Public-to-Private Buyouts and Innovation

Douglas Cumming
DeSantis Distinguished Professor of Finance and Entrepreneurship
College of Business, Florida Atlantic University
777 Glades Road
Boca Raton, Florida 33432 USA
cummingd@fau.edu

Rejo Peter
DeepR Analytics, Co-founder & CEO
20 Bay St, Floor 11, Toronto,
Ontario M3J 1P3, Canada
rejopeter@deepranalytics.com

Monika Tarsalewska*
University of Exeter Business School
XFI building, Streatham Campus,
Rennes Drive, Exeter EX4 4ST, UK
m.tarsalewska@exeter.ac.uk

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*Corresponding author.
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Abstract

We study the effect of public-to-private buyout transactions on investments in innovation using an international sample over the 1997-2017 period. We use patent counts and citations to proxy for the quantity, quality, and economic importance of innovation. Our results are based on time analysis and matched sample regressions. The data indicate that buyouts are associated with a significant reduction in patents and patent citations, including a reduction of radical (i.e., more scientific) patents. When we split the sample into institutional and management buyouts, the negative effect of buyouts is confirmed only for institutional buyouts, suggesting that highly leveraged transactions prevent target firms from adopting long-term investments. This finding is confirmed by reductions in innovator employment and innovation efficiency subsequent to going private. Moreover, the data indicate that the negative effect is mostly prevalent for transactions where the cost of the deal's debt financing is higher than the post-buyout cost of the debt. We rule out alternative explanations for these findings, including but not limited to outliers, truncation bias, and endogeneity.

JEL Classification: G23, G24, O31

Keywords: Buyouts, Private Equity, Innovation

1. Introduction

The global economy has undergone a general shift in ownership structure over the past few decades. A significant share of firms are now owned by institutional investors from the private equity (PE) industry, yet their effects on target firms continue to be debated. PE firms acquire publicly listed firms, delist them, and restructure them. In principle, the existing theories suggest that, post-buyout transaction, the target firm should improve in terms of operating performance, investment, and productivity (Jensen, 1989). The intuition is straightforward: Private equity managers are value-added active investors that put into place efficient incentive and monitoring mechanisms, together with debt discipline, to enhance firm productivity and performance (Ahlers et al., 2017; Amess et al., 2016; Cornelli and Karakaş, 2015; Jensen, 1989).

On the other hand, however, critics argue that PE firms are transitory organizations (Kaplan, 1991). They have an overly strong focus on projects with short-term payoffs, and tend to reduce investments in long-term projects in order to ensure they can meet their debt servicing obligations (Rappaport, 1990). One example of the "dark" side of PE deals is the buyout of Debenhams, a public-to-private deal that took place in 2003 in the U.K. This deal generated enormous profits for the PE owners, but left the firm with massive debt, and its value plummeted after the IPO.[1] In subsequent years it was not able to service its massive debt and was taken by its lenders in 2019.[2] Another example is the $24 billion buyout of Dell Technologies Inc. by PE firm Silver Lake in 2013, which currently stands as the largest technology firm buyout. By 2018, the company was in tatters, with its financial position described as:

[1] https://www.ft.com/content/6fd92a0c-437d-11dc-a065-0000779fd2ac.
[2] https://www.ft.com/content/b784e306-5aad-11e9-9dde-7aedca0a081a

Dell Technologies Inc. seems to be taking a Donald Trump-like approach to determining its self-worth: bold statements, and not a lot of information to back them up… Dell is saddled with a boatload of debt and a messy capital structure…[3]

As a result, the industry as a whole, as well as academics, has begun to question the positive effects of such short-term thinking.[4]

Additional research has explored the effects of taking firms private in buyout transactions, but the empirical evidence is decidedly mixed. In fact, recent empirical evidence has provided some puzzling results about the real outcome of buyout transactions. Several studies show positive effects of buyouts on the productivity and innovation of target firms (Lerner et al., 2011; Davis et al., 2014; Amess et al., 2016); others have questioned the results of performance and productivity improvements following buyouts (Bharath et al., 2014; Cohn et al., 2014; Weir et al., 2015; Ayash and Schütt, 2016; Ayash and Rastad, 2017; Goergen et al., 2014a, 2014b).

With these questions in mind, we revisit how public-to-private buyout transactions impact long-term investment in innovation. Although many papers have studied operating performance, productivity, and employment changes post-buyout, more compelling evidence about the overall effect of buyouts on innovation would be instructive. Davis et al.'s (2014) influential paper on buyouts and productivity call in particular for research to examine deals

[3] https://www.bloomberg.com/opinion/articles/2018-07-17/dell-dvmt-vmw-buyout-math-doesn-t-compute.

[4] For example, Laurence Fink, the CEO of BlackRock, said in 2015 that: *"The effects of the short-termist phenomenon are troubling (...) In the face of these pressures, more and more corporate leaders have responded with actions that can deliver immediate returns to shareholders, such as buybacks or dividend increases, while underinvesting in innovation, skilled workforces or essential CAPEX necessary to sustain long-term growth."* More recently, Elon Musk failed in his attempt to take Tesla's stock private in an effort to avoid the public pressures of the stock market. Stephen Diamond, associate law professor at Santa Clara University, described him as follows: "*Musk represents the leading edge of an unfortunate Silicon Valley trend: the narcissistic CEO and the board that lacks the gravitas, experience and independence to consider ordinary investors' interests" (*https://www.sfchronicle.com/business/article/Tesla-shareholders-reject-move-to-split-CEO-12970015.php).

executed during the most recent buyout wave of 2006-2007 and the post-2008-2009 global financial crisis.

In this paper, we use a comprehensive sample of public-to-private buyout transactions and a dataset that covers the most recent buyout wave and financial crisis. We study two specific types of transactions: institutional buyouts (IBO), and management buyouts (MBO). In an IBO, the PE fund acquires a controlling interest in the target firm, hires new management, and typically exits within five years; in an MBO, current management takes a large ownership stake in the company. We note that prior work on buyouts and innovation generally preceded the global financial crisis, and focused largely on U.S. and U.K. data.

To study how public-to-private buyouts impact long-term innovation, we use a unique international dataset over the 1997-2017 period. It is created by merging several databases, including Zephyr, Orbis, and PATSTAT. We propose a firm-level measure of innovation from EPO's Worldwide Patent Statistical Database (PATSTAT). We note that most prior studies used R&D expenditures or innovations registered with the U.S. Patent and Trademark Office (USPTO), but both measures have limitations. Our measures of innovation are based on patents registered in each country's office. PATSTAT provides data on more than 80 million patent applications filed in over 100 patent offices around the world. The depth of the data offered by PATSTAT allows us to create measures such as radical innovation and innovation efficiency that have not been used in previous studies.

