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Smart Institutions, Foolish Choices: The Limited Partner Performance Puzzle

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ABSTRACT

The returns that institutional investors realize from private equity differ dramatically across institutions. Using detailed, hitherto unexplored records, we document large heterogeneity in the performance of investor classes: endowments' annual returns are nearly 21% greater than average. Analysis of reinvestment decisions suggests that endowments (and to a lesser extent, public pensions) are better than other investors at predicting whether follow-on funds will have high returns. The results are not primarily due to endowments' greater access to established funds, since they also hold for young or undersubscribed funds. Our results suggest that investors vary in their sophistication and potentially their investment objectives.

OVER THE PAST THREE DECADES, institutional investors have controlled an increasing share of the U.S. equity markets with their share of U.S. public equity markets exceeding the 50% threshold in 1995 (Gompers and Metrick (2001)).[1] A significant and growing literature in financial economics seeks to understand the investment decisions of institutional investors and the differences between institutions and other classes of investors, especially individual investors. Gompers and Metrick (2001) document, for example, that institutional investors hold stocks that have greater market capitalization, are more liquid, have higher book-to-market ratios, and realize lower returns in the prior year.[2]

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[1] Their calculation only examines institutions with greater than $100 million of securities under discretionary management, that is, those that are required to file a 13F form with the U.S. Securities and Exchange Commission. Thus, their estimate is a lower bound on institutional holdings.

[2] Of course, this pattern may reflect more frequent trades or other attributes. Massa and Phalippou (2004) seek to econometrically identify mutual funds' preference for illiquidity. Other studies suggest that institutional investors are less likely to buy stocks on days with high trading volume (Barber and Odean (2003)) or to herd into particular stocks (Lakonishok, Shleifer, and Vishny (1992), Grinblatt, Titman, and Wermers (1995)), and that their investments fall into a few well-defined styles (Froot and Teo (2004)).

A question that has attracted much less scrutiny, however, is the extent to which there exists heterogeneity in the performance and investment strategies across different types of institutional investors (Bennett, Sias, and Starks (2003) and Table 2 of Gompers and Metrick (2001) are rare exceptions). While institutional investors as a group vary substantially from retail investors due to the larger size of their portfolios or the resources available to them, there are also systematic differences across institutions in terms of organizational structure, investment objectives, or even (the perceived level of) sophistication. A number of recent theoretical papers suggest that these organizational differences can have profound implications for portfolio allocation decisions and, ultimately, investment returns.[3]

In this paper, we analyze whether there exist systematic differences in the returns and investment strategies across institutional investors, focusing on one asset class, private equity. Since it is generally believed that the private equity market is characterized by greater information asymmetries than public markets, differences among institutions should be most pronounced here. We analyze investment styles and performance across several different classes of institutional investors, known as limited partners (LPs), e.g., banks, corporate and public pension funds, endowments, advisors, insurance companies.

Numerous accounts by both objective observers and practitioners suggest that there is substantial variation in the investment criteria and sophistication of private equity investors. For instance, the manager of a large endowment highlights the advantages that private university endowments enjoy because of their greater flexibility to evaluate nonstandard investment opportunities (Swensen (2000, p. 335)):

> [Endowments] on the cutting edge choose from a broader opportunity set. [...] By considering alternatives outside the mainstream, investors increase the likelihood of discovering the next big winner well before it becomes the next big bust. By evaluating managers without the requisite institutional characteristics, investors might uncover a highly motivated, attractive group of partners. Operating on the periphery of standard institutional norms increases opportunity for success.

Swensen's description, echoed in our conversations with other industry observers, suggests that endowments have greater flexibility in selecting investments. Other institutions, it is suggested, often rely on overly rigid decision criteria or lack a sufficient understanding of the asset class. Observers attribute these failures to several underlying factors: (1) inappropriate incentives, for example, the limited compensation and autonomy that public pension investment officers enjoy, which leads to frequent turnover, and the rewards from

[3] For example, Shleifer and Vishny (1997) suggest that information asymmetries between investors and intermediaries create limits to arbitrage that can affect the portfolio strategies and eventually the returns of the intermediaries (see Gromb and Vayanos (2002) for a similar argument). Because the extent of agency problems may differ dramatically across institutions, considerable differences in the behavior of institutional investors could be expected.

maximizing assets under management that advisors receive, even if they lead to deteriorating returns); (2) poor human resource practices, for example, the rapid rotation of personnel in corporate pension funds; and (3) conflicting objectives (see below). We anticipate that the deleterious effects of such distortions should be most dramatic for those cases for which it is most difficult to assess performance, namely, venture capital (VC), where the progress of the companies in the funds' portfolios is very difficult to assess from traditional accounting data.

Using detailed, hitherto unexplored records of the portfolio composition and performance of funds that different classes of LPs invest in, we document dramatic differences in returns across classes of LPs. The average returns of private equity funds that endowments invest in are nearly 21% greater than those of the average LP in our sample. Funds selected by banks lag sharply. These differences in performance hold even if we control for observable characteristics such as vintage year and type of the fund, which have been shown to be important in prior studies (see, e.g., Gompers and Lerner (1998, 2000) and Kaplan and Schoar (2005)). We also find that across the different groups, older LPs tend to realize better performance than do newer LPs, which indicates that LPs' investment decisions may improve with experience. Overall, these results suggest that LPs differ in their ability to evaluate the quality of funds and to invest based on this information, that is, in their level of sophistication.

To understand whether differences in sophistication can explain these performance differences, we analyze reinvestment decisions across LP classes. The decision to reinvest in the next fund of a general partner (GP) is the central means by which LPs can adjust their portfolio, make use of inside information obtained during the investment process, and exert governance pressure on the GP, since private equity is a very illiquid asset class (investors have little recourse to their investment once the capital has been committed). We find that follow-on funds in which endowments (and to a lesser extent, public pension funds) decide to reinvest achieve much higher performance than those funds in which they decide not to reinvest, suggesting that these LPs are better at forecasting the performance of follow-on funds. Other LP classes do not demonstrate these performance patterns. These findings suggest that endowments proactively use the information they gain as inside investors to improve their investment decisions, while other LPs seem less willing or able to use this information.

Second, to understand whether LPs also differ in their ability to act on public information, we examine investments in young private equity groups (those established after 1990). If performance differences are driven mainly by the superior access of established LPs to older private equity groups, conditioning on younger GPs should eliminate the performance difference. We again find a performance premium for endowments and public pension funds, though the difference is smaller than in the analysis using all GPs.

There are a number of alternative explanations for the observed heterogeneity in performance other than differences in sophistication. First, LPs might differ in the risk profile of the funds they choose. For example, endowments could be systematically investing in riskier funds and therefore have higher returns. Second, LPs may vary in their objective functions. Finally, anecdotes in the private equity industry suggest that established LPs often have preferential access to funds, in which case the performance differentials may be due simply to historical accident: Through their early experience as LPs, endowments may have greater access to established groups with high performance. We analyze these different explanations in turn.

To address the concern that differential performance may be driven by variation in the risk profile of the funds that LPs choose, we control for a number of observable characteristics that are generally considered risk factors, such as the focus and maturity of the investments selected by a fund, and the fund's size, age, and location. While our results are robust to these controls, we cannot completely rule out the possibility that unobservable risk factors might affect our results. Therefore, we also conduct a type of value-at-risk analysis, where for each class of LPs we calculate the likelihood that the internal rate of return (IRR) of a fund falls below a certain cutoff level. If, indeed, endowments achieve their superior returns by taking on riskier investments, we should expect that they have a higher likelihood of having funds in the lowest performance quartiles. However, we do not find any evidence that supports the idea that endowments achieve their superior performance by relying on riskier investment strategies.[4]

Second, the concern that performance differences across LPs could be, at least in part, the result of differences in the objectives of LPs should be most important for banks and public pension funds. For example, banks might diverge from maximizing returns on investments in order to obtain future banking income from the portfolio firms. However, we find that banks underperform the other LPs not only in the buyout industry, where considerations about future business might be important, but even in VC deals, where the benefits from selling future services seem much smaller. Moreover, banks with a small fraction of their profits from corporate clients also underperform. Similarly, public pension funds might face political obligations to invest in in-state funds in an effort to support the local economy even if doing so reduces return on investment. We therefore compare the performance of in-state funds across LPs and find that, indeed, public pension funds underperform other LPs in their in-state funds. However, this does not fully explain the difference in performance, since public pension funds underperform endowments in their out-of-state investments as well.

