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

JESSE EDGERTON

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

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A. Further Data Detail

A.1. Characteristics of LBO Firms

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

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

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

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

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

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

A.2. Additional Detail on Jet and Firm Data

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

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

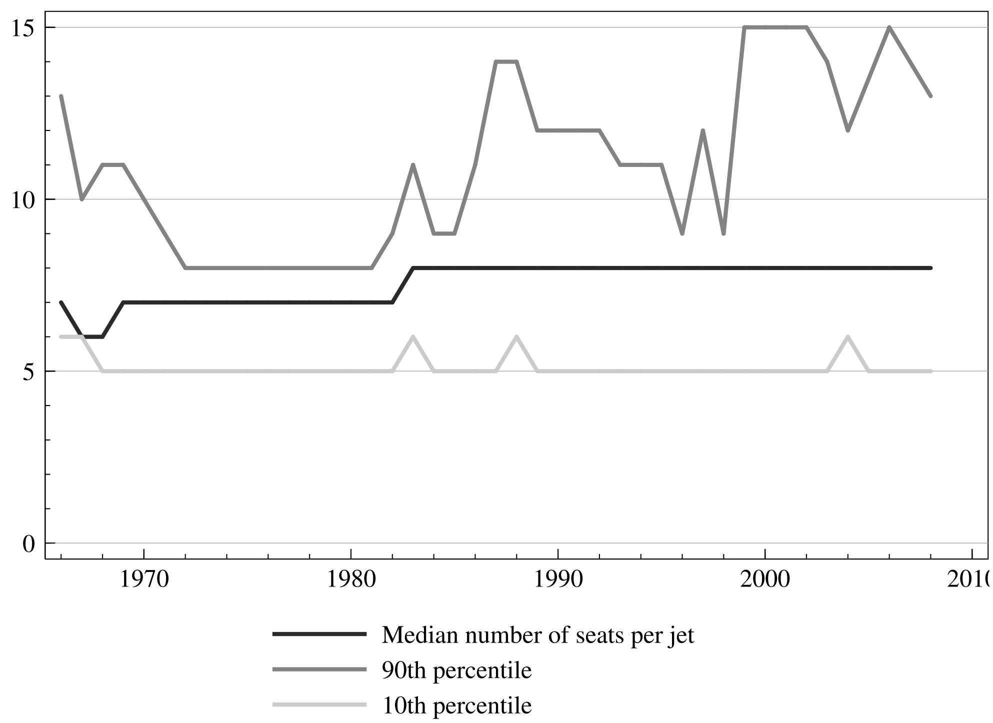


Figure IA.1. Jet size by year of initial delivery for universe of jets in JETNET data. The figure presents the 10th, 50th, and 90th percentiles of the distribution of all jets that ever appeared in the Jetnet data, by the year of their delivery.

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

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

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

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

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

B. Cross-Sectional Probit and Log Seats Results

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

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

Table IA.II

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

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

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

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

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

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

C. Censoring and the Tobit

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

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

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

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

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

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

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

D. Results on Fleet Size from the LBO Panel

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

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

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

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

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

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

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

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

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

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

E. IPOs

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

F. Leverage vs. Monitoring

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

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

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

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

Table IA.VI

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

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

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

from zero.

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

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

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

Table IA.VII

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

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

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

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

G. Quantile Regressions

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

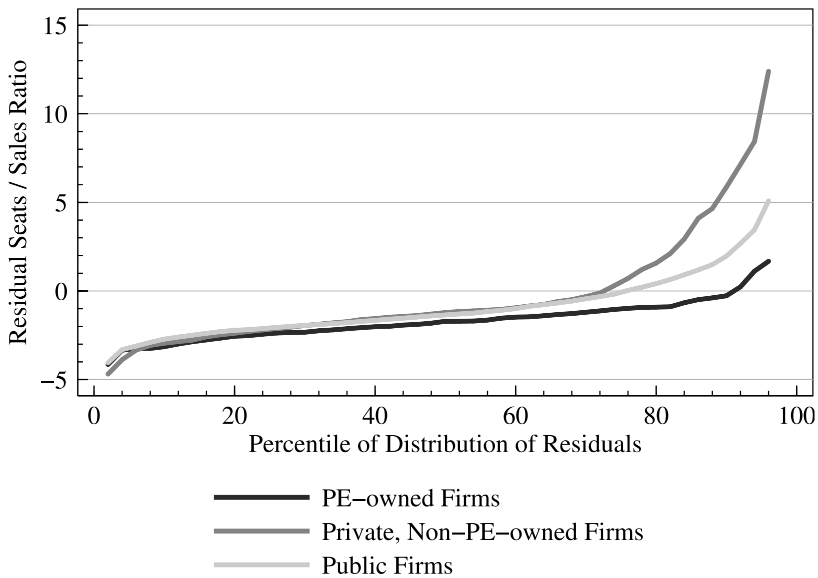
Recall that the quantile regression estimator for the τth quantile is

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

where ρ_τ is the "check function,"

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

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



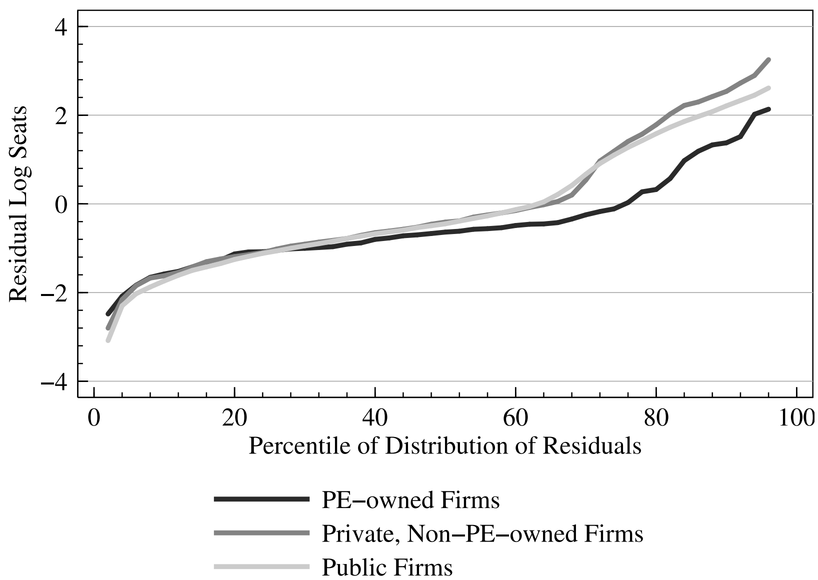


Figure IA.3. Percentiles of residual fleet size by ownership. These figures describe the distribution of the residuals from an OLS regression of fleet size measures on controls for size and location. The top panel corresponds to a regression of the ratio of seats to sales on logarithms and quadratic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

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

G.1. Censored Quantile Regressions

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

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

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

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

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

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

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

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

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

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

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

H. Interpreting Results on Private, non-PE Firms

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Table IA.VIII

OLS Regressions of Log Salary on Jet Variables and Controls

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

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

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

I. Other Alternative Explanations

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

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

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

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

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

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

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

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

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

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

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

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

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