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Growth LBOs ☆

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ABSTRACT

Using a data set of 839 French deals, we look at the change in corporate behavior following a leveraged buyout (LBO) relative to an adequately chosen control group. In the 3 years following a leveraged buyout, targets become more profitable, grow much faster than their peer group, issue additional debt, and increase capital expenditures. We then provide evidence consistent with the idea that in our sample, private equity funds create value by relaxing credit constraints, allowing LBO targets to take advantage of hitherto unexploited growth opportunities. First, post-buyout growth is concentrated among private-to-private transactions, i.e., deals where the seller is an individual, as opposed to divisional buyouts or public-to-private LBOs where the seller is a private or a public firm. Second, the observed post-buyout growth in size and post-buyout increase in debt and capital expenditures are stronger when the targets operate in an industry that is relatively more dependent on external finance. These results contrast with existing evidence that LBO targets invest less or downsize.

1. Introduction

This paper provides evidence that many LBOs foster firm growth by alleviating credit constraints. This finding contrasts with most of the available literature, which argues that the main source of value creation in LBOs is cost cutting. Studying large public-to-private transactions of the 1980s, Kaplan (1989) shows that LBO targets increase their profitability by cutting down investment, selling off assets, while maintaining operating income constant. Lichtenberg and Siegel (1990) find that privately held LBO targets tend to reduce white-collar employment and wages. More recently, Amess and Wright (2007) and Davis, Haltiwanger, Jarmin, Lerner, and Javier (2008), studying the UK and the US market, over a longer time period, find that initially privately held LBO targets experience some employment reduction following a leveraged buyout. Finally, Chevalier (1995) and Chevalier and Scharfstein (1996) provide evidence that, in the 1980s, LBOs in the supermarket industry led to underinvestment in market shares. However, such evidence may not be fully representative of today's typical LBO transaction for at least two reasons.

First, many of these papers were written in the 1980s. But the 1980s was a decade of intense corporate restructuring, in the face of international competition and deregulation of many industries. Against this background, financial pressure served to implement painful cost-cutting

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policies (Jensen, 1993). Since then, the business model of the private equity industry may have changed (Stromberg, 2008). For instance, Guo, Hotckiss, and Song (2011) find weaker effects on profitability for recent large public-to-private deals, suggesting that the huge gains reaped by private equity investors in the 1980s have vanished.[1] The private equity industry may have devised new sources of value creation: one hypothesis is that some funds are now targeting underdeveloped, credit-constrained firms, and help them grow faster.[2]

Second, nearly all studies have concentrated on the US and the UK, where capital and credit markets are large and well functioning.[3] In countries where this is not the case, LBOs may help relax targets' credit constraints, allowing them to take advantage of hitherto unexploited growth opportunity. Even if they do not provide direct financing, there are several reasons why private equity funds could increase targets' debt capacity. For instance, private equity funds may be perceived as transparent and activist shareholders. Because they would monitor the firm better than the previous owner(s), they would exert a positive externality on debt holders, who are more senior claimants. Also, private equity funds may bring financial expertise and connections to hitherto financially unsophisticated firms. Last, private equity sponsors may introduce new, more competent members to the executive suite, which may reassure bankers.

France provides us with a natural testing ground for our main hypothesis that LBOs may foster growth by alleviating targets' credit constraints. First, France is a country with many family-managed businesses [see, e.g., Faccio and Lang (2002) for evidence on publicly listed firms], which sometimes lack the managerial and financial expertise needed to take advantage of all growth opportunities.[4] Thus, focusing on France allows us to study an economy with many "sleeping beauties," i.e., potential private targets with significant margins of improvement and growth. Second, the French credit and stock markets are both much less developed than those of the US and the UK.[5] Against this background, it seems at least plausible that in France, private equity groups could help previously credit-constrained firms get access to outside sources of finance.

Using two separate sources of data, we identify 839 deals over 1994–2004. Our sample is representative of the typical LBO deal in international data. First, sponsors of LBO deals in our sample are mostly large private equity funds: more than 40% of them are not French firms (mostly the UK and the US funds). Second, the average target's enterprise value in our sample is $395m (in 2007 dollars), compared with an average deal size of $280m in the UK, and $389m in the US (figures from Stromberg, 2008).[6] In terms of number of deals, our sample is smaller than the UK sample of Amess and Wright (2007), who study 1350 deals, and the US sample of Davis, Haltiwanger, Jarmin, Lerner, and Javier (2008), who look at more than 5000 deals (over a longer period). Once adjusted for the size of the economy, however, the LBO market in France appears to be quite similar to that of the US, and only slightly less dynamic than that of the UK.

We then track the corporate behavior of targets before and after the deal, using accounting data extracted from tax files. We compare their evolutions with a carefully constructed control group of similar firms that are not targets of an LBO. Like most existing studies, we first show that LBOs lead to a large and statistically significant increase in the target's profitability. This result is robust: it holds for smaller or bigger targets, different time periods, and is independent of the target's pre-buyout ownership structure. In this sense, our paper yields results similar to most of the literature on post-LBO performance (as, e.g., in Kaplan (1989) and Acharya, Hahn, and Hehoe (2009)).

However, in contrast to existing studies, we find that LBO targets grow after the deal significantly more than comparable firms, in terms of employment, sales, and capital employed. This effect is statistically significant and economically large: between the 4 years preceding the transaction, and the four subsequent years, employment, assets, and sales growth of LBO targets are, respectively, 18%, 12%, and 12% higher than their control firms. Our own estimates are extremely robust and are not due to differences in methodology with previous studies. In particular, the strong post-LBO growth we observe in our data is robust across time periods. This increase in post-LBO growth is also accompanied with a strong increase in capital expenditures (24% higher for LBOs relative to their control firms). We also find that, after the deal, LBO targets issue additional debt to finance asset growth: this additional debt represents about one-third of the average asset growth observed among LBO targets.

Interestingly, these effects depend strongly on the target's pre-buyout ownership structure: the post-buyout increase in firm size, capital expenditures, and post-LBO debt increase is concentrated among private-to-private transactions. These are deals where the seller is an individual, in most cases a family cashing out of its business. In contrast, divisional buyouts (where the seller is a larger conglomerate) and public-to-private transactions (where the target is listed on

[1] Another interpretation is that firms now cut their costs without outside intervention, under the pressure of either stockholders or product market competitors (Giroud and Mueller, 2010).

[2] As an illustration, AXA PE, a large French private equity group, argues on its Web site that its Eastern Europe LBO Small Cap fund "will seek to identify targets whose growth opportunities are large but limited by capital constraints."

[3] Regarding France, the only exception we are aware of is Desbrières and Schatt (2002).

[4] Using a restricted sample of medium-sized, privately held firms, Bloom and Van Reenen (2007) find that management practices tend to be poor in family managed firms. Besides, all studies find that family firms tend to be, on average, smaller than non-family firms (see Sraer and Thesmar, 2007, for France).

[5] According to Djankov, Mc Liesh, and Shleifer (2006), the ratio of private credit to gross domestic product (GDP) in France is .9, as opposed to 1.4 in the UK and the US. According to recent data put together by Beck, Demirgüç-Kunt, and Levine (2000), the ratio of private credit plus stock market capitalization to GDP is equal to 1.7 in France, versus 2.7 in the UK, 3.5 in the US. Finally, France scores low on many dimensions of investor protection, such as the creditor rights index reported in Djankov, Mc Liesh, and Shleifer (2006).

[6] In France, as in the UK or the US, public-to-private deals, involving a large publicly listed target, are the exception rather than the norm.

the stock market before the buyout) do not spur growth, even though these deals also create an increase in the target's profitability.[7] As targets of private-to-private deals are more likely to be credit-constrained before the buyout than former divisions of larger companies or publicly held firms, our interpretation of these first results is that private equity funds help targets that were previously limited in their access to capital take advantage of unexploited growth opportunities.[8]

We provide further evidence consistent with this credit-constraint hypothesis. In the spirit of Rajan and Zingales (1998), we focus on industries where internal funds are typically insufficient to finance investment (financially dependent industries). We find that post-LBO growth in size, post-LBO debt issues, and increase in capital expenditures are larger in these industries, but only for private-to-private transactions. For divisional, secondary, and public-to-private transactions, i.e., for transactions where credit-constraint concerns are less likely to be relevant, the target's post-buyout behavior does not depend on its industry financial dependence. In other words, post-buyout growth in size, post-LBO debt issues, and increase in capital expenditures are concentrated among private companies that operate in more financially dependent industries. These findings are consistent with the idea that private equity funds increase their portfolio firms' debt capacity.

The remainder of the paper is organized as follows. Section 2 describes the data used. Section 3 establishes the fact that target growth is accelerated following an LBO. Section 4 provides evidence that financial constraints are relaxed after the deal. Section 5 concludes.

2. Data set

2.1. Data construction

To analyze the impact of LBO transactions at the company level, we use three different databases: Securities Data Company (SDC) Platinum and Capital IQ (to isolate transactions) and Bénéfices Réels Normaux (BRN) (for financial statements).

First, we retrieve all the deals from SDC Platinum with the following characteristics: (i) they are completed between January 1994 and December 2004, (ii) the target company is incorporated in France, (iii) deals are classified as "LBO" by this database.[9] There are 603 deals matching these criteria. We then improve our coverage with transactions from Capital IQ. There, we select all 972 deals that were (i) announced between 1994 and 2004, (ii) either "closed" or "effective," (iii) reported by Capital IQ as being "LBOs." The two data sets overlap: thus, we start with 1193 transactions.

Most of the targets are medium-sized, privately held firms. We obtain financial statements from tax files (called BRN) available from the Institut National de la Statistique et des Études Économiques (INSEE).[10] Our transaction and accounting data do not have the same identifier so we match them by company name. Names are not always identical in both databases, so in case of ambiguity, we resort to company Web sites and annual reports. The matching process for selecting a group of control firms, which we describe more extensively in Section 2.2 below, will further reduce sample size to 839 deals.

One possible concern at this stage is that our data construction technique does not account for the group structure that is so prevalent among French firms. Many firms have subsidiaries that are 100% controlled and that may hold more assets or employees than the parent company. If an LBO is followed by a simplification of the corporate structure that leads to the consolidation of all assets and jobs in the target firm, we will overestimate the post-LBO growth of the firm. We deal with this important issue in three ways. First, notice that the assets of subsidiaries are in general already included in the fixed assets of the parent firm via the value of the parent's equity holdings (in the financial fixed assets accounting item). Hence, post-LBO simplification may lead, for the parent firm, to a mechanical increase in employment and sales, but not in total fixed assets. Second, for each LBO target, we try to make sure that we focus on the main entity with the most real activity, instead of a holding which would own various subsidiaries but no real operation. We do this using company Web sites and annual reports and by looking at employment and sales figures. Third, we use another data source (LIFI, for "Liaisons Financières"), available from the statistical office, which collects ownership links between parents and subsidiaries. The limitation of this database is that it is a survey, but coverage is good during the time period that we consider. Using this survey, we find that only 20% of our targets have one subsidiary or more. As a result, we do not report results using subsidiary data in most regressions, but use this information in a robustness check.

A second concern is that we may have missed many divisional buyouts, as in such cases the target may not be an independent legal entity before the transaction (but just a division of the selling firm). As it turns out, among our 839 LBOs with non-missing accounting data, there are still 233 divisional buyouts (28%) according to SDC and Capital IQ. This fraction is not changed much by the matching process: before matching, 31% of our transactions (out of 1193 deals) are divisional buyouts. It means that divisions

[7] Secondary buyouts, where the target already belongs to a private equity group, also experience some post-buyout growth, although to a lesser extent than what is observed for private-to-private transactions.

[8] Consistent with our own findings, Chung (2009) also finds evidence of spectacular post-LBO growth among 170 private-to-private targets, which he also attributes to the relaxation of credit constraints since in his data, public-to-private deals lead to firm downsizing.

[9] Definition of an LBO according to SDC: an "LBO" occurs when an investor group, investor, or firm offers to acquire a company, taking on an extraordinary amount of debt, with plans to repay it with funds generated from the company or with revenue earned by selling off the newly acquired company's assets. SDC considers an LBO if the investor group includes management or the transaction is identified as such in the financial press and 100% of the company is acquired.

[10] The BRN contains tax files for all French firms, public or private, whose annual sales exceed 100,000 Euros in the service sector and 200,000 Euros in other sectors. See Bertrand, Schoar, and Thesmar (2007) for a description of these data.

that are sold tend to be independent legal entities before the transaction, so that they have their own financial statements. On this front, the group structure of the selling firm, very common in France, helps us in following the LBO target before and after the transaction.