Third, we explore the possibility that the superior performance of endowments or public pension funds results from preferential access to better funds. We test this hypothesis in several ways. First, as noted above, we examine the reinvestment decisions of LPs. Once an LP has invested in a fund, it generally has access to the subsequent funds raised by the GP. We find that, even if we condition only on reinvestments, endowments exhibit much better performance than all other LPs, suggesting that they are better able to predict future

[4] We thank the referee for suggesting this analysis.

performance of the GP. In addition, we want to rule out the possibility that top performing GPs try to "upgrade" their investor base and allow preferential access to endowments that are considered prestigious LPs.[5] To address these concerns, we construct two variables that proxy for the ease of access to a fund; namely, an indicator as to whether a fund was oversubscribed, and the time a fund took to reach its target fundraising. The idea is that GPs that are able to raise a follow-on fund above the target size (and those that close very quickly) have excess demand and can therefore be more selective in who they allow to invest. If endowments even outperform in funds that do not seem access-constrained, it would suggest that differential access by itself cannot explain our results. When we compare the returns for endowments and other types of LPs on investments in both undersubscribed funds and funds that take a long time to close, we find that endowments still outperform other LPs. While these findings do not support the idea that the superior performance of these LPs is merely driven by historical accident, we cannot completely rule out that some of the performance difference is due to their access to superior funds.

This paper is related to the literature on the decision to invest in and the performance of private equity funds. Poterba (1989) and Gompers and Lerner (1998) explore how tax and other public policies affect VC fundraising. Gompers and Lerner (1996) and Lerner and Schoar (2004) examine the contracts entered into between investors and funds, and how they are affected by the nature of both the targeted investments and the LPs. Mayer, Schoors, and Yafeh (2003) analyze the sources of VC financing across countries and how these are correlated with investment choices. Kaplan and Schoar (2005) study how the level of returns affects the ability of private equity groups to raise follow-on funds. The paper closest to our own is probably that of Gottschalg, Phalippou, and Zollo (2003), who highlight the puzzlingly low performance of private equity funds raised between 1980 and 1995. To date, however, the drivers and consequences of the decisions by individual LPs to invest in private equity funds have been unexplored.

The organization of this paper is as follows. Section I summarizes the data used in the analysis. Section II presents the analysis of performance differences across LP classes. Section III examines reinvestment decisions, and Section IV addresses some of the alternative interpretations of our findings such as differences in risk profiles, objective functions, and access to funds. Finally, Section V concludes the paper.

I. The Data

Before describing the data set, we briefly discuss the nature of private equity investing. The bulk of institutional investment in private equity is effected through separate funds run by professional managers (the GPs). The selection of appropriate direct investments requires resources and specialized human capital that few institutional investors have. The funds are raised for a specified

[5] We thank Paul Gompers for pointing out this possibility.

period (typically a decade, though extensions may be possible) and are governed by an agreement between the investors (the LPs) and the principals (the GPs) in the fund. The agreement specifies the nature of the fund's activities, the division of the proceeds, and so forth. Private equity groups will typically raise a fund every few years, beginning the fundraising process as the previous fund nears completion.

In the remainder of this section, we describe the data sources we employ in this paper. Note that the greater disclosure of private equity investments in recent years has allowed us to undertake this study.

Investment Decisions. To ascertain which institutional investors invest in which private equity funds, we employ two sources. Our primary source is Asset Alternatives' compilation of private equity investors. Since 1992, Asset Alternatives has compiled data on investors in private equity funds through informal contacts with the funds and investors themselves. This information is included as part of their *Directory of Alternative Investment Sources*, though the underlying data have not hitherto been made available to researchers. While the database is not comprehensive, it covers a large and diverse fraction of the private equity industry. Our second source comes from the investors themselves. Numerous public pension funds disclose the funds in which they invest. We obtain this information from annual reports or through written request from the LPs directly. In addition, a number of private investors with whom the authors have personal relationships provided us confidential listings of their investments. We obtain detailed information about these portfolio allocations from 20 different institutional investors.

Fund Characteristics. We collect information on the fund's size and stage, the previous funds raised, etc., from the Asset Alternatives funds database (included as part of their *Galante's Venture Capital and Private Equity Directory*, which, again, has not been shared previously with researchers), supplemented by Venture Economics' (VE) online funds database. We distinguish between the overall count of the fund and its sequence within a particular family of funds. In total, our database covers 838 separate funds that belong to an LP portfolio in our sample.

Fund Returns. We use a measure of performance that is very widely used for private equity funds, namely, the IRR. Our primary source for return data is Private Equity Intelligence's *2004 Private Equity Performance Monitor*, which presents return data on over 1,700 private equity funds. This information is compiled by Mark O'Hare, who over the past 5 years has created a database of returns from public sources, Freedom of Information Act requests to public funds, and voluntary disclosures by both GPs and LPs.[6] Private Equity Intelligence makes its own assessment of the reliability of the different sources of performance data available (i.e., reported by LPs and/or GPs, or calculated internally based on realized cash flows and valuations of LPs' remaining interests in the funds), and presents the figure considered most reliable. Ideally,

[6] O'Hare has been highly successful at gathering data not only on the returns of new funds, but also on the returns of many of the more established funds in the industry.

we would like to be able to calculate the IRR figures ourselves using cash flow data, but unfortunately we do not have access to this level of detail on funds' cash flows. We cross-check and supplement the Private Equity Intelligence data with the return data that we gather from public sources. Note that we only use in our analysis funds established prior to 1999, since this performance metric is unlikely to be very meaningful for younger funds.[7] (In unreported analyses, we verify the results for a sample of funds raised prior to 2002.) IRRs are reported net of fees and carried interest.

A potential problem with using IRR is that it is a nonlinear measure of performance. If the variance in returns is very different between classes of funds, it could potentially bias our results. To address this concern, we repeat all the analyses in our paper using two alternative measures of fund performance: (1) the value of actual distributions received by the LPs (expressed as a multiple of the fund's committed capital), and (2) the stated value of the fund plus the value of all distributions (again expressed as a multiple of committed capital). All the results are unchanged. We also find that the correlations between the three different performance measures are very high, between 75% and 98%.[8]

Institutional Investor Characteristics. We compile information on the overall size of the assets managed by the LP, the length of each institution's experience with private equity investing, and its geographic location from Venture Economics' *Directory of Private Equity Investors* and Asset Alternatives' *Directory of Alternative Investment Sources.*

II. Analysis of Performance Differences

In this section, we begin by presenting some descriptive statistics about the data set we described earlier. We then turn to presenting the central puzzle: the dramatic difference in returns across classes of LPs. We also consider in this section the impact of market cycles and the robustness of the results.

A. Descriptive Statistics

Table I presents descriptive statistics of the 838 funds in our sample of funds raised between 1991 and 1998 and the 352 LPs in these funds. Panel A of Table I shows statistics of the funds, broken down into three categories: early-stage VC, later-stage VC, and buyout funds.

Fund Characteristics. The average fund in our sample is $313 million, but there is large heterogeneity across funds. The smallest fund is $4.5 million,

[7] It is well known that IRRs with a large component relating to unrealized portfolio valuations are highly subjective. This element of subjectivity is particularly prominent in the early years of a fund's life, and is not resolved until the realization of all the fund's assets. By using only funds that closed before 1999, we ensure that at least 5 years of cash flows have been realized.

[8] Among the funds in our sample, the mean ratio of distributions to committed capital is 1.35. The ratio of total value to committed capital is 1.63, with the difference representing nonexited investments. These nonexited holdings represent the part of the IRR calculation that is more subjective.

Table I

Descriptive Statistics

This table presents descriptive statistics for the sample of 838 funds raised between 1991 and 1998 and the 352 LPs who invested in these funds, as compiled by Asset Alternatives. Panel A summarizes fund characteristics of 838 distinct funds raised between 1991 and 1998 according to the type of fund (early-stage VC, later-stage VC, and buyout funds). *Excess IRR* is the IRR minus the median IRR of the portfolio formed for each fund category every year. Geographical location by region follows the U.S. Census classification of states: *West* includes California; *Northeast* includes Massachusetts, New York, Pennsylvania; *South* includes Texas; *Midwest* includes Illinois, Ohio. Panel B summarizes the overall investment characteristics as of 2002 of 352 LPs who invested in those 838 funds, split into different LP classes (public pension funds, corporate pension funds, endowments, etc). *Percentage committed to VC funds* includes both early-stage and later-stage VC investments. Percentages committed to VC funds and to buyout funds do not add up to 100% because LPs also invest in other types of specialized private equity funds, such as oil, gas, and energy, real estate, or venture leasing funds, which are not covered by our analyses. Panel C shows the frequency distribution of the 838 funds by vintage year, split into early-stage VC, later-stage VC, and buyout funds. Panel D shows mean characteristics of 341 out of the 838 funds for which performance data are available from *Private Equity Performance Monitor*, compared to the entire sample of funds closed between 1991 and 1998. Standard deviations are in parentheses below the means. Panel E shows a comparison of average IRR for funds in our sample and funds in the Venture Economics database, grouped by vintage years. Cumulative IRRs since inception are calculated as of September 2003 (in our sample, the observation date varies slightly).

