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

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January 21, 2011

Abstract

This paper uses rich, new 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 jet fleets at least 40% smaller than observably similar publicly-traded firms. Similar fleet reductions are observed within firms that go private in leveraged buyouts. I discuss assumptions under which 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 a leveraged buyout. Both censored and standard quantile regressions suggest that results at the mean are driven by firms in the upper 30% of the conditional jet distribution. Results thus suggest that executives in a substantial minority of public firms enjoy more generous perquisites than they would if subject to the pressures of private equity ownership.

Keywords: Agency problem, corporate governance, executive compensation, private equity, corporate jet.
JEL: G34, G39, J33, J44.

*Email: jesse.j.edgerton@frb.gov. Any views expressed here are those of the author and need not represent the views of the Federal Reserve Board or its staff. I thank Bo Becker, Alex Edmans, Xavier Gabaix, Jerry Hausman, Erik Heitfield, George Korniotis, Amanda Kowalski, David Lebow, Josh Lerner, Raven Saks Molloy, Stew Myers, Rick Ruback, Johannes Spinnewijn, Julie Wulf, and seminar participants at Dartmouth Tuck, Harvard Business School, Columbia Business School, Duke Fuqua, the NBER Summer Institute and the AFA Annual Meeting for helpful comments or conversations. I thank Alejandro Companioni for excellent research assistance.

1 Introduction

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 is devoted to their study. These efforts to mitigate agency problems appear to have been particularly successful in the United States. The U.S. scores well on international measures of the quality of corporate governance, and U.S. capital markets are exceptionally deep and vibrant when compared to those in many countries.

Nonetheless, there remains considerable debate as to whether U.S. firms or governments should take further action to protect outside investors from self-interested managers. For example, 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]), while others argue that observed compensation arrangements could actually represent optimal contracts negotiated at arm's length between firms and valuable executive talent (Edmans, Gabaix, and Landier [2009], Kaplan and Rauh [2010]).

Beyond this academic debate, policymakers, investors, and business groups continually propose changes to related policies. In the early 2000s, fraud and abuse at firms like Enron and Worldcom inspired a tightening of governance requirements in the form of the Sarbanes-Oxley Act and more stringent exchange listing standards. Some then argued that these requirements imposed unneccessary compliance costs on firms. Since the financial crisis of fall 2008, the debate has shifted again. SEC and administration officials have called for mandatory shareholder proxy access and "say on pay" rules, the U.S. government has imposed pay restrictions on the firms in which it holds an equity stake, and private shareholders at some firms have pushed harder to limit compensation. Business groups typically oppose proposals like these on the argument that their costs would outweigh any benefits.

This paper seeks to inform the debate on the merits of more forceful public or private efforts to mitigate agency problems in publicly traded firms in the United States. I provide new evidence on the severity of agency problems 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. 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 overuse corporate aircraft 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. These amounts are relatively small, however, 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 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, impose changes in firm operations or incentives, and then sell their shares to other firms or to public shareholders after some period, often several years.[1] Three components are generally identified as key to the PE funds' approach to managing firms: highly performance-sensitive managerial compensation, highly levered financing, 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.

Most evidence suggests that PE ownership succeeds in improving efficiency in portfolio companies. Acharya and Kehoe [2008] observe that "[o]verall, the literature suggests that buyouts do create value through operating improvements, in both US and UK markets,

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

during both the recent and the 1980 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 will 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.

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 show that publicly traded firms operate larger jet fleets than observably similar PE-owned firms. In a sample of all public and private U.S. firms with 2008 sales greater than one billion dollars, I find that PE-owned firms operate significantly smaller jet fleets, even when controlling for size, industry, and location in a variety of flexible ways. Estimates suggest that PE-owned firms are at least 25 percent less likely to operate a jet. Conditional on operating at least one jet, they have smaller fleets as measured by total passenger seating capacity. Overall, PE-owned firms average a ratio of jet seats to firm sales more than 40 percent lower than observably similar public firms. Although this paper focuses on the public versus PE-owned comparison, there are also many firms in the cross-section that are private, but not PE-owned. In fact, these private firms average jet fleets at least as large as those in comparable public firms, and I discuss potential implications of this result in detail.

There are a number of reasons one might be skeptical of the comparison of public and PE-owned firms based on cross-sectional regressions. For example, there is only a sparse set of covariates available for private firms in the sample, so one could worry that results might be biased by the omission of variables that are correlated with PE ownership. I thus also examine changes in jet fleets within firms that were taken private by a PE fund in a leveraged buyout (LBO). I assemble a panel of 69 public-to-private buyouts of large, standalone firms that went private between 1992 and 2008. I find that buyout targets are about 32% less likely to have a jet in the three years after their LBO than in the year before, even after controlling for changes in firm size following the buyout. Their average seats-to-sales ratio falls by more than 40% over the same period. As these changes occur within individual firms, they are unlikely to be driven by omitted firm characteristics.

Of course, the selection of firms that are taken private in LBOs is not random. I discuss the likely relationship between excessive jet use and selection into PE ownership in a simple model that incorporates elements of the heterogeneous treatment effects framework of Rubin [1974] or Angrist, Imbens, and Rubin [1996]. Under the assumption that private equity ownership eliminates inefficient jet use, estimates from the cross-section and the LBO panel provide useful bounds on the average treatment effect across all firms of an LBO, even in the presence of this selection problem. Estimates of within-firm changes from LBOs provide an upper bound, while (unbiased) across-firm estimates from the cross-section would provide a lower bound.

With this model in mind, 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, which 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. 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. I favor the second interpretation, as results from the cross-section are quite stable across a wide variety of approaches to estimation. I thus conclude that the average treatment effect of PE ownership is at least a 40% reduction in jet fleet size.

To provide further insight on the nature of the agency problems driving these results, I describe the cross-sectional distributions of jet fleets in public and private firms using quantile regressions. Results indicate that effects of PE ownership on average jet fleets are driven by firms in the top 30% of the conditional jet fleet distribution. That is, roughly 70% of public firms have jet fleets comparable to observably similar PE-owned firms.

One could worry that the large fraction of firms with zero jets makes these results misleading, and I suggest that the censored quantile regression model is potentially useful in this context. I use the censored quantile algorithm of Chernozhukov and Hong [2002] to select samples of firms that are likely to have a jet at any given quantile of interest. Even when using these selected samples, the effects of PE ownership on jet fleets are still concentrated in the top few deciles of the jet distribution. Overall, results thus suggest that the kind of agency problems that manifest themselves in large jet fleets are far from ubiquitous in public firms. Neither, however, are they limited to a very small number of bad apples.

The next section of the paper reviews related literature in more detail. Section 3 presents a simple heterogeneous treatment effects model of selection into PE ownership. Section 4 describes the data on firms and jet fleets. Section 5 presents results and discusses a variety of alternative explanations. Section 6 concludes.

2 Literature

The discussion of the agency problem in Jensen and Meckling [1976] has inspired three decades of research in corporate finance. I highlight several active areas of research for which this paper's results are particularly relevant.

A few prior papers have studied corporate jet use in samples of public firms. Yermack [2006] finds that initial disclosure of executive jet use in public firms' financial statements causes an immediate 1.1% negative abnormal stock market return and subsequent underperformance, consistent with excessive jet use. He finds no relationship, however, between jet use and executive ownership or governance variables. Using survey data, Rajan and Wulf [2006] find that publicly traded firms are more likely to offer their executives jet access when they are headquartered in remote counties with fewer commercial flights. But, they find little relationship between jet access and cash flows or firm governance variables, consistent with more responsible jet use. Hersch and McDougall [1992], however, find a small, negative relationship between managerial equity ownership and corporate aircraft fleets in a cross-section of public firms.

