

# The Effect of U.S. Public-to-Private Leveraged Buyouts on Innovation

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## Abstract

With a large sample of public-to-private leveraged buyouts from 1980 to 2006, we find that LBO targets are equally as likely to hold patents as other publicly-traded firms. Using a difference-in-differences approach, we find that LBOs reduce patent flows by one third. This reduction results from a 23% decline in new grants and 7% fewer purchases. Around one fifth of patent-holding LBO targets sell patents immediately after their buyout, liquidating an average of 45% of their portfolio. After an LBO, patents are granted at a reduced rate, receive the same level of citations, and make 16% more citations.

**Keywords:** Leverage buyout, private equity, innovation, patents, R&D

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## **Conflict-of-interest disclosure statement**

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I have nothing to disclose

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# 1 Introduction

At the end of the 1980s, the U.S. private equity (PE) industry narrowly escaped greater oversight and regulation. A wave of leveraged buyouts (LBOs) had attracted the attention of the U.S. Congress. Policymakers were concerned that LBOs were reducing innovation, which is a key determinant of long-term firm performance and a crucial driver of the U.S. economy. Despite supporting evidence that LBOs were indeed associated with declines in research and development (R&D) spending, patenting activity, and technical employment, it was ultimately agreed that leveraged buyout targets were not particularly innovative and so the concerns were moot.

Thirty years later, the U.S. private equity industry has thrived. In 2018, U.S. PE funds raised over \$700 billion, which is roughly equivalent to the annual gross state product of Pennsylvania. PE firms have broadened beyond their original focus of buying publicly-traded firms, taking them private, restructuring them and then later relisting them. Private-to-private, divisional, and even secondary buyouts, have each come to outnumber public-to-private transactions. However, public-to-private leveraged buyouts remain particularly important and controversial.

The number of publicly-traded firms has been in decline since before the dot-com bubble and currently stands at more than 50 year lows. Yet publicly-traded firms still play a disproportionate role in the U.S. economy. The Board of Governors of the Federal Reserve System estimated the value of U.S. public corporate equities at \$31.5 trillion for the 4<sup>th</sup> quarter of 2017, while U.S. private corporate equities were valued at \$5.3 trillion. In this consequential context, private equity firms use leverage to finance the purchase of their targets in pursuit of an equivocal goal: They may provide a crucial market for management that restores efficiency, or they may be engaged in transfer-seeking at the expense of America's long-term prosperity.

Following the ‘patent explosion’ of the 1980s, U.S. publicly-traded firms became increasingly more likely to hold patents, and to hold larger patent portfolios. Technology is now pervasive in essentially all publicly-traded firms, irrespective of their sector. And private equity firms have invested more and more in innovative companies. A leading private equity executive, Carlyle Group Co-CEO Kewsong Lee, recently stated that “every deal is a tech deal”. So if public-to-private buyouts do indeed have a deleterious effect on innovation, the consequences from each transaction are now potentially much greater.

Accordingly, this paper reconsiders whether modern public-to-private LBO targets are less innovative than other publicly-traded firms, and then asks the important question “what effect does a public-to-private leveraged buyout have on a firm’s patenting activities?”

The majority of theoretical arguments, which we discuss in the literature section, suggest that public-to-private leveraged buyouts will result in a decline in patent grants. These arguments also imply that we might see sales of patent portfolios to generate operating capital, as well as a refrain from making new patent purchases and perhaps an abandonment of patents to avoid maintenance fees. Such activities would divert cash away from interest payments and other short-term goals. Only under an entrepreneurial view of private equity might a public-to-private buyout result in additional innovation and hence more patent grants.

The empirical literature also leads one to strongly suspect that public-to-private leveraged buyouts cause reduced innovation. An empirical study in the late 1980s and early 1990s established the stylized fact that increased leverage is associated with reduced R&D expenditures. It is natural to view LBOs as shocks that cause firms to incur extreme levels of leverage. More recent research, including Lerner, Sorensen, and Strömberg (2011), Amess, Stiebale, and Wright (2016) and Cumming, Peter, and Tarsalewska (2018), considers the effects of various private equity transaction types on patenting. One interpretation of this literature is that while private-to-private PE investments increase patent grants, public-to-

private buyouts, which invariably involve leverage, decrease them.

Our sample consists solely of material, innovative U.S. public-to-private buyouts that were leverage financed, and it is much bigger than previous samples of its kind. We find that firms targeted for a LBO are no more or less likely to hold patents than other publicly-traded firms on the New York, Nasdaq, and American stock exchanges. We also find that LBO targets are granted and own significantly more patents than other publicly-traded firms. However, the old claim that private equity firms select targets that have low levels of R&D also still appears true.

We use a difference-in-differences approach to address issues of causality. Ninety-two of the patent-holding LBOs in our sample have sufficient accounting data to identify a propensity score matched control firm. The counter-factual context provided by the control firms is crucial to the interpretation of our findings. For example, consistent with Lerner et al. (2011), we find that public-to-private leveraged buyouts have the same volume of patents before and after an LBO. However, patent portfolios tend to increase over time. Accordingly, we find that relative to a control group, public-to-private LBOs are associated with a statistically significant and economically meaningful contraction in patent stocks.

An important contribution of this research is that it goes beyond an analysis of patent stocks that is based solely on patent grants. We consider how patent purchases, sales, abandonments, and term expirations, as well as patent grants, each affect patent flows. We find evidence that private equity causes firms to sell off patent portfolios and curtail their purchase of patents, but does not affect a firm's propensity to abandon patents. We also find that the rate of patent grants drops materially immediately following an LBO event and continues to decline for the duration of the LBO. As patent applications typically take around two years to process, this would be explained by PE firms abandoning costly patent applications and then not applying for new patents at prior rates. Over the course of an LBO, patent grants drop an average of 23%.

Our last quantitative result is that there is no statistically significant change in R&D expenditure in our sample as a consequence of a public-to-private leveraged buyout. Private equity firms appear to select publicly-traded targets with low levels of R&D expenditure and then maintain those low levels.

We also provide a qualitative analysis of changes to a patent portfolio during a leveraged buyout using patent statistics. Like Lerner et al. (2011), we find that patents held by innovative LBO firms receive more citations after their buyouts. However, we are able to attribute this solely to citation inflation: we find no statistically significant difference in citations received between LBO firm patents and control firm patents before or due to an LBO. We also add some other new qualitative findings to the literature. LBO firm patents make about 16% more citations, and are classified slightly more often as explorative, as an effect of a public-to-private leveraged buyout.

The remainder of this paper is organized as follows. In section 2, we discuss the literature and theory relevant to public-to-private leveraged buyouts and innovation. Section 3 describes our data and innovation measures. Section 4 presents our empirical methodology and results, including descriptive statistics, and Section 5 provides a concluding discussion.

## 2 Literature and Theory

The 1980s saw a wave of leveraged buyouts in the U.S. This wave attracted the attention of policymakers, who were concerned about the effects on innovation of LBOs and other leveraged corporate finance events. Recognizing that innovation plays a crucial role in long-run firm performance, and is a key driver of economic growth, the U.S. Congress requested studies and testimony on the effects of LBOs. Examples of notable studies and hearings from this period include *Corporate Takeovers: Public Policy Implications for the Economy and Corporate Governance: A Report from the Chairman of the Subcommittee on Telecommuni-*

*cations, Consumer Protection and Finance*, Senate, 99<sup>th</sup> Cong. (1986), Brancato and Winch (1987), *Hostile Takeovers: Hearings before the Committee on Banking, Housing, and Urban Affairs*, Senate, 100<sup>th</sup> Cong. (1987), *The Detrimental Effect of Hostile Takeover Attempts on U.S Industrial Research & Development Programs and Technical Manpower*, U.S. House of Representatives, 100<sup>th</sup> Cong. (1987), and *Corporate Restructuring and R&D: Hearing before the Subcommittee on Science, Research and Technology of the Committee on Science, Space and Technology*, U.S. House of Representatives, 101st Cong. U.S. Government Printing Office 66-529 O (1989).

In addition, a National Science Foundation (1989) report identified 24 companies among the top 200 R&D performers which had undergone leveraged finance events from 1984 to 1986, including 8 that underwent LBOs. At the time, the top 200 R&D performers accounted for the almost 90% of industrial spending on R&D in the U.S. This report found that R&D expenditures for the 24 firms declined by 5.3%, including a decline of 12.4% for the 8 LBOs from 1986 to 1987, while for the rest of the sample R&D expenditures increased by 5.4% over the same period. The National Science Foundation (1989) report also found that employment of R&D scientists and engineers decreased by 4.1% for firms experiencing leveraged events, as compared with a 1.8% rise for the other top 200 R&D performers. Likewise, an Office of Technology Assessment (1989) report interviewed R&D managers of 19 companies that experienced leveraged corporate finance events. These R&D managers felt strongly that longer-term efforts had been cut in favor of efforts that promised a more immediate payoff.

Zahra and Fescina (1991) summarizes some of these and other studies of the time, noting that almost all of them found that leveraged buyouts “may reduce and stifle corporate R&D”. A notable exception is Kohlberg, Kravis & Roberts (1989), who describe 17 of their own leveraged buyouts and present contentious evidence that public-to-private LBOs enhance operating performance and are associated with increases in employment, R&D and capital expenditure. Unfortunately, Kohlberg, Kravis & Roberts (1989)’s findings, particularly its

innovation findings, are highly suspect. Long and Ravenscraft (1989) documents numerous methodological problems in Kohlberg, Kravis & Roberts (1989) saying, “KKR findings on employment, R&D, and capital spending might be reversed if the study had used actual instead of projected data, omitted acquisitions from the post-LBO data, used ratios instead of levels, adjusted for growth. . . and omitted the LBO year from the pre-LBO data.” Ayash (2018) also recently noted that Kohlberg, Kravis & Roberts (1989)’s sample excludes all of their LBOs which underwent bankruptcies, leading them to overstate their results.

However, throughout the ensuing congressional hearings, it was generally agreed that R&D intensive firms do not make good leveraged buyout targets. While these transactions were controversial, there was, therefore, little cause for concern. Summing up her opinion for members of the U.S. House of Representatives, economist Bronwyn Hall testified that, “Even if all of this R&D spending went away after going private, this would make a very small dent in overall industrial R&D.” (See Hall, 1989). Joseph Grundfest, Commissioner of the Securities and Exchange Commission, supported this view saying, “The companies feeling the heat from restructuring activity appear to be those that have failed to invest as much in R&D as their industry counterparts – not the other way around.” (See Grundfest, 1989). Shortly thereafter, Opler and Titman (1993) provided evidence that firms with high R&D intensity were much less likely to undergo an LBO. Since then, the U.S. has experienced a patent explosion (see Hall, 2004), the number of publicly-traded firms has dropped precipitously, and private equity firms have openly embraced investing in technology companies.

