Agency Problems in Public Firms: Evidence from Corporate Jets in Leveraged Buyouts

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ABSTRACT

This paper uses novel data to examine the fleets of corporate jets operated by both publicly traded and privately held firms. In the cross-section, firms owned by private equity funds average 40% smaller fleets than observably similar public firms. Similar fleet reductions are observed within firms that undergo leveraged buyouts. Quantile regressions indicate that these results are driven by firms in the upper 30% of the conditional jet distribution. The results thus suggest that executives in a substantial minority of public firms enjoy excessive perquisite and compensation packages.

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Managers of a firm might sometimes take actions to benefit themselves at the expense of the firm's investors. Both firms and governments have put in place a variety of mechanisms to mitigate this agency problem, and a great deal of research in corporate finance has been devoted to their study. Nonetheless, there remains considerable debate as to whether further action should be taken to protect outside investors from self-interested managers.

Debate over the desirability of reforming executive compensation arrangements is especially active. Some argue that executives exert too much control over their own compensation and often choose to pay themselves excessively (Yermack (1997), Bebchuk and Fried (2006), Morse, Nanda, and Seru (2011)), while others argue that observed compensation arrangements could represent optimal contracts negotiated at arm's length between firms and valuable executive talent (Himmelberg and Hubbard (2000), Edmans, Gabaix, and Landier (2009), Gayle and Miller (2009), Kaplan and Rauh (2010)). Given this mixed evidence, it is not surprising that controversy has surrounded recent attempts by policymakers to mandate more shareholder influence over compensation and other corporate affairs through initiatives like "say-on-pay" and proxy access.

This paper brings new evidence to these debates by measuring a particular kind of firm behavior where there is potential for managerial abuse—the use of corporate jets. Executives in many firms travel on jets or other aircraft that are owned or leased by their employers. Much of this activity is likely perfectly consistent with the maximization of shareholder value. For example, private jets might save many hours of executives' valuable time, and they might serve as an efficient form of compensation. It is also possible, however, that executives could overconsume corporate jets if shareholders are unable to monitor or incentivize them properly. Jet use itself can be costly—annual operating costs can be as high as $5 million per jet, with $1 million being quite typical—but these amounts are not especially large when compared to revenues or profits earned by the firms studied in this paper. The most important reason to study jet fleets is that any observable waste could represent the tip of a larger iceberg of excessive compensation and other agency costs.

I attempt to distinguish between excessive and efficient use of corporate jets by comparing jet fleets in publicly traded firms with those in firms owned and controlled by private equity (PE) funds. PE funds typically buy controlling shares in mature firms through leveraged buyouts (LBOs), impose changes in firm operations or incentives, and eventually sell their shares to other firms or to public shareholders after some period, often several years or more.[1] Three components have been identified as key to PE funds' approach to managing firms: highly levered financing, highly performance-sensitive managerial compensation, and active monitoring of firm activities by skilled professionals from the PE fund. These changes are intended to transform firms into better-managed, more efficient organizations.

A great deal of evidence suggests that PE ownership succeeds in improving efficiency in portfolio companies. Indeed, evidence for improvements in firm performance following LBOs has been found using a variety of dependent variables, in a variety of countries, in each of the last three decades. In a seminal paper, Kaplan (1989) finds large increases in operating performance and market value following 76 U.S. LBOs between 1980 and 1986. Lichtenberg and Siegel (1990) find substantial increases in total factor productivity (TFP) in a sample of manufacturing plants owned by firms that went through LBOs between 1983 and 1986. Harris, Siegel, and Wright (2005) find similar results on TFP in a sample of U.K. buyouts between 1994 and 1998. Bergstrom, Grubb, and Johnsson (2007) find a significant positive impact on operating performance in a sample of Swedish buyouts, and Boucly, Sraer, and Thesmar (2009) find similar results for French buyouts. Sheen (2009) finds that private U.S. chemical producers made better-timed investments in new capacity than did publicly traded producers. Lerner, Sorensen, and Stromberg (2011) even find that firms register more economically important patents after LBOs. In a sample of recent U.S. buyouts, Guo,Hotchkiss, and Song (2011) find smaller improvements in operating performance than seen in earlier decades, but still conclude that these improvements drive a meaningful component of realized returns to PE funds.

In surveying the literature, Acharya, Hahn, and Kehoe (2009) find that it "suggests that buyouts do create value through operating improvements, in both U.S. and U.K. markets, during both the recent and the 1980s buyout booms." Similarly, Kaplan and Stromberg (2009) find the literature "largely consistent with a view that private equity portfolio companies create economic value by operating more efficiently." I thus interpret jet fleets observed in PE portfolio companies as a benchmark of efficiency against which to compare the fleets of public firms. That is, if public firms have larger fleets than PE-owned firms, I interpret this as evidence of excessive jet use in public firms.

Of course, it is possible that some readers will be unconvinced by this literature documenting performance improvements after LBOs or otherwise reluctant to entertain the notion that one could learn about efficient jet use by studying fleets in PE-owned firms. These readers might prefer to interpret the paper's results in other ways. For example, one could view differences in jet fleets between publicy traded and PE-owned firms as an intriguing fact about behavioral differences across forms of ownership that is still in need of explanation. Or, one could view the results as concrete evidence of LBO-induced cost-cutting, the existence of which has long been assumed but rarely documented.

In any case, it is not often possible to observe behavior in privately held firms, simply because they are not required to produce publicly available information. This shortage of data provides another key motivation for this paper's focus on corporate jets. There are private data vendors who collect information on all business jets—whether their owners are publicly or privately held—primarily for use by other firms in marketing goods and services to jet users. These data provide an unusual opportunity to compare behavior in public and private firms.

Results from the 2008 cross-section indicate that firms owned by private equity funds average about 40% smaller jet fleets than observably similar public firms. In a sample of

LBOs occurring between 1992 and 2007, similar fleet reductions occur within firms after their buyouts. As the selection of firms into PE ownership is not random, I discuss assumptions under which these comparisons across and within firms provide estimates of lower and upper bounds on the average treatment effect of taking a firm from public to private in an LBO. Finally, both traditional and censored quantile regressions indicate that these results at the mean are driven by firms in the upper 30% of the conditional jet distribution. That is, even after controlling for observable characteristics like firm size, there is little difference between public and PE-owned firms at the 60th percentile of the jet distribution or below. Differences emerge, however, at the 70th percentile and above. Overall, results thus suggest that the kind of agency problems that manifest themselves in large jet fleets are far from ubiquitous in public firms. At the same time, however, they are not limited to a very small number of bad apples.

Section I of the paper presents a simple model of selection into PE ownership. Section II describes the data on firms and jet fleets. Section III presents results, and Section IV concludes. An Internet Appendix available on the Journal of Finance website at http://www.afajof.org/supplements.asp also provides a variety of alternative specifications, robustness tests, and other supporting material.

I. Model of Selection into Private Equity Ownership

The selection of firms into PE ownership is unlikely to be random, and I have no source of exogenous variation in ownership to exploit in this paper. Here, I present a simple model to clarify the equilibrium relationship between jets and ownership that we would expect when these can both be determined endogenously. I use this simple reasoning about which firms are likely to become PE-owned to argue that cross-section and panel data can be used to estimate useful bounds on the average causal effect of PE ownership.

Denote firm i's profits by Π_i. The manager can divert a fraction λ_i^{PU} to himself in the form of excessive corporate jet use if the firm is public, and λ_i^{PE} if the firm is PE-owned. There is a cost c of being a private firm.[2] Firm i can maximize its value by being public if $(1-\lambda_i^{PU})\Pi_i > (1-c-\lambda_i^{PE})\Pi_i$, or if $\lambda_i^{PU} - \lambda_i^{PE} < c$. That is, a firm will optimally be public if the cost $(\lambda_i^{PU} - \lambda_i^{PE})$ of managerial diversion of profits is lower than the cost c of being private.

In assessing the need for costly public policies or shareholder actions that could lower λ^{PU} for all firms, we are arguably most interested in estimating the quantity $E_i[\lambda_i^{PU} - \lambda_i^{PE}]$, that is, the average difference across all firms in the fraction of profits inappropriately diverted to jet use when the firm is public instead of PE-owned. Note that consideration of this quantity requires that we imagine counterfactual quantities such as the fraction of profits that would be diverted if a firm were public when the firm is actually PE-owned. This is a hallmark of the heterogeneous treatment effects framework developed by Rubin (1974) and extended by Angrist, Imbens, and Rubin (1996).

If we were to estimate the difference in the averages of the fraction of profits diverted in public and private firms in cross-sectional data, we would instead be measuring $E_i[\lambda_i^{PU}|(\lambda_i^{PU} - \lambda_i^{PE}) < c] - E_i[\lambda_i^{PE}|(\lambda_i^{PU} - \lambda_i^{PE}) > c$. That is, we would observe profits diverted in public firms that optimally choose to be public relative to profits diverted in firms that optimally choose to be PE-owned. Suppose, however, that $\lambda_i^{PE} = 0$ for all i, that is, any jet use observed in PE-owned firms does not represent managerial waste but is instead consistent with maximization of the value of the firm's profits. Further, there is no firm in which PE ownership could not reduce jet use to this efficient level. Note that to argue that this statement is false would be to argue that there are some public firms with self-dealing managers who are so entrenched that even PE ownership could not change their behavior. That there are some public firms with substantial agency problems is, of course, the conclusion of this paper. In this case, we have

$$E_i[\lambda_i^{PU} | (\lambda_i^{PU} - \lambda_i^{PE}) < c] - E_i[\lambda_i^{PE} | (\lambda_i^{PU} - \lambda_i^{PE}) > c] \leq E_i[\lambda_i^{PU} - \lambda_i^{PE}].$$

Thus, the difference between public and private firms observable in the cross-section would give us a *lower* bound on the average effect across all firms of taking them from public to private.

We might also be able to observe firms that optimally change from public to private, in which case we could observe the average λ_i^{PU} among some set of firms for which it is optimal to be PE-owned. In this simple static setting, I leave unmodeled the dynamic process through which such firms might come to find themselves in a suboptimal state. Because $E_i[\lambda_i^{PU} | \lambda_i^{PU} > c] > E_i[\lambda_i^{PU}]$, the change observed in firms that switch from public to private should give us an *upper* bound on the average across all firms in the potential change when taking them from public to private. Thus, the cross-sectional difference between public and PE-owned firms and the within-firm changes among firms that switch from public to private should give us lower and upper bounds on the average treatment effect.

The intuition is simple. We might expect firms to go private when they have the most to gain from doing so. When gains from going private include cost savings from reductions in jet use, then PE-owned firms—if they were instead public—would be the firms with the largest jet fleets. Thus, we might expect to *underestimate* the average effect of being PE-owned when looking in the cross-section, and to *overestimate* this effect when looking at changes in the panel of leveraged buyouts. There is some evidence for this selection into PE-ownerhsip, but estimates from the cross-section and the panel of LBOs will be fairly similar, suggesting that treatment effects do not differ very much between PE targets and other firms.

II. Data

For basic information on private firms, I use Forbes magazine's annual lists of America's largest private companies, which provide data on sales and employees in the largest private U.S. firms. These lists have been published every year since 1985, and in recent years have included all firms with sales greater than $1 billion. For public firms, I use Compustat data on all publicly traded U.S. firms. Cross-sectional results below use a sample of firm data from fiscal year 2008.[3] After selecting firms according to the steps detailed in the Internet Appendix, the resulting sample with nonmissing sales and employees observations consists of 444 private firms, of which 101 are PE-owned, and 1,242 public firms, for a total of 1,686 firms.

To analyze changes in jet fleets within firms that go from public to private in an LBO, I construct a sample of buyouts using the Compustat and Forbes data, along with a sample of LBO transactions from the Dealscan database. After carefully researching many individual transactions, I end up with 68 firms that appeared in Compustat as stand-alone public firms with at least $1 billion in sales (in 2008 dollars), that underwent a PE-led buyout, and that then appeared in a Forbes list as a private firm. Petco went through two LBOs during this period, so there are a total of 69 buyout events in the sample.

To both the cross-sectional and buyout samples of firms, I add data on jet fleets from a private vendor, JETNET LLC.[4] JETNET employs a team of researchers to track the ownership, financing, and operatorship of all business airplanes in the U.S., with additional coverage worldwide. The data are sold on a subscription basis primarily to other firms interested in marketing goods or services to jet owners and operators. Gavazza (2010) uses similar data from a different vendor to study leasing decisions by commercial airlines, but I am not aware of any studies that have used data like these to study business jets.[5]

JETNET data on jet transactions begin in 1989, which unfortunately precludes the study of the many buyouts occuring in the 1980s. A close reading of "Barbarians at the Gate," however, allows one to loosely piece together a history of the "RJR Air Force," the fleet of jets operated by RJR Nabisco, before and after its LBO. According to the book, RJR's eighth jet arrived two months after the buyout had been negotiated, due to a delivery lag. But within a few months of appointing a new CEO, seven out of eight jets had been sold (Burrough and Helyar (1990), p. 511).

The JETNET data include information on jets that are owned outright, jets that are on capital or operating leases, and jets owned through fractional ownership programs like NetJets.[6] The data include all jets marketed as business aircraft (like Gulfstreams and Learjets), as well as "executive airliners," or commercial aircraft fitted for business use. Although the data include some turbo-prop and piston engine planes, I refer to all aircraft as "jets" throughout the paper.

The data include specification information for most jets and estimated operating cost information for some. As the passenger seat capacity field is most frequently populated, I focus on total seat capacity as a measure of firm-level fleet size. Firms owning a fractional share are counted as a jet operator in results below, and are counted as owning the appropriate percentage of the jet's seats. For example, a firm owning one-eighth of a jet that seats eight is counted as having one seat. There is little difference between public and PE-owned firms in their propensity to own fractional shares, conditional on having any jet.

One might be concerned that jets operated by PE portfolio companies could appear in the data under their PE parents. I looked up the jet fleets of several major PE funds like KKR, Blackstone, Bain Capital, and Carlyle, and at most they operate two or three jets each. As these large funds may have dozens of portfolio companies around the world, any access the companies have to the PE funds' jets must be limited.

One might still be concerned that LBOs could cause some other change in the status of jets recorded in the data, even if the jets really remained with their original firms. For example, one could worry that PE funds are likely to create opaquely named subsidiaries to operate the jets of their portfolio companies, perhaps for tax or liability reasons. To explore this possibility, I carefully hand-investigated all firms that reduced their jet fleets within two years of an LBO. None of the fleet reductions look suspicious. Many involved sales of fractional jets back to a fractional jet company (e.g. NetJets), with no subsequent sales of fractions of the same plane to PE firms or potential subsidiaries. Jets from other firms were sold or leased to a collection of charter firms, jet dealers, and other firms. There were no sales to opaque offshore entities, sales to entities with addresses similar to the former operator's, or other transactions that might raise suspicions. I thus see no reason to suspect that fleet reductions observed after LBOs result from any unusual feature of the data related to PE ownership.

[FIGURE 1 ABOUT HERE.]

To provide a high-level description of the JETNET data, Figure 1 plots the total number of jets that have ever appeared in the data by the year in which they were first delivered.[7] There is a notable surge in the number of jets and total seats delivered in the late 1970s, followed by collapse in the early 1980s and a persistent lull through the mid-1990s. This pattern is consistent with the narrative histories of the general aviation aircraft industry provided by Pattillo (1998) and Bednarek and Bednarek (2003). These authors blame the 1980s collapse on liability lawsuits waged against manufacturers, less favorable tax treatment for business aircraft, airline deregulation that facilitated more commercial flights to remote locations, and overbuilding during the 1970s. Jet deliveries then grew robustly during the boom years of the late 1990s, dropped back in the years following the 2001 recession, and rose rapidly through the end of the sample. As the lag between order and delivery of new aircraft can be two years or more, it is likely that the surge in deliveries in the mid-2000s was driven in part by more onerous security requirements for commercial travel implemented after the terrorist attacks of September 11, 2001.

There have also been some notable changes in the size of new jets over time. Although the 10th percentile and median jet have changed little in size over time, the largest jets have gotten larger: the 90th percentile jet among deliveries in the 2000s had 14 seats, compared to 12 in the 1990s, nine in the 1980s, and eight in the 1970s.