Our tests are based on before-after buyout analysis with fixed effects and difference-in-difference methodology for a buyout and control sample of public firms. We find that, in general, buyouts tend to reduce investments in innovation.

We distinguish between institutional and management buyouts because buyout type affects investors' aims and incentives. We also find that, in the case of public-to-private buyouts by institutional investors, the effect of the transaction on innovation remains negative. The analysis of public firms taken private by management is inconclusive.

We find a consistent negative effect following public-to-private buyouts for a sample of firms that engaged in what we refer to as radical innovation, i.e., a higher level of scientific innovation, which cites non-patent literature.

Furthermore, we test whether the target firm becomes more efficient in terms of innovative activities. We develop a new measure of innovation efficiency, whereby we take the number of applications filed and subsequently granted during the year, divided by the number of unique innovators. We find that innovation efficiency decreases after a public-to-private buyout.

We also try to explain the underlying reasons for the negative effect of buyouts on innovation, and find it is mostly driven by the relative cost of debt. In particular, we find that, if the acquirer cannot lock in the financing for the buyout transaction at a lower rate than the other market participants' cost of debt, then this may negatively impact investment and therefore innovation.

This article contributes to the literature on the effects of ownership changes on innovation, and, in particular, the effects of buyout transactions on innovation. Lerner et al. (2011) find that innovation increases after LBOs. Their U.S.-based sample goes through 2005, and the vast majority of their deals are private-to-private transactions. Similarly, Amess et al. (2016) show an increase in innovative activity for their sample of U.K. deals, although they state that most of the effect comes from private-to-private transactions.

Our study, in contrast, differs from prior papers in a number of ways. For example, we study public-to-private buyouts, our sample is international, and we capture a different time span, covering the global financial crisis and the second LBO wave (2005-2007). We provide evidence based on “time-trend” analysis for the buyout sample, as well as “difference-in-differences” results for the matched sample, which mitigate potential endogeneity concerns. Importantly, we also distinguish between management and institutional buyouts, which affect changes in innovation in very different ways.

The rest of this paper is organized as follows. Section 2 discusses the research design. Section 3 presents data, while section 4 discusses the main results. Section 5 presents additional analysis. Section 6 concludes.

2. Literature Review and Hypothesis Development

Prior literature suggests that ownership structure plays an important role in corporate innovation, because it represents the financing choices, governance, and incentives of the owners. An early study by Aghion and Tirole (1994) explores the existence of innovation under different structures. Belenzon, Berkovitz, and Bolton (2009) suggest that companies choose the corporate form that is the most conducive to undertaking research and development. Many studies have also examined how various ownership forms affect innovation, focusing explicitly on short- versus long-term value creation.

For example, firms may not invest in long-term projects due to short-term performance pressures (Stein, 1988; Graham et al., 2005; Bushee, 1998, 2001). Owners may expropriate firm resources and impede innovative activities. Manso (2011) suggests that, ideally, organizing and motivating systems should build in a certain amount of tolerance for failure, as well as reward for long-term success. Moreover, Ferreira et al. (2012) show that going public is optimal when exploiting existing ideas, and going private is optimal when exploring new ideas. Empirical evidence shows that innovation generally declines after private firms go public (Bernstein, 2015).

Alternatively, some owner types may enhance innovative activities. Owners can act as active monitors, and encourage management to invest in long-term projects that enhance innovation (Shleifer and Vishny, 1986; Kahn and Winton, 1998; Burkart et al., 1997; Gillan and Starks, 2000). For example, Aghion et al. (2013) show that institutional ownership is associated with more innovation. The recent study by Boot and Vladimirov (2018) show that ownership and innovation can even exhibit a U-shaped relationship when we take into account market collusion, where public ownership nurtures innovation when the probability of success is either low or high. Financing of innovation also matters. Atanassov et al. (2007) show that public firms innovate more if they rely on equity or public debt.

The buyout transaction is a particular form of ownership change, generally undertaken by PE firms or firm management using a substantial external source of funding (usually debt). Intuitively, the purpose is to restructure the target firm. The investors aim to install more efficient incentive mechanisms and monitoring, and to improve corporate governance and capital structure (Ahlers et al., 2017; Amess et al., 2016; Cornelli and Karakaş, 2015; Lerner et al., 2011; Jensen, 1989).

However, although the intended goals of public-to-private buyouts are to improve target firm performance, the debt burden may ultimately have a negative effect on its long-term investment. Kaplan (1991) states that PE firms are transitory organizations that focus on projects with short-term payoffs, while reducing investments in long-term projects. Rappaport (1990) claims that debt discipline and concentrated ownership can impose significant adjustment costs. Debt significantly increases the leverage of target firms, and default risk becomes a primary concern. Moreover, financing is often sourced from multiple debt providers, so refinancing becomes more difficult to achieve (Demiroglu and James, 2010; Graham and Leary, 2011; Colla et al., 2012; Axelson et al., 2013).

We distinguish further between IBOs and MBOs, the two types of buyouts. In the case of IBOs, the PE fund, as the owner, is in fact an intermediary that must provide returns to its investors. PE firms represent limited partners that provide the funding for their investments. The limited partners typically expect to be repaid within five to ten years. Therefore, although the investment search for the PE fund may take two to three years, the actual turnaround period can last for three to seven years. The most profitable exit for the PE fund is the return of the target firm to public ownership, and most exercise that option between the second and fifth year after buyout (Kaplan, 1991). Therefore, PE funds' investment horizons are generally up to five years.

In contrast, in the case of MBOs, management, together with debtholders, takes the firm private. Management typically takes large equity stakes in the firm.

Thus, theory remains somewhat unclear about the actual effect of public-to-private buyout transactions on long-term investment, but the empirical evidence does not offer a compelling answer either. The literature has mostly debated the effects of buyout transactions on operating performance, productivity, and employment, with mixed results. We summarize below.

Early evidence suggests that the impact of PE leveraged buyouts (LBOs) and MBOs showed positive effects on productivity based on plant-level data (Lichtenberg and Siegel, 1990). There is also some evidence of improved operating performance during the first buyout wave (Kaplan, 1989; Baker and Wruck, 1989; Smith, 1990). Davis et al. (2014) show further that, while buyouts could lead to job losses, they also tended to bring improvements in productivity. Guo et al. (2011) also find evidence of a positive effect on productivity after a buyout. Similarly, Acharya et al. (2012) and Weir et al. (2015) find small improvements in operating performance post-LBO for a U.K. sample, and Boucly et al. (2011) and Bergström et al. (2007) find larger operating improvements post-LBO for other countries. Harford and Kolasinski (2013) study wealth creation at the time of PE investor exit, and do not find any evidence for the short-termism view of buyouts.