This paper makes a number of contributions beyond these other papers on corporate jets. First, it uses richer jet data in a larger sample of firms. Second, and perhaps most importantly, it studies jet use in private firms for the first time. Third, the time series dimension of data in this paper's sample of LBOs makes the case for identification of causal effects much stronger than in previous papers. Fourth, this paper's model of selection into PE ownership facilitates a more sophisticated discussion of endogeneity than has appeared in prior papers. Fifth, the quantile regression results and discussion of heterogeneity among public firms are entirely new, as other papers have presented results only at the mean.

Finally, this paper's finding of notable reductions in jet fleets by firms with private equity owners contrasts with the results in Yermack [2006] and Rajan and Wulf [2006], who find little relationship between jet use and ownership or governance variables. Further, the view of corporate jet use suggested by this paper's results is considerably more negative than that of Rajan and Wulf [2006], who conclude that there is little evidence that jet use represents managerial excess. This paper's results show that the combination of managerial incentives, capital structure, and monitoring associated with PE ownership produces clear differences in corporate jet fleets.

This paper was motivated in part by recent academic debates over the appropriate level and structure of executive compensation in public firms. Jensen and Murphy [1990] inspired a large literature by suggesting that compensation in large firms is insufficiently sensitive to firm performance. Yermack [1997] and Bebchuk and Fried [2006] have argued that executives enjoy too much control over their own compensation and often choose to pay themselves excessively. Edmans and Gabaix [2009] and Edmans, Gabaix, and Landier [2009], however, discuss models in which observed executive compensation arrangements would be optimal. Kaplan [2008] and Kaplan and Rauh [2010] point to evidence on other highly-compensated occupations which supports the notion that high executive pay could indeed result from arm's length negotations between firms and valuable talent. In my view, this paper's results call into question the notion that observed compensation arrangements in all public firms result from optimal contracts negotiated at arm's length.

A number of papers have studied behavior and performance in firms that have undergone leveraged buyouts. Evidence for improvements in firm performance following leveraged buyouts 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 management ownership, 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 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. Boucly, Sraer, and Thesmar [2009] find large and statistically significant increases in profitability in a sample of French buyouts from 1994 to 2004. Sheen [2009] finds that private U.S. chemical producers were more likely than publicly-traded producers to make well-timed investments in capacity increases in advance of positive demand shocks. As noted earlier, Kaplan and Stromberg [2009] and Acharya and Kehoe [2008] both conclude that, overall, existing literature supports the notion that private equity ownership produces improvements in efficiency and operating performance. The literature thus provides a great deal of support for the key assumption underlying the interpretation of results in this paper—that one can hope to learn something about efficient jet use by studying fleets in PE-owned firms.

At least two recent papers have studied executive compensation in private firms. Cole and Mehran [2008] provide facts on executive compensation in small businesses with fewer than 500 employees. They find that compensation is more sensitive to firm size than in public companies, but the radical difference between these small businesses and large public companies may make these results hard to interpret. Leslie and Oyer [2009] find that executives in PE-owned firms had lower base salaries and more performance-sensitive pay than executives in similar public firms, although these differences disappeared after the firms went public. This paper includes results examining a subset of the IPOs studied by Leslie and Oyer [2009], and it finds some evidence that differences in jet fleets begin to dissipate shortly after firms go public, consistent with their results.

Finally, I view this paper as part of a small but growing literature that seeks to identify causal effects of corporate governance on firm performance. There is a large literature that relates measures of firm behavior and performance to governance and ownership variables, paying scant attention to the fact that all of these variables would be determined endogenously in most models of equilibrium. Bertrand and Mullainathan [2003] surmount this difficulty by using difference-in-differences estimation to exploit differential timing of changes in state anti-takeover laws. Becker, Cronqvist, and Fahlenbrach [2008] use an instrumental variables strategy based on wealthy individuals' propensities to invest in firms located close to their homes. I have no source of exogenous variation in private equity ownership to exploit in this paper. In the next section, however, I present 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 private equity ownership.

3 Model of Selection into Private Equity Ownership

I present a very simple model to clarify the equilibrium relationship between jets and ownership that we would expect when these may both be determined endogenously. Firm i's profits will be 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. Firm i would 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,

$$E_i[\lambda_i^{PU} - \lambda_i^{PE}], \tag{1}$$

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 us to imagine counterfactual quantities like 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 estimate the difference in the averages of the fraction of profits diverted in public and private firms in cross-sectional data, however, we would instead be measuring,

$$E_i[\lambda_i^{PU} | (\lambda_i^{PU} - \lambda_i^{PE}) < c] - E_i[\lambda_i^{PE} | (\lambda_i^{PU} - \lambda_i^{PE}) > c]. \tag{2}$$

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. It is not always true that (2) is less than (1). But suppose that,

$$\lambda_i^{PE} = 0 \text{ for all } i.$$

that is, any jet use observed in PE-owned firms does not represent managerial waste, but is consistent with maximization of the value of the firm's profits.[2] Further, there is no firm in which private equity ownership could not reduce jet use to this efficient level.[3] In this case, we have,

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

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 being public to private, in which case we could observe the average λ_i^{PU} among some selection of firms for whom it is optimal to be PE-owned. In this simple, static setting, I leave unmodeled the

[2]Implicit in this framework is the assumptions that the transition from public to private equity ownership does not itself change the optimal size of a firm's jet fleet, for example, due to regulatory requirements or PE investors with objective functions that differ from public investors. I will present several pieces of evidence supporting this assumption later in the paper. Some readers might also be concerned that the transition from public to private equity ownership is triggered by events that also trigger a change in the optimal jet fleet. I think these concerns are misplaced, however, for a number of reasons. Results will include a variety of controls for firm size, so one can rule out the simple idea that jet fleet reductions that accompany post-LBO downsizings are driving results. More subtle stories might involve, for example, technological shocks that change the nature of businesses, making it optimal for them both to become PE-owned and to reduce jet fleets. Note, however, that if the optimality of PE ownership in such a scenario is derived from the inability of public firm managers to perform some needed restructuring, this inability is itself a form of the agency problem that this paper intends to measure.

[3]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 private equity ownership could not change their behavior. That there are some public firms with substantial agency problems is, of course, the conclusion of this paper.

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. It is easy to show that this logic extends to a model where 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.[4]

The intuition is simple. We might expect firms to go private when they have the most to gain from going private. 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 *overestimate* it when looking at changes in the panel of leveraged buyouts. There is some evidence for this selection into PE-ownerhsip,

[4] From the identity,

$$E_i[\lambda_i^{PU}] = Pr[\lambda_i^{PU} > c_i] \times E_i[\lambda_i^{PU}|\lambda_i^{PU} > c_i] + Pr[\lambda_i^{PU} < c_i] \times E_i[\lambda_i^{PU}|\lambda_i^{PU} < c_i],$$

we see,

$$\frac{E_i[\lambda_i^{PU}]}{E_i[\lambda_i^{PU}|\lambda_i^{PU} > c_i]} = Pr[\lambda_i^{PU} > c_i] + Pr[\lambda_i^{PU} < c_i] \times \frac{E_i[\lambda_i^{PU}|\lambda_i^{PU} < c_i]}{E_i[\lambda_i^{PU}|\lambda_i^{PU} > c_i]}. \quad (3)$$

If we observe in the data that,

$$E_i[\lambda_i^{PU}|\lambda_i^{PU} > c_i] > E_i[\lambda_i^{PU}|\lambda_i^{PU} < c_i],$$

then the rightmost term in (3) is less than one, as is the entire right-hand side, establishing,

$$E_i[\lambda_i^{PU}] < E_i[\lambda_i^{PU}|\lambda_i^{PU} > c_i]$$

It then follows from the identity above that,

$$E_i[\lambda_i^{PU}|\lambda_i^{PU} < c_i] < E_i[\lambda_i^{PU}].$$

but, in fact, 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.