Contemporaneous with the policy studies of the late 1980s, a strand of research emerged focusing on the relationship between leverage and R&D. Together these works established the stylized fact that increased leverage is associated with reduced measures of innovation, including research and development spending, the employment of scientists and engineers, and patenting.

For example, Baysinger and Hoskisson (1989) find a strong and statistically significant

negative relationship between leverage and R&D intensity in a sample of 971 COMPUSTAT firms reporting R&D from 1980 to 1982. Hall (1990) studies 834 acquisitions, including 77 LBOs, in a sample of over 2,500 publicly-traded manufacturing firms in existence from 1974 to 1987. Hall (1990) concludes that declines in R&D are attributable to an increase in leverage associated with acquisitions, rather than with the acquisitions themselves. In related work, Hitt, Hoskisson, Ireland, and Harrison (1989) and Hitt, Ireland, Harrison, and Hoskisson (1991) examine 191 mergers of publicly-traded firms between 1970 and 1986. They found that increased leverage is associated with a greater decline in R&D and patenting intensity, respectively. Long and Ravenscraft (1993a) and Long and Ravenscraft (1993b) examine a large sample of public and private companies with assets over \$25 million that underwent whole company (i.e., not divisional) LBOs. They found that R&D intensity decreased 40% below pre-LBO levels in a sub-sample of 72 R&D performing LBO targets. An exception to this literature is Lichtenberg and Siegel (1990), who found no statistically significant decline in R&D intensity using 56 complete-firm LBOs and a large control group, though their point estimates are all negative.

More recently, a smaller line of literature has studied the effects of leveraged buyouts on patent grants with more mixed and nuanced findings. Amess et al. (2016) consider a sample of 407 UK private equity buyout transactions, of which just 37 were public-to-private LBOs. The majority of their sample were small private-to-private transactions that used growth equity investments. Like our research, Amess et al. (2016) uses propensity score matching and a difference-in-differences approach to make a causal inference. They found that private equity transactions are, on average, associated with more patent grants. However, this effect is driven entirely by private-to-private investments, which are typically not leverage financed. They report that public-to-private buyouts, which are leverage financed, are associated with decreases in patent stocks. These decreases are weakly statistically significant two years after the event.

Ughetto (2010) considers European private-to-private investments, which also appear financed predominantly by venture capital. The paper primarily examines how target firms' patenting behavior changes when the lead private equity investor has certain characteristics, like appropriate stage specialization and co-location. However, Ughetto (2010) also finds that average patent grant volumes are around 1.5 times higher in the years after a private equity investment.

Lerner et al. (2011) examine the patents of 495 U.S. firms involved in a private equity buyout. They find that firms' patent grant levels do not change after a private equity investment, but that their patents are more frequently cited. Just 64 firms (12%) in their sample underwent public-to-private leveraged buyouts; 127 firms experienced private-to-private transactions, 219 had divisional acquisitions, 81 received secondary private equity investments, and 4 were classified as having other buyouts. Despite the differences in samples, our study finds results that are consistent with those of Lerner et al. (2011), though our use of a control group gives a markedly different interpretation to these results.

A final addition to the new empirical literature, Cumming et al. (2018), examines a more broadly defined set of public-to-private buyouts. Cumming et al. (2018) considers private equity related events recorded in Bureau Van Dijk's Zephyr acquisitions database. Their data includes management buyouts, leveraged buyouts, acquisitions by firms which in turn have received private equity, and other private controlling interest investments in firms listed on a stock market from 36 countries for the twenty year period from 1997 to 2017. As a consequence, firms in their sample are typically much smaller than our New York, NASDAQ, and American stock exchanges-listed firms purchased in leveraged buyout transactions for more than \$50 million. Their firms had an average of 3.7 patents at the time of their buyout, as compared with 26.5 in our sample, and around 10% of our average revenue.

Cumming et al. (2018) is primarily focused on citation analysis, including 'radical' non-patent citations, and their findings appear largely driven by deals that occurred during or

after the Great Recession. Nevertheless, we have two results in common. First, both papers find no statistically significant effect on citations-received for U.S. private equity deals prior to 2006. And second, Cumming et al. (2018) find a decline in patent grants one year, two years and/or three years after a public-to-private buyout, depending on the regression specification used. In one of their specifications, reported negative binomial coefficients imply a decline of three patent grants over the first three years of a buyout, which is roughly in line with our results (see Figure 2c) despite the difference in samples. In another specification using firm-level fixed effects, they estimate a cumulative drop in patent grants of around 15% in the three years immediately following a buyout. This also compares well with our finding that patent grants drop an average of 23% over the full duration of a U.S. public-to-private LBO.

To the extent that there are differences in findings between the old empirical literature and the new one, these disparities appear mostly driven by the type of buyout and the ownership structure of the target firms studied. The effects of public-to-private leveraged buyouts in Amess et al. (2016) and Cumming et al. (2018) are consistent with the results for leverage in publicly-traded firms in Baysinger and Hoskisson (1989), Hall (1990), Hitt et al. (1989), and Hitt et al. (1991). They are also consistent with the results of Long and Ravenscraft (1993a), who used a sample mostly comprised of public-to-private LBOs. The effects on patenting of private-to-private transactions – particularly low value transactions which typically involve growth equity rather than leverage-based investment (see Boucly, Sraer, and Thesmar, 2011) – appear positive. Ughetto (2010) and Amess et al. (2016) provide examples of this. And studies with mixed samples, like Lerner et al. (2011), have mixed results.

Much of the theoretical literature on leveraged buyouts is concerned with the effects of LBOs on efficiency. Efficiency arguments provide the justification, or lack thereof, for government subsidies, which for LBOs are mostly in the form of preferential tax treatment. Two theories from this literature bear upon innovation.

A myopia-based theory claims that the market is inefficient and does not correctly value long term investment projects. Bernstein (2015), Aggarwal and Hsu (2013), and others compare innovation outcomes of publicly-traded and privately-held firms and find evidence of public market myopia. Embracing this reasoning, Stein (1988) argues that firms that undertake long-term R&D projects will be selected as takeover targets. The treatment effect of an LBO would then be to curtail innovation activities and dispose of the associated assets. There may be indirect consequences of myopia on innovation too. For example, Manso (2011) notes that rewards for long-term success are an important determinant of R&D employee performance.

Jensen (1988) asserts that an agency-based theory of leveraged buyout selection is better supported by data. The agency theory of LBOs claims that managers of firms do not always act in the shareholders' interest, and that high agency costs invite takeover activity. Debt then provides managerial discipline, so the market for corporate control restores efficiency.

Gompers, Ishii, and Metrick (2003), Masulis, Wang, and Xie (2009), Atanassov (2013), and others find that increased agency in publicly-traded firms is associated with reduced innovation investment. On the one hand, an agency view therefore suggests that private equity firms will select unimaginative firms. Leveraged buyouts might then have no treatment effect on innovation as there is nothing to treat. On the other hand, increased agency may be associated with a greater propensity to acquire patents as a side effect of empire building. A treatment effect of leveraged buyouts may therefore be to reduce patent portfolios as new management divests old purchases.

There are also several theories of the treatment effect of leveraged buyouts that bear upon innovation but do not concern efficiency, at least directly.

Shleifer and Summers (1988), Long and Ravenscraft (1989) and others suggest that leveraged buyouts transfer wealth to investors by breaking implicit contracts with employees, bondholders and/or the government. As R&D employees are expensive on a per capita ba-

sis, and as their contributions are not critical to a firm's immediate survival, a decline in innovation may be a consequence of this transfer seeking.

In a similar vein, private equity firms may engage in non-productive balance sheet restructuring. With double-entry bookkeeping, patents that result from R&D efforts, as opposed to those licensed or purchased, are inherently undervalued on a firm's balance sheet. Patents would therefore, under this view, not be pursued after a buyout transaction. A corollary is that selling patents would create book value and so should occur during a buyout.

Long and Ravenscraft (1993a) find that LBOs reduce their innovation efforts after experiencing a loss of tax breaks for R&D projects, since more marginal R&D projects become net present value negative. In our sample of leveraged buyouts, we find that 49% of a firm's operating cash flows are reinvested prior to their LBO, but that reinvestment drops to just 18% of cash flow in the two years following an LBO. Debt service consumes the difference, as well as most of the cash that used to go to dividends and pay taxes, as firms typically generate a net loss after a leveraged buyout.

Balance sheet restructurings, myopia, and other divestitures and parsimonious behaviors may all be forced upon a firm as a consequence of its increased leverage. Innovation is usually a long-term process that does not yield immediate rents. So, following a leveraged buyout, resources that were previously allocated to innovation may be shifted to producing current output and servicing debt. Furthermore, firms may need to reduce expenses and quickly raise operating capital. They could do this by forgoing patent applications and liquidating patent portfolios. Variants on this argument are frequently advanced in the literature. As an example, Lichtenberg and Siegel (1991) find evidence that an LBO induces a temporary positive impact on productivity, which becomes negative after three years when a firm's stock of intangible capital is depleted.

Finally, some authors have sketched an 'entrepreneurial' theory of leveraged buyouts. Under this view, private equity investors bring new value creation to their target firms. Pre-

sumably, their efforts might include refocusing or reoptimizing a firm’s innovation activities, as a more profitable innovation strategy would be valuable when the firm is relisted to generate returns at the end of a leveraged buyout. This entrepreneurial theory of LBOs stands alone in its potential positive connotations for innovation.

### 3 Data and Measures

The construction of our sample begins with a list of 522 U.S. publicly-traded firms. For each of these firms, controlling ownership was acquired by one or more private equity funds in a leveraged buyout between January 1, 1980 and December 31, 2006, with a total transaction value in excess of \$50 million. This list, and the details of the LBOs associated with them, is available for academic use at [www.lboresearchcenter.com](http://www.lboresearchcenter.com).