Table I presents descriptive statistics for both the cross-sectional and LBO samples of firms. The paper's results are all foreshadowed in this table. In both the cross-section and the LBO panel, PE-owned firms are less likely to operate a jet. Conditional on having a jet, they average a smaller fleet. Overall, they average a lower ratio of jet seats to firm sales. The median number of jets in all samples is zero, however, so differences at the mean are driven by the upper parts of the jet fleet distribution. Note that there is not much difference between public and PE-owned firms in the number of commercial flights departing from airports near their headquarters. It is true, however, that public firms are much larger on average than PE-owned firms in the cross-section. The results that follow control for this size difference in a variety of flexible ways. Note that in the panel of LBOs, there is actually relatively little change in firm size from one year before to one year after LBO.

[TABLE 1 ABOUT HERE]

Some readers might also like to compare the pre-buyout characteristics of the LBO firms with other public firms that did not go private.[8] In the Internet Appendix, I compare this paper's sample of LBO firms in the two years prior to their LBOs to other public firms, controlling for size, industry, and year. Most notably, and consistent with the existing literature, the firms that became LBO targets had significantly lower market valuations as measured by average Q. There is also some evidence that the LBO firms underperformed when measured by their ratio of sales to assets, but little evidence of much difference between the LBO firms and others in return on assets. Interestingly, the total salary paid to the top five executives named in firm proxy statements is at least 5% higher in firms that subsequently did an LBO, after controlling for size, industry, and performance. However, results using the CEO's salary alone are less robust. To summarize, results are somewhat mixed, but overall provide support for the notion that buyout targets underperformed their peers and paid their executives generously in the years before the buyout.

III. Results

A. Public and Private Firms in the Cross-Section

[FIGURE 2 ABOUT HERE.]

Figure 2 illustrates the regression results presented in this section. The top panel of the figure shows the fraction of firms in the 2008 cross-section that operate any jet, broken out by ownership (publicly traded, PE-owned, or private and non-PE-owned) and size, as measured with sales.[9] In all size quintiles except the middle one, PE-owned firms are less likely to have any jet than either publicly traded or private, non-PE-owned firms. As there are only 101 PE-owned firms in the sample, the black bars in the figure each represent an average of about 20 firms, and there are at least 14 PE-owned firms in each quintile. Thus, the size of any individual bar is sensitive to the jets in only a handful of firms. But the tendency across quintiles of PE firms to have fewer jets than public firms is quite clear. One can also see that differences between public and PE-owned firms are not driven only by firm size. For example, the PE-owned firms in quintile 4 are less likely to have a jet than the public firms in quintile 3, even though these PE-owned firms are larger.

A.1. Results on the Presence of Any Jet

Regression results in Table II quantify this relationship. In column 1, with no controls, the estimated constant indicates that 40% of publicly traded firms operate at least one jet. The coefficient on a dummy for PE ownership indicates that PE-owned firms are 12.3 percentage points less likely than public firms to have any jets. That is, 28 of the 101 PE-owned firms in the sample operate at least one jet. Private, non-PE firms are 4.2 percentage points less likely to have any jets than public firms, but this effect is not statistically significant at conventional levels.

[TABLE II ABOUT HERE.]

Firms may fundamentally differ in their need for air travel depending on factors such as their size, their location, and the nature of their business. If PE ownership is correlated with any of these characteristics, the coefficient in column 1 may provide a biased estimate of the causal effect of PE ownership on the presence of a jet. Column 2 adds controls for firm size, location, and industry in the form of logarithms of sales and employees, dummies for headquarters state, and dummies for two-digit NAICS industries. It also includes the logarithm of the number of scheduled flights leaving from airports within 50 miles of headquarters, the key location variable used by Rajan and Wulf (2006). To address the potential concern that the logarithmic functional form may not correctly capture relevant variation in size and location, columns 3 and 4 allow for more flexibility. Column 3 adds quadratic polynomials in sales, employees, and scheduled flights, and column 4 includes 30 dummy variables for firms in each of the 10 deciles of sales, employees, and scheduled flights.

In all four columns, the coefficient on the dummy for PE ownership indicates that PE-owned firms are 10 to 12 percentage points less likely to operate jets, even after controlling for size, location, and industry. Relative to the 40% of public firms that have any jet, PE-owned firms are thus at least 25% less likely to have a jet. Columns 5 and 6 repeat the specifications in columns 3 and 4 using dummies for three-digit NAICS industries in place of two-digit industries, and the estimated effect of PE ownership rises a bit. With standard errors clustered by headquarters state, the estimated effects of PE ownership in columns 2 through 6 are statistically different from zero at the 5% level.

A.2. Results on Fleet Size

The second panel of Figure 2 shows the mean number of jet seats in fleets among firms with at least one jet, again broken out by ownership and size quintile. Conditional on having any jets, we see that PE-owned firms have smaller fleets than publicly traded or private non-PE firms within all five size quintiles. The bottom panel of Figure 2 summarizes the data in the top two panels in a single statistic—the mean across firms of the ratio of the number of jet seats in a firm's fleet to its sales in billions of dollars. For both public and PE-owned firms, there is no obvious relationship between sales and the seats-to-sales ratio, suggesting that taking this simple ratio provides a useful way of controlling for size. It is quite clear that PE-owned firms have smaller fleets by this measure than do public or private, non-PE firms. In fact, small, private, non-PE firms stand out as having the largest fleets given their size.

Table III presents regression results that quantify this relationship. In column 1, which contains no controls, we see from the estimated constant that publicly traded firms average 1.586 jet seats per billion dollars of sales. Private non-PE firms have much higher ratios, and PE-owned firms considerably lower.[10] Anyone accustomed to working with firm-level data should be aware that regression results using variables in the form of ratios can often be affected by a small number of extreme values. Indeed, inspection of the data reveals that results in column 1 are somewhat skewed by a relatively small number of observations with very large seats-to-sales ratios. Columns 2 through 6 use data where the seats-to-sales ratio is winsorized at the 95th percentile of its distribution.[11]

[TABLE III ABOUT HERE.]

With this winsorization, column 2 shows that public firms average 1.199 jet seats per billion dollars of sales, while PE-owned firms average 0.485 fewer seats, a difference of 40%. Columns 3 through 6 contain various controls for location, industry, and size, and the estimated magnitude of the effect of PE ownership tends to rise in these specifications. The effect of PE ownership is statistically different from zero at the 10% level in all columns 2 through 6 and at the 1% level in columns 3 and 5.[12]

Note that Table III also shows that private, non-PE-owned firms average larger fleets than publicly traded firms. This result is helpful in ruling out a class of alternative explanations for the results on PE-owned firms. One might suggest various aspects of publicly traded status that could require additional jet use. For example, perhaps executives in public firms must spend more time traveling to meet with outside shareholders who are spread around the country. Or perhaps regulations require public companies to have large boards of directors that meet frequently. Stories like these could produce differences in jet fleets between public and PE-owned firms, even in the absence of agency problems. They would also predict, however, that private, non-PE-owned firms would have smaller, PE-like fleets. Instead, private, non-PE-owned firms have fleets at least as large as public firms. This observation itself might lead some to question my interpretation of results as evidence of agency problems, and I discuss this issue at length in section H of the Internet Appendix.

B. Leveraged Buyouts

Results thus far have shown that PE-owned firms in the 2008 cross-section have smaller jet fleets than do publicly traded or private, non-PE firms. These results are quite insensitive to the choice of dependent variable (dummy for any jet, seats-to-sales ratio, or logarithm of seats), to the model used for estimation (OLS, probit, or Tobit), or to the inclusion of a variety of flexible controls for size, industry, and location.

One could still be concerned, however, that these cross-sectional differences do not represent a causal effect of PE ownership. For example, it is possible that PE funds tend to purchase firms that have more access to commercial air services in a way that is not captured by the state dummies and flexible controls for scheduled flights that I have included in the regressions thus far. The limited amount of information available on private firms also prevents one from controlling for a variety of other firm-level variables that some might wish to include.

This section addresses these concerns by measuring changes in jet fleets within firms that are taken from public to private by a PE fund in a leveraged buyout. These results thus hold constant fundamental firm characteristics like their original location, making results unlikely to be driven by simple omitted variable bias. The model in Section II of the paper also suggested that we might interpret results from these LBOs as a bound on the average treatment effect of PE ownership. I will return to this interpretation at the end of this section.

It should be noted that results in this section come from a relatively small sample of firms. Many of the 101 PE-owned firms in the cross-section came from spinoffs of divisions of public firms or from firms that were already private. Of the 69 large, standalone, public-to-private LBOs in the sample I constructed, 32 had at least one jet in the year before their LBO.[13] Of these 32 firms, 20 reduced the number of seats in their fleet within two years of the buyout, five increased their fleets, and seven made no changes. Thus, the number of firms that reduced their fleets post-LBO is four times the number that increased their fleets. Of course, standard errors are meant to help assess whether this pattern of changes is likely to have arisen by chance, but it is nonetheless important to keep in mind that estimates are from a small sample.

[FIGURE 3 ABOUT HERE.]

B.1. Results on the Presence of Any Jet

The top panel of Figure 3 displays the fraction of the firms in the LBO sample with any jet in the years surrounding their LBO events. This fraction rises from around 40% several years before LBO to a peak of 46% in the year immediately preceding LBO. It then falls immediately and dramatically in the year of and years after LBO. Note that this figure simply graphs raw means from the unbalanced panel of firms that underwent LBOs. The composition of firms in the sample changes from point to point in the figure, and no correction is made for any changes in firm size following the LBO.

[TABLE IV ABOUT HERE.]

Table IV presents regressions of a dummy indicating whether a firm had any jets in a given year on dummies for the years surrounding the firm's LBO (with the year before the LBO omitted) and relevant control variables. All columns include firm fixed effects to control for changes in sample composition that might complicate interpretation of the raw data in Figure 3. Column 2 adds year effects.[14] Columns 3 and 5 instead include the "kitchen sink" of size controls in the form of logarithms and quadratic polynomials in sales and employment. Columns 4 and 6 include both these size controls and the year effects. Columns 3 and 4 exclude all observations from 2008 and 2009 when the financial crisis and recession could affect results, while columns 5 and 6 include these observations.

The estimated constant in column 1 reminds us that 46% (or 32 out of 69) of the firms in the LBO sample had at least one jet in the year prior to LBO. From this base of 46%, the estimated reduction in the probability of having a jet by the end of the calendar year after the LBO ranges from 9.6 percentage points in column 5 to 16.2 in column 2. Four years after the LBO, the estimated reduction ranges from 15.5 percentage points in column 5 to 28.8 in column 2. Note that standard errors rise considerably in years further beyond the LBO as the number of observations falls due to the absence of observations for firms that went private in 2006 and 2007. Effects in the year after LBO (for which the full sample of 69 LBOs is still observed) are statistically different from zero at the 1% level in four of the six columns when clustering by headquarters state. Effects four years after LBO are statistically significant at the 5% level at best.

I take as my preferred estimate of the effects of PE ownership the 14.9 percentage point reduction in the propensity to have a jet in the year following the LBO from column 6. This figure is very close to both the mean and the median effects across all estimates from one to four years after LBO, but obviously this choice is rather arbitrary given the range of estimates in the table. On a base of 46% of firms that had a jet the year prior to LBO, this figure represents a 32% reduction in the propensity to have a jet.[15]

B.2. Characteristics of Fleet Reducers

Table V reports information on the characteristics of the LBO firms that reduced their fleets. It presents simple regressions of a dummy for whether a firm reduced its fleet within two years of the LBO on several characteristics of the pre-buyout firm or the LBO; regressions using the change in seats-to-sales ratio as the dependent variable tell a very similar story. Simple inspection of the data suggests that there is a clear relationship between the firms that reduced their fleets after the buyout and the firms that changed CEO. For example, of the 10 LBOs with the largest fleet reductions measured by change in seats-to-sales ratio, eight (or 80%) involved CEO changes. Of the remaining buyouts, however, only 46% involved a change in CEO. The result in column 1 of the table presents a different perspective on the same data. The constant indicates that 24% of firms that did not change CEO reduced their fleets, while the estimated coefficient on the CEO change dummy indicates that 34% of firms that did change CEOs reduced their fleets. Thus, firms removing their old CEO during the buyout were about 46% more likely to reduce their jet fleets, although this estimate is not statistically significant at conventional levels

[TABLE V ABOUT HERE.]

The second column relates the propensity to reduce jet fleets during the buyout to the magnitude of CEO personal aircraft use before the buyout, which was hand-collected from firm proxy statements. Twenty of the firms in the buyout sample reported a positive amount of CEO personal aircraft use in at least one of the two years before the buyout. Column

2 of the table shows that there is a clear relationship between these pre-buyout amounts of personal aircraft use and subsequent fleet reductions. The estimated coefficient indicates that a one standard deviation increase in the pre-buyout ratio of spending on CEO personal jet use to sales was associated with a 16 percentage point increase in the probability of a fleet reduction. As the fraction of firms reducing fleets in the full sample was 29%, this 16 percentage point increase represents a 55% increase in the probability of a fleet reduction. This estimate is statistically significant at the 1% level.

Column 3 relates the probability of a fleet reduction to the residuals from a regression of the log of pre-LBO CEO salary on controls for size, year, industry, and performance.[16] The coefficient indicates that a one-standard deviation increase in pre-buyout residual CEO salary was associated with an 11 percentage point increase in the probability of a post-LBO fleet reduction. This represents a 38% increase from the mean probability of a fleet reduction, and it is statistically significant at the 5% level. Combined with results from columns 1 and 2, the table thus suggests that the fleet reductions observed in LBO firms are indeed correlated with executives who enjoyed especially generous pre-buyout perquisite and compensation packages and were likely to be removed post-LBO.

In columns 4 to 6, however, there is little evidence that post-LBO fleet reductions are related to pre-LBO underperformance as measured by residual Q, ROA, or the ratio of sales to assets. It is possible, of course, that these variables constructed from accounting data are simply noisy measures of performance that can say little in a small sample like this one. For example, it is also true that there is little evidence of any relation between these performance variables and the probability of CEO turnover after the buyout. On the other hand, at face value the results in Table V would suggest that there might simply be little relation between the CEO removal, compensation, and perquisite variables in columns 1 to 3 and the broad firm performance measures in Columns 4 to 6. Indeed, this explanation seems quite plausible considering the weak evidence for any effect of compensation variables on firm performance within samples of public firms found elsewhere in the literature (see, for example, the surveys by Murphy (1999), Core, Guay, and Larcker (2003), and Frydman and Jenter (2010)).

C. Comparing Cross-Sectional and LBO Results

Returning to the comparison between the cross-sectional and LBO results, it appears that results provide modest support for the hypothesis that firms' selection into PE ownership is positively correlated with their potential reductions in jet use. That is, the effects of PE ownership appear a bit larger in the LBO panel than in the cross-section. Of the 69 public firms that underwent LBOs, 46% began with at least one jet. This figure exceeds the 40% of public firms in the 2008 cross-section that had a jet by 15%, even though the LBO targets are much smaller on average. In the cross-section, PE-owned firms were at least 25% less likely than public firms to have any jet, while my preferred estimate from the LBO panel was 32%. The range of estimates across specifications in both samples is large enough, however, that drawing any firm conclusion from the comparison of these numbers seems unwarranted.

What is quite clear, however, is the substantial average difference between jet fleets in publicly traded and PE-owned firms, both in the cross-section and in the panel of LBOs. Recalling the model of selection into PE ownership from Section II, one could then interpret the combination of cross-sectional and LBO results in two ways. If one was convinced that omitted variables produce an upward bias in the cross-section, one would regard cross-sectional results as providing an upward-biased estimate of a lower bound on the average treatment effect of PE ownership—an estimate that is not very useful. In this case, the LBO results still provide evidence that PE ownership reduces jet fleets in some firms, but relatively little could be said about the average treatment effect.

Alternatively, one might think that the cross-sectional results are unlikely to be badly biased. I favor this second interpretation, as results from the cross-section are quite stable across a wide variety of estimation approaches. For example, estimates change little and often increase in magnitude when more controls are added to a given specification, providing little reason to worry that adding related unobservable variables would have large effects. One would then note that the estimated fleet reductions associated with PE ownership from the cross-section and from the LBO panel are essentially very similar, and one would conclude that these provide a good estimate of the average treatment effect. In particular, one would conclude that the average effect of PE ownership on jet fleets is a reduction in fleet size of about 40%. That is, if firms that are currently public were randomly selected to be taken private, we should expect average fleet reductions of at least this size.