Other studies, however, present a different view of the effect of buyouts on target firm efficiency. Bharath et al. (2014) use a U.S. sample, and find that going private does not seem to change firm productivity. In fact, they find some evidence of underinvestment. Leslie and Oyer (2008) and Cohn et al. (2014), using a sample of U.S. LBOs, find little or no evidence of operating improvements following a buyout. Similarly, Ayash and Schütt (2016) find no economically significant improvement in operating performance following buyouts, and Ayash and Rastad (2017) question productivity improvements claimed in prior literature. In a U.K. buyout context, Goergen et al. (2014a, 2014b) show that the performance and productivity of IBOs decrease after the transaction.

Furthermore, the effect of buyouts may depend on investor type, as Ughetto (2010) finds for private-to-private transactions in Europe. There is some evidence that PE IBOs tend to have a negative effect on employment and productivity (Goergen et al., 2014a, 2014b; Guery et al., 2017), but the opposite is shown for MBOs. Kaplan (1989), Smith (1990), and Smart and Waldfogel (1994) all find very large improvements in the 1980s after U.S. MBOs.

The effect of buyouts on innovation has not attracted sufficient attention, however, and there are few empirical tests. Evidence for the U.K. for 1998-2005 shows that the effect is more pronounced for private-to-private deals (Amess et al., 2016). Lerner et al. (2011) use a U.S. sample over 1983-2005, and find a positive effect of PE on innovation.

To summarize the theoretical arguments and extant empirical evidence, it is not clear ex ante whether public-to-private transactions have a positive or negative effect on innovation. On the one hand, the improvements in corporate governance, managerial incentives, and discipline should positively impact innovation. On the other hand, the debt burden and the short-term constraints imposed on PE investors can significantly hamper innovative activity. Also, the effect may differ depending on investor type (institutional or management). Ultimately, we set our null hypotheses as follows:

H0A: Institutional buyouts (IBOs) have no effect on innovation.

H0B: Management buyouts (MBOs) have no effect on innovation.

2. Research Design

Our research design focuses on two sets of results. First, we analyze the "before-after" time trends for the sample of firms that went private. We compare the level of innovation after going private to the level when the firm was public. Second, we implement "difference-in-differences" (DiD) tests to analyze the changes in innovation of going-private firms compared to a control group of matched firms that remained public.

2.1. The "before-after" methodology

In order to examine the changes within the going-private group, we run the following regression:

$$y_{i,t} = \alpha + \beta_k D_k + \theta\ Controls_{t-1} + FE + \varepsilon_{i,t} \tag{1}$$

where $y_{i,t}$ is the outcome variable (innovation measures), D_k are dummy variables that equal 1 for the year k after the buyout transaction (negative values correspond to years before the buyout), *Controls* is a vector of country and firm characteristics, and *FE* are firm- and year-country fixed effects. The term $\varepsilon_{i,t}$ stands for residual error.

2.2. The "difference-in-differences" methodology

The "before-after" analysis of innovation for firms that went private is ultimately driven by country-, industry-, or firm-related characteristics such as age and size. In order to eliminate this potential source of endogeneity, we form a matched control group for each going-private firm in our sample of buyouts. Similarly to the procedure for the going-private firms, we first ensure that the control group firms have patent activity. Then, we select up to five matched control firms that remained public based on country, industry, event year, age and size. We thus have "cells" of one going-private firm and up to five matched controls. We delete "cells" where the number of control firms is lower than three. We estimate the following regression model:

$$y_{i,t} = \alpha + \boldsymbol{\delta_k}\ \boldsymbol{Buyout} * \boldsymbol{D_{k+}} + \gamma\ Buyout + \beta_k D_k + \varepsilon_{i,t} \qquad (2)$$

where $y_{i,t}$ is the outcome variable (innovation measures), and D_k are dummy variables that equal 1 for the year k after the buyout transaction (negative values correspond to years before the buyout). The term $\varepsilon_{i,t}$ stands for residual error.

3. Data

3.1. Sample construction

To establish our sample, we first obtain buyout transactions from the Zephyr database.[5] We only analyze deals where the acquirer bought 100% of the listed target firm. We choose the Zephyr database because it shares common identities with the Orbis database. We then merge the Zephyr and Orbis datasets with the detailed patent data derived from EPO's Worldwide Patent Statistical Database (PATSTAT). This database provides data on more than 80 million patent applications filed in over 100 patent offices around the world. It contains basic bibliographic information on patents, including the date of application, the date the patent

[5] Zephyr has been used in previous studies, such as, e.g., Erel et al. (2015).

is granted, the track record of patent citations, and inventor identification for each patent application. PATSTAT is published biannually; we use the 2017 Autumn edition.

The PATSTAT database covers patents filed in ninety-three non-U.S. countries. It therefore provides even greater coverage than the National Bureau of Economic Research (NBER) Patent and Citation database, which is compiled from information in the United States Patent and Trademark Office (USPTO) (Moshirian et al., 2015). The USPTO only aggregates U.S. the patents filed in the US.

In summary, using databases that share common identifiers allows us to avoid many pitfalls. Both Zephyr and Orbis are provided by the same supplier, Bureau Van Dyck, so we can match deal information from Zephyr to firm-level data, and from Orbis more accurately. We further match these data with PATSTAT. Using PATSTAT data, we can directly measure firms' innovation levels, regardless of where the patent application was filed.

We include all completed buyout transactions from 1997-2011 for a total of twenty one countries including Austria, Belgium, Canada, Switzerland, Czech Republic, Germany, Denmark, Spain, Finland, France, United Kingdom (UK), Israel, Italy, Japan, Korean Republic, Netherlands, Norway, Poland, Sweden, Singapore, and United States (US). Our sample is mostly dominated by the deals from the US, followed by Canada, Japan, France, UK, and Germany. Our sample of buyout deals terminates in 2011, because we require six years' of post-buyout patent data in order to construct the patent citation measures. We only include buyout deals where the target firm had at least one successful patent applied for and granted from the three years prior to the transaction to the three years afterward (similarly to Lerner et al., 2011). Our final sample is comprised of 307 go-private deals, involving 26,360 patents.