Accounting data for our leveraged buyout targets is taken from COMPUSTAT for the period 1970 to 2017. In total, 348 firms on our list have accounting data available, 138 of which held one or more patents at some point in their lives. For our potential control firms, we use all firms with accounting data in COMPUSTAT that were listed on the NYSE, Nasdaq or Amex exchanges, where our leveraged buyout firms were traded.

We use real amounts, in 2017 dollars, throughout. We stress that coverage of research and development expenditure is far from complete, despite requirements to report R&D introduced in 1972 by the Financial Accounting Standards Board (FASB). Furthermore, a material portion of firms that did report R&D, reported zero. We were therefore not comfortable declaring non-reported R&D as zero, and instead omit those observations. Of our 348 U.S. public-to-private LBOs with accounting data, 217 (63%) report R&D one or more times at some point in their pre- and post-LBO lives. However, only 17% of LBO firm-years provide R&D information.

Our main analysis uses propensity score matching, which requires certain financial vari-

ables to be available for our leveraged buyouts for the year before the LBO. We also require each LBO to be matched to a control firm in the same industry, operating at approximately the same time, and holding within 20% of the LBO target firm’s patent stock. As a consequence, our final sample for our main analysis consists of 92 patent-holding account-reporting U.S. public-to-private leveraged buyouts, along with 92 matched control firms.

We use NAICS codes, and an SIC to NAICS concordance for older firms, to classify firms into four industries: trade, services, manufacturing and ‘other’. We define trade as NAICS codes 44 & 45, services as all of NAICS code 5, and manufacturing as NAICS codes 31, 32 and 33. In our final sample, 61 of our observations are in manufacturing, 12 are in services, 9 are in trade, and 10 are in our ‘other’ category. Excluding the ‘other’ category does not affect our results.

All of our patent data come directly from the U.S. Patent and Trademark Office (USPTO)’s website, [bulkdata.uspto.gov](http://bulkdata.uspto.gov). We process the full text patent grant data from XML, SGML and APS files, covering the period 1976 to 2017. We supplement this grant data with data on patent assignments from 1980 to 2017, and maintenance fee events from September 1981 to 2017. The assignment data are processed in much the same way as described in Marco, Myers, Graham, Agostino, and Apple (2015). Although firms should record intra-firm assignments with the patent office, they do not always do so. We do not expect that this introduces any bias into our results.

We join the patent data to our leveraged buyout targets and control firms using name normalization and matching software. Specifically, we use an implementation of the same methods that are used in the construction of the NBER patent data, which are described in Hall, Jaffe, and Trajtenberg (2001) and were updated in 2006 and 2010. We also validate matches using geographic and timing information where possible.

Our patent and citation measures are calculated at the portfolio level, as our unit of observation will be a firm-year. We use the nomenclature ‘patent stock’ to refer to the level

of a firm’s patent portfolio at a point in time, and ‘patent flow’ to refer to the number of patents entering or leaving a patent portfolio each year. We take into account patent grants, purchases, sales, abandonments (i.e., non-payment of a required maintenance fee), and expirations when calculating patent stocks and flows. Only rare events like court rulings of invalidity and terminal disclaimers are not included in our calculations, as we do not have data on them. Patents that experience these rare events are removed from portfolios either the next time a maintenance fee payment is due or, if they are more than 12 years old when the event occurs, when they reach their statutory term. We count each patent grant or purchase as a whole patent entering the firm’s portfolio, irrespective of whether it was a joint or single assignment, as patents are rights to exclude, not rights to use. And we take into account contemporaneous patent law when calculating patent term limits and maintenance fee requirements.

Patents and patent citations suffer from truncation issues, which are discussed in Jaffe, Trajtenberg, and Romer (2002), Lerner and Seru (2017) and elsewhere. We use two general approaches to ameliorate these issues. First, we assemble data on our patent portfolios as long as possible before and after leveraged buyouts. Every one of our LBO events has at least 4 years of data before it and 11 years of data after it. Jaffe and Trajtenberg (1996) establishes that peak citation intensity usually occurs 4 to 6 years into a patent’s life, so our data always firmly covers the period of greatest citation activity. Second, we use year fixed effects in all of our regression analysis of patents and their statistics, as well as industry fixed effects to reduce industry-based variation in patenting behavior.

We also estimate qualitative changes to patent portfolios using six patent statistics, based on counts of citations to other patents. Each of these measures suffers from notable construct validity issues.

Citations received, also called forward citations, is the most commonly used patent statistic and is typically said to represent either importance (i.e., social value as in Trajtenberg,

1990) or private value. In our context of U.S. publicly-traded firms, and over a similar time horizon, Hall, Jaffe, and Trajtenberg (2005) find statistically significant, and economically meaningful, positive associations between citations received and firm value. However, it should be noted that in other contexts, authors have found either no association or a negative correlation between citations received and private value (see Bloom and Van Reenen, 2002, Shane and Stuart, 2002, and others).

Citations made, also called backwards citations, have been linked to value (e.g., Jaffe, 1986), knowledge flows and geographical proximity (e.g., Jaffe, Trajtenberg, and Henderson, 1993), patent complexity (e.g., Jaffe et al., 2002), and much else besides. One interpretation compatible with many of these results, articulated by Alcacer and Gittelman (2006), is that citations made may represent patent breadth.

Trajtenberg, Henderson, and Jaffe (1997) propose two measures called originality and generality, which were popularized by their inclusion in the NBER patent data. A bias correction factor was added to these measures by Hall (2005). Without this correction, these measures primarily estimate the effects of citation counts whenever they are low.

Originality and generality measures are (one minus) Herfindahl-Hirschman indices (HHI), using shares of citations made or received, respectively, in ‘HJT categories’ of patent classes. HJT categories put together patent classes into meaningful groups and are described in Appendix 1 of Hall et al. (2001). Patents with higher generality receive citations from a broader array of categories, while those with higher originality make citations to a more diverse set of categories.

The formula for the unbiased generality measure is shown in equation 1, below.

$$G_i = \left( \frac{N_i}{N_i - 1} \right) \cdot \left( 1 - \sum_{j=1}^J \left( \frac{N_{ij}}{N_i} \right)^2 \right) \quad (1)$$

Where  $G_i$  is the generality measure for firm  $i$ ,  $N$  is the number of citations received, and  $j$  is an index of HJT categories, so that  $N_{ij}$  is the number of citations received by firm  $i$  in HJT category  $j$ .  $\frac{N_i}{N_i-1}$  is the bias correction factor. The originality measure,  $O_i$ , is the same except that  $N$  then indicates citations made.

Our last two patent statistics, whether a patent is explorative or exploitive, are more recent additions to the literature. We construct them in the same way as Lin, Liu, and Manso (2016). For these measures, we suppose that a firm has a local knowledge base that changes over time. For a given patent, this local knowledge base consists of the patents issued to the firm in the previous five years (a subset of the firm’s potential ‘self-citations’) and the citations that those patents made (i.e., recently cited inventions). Whether a patent is defined as explorative or exploitive then depends on placing cut-offs on the fraction of citations made to the local knowledge base. We follow Lin et al. (2016) and mark patents with below 20% local knowledge citations as explorative and patents with above 80% as exploitive. Our results are essentially unchanged if these boundaries are moved  $\pm 5\%$ .

## 4 Methodology and Results

### 4.1 Selection Effects

We begin this section by considering whether private equity firms select public-to-private leveraged buyout targets that are more or less innovative than other publicly-traded firms. In Table 1, we use pre-buyout patent and R&D data for our 348 leveraged buyouts, along with patent and R&D data for all other firms in COMPUSTAT that are publicly-traded on the New York, NASDAQ or American stock exchange. We then explore whether LBO targets are more or less likely to have any patents, as well as whether, year by year, they are likely to have more or fewer patents, and higher or lower R&D expenditure.

In the 1<sup>st</sup> specification of Table 1 we find no statistically significant difference between firms that experience a leveraged buyout and other publicly-traded firms in their likelihood of having one or more patents. The unit of observation in this specification is a publicly-traded firm, and we use a logit regression. In the other specifications (2<sup>nd</sup> through 5<sup>th</sup>), the unit of analysis is a firm-year, and coefficients are estimated with OLS. We use year and industry fixed effects throughout, which account for our comparatively high (pseudo-)  $R^2$  values.

In the 2<sup>nd</sup> and 5<sup>th</sup> specifications, we find that an LBO is associated with a higher patent stock in a firm-year. LBO firms have around 8% more patents in a given year than non-LBO firms, and this difference rises to over 17% in the subsample of firms that have one or more patents at some point in time. However, this might be explained by a survival effect. Firms that experience an LBO are active in our data for an average of 16 years, which is 2 years (14%) longer than their non-LBO counterparts.

An important result in Table 1 concerns R&D expenditure. Only 15% of our firm-years have R&D data, though LBO firm-years appear about equally likely to have R&D data (17%) as non-LBO firm-years. Nevertheless, the 3<sup>rd</sup> specification shows that a leveraged buyout target is associated with less than half the R&D expenditure per year, even after controlling for differences between industries and over time. In unreported regressions, we find that the difference in R&D expenditure associated with an LBO is not materially or statistically different between patenting and non-patenting firms.

Before we explore our R&D and patenting results, we draw attention to the last result in Table 1. In the 4<sup>th</sup> specification, we find that leveraged buyout targets are associated with 100% more intangible assets than non-targets. In unreported regressions, we find that this effect is significantly more pronounced among patenting firms, which show 136% higher levels of intangibles, than in non-patenting firms, which show 53% higher levels.

Leveraged buyout targets could have higher numbers of patents and lower levels of R&D expenditure if they are purchasing patents either directly or as a part of an acquisition of

another firm. In later analysis, we will show that LBO targets do indeed purchase patents at a statistically higher rate prior to their buyouts. Around one quarter of their patents are purchased in the years leading up to their LBO. This is congruent with our findings in 1 on intangible assets. The scale of our difference for intangible assets, though, strongly suggests that LBO targets are purchasing more than patents. We expect that much of an LBO target’s intangible assets are in goodwill from past acquisitions.

Our selection results so far support an agency-based view of leveraged buyout target selection, and not a myopia-based one. But the answer to our first question – “Are modern public-to-private LBO targets less innovative than other publicly-traded firms?” – is more nuanced. Patents are an output measure of invention, while R&D expenditure is an input measure. One could therefore argue that patents are a better measure of innovation than R&D, and so that private equity firms do not target particularly innovative or non-innovative firms (as in the 1<sup>st</sup> specification of Table 1). We claim that this is true but overly simplistic.