D. Leverage vs. Monitoring

Thus far, results have not addressed the question of which aspects of PE ownership are most important in changing firm behavior. This section discusses changes in jet fleets within certain samples of public firms that might shed light on this question.[17]

To investigate whether increases in leverage alone are enough to drive fleet reductions, I construct a sample of firms that remained public but went through a large increase in leverage within a single year, as firms do when they go through LBOs. I select all public firms with sales greater than $1 billion in 2008 dollars that increased their ratios of total debt to assets and long-term debt to assets by 20 percentage points or more within one year between 1992 and 2008. To isolate firms that were increasing leverage without undergoing other major changes in characteristics, I exclud firms that either increased or decreased total assets by 10% or more in the same year as the leverage increase. This selection procedure identifies 39 firms that went through a large leverage increase over this period. Many, but not all, of these firms reported that their borrowing was associated with a leveraged recapitalization or similar transaction in their SEC filings or press releases. Note that the magnitude of leverage increases in this sample is comparable to those reported by Denis and Denis (1993) in their study of leveraged recapitalizations from the 1980s. However, they are somewhat smaller than the leverage increases associated with the LBOs studied by Kaplan (1989) or Guo, Hotchkiss, and Song (2011).

The Internet Appendix presents results from this sample in the same format as those previously presented for leveraged buyouts. The propensity to operate a jet and the ratio of seats to sales do fall a bit by the end of the year of the leverage increase, but this decline is at best narrowly statistically significant and appears within the range of normal fluctuations in this small sample. Indeed, the decline is fully reversed in subsequent years. To summarize, there is little evidence of any systematic change in jet fleets following these large leverage increases in public firms.

To investigate the alternative hypothesis that changes in control and monitoring drive the fleet reductions seen during LBOs, I also measure the propensities of public firms that went through mergers to retain the jets that they operated in the year before the merger. Using data from CRSP and Compustat, I select a sample of all mergers between two standalone U.S. public firms from 1992 to present, where both the target and the acquirer had sales greater than $1 billion in 2008 dollars in the year before the merger. There were 213 mergers that met all criteria. I then identify all jets operated by either the target or the acquiring firm in the year before the merger and track whether those jets were still listed under the names of the target, the acquirer, or the new combined entity in the years following the merger.

[FIGURE 4 ABOUT HERE.]

Figure 4 presents the results of this exercise. The probability of retaining a jet operated by the acquiring firm before the merger declines in years after the merger, likely due to normal turnover in jet fleets and possible economies of scale in the number of jets required by the new entity. But, more importantly, the probability that the target's jets are retained is much lower. From one to four years after the merger, the targets' jets are about 20 percentage points less likely to be retained than the jets of the acquiring firm. Regression results measuring this effect and more discussion of the data appear in Section F of the Internet Appendix. The difference between the probability of retaining jets from the target and acquiring firms is highly statistically significant in all specifications.

Of course, the jet needs of a merger target may be different from those of a standalone firm that goes private in an LBO. In particular, if economies of scale are realized when the acquisition target is absorbed into the operations of the acquirer, it may be quite natural for some jets to be sold, even in the absence of any agency problems. Nonetheless, the discrepancy between the treatment of the target and acquirer jets is quite suggestive. When public ownwership in the target firms is replaced with a single blockholder—the acquirer—there is a clear reduction in the propensity to retain the firms' jets. When contrasted with the results on leverage above, this fact may suggest that changes in control and monitoring during LBOs are key to subsequent fleet reductions.

E. Quantiles

The previous sections show that PE-owned firms have smaller jet fleets, *on average*, than publicly traded or private, non-PE firms, both across and within firms. This section describes the distribution of jet fleets in more detail to better understand these differences at the mean. In fact, one might already suspect that average differences are driven by the top part of the jet distribution. The descriptive statistics in Table I show that more than 60% of firms in the cross-sectional sample have no jet visible in the data, and the results in Table III show that estimated average magnitudes are sensitive to winsorization of the largest fleets.

Table VI presents quantile regressions that formally test this suspicion. Column 1 indicates that, indeed, there is essentially no difference between the 50th percentiles of the distributions of the ratio of seats to sales in PE-owned and non-PE-owned firms, after controlling for size and location through the quantile regression. The differences at the 60th and 70th percentiles in columns 2 and 3 are well below the mean difference from Table III. The difference at the 80th percentile is a bit larger than the mean difference, and the difference at the 90th percentile is two times larger still. Thus, the difference in conditional averages between public and PE-owned firms that we saw in Table III does not manifest itself throughout the conditional distribution of jet fleets. Instead, it is driven by firms in the upper part of the fleet distribution. Panel B of Table VI shows that results are robust to correcting for censoring using the algorithm of Chernozhukov and Hong (2002).[18]

[TABLE VI ABOUT HERE.]

These quantile results are potentially quite important for understanding the nature of the agency problems that this paper measures. Although there is a large average difference in jet fleets between public and PE-owned firms, the quantile results indicate that large fleets are far from ubiquitous in public firms. However, they are also not limited to a very small number of "bad apples." Instead, it seems that a substantial minority—perhaps 20% to 30%—of public firms have jet fleets that appear excessive by the standards of PE-owned firms.

These results are also helpful in ruling out a class of alternative explanations for the results presented previously. One might suggest various aspects of public or PE ownership itself that would produce differences in jet fleets. For example, perhaps executives in all public firms must spend more time traveling to meet with outside shareholders who are spread across the country. Or perhaps some feature of the PE ownership structure changes a firm's objective function or discount factor in such a way that jets are less attractive investments to PE-owned firms. Stories like this could produce differences in jet fleets between public and PE-owned firms in both cross-sectional and panel data. They would also predict that all public firms would have a modest jet fleet, albeit larger than those in PE-owned firms. Instead we see that most public firms have no visible jet, but a substantial minority have surprisingly large fleets.[19] Results are thus more consistent with the presence of agency problems in this minority of firms than with any structural difference in jet needs between public and PE-owned firms.

IV. Conclusions

This paper presents evidence showing that firms owned by PE funds average smaller jet fleets than do publicly traded or private, non-PE-owned firms. This difference is not likely to be driven by omitted variables in the cross-section, as there are also clear reductions in fleet size within firms when public firms are taken private in an LBO. Most firms in the sample, however, have no jets visible in the data, and both standard and censored quantile regressions indicate that mean differences between PE-owned and other firms are driven by firms in the top 30% of the conditional jet fleet distribution. I argue that these results are most consistent with the presence of agency problems in a substantial minority of public firms.

I view these results as contributing a somewhat nuanced point to the debate on the severity of agency problems in public firms and the need for further reform of executive compensation. PE ownership produces clear reductions in jet fleets, and there are many public firms whose jet fleets appear large by the standards of PE-owned firms. These results conflict with the strict view that observed executive compensation arrangements in all public firms are the result of optimal contracts negotiated at arm's length. On the other hand, public firms with excessive jet fleets are in the minority. The kind of agency problems that manifest themselves in excessive jet fleets are far from ubiquitous in public firms.

These insights might be important for evaluating the costs and benefits of various proposals to improve corporate governance. For example, imposing costly regulations on all firms might be inappropriate if agency problems are severe only in a minority. More research into appropriate policy responses to heterogeneity in governance problems would be welcome.

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Notes

[1]In this paper, I adopt the convention of referring to investors that are in the business of buying firms through LBOs as "private equity" investors, as do Kaplan and Stromberg (2009). Other authors may use the term "private equity" to refer to both these LBO investors as well as to venture capital investors that focus their attention on younger firms.

[2]It is easy to show that the results extend to a model in which the cost c of being private can also vary across firms, potentially representing variation in the amount of profits that managers can divert to themselves through non-jet channels when their firm is public.

[3]The factors that limit my ability to construct cross-sections from multiple years are the availability of the PE ownership dummy described below and the costly process of cleaning the name merge of firm and jet data. I thus focus on carefully constructing one cross-sectional sample and one sample of firms that went from public to PE-owned.

[4]The jet data were merged by firm name onto both the cross-sectional and buyout samples of firms. More details on this procedure are available in the Internet Appendix.

[5]Rajan and Wulf (2006) use data from a survey of 300 publicly traded firms, and Yermack (2006) uses data from public firm proxy statements.

[6]The data do not, of course, include any information on other forms of transportation used by firms that do not own or lease a jet. For example, they do not include information on jets that might be chartered for individual trips. The jet chartering industry has reportedly grown rapidly in the years since the September 11 terrorist attacks made commercial air travel more difficult. It may well be the case that many firms in the data substitute chartered jets for jets that they operate themselves, presumably because they find it cheaper or more efficient to do so.

[7]The JETNET data take the form of a transactions file that lists jet transactions beginning in 1989 and a current cross-sectional file of registered jets. Jets that were delivered before 1989, that were never involved in a transaction after 1989, and that are not currently registered to fly anywhere in the world are likely absent from the JETNET data. Thus, the pre-1989 data plotted in Figure 1 likely undercount the total number of jets delivered in each year, with the bias being more substantial earlier in the sample. However, inspection of the gaps in the sequences of jet serial numbers appearing in the data suggest that the undercount is not too severe for the period shown in the figure, beginning in 1965.

[8]A number of other papers perform comparisons like this using various samples of LBOs—some notable examples include Maupin, Bidwell, and Ortegren (1984), Lehn and Poulsen (1989), Opler and Titman (1993), Servaes (1994), Halpern, Kieschnick, and Rotenberg (1999), Weir, Laing, and Wright (2005), and Stuart and Yim (2010).

[9]To be clear, quintile 5 includes the largest firms, regardless of their ownership. The bars then display averages by ownership group within this size group.

[10]I focus on the ratio of seats to sales to avoid making arbitrary assumptions about the many firms with zero jets, but one might also think it natural to use the logarithm of jet seats as a dependent variable. In the Internet Appendix, I present regressions like this, replacing all zero-jet observations with the logarithm of the smallest nonzero observation. As this method overestimates fleets in zero-jet firms, and PE-owned firms are more likely to have zero jets, it should attenuate the estimated effect of PE ownership. Nonetheless, estimates of the effect of PE ownership on the logarithm of seats range from -0.42 to -0.56, and all are statistically different from zero at the 5% or 1% level.

Due to the many firms in this sample with zero visible jets, some economists might also argue that one should use estimators like the Tobit that account for censoring in the dependent variable. Section C of the Internet Appendix presents Tobit results. Results are quite robust to using the logarithm as the dependent variable or using the Tobit estimator.

[11]That is, observations with values above the 95th percentile are replaced with the value at the 95th percentile. Winsorizing at higher threshholds (like 99%) *raises* the estimated magnitude of the effect of PE ownership.

[12]In this set of results, clustering by headquarters state actually *lowers* the standard errors on the PE ownership dummy, so the table reports the larger, unclustered standard errors. In all results, the choice of clustering matters most in specifications in which state fixed effects are not included, as one would expect.

[13]It is worth noting that the firms with jets prior to their LBO are primarily drawn from the most recent wave of LBOs. Of the 32 firms with jets in the year prior to their LBO, 18 went private in 2007, five in 2006, and three in 2005. One might worry that fleet reductions in these firms were not driven by the LBO, but rather by the oil price spike, financial crisis, and recession of 2008 and 2009. However, the cross-sectional difference between public and PE firms documented in the previous section suggests that these factors are not driving differences between public and PE-owned firms, because the public firms in the cross-section could also have reduced their fleets. Further, because jets are long-lived, the stock of jets is relatively inelastic. It is not possible that all jet operators worldwide substantially reduced their fleets between 2007 and 2009 unless large numbers of jets were retired or destroyed. Finally, there are also notable reductions in fleets among the firms in the panel that went private before 2006. As we will see, regression results change little when excluding observations from 2008 and 2009.

[14]To avoid colinearity with the firm dummies and time variables, the year effects are normalized to sum to zero and to be orthogonal to a time trend using the transformation described by Deaton (1997). With this normalization, the estimated year effects measure any cyclical changes in the probability of having a jet. Any trend could then appear in the firm fixed effects or the LBO dummies.

[15]In the Internet Appendix, I present similar results with the seats-to-sales ratio as the dependent variable. Although there is considerable variation across specifications, preferred results indicate a post-LBO fleet size reduction of about 43%. Section E of the Internet Appendix also discusses results from a small sample of firms that went from PE-owned to public in an IPO. Results are consistent with fleets growing again after IPO, but standard errors are too large to draw firm conclusions.

[16]This regression is presented in the Internet Appendix. Results are quite similar using the residuals from regressions with total salary paid to the top five executives as the dependent variable.

[17]I thank the anonymous referee for suggesting this line of inquiry.

[18]Section G of the Internet Appendix provides a graphical depiction of these quantile results, and it motivates and discusses the Chernozhukov and Hong (2002) results.

[19]Recall also that Rajan and Wulf (2006) and Yermack (2006) find little relation between jet use and ownership variables within the cross-section of public firms.

Table I
Descriptive Statistics

This table presents summary statistics on the firms in the sample. Sales are in billions of 2008 dollars. Employees are in thousands. Flights are in thousands and represent the number of scheduled flights departing in 2008 from airports within 50 miles of the firm's headquarters city. The sample in columns 1 to 3 includes all U.S. firms with 2008 sales greater than $1 billion, with exceptions described in the text. The sample in columns 4 and 5 consists of a panel of 69 firms that went from public to private in a PE-led LBO between 1992 and 2007, where the firm's sales in the year prior to the LBO were at least $1 billion (in 2008 dollars).

	2008 Cross-section Public Firms	2008 Cross-section Private non-PE	2008 Cross-section PE-owned Firms	LBO Panel 1 year before LBO	LBO Panel 1 year after LBO
	(1)	(2)	(3)	(4)	(5)
N	1242	343	101	69	69
Sales, Mean (billions of 2008$)	9.20	4.15	4.16	4.24	4.20
Sales, Standard deviation	24.86	8.91	5.33	4.23	4.32
Sales, Median	2.99	2.21	2.30	2.33	2.49
Employees, Mean (thousands)	25.71	11.74	20.97	27.16	26.28
Employees, Standard deviation	76.78	20.51	36.04	40.40	41.72
Employees, Median	8.80	5.17	9.94	14.6	14.67
Flights within 50 miles, Mean	194.4	168.0	197.5	182.3	
Flights within 50 miles, Std. dev.	149.2	155.8	151.7	142.6	
Flights within 50 miles, Median	161.4	140.3	150.5	149.0	
Dummy for any jet, Mean	0.400	0.359	0.277	0.464	0.362
Number of seats if any jet, Mean	24.96	27.14	10.52	15.0	13.8
Number of seats if any jet, Std. dev.	57.91	64.9	9.57	16.1	10.9
Number of seats if any jet, Median	14	15	8	9	9
Ratio of seats to sales, Mean	1.59	3.55	0.79	1.76	1.35
Ratio of seats to sales, Std. dev.	7.11	16.59	2.23	3.62	3.28
Ratio of seats to sales, Median	0.00	0.00	0.00	0.00	0.00
Ratio of seats to sales, 75th Percentile	1.31	2.79	0.50	1.44	0.73
Ratio of seats to sales, 90th Percentile	4.38	9.12	2.29	7.36	4.16

Table II
OLS Regressions of a Dummy for Operating Any Jets on Ownership Variables and Controls

The dependent variable is a dummy indicating that a firm operates at least one jet, including fractionally owned jets. The sample includes U.S. firms with 2008 sales greater than $1 billion. Other variables are described in Table I. Results from probit models are very similar to these OLS results and are reported in the Internet Appendix. Standard errors (in parentheses) are clustered by headquarters state. Standard errors fall if unclustered or clustered by industry. The symbol *** indicates statistical significance at the 1% level, ** at 5%, and * at 10%.