All in all, we find that the total number of employees in firms that underwent an LBO between 1994 and 2004 stands at 171,507. This represents approximately 1.4% of employment in our accounting data, and some .9% of total French employment. This is smaller than the figure obtained by Davis, Haltiwanger, Jarmin, Lerner, and Javier (2008) in their study of US LBOs.

Using financial statements reported on tax files, we retrieve the following variables: number of employees, fixed assets, working capital (measured as trade receivables plus inventories minus payables), total debt, earnings before interest, taxes, depreciation, and amoritization (EBITDA), amortization and depreciation, net income, capital expenditures, and industry classification (two and four digit). We measure vertical integration as the ratio of value added (sales minus intermediary inputs) divided by sales. The share of exports is the ratio of exports to total sales. Profitability is measured through return of assets (ROA), i.e., EBITDA divided by assets (as measured by fixed assets plus working capital). All the ratios are winsorized at the median plus or minus five interquartile ranges.

Target leverage is the ratio of target debt to target assets. It is important to note that this measure of leverage uses the accounting information of the target itself, and therefore excludes debt raised for the LBO operation itself. The debt raised for the LBO operation is typically borne by a holding company, which in turn owns the target, so it does not appear in the unconsolidated accounts that we have access to. We believe this information is, however, interesting, since it will inform us on the ability of the target to raise debt *after* the LBO, beyond what has already been raised to finance the deal. In the following, we will refer to our measure of leverage as "target leverage", as opposed to "deal leverage" which is the ratio of debt used in the LBO-to-enterprise value.

2.2. Building the control group

In order to analyze the impact of LBO operations, we compare the targets of such transactions to similar companies that did not go through an LBO. A matching company (a "control firm") meets the three following criteria: (1) it belongs to the same two-digit sector as the target, (2) the number of employees one year before the LBO is in the $\pm 50\%$ bracket of the employment of target company, and (3) ROA one year before the LBO is in the $\pm 50\%$ bracket of the ROA of the target company. If there are more than five control firms, we just keep the five neighbors nearest to the target.[11] The choice of ROA and employment is clearly driven by the fact that profitability and size dynamics are the focus of our investigation, and that they tend to mean revert. Regarding the $\pm 50\%$ bracket, there is a trade-off between matching accuracy and the need to get a control firm for as many LBO targets as possible. At this 50% level, 85 targets have no control firm and are thus dropped from the sample. If we require employment and ROA both to be at most 20% away from the target, the number of targets with no control firm rises to more than 100, leading to an important decrease in the number of observations.[12]

The matching methodology allows us to add 3994 control firms to the sample, i.e. 4.76 control firms by target. By construction, the two groups are not too different, as evidenced by Table 1, which presents pre-buyout descriptive statistics for targets and the median of each group of control firms. The median target has 64 employees, and sales of some 13m€. The median control firm is somewhat smaller (60 employees and 7.8m€ of sales). The distribution of ROA and pre-deal leverage is almost identical for control and target firms. Finally, pre-LBO growth is slightly lower for control firms. This is comforting given that we did not match on pre-buyout growth. Hence, before the transaction, LBO targets and control are on similar trends.

Nevertheless, we have to acknowledge that our matching approach has an important limitation since LBOs are not exogenous events. For instance, private equity funds could target firms that are on the verge of expanding. Controlling for pre-buyout characteristics, as we do here, helps make this concern less stringent. The fact that growth occurs precisely at the moment of the LBO is also comforting. Yet, in the absence of a proper source of exogenous variation in the probability to be involved in a deal, our results may be subject to an endogeneity bias and should therefore be interpreted as descriptive more than causal.

2.3. How different are French LBOs from the rest of the world?

Fig. 1 shows the number of LBOs per year in our sample. Overall, the number of deals first peaks in 1999, after which LBO activity stagnates until 2003 and then picks up in 2004. This pattern is similar to the evolution recorded by Davis, Haltiwanger, Jarmin, Lerner, and Javier (2008) in their US sample. While there are, in total, less deals in our sample (they have more than 5000 deals over the 1981–2004 period), part of the reason for this is that the US economy is larger than the French one (GDP is six times bigger). Adjusted for the size of the economy, French LBO activity looks comparable to the US.

The types of sellers involved in our French transactions do not differ much from the typical LBO in the world. Only 4.3% of the deals in our sample are public-to-private transactions, a number close to that found in Stromberg's (2008) sample of LBOs around the world. In France, as in the world, 52% of LBOs are pure "private-to-private"

[11] Distance is defined by the sum of the squares of the difference between the target and the control firm's ROA and the target's and the control firm's number of employees.

[12] However, regression results presented below are almost unchanged with this smaller sample.

Table 1
Sample of LBO targets and their control firms (see text for details). Sample period: 1994–2004. For each firm in the sample, each variable is averaged over the 4 years preceding the transaction. This table shows the distribution of this pre-transaction outcome for actual LBO targets (Panel A) and for the median of each group of control firms (Panel B). Capital Employed (CE) is the sum of fixed assets and operating working capital. CE growth is the yearly growth rate of Capital Employed. ROA is EBITDA normalized by shareholder's equity plus debt minus trade payables. Leverage is financial debt divided by capital employed. Other variables are self-explanatory.

Variable	Median	Mean	S.D.	Q1	Q3	Number of deals
Panel A: Targets						
Sales (m€)	13.09	32.64	46.52	4.79	40.93	839
Employment	64	173	242	27	229	839
Capital employed (m€)	7.77	21.27	31.12	2.47	22.26	839
Capital expenditures (m€)	.39	1.27	1.91	.1	1.38	839
Sales growth	.08	.11	.21	−.01	.19	832
Employment growth	.03	.05	.15	−.02	.11	837
CE growth	.08	.11	.26	−.01	.2	836
ROA	.18	.2	.27	.05	.33	839
Leverage	.2	.24	.21	.08	.37	836
Panel B: Control firms						
Sales (m€)	7.75	23.64	35.59	2.66	29.82	839
Employment	60	153.1	207.45	26	203.5	839
Capital employed (m€)	3.25	10.55	16.02	.93	12.34	839
Capital expenditures (m€)	.16	.62	.96	.04	.7	839
Sales growth	.06	.06	.08	.02	0	832
Employment growth	.01	.03	.05	0	.05	837
CE Growth	.05	.06	.09	0	0	836
ROA	.18	.19	.25	.06	.31	839
Leverage	.18	.21	.15	.08	0	836

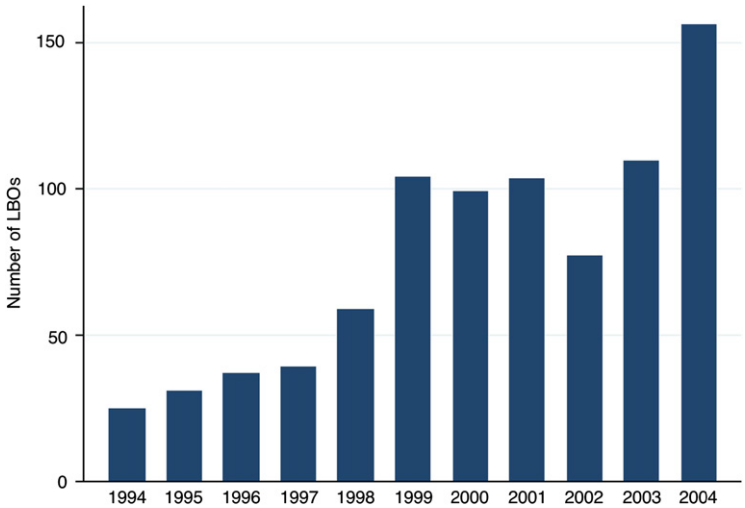

Fig. 1. Number of LBOs per year in the sample of 839 LBOs in SDC and Capital IQ for which accounting data are available. Sample period: 1994–2004.

transactions.[13] Divisional buyouts comprise 27.2%, against 26% in Stromberg's sample.[14] We have slightly more secondary buyouts (15.4% versus 13%), i.e., transactions involving a financial vendor (most often another private equity fund). Finally, less than 1% of our targets are labeled "distressed," but this figure is also very small in Stromberg's sample (2%).

Deal size and capital structure are also very similar to international data. According to Stromberg (2008), who uses Capital IQ only, the median deal size (in terms of enterprise value) is $64 million in the US, and $36 million in the UK. In our French extract of Capital IQ, it is $63

[13] By definition, private-to-private transactions should be deals where an individual owns the target, i.e., where the target is a "family firm." To be sure, we hand-collected the identity of the target's ultimate shareholder for the 47 private-to-private transactions in our data in 2004. Among the 40 targets for which this information was available, we found only three cases where an individual did not own the firm. In one case, the target was a co-op, in another it was held by a financial institution, and in another by an industrial company.

[14] Looking at pre-deal data, we find that targets of private-to-private deals, i.e., "family firms," are smaller than targets of divisional buyouts (122 vs. 286 employees, on average). However, they also grow faster (7% vs. 3% of annual employment growth) and are more profitable (average ROA of .25 vs. 14).

(*footnote continued*)

million (deal size is not well-reported in SDC). We also have reasons to believe that the use of debt is as pervasive in our French sample as in the transactions studied in previous papers. Unfortunately, there are no comprehensive data on deal structures, especially for the older deals we are investigating in this paper. The "target leverage" variable that we use is based on the target's accounts only, and excludes the amount of debt raised for the control transfer itself (as argued above, such debt is borne by a separate holding company which our accounting data do not track). We can nevertheless bring two pieces of evidence suggesting that French deals have comparable leverage to the US and the UK. First, looking at deals made between 2003 and 2006, Standard and Poor's (S&P) reports a mean debt-to-EBITDA ratio of 4.8 for France (232 observations) and the UK (240), and 4.2 for the US (410). Second, we restricted ourselves to the 245 LBOs in Capital IQ in 2005 and have used LoanConnector to retrieve the amount of debt used in these deals.[15] Among them, only 22 have information on both debt used in the deal and enterprise value. Among these 22 deals, average debt-to-enterprise value is .58. It is somewhat smaller than the .67 found by Axelson, Stromberg, and Weisbach (2009) in the global sample but their focus is on large LBO transactions. It is, nevertheless, in a comparable range. Interestingly, deal leverage for the 22 deals we have information on is somewhat similar across LBO types and in particular, divisional buyouts do not appear to be more levered pre-buyout than private-to-private deals, at least on this small sample. Let us note, however, that these 22 deals are far from being representative of our whole sample. The average target in our sample has about 8m euros in assets, while the average enterprise value in the 22 deals is 400m. Hence, a deal leverage of about .67, which is the norm among larger deals (in France or elsewhere), may very well be different for smaller deals, which are more representative of our sample.

The LBO sponsors in our sample are quite representative of the universe of private equity funds around the world. Among the 104 sponsors backing our deals, we have both very large sponsors (such as 3I, Axa PE, CVC Capital Partners, Permira, etc.) as well as smaller ones. There is a majority of French private equity firms (58%), which are, on average, small (1.4bn€ of assets under management). US and UK funds are common (28% of the deals in our sample) and larger, on average (3bn€ of assets under management for US sponsors and 5.6bn for UK sponsors). All in all, domestic funds are prevalent but there is an important fraction of larger US/UK, based funds.

There is, however, one notable difference between US LBOs and the deals in our sample: our transactions involve firms that are older than the typical US targets studied by Davis, Haltiwanger, Jarmin, Lerner, and Javier (2008). In their sample, about 50% of targets are more than 10 years old and 25% are less than 5 years old. In our sample, firms are older: 85% of our targets are more than 10 years old, and only 5% younger than 5. This difference is consistent with the idea that LBOs involve more mature firms in continental Europe than in the US or the UK. It is important to emphasize, however, that targets do not systematically differ from their control firms on the age dimension, even though age was not a criterion in the matching procedure. This comforts us in thinking that our results are not driven by the mere effect of firm age on firm growth.

2.4. Industry level variables

In Section 4, we will use industry-level measures of dependence on external finance and exposure to labor market rigidities. We measure financial dependence at the industry level using the universe of firms present in the tax files with more than 100 employees. We follow the methodology of Rajan and Zingales (1998). For each firm in this sample and for each year, we calculate the difference between capital expenditures and gross cash flows, normalized by investment. Gross cash flows are computed by taking net income plus depreciation and amortization.[16] This ratio thus measures the fraction of investment that is financed externally. We then remove outliers and compute the average by two-digit industry over the 1990–2006 period, using all firms present in the tax files. Again, this ratio is computed only on the sample of firms with more than 100 employees. The reason is that this measure is meant to capture the "technological" financial dependence of an industry and should thus be computed using firms that are less likely to be credit-constrained (Rajan and Zingales, 1998). As larger firms tend to be less credit-constrained, we therefore restrict the sample to firms with more than 100 employees (this corresponds to the two top percentiles of the size distribution). In Table A4, we check the robustness of our results, using 50, 200, or 500, as alternative cutoffs.