The results in the 2<sup>nd</sup> and 5<sup>th</sup> specifications of Table 1 suggest that private equity targets hold more patents than other publicly-traded firms. These results are enhanced, but not driven, by patent purchases. In unreported regressions, we find that patent grants are significantly higher for LBO targets: LBO targets receive 3% more patent grants than other publicly-traded firms overall, and this rises to 8% more within the subsample of firms that have one or more patents. Patent grants are arguably the best readily available measure of innovation. Moreover, it does not matter when assessing innovativeness that an LBO’s higher grant rate is largely, if not wholly, attributable to its greater age. Our grant rate findings also support a myopia-based theory of LBO selection.

## 4.2 Descriptive Statistics

Table 2 shows descriptive statistics for the 348 leveraged buyouts in our sample that have accounting data. The statistics shown are for the fiscal year before the leveraged buyout. They are reported for two groups of firms: 138 LBOs that have some patenting activity and 210 LBOs that do not. The table shows means, medians, standard deviations, and the number of observations for which data are available for some variables of interest. We do not use the firm's age from initial public offering to leveraged buyout, or its intangible assets, in our regression analysis. We only include these variables for reference.

In our sample, firms that experience a leveraged buyout are large, and those that have patents are about half as large again as those that do not. The average LBO with patents has around \$2.5 billion of assets and revenue, EBITDA of around \$300 million, and pays around \$50 million in taxes, in 2017 dollars, in the year before its buyout. For each of these measures, the medians are between one quarter and one third of the means. We therefore use log transformations of these variables in our later analysis.

The size of a leveraged buyout target is at least partially a consequence of its age. Our patenting LBO firms experience their buyout on average 12.5 years after their IPO. Non-patenting LBO firms are on average 10 years old at their LBO. The difference in ages is therefore slight compared with the difference in sizes for these two sets of private equity targets. Leveraged buyouts also take on average around 7 to 8 years to complete. PE funds typically have ten year lives, though some are evergreen, and deals are sometimes passed from one fund to another.

As one would expect, leveraged buyout targets, with or without patents, are comparatively lightly leveraged before their buyouts. They have liability to asset ratios of between 50% and 60%. Likewise, they appear to have moderate valuations in terms of Tobin's Q. They have market-to-book value of equity ratios of around 75% and debt to assets ratios of

25% to 35%.

Expanding upon our results from earlier, and in line with testimony to Congress in the late 1980s, we find that firms that undergo a leveraged buyout have low levels of research and development expenditure. Our patenting LBO firms spend just \$21 million a year on average on R&D; and for our non-patenting LBO firms this number drops to just \$4m, with a median of \$0.

Finally, we review the extent of intangible assets held by leveraged buyout targets. For the patenting firms, mean intangible assets are sizable and close to a quarter of mean total assets, though the median ratio is much lower. We briefly examine the distribution of patent portfolio sizes, but they do not appear commensurate with these intangible asset values. Moreover, even non-patenting firms have sizable intangible assets, averaging \$267 million. These results suggest that private equity investors may actively seek firms with notable goodwill from previous acquisitions, as it makes them suitable for later restructuring.

### 4.3 Propensity Score Matching

In order to make causal inferences, we create a paired set of non-LBO firms to use as a control group with our treatment group of leveraged buyouts.

We use a logit regression to estimate propensities to undergo a leveraged buyout for non-LBO firms for each year they appear in our data. The outcome variable in our logit regression is an indicator that takes the value 1 for firms that have had an LBO, and 0 if a firm has never had an LBO. For firms that have had an LBO, we include all firm-year observations prior to their buyout. We do not include observations from their LBO forward, as these cannot be correctly coded as either 0 or 1. We use the following observable covariates in our logit estimation: *Leverage*, defined as liabilities divided by assets; a measure of *Tobin's Q*, defined as the market-to-book value of equity (note that other definitions of Tobin's

$Q$  typically include liabilities, but we already include liabilities in our leverage measure); *SalesGth*, the firm’s sales growth over the first full year prior to the LBO, since it is often suggested that LBO targets are low-growth firms; *Tax*, the ratio of tax expense to assets, since tax liabilities are an important factor in financing LBOs; *ROA*, defined as EBITDA divided by total assets, to capture differences in performance and in mean-reversion; and *Size*, defined as the log of revenue, to proxy for various measures of the size of the firm as well its life cycle and future growth opportunities.

Table 3, Panel A, reports the results of our logit regression. *Tobin’s Q*, *Tax* and *ROA* are all highly statistically significant predictors of a leveraged buyout. *Leverage* is a moderately significant predictor. In line with our expectations and descriptive statistics, private equity firms appear to choose targets that have lower leverage, giving room for much more leverage, and lower market-to-book values of equity, so that they are currently undervalued by the market. They also choose targets that offer higher returns on assets and that pay higher taxes relative to their assets. Once a firm is leveraged, these cash flows can then be redirected towards paying interest. We were surprised that sales growth and size were unimportant, though these results are derived after controlling for other factors.

For each LBO target firm, we assemble a candidate set of non-LBO firm years. To be included in a candidate set, a firm must: be in the same industry (trade, services, manufacturing and ‘other’) as the LBO; have within 20% of the patent stock of the LBO in its first full fiscal year before its buyout; and have some data within a five year window that starts two years before and ends two years after the buyout. Given these constraints, each candidate control firm can contribute up to five firm-year observations to the logit regression. We then select, without replacement on a firm basis, the firm-year from the candidate set that has the minimum square distance between its estimated propensity score and the LBO firm’s propensity score. Using this procedure, we are able to find control firms for 92 of our 138 innovative U.S. public-to-private leveraged buyouts.

Table 3, Panel B, reports means for our accounting measures, as well as the year of the observation, the patent stock, and the propensity score for our leveraged buyouts and selected control firms. The accounting measures and the patent stock are evaluated in the year before the LBO event. Panel B also reports differences in these means, and  $t$ -statistics and  $p$ -values to assess these differences.

The results in Table 3 suggest that we have good matches. Only the ratio of EBITDA to assets is weakly statistically different between the leveraged buyout targets and their control firms in the year prior to the buyout. Crucially, the propensity scores of the LBOs are not statistically different from those of the control firms. A multivariate test of equality of means with heterogeneous covariates gives an insignificant  $F$  statistic of 0.77, indicating that we are unable to reject the null hypothesis that the means are the same for our two groups.

The match requirements on year, industry and patent stock imply that these measures should be close by construction. Nevertheless, it is reassuring that our decision to allow each candidate firm to contribute up to five firm-year observations to the regression did not systematically select control observations in years before or after the leveraged buyouts, and shows that we have not systematically selected control firms with bigger or smaller patent portfolios.

As we will see later, the difference in R&D expenditure between leveraged buyout targets and control firms is partially ameliorated by the matching process. In Table 1, having an LBO was associated with 55% less R&D expenditure, but in Table 5, this difference is reduced to 33%. The difference in intangible assets, which was so notable in Table 1, is eliminated completely by the matching. In untabulated regressions, we find no statistical difference in intangible assets between LBO targets and their matched control firms in any of the years of the decade leading up to their buyout. These two results provide additional assurance that our matching process is yielding control firms that are operationally similar to LBO targets.

## 4.4 Distributional Considerations

In Table 4, we begin our main analysis by reviewing the distribution of patent stocks for our 92 leveraged buyouts in the year of their LBO. The mean patent portfolio is notably smaller in the year of the LBO (see Table 4, Panel A) than in the year previous (see Table 3, Panel B). We will explore the cause of this drop shortly.

Almost a third of our firms have no patents in their portfolio in the year of their leveraged buyout (see Panel B). We only require that a firm have one or more patents at some point for it to be included in our main analysis. Furthermore, as is common in samples of innovative companies, a small number of firms hold the majority of the patents. The mean patent stock for an LBO is 26.5 patents, but the distribution is heavily skewed to the right, and the median is just 4.5 patents. One firm held 426 patents in the year of its LBO, and four other firms held more than 100 patents. However, including either firms without patents in the year of the buyout, or firms with a large number of patents does drive our results.

All of our count measures suffer from over dispersion. This includes not just patent stocks, shown in Table 4, but patent flows, grants, purchases, sales, abandonments, and expirations, as well as citations-made and received. The variances of our count measures are many orders of magnitude greater than their means, and  $\chi^2$  goodness-of-fit tests firmly reject that they follow Poisson distributions. Visual inspection confirmed this and suggested that our data were better approximated by negative binomial distributions than Poisson distributions. In unreported regressions, we therefore tried all of our count data estimations using negative binomial regressions, as well as GLM estimations of Gamma regressions. The results of these estimations showed the same signs and significances as estimations using OLS on logged variables.

Our R&D variable is also highly skewed, and our explorative and exploitive measures are closer to normally distributed when log transformed. We therefore use the log of one

plus each variable in the analysis that follows, as it does not affect our results, and as the coefficients of OLS regressions then have a simple interpretation as a percentage change in the outcome variable. One exception concerns our patent flow measure. Patent flows can be positive or negative. To log transform patent flows, we therefore use the sign times the log of one plus the absolute count value.

#### 4.4.1 Patent Stocks and R&D

In Figures 1a and 1b we show what happens to patent stocks and R&D expenditures, respectively, in a leveraged buyout.

All of the figures in this paper show data for LBOs and their control firms from ten years before the LBO to ten years after. The LBO event year is denoted  $t = 0$ . The mean time in a leveraged buyout is around 8 years, and we only include an observation if data are available for the matched pair. When a firm exits the LBO process – for example through a new public offering – it is excluded along with its matched control firm. We are also unable to report reliable estimates of R&D expenditures for  $t = +5$  forward, because R&D coverage is always sparse and is even more difficult to obtain when the firm is private post-LBO.

In Figure 1a, there is a clear common trend from period  $t = -10$  to period  $t = -1$ : patent stocks of the control firms are not just within a standard error of the patent stocks of the LBO firms, they are almost identical. The patent stocks diverge in the year of the LBO. Before a leveraged buyout, an LBO firm’s patent stock increases steadily; after a leveraged buyout, an LBO firm’s patent stock decreases steadily. The overall trend for LBO patent stocks is therefore an inverted-U. This inverted-U is statistically symmetrical. T-tests using symmetric durations, for example, comparing patent stocks 5 years or 10 years before and after the event, reveal no statistically significant differences.