4 Data

For basic information on private firms, I use the annual lists of America's Largest Private Companies published by Forbes magazine, which provide data on sales and employees in the largest private U.S. firms. These data are available in electronic form beginning in 1992, and recent years have included all firms with sales greater than one billion dollars. 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 2008.[5] For public firms, these consist of Compustat annual observations with fiscal years that end between July 2008 and June 2009. For private firms, I begin with the Forbes list of private companies that was published in 2009 and refers to 2008 data.[6] I used the LexisNexis Corporate Affiliations database to determine which firms are held by private equity funds, and these determinations were checked against the websites of PE funds where available. I also added 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 United States. The resulting sample of firms with non-missing sales and employees observations consists of

[5]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.

[6]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.

444 private firms, of which 101 are PE-owned, and 1,242 public firms, for a total of 1,686.[7]

To analyze changes in jet fleets in firms that go from public to private in a leveraged buyout, I construct a sample of buyouts using the Compustat and Forbes data, along with a sample of LBO transactions from the Dealscan database. Dealscan consists of data on syndicated loans, which form part of almost all leveraged buyouts, and I also identified a number of LBO firms that appeared in Compustat and the Forbes lists, but not in Dealscan. After carefully researching many individual transactions, I ended up with 68 firms that appeared in Compustat as stand-alone public firms with at least $1 billion in sales (in 2008 dollars), underwent a PE-led buyout, and 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. Jetnet employs a team of researchers to track the ownership, financing, and operatorship of all business airplanes in the United States, with additional coverage worldwide. 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 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. The data begin in the late 1980s, which unfortunately precludes the study of the many buyouts occuring in the 1980s.[8]

[7]One might worry that banks and other financial institutions could incorrectly appear 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.

[8]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, 7 out of 8 jets had been sold. (Burrough and Helyar [1990], p. 511).

The 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. 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.

The data were merged by firm name onto both the cross-sectional and buyout samples of firms.[9] In the cross-section, 1,681 jets operated by 648 unique firms were matched. In the buyout sample, 1,219 jet-firm-year observations involving 39 firms over 17 years were matched. Although this procedure for matching planes to firms is surely 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. As 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.

One might be concerned that jets operated by PE portfolio companies could appear in

[9] I first capitalized names, lengthened abbreviations like "Bros", truncated a list of common end words like "Company" (up to three times), and removed spaces and punctuation in names in Compustat, Forbes, and the jets data. I then attempted 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 did not match on this merge, I attempted 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 tried 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 were hand-verified by comparing the firm addresses listed in each dataset and doing additional internet research where necessary. The first-word merge was particularly fruitful as it caught 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.

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 2 or 3 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 fairly 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 looked 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 (e.g. Millenium Aviation), jet dealers (e.g. Bell Aviation), and other firms (e.g. Admiral Beverage Corporation). 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.

The jets data also include specification information for most jets and estimated operating cost information for some jets. The passenger seat capacity field is most frequently populated; it is non-missing in 98.9% of matched plane observations. I thus focus on total passenger seat capacity as a measure of fleet size at the firm level. Firms owning a fractional share are counted as a jet operator in results below, and they 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. In untabulated results, I find little difference between public and PE-owned firms in their propensity to own fractional shares, conditional on having any jet.

Table 1: Descriptive statistics

	(1) 2008 Cross-section Public Firms	(2) 2008 Cross-section Private non-PE	(3) 2008 Cross-section PE-owned Firms	(4) LBO Panel 1 year before LBO	(5) LBO Panel 1 year after LBO
N	1242	343	101	69	69
Sales, Mean	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	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, Standard Deviation	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, Standard deviation	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, Standard deviation	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

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 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 leveraged buyout between 1992 and 2008, where the firm's sales in the year prior to the LBO were at least one billion 2008 dollars

Table 1 presents descriptive statistics for both the cross-sectional and LBO samples. 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 have 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 these differences in a variety of flexible ways. In the panel of LBOs, there is actually relatively little change in firm size from one year before to one year after LBO.

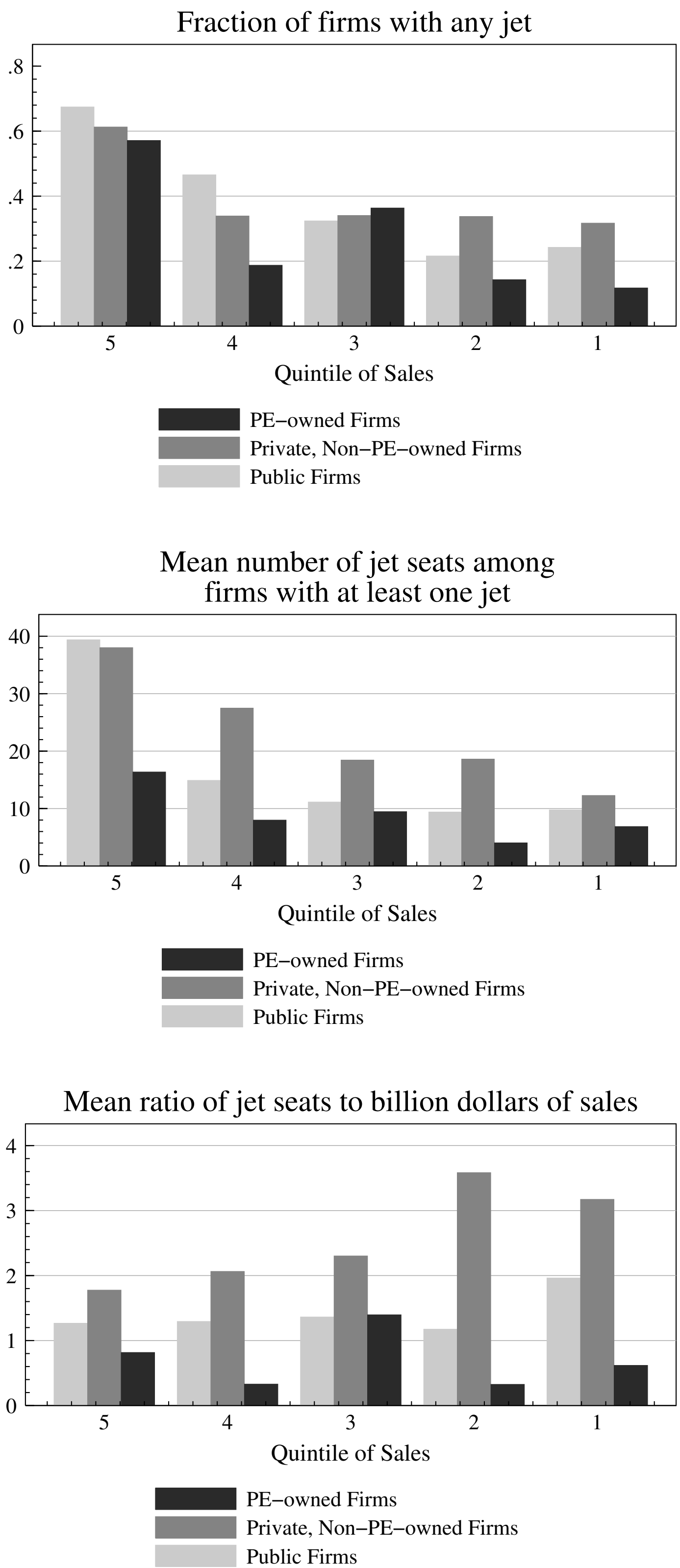
5 Results

5.1 Public and private firms in the cross-section

Figure 1 contains charts that illustrate 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 by size measured with sales.[10] 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

[10] 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.