Panel A: Descriptive Statistics—Funds

	Overall						Early-Stage VC Funds					
	N	Mean	Std. Dev.	Min.	Med.	Max.	*N*	Mean	Std. Dev.	Min.	Med.	Max.
Total closing (MMS)	838	313	574	4.5	129	6,000	183	88	65	5.2	75	299
Overall fund sequence number	833	3.6	3.6	1	3	32	181	3.4	2.1	1	3	11
Closing year	838	1996	2.1	1991	1996	1998	183	1996	2.1	1991	1996	1998
Internal rate of return (%)	341	23.9	59.1	−94.2	10.5	513	71	60.5	99.6	−66.8	27.9	513
Excess IRR (%)	332	11.0	54.9	−90.5	0.8	493	69	40.2	96.1	−62.5	6.5	493
Number of LPs investing in fund	838	5.5	5.9	1	3	46	183	4.8	4.0	1	4	18
Geographical location of U.S.-based funds:												
West	672	0.27		0		1	160	0.55		0		1
Northeast	672	0.50		0		1	160	0.27		0		1
South	672	0.12		0		1	160	0.12		0		1
Midwest	672	0.11		0		1	160	0.06		0		1

(continued)

	Later-Stage VC Funds						Buyout Funds					
	N	Mean	Std. Dev.	Min.	Med.	Max.	*N*	Mean	Std. Dev.	Min.	Med.	Max.
Total closing (MM$)	336	196	248	4.5	122	1,850	319	564	833	10	253	6,000
Overall fund sequence number	333	4.2	4.3	1	3	32	319	2.9	3.2	1	2	28
Closing year	336	1995	2.2	1991	1996	1998	319	1996	2.0	1991	1996	1998
Internal rate of return (%)	134	25.6	45.2	−38.8	14.4	268.4	136	3.1	21.8	−94.2	3.1	57.9
Excess IRR (%)	129	9.0	42.8	−78.4	0.6	248.8	134	−2.1	19.8	−90.5	−0.75	45.5
Number of LPs investing in fund	336	5.1	5.5	1	3	33	319	6.4	7.1	1	4	46
Geographical location of U.S.-based funds:												
West	273	0.24		0		1	239	0.12		0		1
Northeast	273	0.52		0		1	239	0.63		0		1
South	273	0.11		0		1	239	0.12		0		1
Midwest	273	0.13		0		1	239	0.13		0		1

Panel B: Descriptive Statistics—Mean Characteristics of LPs, by Class of LP

	N	Year of Establishment of Private Equity Investment Program	Total Funds under Management (MM$)	Total Private Equity Commitments (MM$)	Percentage Committed to VC Funds (%)	Percentage Committed to Buyout Funds (%)	Number of Funds in Which LP Invested
Public pension funds	66	1986	26,380	2,320	33%	37%	34.9
Corporate pension funds	66	1986	11,731	652	47	31	11.4
Endowments	87	1985	1,698	206	43	23	16.2
Advisors	48	1986	4,811	3,654	43	35	32.8
Insurance companies	29	1982	33,711	1,198	33	32	20.3
Banks and finance companies	23	1984	92,513	655	27	60	23.9
Other investors	33	1989	1,236	155	43	39	6.8
Overall	352	1986	19,167	1,253	39%	33%	21.0

Panel C: Frequency Distribution of Fund Observations by Vintage Year and Type

	1991	1992	1993	1994	1995	1996	1997	1998	All Years
Early-stage VC funds	8	15	11	24	19	21	44	41	183
Later-stage VC funds	22	20	31	36	49	43	66	69	336
Buyouts funds	8	19	28	41	35	41	72	75	319
Overall	38	54	70	101	103	105	182	185	838

(continued)

Table I—*Continued*

Panel D: Availability of Fund Performance Data

	All Funds with Performance Data in Sample, 1991–1998				All Funds in Sample, 1991–1998				Venture Economics Universe, 1991–1998		
	All Funds	Early-Stage VC Funds	Later-Stage VC Funds	Buyout Funds	All Funds	Early-Stage VC Funds	Later-Stage VC Funds	Buyout Funds	All Funds	VC Funds	Buyout Funds
Total closing (MM$)	435	117	222	812	313	88	196	564	139	69	331
	(700)	(72)	(217)	(971)	(574)	(65)	(248)	(833)	(350)	(164)	(580)
Sequence number	3.7	4.3	4.2	3.0	3.6	3.4	4.2	2.9	3.7	3.8	3.5
	(2.9)	(2.3)	(2.9)	(3.0)	(3.6)	(2.1)	(4.3)	(3.2)	(4.5)	(4.8)	(3.6)
Vintage year	1995	1995	1995	1996	1996	1996	1995	1996	1996	1996	1995
	(2.1)	(2.2)	(2.2)	(2.0)	(2.1)	(2.1)	(2.2)	(2.0)	(2.1)	(2.1)	(2.2)
Total number of LPs investing in fund	8.1	6.7	7.8	9.1	5.5	4.8	5.1	6.4			
	(6.8)	(4.6)	(6.7)	(7.6)	(5.9)	(4.0)	(5.5)	(7.1)			
Fraction first funds	18%	11%	15%	26%	26%	20%	22%	35%	37%	37%	36%
Fraction second funds	22%	11%	17%	32%	21%	19%	17%	27%	17%	17%	19%
Fraction third funds	18%	20%	18%	16%	18%	20%	17%	16%	10%	9%	12%
Number of observations	341	71	134	136	838	183	336	319	4,418	3,386	1,032
% of all funds in the sample	*41%*	*39%*	*40%*	*43%*							

Panel E: Comparison of Average IRR by Vintage Year between Funds in Our Sample and Funds in the Venture Economics Database

	Our Sample		Venture Economics Database	
Vintage Year	*N*	Mean	*N*	Mean
1991	17	21.2	25	16.9
1992	22	24.6	48	20.4
1993	31	28.5	67	21.4
1994	45	21.4	72	21.4
1995	42	46.7	74	32.8
1996	48	33.6	70	39.7
1997	66	26.0	115	25.4
1998	70	1.7	149	14.5
Overall	341	23.9	620	23.7

while the largest is $6 billion. Not surprisingly, buyout funds are much larger, with an average size of $564 million. The average fund is a fourth fund (the average sequence number is 3.6), but again there is substantial variation. The funds in our sample are concentrated on the East and West coasts, with 50% and 27% of the sample funds, respectively. The average performance for all funds in the sample is 24%; excess returns, that is, the fund's return net of the median IRR of the funds formed in that category and year, average 11%. This performance is comparable in magnitude to (but a little higher than) the average performance found in Gompers and Lerner (2001), Kaplan and Schoar (2005), and Jones and Rhodes-Kropf (2002).[9] Early- and later-stage VC funds in our sample have significantly higher performance than the buyout funds, at 60% and 25% versus 3%, respectively (on an unadjusted basis).

Composition of Limited Partners. Panel B of Table I describes the distribution of LPs in our sample and their characteristics. Endowments comprise the largest group, with 87 LPs, followed by public pension and corporate pension funds (66 each). When we differentiate among the different subclasses of endowments, we find that the majority of the endowments in our sample are private university endowments (55), followed by foundations (23) and public university endowments (9). There are 48 advisors in the sample, 29 insurance companies, 23 commercial and investment banks, and 33 LPs that cannot be classified in any of the above categories (among such LPs are investment agencies of foreign governments, VC departments of large corporations, and religious organizations). We also collect data on 17 of the 23 banks in our sample of LPs in order to group them into those with mainly retail banking businesses (10 banks) and those with relatively important income from corporate banking activities (7 banks). Advisors and public pension funds contribute the largest amounts of capital committed to the industry overall (averaging $3.6 billion and $2.3 billion committed to private equity investments, respectively).