The patent portfolios of the control firms, on the other hand, continue to increase markedly in size through  $t = 0$ , albeit with some noise. We attribute this rise to ‘patent

inflation’; the expansion of U.S. firm patent portfolios was both a cause and a consequence of the patent explosion. With control firm patent stocks growing and LBO patent stocks shrinking, paired t-tests find two-sided statistically significant differences beginning five years after the event (i.e., at  $t \geq +5$ ).

Figure 1b shows real R&D expenditure, in millions of 2017 dollars, for both leveraged buyouts and control firms. The difference in levels of R&D expenditure between LBO firms and non-LBO control firms is readily apparent. LBO firms spend around \$20 million each year on R&D, in real terms. The data are right-censored because we do not have reliable estimates for both sides of the matched pairs for  $t = +5$  forward. Nevertheless, there is a clear common trend, especially in the pre-LBO period. After a leveraged buyout there is a slight apparent divergence between LBO firms and their control firms around year  $t = +3$ , with control firms increasing their R&D expenditure as LBOs decrease theirs. However, as we show shortly, this divergence is not statistically significant.

In Table 5 we explore how patent stocks and R&D expenditure are affected by a leveraged buyout in a difference-in-differences OLS regression framework.

In each of the regression analyses that follow, the *Time Indicator* is zero before the leveraged buyout and one in the year of the LBO and afterwards. Likewise, the *LBO Indicator* is zero for control firms and one for LBOs. The difference-in-differences is given by the  $Time \times LBO$  variable. We also use year and industry fixed effects throughout. As with the figures, we start including observations ten years prior to the LBO (i.e., at  $t = -10$ ) and exclude observations where data are not available for both firms in a pair. However, we now include observations beyond  $t = +10$ , provided that the LBO is still privately-held by the private equity fund (i.e., still undergoing treatment).

Table 5 sheds light on the causality of the narratives visible in Figures 1a and 1b. We find that patent stocks do indeed inflate over time. Patent stocks are around 63% higher in the leveraged buyout period, relative to the ten years previous. Were it not for the buyout,

LBOs and control firms would have identical patent stock. However, patent stocks decline by 28% as a consequence of a leveraged buyout by private equity firms.

The results for R&D expenditure are very different. We find that real R&D expenditure does not vary with time, and there is no effect from a leveraged buyout. LBO targets just have one third lower R&D expenditures than the matched control firms, after controlling for industry and timing differences.

#### 4.4.2 Changes in Patenting Activities

In Figure 2, we deconstruct the flow of patents (Figure 2a) in and out of a firm's portfolio before and during a leveraged buyout. Patents flow out of a firm's portfolio as it abandons patents by not paying their maintenance fees (Figure 2b), sells patents (Figure 2d), or has patents expire at the end of their terms (Figure 2f). Alternatively, patents flow into a firm's portfolio as they are granted to the firm (Figure 2c) or purchased by the firm (Figure 2e). Together these five activities give a close approximation of a firm's real world changes to patent stock.

Leveraged buyout targets appear to change their patent portfolios dramatically in the year that their buyout occurs. In Figure 2a, we can see that their flow spikes down as they drop an average of seven patents from their portfolios and before they recover to an insignificantly positive flow in the year following their purchase by a private equity firm. In the subsequent decade, their patent stock seems to atrophy a little, though their flow is always insignificantly different from zero. In year 10 there is a small insignificant change back towards a positive flow.

The long atrophy of a leveraged buyout target firm's patent portfolio appears predominantly caused by its reduced grant rate. In Figure 2c, there is a strong upward common trend for both LBOs and control firms, which then splits apart somewhere between  $t = -3$  and  $t = +1$ . In unreported t-tests, we find that LBOs have weakly statistically significantly

fewer patent grants in the year after their buyout, with the difference becoming strongly statistically significant six years after their buyout. The difference is not statistically significant before  $t = +1$ , though it then widens over the next two years until, by year three, LBOs are granted three fewer patents each year than their control firms. As both LBOs and control firms have an average grant rate of about three patents per year just before a buyout, the decline in the grant rate from an LBO is materially and economically meaningful.

The lag between a patent application and a grant averages around two years over the period of our sample. Because we observe an immediate significant effect from the buyout, our data suggest that firms decline to continue some of their patent applications when they undertake a leveraged buyout. And because this gap widens and then persists, our data suggest that during an LBO, target firms continue to innovate – and so apply for and are granted patents – but at a reduced rate. It costs between \$20,000 and \$50,000 to prosecute a patent application (see Lemley, 2000 and others). With an average LBO duration of eight years, a reduction of three patents per year would therefore save between \$420,000 and \$1.2m in prosecution costs, and presumably at least an order of magnitude more in research and development expenditure.

Perhaps the most dramatic finding in all of Figure 2 is the sudden burst of patent sales associated with a leveraged buyout, shown in Figure 2d. These sales provide the explanation for the sharp spike down in an LBO firm’s patent flow in the year of their buyout in Figure 2a. Patent sales are usually comparatively rare, with publicly-traded firms typically selling a patent less than once per year. In our data, both LBOs and control firms never exceed an average of 3.2 patents sold per year, except for when a firm undergoes a leveraged buyout and makes a one-time sell-off. In the year of their leveraged buyout, a firm sells an average of 10 patents. Hall et al. (2005) calculates the average value of a patent for U.S. publicly-traded firms at \$1.8m in 2017 dollars. Issues with a market-for-lemons aside, this would imply that private equity firms raise an average of \$18m from selling off their acquisition’s patents.

Our mean sell-off statistic includes, but is not driven by, a single leveraged buyout selling 798 patents – this is the same firm that went on to have 426 patents by the end of the year of its buyout, shown in Table 4. In unreported t-tests, we find the difference between LBO firms and control firms to be highly statistically significant at  $t = 0$ , whether or not this firm is included in our sample. Thirteen LBOs – over 14% of our sample – sell patents in their buyout year. When one considers that only 63 of our LBOs had any patents to sell at this time, the portion of patent sellers among patent holders rises to around one-fifth. The average LBO year patent seller disposes of 45% of their previous year’s patent stock. The lowest fraction of a portfolio sold is 1% and the highest is 89%.

Patent purchases, like patent sales, are also comparatively rare. Because one cannot buy a fraction of a patent, the rarity of patent purchases makes for sharply angled graphs. Nevertheless, there is an apparent reversal in the behavior of LBO firms in Figure 2e. Leverage buyout targets seem to buy patents somewhat more often than control firms prior to their LBO, and then buy patents less often than control firms after their LBO. Indeed, Figure 2e shows that LBO targets buy an average of one patent each year in the six years prior to their buyouts but, from  $t = +1$  forward, patent purchases are close to zero.

Given the results so far, one might expect that firms that undergo leveraged buyouts will then decline to pay maintenance fees, and so abandon patents. We don’t find this. Instead, LBO firm patent abandonments, shown in Figure 2b, appear to continue their slight upward trend from before an LBO. Since the 1990s, it has become somewhat more common for patent-holders to decline to renew their patents. The slight upward trend in our data reflects both this and patent inflation; all else equal, having more patents will result in more abandonments.

We find that a firm abandons about one patent every two years during their leveraged buyout. An average LBO holds a portfolio of 26.5 patents at the end of their acquisition year and the statutory term for a patent is 20 years. This finding is therefore in line with

the stylized fact that around half of U.S. corporate patents are renewed to their statutory limit. Maintenance fees are modest; using renewal rates given by Moore (2005), the average maintenance fee now stands at around \$4,350. Doubling the abandonment rate to one patent a year for the duration of an eight year leveraged buyout would therefore save just \$17,400. As this is a very small saving, private equity firms may prefer to renew their patents without regard to the leveraged buyout.

Patents that aren't abandoned expire at the end of their statutory term. Term expirations are therefore inversely related to abandonments with a lag. Because we do not see an abandonment effect, we do not see an LBO effect in patent expirations in Figure 2f either.

In Table 6, we examine the effect of a leveraged buyout on patent flows, and on each of our five patenting activities, in a difference-in-differences regression framework.

Only the overall flow of patents itself, and the two activities that should add to a firm's stock of patents – patent grants and patent purchases – show a statistically significant effect as a consequence of a leveraged buyout. An LBO causes a one-third drop in the target firm's patent flow, which comes from around 23% fewer grants and 7% fewer purchases. The three activities that decrease patent stocks – selling patents, abandoning patents, and having patents expire – are not statistically significantly affected by an LBO. The sales spike at year zero, which is included in the post-LBO period, is apparently too isolated an event to create statistical significance when one takes into account the other years of a buyout.

Leveraged buyout targets, however, do purchase around 4% more patents and sell around 6% more patents than their control firms, irrespective of the buyout event itself. Purchasing more patents could be consistent with empire building, though both purchase and sale volumes are typically so low that, outside of unprecedented one-off sales, it is unclear that they have much economic meaning. Our *Time Indicator* is also associated with a 15% rise in grants, an 8% rise in abandonments, and a 7% increase in expirations. These results are explained by patent inflation and abandonments becoming relatively more frequent over

time.

Overall, our results on the effect of a leveraged buyout on various patenting activities paint a picture of private equity investors selling off assets and economizing on costs. There are many reasons that PE firms might engage in such behaviors, like the simple necessity to service financing debt or to reflect increases in efficiency. We discuss this further in the conclusion.

#### 4.4.3 Patent Citation Statistics

In this final section of our analysis and results, we consider whether the patent portfolios of firms experiencing a leveraged buyout undergo a qualitative change. As was discussed in the data and measures section, we use patent statistics, based on patent-to-patent citations, to measure various aspects of a firm’s patent portfolio’s quality.

In Table 7, we use OLS difference-in-difference regressions with six commonly used patent statistics as the outcome variables: citations received, citations made, generality, originality, explorativeness and exploitiveness. As before, we use industry and year fixed effects throughout. Year fixed effects are particularly important in this context as it takes time to accumulate citations, and, akin to patent inflation, there is considerable citation inflation over the period of our sample. We find that our *Time Indicator* is associated with a 30% rise in citations-received.

Our most important qualitative findings are null results. We find that leveraged buyouts do not cause firms to hold patents that receive more or fewer citations. Nor do private equity firms select LBO targets that have highly or poorly cited patent portfolios. There is simply no statistical difference in citations received to a patent portfolio of an LBO as compared with that of a control firm, either intrinsically or as a consequence of a buyout in our sample. Of all patent statistics, beyond simple patent counts, citations-received has the greatest agreement in its semantics; it is very commonly assumed to reflect importance

or value. Leverage buyouts are therefore not associated with our foremost patent portfolio quality measure.