The US firms-based measure of financial dependence, as initially computed by Rajan and Zingales (1998), and our measure based on French data are clearly correlated. The linear correlation coefficient is .48 and is significantly different from zero at the 1.2% confidence level. The Spearman rank correlation coefficient is equal to .51, and the test of (possibly non-linear) independence of the two variables is rejected at .7%. Hence, the industry variation we capture is similar to the one studied by Rajan and Zingales (1998).[17]

[15] We use 2005 because LoanConnector coverage is really too incomplete before this year and the debt market in 2005 is not yet as over-heated as in 2006/7.

[16] In alternative (unreported) specifications, we used a measure closer to cash flows from operations (gross cash flows plus interest payments). This alternative measure gives very similar results both in terms of statistical and economic significance. A second alternative is to use, instead of gross cash flows, gross cash flows minus change in operating working capital (increase in receivables plus increase in inventories minus increase in payables). This second alternative is closer to the actual measure of cash flows from operations, and provides very similar estimates. In the main text, we report regression results with gross cash flows because these are the ones used in Rajan and Zingales' (1998) paper.

[17] To get a better sense of the robustness of our measure of financial dependence, we correlate it with various industry characteristics. We find that financially dependent industries are growth industries and that they are marginally more capital intensive (and hence need investment).

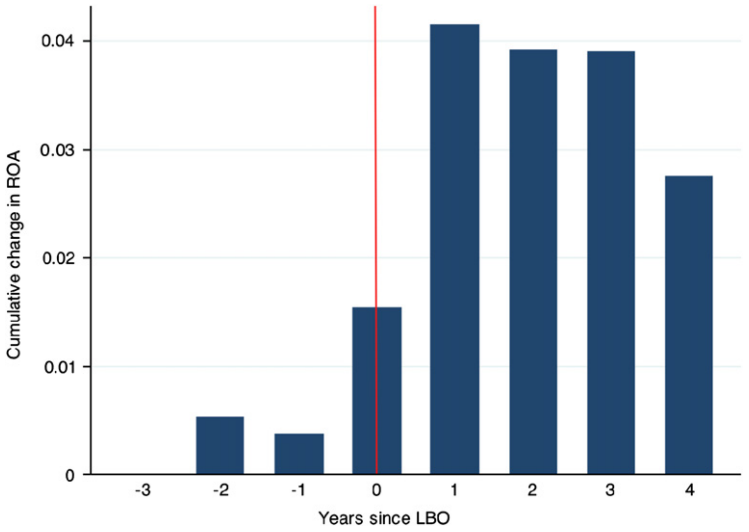

Fig. 2. Mean-adjusted increase in profitability around the LBO. For each deal in our sample, let t be then number of years since the LBO. For each t and each LBO target, we first compute the change in ROA between 3 years before the deal ($t=-3$) and t. For each LBO target, we then take all control firms and compute the mean change in ROA between -3 and t. We then compute the difference between the ROA change of the target and the mean ROA change of the control firms: this is the adjusted change of ROA at the target level. The figure plots the average adjusted change in ROA for $t=-2, -1, 0, 1, 2, 3$ and 4 and across all targets in our sample.

We measure industry exposure to labor market rigidity by using the 1998 wave of the REPONSE survey.[18] This survey is run every 6 years by the French ministry of labor, and collects information about working conditions at the employee level in a large number of French firms. We use two variables that we first compute at the firm level. The first variable is the fraction of workers that belong to a union. It measures the ability of workers to resist restructuring. The second variable is the firm-level fraction of workers that are hired under fixed-term-contracts (FTC). It measures the fraction of the labor force that is "flexible," since, although it is costly to anticipate the termination of FTC, the firm does not have to renew them when they mature (typical duration is one year). We then separately aggregate these two firm-level rigidity measures at the two-digit industry level.

3. Post LBO profits and growth: evidence and robustness

3.1. Profitability

We first start by documenting the impact of LBOs on target profitability in our sample. Fig. 2 presents the evolution of mean profitability before and after the transaction, compared to control firms. In the spirit of Kaplan (1989), we first compute, for each target and each year before or after the LBO, the difference between ROA and the median ROA of its control firms taken the same year. We call this the "excess ROA" of the target. We then compute the change in excess ROA between each year t and 3 years before the deal. Finally, we compute the mean of such evolutions of excess ROAs, and report this in Fig. 2. The average deal is followed by an improvement in operating profitability of around four percentage points. The timing of the improvement offers convincing evidence that something massive happens to LBO targets around the deal. The relative flatness of the evolution of ROA prior to the deal year gives us confidence in the construction of our control firms. Unreported t-tests (as well as a Wilcoxon test of median equality) suggest that this sharp increase in ROA of target firms relative to their control firms is highly significant (at the 1% confidence level) from year 1.

To formalize our statistical tests, we perform the following regression:

$$Y_{jt} = \alpha_j + \delta_t + POST_{jt} + POST_{jt} LBO_j + \varepsilon_{jt}, \quad (1)$$

where j is a firm index and t a time (year) index. Y_{jt} is the performance variable (in this subsection, ROA). If firm j is an LBO target, $POST_{jt}$ equals one after the deal and zero before. If j is a control firm, $POST_{jt}$ equals one when the target corresponding to j has undergone the LBO, and zero before. LBO_j is equal to one for targets, and zero for control firms. This regression includes firm and time fixed effects. As recommended by Bertrand, Duflo, and Mullainathan (2004), we cluster error terms at the $firm \times POST$ level. Results using ROA and log EBITDA as dependent variables are reported in Table 2, columns 1 and 2, respectively.

LBOs in our sample are associated with an increase of 4.4 percentage points in operating performance. This is economically large. The sample mean of operating profitability is 19% and its standard deviation is about 29%. LBOs are thus associated with a 15% standard deviation increase in ROA. Confirming this result, column 2 of

(*footnote continued*)
They are also more concentrated (which is consistent with capital intensity and credit constraints acting as barriers to entry). And they have lower productivity, perhaps because financing constraints prevent them from reaching the efficient scale.

[18] For a description of this data set, see Acemoglu, Aghion, Lelarge, Van Reenen, and Zilibotti (2007).

Table 2
Sample of LBO targets and their control firms (see text for details). Sample period: 1994–2004. OLS estimates of the impact of a LBO on targets' behavior. All regressions include firm and year fixed effects. post is a dummy equal to 1 for the 3 years following the LBO and 0 for the 3 years preceding the LBO. LBO is a dummy equal to 1 if the observation is an LBO target and 0 if it is a control firm. log(Empl) is the logarithm of employment. WC is working capital. FA is fixed assets. CAPEX is capital expenditures. Other variables are self-explanatory. Error terms are clustered at the deal × post level.

	ROA (1)	log(EBITDA) (2)	log(Empl) (3)	log(Sales) (4)	log(FA+WC) (5)	Leverage (6)	log(CAPEX) (7)
post × LBO	.044***	.18***	.12***	.12***	.07***	.026***	.24***
	(.007)	(.029)	(.021)	(.024)	(.02)	(.0063)	(.045)
post	−.025***	−.12***	−.066***	−.083***	−.038***	−.0024	−.11***
	(.0048)	(.018)	(.009)	(.013)	(.012)	(.0038)	(.032)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	33,003	27,478	32,887	33,061	32,735	32,366	32,744
Number of deals	839	793	839	936	839	838	839
Adj. R^2	.53	.87	.93	.92	.93	.61	.72

*** Means statistically significant at the 1% level.

Table 2 reports that target firms' EBITDA increases, relative to their control firms, by a significant 18% following the deal.

A potential concern with our data is that small, privately held firms have incentives to underreport their earnings in order to avoid the corporate income tax. As a consequence, when these firms are taken over by a private equity fund, their earnings could increase simply because underreporting stops. According to a recent report by the French branch of auditing firm Deloitte (2005), manipulations can be one of four types: (1) optimizing the depreciation/amortization schedule, (2) optimizing the valuation of inventories, (3) smoothing income through exceptional items, and (4) underreporting sales. In unreported regressions, we show that after a deal (i) depreciation accelerates, (ii) changes in inventories remain constant, and (iii) exceptional items are not more frequent. Hence, accounting manipulations such as (1), (2), or (3), cannot explain the observed increase in performance following a buyout. Unfortunately, our data do not allow us to address directly manipulation (4), i.e., outright sales underreporting. However, the firms in our sample are incorporated companies of reasonable size (the median target in the sample has 63 employees the year of the deal) so that sales underreporting should not be a frequent behavior.[19]

Another concern is that these results on profitability could be driven by asset write-ups at the time of the LBO. If depreciation is accelerated after the buyout, then ROA could increase without a real improvement in the firm's profitability. The results of column 2 of Table 2, using log (EBITDA) as a dependent variable, already suggest that profits increase following a leveraged buyout. We also present in column 2 of Table A1 the estimation of Eq. (1) where the dependent variable is return on sales (ROS), i.e. EBITDA normalized by the firm total sales instead of assets. This measure of profitability is immune to changes in the depreciation schedule. We find that ROS increases significantly by 1.4 percentage point following an LBO. As the standard deviation of pre-buyout ROS is .19, this represents a 7% standard deviation increase in ROS. The effect is statistically significant at the 1% confidence level. Thus, in our sample and however measured, profitability increases following a leveraged buyout.

3.2. Growth

We turn to the main contribution of this paper: the evidence on growth, debt, and capital expenditures. As an illustration, we start with a graphical display of the timing of job creation and capital expenditures after the LBO in Figs. 3 and 4. To construct these figures, we first compute, for each firm in the target and control samples, the change in log employment and log capital expenditures between 3 years before the LBO and each year t. We then compute, for each target, the median value of such cumulative employment and capital expenditure growth of control firms, and subtract it from the target's own cumulative growth. We then compute the average of such "excess growth" over all targets and for each year, starting 3 years before the LBO. Fig. 3 shows strong employment growth following LBOs in our sample, up to 18% four years after the transaction. Approximately a third of this employment growth takes place in the year following the buyout. This differential job growth is statistically significant, whether we use a Wilcoxon median test or a Student's t-test. Similarly, Fig. 4 shows very strong capital expenditure growth following LBOs in our sample, up by 40% four years after the transaction. Again, this differential investment growth is statistically significant at the 1% level.

A regression analysis complements this graphical evidence. In Table 2, we estimate Eq. (1) using as dependent variables different measures of size of operations: log

[19] Another related concern is that, before an LBO, managers could be expensing private consumption items to the firm, thus decreasing its accounting profitability. After the LBO, the new management team would stop such behavior, which would automatically increase its accounting profitability. It is hard to test for this channel empirically. Nevertheless, one would suspect that this concern should be stronger for smaller firms where these "private" expenses represent a larger share of overall operational profits. However, we know—from unreported regressions available from the authors upon request—that the post-LBO increase in profitability is similar for small *and* large targets.

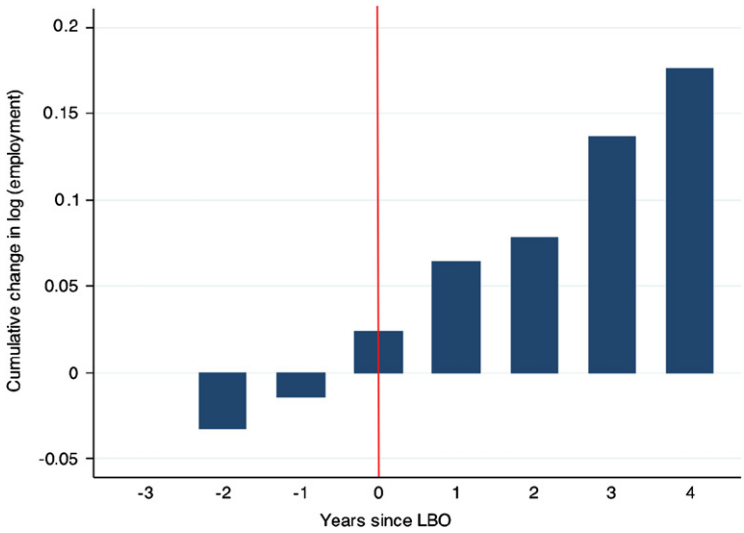

Fig. 3. Mean-adjusted increase in employment around the LBO. For each deal in our sample, let t be then number of years since the LBO. For each t and each LBO target, we first compute the growth in employment between 3 years before the deal ($t=-3$) and t. For each LBO target, we then take all control firms and compute the mean employment growth between -3 and t. We then compute the difference between the employment growth of the target and the mean employment growth of the control firms: this is the adjusted employment growth at the target level. The figure plots the average adjusted employment growth for $t=-2$, -1, 0, 1, 2, 3 and 4 and across all targets in our sample.