Sample Period. Panel C shows the breakdown of vintage years for the funds in our sample. The number of funds in our sample increases over the 1990s. This is due to two different phenomena. First, the coverage of the Galante's database appears to become more comprehensive in the later part of the sample period. Second, the 1990s represent a period of massive growth of the private equity industry, in terms of the number of funds raised and the number of investors participating in the industry. To alleviate concerns that sample selection issues due to improved coverage of LPs over time might drive our result, we replicate our findings for the sample of 20 LPs for which we have their complete investment history.

Availability of Fund Performance Data. Panel D displays characteristics of funds for which we were able to collect performance data, compared to the entire

[9] The differences with Ljungqvist and Richardson (2003) are more substantial. This reflects the fact that the former sample is primarily from the 1980s (1992 and 1993 are the only years of overlap between the two samples). It also appears to reflect some selection effects among the 73 funds in those authors' sample, since the patterns that they report do not appear to conform to the more general trends identified by Venture Economics.

sample and the Venture Economics universe of funds with the same vintage years. IRR data are available for just over 40% of all funds in the sample across the various fund categories. The funds for which we have performance data tend to be slightly larger in size, have higher sequence numbers, and have more LPs investing in them.

Finally, in Panel E we check for potential selection bias in the reporting of returns in our sample. We find that the annual average IRRs in our sample are slightly higher than the corresponding IRRs in the Venture Economics database; however, the differences are not statistically significant (except for 1998, the most recent year in our sample). Moreover, the directions of the annual changes in performance in our sample are parallel to the VE data. While the average IRR might be slightly higher than in the VE sample, the more important question is whether there are *differential* biases among the LP classes. Given the way we construct our data set, it is difficult to believe that LPs selectively report their investments to Asset Alternatives since the latter collect their data from GPs and LPs alike and, in fact, their database does not include any performance measures. Thus, at the time of reporting the investments, the institutions could not have anticipated that we would be able to match performance data to their investments. Similarly, it is difficult to believe that the better performance of endowments is solely driven by selection bias, since we capture a larger fraction of endowments in our sample than of the other classes of LPs. Nonetheless, we also run sample selection regressions below to address this potential issue.

B. Performance Differences across LP Classes

Table II provides an overview of the investments made by each LP class, showing the different fund categories separately. There is enormous heterogeneity in the performance of funds in which different classes of institutions invest. The funds that endowments invest in have by far the best overall performance, with an average IRR of 44%. This high performance is entirely driven, however, by endowments' VC investments. On average, early- and later-stage VC funds that endowments invest in return an IRR of 95% and 36%, respectively. In contrast, the buyout investments of endowments only have an IRR of 2%. If we now break down endowments into the different types (public, private, and foundations), we find that foundations and private university endowments have higher IRRs than have public endowments.[10] All endowments perform relatively poorly in the buyout arena. This difference in performance across classes of private equity investments might suggest that endowments have specific human or organizational capital that allows them to outperform in the VC investments.

[10] This difference becomes particularly large when we form the weighted average IRRs discussed in footnote 11. Public endowments have a weighted average IRR of 20%, while private university endowments and foundations have weighted average IRRs of 43% and 41%, respectively. Interestingly, public university endowments perform much worse across all different types of private equity classes relative to other endowments once we weight by size.

Table II

Mean Fund Characteristics by Class of LP and by Fund Type

The table shows groupings of 4,618 investments by 352 LPs in 838 funds, and mean values of selected characteristics of those funds. *Fund size* refers to the total dollar value raised from all investors in the fund, *fund sequence number* refers to the order in which a fund was raised in the private equity firm's family of funds, *fund IRR* is the IRR of the fund reported by *Private Equity Performance Monitor* as of September 2003, *weighted fund IRR* is the IRR weighted by each LP's commitment to the fund as a proportion of its total commitments to private equity funds, and *excess IRR* is the IRR minus the median IRR of the portfolio formed for each fund category every year.

	Overall						Early-Stage VC Funds					
	N	Fund Size (MM$)	Fund Sequence Number	Fund IRR (%)	Weighted Fund IRR (%)	Excess IRR (%)	*N*	Fund Size (MM$)	Fund Sequence Number	Fund IRR (%)	Weighted Fund IRR (%)	Excess IRR (%)
Public pension funds	1,483	814	4.6	20.2	12.6	8.9	171	129	4.3	57.9	31.2	37.3
Corporate pension funds	572	740	4.2	13.5	12.0	4.1	89	110	3.8	36.9	33.5	15.7
Endowments	923	465	4.7	44.3	38.7	30.6	294	129	4.4	95.4	83.9	69.1
Private endowments	*597*	*494*	*4.8*	*46.1*	*43.1*	*32.7*	*203*	*131*	*4.6*	*92.3*	*86.6*	*65.9*
Public endowments	*129*	*478*	*4.8*	*40.1*	*20.2*	*27.7*	*28*	*135*	*4.7*	*97.8*	*50.2*	*74.3*
Foundations	*197*	*369*	*4.0*	*42.6*	*41.1*	*26.3*	*63*	*120*	*3.9*	*106.4*	*103.5*	*78.3*
Advisors	732	716	4.5	23.8	21.2	16.5	166	130	4.0	69.7	66.1	51.2
Insurance companies	385	429	3.7	20.1	15.0	8.3	85	102	3.6	47.2	32.4	27.7
Banks and finance companies	363	699	3.4	4.3	3.0	−0.2	31	106	3.3	17.3	14.1	2.8
Mainly retail banking	*214*	*673*	*3.5*	*6.0*	*4.2*	*2.3*	*23*	*103*	*3.1*	*11.5*	*8.1*	*1.4*
Substantial corporate segment	*90*	*1,003*	*3.3*	*−3.1*	*−3.1*	*−5.6*	*4*	*114*	*3.5*	*67.5*	*67.5*	*40.5*
Indeterminate bank type	*59*	*330*	*3.0*	*9.9*	*8.9*	*−2.4*	*4*	*113*	*4.0*	*21.0*	*17.8*	*−9.5*
Other investors	160	315	3.8	14.5	16.2	6.3	48	69	3.1	15.8	16.8	2.9
Overall	4,618	661	4.4	23.7	18.8	13.3	884	121	4.1	68.9	56.0	47.0

(*continued*)

Table II—*Continued*

	Later-Stage VC Funds						Buyout Funds					
	N	Fund Size (MM$)	Fund Sequence Number	Fund IRR (%)	Weighted Fund IRR (%)	Excess IRR (%)	*N*	Fund Size (MM$)	Fund Sequence Number	Fund IRR (%)	Weighted Fund IRR (%)	Excess IRR (%)
Public pension funds	589	332	5.7	26.3	17.0	9.9	723	1,368	3.8	6.7	4.8	1.5
Corporate pension funds	195	273	5.3	21.3	18.1	5.5	288	1,251	3.6	3.5	3.3	0.3
Endowments	335	310	5.5	35.9	30.3	22.1	294	978	3.9	2.1	2.7	1.5
Private endowments	*201*	*330*	*5.8*	*36.9*	*34.1*	*24.6*	*193*	*1,046*	*4.1*	*4.3*	*4.5*	*4.4*
Public endowments	*63*	*296*	*5.5*	*31.6*	*15.2*	*18.1*	*38*	*1,033*	*3.6*	*−2.5*	*−0.7*	*−3.0*
Foundations	*71*	*268*	*4.7*	*37.4*	*34.8*	*19.1*	*63*	*734*	*3.5*	*−0.9*	*0.0*	*−3.9*
Advisors	269	330	5.6	27.3	22.3	18.5	297	1,394	3.7	−2.6	−2.4	−3.0
Insurance companies	143	278	4.4	26.0	20.9	10.8	157	742	3.1	1.0	0.4	−3.7
Banks and finance companies	109	286	3.5	10.8	8.0	2.7	223	983	3.3	−0.2	−0.5	−1.9
Mainly retail banking	*70*	*299*	*3.8*	*12.4*	*9.7*	*6.9*	*121*	*998*	*3.4*	*1.2*	*0.2*	*−0.2*
Substantial corporate segment	*17*	*316*	*2.6*	*−6.0*	*−6.0*	*−8.3*	*69*	*1,224*	*3.5*	*−4.6*	*−4.6*	*−6.5*
Indeterminate bank type	*22*	*223*	*3.3*	*14.4*	*9.4*	*−8.1*	*33*	*427*	*2.7*	*5.4*	*7.2*	*2.2*
Other investors	64	158	5.5	29.3	32.7	15.3	48	772	2.5	−3.1	−3.1	−3.1
Overall	1,704	307	5.3	27.4	21.2	13.5	2,030	1,194	3.7	3.2	2.4	0.0