Figure 1: Jet Fleets by Quintile of Firm Sales



Quintile 5 contains the largest firms and quintile 1 the smallest. The second two panels exclude 2 non-PE firms with ratios of jet seats to billions of dollars of sales greater than 200.

driven entirely by firm size. For example, the PE-owned firms in quintile 4 are less likely to have a jet than the public firms in the quintile 3, even though these PE-owned firms are larger.

5.1.1 Results on the presence of any jet

Regression results in Table 2 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 have 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.

Firms may fundamentally differ in their need for air travel depending on factors like their size, their location, and the nature of their business. If PE ownership is correlated with any of these characteristics, the coefficients 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 in the form of logarithms of sales and employees. Column 3 adds dummies for headquarters state and 2-digit NAICS industries. Column 4 adds 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 5 and 6 allow for more flexibility. Column 5 adds quadratic polynomials in sales, employees, and scheduled flights, and column 6 includes thirty dummy variables for firms in each of the ten deciles of sales, employees, and scheduled flights.

In all six columns, the coefficient on the dummy for PE ownership indicates that PE-owned firms are 10 to 12 percentage points less likely to have any jets, after controlling for size 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 7 and 8 repeat the specifications in columns 5 and 6 using dummies for 3-digit NAICS industries in place of 2-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 3 through 8 are statistically different from zero at the five percent level. Results from probit models are very similar to these OLS results and are reported in Table 7 in the Appendix.

Table 2: OLS regressions of a dummy for operating any jets on ownership variables and controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private Equity Owned	-.123	-.099	-.121	-.112	-.112	-.107	-.128	-.125
	(.071)*	(.065)	(.057)**	(.055)**	(.055)**	(.053)**	(.052)**	(.050)**
Private, not PE	-.042	.034	.023	-.009	-.010	-.0003	-.021	-.017
	(.038)	(.037)	(.035)	(.032)	(.033)	(.034)	(.039)	(.040)
Log Sales		.112	.114	.122	.136		.126	
		(.016)***	(.017)***	(.017)***	(.020)***		(.019)***	
Log Employees		.055	.063	.060	.054		.060	
		(.012)***	(.012)***	(.012)***	(.014)***		(.014)***	
Log Flights within 50 Miles				-.074	-.084		-.082	
				(.010)***	(.039)**		(.041)**	
Sales (billions)					-.002		-.001	
					(.002)		(.002)	
Sales^2					3.35e-06		2.94e-06	
					(3.44e-06)		(3.38e-06)	
Employees (thousands)					.0004		.0005	
					(.0003)		(.0004)	
Employees^2					-2.43e-07		-2.75e-07	
					(1.59e-07)		(1.66e-07)*	
Flights within 50 Miles					.0004		.0004	
					(.001)		(.001)	
Flights^2					-6.90e-07		-8.99e-07	
					(2.03e-06)		(2.19e-06)	
Const.	.400	.125	-.573	1.128	1.221	-.516	1.809	.172
	(.020)***	(.029)***	(.234)**	(.205)***	(.412)***	(.280)*	(.406)***	(.224)
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	1686	1672	1672	1686	1672	1686
R^2	.004	.126	.21	.235	.236	.239	.262	.264

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, as described in the text. Standard errors (in parentheses) are clustered by headquarters state. Standard errors fall if clustered by NAICS 2 or 3 digit industries.
*** indicates statistical significance at the 1% level, ** at 5%, and * at 10%

5.1.2 Results on fleet size

The second panel of Figure 1 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. This panel helps to convince us that the disparity between PE-owned and other firms in the top panel is not just an artifact of the name-matching procedure. Even among firms that have been successfully matched by name, PE-owned firms have fewer or smaller jets.

The bottom panel of Figure 1 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 3 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. 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 8 use data where the seats-to-sales ratio is winsorized at the 95th percentile of its distribution.[11] Winsorizing at higher threshholds (like 99%) *raises* the estimated magnitude of the effect of PE ownership.

[11]That is, observations with values above the 95th percentile are replaced with the value at the 95th percentile.

With this winsorization, Column 2 shows that public firms average 1.199 jet seats per billion dollars of sales, while PE-owned firms average .485 fewer seats, a difference of 40%. Columns 3 through 8 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 ten percent level in all columns 2 through 8 and at the one percent level in columns 4, 5, and 7.[12]

[12]In this set of results, clustering 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 where state fixed effects are not included, as one would expect.

Table 3: OLS regressions of ratio of jet seats to billions of dollars of sales on ownership variables and controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private Equity Owned	-.791	-.485	-.566	-.679	-.651	-.609	-.714	-.674
	(1.000)	(.252)*	(.257)**	(.255)***	(.249)***	(.254)**	(.259)***	(.265)**
Private, not PE	1.969	.658	.640	.611	.532	.598	.524	.572
	(.590)***	(.148)***	(.160)***	(.159)***	(.161)***	(.162)***	(.177)***	(.179)***
Log Sales					-.177		-.199	
					(.115)		(.125)	
Log Employees					.352		.371	
					(.079)***		(.089)***	
Log Flights within 50 Miles					-.428		-.401	
					(.157)***		(.159)**	
Sales (billions)					-.007		-.007	
					(.009)		(.010)	
Sales^2					.00002		.00002	
					(.00003)		(.00003)	
Employees (thousands)					-.002		-.002	
					(.003)		(.003)	
Employees^2					4.72e-07		3.71e-07	
					(1.41e-06)		(1.42e-06)	
Flights within 50 Miles (thousands)					-.001		-.002	
					(.004)		(.004)	
Flights^2					3.72e-06		4.86e-06	
					(7.57e-06)		(7.73e-06)	
Constant	1.586	1.199	.591	-2.605	6.719	-5.859	9.741	-2.529
	(.274)***	(.069)***	(.734)	(2.557)	(1.753)***	(2.586)**	(2.826)***	(3.400)
Observations	1686	1686	1686	1686	1672	1686	1672	1686
R^2	.007	.015	.052	.127	.169	.173	.198	.201

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 in Column 1 includes all firms with 2008 sales greater than $1 billion. Columns 2 through 8 winsorize the dependent variable at the 95th percentile. OLS standard errors are in parentheses. Standard errors fall if clustered by headquarters state.
*** indicates statistical significance at the 1% level, ** at 5%, and * at 10%.

I have focused 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. Table 8 in the Appendix presents 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 in Table 8 of the effect of PE ownership on the logarithm of seats range from -.42 to -.56, and all are statistically different from zero at the five or one percent level.