3.2. Measuring innovation

Our primary goal is to measure innovation quantity and quality. We use a simple patent count to proxy for innovation quantity. In order to evaluate the quality and importance of innovation, we use two other measures. The first is absolute citation count, which captures the citations made within the three-year period, beginning from the year of the patent grant date and ending three years afterward. We use this measure to mitigate the issue of truncation at the end of the sample. The second is relative citation count. This measure calculates the citations received for patents filed and subsequently granted during the year of the patent grant through the three years afterward, less the average number of citations during the period received by the matching patents. We follow Lerner et al. (2011), and define matching patents as those granted in the same year and assigned to the same technology class.

Because absolute and relative citation measures require three years' of forward patent data, and because our study requires citation measures for three years from the date of the buyout, we require a total of six years' of patent data from the date of the buyout. This limits us to considering buyout transactions up to 2011.

We also analyze the novelty of innovation. Following Griffith and Macartney (2014), we consider a patent as radical if it has at least one citation to non-patent literature (NPL). NPL generally refers to scientific journals, and therefore, patents making citations to NPL are likely to be new and to represent a radical innovation.

3.3. Control variables

There are many factors that drive innovative activity at the country and firm level. Following previous literature on innovation, we control for these characteristics. In particular, we include the intellectual property protection index created by Park (2008), and the level of a country's innovativeness as measured by patent applications scaled by GDP.

Nanda and Rhodes-Kropf (2013) and Hsu et al. (2014) show that financial market development affects innovative activity. Thus, we include equity development measures as proxied for by the value of shares traded and scaled by GDP, and two credit market development measures. CMD1 is domestic credit to the private sector. This is an important indicator of the ability to finance production, consumption, and capital formation, which in turn affect economic activity. CMD2 is domestic credit provided by the financial sector scaled by GDP, which measures banking sector depth and financial sector development in terms of size. We also include the GDP growth of a country to proxy for general economic conditions.

At the firm level, the vector of control variables includes firm size (SIZE), firm age (AGE), and profitability (ROA). We winsorize all variables at the 1st and 99th percentiles to eliminate the effects of outliers. We provide all definitions of variables and data sources in Appendix AI.

3.4. Sample characteristics

Table 1 gives the summary statistics. We first present the yearly distribution in panel A. Note that the number of patents increased significantly from the year 2000. Similarly to Lerner et al. (2011), we attribute this to the increasing volume and growing share of technology firms, which typically innovate to a greater extent. There was another sharp increase in the number of public-to-private buyout transactions after 2005, primarily due to the cheap access to credit that was the main source of financing for LBOs. Subsequently, in 2008, the number of deals plummeted, to a level not seen since the late 1990s. This was attributable to the financial crisis, which caused a total standstill in deals, although the number began to rise again soon afterward.

In panel B of Table 1, we show the distribution of transactions by industry. Similarly to previous studies (e.g., Lerner et al., 2011), we find that manufacturing industries dominate in our sample.

[Table 1 here]

4. Innovation Changes Analysis

4.1. Summary statistics

Table 2 presents the descriptive statistics for the full buyout sample. The firms targeted in the buyout transactions have an average of 11.26 patents. The relative and absolute citations are, at 10.68 and 5.40, respectively. The mean number of radical patents is 1.29.

Our country-level controls, such as the measure of Intellectual Property Rights (IPR), have a mean of 7.98. The country innovativeness intensity (INV) measure implies there are 1.71 patent applications submitted per 100 million of GDP, measured in U.S. dollars. Equity market development (EMD) has a mean value of 173.68. Mean credit market development measured as private credit to GDP (CMD1) is 165.51, and measured as domestic credit provided by the financial sector to GDP (CDM2) it is 198.25. Average GDP growth is 2.20.

Note that we have only a limited amount of data at the firm level. Therefore, our sample size drops by almost one-half. Nevertheless, we opted to present some controls related to the financial position of the target firm. On average, target firms have total sales of 11.79 million Euro, are thirty-eight years old, and have negative return on assets.

[Table 2 here]

4.2. Baseline regressions

In the multivariate analysis, we use patent count and citations as dependent variables. Given that the patent count variable is highly skewed, we transform it into *ln(1+patent count)* in the regression analysis. In column (1) of Table 3 we present the results of "*before-after*" analysis where we include industry, firm, and country-year fixed effects and we cluster the standard errors by firm. We find a significant decline in the number of patent applications post-buyout transaction, ranging from 18% in year 2, and up to 21% in year 3.[6]

The innovation drop may be due to the fact that PE firms tend to buy certain firms. In this analysis, we match the buyout firms to the public firms by age, profitability, year, and country, in order to mitigate those concerns. Our empirical tests are based on DiD methods, where we compare the change in innovation among firms that went through a public-to-private buyout (the treatment group) with the change in innovation among a matched group of public firms that remained public (the control group). In column (2) of Table 3 we present the results from the DiD methodology discussed in subsection 2.2. The results resemble similar pattern showing a drop in the number of patents of 17% in year 2, and 22% in year 3 compared to public firms matched by year, size, three-digit industry, and age.

[Table 3 here]

In column (1) and (2) of Table 4 we present the results of "*before-after*" analysis where the dependent variable are *Absolute Citations* and *Relative Citations*, respectively. We include industry, firm, and country-year fixed effects and we cluster the standard errors by firm. We find a significant decline in the number of patent applications post-buyout transaction, ranging from 32 to 41% in year 2, and up to 36 to 46% in year 3.[7] In column (2) of Table 4 we present the results from the difference-in-differences methodology. The results show a decline in the number of patents of 24 to 33% in year 2, and 22 to 30% in year 3 compared to public firms matched by year, size, three-digit industry, and age.

[Table 4 here]

[6] The untabulated results are robust if we include country and firm characteristics, yet including them reduces the number of observations.
[7] The untabulated results are robust if we include country and firm characteristics, yet including them reduces the number of observations.

4.3. Addressing endogeneity of the going-private decision

A decision to delist the public firm is not random and therefore our analysis is subject to endogeneity. In the previous section we try to mitigate this concern by performing the DiD analysis by matching on industry, size, age, and year. In this subsection, we extend this analysis by employing two alternative matching methods. First, we construct a sample of matched firms that are similar in terms of going-private characteristics. In particular, following Bharath and Dittmar (2010) we identify several characteristics that were identified as future predictors of going-private. We identify a sample that is similar to the buyout sample in terms of total assets, sales, R&D, Capex, dividend, free cash flow, debt, cash, net fixed assets[8] and that are measured at the time of IPO that is on average 13 years before going-private. Similarly, as discussed in subsection 2.2 we include the "cell" effects.