The picture looks quite different for public pension funds and advisors (and to some extent, insurance companies). On average, the funds that these classes of LPs invest in have more moderate IRRs (20% and 23%, respectively), but the drivers of the positive returns are less skewed towards VCs, especially in the case of public pension funds. Finally, we see that the funds picked by corporate pension funds and especially banks have very poor performance on average (IRRs of 13% and 4%, respectively). This trend seems to hold across all different types of private equity investments. Note that we must be somewhat cautious in interpreting these findings, since this calculation does not reflect the actual size of the allocations to each of the different funds. Rather, this exercise represents the ability of different groups of LPs to identify (good) funds on average.[11]

Table II also reveals that public and corporate pension funds tend to invest in larger funds, whereas endowments and insurance companies invest in smaller funds. Interestingly, we see that the smaller fund size for endowments is driven by their allocations to small buyout funds and the greater share of VC funds in their portfolio: The VC funds they invest in are larger than average. We find that insurance companies and banks tend to invest in early funds (lower sequence numbers) across all fund categories.

C. Regression Analysis

A natural question to ask is whether these univariate results are robust to controlling for the time period during which the investments were made, or the choice between VC and buyout funds. For these and subsequent analyses, we analyze investments at the LP-fund level, that is, we use each investment by an LP in a fund as a separate observation. We control for the fact that we have multiple observations by clustering the standard errors at the fund level. We regress the realized IRR of a fund on a set of dummies for the different classes of LPs and control variables for year fixed effects, fund category fixed effects, the year the LP's private equity investment program was launched,[12] and the geographical colocation of the GP and LP. Public pension funds are the omitted category from the set of LP dummies.

In Table III, column (1) shows that funds in which endowments invest outperform public pension funds, while on average other LPs pick funds that underperform relative to those groups. In particular, corporate pension funds and banks invest in funds with significantly lower IRRs. Column (2) shows that these results are virtually unchanged if we include year fixed effects in the regression. Thus, the results are not driven simply by the timing of investments.

[11] We also estimate the investment performance of LPs by assigning weights to the returns from each fund by the amount committed to the fund in relation to the LP's total private equity commitments. For all remaining funds in an LP's portfolio for which the commitment amount is not known, we simply assume that the LP invests an equal amount in each fund. The results indicate that the pattern of performance differences across LP classes changes little.

[12] The vintage is expressed relative to that of the median LP in the sample, which began its private equity program in 1987. Thus, a program begun in 1991 would be coded as +4.

Table III

Fund Performance Regressions

This table reports the results of pooled regressions of fund IRR on dummy variables for LP classes and control variables. The sample consists of 4,618 investments by 352 LPs in 838 funds closed between 1991 and 1998, as compiled by Asset Alternatives. Several versions of the following pooled regression are run and coefficient estimates and standard errors are reported by columns in the table:

$$FundIRR_{ij} = \beta_0 + \sum_k \beta_{0k} DummyLP_{jk} + \sum_k \beta_{1k} DummyLP_{jk} \times FundInflow_{ij} + \sum_k \beta_{2k} DummyLP_{jk} \times LPvintage_j + \beta_3 D\_sameregion_{ij} + controls.$$

$FundIRR_{ij}$ is the IRR of fund i in %. Six dummy variables identify the class of LP for each LP-fund pair, with $DummyLP_{jk}$ taking a value of one for each observation consisting of an investment in fund i by LP j belonging to LP class k, and zero otherwise. "Public pension funds" is the base LP class, with zero values for all LP dummy variables. $FundInflow_{ij}$ is the year-over-year change in the amount of fund inflows into private equity in the country-year of the closing of fund i, and is a proxy for market conditions. $LPvintage_j$ is the year of establishment of the private equity program at LP j relative to that of the median LP in the sample, which began its private equity program in 1987. $D\_sameregion_{ij}$ is a dummy variable that equals one if both LP j and the private equity firm managing fund i are headquartered in the same region in the U.S. (Midwest (includes Illinois and Ohio), Northeast (includes Massachusetts, New York, and Pennsylvania), South (includes Texas), and West (includes California)), and zero otherwise. Robust standard errors allowing for data clustering by funds in all the regressions are shown in brackets below the coefficient estimate. Intercepts are not reported.

For the Heckit regression in column (7), the first-stage probit regression is:

$$Prob\,(Fund_{ij} \text{ has observable IRR data} \mid \text{independent variables}) = \Phi\left(\begin{array}{l}\gamma_0 + \sum_k \gamma_{1k} DummyLP_{jk} + \gamma_2 FundSize_i + \sum_m \gamma_{3m} GPRegion_{im} \\ + \sum_n \gamma_{4n} FundCategory_{in} + \sum_o \gamma_{5o} FundVintageYear_{io}\end{array}\right)$$

where $FundSize_i$ is the natural logarithm of the total closing amount of fund i in MM\$, and $GPRegion_{im}$ is a dummy variable for the geographical location of the GP managing fund i. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Dependent variable: Fund IRR	(1)	(2)	(3)	(4)	(5)	(6)	Heckit (7)
Dummy for LP class: (comparison category is public pension funds)							
Corporate pension funds	−6.26**	−6.59**	−7.80**	−10.99***	−9.91***	−7.39	−6.76**
	(2.78)	(2.83)	(3.70)	(4.16)	(3.75)	(6.01)	(3.25)
Endowments	11.92**	11.58***	9.04**	9.35**	9.78**	25.12***	13.58***
	(4.89)	(4.37)	(4.26)	(4.33)	(4.39)	(8.12)	(4.78)
Advisors	−1.96	2.92	2.95	3.64	1.86	26.51**	5.24
	(3.29)	(2.85)	(5.12)	(5.72)	(5.41)	(10.64)	(3.21)
Insurance companies	−4.87	−5.65	−3.44	−3.96	−4.95	7.40	−3.81
	(3.95)	(3.89)	(4.33)	(4.64)	(4.59)	(8.60)	(4.58)
Banks	−11.23***	−9.05***	−4.91	−1.08	−5.80	−11.22	−7.49**
	(2.85)	(2.96)	(4.50)	(6.20)	(4.49)	(9.29)	(3.66)
Other LPs	−7.82	−7.90	−31.29***	−28.04***	−27.59**	−40.71**	−4.63
	(5.12)	(5.03)	(9.98)	(7.00)	(10.97)	(15.62)	(4.86)
LP and GP in same region			−7.34***	−7.14***	−6.80***	−6.31***	
			(2.38)	(2.38)	(2.33)	(2.30)	
LP vintage			0.35	0.85**	0.31	0.71	
			(0.22)	(0.43)	(0.23)	(0.43)	
LP size (log of total commitments to private equity)			−0.80	−0.42	−0.83	−0.38	
			(0.69)	(0.73)	(0.78)	(0.79)	
Total private equity fund inflow					−31.57***	−23.05***	
					(6.69)	(6.43)	

(continued)

Table III—*Continued*

Dependent variable: Fund IRR	(1)	(2)	(3)	(4)	(5)	(6)	Heckit (7)
Interaction effects:							
Corporate pension funds * LP vintage				−1.60**		−0.99	
				(0.72)		(0.65)	
Endowments * LP vintage				−0.69		−0.44	
				(0.61)		(0.61)	
Advisors * LP vintage				−0.24		−0.07	
				(0.83)		(0.83)	
Insurance companies * LP vintage				−0.68		−0.86	
				(0.81)		(0.86)	
Banks * LP vintage				0.85		−0.68	
				(1.55)		(1.29)	
Other LPs * LP vintage				−1.27		−2.27	
				(1.39)		(1.53)	
Corporate pension funds * inflow						−8.05	
						(10.60)	
Endowments * inflow						−30.71**	
						(12.37)	
Advisors * inflow						−48.23***	
						(15.57)	
Insurance companies * inflow						−28.28**	
						(13.93)	
Banks * inflow						9.62	
						(13.16)	
Other LPs * inflow						38.49	
						(24.64)	
Heckit Lambda							−7.82*
							(4.22)
Fund category fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	Yes	Yes	Yes	No	No	Yes
LP region dummies	No	No	Yes	Yes	Yes	Yes	No
R-squared	19.8%	26.9%	23.5%	23.8%	20.9%	22.0%	n/a
Number of observations	2,755	2,755	1,582	1,582	1,531	1,531	4,402

To understand the difference in the performance of endowments in more detail, we also replicate this regression such that we distinguish between private university endowments, public university endowments, and foundations. In untabulated results, we find that the superior performance of endowments overall is predominantly driven by the private universities. The public universities and foundations have positive but statistically insignificant coefficients.