Tables 3 and Table 8 both also show that private, non-PE-owned firms average larger fleets than publicly-traded firms (although this effect largely disappears when including the most detailed sets of controls in Table 8). This result is helpful in ruling out a class of alternative explanations for the results on PE-owned firms. One might suggest various particular 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 return to this issue at length in section 5.5.

5.1.3 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.[13] 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 9 in the Appendix presents Tobit results for the same specifications as the OLS results in Table 8. 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.

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,

[13]For example, suppose total expenditure on executive travel is y. Jet seats are equal to 0 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 who 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 a reasonable simplification.

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.

5.2 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), and to the inclusion of a variety of flexible controls for size, industry, and location.

One could still be worried, 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 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 omitted variable bias.[14] The model in section 2 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

[14] If PE funds take actions like moving firms to more accessible locations to reduce travel costs, I would consider it appropriate that my estimates capture this behavior.

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 LBOs. Of these 32 firms, 20 reduced the number of seats in their fleet within two years of the buyout, 5 increased their fleets, and 7 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.

Note also 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, 5 in 2006, and 3 in 2005. One could worry that fleet reductions in these firms were not driven by the LBO, but 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.

5.2.1 Results on the presence of any jet

The top panel of Figure 2 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

Figure 2: Jet fleets in years surrounding leveraged buyouts

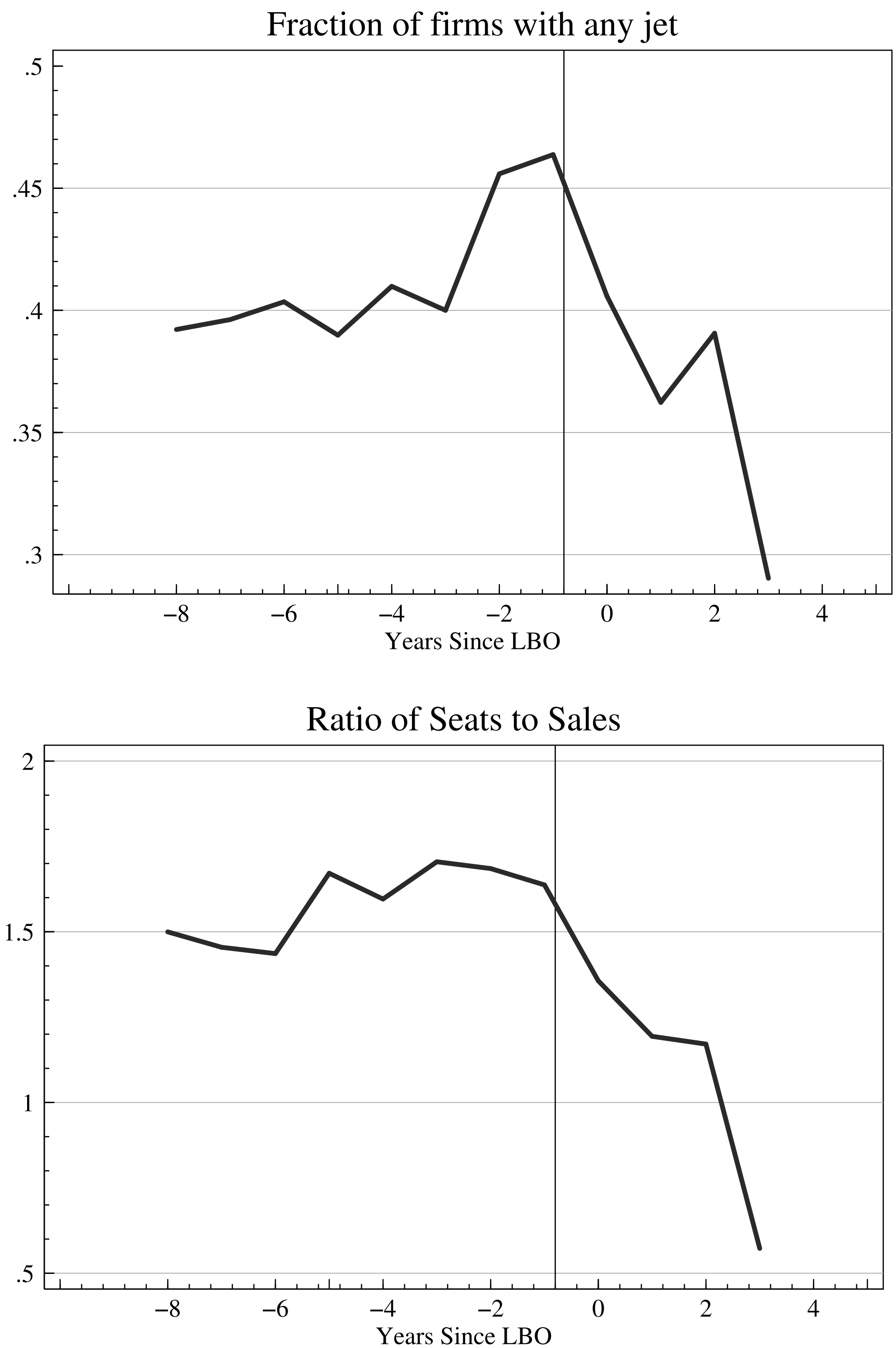


Table 4: Regressions of a dummy for operating any jets on dummies for years surrounding a leveraged buyout

	(1)	(2)	(3)	(4)	(5)	(6)
t = LBOyear - 3	-.048	-.019	.031	.042	.030	.031
	(.046)	(.078)	(.029)	(.056)	(.028)	(.056)
t = LBOyear - 2	.002	.040	.031	.055	.031	.051
	(.023)	(.044)	(.024)	(.043)	(.023)	(.042)
t = LBOyear	-.058	-.102	-.064	-.097	-.061	-.095
	(.030)*	(.029)***	(.033)**	(.035)***	(.031)**	(.033)***
t = LBOyear + 1	-.101	-.162	-.108	-.141	-.096	-.149
	(.038)***	(.039)***	(.042)**	(.047)***	(.038)**	(.041)***
t = LBOyear + 2	-.106	-.164	-.106	-.137	-.082	-.129
	(.043)**	(.056)***	(.077)	(.077)*	(.051)	(.063)**
t = LBOyear + 3	-.142	-.217	-.112	-.157	-.120	-.176
	(.074)*	(.112)*	(.105)	(.112)	(.055)**	(.103)*
t = LBOyear + 4	-.209	-.288	-.212	-.259	-.155	-.212
	(.085)**	(.146)**	(.154)	(.197)	(.066)**	(.136)
Constant	.462	.478	-.237	-.130	-.022	.084
	(.020)***	(.032)***	(.334)	(.280)	(.285)	(.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	.814	.824	.862	.87	.85	.857

The dependent variable is a dummy indicating that 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 leveraged buyout between 1992 and 2008, where the firm's sales in the year prior to the LBO were at least one billion 2008 dollars. All nonmissing observations from 3 years before each LBO to 4 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.
*** indicates statistical significance at the 1% level, ** at 5%, and * at 10%.

simply graphs raw averages 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 4 presents regressions of a dummy indicating that 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 2. Column 2 adds year effects.[15] Columns 3 and 5 instead includes 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 of 69) of the firms in the LBO sample had at least one jet in the year prior to LBO. 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. The number of observations in later years falls primarily due to the absence of observations for firms that went public 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 one percent level in four of the six columns when clustering by headquarters state. Effects four years after LBO are statistically significant at the five percent 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 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]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.