Second, in order to mitigate the concern that the pre-buyout innovation might affect the results we construct a sample of matched firms that are similar in terms of pre-innovation characteristics. That should resolve the problem that we just analyse firms that are at the different timeline of innovation cycle. We present the results from these two alternative DiD analysis in Table 5. In columns (1) and (2) we present the DiD results when firms are matched on the going-private characteristics, and in columns (1) and (2) when they are matched on 3 years pre-innovation, industry, size, age, and year. The results also show a negative effect of going-private transaction on innovation.

[Table 5 here]

5. Further Analysis

5.1. Institutional and management buyouts

[8] There are many missing values for those variables. When possible we replace the missing values by industry-year averages.

In this subsection we distinguish between IBOs and MBOs. This is because we expect institutional investors to have different incentives and long-term objectives than insiders such as firm management. Theoretically, going private in a highly leveraged IBO transaction does not relieve the target firm from short-term pressures. In fact, servicing a huge debt might preclude the firm from realizing any long-term investment strategies. In contrast, in an MBO, the insiders may be focused on servicing debt as well as long-term planning. They may have reputational and career concerns, and, as a result, they may aim to keep the firm in solid shape after returning it to the public sector.

Table 6 Panel A shows "before-after" and DiD results for limiting the sample to IBOs only. We find a significant drop in both absolute and relative citations following IBOs. Table 6 Panel B shows our results for limiting the sample to MBOs only. Within all models we observe no statistically significant effect on absolute or relative citations post-buyout. These results indicate that the negative effect from buyouts is predominantly observed for IBOs, but not for MBOs.

[Table 6 here]

5.2. Radical innovation

Thus far, we have analyzed various general measures of innovation. However, the nature of innovation can differ. Certain patents, for example, may refer directly to scientific literature, and therefore be considered more radical than incremental in nature. We thus define radical innovation as the total number of patents granted to firm i in year t that have at least one citation to a non-patent literature. In order to identify the effect of buyouts on radical innovation, we limit our sample to target firms that had at least one radical patent applied for and granted within a period of three years before to three years after the buyout.

The results are in Table 7. We find that the number of radical innovations tends to drop after the buyout transaction. We observe a statistically and economically significant decrease in radical innovation one, two, and three years post-buyout.

[Table 7 here]

5.3. Innovation efficiency or short term-payoffs?

In previous subsections, we demonstrated that innovation generally drops after going private. This may be due to PE firms restructuring R&D departments. We therefore look next at innovator employment changes. If the PE firms are focused on long-term investment projects, we expect them to expand and keep the R&D units operational. Alternatively, if their focus is solely short term, we expect to observe employment reductions in innovator employment.

We create a novel measure of innovation efficiency, computed as the number of patent applications filed and subsequently granted during the year, divided by the number of unique innovators. We consider unique innovators as those listed on the patent application. If the same person is included in multiple applications, we count that person only once. This measure also considers how efficiently a firm uses its R&D team following a buyout. Innovation efficiency can be improved by either increasing the number of patent applications while keeping the size of the R&D team constant, or by producing the same number of patent applications using a smaller R&D team.

The results for innovation efficiency are in Table 8. Similarly to the findings for patent counts, radical patent counts, absolute citations, and relative citations, we find that buyouts have a significant negative effect on innovation efficiency.

[Table 8 here]

5.4. Buyouts and the cost of debt

Now that we have shown that innovation drops after going private, the question is: Why do PE firms pay for positive net present value (NPV) projects, and then abandon them afterward? It would be illuminating to examine the underlying reasons for the post-buyout drop in innovation. Deep pocket investors may seem more likely to nurture innovation, as we expect they would be able to tolerate short-term failure. However, they frequently rely heavily on debt financing. The debt overhang theory of Myers (1977) posits that management of an excessively leveraged firm will forgo positive-NPV projects if the new projects benefit debtholders rather than equityholders.

In general, the buyout transaction is not only related to the change in ownership, it also changes the target firm's capital structure and shifts it toward higher leverage. The buyouts are financed mostly with debt, so as much as 80% of the transaction cost may be debt financing. At the time of the buyout announcement, the acquirer and the lender have agreed upon the terms and the payout structure. The debt part can be a significant burden for the planned restructuring of the target firm during the buyout period, however. Also, financing may be received from multiple debt providers, which makes it more difficult to effect a refinancing (Demiroglu and James, 2010; Graham and Leary, 2011; Colla et al., 2012; Axelson et al., 2013).

Most prior studies analyzed the effects of buyouts on investment and productivity in isolation. However, the investment and financing decisions are typically not separate. In this subsection, we analyze the effects together. It is critical for the acquirer to negotiate the best debt terms for the buyout transaction. If the acquirer can lock in the deal financing at a lower rate than other market participants for the subsequent post-buyout years, it will have a critical investment advantage over the competition. Thus, the effect of the buyout on innovation should be positive. But the reverse also holds: If the acquirer locks in the deal financing at a higher rate than other market participants, this may have a negative effect on its investment, and on innovation.

In order to control for the cost of debt, we include in the regression analysis the relative ratio of the initial cost of debt at the time of announcement, and the cost of debt in the first, second, and third years post-buyout, respectively. Data on the cost of debt comes from FRED Economic Data, and we use the corporate debt yield at the time of announcement at the subgroup country level.

Our results are presented in Table 9 It shows that the effect of the relative cost of debt after the buyout transaction negatively affects innovation. In particular, the CD (cost of debt) in year 1 after the buyout, that is, the ratio of the cost of debt at the time of the announcement to the cost of debt at the first year post-announcement, has a negative and significant effect on innovation. The CD in year 2 post-buyout, that is, the ratio of the cost of debt at the time of the announcement to the cost of debt at the second year post-announcement, has a negative and yet not significant effect on innovation. The CD in year 3 post-buyout, that is, the ratio of the cost of debt at the time of announcement to the cost of debt at the third year post-announcement, has a negative and significant effect on innovation. Overall, this means the decrease in the post-buyout cost of debt compared to the initial cost of debt at the time of announcement has a negative effect on innovation.

Interestingly, the effect of the post-buyout years becomes positive in years 1 and 3, suggesting that the effect of the buyout on innovation is dependent on the relative cost of debt at the year of announcement relative to the current cost of debt. We posit that if the acquirer is able to lock in at the lower cost of debt for the time of the restructuring, compared to the post-buyout cost of debt, then the incentives to innovate are stronger. Yet, if the current cost of debt is lower than the cost of debt at announcement, the investment in innovation will no longer be lucrative, and the incentives to innovate decrease.