In column (3), we include controls for whether the LP and GP are in the same region of the United States and the age of the private equity program of the LP. We also add several other LP-specific controls, such as the logarithm of the LP size (measured as committed capital) and dummies for the region in which the LP is located. We find that most of the main results described above are not affected by the inclusion of these controls. However, the negative coefficient on the bank dummy becomes smaller and statistically insignificant.[13]

[13] One could imagine that there are severe capacity constraints in the industry, for example, in terms of how much an LP can invest in a given fund and at what pace new fund managers enter. Under this model, larger endowments might be forced to experiment more and invest in

The coefficient on LP vintage is positive but insignificant. We interact the LP class dummies with the vintage of the LP's private equity investment program to understand if those LPs that started investing in private equity earlier display different performance from those that started to invest later. We find negative coefficients on the interaction terms for most LP classes. In particular, among corporate pension funds, those LPs that started investing in private equity earlier have significantly higher IRRs.

D. Importance of Market Cycles

To analyze how sensitive fund returns are to market cycles, in column (5) of Table III we replace year fixed effects with a measure of the aggregate annual inflow of capital into the industry. From earlier papers by Gompers and Lerner (1998, 2000) and Kaplan and Schoar (2005), we know that capital flows and returns in private equity are extremely cyclical. Therefore, our measure of industry capital flows can be interpreted as a proxy for the ability of funds to time the market. The coefficient on the aggregate inflow of capital is negative and highly significant.[14] As aforementioned, we interact the LP class dummies with the measure of aggregate capital inflow. Column (6) shows that the coefficient on the interaction term between LP class and aggregate inflow of capital is negative and highly significant in general, but particularly so for advisors, endowments, and insurance companies. These LPs have significantly lower returns if they invest during periods of high capital inflows into the industry, which is consistent with investor herding behavior when the market is "hot." Interestingly, even endowments do not seem to be exempt from this effect.

E. Additional Robustness Checks

We replicate the results in Table III using excess IRR as the performance measure. Excess IRR is measured as the fund's own IRR minus the median IRR of all private equity funds in that year and category. These results are reported in Table A.1 of the Appendix. The results are equivalent to the results reported above. We also repeat our analysis for all funds that we can identify that were raised between 1991 and 2001 (1,397 funds in total) and obtain very similar results.

To address the concern that a few lucky LPs drive the returns of the different LP classes, we undertake two unreported analyses. First, we calculate median instead of mean performance for the different LP classes. We find that the endowments still significantly outperform the rest of the LPs, but the estimated coefficient is much smaller (only about 9% instead of 21%). This is not surprising since the distribution of private equity returns is highly positively

new fund managers to secure the future choice of GPs. The need for this type of investment might further depress the performance of large LPs, since we know from Kaplan and Schoar (2005) that on average first-time funds underperform the industry.

[14] This pattern continues to hold when we employ other proxies, such as the inflows into VC funds only or the level of the NASDAQ. We employ similar alternative controls in subsequent analyses.

skewed. We also rerun our regressions on LP performance excluding the 1% of funds with the highest IRRs. This allows us to get rid of some of the truly lucky draws that LPs might have had without eliminating the entire positive tail of the return distribution. When we run these regressions, we find that endowments again significantly outperform all other LP classes, while banks and corporate pension funds underperform. Overall this suggests that endowments consistently realize better performance than other LPs, and this difference is not driven simply by a few lucky investments.[15]

Another concern is that the missing returns data may vary in a systematic manner. For instance, as noted above, returns data were more likely to be missing for smaller funds. If banks were more adept at investing in small funds, it might be that their level of underperformance was less than what appeared in the reported regressions. To address this concern, we run a Heckit sample selection regression, as reported in column (7) of Table III (see, e.g., Carhart et al. (2002)). The results are virtually unchanged.[16]

Lastly, to address concerns about multiple observations in the sample (typically several LPs invest in the same fund), we turn to an alternative empirical approach whereby we collapse the data at the fund level (Appendix Table A.2). We use the number of LPs of each class that invests in a given fund in our sample as explanatory variables for fund performance, together with fund size and controls for year fixed effects and fund category effects. The results from this exercise reconfirm our earlier findings in Table III.[17]

III. Differences in Reinvestment Decisions of LPs

In the subsequent analyses, we try to explain what drives the above differences in the performance of LPs. One of the most important decisions for LPs is whether they reinvest in the next fund of a partnership or not. Reinvestment decisions of LPs are particularly important in the private equity industry, where

[15] We also investigate whether there are significant differences across LPs within a class. While we find significant LP-specific fixed effects, the distributions of fixed effects are relatively tight around the mean, in particular for the endowments. This implies that the performance differences across LP classes are not driven just by a few individual investors who happen to be lucky or unlucky.

[16] The independent variables used for the first-stage probit are: dummies for the different LP classes, fund size, GP region, fund category, and fund vintage year. All of the LP class dummies as well as fund size are significant, with coefficient estimates (standard errors in parentheses) as follows: corporate pension funds −0.21 (0.08), endowments −0.26 (0.09), advisors −0.19 (0.07), insurance companies −0.56 (0.10), banks −0.43 (0.10), other LPs −0.53 (0.13), and fund size 0.36 (0.08). These results suggest that IRR data are more likely to be available for funds in which public pension funds invest and for larger funds.

[17] Another concern might be that these results are driven by salary differentials. To pick two extremes, the endowments of private universities are frequently reputed to be far more generous than state pension funds. The salaries of internal investment staff are not reflected in the stated returns. Could the differential in performance be substantially eroded, once the endowment's higher salaries are factored in? A few illustrative calculations can show that the answer appears to be decisively no, because of the relatively small sizes of these staff and the still relatively modest salaries. (If the performance differential across LPs were due to the differential skills of small teams, the patterns noted above would suggest that the labor market for LPs is inefficient.)

information about the quality of different private equity groups is more difficult to obtain and is often restricted to existing investors (see Lerner and Schoar (2004) for a discussion of asymmetric information in private equity).

For each fund in our sample, we identify whether the private equity organization raises a follow-on fund of the same type. For each LP investing in the fund, we then determine whether the same LP reinvests in the follow-on fund. In this way, we make sure that we do not miscode situations in which no follow-on fund was raised as a decision not to reinvest. Panel A of Table IV shows the reinvestment outcomes by class of LP and fund type. Public pension funds and insurance companies are most likely to reinvest in the next fund of a given partnership (59% and 54%, respectively). They are followed by endowments and advisors, who reinvest about 50% of the time when presented with reinvestment opportunities, while the likelihood of corporate pension funds and banks reinvesting is only 39%. Interestingly, endowments and advisors differ in their reinvestment rates across different fund categories. They are more likely to reinvest in venture funds than in buyout funds. Most other LPs do not show pronounced differences in reinvestment rates across fund categories. Moreover, funds in which endowments have an opportunity to reinvest have a much higher average IRR than those of other classes of LPs. Again, these higher average IRRs are driven especially by VC funds. By contrast, the funds that banks and corporate pension funds can reinvest in show particularly poor performance.