Table 5: Regressions of ratio of jet seats to billions of dollars of sales on dummies for years surrounding a leveraged buyout

	(1)	(2)	(3)	(4)	(5)	(6)
t = LBOyear - 3	.158	.155	.158	.151	.342	.232
	(.183)	(.186)	(.190)	(.194)	(.215)	(.182)
t = LBOyear - 2	.148	.148	.153	.151	.209	.176
	(.138)	(.140)	(.144)	(.146)	(.140)	(.137)
t = LBOyear	-.160	-.162	-.164	-.166	-.166	-.161
	(.106)	(.109)	(.112)	(.115)	(.103)	(.108)
t = LBOyear + 1	-.360	-.255	-.405	-.212	-.323	-.208
	(.152)**	(.092)***	(.285)	(.161)	(.156)**	(.090)**
t = LBOyear + 2	-.446	-.332	-.833	-.532	-.393	-.285
	(.186)**	(.124)***	(.490)*	(.330)	(.159)**	(.115)**
t = LBOyear + 3	-.487	-.253	-1.097	-.524	-.474	-.202
	(.351)	(.219)	(.842)	(.595)	(.321)	(.180)
t = LBOyear + 4	-1.053	-.707	-2.109	-1.225	-.910	-.597
	(.528)**	(.337)**	(1.035)**	(.560)**	(.425)**	(.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	.876	.909	.866	.903	.885	.913

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 leveraged buyout between 1992 and 2008, where the firm's sales in the year prior to the LBO were at least one billion 2008 dollars. All nonmissing observations from 3 years before each LBO to 4 years after are included. All specification 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.
*** indicates statistical significance at the 1% level, ** at 5%, and * at 10%.

5.2.2 Results on fleet size

The bottom panel of Figure 2 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 falling sharply as early as the end of the year in which the LBO occurs. Again, 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 5 presents regressions where the dependent variable is the firm-level ratio of jet seats to billions of dollars of sales and 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 from Table 4. Inspection of the data reveals that the magnitude of results can be rather sensitive to the inclusion or exclusion of particular firms with large values of the seats-to-sales ratio prior to their LBOs. Columns 2, 4, and 6 present results excluding one such firm.[16]

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 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 4. Estimated fleet reductions are statistically different from zero at the five percent level or better 1, 2, and 4 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 some weak evidence of a "pre-trend," that is, a ratio of seats-to-sales 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 2. There is also no evidence of such a trend in the results in Figure 2 and Table 4 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 .597 reduction in 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,

[16] I call the firm Firm X. Firm X appears in Dealscan as an LBO, and 90% of its shares were purchased by management and a private equity 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 2008 dollars, this represents a very large reduction in its seat-to-sales ratio which can significantly affect overall results.

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%.

5.2.3 Comparing cross-sectional and LBO results

Returning to the comparison between the cross-sectional and LBO results, it appears that results provide some 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 these firms from the cross-section are much larger 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.

The comparison of cross-sectional and panel results using the seats-to-sales ratio as the dependent variable is even less clear. From the descriptive statistics in Table 1, we see that public firms that underwent LBOs began with a seats-to-sales ratio of 1.8, while public firms in the 2008 cross-section averaged a ratio of 1.6. In the cross-section, PE-owned firms averaged seats-to-sales ratios around 0.5 or 0.6 below those of public firms, and panel estimates three and four years post-LBO are similar. In percentage terms, these represent a roughly 40% reduction in average seats-to-sales ratios. But again, the LBO panel in particular produces a wide range of estimates at different horizons and in different specifications.

In any case, it is quite clear that there is a 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 2, 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 which 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 approaches to estimation. 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 at least a 40% reduction in fleet size. That is, if firms that are currently public were taken private, we should expect average fleet reductions of at least this size.[17]

The similarity between cross-sectional and within-firm results might, however, cause one to question the usefulness of jet fleets as a "tip of the iceberg" measure of agency costs throughout a firm. Firm characteristics that tend to attract private equity buyers are apparently only modestly correlated with jet fleet size. If agency problems are a key driver of which firms are taken private, this observation would suggest that there are some firms with severe agency problems that do not have large fleets, and many firms with large fleets but without severe agency problems. In this case, the distribution of jet fleets across firms would provide only limited information about the distribution of agency problems across firms.

[17] Results here are likely to be relevant in partial equilibrium only. That is, if *all* firms were taken private by PE funds, reducing aggregate demand for jets and pushing down jet prices, they may not be relevant for predicting changes in jet quantities. This scenario is unlikely to occur, but this caveat would be important if one were evaluating the effects of a potential policy that could change demand for jets by many firms.

On the other hand, there are reasons to think that one should not be too concerned by this line of argument. Conventional wisdom suggests that the private equity industry has evolved since the 1980s so that modern LBOs typically involve a more friendly relationship between PE investors and firm managers. For example, the KKR buyout of RJR-Nabisco in 1988 involved a relatively hostile relationship between KKR and RJR's CEO Ross Johnson, who was immediately replaced after the buyout (Burrough and Helyar [1990]). More recent deals typically feature cooperation between the buyout funds and managers, and incumbent management is often retained after the deal. Thus it is entirely plausible that the largest residual jet fleets in the publicly-traded cross-section are operated by firms that indeed suffer from severe, firm-wide agency problems. However, they may simply have managers who are uninterested in facing the pressures associated with a buyout and entrenched enough to resist any push for change from shareholders or board members. In such a situation, we would expect to observe large jet fleets in public firms that are not taken private, even if large fleets are indeed accurate indicators of agency problems.

5.3 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 twenty-five 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, unreported results show that this change is not statistically different from zero at conventional levels in regressions like those from the LBO panel in Table 4. 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.

5.4 Quantiles

The previous sections have shown that PE-owned firms have smaller jet fleets, *on average*, than publicly traded or private, non-PE firms, both in the cross-section and in a panel of LBOs. 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 1 show that more than 60% of firms in the cross-sectional sample have no jet visible in the data, and the results in Table 5 show that estimated average magnitudes are sensitive to the inclusion or exclusion of small numbers of large values.