We do, however, find two statistically significant effects of leveraged buyouts on patent statistics. As a consequence of a buyout, an LBO target's patents make 16% more citations and score 1% higher on a measure of explorativeness. Citations-made may measure the breadth of an invention. A 16% increase in the breadth of inventions is sizeable and likely to be economically important. Greater explorativeness, on the other hand, measures the extent to which a firm's patents are referencing more material from outside of its local knowledge base. A 1% increase in explorativeness therefore does not seem likely to be economically important.

There is also a notable selection effect in Table 7. We find that LBO targets have patents with on average 6% higher measures of generality, irrespective of a buyout. Our measure of generality is unbiased, so this result is not driven by counts of citations received. More general patents might be more saleable, indicative of a less specialized innovation strategy, or otherwise amenable to private equity investment.

Two of our six quality measures, originality and exploitiveness, have no statistically significant relationships to discuss in Table 7. We therefore finish this section by reviewing figures for the other four quality measures.

In Figure 3a, we re-examine our most obvious patent quality measure, citations-received. The figure shows clear evidence of citation inflation but no clear difference between LBOs and control firms at any point in time. Using t-tests we confirmed that the minor differences at  $t = +5$ ,  $t = +6$  and  $t = +9$  are all statistically insignificant. However, tests of differences in levels before and after the LBO give statistically significant positive results for suitable durations. For example, levels of citations-received 3 years after an LBO are statistically significantly higher than those of citations-received 3 years before an LBO. Without a control group, or without otherwise correcting for citation inflation, it would erroneously seem that

an LBO is associated with more highly-cited patents, and so more important or valuable innovations.

The statistically significant effects of leveraged buyouts on citations-made and explorativeness are evident in Figures 3b and 3d, respectively. Both figures exhibit common trends from  $t = -10$  to  $t = -1$ . Citations made by LBOs appear to diverge from those of control firms at around  $t = 0$  and then rejoin them around  $t = +8$ . There is a clear separation of the error bars for six of these years. Because the effect of an LBO on citations-made is immediate, it is likely initially driven by the sell-off of patents and/or through the cancellation of pending patent applications. However, its continuance suggests that new innovations undertaken during the early years of the LBO are different; after an LBO, target firms appear to pursue materially broader patents. For our explorativeness measure, which is shown at a much closer scale, the divergence between LBOs and control firms happens later, around  $t = +6$ . This could indicate a small change in the types of inventions being pursued through medium-term (perhaps two to five year horizon) R&D projects initiated subsequent to the LBO.

Finally, the difference between LBOs and control firms in terms of their patent generality is readily apparent in Figure 3c. The two data series statistically overlap briefly around  $t = -6$  and then more firmly from  $t = +7$  to the end of the figure. Nevertheless, there is a clear level difference: the level of LBO patent generality is between 5% to 10% higher than the level of control firm generality for the vast majority of the figure. This result is consistent with private equity firms selecting, consciously or otherwise, LBO targets that have more general patents.

## 5 Concluding Discussion

We begin this paper by asking whether public-to-private leveraged buyout targets are differently innovative. We compare 348 public-to-private leveraged buyout targets, 138 of which had one or more patents, with all other firms traded on the New York, Nasdaq, and American stock exchanges with data in COMPUSTAT.

We find that LBO targets are no more or less likely to hold patents than other publicly-traded firms. This result is tempered in three ways. First, LBO targets hold significantly more patents on average in their portfolios each year. We attribute this result primarily to a survival effect; LBO targets are present in our data for longer than non-targets and so have more time to accumulate patents. Second, LBO targets have around half the level of R&D expenditure as non-targets. They also purchase more patents, or more firms that hold patents, than non-targets, which appears to partially make up for their reduced R&D investment. And third, LBO targets have significantly and economically meaningfully higher grant rates than other publicly-traded firms.

Most of these findings support an agency-based theory of leveraged buyout selection, though one – the higher grant rate – supports a myopia-based theory. Together they also give a nuanced answer to our initial question, which depends on the definition of innovation.

The main focus of this paper is then on the question: What effect does a public-to-private leveraged buyout have on a firm’s innovation? To address this question, we use a large sample that consists of 92 innovative U.S. public-to-private LBOs, which have sufficient data to locate suitable control firms using propensity score matching. We then assess the causal effect of an LBO using a difference-in-differences framework.

Our high-level findings are that a firm’s flow of patents is reduced by 33% for the duration of a leveraged buyout, which leads to a decrease of 28% in an acquired firm’s patent stock relative to a counterfactual firm. The signs and significance of our results are in line

with expectations from the literature. Nevertheless, we believe we are the first to estimate the magnitudes of these effects for U.S. public-to-private LBOs, and so shed light on this important economic impact of private equity.

The relative decline in an LBO's patent stock is primarily caused by fewer patent grants. We find that patent grants drop immediately following an LBO, suggesting that target firms abandon patent applications in process. We also find that LBO firms are then granted fewer patents through-out their buyouts. Between these two effects, a leveraged buyout causes a 23% drop in patent grants. However, we do not find a statistically significant change in R&D expenditure as a consequence of an LBO. We do not attribute this null result to issues with coverage of R&D expenditure, which is poor especially after a leveraged buyout has begun. Instead, we suggest that leveraged buyout targets do not further economize on R&D once they have received private equity.

A main contribution of this paper is that we break out five patenting activities that could influence a target firm's patent flow in a leveraged buyout. Specifically, we consider the impact of an LBO on patent purchases, sales, abandonments, and expirations, as well as patent grants.

We find that around one-fifth of innovative publicly-traded firms sell patents when they receive private equity. LBO firms that sell patents liquidate an average of 45% of their portfolios. An LBO also causes a target firm to reduce patent purchases by 7%. Although patent purchases are comparatively rare as compared with grants, this effect does materially affect patent stocks. We do not find that private equity firms decline to pay maintenance fees, which we suggest may be because the savings would be small. Instead, LBO targets are equally likely as control firms to renew their patents and so equally likely to have them expire at term in our data.

In addition, we assess qualitative changes to patent portfolios as a consequence of a leveraged buyout. We conduct this assessment using patent statistics based on patent-to-

patent citations. Specifically, we use citations received and made, and measures of originality, generality, exploitiveness and explorativeness. A patent that makes more citations is often interpreted as being broader, and a patent that receives more citations is typically thought to be more important or valuable. Using these interpretations, we find that a buyout does not cause firms to create more or less important or valuable patents. However, LBOs do cause some qualitative changes to patent portfolios: post-LBO patents are 15% broader and have a slightly higher measure of explorativeness.

Viewed in the context of private equity's historic examination by policymakers, our findings are worrisome. At the end of the 1980s, private equity narrowly escaped regulation. A report from industry to congress, Kohlberg, Kravis & Roberts (1989), which claimed that leveraged buyouts were (among other things) innovation enhancing, and the consensus that LBO targets were not innovative, were instrumental in this escape. Our analysis, based on a predominately later period, provides evidence largely to the contrary: We find that private equity firms select targets that are equally likely to have patents but that have larger patent stocks and higher grant rates; and that a leveraged buyout causes a substantial drop in a firm's patent stock, driven in part by a material drop in its grant rate.

However, we also support the old claim that private equity targets have low levels of R&D expenditure, and our findings could be consistent with a variety of leveraged buyout treatment theories and motivations.

In particular, our results could be consistent with efficient behavior and an entrepreneurial narrative for private equity. Griliches (1998) repeats the stylized fact that around half of all patents are shelved and never used. In this light, LBO targets that sell, on average, 45% of their patent portfolios, may be optimizing correctly. Likewise, some target firms may have a poorly aligned innovation strategy. This may even be one reason that are acquired. Then an abandonment of pending patent applications and a reduction in new patent filings after a buyout may reflect an efficient realignment. Moreover, our data suggest that LBO firms

file for new patents that are of the same importance but more explorative and broader than pre-buyout patents. As they maintain the same R&D budgets after receiving private equity, LBO firms presumably spend more on fewer – and perhaps better – projects.

On the other hand, many authors have argued that private equity firms engage in transfer seeking, non-productive balance sheet restructurings, or other lucrative but ultimately value destroying activities. Lichtenberg and Siegel (1991) makes a typical charge: that LBOs deplete intangible capital and other long-term drivers of a firm’s success to create a temporary positive impact on productivity. Our results could be consistent with these theories too.

Patents that result from R&D efforts are undervalued on a firm’s books as a consequence of double-entry bookkeeping. Selling granted patents can therefore generate cash without unduly depleting a firm’s balance sheet. We estimate that private equity firms raise an average of \$18m through patent sales. Refraining from buying patents and reducing new patent grants would materially reduce short-term expenditures. Patents may cost millions of dollars to buy and presumably pursuing fewer innovations would save on R&D personnel and other expenses beyond just patent application costs. These savings would potentially all come at a cost to long-term benefits for both the firm and the U.S. economy, though.

Finally, we note that private equity firms may be engendering innovation in their targets in ways that are not measured by patenting activities or R&D expenditure. For example, Le Nadant and Perdreau (2011) find that French leveraged buyouts result in “improvements in how services are provided”, as well as “innovations in design and packaging” and in “products promotion”. Ultimately, definitions of business model innovation typically involve reconfiguring a firm so that it delivers greater value, which is what PE firms claim to do.