	(1)	(2)	(3)	(4)	(5)	(6)
Private Equity Owned	-0.123	-0.112	-0.112	-0.107	-0.128	-0.125
	(0.071)*	(0.055)**	(0.055)**	(0.053)**	(0.052)**	(0.050)**
Private, not PE	-0.042	-0.009	-0.010	-0.0003	-0.021	-0.017
	(0.038)	(0.032)	(0.033)	(0.034)	(0.039)	(0.040)
Log Sales		0.122	0.136		0.126	
		(0.017)***	(0.020)***		(0.019)***	
Log Employees		0.060	0.054		0.060	
		(0.012)***	(0.014)***		(0.014)***	
Log Flights within 50 Miles		-0.074	-0.084		-0.082	
		(0.010)***	(0.039)**		(0.041)**	
Constant	0.400	1.128	1.221	-0.516	1.809	0.172
	(0.020)***	(0.205)***	(0.412)***	(0.280)*	(0.406)***	(0.224)
State Dummies	No	Yes	Yes	Yes	Yes	Yes
NAICS Dummies	None	2 Dig.	2 Dig.	2 Dig.	3 Dig.	3 Dig.
Sales, Emps., & Flights Quadratics	No	No	Yes	No	Yes	No
Sales, Emps., & Flights Decile Dummies	No	No	No	Yes	No	Yes
Observations	1686	1672	1672	1686	1672	1686
R^2	0.004	0.235	0.236	0.239	0.262	0.264

Table III

OLS Regressions of Ratio of Jet Seats to Billions of Dollars of Sales on Ownership Variables and Controls

The dependent variable in all columns is the ratio of the total seat capacity of a firm's aircraft fleet to its sales in billions of 2008 dollars. The sample includes U.S. firms with 2008 sales greater than $1 billion. Other variables are described in Table I. Columns 2 through 6 winsorize the dependent variable at the 95th percentile. OLS standard errors are in parentheses. Standard errors fall if clustered by headquarters state or industry. The symbol *** indicates statistical significance at the 1% level, ** at 5%, and * at 10%.

	(1)	(2)	(3)	(4)	(5)	(6)
Private Equity Owned	-0.791	-0.485	-0.651	-0.609	-0.714	-0.674
	(1.000)	(0.252)*	(0.249)***	(0.254)**	(0.259)***	(0.265)**
Private, not PE	1.969	0.658	0.532	0.598	0.524	0.572
	(0.590)***	(0.148)***	(0.161)***	(0.162)***	(0.177)***	(0.179)***
Constant	1.586	1.199	6.719	-5.859	9.741	-2.529
	(0.274)***	(0.069)***	(1.753)***	(2.586)**	(2.826)***	(3.400)
State Dummies	No	No	Yes	Yes	Yes	Yes
NAICS Dummies	None	None	2 Dig.	2 Dig.	3 Dig.	3 Dig.
Sales, Emps., Flights Logs & Quadratics	No	No	Yes	No	Yes	No
Sales, Emps., Flights Decile Dummies	No	No	No	Yes	No	Yes
Observations	1686	1686	1672	1686	1672	1686
R^2	0.007	0.015	0.169	0.173	0.198	0.201

Table IV

Regressions of a Dummy for Operating any Jets on Dummies for Years Surrounding a Leveraged Buyout

The dependent variable is a dummy indicating whether a firm operates at least one jet (including fractionally-owned jets) in a given year. The sample consists of a panel of 69 firms that went from public to private in a PE-led LBO between 1992 and 2007, where the firm's sales in the year prior to the LBO were at least one billion 2008 dollars. All nonmissing observations from three years before each LBO to four years after are included. All specifications include firm fixed effects. Specifications with size controls include logarithms and quadratic polynomials in sales and employees, but coefficients are not reported. Specifications with year effects include year dummies normalized with the transformation of Deaton (1997), but coefficients are not reported. Columns 3 and 4 exclude all observations from 2008 and 2009 when the financial crisis and recession could affect results. Standard errors (in parentheses) are clustered by headquarters state. The symbol *** indicates statistical significance at the 1% level, ** at 5%, and * at 10%.

	(1)	(2)	(3)	(4)	(5)	(6)
t = LBOyear - 3	-0.048	-0.019	0.031	0.042	0.030	0.031
	(0.046)	(0.078)	(0.029)	(0.056)	(0.028)	(0.056)
t = LBOyear - 2	0.002	0.040	0.031	0.055	0.031	0.051
	(0.023)	(0.044)	(0.024)	(0.043)	(0.023)	(0.042)
t = LBOyear	-0.058	-0.102	-0.064	-0.097	-0.061	-0.095
	(0.030)*	(0.029)***	(0.033)**	(0.035)***	(0.031)**	(0.033)***
t = LBOyear + 1	-0.101	-0.162	-0.108	-0.141	-0.096	-0.149
	(0.038)***	(0.039)***	(0.042)**	(0.047)***	(0.038)**	(0.041)***
t = LBOyear + 2	-0.106	-0.164	-0.106	-0.137	-0.082	-0.129
	(0.043)**	(0.056)***	(0.077)	(0.077)*	(0.051)	(0.063)**
t = LBOyear + 3	-0.142	-0.217	-0.112	-0.157	-0.120	-0.176
	(0.074)*	(0.112)*	(0.105)	(0.112)	(0.055)**	(0.103)*
t = LBOyear + 4	-0.209	-0.288	-0.212	-0.259	-0.155	-0.212
	(0.085)**	(0.146)**	(0.154)	(0.197)	(0.066)**	(0.136)
Constant	0.462	0.478	-0.237	-0.130	-0.022	0.084
	(0.020)***	(0.032)***	(0.334)	(0.280)	(0.285)	(0.251)
Size Controls	No	No	Yes	Yes	Yes	Yes
Year Effects	No	Yes	No	Yes	No	Yes
2008 & 2009 Included	Yes	Yes	No	No	Yes	Yes
Observations	452	452	348	348	452	452
R^2	0.814	0.824	0.862	0.87	0.85	0.857

Table V

Regressions of a Dummy for Fleet Reduction on Pre-LBO Firm Characteristics

The dependent variable is a dummy indicating whether a firm in the sample of 69 LBOs used in the paper reduced its jet fleet within two years of the LBO. The independent variable in the first column is a dummy variable equal to one if the firm changed its CEO between one year before and one year after the LBO. The independent variables in columns 2 to 6 are all winsorized at the 5% and 95% levels and normalized by their standard deviation, so that the coefficients indicate the impact of a one-standard deviation increase. In column 2 the independent variable is the ratio of a firm's reported spending on personal jet use for its CEO to sales in the year before the LBO. In columns 3 to 6, the dependent variables are residuals from regressions presented in the Internet Appendix. The symbol *** indicates statistical significance at the 1% level, ** at 5%, and * at 10%.

	(1)	(2)	(3)	(4)	(5)	(6)
CEO Change	0.108					
	(0.110)					
CEO Personal Aircraft Use / Sales		0.159				
		(0.052)***				
Residual CEO Salary			0.109			
			(0.054)**			
Residual Q				0.055		
				(0.055)		
Residual Sales / Assets					-0.004	
					(0.056)	
Residual ROA						0.030
						(0.056)
Constant	0.235	0.279	0.279	0.290	0.290	0.290
	(0.078)***	(0.052)***	(0.054)***	(0.055)***	(0.055)***	(0.055)***
Observations	69	68	68	69	69	69
R^2	0.014	0.123	0.059	0.015	0.00006	0.004

Table VI
Quantile Regressions of Ratio of Jet Seats to Billions of Dollars of Sales on Ownership Variables and Controls

The dependent variable in all columns is the ratio of the total seat capacity of a firm's aircraft fleet to its sales in billions of dollars. The column headers indicate the quantile of the conditional seats-to-sales distribution at which effects are estimated. Estimates in all columns include controls for logarithms and quadratic polynomials in sales, employees, and flights within 50 miles, but coefficients are not reported. The censored quantile estimates in Panel B are computed using the algorithm of Chernozhukov and Hong (2002) as described in the Internet Appendix. The number of observations reported for these estimates refers to the number of observations used in the quantile regression in the final step of the estimation. Standard errors are calculated using the block bootstrap, with headquarters state as the block variable. Thus, standard errors are robust to error correlation within states. The symbol *** indicates statistical significance at the 1% level, ** at 5%, and * at 10%.

	50th	60th	70th	80th	90th
	(1)	(2)	(3)	(4)	(5)
Panel A: Quantile Regression Estimates					
Private Equity Owned	-0.008	-0.073	-0.235	-0.837	-1.636
	(0.017)	(0.061)	(0.147)	(0.412)**	(0.602)***
Private, not PE	-0.0004	-0.012	0.006	0.911	2.526
	(0.007)	(0.048)	(0.350)	(0.789)	(0.882)***
Observations	1672	1672	1672	1672	1672
Panel B: Censored Quantile Estimates					
Private Equity Owned	-0.006	-0.103	-0.209	-1.156	-2.256
	(0.740)	(0.284)	(0.306)	(0.551)**	(0.764)***
Private, not PE	-0.0002	0.026	0.114	1.029	1.933
	(0.174)	(0.175)	(0.392)	(0.734)	(0.820)**
Observations	1445	1560	1567	1582	1588

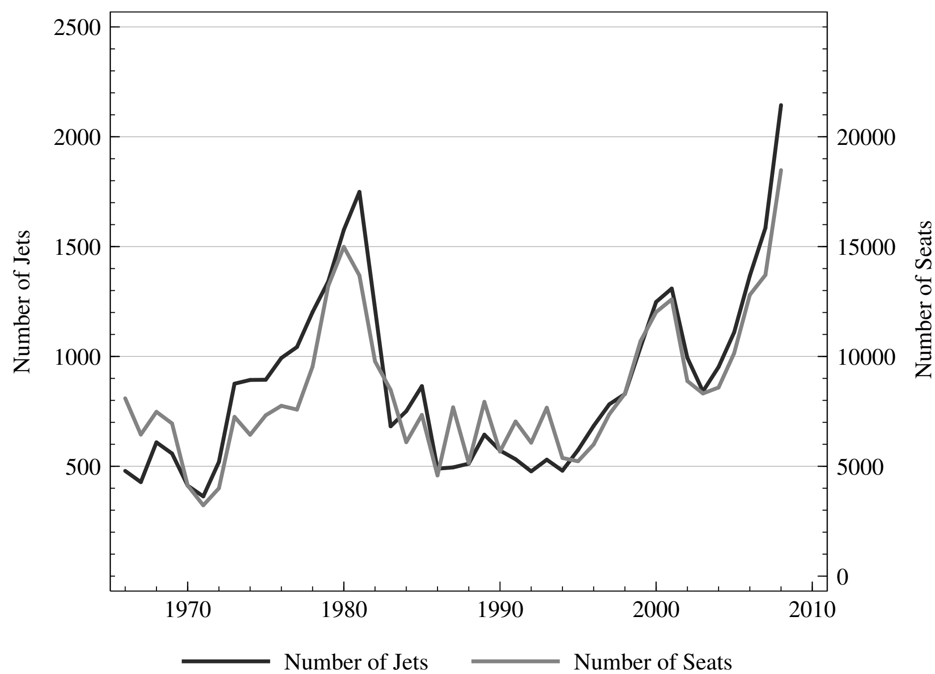

Figure 1. Total jets and total seats by year of initial delivery for universe of jets in JETNET data. This figure displays aggregate time-series data on the total number of jets and total seats delivered by year of delivery for the universe of jets that ever appear in the JETNET data.

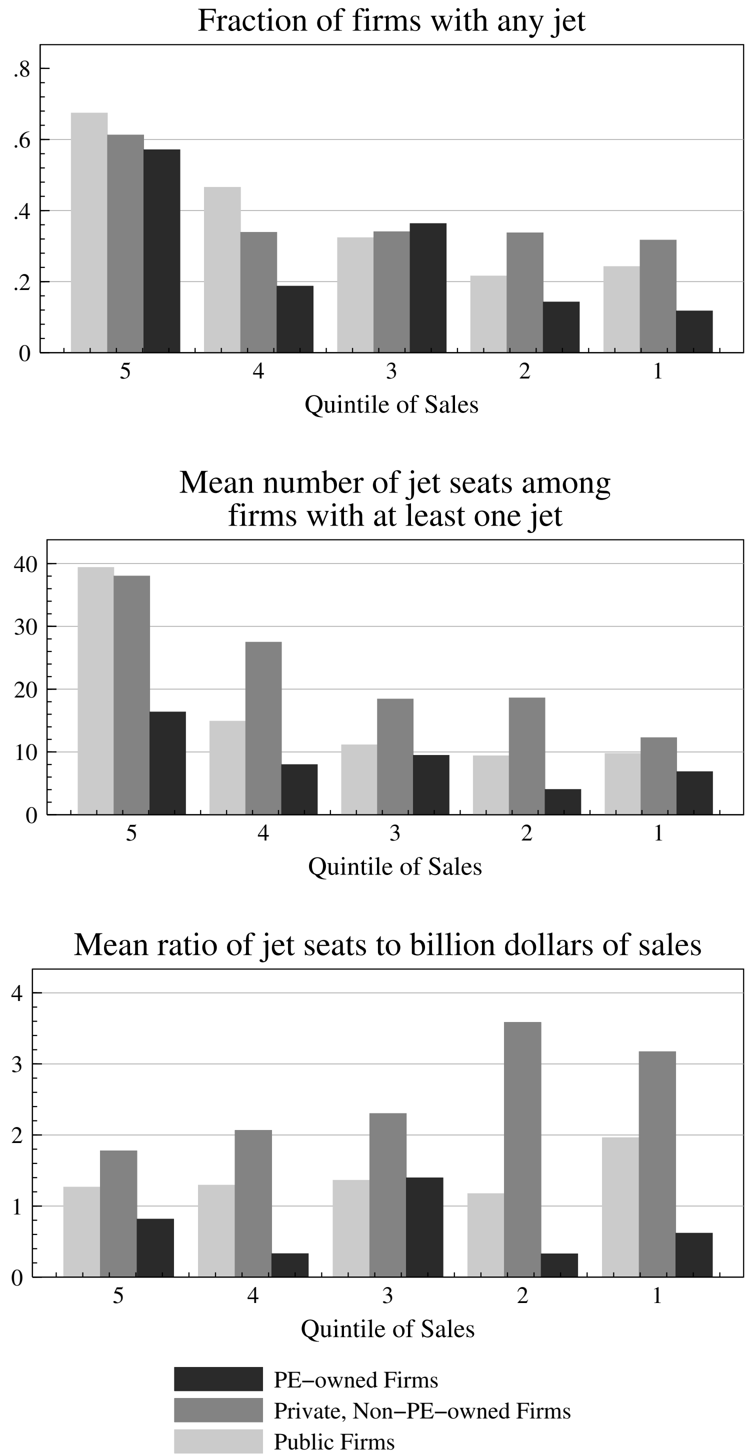

Figure 2. Jet fleets by quintile of firm sales. This figure describes corporate jet fleets in the cross-section, broken out by size and ownership. Quintile 5 contains the largest firms and quintile 1 the smallest. The second two panels exclude two non-PE firms with ratios of jet seats to billions of dollars of sales greater than 200.

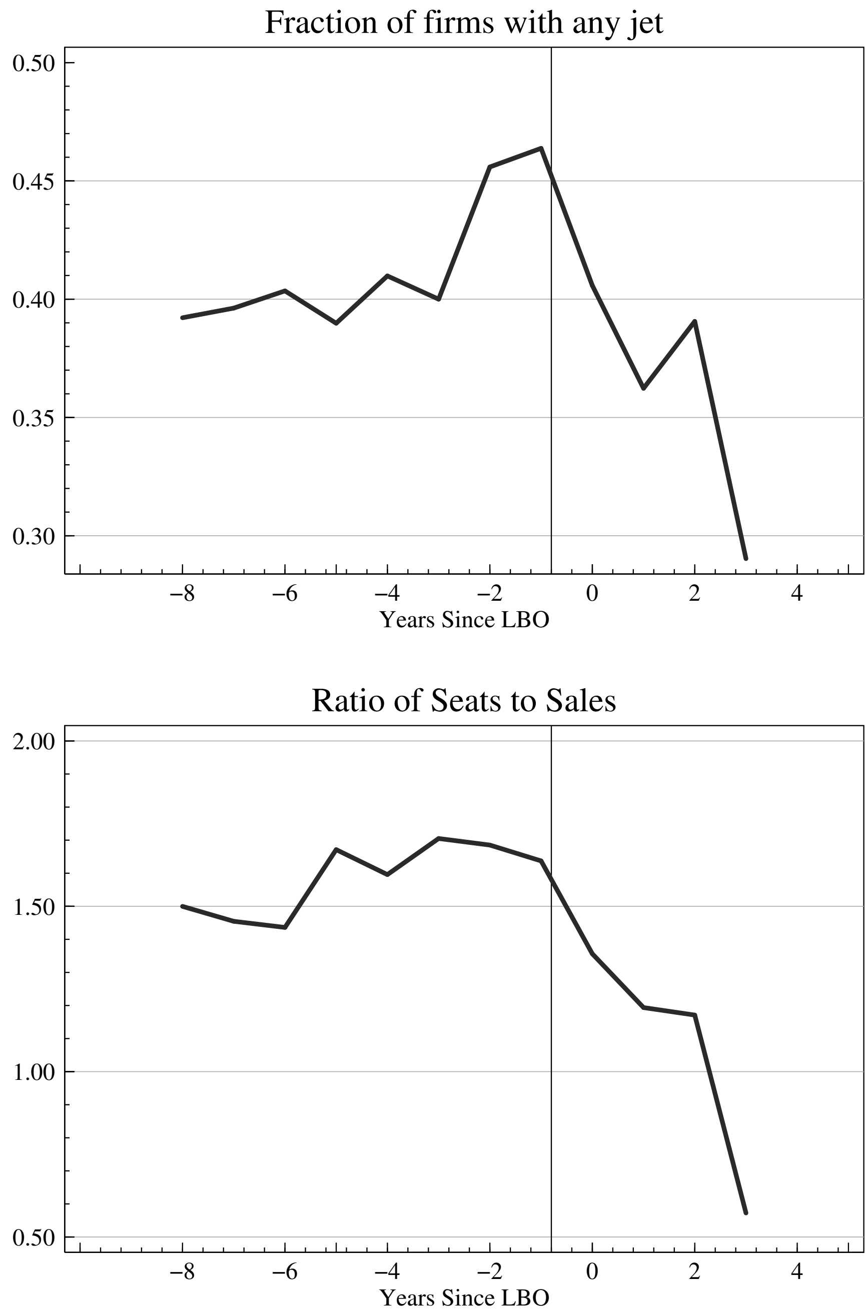

Figure 3. Jet fleets in years surrounding leveraged buyouts. The top panel plots the fraction of firms with any jet in the unbalanced panel of 69 LBOs that occurred between 1992 and 2007. The bottom panel plots the aggregate ratio of jet seats to billions of dollars of sales in the same sample.