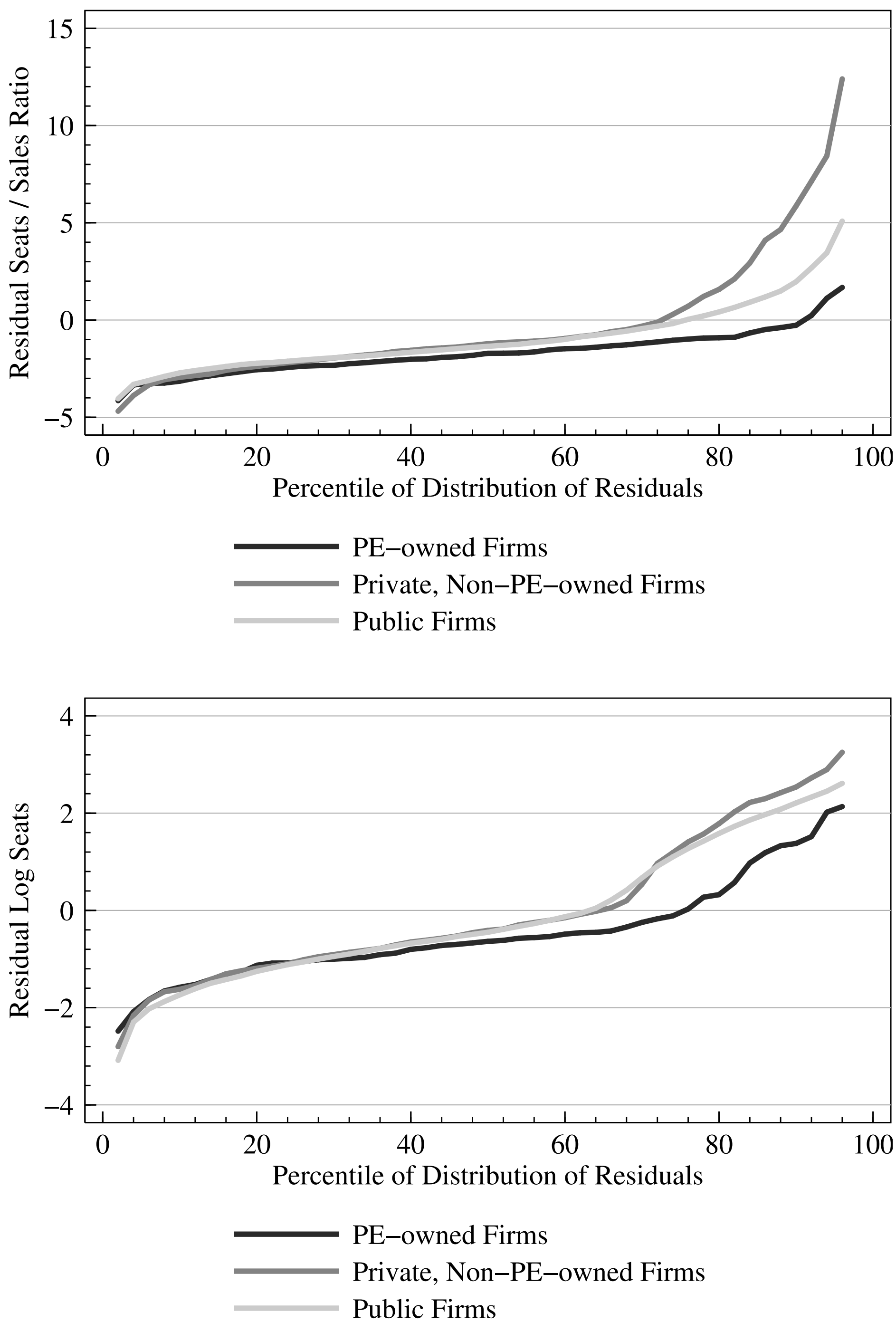
Figure 3 further describes the differences in the distribution of jet fleets between PE-owned and other firms by graphing the percentiles of the residual fleet size distribution for different kinds of firms. That is, a fleet size measure is regressed on a set of controls (not including PE or private 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.

Panel A of Table 6 presents quantile regressions that quantify this relationship.[18] Column

[18]The quantile regression estimator for the τth quantile is,

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

Figure 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. In the top panel, this is 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. In the lower panel, this is 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 (-.827, representing a 1/16 share of a 7-seat jet).

1 indicates that 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 3. 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 3 does not manifest itself throughout the conditional distribution of jet fleets. Instead, it is driven by firms in the upper part of the jet fleet distribution.

These quantile results are potentially quite important for understanding the nature of the agency problems that this paper measures. They suggest that the kind of agency problems that manifest themselves in large jet fleets are far from ubiquitous in public firms. Neither, however, are they limited to a very small number of "bad apples."

These results are also helpful in ruling out a class of alternative explanations for the results presented previously. One might suggest various particular 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 around 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

where ρ_τ is the "check function,"

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

[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 6: Quantile regressions of ratio of jet seats to billions of dollars of sales on ownership variables and controls

	50th	60th	70th	80th	90th
	(1)	(2)	(3)	(4)	(5)
	Panel A: Quantile Regression Estimates				
Private Equity Owned	-.008	-.073	-.235	-.837	-1.636
	(.017)	(.061)	(.147)	(.412)**	(.602)***
Private, not PE	-.0004	-.012	.006	.911	2.526
	(.007)	(.048)	(.350)	(.789)	(.882)***
Observations	1672	1672	1672	1672	1672
	Panel B: Censored Quantile Regression Estimates				
Private Equity Owned	-.006	-.103	-.209	-1.156	-2.256
	(.740)	(.284)	(.306)	(.551)**	(.764)***
Private, not PE	-.0002	.026	.114	1.029	1.933
	(.174)	(.175)	(.392)	(.734)	(.820)**
Observations	1445	1560	1567	1582	1588

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 text. 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.
*** indicates statistical significance at the 1% level, ** at 5%, and * at 10%.

problems in this minority of firms than with any structural difference in jet needs between public and PE-owned firms.

5.4.1 Censored quantile regressions

These conclusions from the quantile results 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 feasible that private equity 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 private equity 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.[20] 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 a simple and robust algorithm which 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.[21]

Panel B of table 6 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

[20]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 is a linear function of covariates, which is similar to the Tobit model's assumption about the mean.

[21]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.

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 3.[22] 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.

5.5 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 1 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 paper 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].[23] In another early

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

[23] A relevant example comes from the Berwind Group, a collection of family-owned firm active in several

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-Silane, 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 United States. In fact, these private firms are also frequent targets of private equity acquisitions (Kaplan and Stromberg [2009]).[24]

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 simply willing to spend their own money on jets or other non-pecuniary 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.[25] 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,

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 to make money for David (Gotlieb [2000]).

[24]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).

[25]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).

potentially imperiling the interpretation of the presence of large jet fleets in these firms as 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: *ceteris paribus*, 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 tested this hypothesis using the Compustat Executive Compensation data merged with the data on public firms used thus far in the paper. Table 10 in the Appendix 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.[26] 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.

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.

5.6 Other Alternative Explanations

Finally, I will 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 fall under the category of explanations that are made less compelling by the observed concentration of public firms' jets in a minority of firms with

[26] 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.

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 sizes. 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 US who are listed on their funds' websites as serving on portfolio company boards.[27] Less than 20% appeared in any form in the data, 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 private equity.

I also investigated the boards of some comparably-sized public firms, including the pre-buyout boards of some recent LBOs.[28] 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.[29] Overall, there is thus little evidence that

[27]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 there are many jets owned by shell companies with opaque names (e.g. JJSA Aviation II LLC), but 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 twenty Gulfstream G-IV SP jets (the most popular model among public firms in the dataset) and twenty Bombardier Challenger 604 jets (a popular model among smaller firms and wealthy individuals). Of these forty 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.

[28]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 owners.

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

the 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.[30]

6 Conclusions

This paper has presented graphical and econometric evidence showing that firms owned by private equity 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 a leveraged buyout. 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 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 over the severity of agency problems in public firms and the need for further reform of executive compensation. The changes in leverage, compensation, and monitoring associated with PE ownership produce 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

[30]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.

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.

This paper has not addressed the question of which aspects of private equity ownership—leverage, compensation, or monitoring—are most important in changing firm behavior. The opacity of private firms and the lack of exogenous variation in these variables make it difficult to determine which of them have the largest impacts on behavior within the sample of PE-owned firms. Disentangling the alternative mechanisms through which PE ownership affects firm behavior and drawing lessons for the governance of public firms would also be a useful goal for future research.