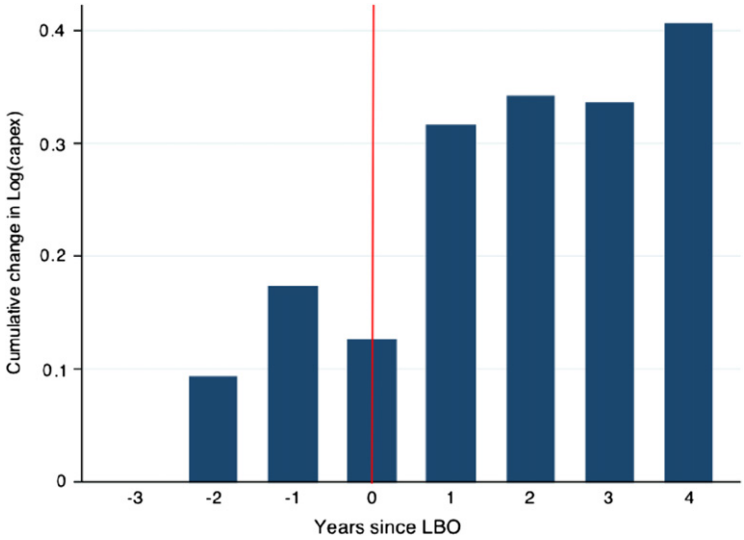

Fig. 4. Mean-adjusted increase in capital expenditures around the LBO. For each deal in our sample, let t be then number of years since the LBO. For each t and each LBO target, we first compute the growth in capital expenditures between 3 years before the deal ($t=-3$) and t. For each LBO target, we then take all control firms and compute the mean capital expenditures growth between -3 and t. We then compute the difference between the capital expenditures growth of the target and the mean capital expenditures growth of the control firms: this is the adjusted capital expenditures growth at the target level. The figure plots the average adjusted capital expenditures growth for $t=-2$, -1, 0, 1, 2, 3 and 4 and across all targets in our sample.

employment (column 3), log sales (column 4), log of fixed assets plus working capital (column 5), target (not deal) leverage (column 6), and log capital expenditures (column 7). We find that capital expenditures of LBO targets increase by 24% more than their control firms, leading to a relative increase of 7% in assets (fixed assets plus working capital). Both these increases are significant at the 1% confidence level. The surge in operating assets is also accompanied by a strong and significant increase in sales (12%) and employment (12%). Target's leverage (i.e., excluding LBO related debt) also increases by about 2.6 percentage points: hence, after the deal, the target issues additional debt to finance part of its asset growth. All these results are significant at the 1% confidence level.

The increase in post-buyout capital expenditures is particularly remarkable. An important question then is whether this increase in investment takes place through external or internal growth. Our data do not allow us to

break down directly capital expenditures into acquisitions and organic growth. However, we can use plant-level data to see whether plant creations/destructions are affected by buyouts. In column 1 of Table A2, we show that the number of plants created/destructed by LBO targets after the deal is not significantly different from that of their control firms. This suggests that most of the post-LBO growth we observe is likely to arise through organic growth, not through acquisitions.

3.3. Potential sources of value and growth creation

In Table 3, we look at further measures of corporate behavior that may explain the source of post-LBO growth. One possibility is that targets outsource part of their production to more cost-effective domestic or foreign firms. This can be measured through the ratio of intermediate input consumption (the difference between sales and value added) to total sales, which we use as the dependent variable in column 1. As far as this ratio is concerned, there is no difference in its evolution between LBO targets and their control firms. In column 2 of Table 3, we look at the share of working capital in total assets. It is often argued that part of the wealth creation in LBOs comes from leaner inventories and faster payments by customers, which together reduce working capital and allows putting the firm's cash to more productive use. Statistically, however, LBO targets and their control firms exhibit a similar working capital evolution. Finally, we ask if post-LBO growth can be explained by an expansion on international markets. We do find that LBO targets, compared to their control firms, increase significantly their sales to foreign markets. However, this effect is economically small. The share of exports in sales increases by 1.3 percentage point. For the sake of comparison, the sample mean of the share of exports in total sales is 12%. One possibility is that our linear model does not fit the data very well, as 40% of the firms in our sample do not export at all.

Table 3
Note: Sample of LBO targets and their control firms (see text for details). Sample period: 1994–2004. OLS estimates of the impact of a LBO on targets' behavior. All regressions include firm and year fixed effects. post is a dummy equal to 1 for the 3 years following the LBO and 0 for the 3 years preceding the LBO. LBO is a dummy equal to 1 if the observation is an LBO target and 0 if it is a control firm. WC is working capital. FA is fixed assets. Other variables are self-explanatory.

	Intermediate inputs/sales (1)	WC/(FA+WC) (2)	Exports/sales (3)
postxLBO	−.0013	.0086	.013***
	(.0052)	(.0058)	(.0035)
post	.0081***	−.00055	−.0022
	(.0032)	(.0047)	(.002)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	33,061	33,102	33,061
Number of deals	836	839	836
Adj. R^2	.77	.62	.86

*** Means statistically significant at the 1% level.

3.4. Robustness checks

The magnitude of these effects raises some concerns. First, as we mentioned earlier, it could be that private equity funds simply pick targets that were already growing very fast before the transaction. Absent an instrument for the LBO decision, we can partially address this valid concern by including an interaction term designed to control for pre-LBO growth:

$$Y_{jt} = \alpha_j + \delta_t + POST_{jt} + POST_{jt}LBO_j + POST_{jt}GR_j + \varepsilon_{jt}, \quad (2)$$

where GR_j is mean firm sales growth, in the three years preceding the transaction. The additional interaction term, $POST_{jt} \times GR_j$, captures the fact that LBO targets, compared to their control firms, may initially start with stronger growth. We run these regressions in Table 4, Panel A, for each dependent variable used in Table 2. Pre-LBO growth is a strong predictor of post-LBO growth, but it does not affect our initial estimates. This is not too surprising given the descriptive statistics of Table 1: targets and control firms tend to have similar pre-buyout growth, which is confirmed by the fact that Figs. 3 and 4 show a clear break in differential trend after the deal.

Second, we have also checked that our results are not driven by the fact that we pool the 4 years following the transaction into a single "post" period. This is already apparent in the graphical evidence of Figs. 2–4. Nevertheless, we looked at this more formally by computing year-by-year "median-adjusted" changes, as for instance in Kaplan (1989) and Guo, Hotckiss, and Song (2011). More precisely, we compute changes in targets' behavior at different points in time and compare them to the median change in behavior of their control firms. We report these results in Panel A of Table A3, where the changes in behavior are observed from $t-2$ to $t-1$ (column 1), $t-1$ to $t+1$ (column 2), $t-1$ to $t+2$ (column 3) and $t-1$ to $t+3$ (column 4), where t is the year of the ownership change. We find the results in Table 2 to be robust to this different specification: size (measured by employment, sales, or capital employed), profitability, post-buyout debt increase, and capital expenditures significantly increase immediately after the LBO and this increase remains significant at least up to 3 years after the transaction.

A third concern related to our results in Table 2 is that our evidence is based on recent deals, while older LBOs, even in France, were essentially motivated by the need to cut costs and downsize. Under this interpretation, the results in Table 2 would simply reflect the fact that we use a more recent sample than previous studies. This is not entirely true, as some papers (Amess and Wright, 2007; Davis, Haltiwanger, Jarmin, Lerner, and Javier, 2008) also use data from recent transactions, yet still find strong evidence of downsizing. As additional evidence, we run our regressions separately for years before and after 2000, and report the results in Panel B, Table 4. Although post-buyout employment and sales growth are slightly lower post-2000, all the effects described in Table 2 remain strongly significant in both periods, and not statistically different across sub-periods.

Fourth, we need to take into account the fact that our financial statements are not consolidated. LBO targets may initially have subsidiaries that are part of the entity

Table 4
Sample of LBO targets and their control firms (see text for details). Sample period: 1994–2004. OLS estimates of the impact of a LBO on targets' behavior including pre-buyout growth controls (Panel A) and for different sub-periods (Panel B). All regressions include firm and year fixed effects. post is a dummy equal to 1 for the 3 years following the LBO and 0 for the 3 years preceding the LBO. LBO is a dummy equal to 1 if the observation is an LBO target and 0 if it is a control firm. log(Empl) is the logarithm of employment. WC is working capital. FA is fixed assets. CAPEX is capital expenditures. Other variables are self-explanatory. Panel A controls for pre LBO growth by adding an interaction between firm level pre LBO sales growth and the post dummy. Panel B runs the estimation separately for pre and post 2000 transactions. Error terms are clustered at the deal × post level.

	ROA (1)	log(EBITDA) (2)	log(Empl) (3)	log(Sales) (4)	log(FA+WC) (5)	Leverage (6)	log(CAPEX) (7)
Panel A: Including controls for pre-buyout growth							
post × LBO	.043***	.18***	.12***	.11***	.06***	.026***	.23***
	(.007)	(.028)	(.019)	(.023)	(.019)	(.0063)	(.044)
post	.0056	.91***	.97***	1.4***	1.1***	.027**	.72***
× pre LBO sales growth	(.017)	(.071)	(.044)	(.059)	(.055)	(.014)	(.1)
post	−.025***	−.22***	−.17***	−.24***	−.15***	−.0055	−.19***
	(.0051)	(.02)	(.011)	(.015)	(.014)	(.004)	(.034)
Observations	32,861	27,454	32,755	33,014	32,596	32,229	32,603
Number of deals	839	793	839	936	839	838	839
Adj. R^2	.53	.87	.94	.92	.93	.61	.72
Panel B: Sub period robustness							
Year of deal ≤ 2000							
post × LBO	.036***	.15***	.18***	.15***	.084***	.028***	.22***
	(.0095)	(.042)	(.03)	(.038)	(.03)	(.0093)	(.061)
post	−.03***	−.14***	−.075***	−.098***	−.051***	−.0052	−.072
	(.0068)	(.028)	(.014)	(.019)	(.017)	(.0056)	(.046)
Observations	16,473	14,137	16,405	16,532	16,355	16,124	16,340
Number of deals	394	377	394	393	394	393	394
Adj. R^2	.48	.86	.93	.91	.94	.59	.73
Year of deal > 2000							
post × LBO	.052***	.22***	.067**	.083***	.056**	.025***	.27***
	(.01)	(.04)	(.028)	(.03)	(.026)	(.0083)	(.066)
post	−.013*	−.078***	−.054***	−.068***	−.021	−.0017	−.16***
	(.0071)	(.027)	(.013)	(.017)	(.017)	(.0052)	(.05)
Observations	16,530	13,341	16,482	16,529	16,380	16,242	16,404
Number of deals	445	416	445	443	445	445	445
Adj. R^2	.56	.88	.94	.93	.93	.63	.72
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* Means statistically significant at the 10% level.
** Means statistically significant at the 5% level.
*** Means statistically significant at the 1% level.

bought out.[20] One possible outcome of the LBO could be a formal simplification where the buyout target is merged with its subsidiaries. This would mechanically increase the employment and perhaps even the assets of the target. To check this, Table 5 presents two separate robustness checks. First, we restrict the sample to firms that have no subsidiary according to INSEE data (both control firms and targets). This reduces the sample by approximately one-third. Estimates are reported in column 1 (log employment) and column 3 (log of fixed tangible assets, a subset of total fixed assets reported in Table 2). Post-LBO employment growth remains similar to the estimates provided in Table 2 (14% vs. 12%). Second, we use all targets, but add the employment and tangible fixed assets of the firms in our sample to that of their potential subsidiaries.[21] This is done in column 2 (for employment) and column 4 (for tangible assets). Again, these estimates are very close to the original ones in Table 2.

A fifth concern is attrition. While some buyouts create jobs, others may simply lead to firm destruction. When a firm closes a plant, it still appears in our sample the year after, so that the negative contribution of plant closure to firm job growth would still indirectly appear in our data. But when the firm itself disappears from our sample, in principle, job growth should be 100%, while our data would report the firm missing. In the data, attrition is, however, not likely to be a concern. Looking directly at the attrition rate from our tax files, we find that 15.24% of our targets exit the tax files in the three years following the deal compared to 15.4% for control firms. Since firm exit from tax files may be a bad proxy of actual job

[20] We defer the reader to Section 2.1 on data construction for more details on conglomerates in our sample.

