*	-0.888***	-0.583	0.032	0.721	-0.474***	3.120***	0.659***
	(0.958)	(0.319)	(0.763)	(0.252)	(0.707)	(0.090)	(0.559)	(0.053)
# observations	18,583	10,500	18,583	10,500	10,532	3,692	10,532	3,692

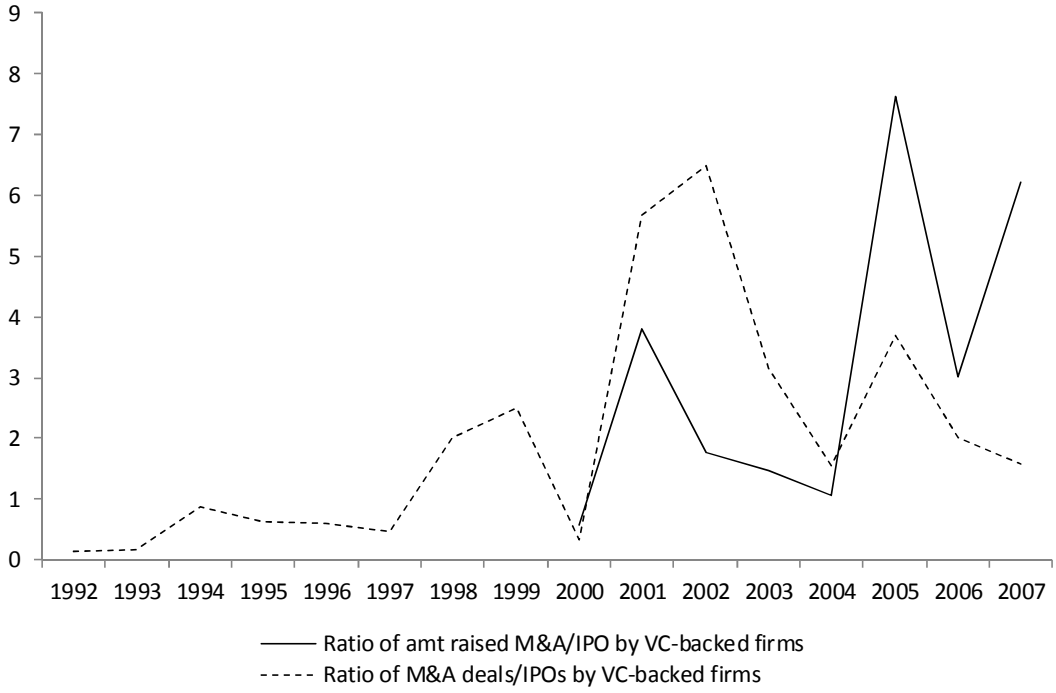
Values are regression coefficients (standard errors). *, ** or *** indicates statistical significance at 10%, 5%, and 1%. Firm-level controls include *L firm age* and *L VC inflows stock*; inventor-level control is *L years since first invention*. *L patent applications stock* is also a control for 9-3, 9-4, 9-7, and 9-8 only. The CEM procedure involves matching on the *L years since first invention*, *L firm age*, and *L VC inflows stock*.

Figure 1

Panel A: Relative intensity of M&A to IPOs in VC-backed start-ups, 1992-2007



Panel B: Relative intensity of M&A to IPOs in VC-backed biotech firms, 1992-2007



Note: data for M&A deal value for Panel B is unavailable for 1992-1999.
Source: DowJones/VentureSource

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