Finally, this paper has presented facts on private, non-PE-owned firms that might surprise some readers. Jet fleets in these firms look more like those in publicly-traded firms than those in PE-owned firms. Although they control large portions of the economy, private firms have received far less attention from researchers than public firms, presumably due to a scarcity of data. Further efforts to unearth data on private firms could produce useful insights into performance and governance in this important sector of the economy.

A Appendix Tables

Table 7: Probit regressions of a dummy for operating any jets on ownership variables and controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private Equity Owned	-.122	-.095	-.117	-.110	-.110	-.105	-.129	-.125
	(.070)*	(.064)	(.055)**	(.054)**	(.055)**	(.052)**	(.047)***	(.045)***
Private, not PE	-.041	.036	.025	-.010	-.009	-.0004	-.024	-.021
	(.038)	(.037)	(.034)	(.032)	(.032)	(.032)	(.037)	(.037)
Log Sales		.106	.105	.112	.123		.115	
		(.016)***	(.018)***	(.017)***	(.020)***		(.020)***	
Log Employees		.055	.065	.061	.056		.062	
		(.013)***	(.014)***	(.014)***	(.017)***		(.017)***	
Log Flights within 50 Miles				-.069	-.071		-.070	
				(.010)***	(.036)**		(.037)*	
Sales (billions)					-.002		-.001	
					(.002)		(.002)	
Sales^2					3.69e-06		2.98e-06	
					(4.01e-06)		(3.87e-06)	
Employees (thousands)					.0003		.0003	
					(.0008)		(.0008)	
Employees^2					5.47e-08		2.85e-08	
					(1.58e-06)		(1.53e-06)	
Flights within 50 Miles					.0002		.0003	
					(.001)		(.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

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.
*** indicates statistical significance at the 1% level, ** at 5%, and * at 10%

Table 8: OLS regressions of log jet seats on ownership variables and controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private Equity Owned	-.560	-.422	-.430	-.508	-.466	-.453	-.537	-.528
	(.208)***	(.180)**	(.175)**	(.156)***	(.150)***	(.139)***	(.137)***	(.133)***
Private, not PE	-.094	.257	.248	.236	.121	.149	.069	.079
	(.128)	(.125)**	(.134)*	(.119)**	(.111)	(.116)	(.136)	(.146)
Log Sales		.582	.566	.603	.571		.526	
		(.058)***	(.061)***	(.060)***	(.074)***		(.070)***	
Log Employees		.216	.257	.235	.212		.248	
		(.053)***	(.054)***	(.052)***	(.052)***		(.050)***	
Log Flights within 50 Miles					-.252		-.233	
					(.117)**		(.127)*	
Sales (billions)					.004		.005	
					(.006)		(.006)	
$Sales^2$					-4.85e-06		-4.87e-06	
					(1.00e-05)		(1.00e-05)	
Employees (thousands)					.001		.001	
					(.002)		(.002)	
$Employees^2$					-5.89e-07		-6.19e-07	
					(8.24e-07)		(8.05e-07)	
Flights within 50 Miles					-.0006		-.0008	
					(.004)		(.004)	
$Flights^2$					1.47e-06		1.34e-06	
					(6.62e-06)		(7.33e-06)	
Const.	.488	-.781	-.747	-3.191	2.919	-2.068	5.417	.647
	(.079)***	(.111)***	(.476)	(.701)***	(1.259)**	(.868)**	(1.289)***	(.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	.006	.21	.242	.296	.32	.323	.347	.349

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 (-.827, representing a 1/16 share of a 7-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.
*** indicates statistical significance at the 1% level, ** at 5%, and * at 10%.

Table 9: Tobit regressions of log jet seats on ownership variables and controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private Equity Owned	-1.439	-.994	-1.059	-1.259	-1.225	-1.210	-1.414	-1.403
	(.755)*	(.631)	(.604)*	(.538)**	(.530)**	(.493)**	(.455)***	(.424)***
Private, not PE	-.308	.460	.573	.533	.243	.307	.100	.123
	(.354)	(.318)	(.322)*	(.283)*	(.275)	(.272)	(.318)	(.325)
Log Sales		1.082	.967	1.039	1.260		1.139	
		(.127)***	(.141)***	(.134)***	(.161)***		(.156)***	
Log Employees		.510	.685	.647	.635		.710	
		(.126)***	(.142)***	(.139)***	(.155)***		(.148)***	
Log Flights within 50 Miles					-.489		-.453	
					(.253)*		(.274)*	
Sales (billions)					-.013		-.009	
					(.010)		(.009)	
Sales^2					.00003		.00002	
					(.00002)		(.00002)	
Employees (thousands)					-.0005		-.0005	
					(.003)		(.003)	
Employees^2					-2.20e-07		-3.32e-07	
					(1.23e-06)		(1.31e-06)	
Flights within 50 Miles (thousands)					-.002		-.002	
					(.009)		(.009)	
Flights^2					1.21e-06		5.37e-07	
					(.00002)		(.00002)	
Const.	-1.553	-4.055	-3.878	-2.084	2.804	-.381	6.984	4.350
	(.260)***	(.384)***	(1.442)***	(1.419)	(2.744)	(1.535)	(2.844)**	(.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

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 prevented the Tobit algorithm from converging. Standard errors are clustered by headquarters state.

*** indicates statistical significance at the 1% level, ** at 5%, and * at 10%.

Table 10: OLS regressions of log salary on jet variables and controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dummy for any jet	.094	.101			.090	.097		
	(.017)***	(.019)***			(.017)***	(.020)***		
Seats / sales			.007	.008			.007	.008
			(.002)***	(.003)***			(.002)***	(.003)***
Tobin's Q	.035	.024	.030	.019	.243	.190	.260	.206
	(.026)	(.025)	(.026)	(.026)	(.100)**	(.088)**	(.100)***	(.087)**
Q^2					-.045	-.036	-.049	-.041
					(.024)*	(.021)*	(.024)**	(.021)*
Return on Assets	-.651	-.571	-.602	-.549	-1.386	-1.083	-1.353	-1.070
	(.168)***	(.174)***	(.174)***	(.182)***	(.556)**	(.536)**	(.578)**	(.569)*
RoA^2					2.112	1.488	2.120	1.489
					(1.720)	(1.582)	(1.804)	(1.723)
Log Sales	.135	.157	.150	.173	.166	.202	.184	.222
	(.012)***	(.011)***	(.011)***	(.010)***	(.018)***	(.020)***	(.017)***	(.018)***
Log Employees	.034	.008	.036	.011	.040	.011	.041	.011
	(.009)***	(.012)	(.009)***	(.012)	(.010)***	(.014)	(.010)***	(.014)
Log Flights within 50 Miles	.034	.020	.031	.018	.033	.025	.031	.022
	(.011)***	(.010)**	(.011)***	(.010)*	(.017)**	(.018)	(.016)*	(.018)
Sales (billions)					-.003	-.004	-.003	-.004
					(.002)*	(.001)**	(.002)*	(.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)					-.0004	-.0004	-.0003	-.0003
					(.0004)	(.0003)	(.0003)	(.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					-.0007	-.0001	-.0007	-.00003
					(.0005)	(.0006)	(.0005)	(.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
	(.122)***	(.106)***	(.116)***	(.109)***	(.166)***	(.187)***	(.152)***	(.184)***
Observations	930	930	929	929	930	930	929	929
R^2	.406	.519	.396	.51	.431	.533	.422	.526

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 2-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.

*** indicates statistical significance at the 1% level, ** at 5%, and * at 10%.

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