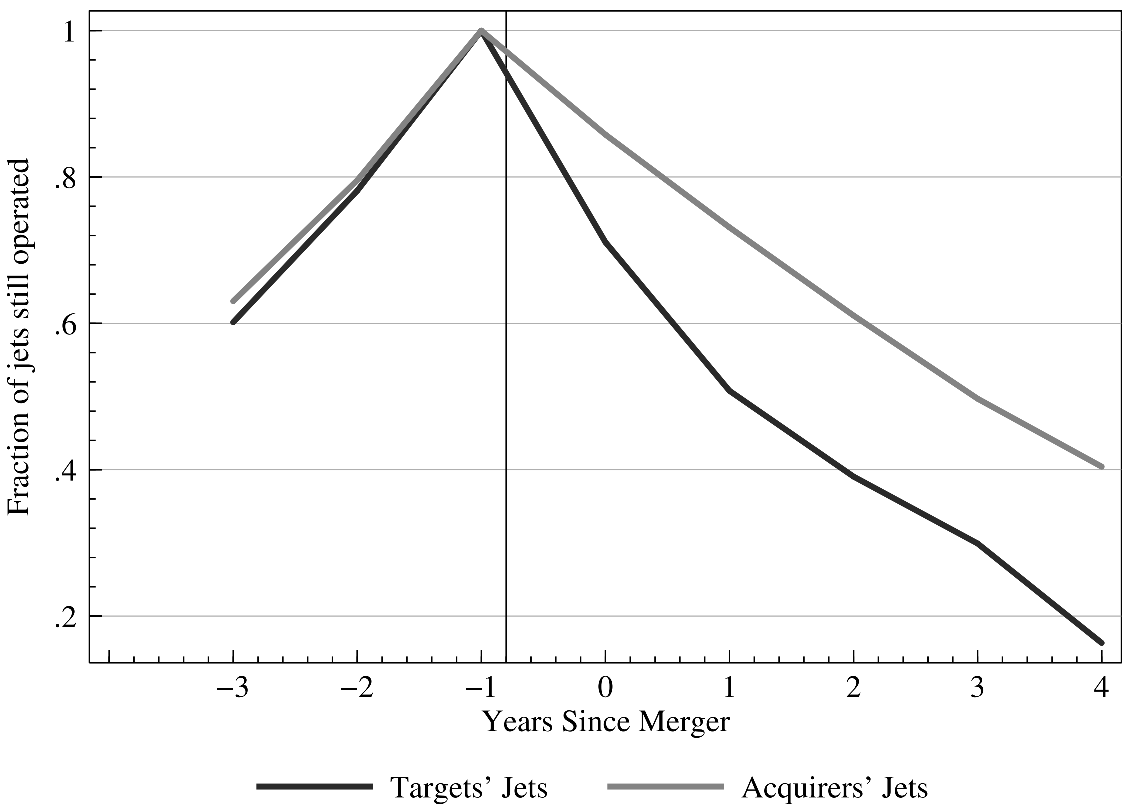

Figure 4. Retention of jets operated by public firms involved in mergers. The figure plots the probability that a jet operated by a target or acquiring firm in the year before a merger of two public firms is operated by the same firm in the years prior to merger or by the new, combined entity in years following the merger. This probability is equal to one by construction in the year before the merger, as this is how the sample of jets is chosen.

Internet Appendix for "Agency Problems in Public Firms: Evidence from Corporate Jets in Leveraged Buyouts"*

JESSE EDGERTON

This appendix contains a variety of robustness tests, supplementary results, and additional discussion related to the material in the paper. In particular, Section A provides additional detail on the characteristics and construction of the samples of firms and jets used in the paper. Section B presents alternative specifications for the cross-sectional results in Section III.A of the paper. Section C discusses the issue of censoring in the cross-section and presents Tobit estimates. Section D presents alternative results using fleet size as the dependent variable in the LBO panel used in section III.C of the paper. Section E discusses results from a small sample of PE-owned firms that went public. Section F provides details behind the results distinguishing the effects of leverage and monitoring in Section III.D of the paper. Section G provides additional detail on the quantile results in Section III.E of the paper. Section H further discusses results on private, non-PE-owned firms, and Section I further discusses concerns related to two potential alternative explanations for the results in the paper.

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A. Further Data Detail

A.1. Characteristics of LBO Firms

Table IA.I compares the characteristics of firms that went private in LBOs with observably similar large public firms that did not do an LBO. It uses data from Compustat and Execucomp and regresses firm performance and compensation measures on a dummy equal to one for firms that would go through an LBO in the subsequent two years, controlling for size, industry, and year. The estimated coefficient on the LBO dummy thus measures the difference in means between LBO and non-LBO firms, conditional on these covariates.

Table IA.I
OLS Regressions of Firm Performance and Compensation Variables on a Dummy for Subsequent LBOs and Controls

The dependent variable in each column is named in the column header. The sample includes all public firms with sales greater than $1 billion (in 2008 dollars) from 1992 to 2008. Columns 4 to 6 are limited to firms that appear in Execucomp and the firms from the sample of 69 LBOs used throughout the paper that did not appear in Execucomp, for which I hand collect salary information. LBO Firm Dummy takes the value of one for firms that went private in an LBO within the subsequent two years. These firms are omitted from the sample in years prior to two years before their LBO. The dependent variables in columns 1 to 3 are winsorized at the 1st and 99th percentiles. The dependent variables in columns 4 to 6 are in logarithms, where any zero values are replaced by the 1st percentile of nonzero values. Standard errors are clustered by NAICS two-digit industries. The symbol *** indicates statistical significance at the 1% level, ** at 5%, and * at 10%.

	Average Q	Sales to Assets	ROA	Top 5 Exec. Salary	Top 5 Exec. Salary	CEO Salary
	(1)	(2)	(3)	(4)	(5)	(6)
LBO Firm Dummy	-0.357	-0.146	-0.002	0.100	0.046	0.001
	(0.107)***	(0.079)*	(0.012)	(0.025)***	(0.022)**	(0.042)
Log Prior Year Assets	-0.065	-0.271	-0.014	0.174	0.191	0.166
	(0.038)*	(0.027)***	(0.003)***	(0.020)***	(0.018)***	(0.020)***
Average Q				0.019	0.003	-0.085
				(0.017)	(0.017)	(0.035)**
ROA				0.530	0.331	1.005
				(0.183)***	(0.127)***	(0.194)***
Sales to Assets Ratio				0.041	0.016	0.008
				(0.019)**	(0.013)	(0.017)
Observations	20713	20713	20713	15979	15979	15979
R^2	0.13	0.541	0.22	0.442	0.527	0.086
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
NAICS Dummies	2 Dig.	2 Dig.	2 Dig.	None	2 Dig.	2 Dig.

The LBO firms stand out for having lower market valuations as measured by average Q: column 1 of the table shows that Q averages about 0.36 lower in the LBO firms. There is also some evidence that they have lower sales to assets ratios. There is little evidence, however, of any difference between LBO and other firms in terms of ROA.

Columns 4 through 6 present regressions with executive salary measures as the dependent variable. In column 4, the LBO firms are found to have paid the top five executives named in their proxy statements 10% more in total salary than other firms, controlling for size, year, and the three performance measures used as dependent variables in columns 1 to 3. This coefficient is statistically significant at the 1% level. However, adding industry dummies to the regression in column 4 lowers the estimated effect to about 5%. Although the standard error also falls, the effect is only statistically significant at the 5% level in this specification. Column 6 uses the salary of the CEO alone in a specification that is otherwise the same as column 5. There is little evidence of any effect, although these data are considerably noisier than the five-executive aggregate, and standard errors cannot rule out effects as large as those in column 5. To summarize, results are mixed, but provide some support for the notion that buyout targets underperformed their peers and paid their executives generous salaries in the years before the buyout.

A.2. Additional Detail on Jet and Firm Data

Figure IA.1 presents statistics describing the size of the new jets delivered in each year that eventually appeared in the Jetnet data. The median new jet had seven seats throughout the 1970s and stepped up to eight seats from the 1980s to the end of the sample. The 10th percentile jet bounced around five seats throughout the sample period. The 90th percentile jet, however, got notably larger over time.

For public firms, the main cross-sectional sample used in the paper consists of Compustat annual observations with fiscal years that end between July 2008 and June 2009. For private

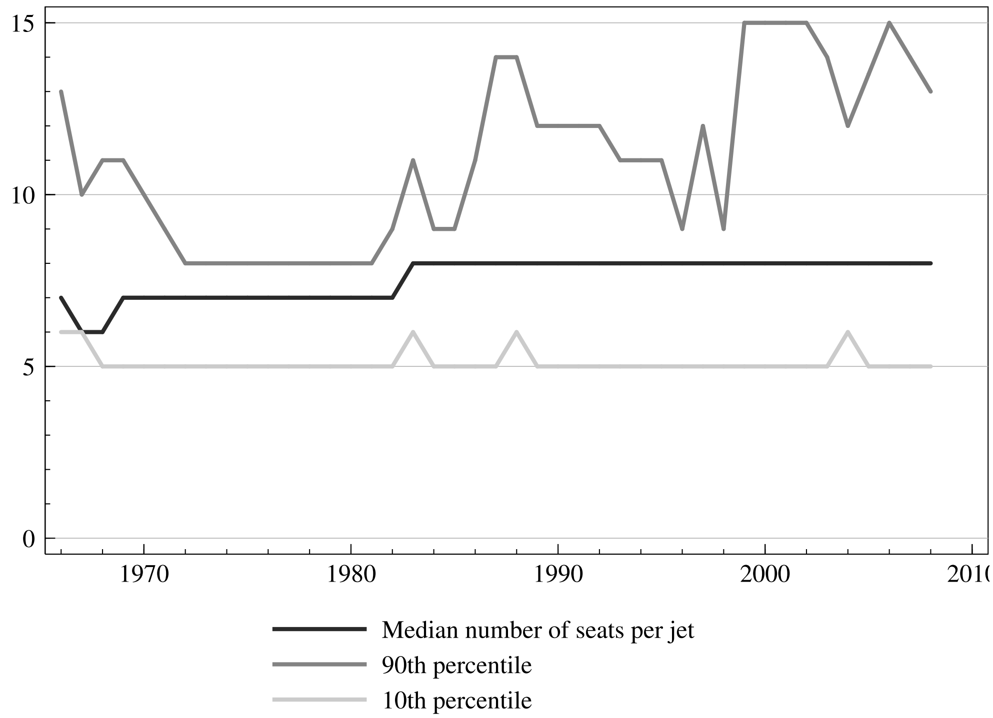


Figure IA.1. Jet size by year of initial delivery for universe of jets in JETNET data. The figure presents the 10th, 50th, and 90th percentiles of the distribution of all jets that ever appeared in the Jetnet data, by the year of their delivery.

firms, I begin with the Forbes list of private companies that was published in 2009 and refers to 2008 data.[1] I use the LexisNexis Corporate Affiliations database to determine which firms are held by PE funds, and I checked these determinations against the websites of PE funds where available. I also add three observations for firms that went private in 2008 and 2009 from the sample of buyouts described below. I limit the sample of Compustat firms to those with 2008 sales greater than $1 billion for comparability to the Forbes data. I exclude aircraft manufacturers, air transportation firms, and firms domiciled or headquartered outside of the U.S. One might worry that banks and other financial institutions could incorrectly appear

[1]Unfortunately, in 2009 Forbes limited the list to firms with sales greater than $2 billion, and there are only 248 such firms. In 2008, however, the Forbes list extended to firms with as few as $1 billion in sales, of which there were 441 firms. For firms that appeared in the Forbes list in 2008 and not in 2009, I impute 2009 values of sales and employees by scaling 2008 values by the median growth rate among firms in the same two-digit NAICS industry.

as operating large fleets when, in fact, they are lessors. The data appear to do a good job distinguishing operators from lessors, as there are no financial institutions that have suspiciously large fleets.

To merge the firm and jet data sets, I first capitalize names, lengthen abbreviations like "Bros," truncate a list of common end words like "Company" (up to three times), and remove spaces and punctuation in names in Compustat, Forbes, and the jets data. I then attempt to merge these standardized names from the firm files with the firm listed as the "Operator," "Lessee," or "Flight Department" for each plane. If a plane does not match on this merge, I attempt to merge on the name of the second firm listed on the plane record, usually listed as "Owner" or "Parent Company." For planes that were still not matched to firms, I try another match using only the first word of the primary firm name (or the first two words if the first is very common), and then a third "fuzzy" merge using the Stata *reclink* command. All potential merges from the first-word or fuzzy merges are hand-verified by comparing the firm addresses listed in each data set and doing additional internet research where necessary. The first-word merge is particularly fruitful as it catches many occasions where, for example, planes owned by Microsoft were listed under "Microsoft Flight Department" in the jets data. Further details on the merging procedure are available upon request.

In the cross-section, 1,681 jets operated by 648 unique firms are matched. In the buyout sample, 1,219 jet-firm-year observations involving 39 firms were matched. Although this procedure for matching planes to firms is not perfect, it appears to have done a good job matching planes to many firms. For example, of the 100 largest firms by sales in 2008, 81 are matched to at least one jet. Problems with the match seem most likely to arise when a jet is listed only under the name of a subsidiary (e.g., GEICO), when the parent (e.g., Berkshire-Hathaway) is what appears in Compustat or Forbes. This issue is most likely to affect large firms with disparate subsidiaries. Because such firms are likely to be publicly traded, any bias introduced by an imperfect match is likely to understate the difference observed between public and private firms.

B. Cross-Sectional Probit and Log Seats Results

Table II in the main text of the paper discusses results using a dummy for operating any jet as the dependent variable. These are simple linear probability models estimated with OLS. Table IA.II presents similar results using a probit specification. Results are very similar to those in the text.

Table III in the paper uses the ratio of seats to sales as a dependent variable, and I calculate the percentage change in this variable associated with PE ownership, relative to the mean for public firms. Table IA.III presents regressions that instead use the logarithm of seats as the dependent variable, where observations with zero jets are set equal to the logarithm of the smallest nonzero fleet. Implications for the percentage change in fleet size associated with PE ownership are very similar to those from the results presented in the text.