[Table 9 here]

6. Conclusion

This paper explores the impact of public-to-private buyout transactions on the innovation of target firms. We analyze both quantity (patent count) and quality (citations) of patent activity, and find that, following public-to-private buyouts, firms tend to have fewer patents overall, and to receive fewer citations on those patents. We also show that firms have fewer radical (e.g., scientific) innovations.

We determine that the drop in innovation is likely attributable to the fact that excessive debt and servicing costs can hinder target firms from adopting long-term strategies. First, we observe that the negative effect of public-to-private buyouts is only significant for institutional buyouts. Second, we identify a significant decrease in innovation efficiency post-going private. Third, we show that the negative effect is most prevalent for transactions where the cost of the debt financing is higher than the post-buyout cost of debt.

Our results both add to the previous literature, and contrast with Lerner et al. (2011) and Amess et al. (2016), who show innovation increases after buyout transactions. However, their results are mostly driven by private-to-private transactions. Our study contributes by showing contrasting results for public-to-private transactions. The evidence is based on buyout sample and matched sample analysis.

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Table 1. Sample distribution

This table presents the sample construction and distribution of sample by announcement year (panel A), target industry (panel B), target country (panel C), and deal type (panel D) for deals announced in 1997 to 2011 with at least one patent granted to the target firm three years before to three years after the transaction.

Panel A: Distribution by year

Year	Deals #	Patents #	Deals # with Radical Patents	Radical Patent #
1997	1	9	-	-
1998	2	104	-	-
1999	14	186	3	7
2000	16	2,431	10	93
2001	13	830	9	53
2002	14	410	3	16
2003	35	4,314	16	591
2004	19	725	11	25
2005	34	2,455	17	106
2006	30	3,967	15	1,024
2007	58	7,149	31	761
2008	19	520	9	47
2009	13	909	10	107
2010	14	243	11	73
2011	25	2,108	13	191
Total	307	26,360	158	3,094

Panel B: Distribution by industry

Industry	Deals#	Patents#	Deals# with Radical Patents	Radical Patents#
Agriculture	2	92	2	6
Construction	1	1	-	-
Finance, Insurance	2	954	2	192
Manufacturing	187	23,888	94	2,608
Mining	3	12	1	1
Retail Trade	13	171	5	25
Services	80	1,108	44	237
Transportation	14	110	9	24
Wholesale Trade	5	24	1	1
Total	307	26,360	158	3,094

Table 2. Summary statistics

This table presents the summary statistics for deals announced from 1997-2011, with at least one patent granted to the target firm for three years before to three years after the transaction.

Variable	Mean	S.D.	25%	Median	75%
Innovation Variables					
Patent Count	11.26	31.96	0.00	1.00	5.00
Radical Patent Count	1.29	4.52	0.00	0.00	0.00
Absolute Citation	10.68	41.14	0.00	0.00	0.00
Relative Citation	5.40	23.57	0.00	0.00	0.00
Country-level Variables					
IPR	7.98	0.53	8.00	8.00	8.20
INV	1.71	1.73	1.29	1.59	1.66
EMD	173.68	89.24	87.64	162.97	240.74
CMD1	165.51	42.46	161.68	180.02	192.13
CDM2	198.25	54.67	184.20	216.32	231.45
GDP_GR	2.20	1.69	1.68	2.53	3.08
Firm-level Variables					
SIZE	11.79	2.14	10.74	12.10	13.09
AGE	38.14	32.78	18.00	28.00	48.00
ROA	-0.041	0.46	-0.03	0.05	0.10

Table 3. Changes in innovation around the "going-private" decision: BA and DID specifications (from both going-private and control samples)

Columns (1) and (2) present OLS panel regressions for Before-After analysis where the dependent variable ln(1+number of patents) in model (1) and (2). In model (1) we include industry, firm, and country-year fixed effects. Standard errors are clustered by firm. Column (2) presents difference-in-differences regression results. For each firm in the going-private sample, we include up to five public firms (based on data availability) that are matched to the going-private firms by year, size, three-digit industry, and age. t-statistics based on standard errors clustered by industry-size-age cells are in parentheses. In all models, we show the regression where the independent variables are the relative years pre- and post-buyout (event year 0 is the omitted base category, with a coefficient normalized to 1). All variables are defined in the Appendix. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.

	Before-After		DiD	
	(1)		(2)	
	Coeff.	t-stat	Coeff.	t-stat
Event year −3	0.0009	[0.01]	0.0395	[0.56]
Event year −2	0.0904	[1.17]	0.1061*	[1.78]
Event year −1	-0.0039	[-0.06]	0.0404	[0.79]
Event year 1	-0.0862	[-1.52]	-0.0712	[-1.41]
Event year 2	-0.1751***	[-2.68]	-0.1739***	[-3.11]
Event year 3	-0.2194***	[-2.78]	-0.2228***	[-3.20]
Fixed Effects	Industry, Firm, Country *x* Year		Industry-size-age-year	
Obs.	1,505		8,603	

Table 4. Changes in innovation around the "going-private" decision: BA and DID specifications (from both going-private and control samples)

Columns (1) and (2) present OLS panel regressions for Before-After analysis where the dependent variable ln(1+absolute citations) in model (1) and ln(1+relative citations) in model (2). In model (1) and (2) we include industry, firm, and country-year fixed effects. Standard errors are clustered by firm. Columns (3) and (4) present difference-in-differences regression results where the dependent variable ln(1+absolute citations) in model (3) and ln(1+relative citations) in model (4). For each firm in the going-private sample, we include up to five public firms (based on data availability) that are matched to the going-private firms by year, size, three-digit industry, and age. t-statistics based on standard errors clustered by industry-size-age cells are in parentheses. In all models, we show the regression where the independent variables are the relative years pre- and post-buyout (event year 0 is the omitted base category, with a coefficient normalized to 1). All variables are defined in the Appendix. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.