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Figure 1: Patent Stocks and R&D for LBO and Control Firms

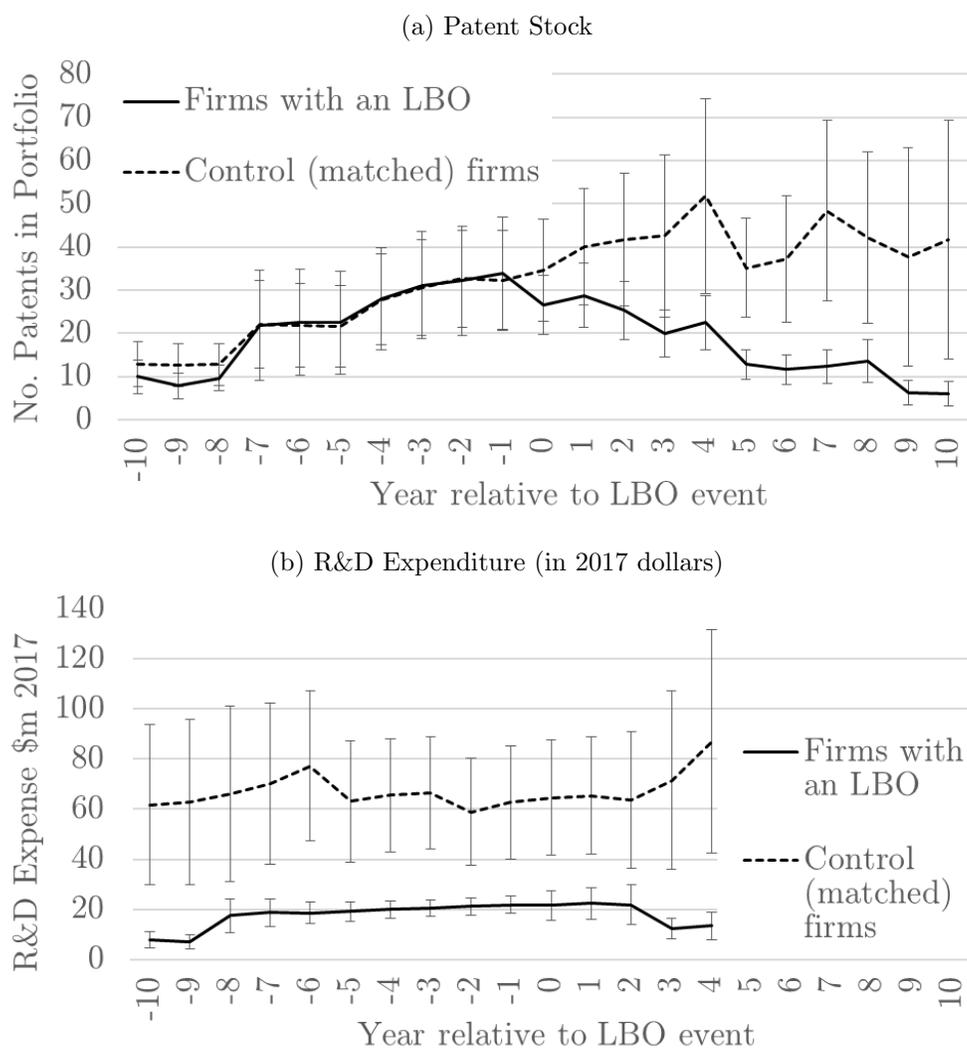
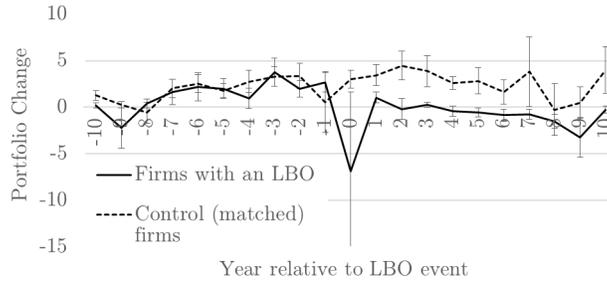


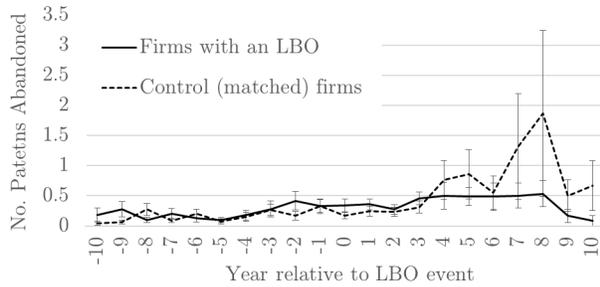
Figure 2: Sources of Change in Patent Flows

(a) Patent Flows



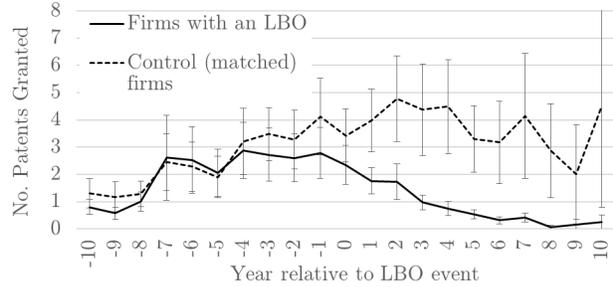
*Subtracting from Stocks:*

(b) Patent Abandonment (non-renewal)

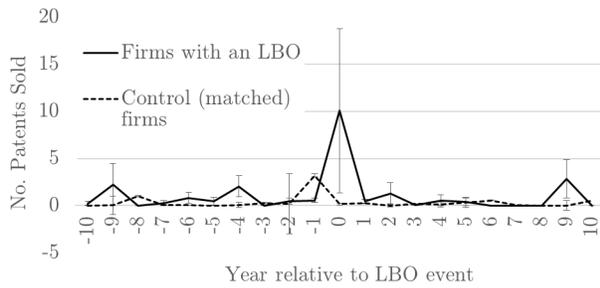


*Adding to Stocks:*

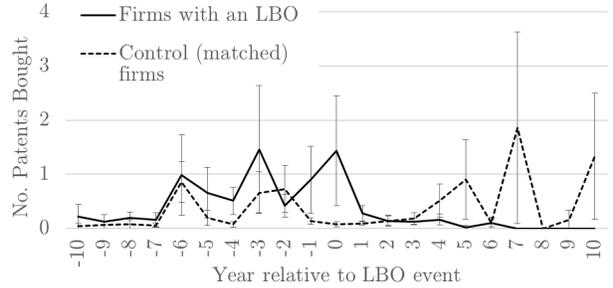
(c) Patent Grants



(d) Patent Sales



(e) Patent Purchases



(f) Patent Term Expirations

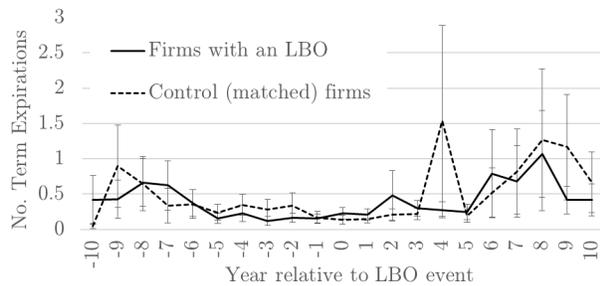


Figure 3: Patent Citation Statistics for LBOs and Control Firms

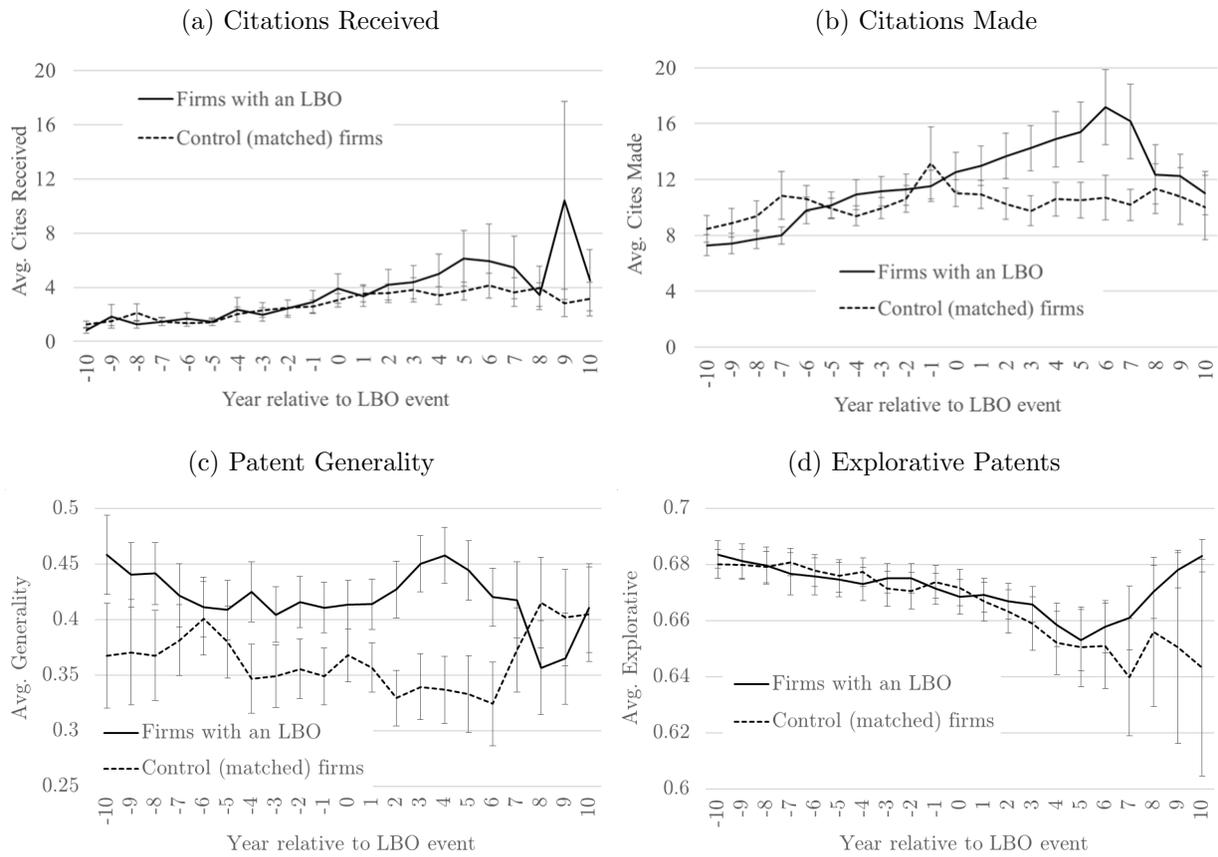


Table 1: Differences in Innovation between LBO and Other Publicly-traded Firms

Table 1 reports specifications where the explanatory variable is an indicator that takes the value 1 if a firm ever experiences an LBO and 0 otherwise. Year and industry fixed effects are used throughout. The first specification reports results of a logit regression where the dependent variable takes the value 1 if the firm ever has any patents and 0 otherwise. The unit of observation in this specification is a firm. In the other specifications, the unit of observation is a firm-year, the outcome measure is the log of one plus a count or continuous variable, and OLS is used. Data on R&D expenditure is not available for the majority of firm-years. Robust standard errors are in parentheses. \*\*\*, \*\*, and \* indicate p-values of less than 0.01, 0.05, and 0.1, respectively.