Table IA.II

Probit Regressions of a Dummy for Operating any Jets on Ownership Variables and Controls

The dependent variable is a dummy indicating that a firm operates at least one jet (including fractionally owned jets). The reported coefficients are marginal effects on the probability of operating a jet for continuous variables and the effect of moving from zero to one for binary variables. The sample includes all U.S. firms with 2008 sales greater than $1 billion, as described in the text. Standard errors are clustered by headquarters state. The symbol *** indicates statistical significance at the 1% level, ** at 5%, and * at 10%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private Equity Owned	-0.122	-0.095	-0.117	-0.110	-0.110	-0.105	-0.129	-0.125
	(0.070)*	(0.064)	(0.055)**	(0.054)**	(0.055)**	(0.052)**	(0.047)***	(0.045)***
Private, not PE	-0.041	0.036	0.025	-0.010	-0.009	-0.0004	-0.024	-0.021
	(0.038)	(0.037)	(0.034)	(0.032)	(0.032)	(0.032)	(0.037)	(0.037)
Log Sales		0.106	0.105	0.112	0.123		0.115	
		(0.016)***	(0.018)***	(0.017)***	(0.020)***		(0.020)***	
Log Employees		0.055	0.065	0.061	0.056		0.062	
		(0.013)***	(0.014)***	(0.014)***	(0.017)***		(0.017)***	
Log Flights within 50 Miles				-0.069	-0.071		-0.070	
				(0.010)***	(0.036)**		(0.037)*	
Sales (billions)					-0.002		-0.001	
					(0.002)		(0.002)	
Sales^2					3.69e-06		2.98e-06	
					(4.01e-06)		(3.87e-06)	
Employees (thousands)					0.0003		0.0003	
					(0.0008)		(0.0008)	
Employees^2					5.47e-08		2.85e-08	
					(1.58e-06)		(1.53e-06)	
Flights within 50 Miles					0.0002		0.0003	
					(0.001)		(0.001)	
Flights^2					-5.72e-07		-7.52e-07	
					(1.92e-06)		(2.03e-06)	
State Dummies	No	No	Yes	Yes	Yes	Yes	Yes	Yes
NAICS Dummies	None	None	2 Dig.	2 Dig.	2 Dig.	2 Dig.	3 Dig.	3 Dig.
Sales, Employees, & Flights Decile Dummies	No	No	No	No	No	Yes	No	Yes
Observations	1686	1686	1661	1647	1647	1661	1635	1649

Table IA.III
OLS Regressions of Log Jet Seats on Ownership Variables and Controls

The dependent variable in all columns is the logarithm of the total seat capacity of a firm's aircraft fleet. Observations with zero jets are set equal to the logarithm of the smallest nonzero fleet (-0.827, representing a 1/16 share of a seven-seat jet). The sample in all columns includes all firms with 2008 sales greater than $1 billion, as described in the text. Standard errors are clustered by headquarters state. The symbol *** indicates statistical significance at the 1% level, ** at 5%, and * at 10%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private Equity Owned	-0.560	-0.422	-0.430	-0.508	-0.466	-0.453	-0.537	-0.528
	(0.208)***	(0.180)**	(0.175)**	(0.156)***	(0.150)***	(0.139)***	(0.137)***	(0.133)***
Private, not PE	-0.094	0.257	0.248	0.236	0.121	0.149	0.069	0.079
	(0.128)	(0.125)**	(0.134)*	(0.119)**	(0.111)	(0.116)	(0.136)	(0.146)
Log Sales		0.582	0.566	0.603	0.571		0.526	
		(0.058)***	(0.061)***	(0.060)***	(0.074)***		(0.070)***	
Log Employees		0.216	0.257	0.235	0.212		0.248	
		(0.053)***	(0.054)***	(0.052)***	(0.052)***		(0.050)***	
Log Flights within 50 Miles					-0.252		-0.233	
					(0.117)**		(0.127)*	
Sales (billions)					0.004		0.005	
					(0.006)		(0.006)	
Sales^2					-4.85e-06		-4.87e-06	
					(1.00e-05)		(1.00e-05)	
Employees (thousands)					0.001		0.001	
					(0.002)		(0.002)	
Employees^2					-5.89e-07		-6.19e-07	
					(8.24e-07)		(8.05e-07)	
Flights within 50 Miles					-0.0006		-0.0008	
					(0.004)		(0.004)	
Flights^2					1.47e-06		1.34e-06	
					(6.62e-06)		(7.33e-06)	
Const.	0.488	-0.781	-0.747	-3.191	2.919	-2.068	5.417	0.647
	(0.079)***	(0.111)***	(0.476)	(0.701)***	(1.259)**	(0.868)**	(1.289)***	(0.693)
State Dummies	No	No	No	Yes	Yes	Yes	Yes	Yes
NAICS Dummies	None	None	2 Dig.	2 Dig.	2 Dig.	2 Dig.	3 Dig.	3 Dig.
Sales, Employees, & Flights Decile Dummies	No	No	No	No	No	Yes	No	Yes
Observations	1686	1686	1686	1686	1672	1686	1672	1686
R^2	0.006	0.21	0.242	0.296	0.32	0.323	0.347	0.349

C. Censoring and the Tobit

Due to the many firms in this sample with zero visible jets, some economists might argue that one should use estimators like the Tobit that account for censoring in the dependent variable. Others would say that there is no censoring in this situation. The observations with zero jets do not result from any detrimental transformation of the data—they simply represent firms that really have zero jets (see, for example, Angrist and Pischke (2008)). This second statement is technically correct, but there is a sense in which the data in this application might usefully be thought of as censored. One could argue that the dependent variable that is truly of interest is "firm expenditure on executive travel" or something similar. The number of jet seats in a firm's fleet proxies for this variable, but it will be censored for all firms whose spending is too low to involve a jet.[2] Note that this censoring will tend to obscure any differences between PE-owned and public firms in the censored group. If one is willing to assume that PE-owned firms spend less on travel than publicly traded firms within the censored group, then the OLS estimate of the effect of PE ownership will be biased upward—towards zero in this case—making one less likely to find a large effect of PE ownership on expenditures.

The Tobit addresses any bias introduced by censoring by making the strong assumption that the error term in the uncensored, latent variable of interest is normally distributed. The effects of covariates on this latent variable are then estimated via maximum likelihood. Table IA.IV in this appendix presents Tobit results for the same specifications as the OLS results in Table IA.III. Indeed, estimates of the effects of PE ownership on the latent expenditure variable are *larger* than in the OLS results, by a factor of more than two.

[2]For example, suppose total expenditure on executive travel is y. Jet seats are equal to zero when y is less than some constant $\tilde{y}$ and equal to $\alpha(y - \tilde{y})$ when $y > \tilde{y}$. In this case, the number of jet seats is an ideal proxy for travel expenditure, except it is censored when total expenditure is too low. Obviously, there could be firms that do not have a jet that spend more on travel than some firms that do (or vice versa), but the assumption that firms without a jet spend less than those that do seems to be a reasonable simplification.

It is important to understand how the Tobit assumptions affect these results. Because PE-owned firms are less likely to have jets and tend to have smaller fleets when they do, the Tobit will assume that latent travel expenditures in PE-owned firms are drawn from a conditional normal distribution that is shifted leftward from that of public firms. That is, the Tobit infers that we would find that PE-owned firms have lower travel expenditures than similar public firms within the sample of censored firms, if we were able to observe these expenditures. Thus, the coefficient on PE ownership is more negative than in the OLS results, which assume that both PE and public firms with zero jets have equal travel expenditures. If one is concerned about the effects of censoring but unsatisfied with the bound provided by the OLS estimates, then one might prefer an estimator that is less dependent on assumptions about the distribution of an unobserved variable than is the Tobit. The censored quantile regression estimator presented later will be motivated in part by the desire to avoid these assumptions.

Table IA.IV
Tobit Regressions of Log Jet Seats on Ownership Variables and Controls

The dependent variable in all columns is the logarithm of the total seat capacity of a firm's aircraft fleet. The reported coefficients are effects on the conditional expectation of the latent variable in the Tobit model. Columns with state dummies exclude 21 observations of firms in states where all firms are censored, as these observations prevent the Tobit algorithm from converging. Standard errors are clustered by headquarters state. The symbol *** indicates statistical significance at the 1% level, ** at 5%, and * at 10%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private Equity Owned	-1.439	-0.994	-1.059	-1.259	-1.225	-1.210	-1.414	-1.403
	(0.755)*	(0.631)	(0.604)*	(0.538)**	(0.530)**	(0.493)**	(0.455)***	(0.424)***
Private, not PE	-0.308	0.460	0.573	0.533	0.243	0.307	0.100	0.123
	(0.354)	(0.318)	(0.322)*	(0.283)*	(0.275)	(0.272)	(0.318)	(0.325)
Log Sales		1.082	0.967	1.039	1.260		1.139	
		(0.127)***	(0.141)***	(0.134)***	(0.161)***		(0.156)***	
Log Employees		0.510	0.685	0.647	0.635		0.710	
		(0.126)***	(0.142)***	(0.139)***	(0.155)***		(0.148)***	
Log Flights within 50 Miles					-0.489		-0.453	
					(0.253)*		(0.274)*	
Sales (billions)					-0.013		-0.009	
					(0.010)		(0.009)	
$Sales^2$					0.00003		0.00002	
					(0.00002)		(0.00002)	
Employees (thousands)					-0.0005		-0.0005	
					(0.003)		(0.003)	
$Employees^2$					-2.20e-07		-3.32e-07	
					(1.23e-06)		(1.31e-06)	
Flights within 50 Miles (thousands)					-0.002		-0.002	
					(0.009)		(0.009)	
$Flights^2$					1.21e-06		5.37e-07	
					(0.00002)		(0.00002)	
Const.	-1.553	-4.055	-3.878	-2.084	2.804	-0.381	6.984	4.350
	(0.260)***	(0.384)***	(1.442)***	(1.419)	(2.744)	(1.535)	(2.844)**	(0.798)***
State Dummies	No	No	No	Yes	Yes	Yes	Yes	Yes
NAICS Dummies	None	None	2 Dig.	2 Dig.	2 Dig.	2 Dig.	3 Dig.	3 Dig.
Sales, Employees, & Flights Decile Dummies	No	No	No	No	No	Yes	No	Yes
Observations	1686	1679	1686	1668	1655	1668	1655	1668

D. Results on Fleet Size from the LBO Panel

The bottom panel of Figure 3 in the main text displays the aggregate ratio of jet seats to firm sales in an event-study chart like the top panel. This ratio is roughly flat in the years immediately preceding an LBO, but begins to fall sharply as early as the end of the year in which the LBO occurs. This figure simply presents raw aggregate data, and no correction is made for changes in the composition of the sample from point to point.

Table IA.V presents regressions in which the dependent variable is the firm-level ratio of jet seats to billions of dollars of sales, with firm fixed effects are included in all specifications. Columns 3 and 4 include no observations from 2008 and 2009, while other columns include them. Columns 5 and 6 include the same set of size controls as in Table IV in the text. Inspection of the data reveals that the magnitude of results can be rather sensitive to the inclusion or exclusion of particular firms with a large seats-to-sales ratio prior to their LBO. Columns 2, 4, and 6 present results excluding one such firm.[3]

Estimates of the average reduction in the seats-to-sales ratio by the end of the calendar year after the LBO range from 0.212 in column 4 to 0.405 in column 3. Estimates of the average reduction after four years range from 0.597 in column 6 to 2.109 in column 3. Although large reductions in the ratio of seats-to-sales are evident in the point estimates across all specifications, these estimates are somewhat less often statistically significant than the results in Table IV of the text. Estimated fleet reductions are statistically different from zero at the 5% level or better one, two, and four years after LBO in four of the six columns in the table.

It is also worth noting that the top two rows of the table present weak evidence of a

[3]I refer to the firm as Firm X. Firm X appears in Dealscan as an LBO, and 90% of its shares were purchased by management and a PE fund, but the remaining 10% of shares remained publicly traded. Thus, it should arguably be excluded from the sample because it did not truly go private. The firm had one jet with 11 seats prior to its LBO, and the jet was sold the following year. As the firm is among the smallest firms in the sample with sales around $1 billion (in 2008 dollars), this represents a very large reduction in its seat-to-sales ratio, which can significantly affect overall results.

Table IA.V
Regressions of Ratio of Jet Seats to Billions of Dollars of Sales on Dummies for Years Surrounding an LBO

The dependent variable in all columns is the ratio of the total seat capacity of a firm's jet fleet to its sales in billions of 2008 dollars. The sample consists of a panel of 69 firms that went from public to private in a PE-led LBO between 1992 and 2007, where the firm's sales in the year prior to the LBO were at least $1 billion (in 2008 dollars). All nonmissing observations from three years before each LBO to four years after are included. All specifications include firm fixed effects. Columns 3 and 4 exclude all observations from 2008 and 2009 when the financial crisis and recession could affect results. Standard errors (in parentheses) are clustered by headquarters state. The symbol *** indicates statistical significance at the 1% level, ** at 5%, and * at 10%.

	(1)	(2)	(3)	(4)	(5)	(6)
t = LBOyear - 3	0.158	0.155	0.158	0.151	0.342	0.232
	(0.183)	(0.186)	(0.190)	(0.194)	(0.215)	(0.182)
t = LBOyear - 2	0.148	0.148	0.153	0.151	0.209	0.176
	(0.138)	(0.140)	(0.144)	(0.146)	(0.140)	(0.137)
t = LBOyear	-0.160	-0.162	-0.164	-0.166	-0.166	-0.161
	(0.106)	(0.109)	(0.112)	(0.115)	(0.103)	(0.108)
t = LBOyear + 1	-0.360	-0.255	-0.405	-0.212	-0.323	-0.208
	(0.152)**	(0.092)***	(0.285)	(0.161)	(0.156)**	(0.090)**
t = LBOyear + 2	-0.446	-0.332	-0.833	-0.532	-0.393	-0.285
	(0.186)**	(0.124)***	(0.490)*	(0.330)	(0.159)**	(0.115)**
t = LBOyear + 3	-0.487	-0.253	-1.097	-0.524	-0.474	-0.202
	(0.351)	(0.219)	(0.842)	(0.595)	(0.321)	(0.180)
t = LBOyear + 4	-1.053	-0.707	-2.109	-1.225	-0.910	-0.597
	(0.528)**	(0.337)**	(1.035)**	(0.560)**	(0.425)**	(0.262)**
Mean in LBOyear - 1	1.489	1.382	1.489	1.382	1.489	1.382
Size Controls	No	No	No	No	Yes	Yes
Firm X Included	Yes	No	Yes	No	Yes	No
2008 & 2009 Included	Yes	Yes	No	No	Yes	Yes
Observations	452	444	348	340	452	444
R^2	0.876	0.909	0.866	0.903	0.885	0.913

"pre-trend," that is, a seats-to-sales ratio that was already declining in the year or two prior to LBO. Note, however, that these estimates are not statistically significant. Further, there is no evidence of an important pre-trend in the aggregate data graphed in the bottom panel of Figure 3. There is also no evidence of such a trend in the results in Figure 3 and Table II of the text using the dummy for any jet as a dependent variable. Thus there seems to be little reason to worry that the observed fleet reductions after LBO are driven by trends that began prior to the LBO.

As a preferred estimate of the within-firm effect of an LBO, I take the 0.597 reduction in the ratio of seats to sales four years after LBO from column 6. This is the smallest estimated effect four years after LBO, and it is near the mean of all estimates one to four years after LBO. But, again, this choice is somewhat arbitrary given the wide range of estimates in the table. On a base of the average ratio of 1.382 in the year prior to LBO, this figure represents a post-LBO fleet reduction of 43%.

E. IPOs

It is also natural to wonder whether firms increase their jet fleets after being freed from PE ownership in a public offering. Leslie and Oyer (2009) construct a sample of 144 firms that exited PE ownership in an initial public offering between 1996 and 2005. This sample includes 25 U.S. firms with sales greater than $1 billion in 2008 dollars in the year prior to their IPO. One year prior to their IPOs, three of these 25 firms had at least one jet. Two years after their IPOs, seven had at least one jet. This change from three to seven represents an increase in the fraction of firms with any jet of about 16 percentage points, a bit above estimates of the effects of PE ownership from the cross-section and the LBO panel. However, regression results show that this change is not statistically different from zero at conventional levels in regressions like those from the LBO panel in Table IV. Changes in the average ratio of seats to sales before and after IPOs are also small and statistically insignificant. Thus, there is some evidence that firms expand their jet fleets when the constraints of PE ownership are lifted, but these results can only be considered suggestive due to the small number of firms involved.

F. Leverage vs. Monitoring

The text of the paper refers to results from a sample of all public firms with sales greater than $1 billion in 2008 dollars that increased their ratios of total debt to assets and long-term debt to assets by 20 percentage points or more within one year between 1992 and 2008.

Figure IA.2. Jet fleets in years surrounding large leverage increases. This figure plots the aggregate ratio of jet seats to billions of dollars of sales in a sample of 39 public firms that increased their total and long-term leverage ratios by 20 percentage points or more in a single year.