[21] We focus on tangible fixed assets since these are the assets than can be consolidated between a target and its subsidiaries. For a parent (*footnote continued*) firm, another part of fixed assets is financial assets, which includes equity holding in, and loans to, subsidiaries. Consolidating such assets would amount to double-counting the subsidiaries' assets.

Table 5
Sample of LBO targets and their control firms (see text for details). Sample period: 1994–2004. OLS estimates of the impact of a LBO on employment and assets for stand-alone and multi-division targets. All regressions include firm and year fixed effects. post is a dummy equal to 1 for the 3 years following the LBO and 0 for the 3 years preceding the LBO. LBO is a dummy equal to 1 if the observation is an LBO target and 0 if it is a control firm. Error terms are clustered at the deal × post level.

	Log(employment)		Log(tangible fixed assets)	
	Stand alones only (1)	Target + subsidiaries (2)	Stand alones only (3)	Target + subsidiaries (4)
post × LBO	.14***	.11***	.13***	.093***
	(.027)	(.021)	(.038)	(.024)
post	−.061***	−.057***	−.028**	−.032**
	(.011)	(.011)	(.014)	(.014)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	18,458	32,960	18,141	32,538
Number of deals	316	839	315	838
Adj. R^2	.94	.91	.94	.93

** Means statistically significant at the 5% level.
*** Means statistically significant at the 1% level.

destruction, we also used actual bankruptcy files from the statistical office, which report the identifying numbers and date of filing of all bankrupt firms in France. We find no difference in bankruptcy rates between targets and control firms after the deal. At some point, 6.67% of targets and 6.7% of control firms will be bankrupt. Following the buyout, 4.58% of control firms and 4.17% of targets will be bankrupt within three years. Overall, it does not seem that attrition from the sample, either because of bankruptcy or takeover, is significantly different for targets and their control firms.

A sixth concern comes from the fact that the rigidity of labor laws in France may be driving our results. For instance, the Organization for Economic Cooperation and Development (OECD) ranks French law as the sixth most protective amongst the 28 member countries (OECD, 2004). Because it is difficult to lay off workers in France, it could be that we do not observe in our data the "cost-cutting" buyouts that are possibly more prevalent in the US and the UK, where labor markets are supposed to be more flexible. Hence, the observed post-buyout growth would be mechanically higher in the French sample. To test for this selection effect, we ask if post-LBO growth is, on average, higher in industries where employment rigidity is higher. We run the following modified version of Eq. (1):

$$Y_{jt} = \alpha_j + \delta_t + POST_{jt} + POST_{jt}LBO_j + POST_{jt}RIGID_j + RIGID_jPOST_{jt}LBO_j + \varepsilon_{jt}, \quad (3)$$

where Y_{jt} stands for employment, assets (in logs), or ROA. $RIGID_j$ is one of the two measures of employment "rigidity" and is defined at the level of the industry of firm j: fraction of unionized workers and fraction of workers under fixed-term contracts (see Section 2.4).

Estimates of the above equation are reported in Table 6. Panel A uses the fraction of unionized workers as proxy for employment rigidity and Panel B uses the fraction of workers under fixed-term contracts. It is apparent from columns 3 to 8 of Table 6 that post-buyout growth is not stronger in industries where employment is more rigid: none of the interaction coefficients is significant at the 5% confidence level and the point estimates of the interaction are small. This is not consistent with the hypothesis that post-LBO growth in France is larger because employees are more protected. Additionally, columns 1 and 2 of Table 6 indicate that post-LBO increase in performance is not higher in industries with less rigid employment. We view this as additional proof that value creation does not seem to come primarily from cost-cutting strategies.

4. Financial constraints and post LBO growth

Our results so far appear to be very dissimilar to pre-existing studies, in particular, those focusing on large public-to-private transactions (Kaplan, 1989; Guo, Hotckiss, and Song, 2011), but also smaller deals (Amess and Wright, 2007; and Davis, Haltiwanger, Jarmin, Lerner, and Javier, 2008). Both types of studies find evidence consistent with private equity funds implementing measures that aim at downsizing target operations, while maintaining its ability to create value (i.e., holding EBITDA constant). Such a discrepancy between these results and ours begs for a more thorough investigation, which we attempt to provide here.

In this section, we provide evidence consistent with the following hypothesis: French LBO targets tend to be credit-constrained firms with growth opportunities, and private equity (PE) funds help these firms get access to additional sources of outside finance. Some of these funds, affiliated with local banks, may be able to help firms grow through (almost direct) lending.[22] But we believe the mechanism is much more pervasive that this, because PE sponsors help make their portfolio firms more credible borrowers on credit markets. First, as better monitors but still residual claimants,

[22] Consistent with this idea, Demiroglu and James (2010) find that reputable private equity groups obtain narrower bank spreads to finance their acquisitions, suggesting that some of these groups may be able to decrease the financing costs of their portfolio firms.

Table 6
Sample of LBO targets and their control firms (see text for details). Sample period: 1994–2004. OLS estimates of the impact of a LBO on targets' behavior in industries with different levels of employment rigidity. All regressions include firm and year fixed effects. post is a dummy equal to 1 for the 3 years following the LBO and 0 for the 3 years preceding the LBO. LBO is a dummy equal to 1 if the observation is an LBO target and 0 if it is a control firm. log(Empl) is the logarithm of employment. WC is working capital. FA is fixed assets. CAPEX is capital expenditures. In Panel A, rigidity is the fraction of unionized workers in the industry. In Panel B, rigidity is minus the fraction of workers under fixed term contracts in the industry. Other variables are self-explanatory. Error terms are clustered at the deal × post level.

	ROA (1)	log(EBITDA) (2)	log(Empl) (3)	log(Sales) (4)	log(FA+WC) (5)	Leverage (6)	log(CAPEX) (7)
Panel A: Rigidity is unionization rate							
post × LBO	−.005	.31	.14	−.3	−.26	−.14*	−.71
× Rigidity	(.086)	(.28)	(.14)	(.27)	(.23)	(.084)	(.54)
post × LBO	.056***	.11*	.076**	.16***	.1**	.048***	.32***
	(.017)	(.063)	(.034)	(.048)	(.045)	(.016)	(.1)
lbo × rigidity	.054	−1.3	−1.6	−.67	−1.3	−.0074	−3.3
	(.29)	(.83)	(1.1)	(.77)	(.82)	(.26)	(2)
post × rigidity	−.013	.063	−.012	.0033	.2***	.095***	.1
	(.031)	(.11)	(.053)	(.075)	(.079)	(.027)	(.18)
rigidity	−.081	−.19	−.39	−.096	.073	.22***	−.54
	(.12)	(.37)	(.37)	(.43)	(.39)	(.09)	(.72)
post	−.029***	−.092***	−.046***	−.048***	−.037*	−.019***	−.08
	(.0091)	(.032)	(.013)	(.019)	(.021)	(.0071)	(.051)
Observations	17,218	14,859	17,274	17,344	17,028	16,838	17,095
Number of deals	438	423	438	437	438	438	438
Adj. R^2	.54	.87	.95	.94	.94	.62	.75
Panel B: Rigidity is fraction of FTC in industry							
post × LBO	.13	.39	.24	.085	.29	−.012	−.17
× Rigidity	(.099)	(.32)	(.19)	(.19)	(.25)	(.08)	(.39)
post × LBO	.063***	.19***	.12***	.11***	.075**	.021**	.18***
	(.012)	(.049)	(.029)	(.035)	(.034)	(.0099)	(.068)
lbo × rigidity	.83**	−1.3	−.55	−.91	−1.9	−.15	−2.2
	(.36)	(1)	(1)	(.93)	(1.2)	(.34)	(2.3)
post × rigidity	−.055	−.22	−.14**	−.12	.14	.086**	−.16
	(.049)	(.14)	(.071)	(.08)	(.11)	(.04)	(.21)
rigidity	−.51**	−.15	.42	.62*	.96**	.41*	1.5
	(.23)	(.65)	(.33)	(.34)	(.44)	(.24)	(1)
post	−.034***	−.096***	−.057***	−.055***	.0079	.0034	−.073*
	(.0076)	(.026)	(.012)	(.015)	(.017)	(.0056)	(.043)
Observations	17,218	14,859	17,274	17,344	17,028	16,838	17,095
Number of deals	438	423	438	437	438	438	438
Adj. R^2	.54	.87	.95	.94	.94	.62	.75
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* Means statistically significant at the 10% level.
** Means statistically significant at the 5% level.
*** Means statistically significant at the 1% level.

they make debt relatively safe and attractive to bankers. Second, private equity sponsors may introduce new, often financially savvy, members to the executive suite, which probably reassures creditors. Third, because of their long lock-up periods, private equity funds may be more patient than families, who need dividends to consume, and as a result are more ready to reinvest free cash flows into the company. Last, an oft-cited argument in the profession is also that with capital gains being less taxed than dividends, private equity funds are encouraged by their investors to reinvest cash flows instead of paying out dividends.

4.1. Private-to-private transactions versus divisional buyouts

One first implication of our hypothesis is that firms that are initially financially unconstrained should not grow after the LBO. Subsidiaries of larger industrial groups are a good example of firms less likely to be credit-constrained, since they initially benefit from internal capital markets (Hoshi, Kashyap, and Scharfstein, 1991). Publicly listed firms are another example of firms that are less likely to be credit-constrained.

Following this intuition, we break down the sample into four different groups based on pre-buyout ownership: (1) private-to-private LBOs, where the seller typically is an individual or the founding family,[23] (2)

[23] The label "private-to-private transaction" comes from Capital IQ or SDC. We have cross-checked it for a subsample of our transactions. Using company Web sites, the business press, and the DIANE data set, we have manually checked the seller's identity for all 47 private-to-private deals that took place in 2004. We could find information for 40 sellers only: only three of them were not individuals or families. One of

divisional LBOs, where the target is initially an affiliate of a conglomerate, (3) secondary LBOs, where the seller is another private equity group and (4) public-to-private LBOs, where the target is publicly traded before the deal. We report regression results in four separate panels in Table 7. We first note from columns 1 and 2 that the increase in profitability following an LBO is pervasive across these four groups. It is slightly larger for private-to-private transactions and public-to-private deals, but the difference between these deals and secondary and divisional deals is not statistically different from zero. This is not entirely surprising since irrespective of the type of deal, an LBO is supposed to generate value for the private equity fund. However, this leaves open the question of whether this increase in profitability observed across LBO types is necessarily achieved through a "growth" strategy.

In columns 3–5, we look at firm size (employment, sales, and capital employed), and find striking differences between the various types of LBOs. Most notably, we find that post-buyout growth in divisional buyouts is small (-2.6% for capital employed, 3.5% for sales, and 6% only for employment), and statistically insignificant. Public-to-private deals (in Panel D) lead to a decline in firm size (-16% for sales growth and -5.8% for capital employed). Consistent with Kaplan (1989), downsizing for this sample is strong but statistically insignificant, probably due to small sample size. Following a private-to-private LBO, however, employment (respectively sales and capital employed) increases by about 18% (respectively, 18% and 13%). Such growth is not only statistically significant, but is also statistically different (smaller) from growth observed in public-to-private transactions. Analyzing a British data set of private-to-private buyouts, Chung (2009) finds similar levels post-LBO growth: for instance, sales of LBO targets grow by 30% more than the rest of the industry.

As further evidence that LBOs are indeed alleviating credit constraints for targets in private-to-private transactions, we find that these deals are followed by a significant 36% increase in capital expenditures (column 7), compared to an insignificant increase of 11% for divisional buyouts, and a -31% decrease for public-to-private deals (the difference is statistically significant). The evidence regarding post-buyout debt increase reported in column 4 is also consistent with our credit-constraint hypothesis. On the one hand, target leverage in private-to-private transactions increase, by a significant 4 percentage points; on the other hand, it remains constant for targets of divisional buyouts. This difference is again significant at the 3% confidence level. Private equity groups, when they acquire firms that are likely to be credit-constrained (i.e., family firms as opposed to divisions of conglomerates) thus seem to help these targets increase their debt capacity, allowing them to increase their capital expenditures and eventually to grow faster.