	Before-After				DiD			
	(1)		(2)		(3)		(4)	
	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat
Event year −3	0.0358	[0.21]	-0.0401	[-0.26]	0.1013	[0.96]	0.0565	[0.62]
Event year −2	-0.0174	[-0.12]	-0.0503	[-0.39]	0.1029	[1.17]	0.0995	[1.26]
Event year −1	-0.0779	[-0.60]	-0.0734	[-0.63]	-0.0021	[-0.03]	-0.0011	[-0.02]
Event year 1	-0.1162	[-0.79]	-0.1127	[-0.89]	-0.0878	[-1.00]	-0.0749	[-1.02]
Event year 2	-0.4070***	[-2.94]	-0.3176***	[-2.67]	-0.3284***	[-3.86]	-0.2449***	[-3.35]
Event year 3	-0.4554***	[-3.41]	-0.3632***	[-2.91]	-0.3030***	[-3.34]	-0.2162***	[-2.74]
Fixed Effects	Industry, Firm, Country *x* Year				Industry-size-age-year			
Obs.	1,505		1,505		8,603		8,603	

Table 5. Changes in innovation around the "going-private" decision: DID specifications (from both going-private and control samples)
Columns (1) to (4) present difference-in-differences regression results where the dependent variable ln(1+absolute citations) in model (1) and (3) and ln(1+relative citations) in model (2) and (4). In columns (1) and (2) for each firm in the going-private sample, we include one public firm (based on data availability) that are matched to the going-private firms on variables that determine the propensity of going private measured 13 years before going-private: total assets, sales, R&D, Capex, dividend, free cash flow, debt, cash, net fixed assets (when variables are not available we replace the missing values by industry-year averages. In columns (3) and (4) for each firm in the going-private sample, we include one public firm (based on data availability) that are matched to the going-private firms by pre-innovation measures, year, size, three-digit industry, and age. t-statistics based on standard errors clustered by industry-size-age cells are in parentheses. In all models, we show the regression where the independent variables are the relative years pre- and post-buyout (event year 0 is the omitted base category, with a coefficient normalized to 1). All variables are defined in the Appendix. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.

	DiD				DiD			
	(1)		(2)		(3)		(4)	
	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat
Event year −3	0.1679	[1.16]	0.0952	[0.72]	0.1491	[1.29]	0.1068	[1.08]
Event year −2	0.0896	[0.69]	0.0557	[0.48]	0.1441	[1.35]	0.1357	[1.44]
Event year −1	0.0104	[0.09]	-0.0214	[-0.19]	0.0125	[0.14]	0.0023	[0.03]
Event year 1	-0.1151	[-0.85]	-0.1020	[-0.86]	-0.1362	[-1.25]	-0.0953	[-1.02]
Event year 2	-0.3250**	[-2.37]	-0.2699**	[-2.26]	-0.3363***	[-3.06]	-0.2660***	[-2.83]
Event year 3	-0.2528**	[-2.10]	-0.1825*	[-1.68]	-0.2136*	[-1.82]	-0.1516	[-1.52]
Fixed Effects	Going-private characteristics				Pre-innovation, industry-size-age-year			
Obs.	2,086				2,996			

Table 6. Institutional and management buyouts: changes in innovation around the "going-private" decision: BA and DID specifications (from both going-private and control samples)

Columns (1) and (2) present OLS panel regressions for Before-After analysis where the dependent variable ln(1+absolute citations) in model (1) and ln(1+relative citations) in model (2). In model (1) and (2) we include industry, firm, and country-year fixed effects. Standard errors are clustered by firm. Columns (3) and (4) present difference-in-differences regression results where the dependent variable ln(1+absolute citations) in model (3) and ln(1+relative citations) in model (4). For each firm in the going-private sample, we include up to five public firms (based on data availability) that are matched to the going-private firms by year, size, three-digit industry, and age. t-statistics based on standard errors clustered by industry-size-age cells are in parentheses. In all models, we show the regression where the independent variables are the relative years pre- and post-buyout (event year 0 is the omitted base category, with a coefficient normalized to 1). All variables are defined in the Appendix. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.

Panel A. Institutional buyouts

	Before-After				DiD			
	(1)		(2)		(3)		(4)	
	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat
Event year −3	0.0388	[0.21]	-0.0467	[-0.29]	0.0912	[0.81]	0.0581	[0.59]
Event year −2	0.0033	[0.02]	-0.0310	[-0.23]	0.0994	[1.05]	0.1087	[1.29]
Event year −1	-0.0721	[-0.53]	-0.0708	[-0.57]	-0.0019	[-0.02]	0.0061	[0.09]
Event year 1	-0.1078	[-0.69]	-0.1110	[-0.83]	-0.0884	[-0.95]	-0.0778	[-0.99]
Event year 2	-0.4402***	[-3.00]	-0.3290***	[-2.59]	-0.3552***	[-3.93]	-0.2517***	[-3.24]
Event year 3	-0.4679***	[-3.26]	-0.3451***	[-2.61]	-0.3392***	[-3.51]	-0.2404***	[-2.86]
Fixed Effects	Industry, Firm, Country *x* Year				Industry-size-age-year			
Obs.	1,414		1,414		7,983		7,983	

Panel B. Management buyouts

	Before-After				DiD			
	(1)		(2)		(3)		(4)	
	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat
Event year −3	-0.0022	[-0.01]	0.0415	[0.11]	0.1426	[0.87]	-0.0158	[-0.10]
Event year −2	-0.2758	[-0.68]	-0.2924	[-0.70]	0.1237	[1.05]	0.0005	[0.00]

Event year −1	-0.1505	[-0.32]	-0.1053	[-0.25]	-0.0236	[-0.11]	-0.1177	[-0.55]
Event year 1	-0.2213	[-0.54]	-0.1339	[-0.34]	-0.0851	[-0.48]	-0.0815	[-0.54]
Event year 2	0.0081	[0.02]	-0.1746	[-0.52]	-0.0846	[-0.45]	-0.1819	[-1.08]
Event year 3	-0.2981	[-1.00]	-0.5887	[-1.37]	0.0253	[0.14]	0.0052	[0.03]
Fixed Effects	Industry, Firm, Country *x* Year				Industry-size-age-year			
Obs.	168		168		798		798	

Table 7. Estimates of radical patent count

We present OLS panel regressions where the dependent variable ln(1+radical count). In all models, we show the regression where the independent variables are the relative years pre- and post-buyout (event year 0 is the omitted base category, with a coefficient normalized to 1). Standard errors are clustered by firm. All variables are defined in the Appendix. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.