To isolate firms in which leverage was increased without other major changes to the firm's structure, the sample also excludes any firms that experienced an increase or decrease in total assets of more than 10% in the same year as the leverage increase.

Figure IA.2 and Table IA.VI of this appendix present results from this sample of large leverage increases that are constructed just like the results on LBOs in Figure 3 of the text and Table IA.V. In Figure IA.2 we see that the ratio of seats to sales in the sample of leverage increases fell a bit in the year of the leverage increase, but stayed within its pre-leverage-increase range. In fact, the ratio actually jumps up two years after the leverage increase. Table IA.VI shows that the fleet decline in the year of the leverage increase is statistically significant only at the 10% level, and none of the effects in other years is statistically different

Table IA.VI
Regressions of Ratio of Jet Seats to Billions of Dollars of Sales on Dummies for Years Surrounding a Large Leverage Increase

The dependent variable in all columns is the ratio of the total seat capacity of a firm's jet fleet to its sales in billions of 2008 dollars. The sample consists of a panel of 39 firms that increased their long-term and total debt ratios by 20 percentage points or more in a single year between 1992 and 2008, where the firm's sales in the prior year were at least $1 billion in 2008 dollars. All nonmissing observations from three years before to four years after are included in the regressions. All specifications include firm fixed effects. Columns 3 and 4 exclude all observations from 2008 and 2009 when the financial crisis and recession could affect results. Columns 2, 4, and 6 exclude observations from a single firm that increased its fleet two years after its leverage increase and drives some of the notable spike at that point in Figure IA.2. Standard errors (in parentheses) are clustered by headquarters state. The symbol *** indicates statistical significance at the 1% level, ** at 5%, and * at 10%.

	(1)	(2)	(3)	(4)	(5)	(6)
t = LBOyear - 3	-0.139	-0.139	-0.140	-0.139	-0.169	-0.201
	(0.144)	(0.150)	(0.143)	(0.149)	(0.111)	(0.113)*
t = LBOyear - 2	-0.107	-0.108	-0.107	-0.108	-0.116	-0.124
	(0.114)	(0.117)	(0.116)	(0.119)	(0.089)	(0.097)
t = LBOyear	-0.271	-0.278	-0.271	-0.278	-0.280	-0.284
	(0.152)*	(0.155)*	(0.155)*	(0.158)*	(0.144)*	(0.150)*
t = LBOyear + 1	-0.090	-0.091	0.112	0.119	-0.170	-0.182
	(0.239)	(0.246)	(0.281)	(0.292)	(0.168)	(0.176)
t = LBOyear + 2	0.061	0.024	0.454	0.413	-0.076	-0.122
	(0.269)	(0.274)	(0.369)	(0.389)	(0.203)	(0.209)
t = LBOyear + 3	0.057	0.020	0.223	0.175	-0.044	-0.080
	(0.153)	(0.155)	(0.199)	(0.208)	(0.237)	(0.241)
t = LBOyear + 4	-0.154	-0.212	-0.128	-0.209	-0.473	-0.541
	(0.302)	(0.317)	(0.399)	(0.428)	(0.371)	(0.373)
Constant	2.126	2.158	1.927	1.956	3.844	3.801
	(0.110)***	(0.111)***	(0.108)***	(0.111)***	(1.312)***	(1.279)***
Mean in LBOyear - 1	2.138	2.167	2.138	2.167	2.138	2.167
Size Controls	No	No	No	No	Yes	Yes
Firm Z Included	Yes	No	Yes	No	Yes	No
2008 & 2009 Included	Yes	Yes	No	No	Yes	Yes
Observations	244	236	211	203	240	232
R^2	0.893	0.893	0.896	0.896	0.901	0.903

from zero.

The text also refers to results from a sample of 213 public firm mergers between 1992 and 2010, where both the target and the acquirer had greater than $1 billion 2008 dollars of sales in the year before the merger. I identify all jets that were operated by either the target or the acquirer in the year before the merger, and test whether those jets remained with the combined entity in the years following the merger. One might be concerned that some artifact of the treatment of the merger in the Jetnet data would bias me away from finding the target's jets. I took two important steps to make sure that this matching was not biased against finding the target's jets in subsequent years. First, I considered jets to match with the combined entity in years after the merger if they matched with any one of the target's name, the acquirer's name, or the name of the combined entity. Second, I hand-investigated all target jets that were identified as subsequently dropped by the combined entity, and I visually inspected the list of all subsequent operators of each jet to ensure that the acquirer or new entity was not simply showing up under an alternate name. This procedure did indeed turn up a handful of jets that would have been improperly identified as no longer operated after the merger. As I did *not* perform this same check for the acquirer's jets (due to the amount of labor that would be required), I bias myself towards finding *more* of the targets' jets than of the acquirers' jets.

Figure 4 in the text plots the probability that a jet operated by either a target or acquirer in the year before a merger is still operated by the combined entity in the years following the merger, and Table IA.VII below presents regression results that make the same point. The bottom half of the table presents estimated coefficients on simple dummies for years surrounding the merger. These estimates show that jets operated by the acquiring firm in the year before the merger are less likely to be operated by the same firm as one moves further from the merger in either direction. The top half of the table presents coefficients on these same dummy variables, interacted with a dummy for the target firms' jets. These coefficients thus measure the difference between targets' and acquirers' jets in terms of the probability that jets are operated by the same firm in years surrounding the merger.[4] The top two rows of the table show that there is no significant difference between targets' and

[4]Note that there is no target firm main effect in the regression, because both the target and the acquirer have the dependent variable equal to one in the omitted year. There is a constant (equal to exactly one) in the regresssion, but it is not reported in the table.

Table IA.VII
Regressions of Dummy for Still Operating Jet Operated in Year Before Merger on Dummies for Years Surrounding a Public Firm Merger

The dependent variable is a dummy indicating that a given jet is operated by the firms involved in a merger in years surrounding the merger. The sample consists of jets that were operated by either a target or an acquirer in the year before the merger, where both the target and acquirer were standalone US public firms with sales greater than $1 billion in 2008 dollars. All observations from three years before each merger to four years after are included. Columns 2 and 4 exclude all observations from 2008 and 2009 when the financial crisis and recession could affect results. Columns 3 and 4 include transformed year effects; columns 1 and 2 do not. Standard errors (in parentheses) are clustered by headquarters state. The symbol *** indicates statistical significance at the 1% level, ** at 5%, and * at 10%.

	(1)	(2)	(3)	(4)
Target × (t = MergerYear - 3)	-0.025	-0.025	-0.027	-0.027
	(0.053)	(0.053)	(0.054)	(0.053)
Target × (t = MergerYear - 2)	-0.012	-0.012	-0.016	-0.015
	(0.052)	(0.052)	(0.053)	(0.053)
Target × (t = MergerYear)	-0.149	-0.149	-0.148	-0.148
	(0.048)***	(0.048)***	(0.050)***	(0.050)***
Target × (t = MergerYear + 1)	-0.225	-0.246	-0.221	-0.242
	(0.048)***	(0.050)***	(0.050)***	(0.050)***
Target × (t = MergerYear + 2)	-0.222	-0.266	-0.217	-0.261
	(0.048)***	(0.054)***	(0.048)***	(0.055)***
Target × (t = MergerYear + 3)	-0.198	-0.243	-0.197	-0.246
	(0.049)***	(0.068)***	(0.051)***	(0.070)***
Target × (t = MergerYear + 4)	-0.241	-0.269	-0.239	-0.268
	(0.043)***	(0.057)***	(0.041)***	(0.056)***
t = MergerYear - 3	-0.373	-0.373	-0.365	-0.366
	(0.032)***	(0.032)***	(0.034)***	(0.033)***
t = MergerYear - 2	-0.206	-0.206	-0.198	-0.199
	(0.027)***	(0.027)***	(0.026)***	(0.025)***
t = MergerYear	-0.140	-0.140	-0.135	-0.135
	(0.021)***	(0.021)***	(0.019)***	(0.019)***
t = MergerYear + 1	-0.268	-0.275	-0.262	-0.272
	(0.024)***	(0.026)***	(0.023)***	(0.023)***
t = MergerYear + 2	-0.387	-0.377	-0.382	-0.377
	(0.031)***	(0.030)***	(0.027)***	(0.029)***
t = MergerYear + 3	-0.503	-0.479	-0.503	-0.486
	(0.036)***	(0.039)***	(0.034)***	(0.037)***
t = MergerYear + 4	-0.596	-0.589	-0.596	-0.591
	(0.041)***	(0.049)***	(0.038)***	(0.043)***
2008 & 2009 Included	Yes	No	Yes	No
Year Effects	No	No	Yes	Yes
Observations	5136	4705	5136	4705
R^2	0.184	0.184	0.187	0.187

acquirers' jets in the probability that the jets were operated by the same firm two or three years *before* the merger. However, by the end of the year of the merger, we see that target firms' jets are about 15 percentage points less likely to be retained than acquirers' jets. From two to four years after the merger, this discrepancy is between roughly 20 and 25 percentage points. All of these effects are statistically significant at the 1% level.

G. Quantile Regressions

Figure IA.3 describes the differences in the distribution of jet fleets between PE-owned and other firms by graphing the percentiles of the residual fleet size distribution for different kinds of firms. That is, a fleet size measure is regressed on a set of controls (not including PE or private ownership), and the values of the residuals at each percentile of the distribution of residuals are displayed, where the percentiles are calculated separately by ownership group. The top panel performs this exercise using the ratio of seats to sales; the bottom uses the logarithm of seats. One sees that there is relatively little difference between PE-owned and other firms through about the 65th percentile of the residual distribution. As we move further up into the distribution, however, the gap between PE-owned and other firms widens considerably.

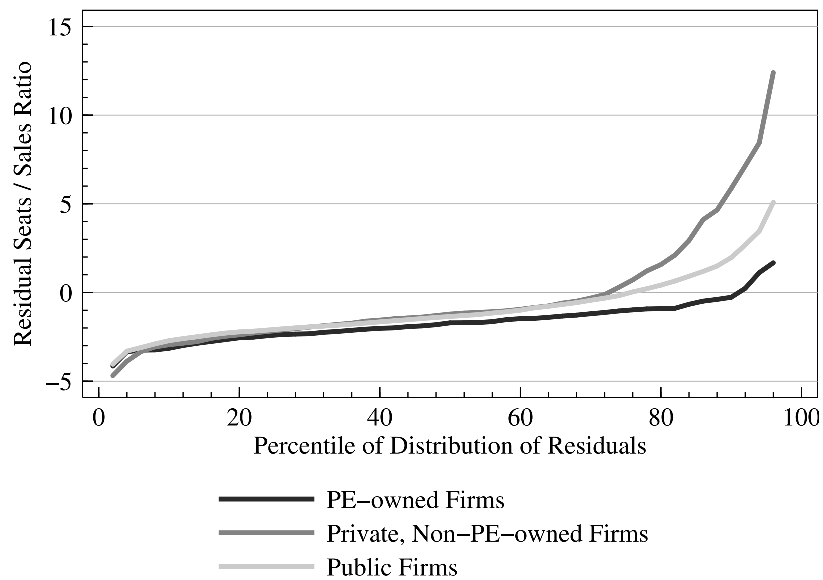
Recall that the quantile regression estimator for the τth quantile is

$$\hat{\beta}(\tau) = \text{argmin}_{\beta} \sum_{i=1}^{N} \rho_{\tau}(Y_i - X_i'\beta),$$

where ρ_{τ} is the "check function,"

$$\rho_{\tau}(x) = (\tau - \mathbf{1}(x \leq 0))x.$$

Essentially, coefficients on the PE dummy in these quantile regressions measure the difference



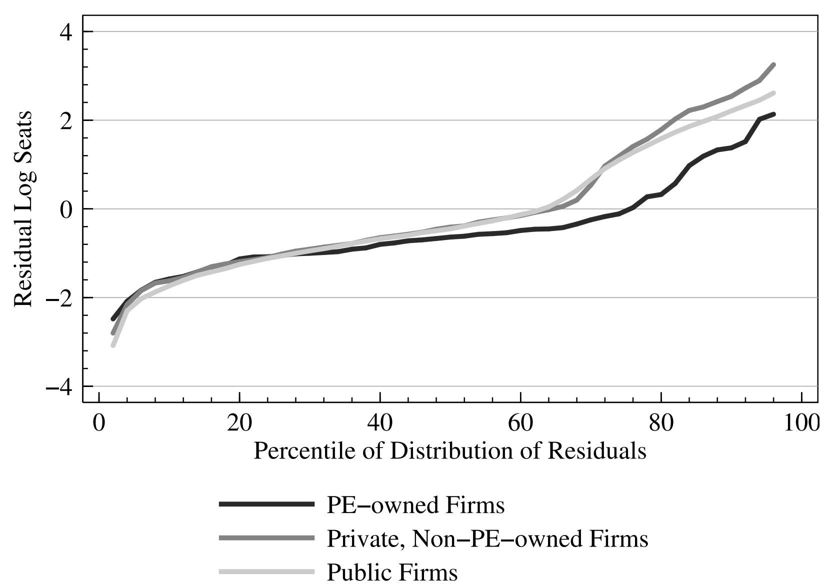


Figure IA.3. Percentiles of residual fleet size by ownership. These figures describe the distribution of the residuals from an OLS regression of fleet size measures on controls for size and location. The top panel corresponds to a regression of the ratio of seats to sales on logarithms and quadratic polynomials in sales, employees, and the number of scheduled flights departing from within 50 miles of headquarters. The lower panel corresponds to a regression of log seats on the same set of controls, where the dependent variable for observations with zero jets is set equal to the logarithm of the smallest nonzero fleet (-0.827, representing a 1/16 share of a seven-seat jet).

between the conditional percentiles of the distributions of publicly traded and PE-owned firms, much like the gap between lines in the figure. The conditioning on size and location is determined by the quantile regression function, however, rather than the OLS regressions used to make the figure.

G.1. Censored Quantile Regressions

The conclusions from the quantile results discussed in the paper are potentially sensitive to the censoring concerns discussed previously. Think again of the jet fleet variables that I observe as a proxy for total expenditure on executive travel, where this proxy is censored for firms that do not spend enough to have a jet. It is possible that PE ownership causes similar large reductions in executive travel expenditures throughout the conditional distribution of expenditures. These reductions would be invisible in the jets data for the many firms whose level of expenditure is not high enough to include a jet. The quantile results just presented would then give the misleading impression that the effect of PE ownership on travel expenditures is concentrated among firms at the top of the distribution, when in fact it is widespread.

The censored quantile regression estimator was developed by Powell (1986) to deal with situations like this.[5] It allows one to estimate effects of covariates on quantiles of the latent variable of interest without strong assumptions about the distribution of error terms like those required by the Tobit. Computing the Powell estimator is often difficult, because the minimand is not a smooth function of the covariates. Chernozhukov and Hong (2002) propose

[5]The Powell censored quantile estimator for the τth quantile is

$$\hat{\beta}(\tau) = \text{argmin}_{\beta} \sum_{i=1}^{N} \rho_{\tau}(Y_i - \max(X_i'\beta, C)),$$

where C is the censoring point, that is, the smallest value that is not censored. Note that the estimator does require the assumption that the relevant quantile be a linear function of covariates, which is similar to the Tobit model's assumption about the mean.

a simple and robust algorithm that produces an efficient estimator and is also intuitively appealing. Their algorithm uses independent variables to identify observations that are unlikely to be censored at the quantile at which one wants to estimate coefficients. A traditional quantile regression is then run using only these observations.

I implement the Chernozhukov and Hong (2002) algorithm as follows:

Step 1. Estimate a probit model with the dummy for any jet as the dependent variable and calculate the predicted probability for each observation. Select the sample for which the predicted probability p is greater than $1 - \tau + c$, where c is chosen to eliminate 10% of the observations with $p > 1 - \tau$.

Step 2. Compute the standard quantile regression estimator $\hat{\beta}_0(\tau)$ using this selected sample. Select the sample for which the predicted values $X_i'\hat{\beta}_0 > C + \delta$, where δ is chosen to eliminate 5% of the observations with $X_i'\hat{\beta}_0 > C$. Intuitively, this step selects observations with covariate values that make the observations unlikely to be censored at the quantile of interest.

Step 3. Compute the standard quantile regression estimator using this new selected sample.

In practice, it was difficult to find a set of control variables that perform well in identifying observations likely to be censored at the end of Step 2. One might worry that too many censored firms are still entering the sample, particularly when trying to estimate effects at the lower quantiles. However, raising the constant δ has little effect on the estimates in Step 3. That is, further limiting the sample to smaller and smaller subsets of firms that are more and more likely to have a jet has little effect on the estimates at the lower quantiles.