Panel B of Table IV explores some of the consequences of LPs' reinvestment decisions. We find that, across all LP classes, there are significant performance differences between funds in which LPs do and do not reinvest. We see that, overall, LPs tend to reinvest in the next fund of the partnership if the current fund has a high IRR. In those instances in which LPs decide not to reinvest, on average the IRR of the current fund is significantly lower (on the order of 9%). The same pattern holds when we look at the IRRs of the subsequent fund. Funds in which LPs reinvest have significantly higher performance than those in which they do not reinvest (14% vs. 3%, respectively). In the first seven rows of Panel B of Table IV, we break out the reinvestment decisions by class of LP. Interestingly, we see that public pension funds, advisors, and insurance companies tend to reinvest when *current* fund performance is higher. In contrast, there is no significant difference in the current performance of partnerships in which endowments decide to reinvest versus those in which they do not (50% vs. 54%). However, this picture reverses when we look at the performance of the next fund. Funds in which endowments (and to a lesser degree, public pension funds) decide to reinvest have much higher performance than those in which they decide not to reinvest (43% vs. 17%). They appear to be able to select funds that maintain their high performance and avoid those that will have lower performance going forward. Moreover, endowments tend to reinvest when current funds are smaller in size. Public pension funds show a similar ability to differentiate between good and bad performers, but at a much lower average performance level. Advisors also appear to follow a similar approach of reinvesting when the current fund is smaller, but are less successful

Table IV

Reinvestment Decisions by LPs

This table summarizes reinvestment decisions faced by LPs when considering follow-on funds within same-family funds raised by a given GP. The sample consists of 2,716 reinvestment opportunities identified by reference to the sequence number of funds within the same fund family. Panel A shows characteristics of 2,716 reinvestment opportunities faced by 300 LPs in 388 funds that had a follow-on fund within the same family. *Reinvested* is a dummy variable taking the value one if the LP reinvested and zero if investment in the follow-on fund was "discontinued" by the LP. *Current fund IRR* is the IRR of the current fund (in which the LP has invested) that has a follow-on fund (in which the LP may or may not have invested), *next fund IRR* is the IRR of the follow-on fund, *current fund size* represents the total dollar value raised from all investors in the current fund, and *change in fund size* is the percentage change in fund size from the current fund to the follow-on fund. Panel B shows characteristics of the 2,716 reinvestment opportunities, split according to whether the LP decided to reinvest or not. Variable definitions are the same as in Panel A. *Excess IRR* is the IRR minus the median IRR of the portfolio formed for each fund category every year. Also reported in Panel B are p-values from t-tests of differences in the means between reinvested and non-reinvested funds. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: Reinvestment Opportunities by Class of LP and by Fund Type

	Overall						Early-Stage VC Funds					
	N	Reinvested (Yes = 1; No = 0)	Current Fund IRR (%)	Next Fund IRR (%)	Current Fund Size (MM$)	Change in Size, Current to Next Fund (%)	N	Reinvested (Yes = 1; No = 0)	Current Fund IRR (%)	Next Fund IRR (%)	Current Fund Size (MM$)	Change in Size, Current to Next Fund (%)
Public pension funds	856	0.59	26.7	6.8	792	101	150	0.49	60.9	22.9	136	113
Corporate pension funds	317	0.38	16.7	3.5	760	95	72	0.38	38.7	22.7	123	98
Endowments	636	0.49	52.7	30.7	417	95	256	0.57	94.9	69.4	134	99
Advisors	463	0.50	33.4	1.5	691	106	137	0.58	72.1	16.1	139	137
Insurance companies	197	0.54	27.4	4.0	385	100	65	0.63	51.5	7.1	114	104
Banks and finance companies	175	0.40	6.3	−7.9	615	110	17	0.47	18.4	−14.2	139	137
Other investors	72	0.36	18.7	−0.7	369	158	26	0.42	25.2	−25.4	80	228
Overall	2,716	0.51	31.3	9.6	631	102	723	0.54	70.6	33.4	131	115

	Later-Stage VC Funds						Buyout Funds					
	N	Reinvested (Yes = 1; No = 0)	Current Fund IRR (%)	Next Fund IRR (%)	Current Fund Size (MM$)	Change in Size, Current to Next Fund (%)	N	Reinvested (Yes = 1; No = 0)	Current Fund IRR (%)	Next Fund IRR (%)	Current Fund Size (MM$)	Change in Size, Current to Next Fund (%)
Public pension funds	263	0.63	33.5	10.0	309	94	443	0.60	9.9	−0.8	1,301	102
Corporate pension funds	81	0.38	28.4	5.0	227	114	164	0.39	3.8	−4.5	1,303	84
Endowments	219	0.49	39.8	18.6	297	86	161	0.36	3.9	−2.1	1,028	102
Advisors	167	0.59	35.6	−4.9	332	111	159	0.33	−3.1	−3.0	1,545	76
Insurance companies	67	0.58	33	5.9	270	89	65	0.42	3.2	0.0	774	106
Banks and finance companies	50	0.42	15.6	−14.4	235	94	108	0.38	0.8	−4.3	865	114
Other investors	27	0.48	32.1	15.4	197	133.2	19	0.11	−3.5	−3.5	1,008	97
Overall	874	0.55	34.5	7.7	292	98	1,119	0.46	5.2	−2.1	1,219	97

(continued)

Table IV—*Continued*

Panel B: Consequences of Reinvestment Decisions by Class of LP

		Mean Current Fund IRR (%)	Mean Current Fund Excess IRR (%)	Mean Next Fund IRR (%)	Mean Next Fund Excess IRR (%)	Mean Current Fund size (MM$)	Mean Change in Size, Current to Next Fund (%)
Public pension funds	Reinvested	+31.1%	+15.9%	+11.0%	+12.9%	772.2	+115.3%
	Did not reinvest	+20.3%	+8.5%	0.4%	+6.4%	821.4	+80.6%
	t-test	0.014**	0.069**	0.006***	0.043**	0.476	0.000**
Corporate pension funds	Reinvested	22.0	9.8	2.1	6.3	685.5	102.0
	Did not reinvest	13.5	1.5	4.5	7.8	806.7	90.2
	t-test	0.119	0.076*	0.661	0.727	0.294	0.259
Endowments	Reinvested	50.5	30.9	43.7	38.9	310.1	92.7
	Did not reinvest	54.7	40.9	17.1	22.4	519.7	97.2
	t-test	0.601	0.179	0.002***	0.033**	0.000***	0.527
Advisors	Reinvested	41.1	27.6	1.0	11.8	526.5	111.6
	Did not reinvest	24.7	17.7	2.0	11.3	855.4	101.1
	t-test	0.038**	0.171	0.877	0.926	0.000***	0.462
Insurance companies	Reinvested	35.7	18.2	10.3	14.3	329.4	103.9
	Did not reinvest	16.8	6.7	−5.2	3.5	450.4	94.9
	t-test	0.078*	0.237	0.093*	0.140	0.135	0.424
Banks and finance companies	Reinvested	8.1	1.6	−4.3	0.6	555.8	113.9
	Did not reinvest	5.2	−1.0	−10.2	−1.4	654.1	108.0
	t-test	0.563	0.567	0.126	0.537	0.465	0.698
Other investors	Reinvested	39.3	24.7	14.0	20.5	195.3	96.8
	Did not reinvest	6.1	−3.9	−11.7	1.6	467.1	192.3
	t-test	0.005***	0.033**	0.046**	0.068*	0.078*	0.183
Overall	Reinvested	35.7	20.3	14.8	17.3	561.8	107.1
	Did not reinvest	26.8	15.9	3.7	10.2	702.2	96.5
	t-test	0.003***	0.104	0.000***	0.004***	0.000***	0.023**

Table V
Value-at-Risk Analysis

This table reports the results of an analysis of value-at-risk by LP class. Each column heading shows the common cutoff point used across all LP classes. The estimated probabilities that funds in which LPs of each class invested fall below the given threshold are shown in the columns. The sample consists of 2,755 investments by 352 LPs in 341 funds that closed between 1991 and 1998 for which IRR data are available.

	Negative IRR	IRR < 20%	IRR Falls in Lowest Quartile	IRR Falls in Lowest Half	IRR Falls in Bottom 3 Quartiles
Public pension funds	28.5%	64.4%	21.3%	48.3%	77.2%
Corporate pension funds	36.3%	70.7%	30.2%	53.5%	79.5%
Endowments	22.5%	50.1%	17.3%	37.1%	61.4%
Advisors	38.4%	66.7%	32.6%	56.1%	73.1%
Insurance companies	32.7%	66.7%	28.7%	50.3%	76.6%
Banks and finance companies	47.6%	80.0%	38.2%	69.4%	88.2%
Other investors	32.2%	72.9%	25.4%	49.2%	81.4%
Overall	31.3%	63.9%	24.9%	49.3%	74.4%

at picking the better-performing next funds. In short, some investors (especially endowments) appear far more able to benefit from and/or act on the inside information that being a LP provides.[18]

IV. Alternative Explanations

A. Differences in Risk Profiles

As previously mentioned, there are a number of alternative explanations for the observed heterogeneity in performance other than differences in sophistication. First, LPs might differ in the risk profile of the funds that they choose. For example, endowments could be systematically investing in riskier funds, and therefore enjoy higher returns. To address this concern, we control for a number of observable characteristics that are generally considered risk factors, such as the focus and maturity of the investments selected by a fund, and the fund's size, age, and location. While our results are robust to these controls, we cannot completely rule out the possibility that unobservable risk factors might affect our results.