	(1)		(2)		(3)	
	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat
Event year −3	0.0035	[0.08]	0.0036	[0.08]	0.0479	[0.64]
Event year −2	0.0318	[0.84]	0.0309	[0.78]	0.0574	[0.97]
Event year −1	-0.0051	[-0.16]	-0.0088	[-0.27]	0.0083	[0.17]
Event year 1	-0.0391	[-1.21]	-0.0479	[-1.44]	-0.0199	[-0.44]
Event year 2	-0.1071***	[-2.81]	-0.1108***	[-2.79]	-0.0936*	[-1.68]
Event year 3	-0.1564***	[-3.66]	-0.1664***	[-3.71]	-0.1491***	[-2.64]
Country controls	No		Yes		No	
Firm FE	No		No		Yes	
Country-Year FE	No		No		Yes	
Obs.	1,456		1,407		1,456	

Table 8. Estimates of innovation efficiency

We present OLS panel regressions where the dependent variable innovation efficiency is winsorized at 1%. In all models, we show the regression where the independent variables are the relative years pre- and post-buyout (event year 0 is the omitted base category, with a coefficient normalized to 1). Standard errors are clustered by firm. All variables are defined in the Appendix. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.

	(1)		(2)		(3)	
	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat
Event year −3	-0.0008	[-0.02]	-0.0002	[-0.01]	-0.0388	[-0.74]
Event year −2	0.0172	[0.48]	0.0174	[0.49]	-0.0085	[-0.17]
Event year −1	0.0246	[0.81]	0.0133	[0.44]	-0.0263	[-0.68]
Event year 1	-0.0694**	[-2.07]	-0.0804**	[-2.35]	-0.1086**	[-2.31]
Event year 2	-0.0745**	[-2.22]	-0.0854**	[-2.53]	-0.1035**	[-2.15]
Event year 3	-0.0776**	[-2.05]	-0.0854**	[-2.20]	-0.0870	[-1.54]
Country controls	No		Yes		No	
Firm FE	No		No		Yes	
Country-Year FE	No		No		Yes	
Obs.	1,456		1,407		1,456	

Table 9. Estimates of citations with the cost of debt

This table presents regressions where the dependent variable citation count is measured by absolute citations in columns (1) and (2) and by relative citations in columns (3) and (4). In all models, we show the regression where the independent variables are the relative years pre- and post-buyout (event year 0 is the omitted base category, with a coefficient normalized to 1). Standard errors are clustered by firm. All variables are defined in the Appendix. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.

	(1)		(2)		(3)		(4)	
	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat
Event year −3	0.0294	[0.27]	0.0358	[0.21]	-0.0192	[-0.20]	-0.0401	[-0.26]
Event year −2	0.0111	[0.12]	-0.0174	[-0.12]	0.0142	[0.16]	-0.0503	[-0.39]
Event year −1	-0.0523	[-0.63]	-0.0779	[-0.60]	-0.0386	[-0.53]	-0.0734	[-0.62]
Event year 1	0.4005*	[1.67]	0.4945*	[1.68]	0.3216*	[1.81]	0.4089*	[1.82]
Event year 2	-0.1289	[-0.77]	-0.1960	[-1.00]	-0.1233	[-0.98]	-0.1612	[-1.06]
Event year 3	0.0563	[0.29]	0.0435	[0.19]	0.0722	[0.41]	0.0481	[0.21]
CD year 1	-0.4493**	[-2.19]	-0.5743**	[-2.37]	-0.3776**	[-2.52]	-0.4905***	[-2.62]
CD year 2	-0.1743	[-1.45]	-0.1903	[-1.43]	-0.1141	[-1.30]	-0.1410	[-1.32]
CD year 3	-0.3241**	[-2.21]	-0.3937***	[-2.61]	-0.2827**	[-2.27]	-0.3246**	[-2.29]
Country controls	Yes		No		Yes		No	
Firm FE	No		Yes		No		Yes	
Country-Year FE	No		Yes		No		Yes	
Obs.	1,393		1,561		1,393		1,561	

Table AI. Variable definitions

Variable	Definition	Data source
Innovation Measures		
Patent count	Total number of patents applied for and granted to firm i in year t.	PATSTAT
Radical innovation	Total number of patents granted to firm i in year t that have at least one citation to a non-patent literature.	PATSTAT
Absolute citation	Total number of citations received for patents filed and subsequently granted during the year a patent is granted and the following three periods.	PATSTAT
Relative citation	Total number of citations received for patents filed and subsequently granted during the year a patent is granted and the following three periods, less the average number of citations during this period received by matching patents.	PATSTAT
Innovation efficiency	Number of applications filed and subsequently granted during the year, divided by the number of unique innovators.	PATSTAT
Country-level Characteristics		
IPR	Intellectual Property Rights measures aspects of intellectual property, such as protection. It also reviews a country's policies toward patents, copyrights, and trademarks and their effectiveness .	Park (2008)
INV	Country innovativeness measures the number of resident patent applications scaled by GDP (in mln).	WDI/GDF database
EMD	Equity Market Development is measured as the value of shares traded (total number of shares traded, both domestic and foreign, multiplied by their respective matching prices), scaled by GDP.	WDI/GDF database

CMD1	Credit Market Development is measured as domestic credit to the private sector (% of GDP), which refers to the financial resources provided to the private sector by financial corporations, such as through loans, purchases of non-equity securities, and trade credits and other accounts receivable that establish a claim for repayment. Credit is an important link in monetary transmission, as it finances production, consumption, and capital formation, which in turn affect economic activity.	WDI/GDF database
CMD2	Credit Market Development is measured as domestic credit provided by the financial sector (% of GDP). It includes all credit to various sectors on a gross basis, with the exception of credit to the central government, which is net. The financial sector includes monetary authorities and depositary banks, as well as other financial corporations where data are available (including corporations that do not accept transferable deposits, but do incur such liabilities as time and savings deposits). Domestic credit provided by the financial sector as a share of GDP measures banking sector depth and financial sector development in terms of size.	WDI/GDF database
GDP_GR	GDP growth (annual %) is the annual percentage growth rate of GDP at market prices based on constant local currency. Aggregates are based on constant 2010 U.S. dollars. GDP is the sum of gross value added by all resident producers in the economy, plus any product taxes, and minus any subsidies not included in the value of the products. It is calculated without making deductions for depreciation of fabricated assets or for depletion and degradation of natural resources.	WDI/GDF database
CD	The relative cost of debt is defined as the ratio of the cost of debt at the time of the	FRED Federal Reserve Bank

	buyout announcement to the cost of debt at the *n*-th year post-announcement. The cost of debt is measured as the corporate effective yield at the subgroup country level.	of St. Louis Economic Data
Firm-level Characteristics		
SIZE	Natural logarithm of operating revenue.	ORBIS
AGE	Natural logarithm of the number of years since firm incorporation date.	ORBIS
ROA	Return on assets, defined as operating income before depreciation divided by total assets (book value of total assets).	ORBIS