Specification	All Firms				Patenters
	Has patents (1/0)	Log patents	Log R&D	Log Intangibles	Log Patents
	1 <sup>st</sup>	2 <sup>nd</sup>	3 <sup>rd</sup>	4 <sup>th</sup>	5 <sup>th</sup>
Firm has an LBO (1/0)	-0.0605	0.0815***	-0.548***	1.000***	0.173***
	-0.139	(0.00950)	(0.0514)	(0.0667)	(0.0228)
Constant	-1.175***	-0.260***	-3.115***	-2.748***	-0.359***
	-0.0764	(0.00371)	(0.115)	(0.0777)	(0.0112)
Year & Industry F.E.	yes	yes	yes	yes	yes
(Pseudo) R2	0.199	0.147	0.393	0.163	0.189
No. Observations	12,903	588,280	88,009	166,869	207,630
No. Firms	12,903	12,903	4,571	12,681	6,931

Table 2: Descriptive Statistics

Table 2 shows descriptive statistics for firms that underwent an LBO in our sample in the next year. We separately report statistics for firms with one or more patents and those with no patents throughout their history.

	LBO firms with patents (138)				LBO firms w/o patents (210)			
	Obs.	Mean	Median	S.D.	Obs.	Mean	Median	S.D.
Total Assets (2017 \$m)	102	2,287	619	5,817	169	1,453	525	3,573
Revenue (2017 \$m)	102	2,492	725	6,257	168	1,650	548	4,890
EBITDA (2017 \$m)	101	317	74	770	161	195	60	632
Leverage	102	0.52	0.52	0.19	169	0.58	0.58	0.22
Tax (2017 \$m)	102	54	16	119	168	36	11	150
Tobin's Q	100	0.77	0.65	0.54	164	0.75	0.62	0.54
R&D Expenditure (2017 \$m)	64	21	12	24	69	4	0	10
Intangible Assets (2017 \$m)	83	454	42	1,488	144	267	32	959
IPO to LBO (yrs)	74	12.5	11	6.29	155	10.15	10	4.99
LBO Duration (yrs)	138	8.3	6	7.17	210	7.29	6	5.73

Table 3: Logit Regression for Propensity Score Matching

Panel A shows the results of a logit regression. The outcome is an indicator variable that takes the value 1 in the year that a firm has an LBO, and 0 if a firm is yet to, or has never had, an LBO. Observations of firms post-LBO are excluded. Only firms with one or more patents at some point, which operate in the same industries and at the approximately the same time as firms experiencing an LBO, are included in the sample. All explanatory variables are lagged by one year. Robust standard errors are in parentheses. \*\*\*, \*\*, and \* indicate p-values of less than 0.01, 0.05, and 0.1, respectively. Panel B shows the means of measures for control firms, and LBO firms, as well as the difference between them. The measures are the (lagged) regressors from the propensity score estimation, the estimated propensity score, and the year and lagged patent stock. \*\*\*, \*\*, and \* indicate p-values of less than 0.01, 0.05, and 0.1, respectively.

<b>Panel A:</b> Logit regression used for propensity score matching							
Parameter	Intercept	Leverage	Tobin's Q	SalesGth	Tax	ROA	Size
Estimate	-5.128	-0.695	-1.245	-0.0256	3.286	4.074	-0.0346
Standard Error	(0.395)	(0.470)	(0.215)	(0.0784)	(0.974)	(0.599)	(0.0384)
	***		***		***	***	
Model $\chi^2$		89.58	p < 0.001	Pseudo $R^2$	6.1%	N	54,275

<b>Panel B:</b> Comparison of LBO sample to benchmark sample							
Variable	Mean <sub>Control</sub>	Mean <sub>LBO</sub>	Difference	N <sub>Control</sub>	N <sub>LBO</sub>	t-value	p-value
<i>Leverage</i>	0.509	0.525	-0.016	92	92	0.503	0.616
<i>Tobin's Q</i>	0.72	0.779	-0.059	92	92	0.783	0.435
<i>SalesGth</i>	1.143	1.133	0.01	92	92	0.133	0.895
<i>Tax</i>	0.026	0.033	-0.006	92	92	1.277	0.203
<i>ROA</i>	0.128	0.148	-0.02**	92	92	1.824	0.700
<i>Size</i>	5.974	6.207	-0.233	92	92	0.989	0.324
Lagged Patent Stock	31.7	33.3	-1.7	92	92	0.990	0.921
Year	1995.8	1996	-0.2	92	92	0.166	0.868
Propensity Score	0.0031	0.003	0	92	92	0.043	0.966

<b>Panel C:</b> Test of equal means in LBO and control group						
Propensity matched control group test of H0: "No overall LBO effect"						
Statistics	Value	F statistic	NumDF	DenDF	Pr > F	
Wilks' lambda	0.9746	0.77	6	177	0.5955	

Table 4: Patent Stock Distribution (Among Patenters) at the Time of an LBO

The two panels show the distribution of patent stocks in the year of an LBO for the 92 firms that experienced an LBO, had 1 or more patents at some time, and had sufficient data to determine a matched control firm.

<b>Panel A: Patent Stock Statistics at <math>t=0</math></b>		<b>Panel B: Patent Stock Distribution at <math>t=0</math></b>	
Statistic	Value	Patent Stock	No. Firms
No. Firms	92	Zero Patents	30
Mean	26.5	One Patent	7
Standard Dev.	65.8	1 > No. Patents $\leq$ 5	11
Minimum	0	5 > No. Patents $\leq$ 10	11
10th Percentile	0	10 > No. Patents $\leq$ 20	12
Median	4.5	20 > No. Patents $\leq$ 50	10
90th Percentile	75	50 > No. Patents $\leq$ 100	6
Maximum	426	No. Patents > 100	5

Table 5: Effect of an LBO on Patent Stock and R&D Expenditure

The table reports results of OLS estimations. The dependent variables are one plus the log of the size of a firm's patent portfolio and of its R&D expenditure in millions of 2017 dollars. The explanatory variables are indicator variables. *Time* takes the value 1 if the firm is within the LBO process and 0 otherwise. *LBO* takes the value 1 if a firm experiences an LBO at some point, and zero otherwise. Observations of LBOs and control firms are paired together, so that both are included only if data for both is available and if the LBO has not concluded. Robust standard errors are in parentheses. \*\*\*, \*\*, and \* indicate p-values of less than 0.01, 0.05, and 0.1, respectively.

	Log Patent Stock	Log R&D Expenditure (\$m)
Time Indicator	0.626*** (0.101)	0.0323 (0.121)
LBO Indicator	-0.0828 (0.0846)	-0.329*** (0.0992)
Time $\times$ LBO	-0.281** (0.125)	0.0347 (0.182)
Constant	-1.028*** (0.113)	2.795*** (0.272)
Year & Ind. F.E.	yes	yes
Observations	2,753	1,332
R-squared	0.105	0.139

Table 6: Effect of an LBO on Patent Flow and its Components

Table 6 reports results of OLS estimations. The dependent variables describe the extent of activities that affect a firm's patent flow. The explanatory variables are indicator variables. *Time* takes the value 1 if the firm is within the LBO process and 0 otherwise. Observations of firms post-LBO are excluded from the sample. *LBO* takes the value 1 if a firm experiences an LBO at some point, and zero otherwise. Observations of LBOs and control firms are paired together, so that they are included only if data for both is available and if the LBO has not concluded. Robust standard errors are in parentheses. \*\*\*, \*\*, and \* indicate p-values of less than 0.01, 0.05, and 0.1, respectively.

	Log Flow	Log Grants	Log Purchases	Log Sales	Log Abandons	Log Expires
Time Indicator	0.0325 (0.0729)	0.147** (0.0610)	-0.00606 (0.0269)	0.00786 (0.0229)	0.0809*** (0.0257)	0.0727*** (0.0279)
LBO Indicator	-0.0866 (0.0590)	-0.0709 (0.0486)	0.0430* (0.0247)	0.0576** (0.0228)	0.00864 (0.0174)	-0.0254 (0.0218)
Time x LBO	-0.334*** (0.0883)	-0.227*** (0.0722)	-0.0715** (0.0337)	-0.00693 (0.0332)	-0.0349 (0.0309)	0.0507 (0.0341)
Constant	-0.161** (0.0788)	-0.374*** (0.0651)	-0.181*** (0.0328)	-0.121*** (0.0317)	-0.157*** (0.0255)	-0.134*** (0.0268)
Year & Ind. F.E.	yes	yes	yes	yes	yes	yes
Observations	2,753	2,753	2,753	2,753	2,753	2,753
R <sup>2</sup>	0.060	0.061	0.039	0.024	0.072	0.098

Table 7: Effect of an LBO on Patent Citation Statistics

The table reports results of OLS estimations, where the dependent variables are measures based on commonly-used patent citations statistics. The explanatory variables are indicator variables. *Time* takes the value 1 if the firm is within the LBO process and 0 otherwise. Observations of firms post-LBO are excluded from the sample. *LBO* takes the value 1 if a firm experiences an LBO at some point, and zero otherwise. Observations of LBOs and control firms are paired together, so that both are included only if data for both is available and if the LBO has not concluded. Robust standard errors are in parentheses. \*\*\*, \*\*, and \* indicate p-values of less than 0.01, 0.05, and 0.1, respectively.

	Log Cites Rec'd	Log Cites Made	Generality	Originality	Log Explorative	Log Exploitive
Time Indicator	0.302*** (0.0498)	-0.120*** (0.0371)	-0.0196 (0.0144)	0.00689 (0.0112)	-0.00683* (0.00377)	-3.26e-05 (0.00124)
LBO Indicator	-0.0616 (0.0409)	0.0542 (0.0356)	0.0602*** (0.0136)	-0.00308 (0.0119)	-0.00448 (0.00306)	-0.00111 (0.000981)
Time x LBO	-0.0196 (0.0665)	0.156*** (0.0491)	0.00938 (0.0179)	0.00667 (0.0162)	0.0104** (0.00497)	0.00142 (0.00159)
Constant	-0.340*** (0.0757)	1.883*** (0.198)	0.236*** (0.0735)	0.361** (0.147)	0.729*** (0.00466)	-0.00287** (0.00130)
Year & Ind. F.E.	yes	yes	yes	yes	yes	yes
Observations	2,753	1,878	1,874	1,855	1,877	1,877
R <sup>2</sup>	0.151	0.274	0.070	0.060	0.140	0.152