Finally, Panel C of Table 7 shows that secondary LBOs look very much like private-to-private LBOs on many dimensions. For these deals, ROA increases by a significant 3.4 percentage points (column 1), and employment (respectively, sales and capital expenditures) increases by a significant 11% (respectively, 17% and 31%). The post-buyout growth in capital employed is a bit smaller (6.6% significant at the 10% confidence level) but the difference with private-to-private capital growth is not significant at the 10% confidence level. An important distinction, however, between these secondary LBOs and private-to-private LBOs is that secondary buyouts are not followed by a post-buyout increase in debt issuance: target leverage does not rise. One possible interpretation of these results is that the "infusion" of capital made at the time of the first LBO, as well as retained earnings, may be sufficient to allow the secondary fund to keep on implementing a "growth" strategy, but without a need to further access the credit market.

Another source of cross-sectional heterogeneity we have explored in order to test for the credit-constraint hypothesis is target size. Indeed, firm size is a widely used indicator of credit constraints in corporate finance (since at least Fazzari, Petersen, and Hubbard, 1988). In unreported regressions, we split our sample by pre-buyout target size. Large LBO targets are those whose number of employees is larger than the median one year prior to the transaction. Small targets are the rest. Using this breakdown, we find that post-LBO growth in employment and capital employed is significantly larger (by 16% and 22%, respectively) in small firms relative to large firms.[24] This last result is also interesting in that it can reconcile our results with the previous literature on LBOs. This literature uses mostly large deals and finds no (or a negative) effect of LBOs on growth: in our sample too, large LBOs generate less growth.

All in all, these and the previous results[25] suggest that, for firms that are more likely to be initially credit-constrained (privately held family firms, small firms, stand-alone firms), leveraged buyouts lead to an increase in debt capacity, corporate investment, and eventually firm growth in assets, jobs, and sales.

4.2. Financial dependency and post LBO growth

In this section, we propose an additional test for the hypothesis that private equity sponsors help previously credit-constrained firms access additional sources of outside finance, leading to an increase in investment and firm growth. As Rajan and Zingales (1998) show, some industries rely more on outside finance to fund investment

(*footnote continued*)
them was a co-op, another one had a financial institution as majority shareholder, and a manufacturing firm controlled the last one. We thank Laurent Bach for letting us access his data set on private firms' ownership.

[24] Note that this is not a return-to-the-mean effect, as this higher post-LBO growth is relative to control firms, which have similar sizes to LBO targets by construction.

[25] Note that in Panels B–E of Table A3, we test the robustness of these results to the alternative specification presented in Section 2.4, i.e., where we adjust each target's behavior for the median of the group of its control firms. The results are broadly similar with the results in Table 7.

Table 7
Sample of LBO targets and their control firms (see text for details). Sample period: 1994–2004. OLS estimates of the impact of a LBO on targets' behavior for different types of deals. All regressions include firm and year fixed effects. post is a dummy equal to 1 for the 3 years following the LBO and 0 for the 3 years preceding the LBO. LBO is a dummy equal to 1 if the observation is an LBO target and 0 if it is a control firm. log(Empl) is the logarithm of employment. WC is working capital. FA is fixed assets. CAPEX is capital expenditures. Panel A uses private-to-private transactions only; Panel B uses divisional buyouts only; Panel C uses secondary buyouts only; Panel D uses public-to-private deals only. "Test divisional = private-to-private" (resp. "Test secondary = private-to-private" and "Test public = private-to-private") is the p-value of a test of equality of the post × LBO coefficient obtained using private-to-private deals only and the post × LBO coefficient obtained using divisional (resp. secondary and public) buyouts only. Error terms are clustered at the deal × post level.

	ROA (1)	log(EBITDA) (2)	log(Empl) (3)	log(Sales) (4)	log(FA+WC) (5)	Leverage (6)	log(CAPEX) (7)
Panel A: Private-to-private LBOs							
post × LBO	.051***	.21***	.18***	.18***	.13***	.04***	.36***
	(.01)	(.042)	(.03)	(.035)	(.028)	(.0077)	(.062)
post	−.035***	−.17***	−.058***	−.09***	−.031**	.0005	−.11***
	(.0068)	(.025)	(.012)	(.017)	(.016)	(.0048)	(.043)
Observations	17,767	15,208	17,715	17,819	17,600	17,398	17,580
Number of deals	438	421	438	438	438	437	438
Adj. R^2	.52	.85	.92	.91	.93	.6	.71
Panel B: Divisional LBOs							
post × LBO	.034***	.16***	.062*	.035	−.026	.0043	.11
	(.012)	(.056)	(.038)	(.047)	(.043)	(.014)	(.091)
post	.0055	−.015	−.077***	−.045*	−.039	−.0052	−.11*
	(.0093)	(.037)	(.019)	(.025)	(.027)	(.008)	(.063)
Observations	8,647	6,924	8,619	8,664	8,588	8,472	8,600
Number of deals	229	216	229	228	229	229	229
Adj. R^2	.49	.88	.93	.92	.92	.6	.73
Test divisional = private-to-private	(.31)	(.46)	(.02)**	(.01)***	(.005)***	(.03)**	(.03)**
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel C: Secondary LBOs							
post × LBO	.034**	.17***	.11**	.17***	.066*	.016	.31***
	(.017)	(.056)	(.048)	(.048)	(.037)	(.015)	(.1)
post	−.036***	−.13***	−.068***	−.12***	−.041	−.0013	−.061
	(.012)	(.048)	(.021)	(.033)	(.031)	(.01)	(.087)
Observations	4,943	4,111	4,917	4,935	4,912	4,871	4,929
Number of deals	129	120	129	127	129	129	129
Adj. R^2	.57	.87	.95	.92	.93	.64	.72
Test secondary = private-to-private	(.38)	(.65)	(.25)	(.79)	(.17)	(.13)	(.70)
Panel D: Public-to-private LBOs							
post × LBO	.05*	.069	.0011	−.16	−.058	.053*	−.31
	(.029)	(.16)	(.076)	(.12)	(.063)	(.03)	(.21)
post	−.015	.0086	−.099*	−.1	−.09*	−.014	−.29*
	(.017)	(.1)	(.053)	(.078)	(.05)	(.02)	(.18)
Observations	1,438	1,076	1,430	1,434	1,428	1,424	1,430
Number of deals	36	32	36	36	36	36	36
Adj. R^2	.58	.91	.94	.93	.97	.65	.75
Test public = private-to-private	(.96)	(.36)	(.03)**	(.01)***	(.01)***	(.69)	(.01)***
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* Means statistically significant at the 10% level.
** Means statistically significant at the 5% level.
*** Means statistically significant at the 1% level.

while firms that belong to other, less "financially dependent" industries suffer less from credit constraints (they tend to finance investment internally). As a consequence, post-buyout growth, post-buyout debt increase (as measured by target's leverage, i.e., excluding LBO-related debt), and capital expenditures should be concentrated among targets that operate in such industries.

More specifically, we run the following set of regressions:

$$Y_{jt} = \alpha_j + \delta_t + POST_{jt} + POST_{jt}LBO_j + POST_{jt}FD_j + FD_jPOST_{jt}LBO_j + \varepsilon_{jt}, \quad (4)$$

for firm i in year t. Y_{jt} stands for ROA, log EBITDA, log employment, log sales, log capital employed, target leverage,

Table 8

Note: Sample of LBO targets and their control firms (see text for details). Sample period: 1994–2004. OLS estimates of the impact of a LBO on targets' behavior, as a function of industry financial dependence. All regressions include firm and year fixed effects. post is a dummy equal to 1 for the 3 years following the LBO and 0 for the 3 years preceding the LBO. LBO is a dummy equal to 1 if the observation is an LBO target and 0 if it is a control firm. log(Empl) is the logarithm of employment. WC is working capital. FA is fixed assets. CAPEX is capital expenditures. Fin. Dep. is a measure of industry financial dependence. Panel A uses private-to-private transactions only; Panel B uses divisional buyouts only; Panel C uses secondary buyouts only; Panel D uses public-to-private deals only. Error terms are clustered at the deal × post level.

	ROA (1)	log(EBITDA) (2)	log(Empl) (3)	log(Sales) (4)	log(FA+WC) (5)	Leverage (6)	log(CAPEX) (7)
Panel A: Private-to-private LBOs							
post × LBO	−.035*	.062	.16***	.11**	.2***	.028**	.35***
× Fin. Dep.	(.02)	(.078)	(.037)	(.049)	(.048)	(.014)	(.12)
post × LBO	.064***	.17***	.094***	.11***	.038	.025***	.18**
	(.013)	(.047)	(.023)	(.031)	(.03)	(.0091)	(.078)
post × Fin. Dep.	.06***	.09***	.025	.015	−.013	−.0093	.066
	(.0082)	(.033)	(.016)	(.021)	(.02)	(.006)	(.051)
LBO × Fin. Dep.	.021	.099	−.49***	−.44***	−.081	−.044*	−.47**
	(.035)	(.15)	(.069)	(.092)	(.084)	(.025)	(.24)
Fin. Dep.	−.12***	−.9***	−.5***	−.78***	−.12***	−.012	−1.6***
	(.018)	(.08)	(.037)	(.048)	(.044)	(.013)	(.12)
post	−.062***	−.2***	−.068***	−.095***	−.025	.0053	−.14***
	(.0076)	(.028)	(.014)	(.019)	(.018)	(.0055)	(.047)
Observations	17,501	14,957	17,438	17,542	17,338	17,139	17,373
Number of deals	438	421	438	438	438	437	438
Adj. R^2	.53	.85	.93	.92	.93	.61	.71
Panel B: Divisional LBOs							
post × LBO	.018	.16	−.019	.014	−.19**	−.05**	.28
× Fin. Dep.	(.026)	(.12)	(.059)	(.075)	(.083)	(.023)	(.19)
post × LBO	.029*	.083	.06*	.017	.039	.028**	−.024
	(.016)	(.07)	(.036)	(.045)	(.051)	(.014)	(.11)
post × Fin. Dep.	.017	.028	−.065**	−.041	.087**	.0095	.049
	(.012)	(.055)	(.027)	(.033)	(.037)	(.01)	(.084)
LBO × Fin. Dep.	.086**	.19	−.15	.39***	.39***	.05	−.34
	(.043)	(.26)	(.11)	(.13)	(.14)	(.039)	(.32)
Fin. Dep.	−.025	−.48***	−.48***	−.37***	−.43***	−.0066	−.94***
	(.021)	(.11)	(.058)	(.065)	(.068)	(.019)	(.16)
post	−.0027	−.027	−.043**	−.024	−.071**	−.0095	−.13*
	(.01)	(.042)	(.022)	(.028)	(.032)	(.0087)	(.072)
Observations	8,540	6,833	8,511	8,554	8,481	8,366	8,533
Number of deals	229	216	229	228	229	229	229
Adj. R^2	.5	.88	.93	.92	.92	.6	.73
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel C: Secondary LBOs							
post × LBO	−.066**	−.35**	−.077	−.1	−.11	−.048*	.17
× Fin. Dep.	(.034)	(.16)	(.058)	(.09)	(.087)	(.027)	(.24)
post × LBO	.067***	.29***	.1***	.18***	.11**	.031*	.24
	(.021)	(.079)	(.035)	(.056)	(.054)	(.017)	(.15)
post × Fin. Dep.	.071***	.12**	.094***	.12***	.094***	−.02*	.81***
	(.013)	(.059)	(.023)	(.036)	(.034)	(.011)	(.094)
LBO × Fin. Dep.	.2***	.45	−2.1***	−1.4***	−.2	−.072	−1.2**
	(.072)	(.46)	(.13)	(.19)	(.19)	(.058)	(.5)
Fin. Dep.	−.089***	−.34***	−.038	−.31***	−.41***	.057***	−1.5***
	(.027)	(.13)	(.053)	(.079)	(.069)	(.022)	(.19)
post	−.069***	−.17***	−.11***	−.18***	−.088***	.0096	−.46***
	(.013)	(.051)	(.023)	(.036)	(.034)	(.011)	(.094)
Observations	4,918	4,087	4,892	4,910	4,887	4,847	4,913
Number of deals	129	120	129	127	129	129	129
Adj. R^2	.57	.87	.96	.93	.93	.64	.73
Panel D: Public–to–private LBOs							
post × LBO	−.014	.2	.11	−.025	.0083	.029	.47
× Fin. Dep.	(.034)	(.27)	(.12)	(.14)	(.096)	(.036)	(.38)
post × LBO	.064**	−.0043	−.066	−.14	−.067	.036	−.6*
	(.029)	(.19)	(.099)	(.13)	(.084)	(.031)	(.33)
post × Fin. Dep.	.02	.089	−.098**	−.046	.086**	.0015	.36**
	(.014)	(.11)	(.048)	(.061)	(.04)	(.015)	(.16)
LBO × Fin. Dep.	−.18*	−.26	−.14	−.059	.38	−.066	−.56
	(.096)	(.64)	(.33)	(.41)	(.28)	(.1)	(1.1)
Fin. Dep.	−.054*	−1.6***	−.77***	−1***	−.044	−.049	−.51
	(.029)	(.23)	(.098)	(.12)	(.085)	(.031)	(.32)

Table 8 (*continued*)

	ROA (1)	log(EBITDA) (2)	log(Empl) (3)	log(Sales) (4)	log(FA+WC) (5)	Leverage (6)	log(CAPEX) (7)
post	−.03*	−.078	−.057	−.11	−.15***	−.017	−.56***
	(.018)	(.12)	(.061)	(.078)	(.052)	(.019)	(.21)
Observations	1,438	1,076	1,430	1,434	1,428	1,424	1,430
Number of deals	36	32	36	36	36	36	36
Adj. R^2	.58	.91	.94	.94	.97	.65	.76
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* Means statistically significant at the 10% level.
** Means statistically significant at the 5% level.
*** Means statistically significant at the 1% level.