Panel B of Table VI in the text presents results from this censored quantile estimator of the same specifications as those in Panel A. For example, the result in column 1 tells us that the effect of PE ownership on the 50th percentile of the conditional seats-to-sales distribution is only -0.06, even after restricting the sample to a set of firms that are large or remote enough that the firm at the 50th percentile is still likely to have a jet. Point estimates at the 50th, 60th, and 70th quantiles are little changed from the traditional quantile regression estimates in Panel A. Standard errors at these lower quantiles, however, increase considerably and can no longer rule out effects as large as the mean effect in Table III.[6] Thus, the point estimates from censored quantile regressions provide little reason to be concerned that the small effect of PE ownership on travel expenditures observed in lower quantiles is driven by censoring, but the standard errors do not permit one to be certain.

H. Interpreting Results on Private, non-PE Firms

This paper set out to measure differences in jet fleets between publicly traded and PE-owned firms. In estimating this difference, another feature of the data became apparent: some private—but not PE-owned—firms also have large jet fleets by the standards of PE-owned firms. Figure 2 showed that the private, non-PE firms with the largest ratios of seats to sales are concentrated among the smallest firms in the sample. This section of the appendix discusses these results in more detail, focusing on their relevance for interpreting the difference between public and PE-owned firms as evidence of agency problems.

Two explanations for the difference in fleets between PE-owned and other private firms come immediately to mind. The first is the same agency problem that motivated this paper. Although few hard data on ownership shares are available, one suspects that many private firms are owned and run largely by founders and their descendants. Although managerial ownership mitigates the agency problem in the setting of Jensen and Meckling (1976), others have pointed out that it could lead to managers with too few checks on their control over the firm—"entrenchment" in the words of Morck, Shleifer, and Vishny (1988).[7] In another early

[6] In both panels, standard errors are computed using a block bootstrap procedure that allows for error correlation within states.

[7] A relevant example comes from the Berwind Group, a collection of family-owned firms active in several industries. In 2000, David Berwind sued his brother, Charles Berwind, Jr., over a variety of alleged misdeeds in his management of the firm founded by their father. The allegations essentially amounted to tunneling. Charles was the Chairman of the Berwind Group, which was the controlling shareholder of Berwind Pharmaceuticals, in which David had a large share. David said that Charles used his influence to tunnel funds from Berwind Pharmaceuticals to himself or the Berwind Group in various ways. Among them was the location of aircraft used mainly by Charles and his family in Berwind Pharmaceuticals, where they did little

paper, McConnell and Servaes (1990), find that public firm valuation is decreasing in insider ownership once that ownership exceeds around 50%. Several recent papers find that descendant CEOs destroy value in U.S. public firms (Villalonga and Amit (2006), Perez-Gonzalez (2006)). Outside of the U.S., the problem of entrenched owner-managers is thought to be particularly severe. As La Porta, Lopez-de-Silanes, and Shleifer (1998) put it, "the principal agency problem in large corporations around the world is that of restricting expropriation of minority shareholders by controlling shareholders." My results may point to similar problems in privately held firms in the U.S. In fact, these private firms are also frequent targets of PE acquisitions (Kaplan and Stromberg (2009)).[8]

A second potential explanation for the large fleets observed in some private firms is that the owner-managers in these firms are very wealthy and willing to spend their own money on jets or other nonpecuniary benefits. Given that depreciation and debt tax shields are useful to firms and not to individuals, wealthy owners willing to purchase jets can minimize taxes when their firms are the jets' legal owners. An owner-manager of a firm with more than a billion dollars in annual revenues is likely to have a net worth in the hundreds of millions of dollars or more. Many appear in the Forbes annual list of the world's billionaires.[9] It thus seems likely that wealthy owner-managers of private firms are simply choosing to consume some of their wealth in the form of jets. If they own their firms outright, the presence of large jet fleets need not be an indication of agency problems.

The ranks of public firm executives might also include some very wealthy owner-managers, potentially imperiling the interpretation of the presence of large jet fleets in these firms as

to make money for David ("Berwind family in 83 million dollar fight," *Philadelphia Business Journal*, Dec. 18, 2000.).

[8]Empirical work on performance and productivity in private, non-PE U.S. firms appears to be scarce. Maksimovic, Phillips, and Yang (2010) find that high-productivity firms are more likely to go public than low-productivity firms. Sheen (2009) finds, however, that private, non-PE chemical producers time their investments better than public firms (although not as well as PE-owned firms).

[9]Some examples include Archie "Red" Emmerson (founder of Sierra Pacific Industries), Alexander Spanos (founder of A.G. Spanos Construction), and James Goodnight (founder of SAS Institute).

evidence of agency problems. Of course, the public shareholders in these firms would prefer that the firms not be run solely for the benefit of the managers, as fully manager-owned firms might be. It is feasible, however, that even if their total compensation packages are appropriate, these wealthy managers might choose to receive more compensation in the form of jets and less in other forms. If there happen to be more wealthy managers in public firms than in PE-owned firms, I would improperly be interpreting differences between PE and public firms in the cross-section as evidence of agency problems.

I discuss two pieces of evidence suggesting that managerial wealth effects do not drive the results presented thus far. The first involves the now-familiar appeal to results from the LBO panel. Even if wealthy CEOs' tendencies to favor compensation in the form of jets biased results in the cross-section, we still observe declines in jet fleets within firms when they are taken private. One could then object that these declines might be driven simply by the replacement of wealthy owner-managers with poorer professional managers who prefer to receive more non-jet compensation. Theory suggests that if the average LBO involved a transition from wealthy to poor management with no change in governance, we should see guaranteed pay rise because poorer CEOs are more risk averse. Indeed, Becker (2006) finds evidence of a positive relationship between performance sensitivity and CEO wealth using data from Sweden, where wealth can be observed. Leslie and Oyer (2009) find, however, that executives' base salaries fall and performance-sensitive compensation rises in PE-owned firms. Thus, the evidence from Leslie and Oyer (2009) is inconsistent with the notion that changes in executive compensation around LBOs are driven by a transition from wealthy to poor managers with no change in governance.

Second, the argument that wealthy CEOs in public firms substitute jets for other forms of compensation has a clear, testable implication for the cross-section of public firms: all else equal, firms with larger jet fleets should provide lower non-jet compensation. Rajan and Wulf (2006) find the opposite in their sample of public firms: firms that pay more in salary and bonus are more likely to offer their executives jet access, even when including controls for size, industry, and performance. I also test this hypothesis using the Compustat Executive Compensation data merged with the data on public firms used thus far in the paper. Table IA.VIII presents regressions of the logarithm of total executive base salaries on jet use variables, the various sets of controls used in this paper, and performance measures in the form of return on assets and Tobin's Q. Results show that firms that operate a jet pay their executives higher base salaries, even after including these controls. This result is often highly statistically significant, and it is robust to a wide variety of specification changes.[10] Results using the seats-to-sales ratio are less often statistically significant, but they still provide no evidence that public firms with jets or with larger fleets provide less compensation in other forms, as the substitution hypothesis would require.

[10]These include winsorizing the measures of ROA and Q at various levels, including flexible polynomials in these variables, using bonus or total compensation in place of salary, and using compensation variables for the CEO only instead of the top five executives.

Table IA.VIII
OLS Regressions of Log Salary on Jet Variables and Controls

The dependent variable in all columns is the logarithm of the sum of the base salaries paid to the five executives listed in Execucomp. The even-numbered columns include state and two-digit NAICS industry dummies; the odd-numbered columns do not. The sample consists of all public firms from the sample in this paper with nonmissing Execucomp data. Q and ROA are winsorized at the 5th and 95th percentiles. Standard errors are clustered by headquarters state. The symbol *** indicates statistical significance at the 1% level, ** at 5%, and * at 10%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dummy for any jet	0.094	0.101			0.090	0.097		
	(0.017)***	(0.019)***			(0.017)***	(0.020)***		
Seats / sales			0.007	0.008			0.007	0.008
			(0.002)***	(0.003)***			(0.002)***	(0.003)***
Tobin's Q	0.035	0.024	0.030	0.019	0.243	0.190	0.260	0.206
	(0.026)	(0.025)	(0.026)	(0.026)	(0.100)**	(0.088)**	(0.100)***	(0.087)**
Q^2					-0.045	-0.036	-0.049	-0.041
					(0.024)*	(0.021)*	(0.024)**	(0.021)*
Return on Assets	-0.651	-0.571	-0.602	-0.549	-1.386	-1.083	-1.353	-1.070
	(0.168)***	(0.174)***	(0.174)***	(0.182)***	(0.556)**	(0.536)**	(0.578)**	(0.569)*
RoA^2					2.112	1.488	2.120	1.489
					(1.720)	(1.582)	(1.804)	(1.723)
Log Sales	0.135	0.157	0.150	0.173	0.166	0.202	0.184	0.222
	(0.012)***	(0.011)***	(0.011)***	(0.010)***	(0.018)***	(0.020)***	(0.017)***	(0.018)***
Log Employees	0.034	0.008	0.036	0.011	0.040	0.011	0.041	0.011
	(0.009)***	(0.012)	(0.009)***	(0.012)	(0.010)***	(0.014)	(0.010)***	(0.014)
Log Flights within 50 Miles	0.034	0.020	0.031	0.018	0.033	0.025	0.031	0.022
	(0.011)***	(0.010)**	(0.011)***	(0.010)*	(0.017)**	(0.018)	(0.016)*	(0.018)
Sales (billions)					-0.003	-0.004	-0.003	-0.004
					(0.002)*	(0.001)**	(0.002)*	(0.002)**
$Sales^2$					7.07e-06	8.23e-06	7.53e-06	8.82e-06
					(4.00e-06)*	(3.58e-06)**	(4.19e-06)*	(3.80e-06)**
Employees (thousands)					-0.0004	-0.0004	-0.0003	-0.0003
					(0.0004)	(0.0003)	(0.0003)	(0.0003)
$Employees^2$					7.22e-08	1.24e-07	4.98e-08	9.28e-08
					(1.77e-07)	(1.59e-07)	(1.68e-07)	(1.52e-07)
Flights within 50 Miles					-0.0007	-0.0001	-0.0007	-0.00003
					(0.0005)	(0.0006)	(0.0005)	(0.0006)
$Flights^2$					1.81e-06	1.32e-07	1.75e-06	-8.45e-08
					(1.01e-06)*	(1.12e-06)	(9.85e-07)*	(1.15e-06)
Const.	7.267	7.493	7.305	7.538	7.123	7.291	7.137	7.329
	(0.122)***	(0.106)***	(0.116)***	(0.109)***	(0.166)***	(0.187)***	(0.152)***	(0.184)***
Observations	930	930	929	929	930	930	929	929
R^2	0.406	0.519	0.396	0.51	0.431	0.533	0.422	0.526

Thus, the data provide no support for the idea that large average jet fleets observed in public firms result from managers choosing to substitute jet use for other forms of compensation. Therefore, I see little reason to worry that the large jet fleets observed in some private, non-PE firms should alter our interpretation of the difference between public and PE-owned firms.

I. Other Alternative Explanations

Finally, I discuss two alternative explanations for my results that might still concern some readers. The first is that executives and board members in public firms must spend more time traveling to meet with each other or with outsiders than they do in PE-owned firms. The second is that the PE-owned firms substitute jets owned by PE professionals for those operated directly by the firm.

Note first that both of these explanations are made less compelling by the observed concentration of public firms' jets in a minority of firms with large fleets. Under the most straightforward version of both alternatives, one would expect all observably similar public firms to have similar jet fleets, albeit larger than those in PE-owned firms. Instead we see jets concentrated in a minority of public firms for no observable reasons. Further, the first alternative also appears less compelling in light of my results on private but non-PE-owned firms. Given that some of these firms also have large fleets, it is unlikely that any requirements specific to public status are driving results.

Acharya, Kehoe, and Reyner (2009) present data from interviews with public and PE executives and board members in the UK that are useful in further assessing the first claim—that public firm executives or board members must spend more time traveling than their counterparts in private firms. The paper suggests that meetings with outside investors could consume up to 10% of a public board's time, although the source of this figure is unclear. They also find that PE-owned firms average smaller boards than public firms, although they do not appear to be comparing firms of similar size. Gertner and Kaplan (1996) compare boards in PE-owned firms undergoing IPOs with a size and industry-matched sample of public firms. They find that public firms average 9.95 board members and the PE-owned firms 8.19, a reduction of 18%. It is thus true that larger boards and meetings with outside investors could tend to increase jet use in public firms.

It appears that these effects are vastly outweighed, however, by the much larger amount of time that PE board members invest in monitoring their firms. Acharya, Kehoe, and Reyner (2009) report that non-executive, non-chairman board members in their sample spent an average of 19 days per year on their public board memberships and 54 days per year on their PE board memberships, an increase of 284%. It is not clear that either of these figures represent full days rather than partial days or exactly how much of this time spent would require travel. Nonetheless, it seems that the total amount of time spent in interactions among combinations of executives, board members, and investors almost certainly *increases* in PE-owned firms.

One could also wonder, however, if board members in PE-owned firms are wealthy enough to have jets themselves, reducing the need for firms to operate jets. Quantifying the effect of board changes on firms' jet needs is difficult, but available evidence suggests that any effect is unlikely to be large enough to explain the results in this paper. Previous authors have documented that PE funds replace some, but not all, outside directors with fund employees, who are potentially quite wealthy. Gertner and Kaplan (1996) find that 38% of board members in PE-owned firms in their sample were fund employees; Acharya, Kehoe, and Reyner (2009) find 23% in their sample. I searched the jet data for the names of a selection of individual PE employees based in the U.S. who are listed on their funds' websites as serving on portfolio company boards.[11] Less than 20% appeared in any form in the data,

[11]See, for example, http://www.kkr.com/team/theteam.cfm. One might worry that additional individuals may own jets through opaquely named firms in such a way that their names do not appear in the data. It is true that many jets are owned by shell companies with opaque names (e.g., JJSA Aviation II LLC), but

and most of these were as owners of a fraction of a jet. Many of the PE employees that serve on boards come from the ranks of professionals more junior than the founders or partners. These employees may be less likely to own jets than their wealthier senior colleagues who come to mind when one first thinks of privat equity.

I also investigated the boards of some comparably-sized public firms, including the pre-buyout boards of some recent LBOs.[12] These boards also include some wealthy members that own jets, and only somewhat fewer than among the PE professionals. They also include many active CEOs who may travel on their firms' jets.[13] Overall, there is little evidence that the PE professionals who serve on boards are substantially more likely to have access to outside jets than the current and retired executives, bankers, and investors that often populate the boards of public firms. It thus seems unlikely that a shift towards wealthier board members in PE-owned firms could explain more than a modest portion of the observed reductions in fleet size.[14]

the data vendor appears to do an excellent job in providing the names of individuals associated with these firms. I investigated the ownership of a random selection of 20 Gulfstream G-IV SP jets (the most popular model among public firms in the data set) and 20 Bombardier Challenger 604 jets (a popular model among smaller firms and wealthy individuals). Of these 40 random selections from the universe of jets, only two appeared in the data with no reference to entities that could be easily identified through a Google search. That is, 95% of jets were associated with firms that have operational websites or with names that produce Google search results related to wealthy individuals. Thus, it seems unlikely that there are a large number of PE employees who own jets but do not appear in the data.

[12]For example, consider HCA Inc., one of the largest LBOs of all time. Prior to the LBO, the HCA board included three academics, two physicians, a senior investment banker, current and former CEOs from three Fortune 100 companies, a former Managing Director (CEO) of McKinsey, and the host of a nationally syndicated court television show. All of these board members have since been replaced with employees of Bain Capital, KKR, and Bank of America. Both the pre- and post-buyout boards include individuals who appear in the data as jet operators.

[13]In fact, some firms report that they require their executives to travel only on company aircraft, ostensibly for liability reasons.

[14]Even if some of the observed differences in jet fleets between public and PE-owned firms are driven by changes in board membership, it seems that implications for governance in public firms would change only subtly. Under this view, the large fleets observed in a minority of public firms need not indicate that managers themselves overconsume jet travel. Instead, the fleets would merely be symptomatic of boards that fail to maximize value in ways that might be reflected in many aspects of firm behavior. Shareholders in these firms would still stand to benefit if their boards became more like those in PE-owned firms or those in the majority of public firms that do not have large fleets.

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