Table A1
Sample of targets of LBOs and their control firms (see text for details). Sample period: 1994–2004. OLS estimates of the impact of a LBO on targets' performance, for different types of deals. All regressions include firm and year fixed effects. post is a dummy equal to 1 for the 3 years following the LBO and 0 for the 3 years preceding the LBO. LBO is a dummy equal to 1 if the observation is an LBO target and 0 if it is a control firm. Column 1 and 2 use the whole sample of LBOs; column 3 and 4 use private-to-private deals only; column 5 and 6 use divisional buyouts only; column 7 and 8 use secondary LBOs only; column 9 and 10 use public-to-private transactions only. Error terms are clustered at the deal × post level. Test equality with private-to-private LBO is is the *p*-value of a test of equality of the post × LBO coefficient obtained using private-to-private deals only and the post × LBO coefficient obtained using divisional (resp. secondary and public) buyouts only.

	All transactions		***Private-to-private***		***Divisional***		***Secondary***		***Public-to-private***	
	ROA	EBITDA/ sales	ROA	EBITDA/ sales	ROA	EBITDA/ sales	ROA	EBITDA/ sales	ROA	EBITDA/ sales
post × LBO	.044***	.014***	.051***	.011**	.034***	.018**	.034**	.012	.05*	.028
	(.007)	(.0046)	(.01)	(.0051)	(.012)	(.0084)	(.017)	(.014)	(.029)	(.044)
post	−.025***	−.0083***	−.035***	−.0089**	.0055	−.0016	−.036***	−.014*	−.015	−.014
	(.0048)	(.0029)	(.0068)	(.0038)	(.0093)	(.0051)	(.012)	(.0079)	(.017)	(.025)
Observations	33,003	32,440	17,767	17,502	8,647	8,517	4,943	4,818	1,438	1,396
Number of deals	839	831	438	435	229	228	129	126	36	35
Adj. R^2	.53	.59	.52	.52	.49	.53	.57	.69	.58	.7
Test equality with private-to-private LBOs					.31	.47	.38	.92	.96	.69

* Means statistically significant at the 10% level.
** Means statistically significant at the 5% level.
*** Means statistically significant at the 1% level.

and log capital expenditures. FD_j is the industry-level measure of a firm's dependence on external finance, whose construction is described in Section 2.4.

Regression results are reported in Table 8. Panel A focuses on private-to-private LBOs. Columns 1 and 2 show that financial dependence has little explanatory power on the post-buyout increase in profitability of private-to-private transactions. If anything, ROA seems to experience less of an increase following the buyout in more financially dependent industries. One possible interpretation is that targets in non-financially constrained industries are also able to increase their profits, but through cost-cutting strategies rather than growth strategy.

More interestingly, columns 3–7 of Panel A in Table 8 show that, for private-to-private LBOs (i.e., those deals where targets are the most likely to suffer from credit constraints), post-buyout growth and increase in target leverage and capital expenditures are concentrated among targets that operate in industries that are more financially dependent: all the estimates of the interaction term $FD \times POST \times LBO$ are statistically significant. These effects are also economically important. Going from the 25th percentile to the 75th percentile of financial dependence increases the level of financial dependence by about .6. Hence, post-LBO employment (respectively sales, capital employed and capital expenditure) growth difference between two such industries would be around 10% (respectively, 7%, 12%, and 21%). This has to be compared with an average excess post-LBO growth, for private-to-private deals, of about 18% for employment, 18% for sales, 13% for capital employed, and 36% for capital expenditures (see Table 7). Hence, greater growth in financially dependent industries explains a large fraction of the average post-LBO growth in the sample. Similarly, and consistent with the idea that initial credit constraints are at the origin of post-LBO growth in private-to-private deals, we find in column 6 of Panel A, Table 8, that the post-buyout increase in debt following private-to-private LBOs is stronger for targets that operate in industries that are more financially dependent. Going

Table A2
Sample of LBO targets and their control firms (see text for details). Sample period: 1994–2004. OLS estimates of the impact of a LBO on targets' number of plants. All regressions include firm and year fixed effects. The dependent variable is a dummy variable equal to 1 if the firm has increased the number of its plants from year t to year $t+1$. post is a dummy equal to 1 for the 3 years following the LBO and 0 for the 3 years preceding the LBO. LBO is a dummy equal to 1 if the observation is an LBO target and 0 if it is a control firm. Column 1 uses the whole sample; column uses private-to-private transactions only; column 3 uses divisional buyouts only; column 4 uses secondary buyouts only; column 5 uses public-to-private deals only. Error terms are clustered at the deal × post level.

	(# of plants > # of plants($t-1$))				
	Whole sample	Private-to-private LBOs	Divisional LBOs	Secondary LBOs	Public-to-private LBOs
	(1)	(2)	(3)	(4)	(5)
post × LBO	.012	.011	.028	.0053	−.038
	(.0092)	(.012)	(.019)	(.023)	(.038)
post	−.0035	.0053	−.034**	.025	−.077*
	(.0074)	(.0092)	(.016)	(.021)	(.044)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	24,084	13,508	6,169	3,273	1,020
Number of deals	762	409	205	107	36
Adj. R^2	.22	.22	.22	.24	.22

* Means statistically significant at the 10% level.
** Means statistically significant at the 5% level.

Table A3
Note: Sample of LBO targets and their control firms (see text for details). Sample period: 1994–2004. In this table we compute, for each target, the change in performance between year $T+t$ and year $T+t'$, where T is the year of the deal. For ROA and leverage, we compute level changes from year t to t'; for all other variables, we compute percentage changes from year t to year t'. We adjust each target's change in performance by subtracting the median change in performance of its control firms over the same period. The table reports the median of this adjusted-change in performance, by LBO type. In column (1), $t=-2$ and $t'=-1$; in column (2), $t=-1$ and $t'=1$; in column (3), $t=-1$ and $t'=2$; in column (4), $t=-1$ and $t'=3$. Significance levels of medians are based on a two-tailed Wilcoxon rank test. WC is working capital. FA is fixed assets. All other variables are self-explanatory. Panel A uses all types of deals. Panel B uses private-to-private deals only; Panel C uses divisional LBOs only; Panel D uses secondary LBOs only; Panel E uses public-to-private deals only. Numbers of observations are between brackets.

	Median of the differential change between targets and controls			
	From −2 to −1	From −1 to +1	From −1 to +2	From −1 to +3
	(1)	(2)	(3)	(4)
Panel A: All LBOs				
ROA	.005	.014***	.022***	.021***
	[810]	[757]	[719]	[556]
Employment	0	.007***	.057***	.075***
	[810]	[773]	[732]	[567]
Sales	.013***	.021***	.017***	.047***
	[803]	[767]	[726]	[562]
FA+WC	.005**	.039***	.031***	.095***
	[813]	[769]	[728]	[564]
Leverage	.001	.01*	.012**	.017***
	[793]	[743]	[701]	[535]
CAPEX	.023***	.087***	0***	.091***
	[797]	[752]	[711]	[555]
Panel B: Private-to-private LBOs				
ROA	.002	.016***	.023*	.02*
	[423]	[394]	[373]	[297]
Employment	0	.026***	.086***	.097***
	[421]	[403]	[379]	[301]
Sales	.023***	.025***	.025**	.086***
	[417]	[399]	[375]	[298]
FA+WC	.006*	.063***	.08***	.146***
	[424]	[400]	[376]	[299]
Leverage	0	.015***	.016***	.042***
	[416]	[387]	[366]	[289]
CAPEX	.027**	.157***	.043***	.115***
	[411]	[386]	[362]	[291]
Panel C: Divisional LBOs				
ROA	.011	.012	.028**	.006
	[220]	[208]	[198]	[163]
Employment	−.023*	−.009	.025	.034
	[220]	[214]	[204]	[168]
Sales	−.031	−.003	.013	−.011
	[218]	[212]	[202]	[166]
FA+WC	−.002	.025*	.005	.06
	[222]	[213]	[203]	[167]
Leverage	.007	−.007	.004	.001
	[215]	[205]	[190]	[156]
CAPEX	.022**	−.037	−.049	.058**
	[221]	[212]	[202]	[167]
Panel D: Secondary LBOs				
ROA	.004	.033***	.003	.029
	[124]	[116]	[111]	[74]
Employment	.01**	.07***	.055***	.086**
	[126]	[117]	[112]	[76]
Sales	.04***	.081***	.03*	.053**
	[125]	[117]	[112]	[76]
FA+WC	.009	.025**	−.008	.003
	[124]	[117]	[112]	[76]
Leverage	−.001	.020	−.003	.011
	[121]	[113]	[110]	[70]
CAPEX	−.041	.254***	0	.236*
	[123]	[116]	[111]	[76]
Panel E: Public-to-private LBOs				
ROA	.003	−.005	.012	.08**
	[36]	[34]	[32]	[21]
Employment	−.016	−.02	−.014	.11
	[36]	[34]	[32]	[21]
Sales	−.028	−.03	−.037	.167
	[36]	[34]	[32]	[21]
FA+WC	.036	−.043	−.107	−.133
	[36]	[34]	[32]	[21]

Table A3 (*continued*)

	Median of the differential change between targets and controls			
	From −2 to −1 (1)	From −1 to +1 (2)	From −1 to +2 (3)	From −1 to +3 (4)
Leverage	−.010	.007	.024*	−.003
	[36]	[34]	[31]	[19]
CAPEX	.171	−.441	−.383	−.453
	[35]	[33]	[31]	[20]

* Means statistically significant at the 10% level.
** Means statistically significant at the 5% level.
*** Means statistically significant at the 1% level.

from the 25th to the 75th percentile of financial dependence leads to an increase in the post-buyout increase in target's leverage of 1.7 percentage points, i.e., around 40% of the overall post-buyout effect for private-to-private LBOs (column 6 of Panel A in Table 7).

Interestingly, Panels B through D of Table 8 show that none of the interaction terms ($FD \times POST \times LBO$) is positive and significant when estimated on other LBO types. While the financial dependence of a target's industry has a strong predictive power on post-buyout growth for private-to-private transactions, it does not explain targets' behavior following divisional, secondary, or public-to-private LBOs. Also, these results do not depend on the specific definition of financial dependence we use. Indeed, we present in Table A4 the estimation of Eq. (4) when we

Table A4
Sample of LBO targets and their control firms (see text for details). Sample period: 1994–2004. OLS estimates of the impact of a LBO on targets' behavior, as a function of industry financial dependence. All regressions include firm and year fixed effects. post is a dummy equal to 1 for the 3 years following the LBO and 0 for the 3 years preceding the LBO. LBO is a dummy equal to 1 if the observation is an LBO target and 0 if it is a control firm. log(Empl) is the logarithm of employment. WC is working capital. FA is fixed assets. CAPEX is capital expenditures. Fin. Dep. is a measure of industry financial dependence. Panel A uses private-to-private transactions only; Panel B uses divisional buyouts only; Panel C uses secondary buyouts only; Panel D uses public-to-private deals only. In the first four panels, industry financial dependence is computed over firms with more than 50 employees. In the last four panels, industry financial dependence is computed over firms with more than 200 employees. Error terms are clustered at the deal × post level.

	ROA (1)	log(EBITDA) (2)	log(Empl) (3)	log(Sales) (4)	log(FA+WC) (5)	Leverage (6)	log(CAPEX) (7)
Financial dependence is computed over firms with employees > 50							
Panel A: Private-to-private LBOs							
post × LBO	−.041**	.0087	.13***	.085*	.21***	.021	.37***
× Fin. Dep.	(.02)	(.079)	(.038)	(.05)	(.048)	(.015)	(.13)
post × LBO	.067***	.2***	.11***	.13***	.038	.028***	.18**
	(.013)	(.047)	(.024)	(.031)	(.03)	(.0092)	(.079)
post × Fin. Dep.	.055***	.076**	.025	.00025	−.02	−.0086	.054
	(.0083)	(.034)	(.016)	(.021)	(.02)	(.0061)	(.052)
LBO × Fin. Dep.	.026	.17	−.4***	−.32***	−.0098	−.028	−.31
	(.034)	(.14)	(.067)	(.089)	(.081)	(.024)	(.23)
Fin. Dep.	−.1***	−.77***	−.44***	−.68***	−.091**	−.0098	−1.5***
	(.018)	(.077)	(.036)	(.046)	(.042)	(.013)	(.11)
post	−.059***	−.2***	−.069***	−.09***	−.022	.0049	−.14***
	(.0076)	(.028)	(.014)	(.019)	(.018)	(.0055)	(.047)
Observations	17,501	14,957	17,438	17,542	17,338	17,139	17,373
Number of deals	438	421	438	438	438	437	438
Adj. R^2	.53	.85	.93	.91	.93	.61	.71
Panel B: Divisional LBOs							
post × LBO	.0083	.092	−.0015	−.015	−.19**	−.056**	.11
x Fin. Dep.	(.027)	(.13)	(.061)	(.078)	(.086)	(.024)	(.19)
post × LBO	.033**	.11	.052	.03	.039	.031**	.058
	(.016)	(.071)	(.037)	(.046)	(.052)	(.014)	(.12)
post × Fin. Dep.	.0081	.041	−.083***	−.068**	.088**	.011	.11
	(.012)	(.057)	(.027)	(.034)	(.038)	(.01)	(.086)
LBO × Fin. Dep.	.1**	.26	−.13	.45***	.34***	.07*	−.36
	(.043)	(.25)	(.1)	(.13)	(.14)	(.038)	(.31)
Fin. Dep.	−.028	−.47***	−.5***	−.41***	−.46***	−.0043	−1.1***
	(.021)	(.11)	(.057)	(.065)	(.068)	(.019)	(.16)
post	.0011	−.031	−.036	−.012	−.071**	−.01	−.15**
	(.01)	(.043)	(.023)	(.028)	(.032)	(.0088)	(.072)
Observations	8,540	6,833	8,511	8,554	8,481	8,366	8,533
Number of deals	229	216	229	228	229	229	229
Adj. R^2	.5	.88	.93	.92	.92	.6	.73
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Financial dependence is computed over firms with employees > 50							
Panel C: Secondary LBOs							
post × LBO	−.087***	−.43**	−.12**	−.16*	−.16*	−.056**	.15
× Fin. Dep.	(.035)	(.17)	(.06)	(.093)	(.09)	(.028)	(.24)

Table A4 (*continued*)

	ROA (1)	log(EBITDA) (2)	log(Empl) (3)	log(Sales) (4)	log(FA+WC) (5)	Leverage (6)	log(CAPEX) (7)
post × LBO	.078***	.32***	.12***	.21***	.13**	.035**	.25*
	(.021)	(.084)	(.036)	(.058)	(.055)	(.017)	(.15)
post × Fin. Dep.	.071***	.12**	.086***	.12***	.094***	−.02*	.85***
	(.014)	(.061)	(.023)	(.037)	(.035)	(.011)	(.096)
LBO × Fin. Dep.	.23***	.16	−2.1***	−1.3***	−.21	−.065	−1.1**
	(.071)	(.53)	(.13)	(.19)	(.18)	(.057)	(.49)
Fin. Dep.	−.085***	−.41***	−.031	−.33***	−.38***	.056***	−1.5***
	(.025)	(.13)	(.048)	(.072)	(.064)	(.02)	(.18)
post	−.071***	−.18***	−.11***	−.19***	−.09***	.0098	−.49***
	(.013)	(.052)	(.023)	(.036)	(.035)	(.011)	(.095)
Observations	4,918	4,087	4,892	4,910	4,887	4,847	4,913
Number of deals	129	120	129	127	129	129	129
Adj. R^2	.57	.87	.96	.92	.93	.64	.73
Panel D: Public-to-private LBOs							
post × LBO	−.016	.077	.096	−.12	−.03	.0085	.5
x Fin. Dep.	(.033)	(.27)	(.11)	(.14)	(.093)	(.035)	(.37)
post × LBO	.063**	.04	−.063	−.075	−.044	.049	−.64*
	(.03)	(.19)	(.1)	(.13)	(.085)	(.032)	(.34)
post × Fin. Dep.	.02	.14	−.095**	−.035	.092**	−.00037	.38**
	(.014)	(.11)	(.047)	(.06)	(.039)	(.015)	(.16)
LBO × Fin. Dep.	−.03	.23	−.064	.0047	.54**	−.094	−.53
	(.085)	(.54)	(.29)	(.37)	(.24)	(.09)	(.96)
Fin. Dep.	−.047*	−1.4***	−.62***	−.9***	−.074	−.046*	−.49*
	(.027)	(.21)	(.089)	(.11)	(.079)	(.028)	(.3)
post	−.032*	−.11	−.054	−.11	−.16***	−.016	−.59***
	(.018)	(.12)	(.063)	(.079)	(.052)	(.02)	(.21)
Observations	1,438	1,076	1,430	1,434	1,428	1,424	1,430
Number of deals	36	32	36	36	36	36	36
Adj. R^2	.58	.91	.94	.94	.97	.65	.76
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Financial dependence is computed over firms with employees > 200							
Panel A: Private-to-private LBOs							
post × LBO	−.026	.012	.13***	.12***	.22***	.022	.41***
× Fin. Dep.	(.019)	(.076)	(.036)	(.048)	(.046)	(.014)	(.12)
post × LBO	.061***	.2***	.11***	.12***	.03	.029***	.15**
	(.013)	(.046)	(.023)	(.031)	(.03)	(.0091)	(.077)
post × Fin. Dep.	.055***	.1***	.029**	.0082	−.02	−.0093	.072
	(.0079)	(.031)	(.015)	(.02)	(.019)	(.0058)	(.049)
LBO × Fin. Dep.	.01	.46***	−.31***	−.34***	.16*	−.06**	−.26
	(.035)	(.15)	(.071)	(.094)	(.084)	(.025)	(.23)
Fin. Dep.	−.1***	−.81***	−.4***	−.67***	−.09**	−.017	−1.5***
	(.018)	(.078)	(.038)	(.048)	(.044)	(.013)	(.12)
post	−.06***	−.21***	−.07***	−.092***	−.023	.0054	−.14***
	(.0076)	(.028)	(.014)	(.019)	(.018)	(.0055)	(.047)
Observations	17,501	14,957	17,438	17,542	17,338	17,139	17,373
Number of deals	438	421	438	438	438	437	438
Adj. R^2	.53	.85	.93	.91	.93	.61	.71
Panel B: Divisional LBOs							
post × LBO	.02	.056	−.022	.06	−.18**	−.047**	.17
× Fin. Dep.	(.026)	(.12)	(.057)	(.072)	(.08)	(.022)	(.18)
post × LBO	.029*	.13*	.053	−.00065	.028	.027**	.016
	(.016)	(.067)	(.035)	(.043)	(.049)	(.014)	(.11)
post × Fin. Dep.	.016	.0075	−.058**	−.045	.094***	.012	.13
	(.011)	(.052)	(.026)	(.032)	(.035)	(.0097)	(.08)
LBO × Fin. Dep.	.035	.17	−.2*	.14	.25*	.079**	−.45
	(.042)	(.24)	(.1)	(.13)	(.13)	(.038)	(.31)
Fin. Dep.	−.038*	−.4***	−.43***	−.35***	−.45***	−.045**	−1***
	(.021)	(.1)	(.057)	(.064)	(.066)	(.018)	(.15)
post	−.0019	−.017	−.045**	−.022	−.07**	−.01	−.15**
	(.0098)	(.041)	(.022)	(.027)	(.031)	(.0085)	(.07)
Observations	8,540	6,833	8,511	8,554	8,481	8,366	8,533
Number of deals	229	216	229	228	229	229	229
Adj. R^2	.5	.88	.93	.92	.92	.6	.73
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A4 (*continued*)

	ROA (1)	log(EBITDA) (2)	log(Empl) (3)	log(Sales) (4)	log(FA+WC) (5)	Leverage (6)	log(CAPEX) (7)
Financial dependence is computed over firms with employees > 200							
Panel C: Secondary LBOs							
post × LBO	−.068**	−.3**	−.045	−.066	−.079	−.034	.12
× Fin. Dep.	(.033)	(.16)	(.056)	(.087)	(.084)	(.026)	(.23)
post × LBO	.067***	.28***	.086**	.16***	.096*	.027	.24*
	(.021)	(.079)	(.035)	(.056)	(.054)	(.017)	(.15)
post × Fin. Dep.	.068***	.1*	.071***	.078**	.07**	−.018*	.76***
	(.013)	(.059)	(.022)	(.035)	(.034)	(.011)	(.093)
LBO × Fin. Dep.	.14*	.37	−2***	−1.5***	−.22	−.046	−1.2**
	(.073)	(.51)	(.13)	(.19)	(.19)	(.058)	(.5)
Fin. Dep.	−.068***	−.23**	−.12**	−.32***	−.39***	.058***	−1.6***
	(.026)	(.12)	(.05)	(.074)	(.066)	(.021)	(.19)
post	−.068***	−.17***	−.1***	−.16***	−.076**	.0088	−.44***
	(.013)	(.051)	(.023)	(.036)	(.034)	(.011)	(.094)
Observations	4,918	4,087	4,892	4,910	4,887	4,847	4,913
Number of deals	129	120	129	127	129	129	129
Adj. R^2	.57	.87	.96	.93	.93	.64	.73
Panel D: Public-to-private LBOs							
post × LBO	.00075	.17	.14	−.046	−.046	.013	.49
× Fin. Dep.	(.033)	(.26)	(.11)	(.14)	(.095)	(.036)	(.38)
post × LBO	.055*	.0024	−.096	−.13	−.027	.045	−.62*
	(.03)	(.19)	(.1)	(.13)	(.085)	(.032)	(.34)
post × Fin. Dep.	.017	.14	−.096**	−.029	.078**	.0053	.37**
	(.014)	(.1)	(.047)	(.06)	(.04)	(.015)	(.16)
LBO × Fin. Dep.	−.17**	.18	.21	.29	.25	.012	−.43
	(.071)	(.51)	(.24)	(.31)	(.2)	(.077)	(.81)
Fin. Dep.	−.056*	−1.5***	−.76***	−1***	−.055	−.056*	−.56*
	(.029)	(.23)	(.096)	(.12)	(.084)	(.031)	(.32)
post	−.029	−.11	−.055	−.12	−.15***	−.02	−.58***
	(.018)	(.12)	(.062)	(.079)	(.052)	(.02)	(.21)
Observations	1,438	1,076	1,430	1,434	1,428	1,424	1,430
Number of deals	36	32	36	36	36	36	36
Adj. R^2	.58	.91	.94	.94	.97	.65	.76
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* Means statistically significant at the 10% level.
** Means statistically significant at the 5% level.
*** Means statistically significant at the 1% level.

use two alternative measures of industry financial dependence (see Section 2.4 for a definition of these measures). As is apparent from this table, the results are left unchanged with these alternative measures. Overall, all the results presented in Table 8 and Table A4 are very consistent with the idea that (1) divisions of conglomerates, public firms, and firms owned by a private equity group are less likely to suffer from credit constraints, and (2) the relaxation of credit constraints is at the heart of the post-buyout growth and increase in capital expenditures and post-buyout debt issue observed in our sample of LBOs.

5. Conclusion

Like most commentators in the public debate, many financial economists have come to see LBOs as a way to implement drastic, "cost-cutting" measures that the target was initially reluctant to put in place. This view is largely influenced by studies on large, US-based, public-to-private deals in the 1980s, or more recent studies that focus exclusively on US and UK data. This paper provides some evidence that LBOs may alleviate credit constraints, and be an actual engine of growth for small- and medium-sized enterprises. In France, LBO targets experience a very strong growth in sales, assets, and employment, in particular when they were previously more likely to be credit-constrained. Hence, instead of reinforcing credit constraints, modern LBOs can make them less tight. This effect is large in France, but existing studies have not yet demonstrated that this growth motivation is absent from smaller the US or the UK transactions.

Appendix A

See Appendix Tables A1–A4.

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