Therefore, in Table V we also conduct a type of value-at-risk analysis, whereby we calculate for each class of LPs the likelihood that the IRR of a fund is negative, falls below 20%, or falls into the lowest quartile, the lowest half, or the

[18] In an unreported analysis, we estimate a linear probability model of reinvestment. Individual fund performance only has a weak impact on the reinvestment decisions. By way of contrast, market cycles have a much more significant effect on reinvestments: In times when more capital flows into the private equity industry, LPs are also more likely to reinvest. LPs tend to be more likely to reinvest if the GP is geographically proximate. Corporate pension funds and endowments are less likely to reinvest on average.

lowest three quartiles of funds ranked by performance. If endowments achieve their superior returns by taking on riskier investments, we should expect that they have a higher likelihood of having funds in the lowest performance quartiles. However, no matter which threshold we use, we see that endowments are not more likely to be investing in poorly performing funds. These results do not support the idea that endowments achieve their superior performance by relying on riskier investment strategies.[19]

B. Differences in Objective Functions

Most practitioners believe that the two classes of LPs that are most likely to diverge from a pure return maximization objective are banks and public pension funds. For example, Hellmann, Lindsey, and Puri (2004) suggest that banks might diverge from maximizing returns on investments in order to obtain future banking income from the portfolio firms. The cross-selling of services to the portfolio companies of the fund might justify accepting a lower return on the initial LP investment. For instance, many banks generate substantial profits from lending to firms undergoing leveraged buyouts or from advising on these transactions. As a result, they may invest in a buyout fund that they do not expect to yield high returns, if the investment is expected to increase the probability that they will generate substantial fee income from the group's transactions.[20]

This alternative explanation receives only limited support. First, Table II shows that banks underperform other LP classes not only in the buyout industry, where considerations about future business might be important, but even in their VC investments, where their prospects for generating future banking business are not as strong. Second, we address this question by collecting data on the fraction of revenues the different banks make from M&A and corporate lending activities. If the inferior returns of banks are purely driven by differences in their objective function, the lower performance should be most pronounced in banks that stand to gain a lot from those activities. We group the banks in our sample into those with mainly retail banking businesses and those with relatively important income from corporate banking activities. The tabulation in Table II shows that the latter banks perform slightly worse overall

[19] We also compare the standard deviations of returns across the different LP classes, and find that endowments are among the LP classes with higher variance. However, this variance is entirely driven by the positive skewness of the return distribution of endowments. Once we condition on the lower 75% of the funds across all LPs, we see that in fact endowments have the lowest variation across all LP classes.

[20] Banks were early investors in VC, and continue to be active today. Because their equity ownership of commercial enterprises was historically restricted, commercial banks typically invest in private equity through separately capitalized bank holding company subsidiaries. Under Section 4(c)(6) of the Bank Holding Company Act of 1956, bank holding companies may invest in the equity of companies as long as the position does not exceed more than 5% of the outstanding voting equity of the portfolio company, which is unlikely to be the case if the bank is just one of many LPs in a fund. In addition, many banks also make direct investments in private firms through licensed Small Business Investment Companies (SBICs). For a discussion of these issues, see Fenn, Liang, and Prowse (1995) and Hellmann, Lindsey, and Puri (2004).

than retail banks, and that this lower performance is due to their investments in later-stage VC and buyout funds. This lends some support to the idea that banks' lower performance may be partly due to their different objectives when investing in private equity funds, but this is unlikely to be the full explanation, since the retail banks still underperform the other LP classes by a large margin across all fund categories.

A second set of LPs that might diverge from a pure return motive are public pension funds, which often face obligations to invest in in-state funds in an effort to support the local economy. We therefore compare the performance of in-state funds across LPs and find that, indeed, public pension funds underperform other LPs in their in-state funds. In unreported regressions along the lines of Table III, we interact the dummy for whether LP and GP are in the same region with the dummies for different LP classes and find that the negative effect is entirely driven by the public pension funds. Only public pension funds display a large negative coefficient in the interaction term. We also differentiate whether LP and GP are in the same region or in the same state. We find that public pension funds continue to display poor performance when investing in funds that are in the same state, while funds in the same broad region of the United States, but not in the same state, do not underperform. When we disaggregate the endowments as above, there is also a strong negative effect for public universities. These findings are consistent with the idea that public pension funds and public endowments face politically motivated pressures or constraints to invest in their local areas despite possibly unfavorable effects on performance.

However, we do not believe that differences in the objective function fully explain the lower performance of public pension funds relative to private endowments. When we concentrate only on the performance of public pensions in their out-of-state investments, we find that these LPs still distinctly underperform private endowments in their out-of-state investments. However, when making out-of-state investments, public pension funds should not have an objective to invest in underperforming funds. These findings suggest that public institutions' nonfinancial motives (or constraints) to support local funds cannot explain the story fully.

C. Are These Patterns Driven by Fund Access?

Another possible explanation for the patterns documented in Section II is that the superior performance of endowments is an accident of history. As Kaplan and Schoar (2005) document, private equity funds display a concave relationship between fund size and performance: The best funds apparently limit their size, even if they could raise far more capital. Typically, these limitations are implemented by restricting access to existing LPs, who are given the right to reinvest a set amount, and not accepting new investors. These facts may imply that endowments enjoy superior returns not because of better fund selection, but because their early experience gave them a "seat at the table" among superior groups.

To explore the possibility that the results simply reflect superior access, we first analyze recent investment decisions in young private equity groups. In these cases, access to the funds is much less critical: Existing LPs should have little preferential access.[21]

Table VI summarizes the performance of different classes of LPs for funds managed by recently established private equity groups. We use the median founding year (1990) of all private equity groups in our sample as a cutoff, and explore whether endowments continue to enjoy superior performance when they invest in the younger private equity groups. In this case, we again find that endowments and public pension funds outperform the rest of the sample. However, the differences in performance across the different LPs are less pronounced. Note that banks (and, to some extent, advisors) still seem to perform worst when we condition on the younger GPs.[22]

Another way to assess ease of access to funds is to consider the degree of over- or undersubscription that the funds experience. A fund that is at or above its target size is likely to have been in heavy demand by investors, and the GPs could therefore have afforded to be more selective in terms of who they allowed to invest in the fund. On the other hand, a fund that closed below target probably would not have restricted access. We collect data on the target size of funds from *The Private Equity Analyst*.

Panel A of Table VII shows the availability of target close data for our sample. Out of 507 funds (corresponding to 3,435 LP-fund pairs) for which we have target fund size data, 103 were undersubscribed, 48 closed at the target size, and 356 were over-subscribed (it is standard industry practice to state a target size somewhat below one's actual goal). Since over 70% of the funds in our sample were oversubscribed (which is not surprising given the rapid growth of the private equity industry during the 1990s), we are able to obtain only a limited amount of variability along this dimension of our measure of ease of access to funds.

As an alternative proxy, we also collect data on the time it took GPs to raise the target amount of capital for their funds. Our assumption is that funds with a longer closing time are less restrictive in granting access to LPs than funds that close very quickly. Panel B shows that, of the 471 funds (corresponding to 3,246 LP-fund pairs) for which we have data on fundraising speed, 245 took longer than 1 year to close (we classify these as "slow-to-raise" funds), and 226 closed within 1 year (we label these as "fast-to-raise" funds). Panel C shows the results of performance regressions similar to those in Table III, but where we

[21] While it is possible that the existing relationships and prestige of an established LP help in gaining access to the hottest new funds, typically new funds are not in the position of turning away new investors.

[22] We also repeat a regression approach along similar lines as in Table III. Parallel to the descriptive statistics, we find that young funds in which banks invest do significantly worse. All LP dummies except for endowments have a negative coefficient relative to the omitted category (public pension funds) but none of these are significant. When we use excess IRR as the dependent variable, endowments have significantly positive performance.

Table VI

Recent Investments in Young Private Equity Groups

This table shows mean IRRs for funds managed by recently established GPs, grouped by LP class and fund type. The sample consists of 805 investments for which fund performance data are available, made by 226 LPs in 118 funds that closed between 1991 and 1998 and that were managed by 90 "young" private equity groups (i.e., established after 1990), as compiled by Asset Alternatives. *Fund IRR* is the IRR of each fund obtained from *Private Equity Performance Monitor*, and *weighted fund IRR* is the IRR weighted by proportional commitment to the fund in each LP's private equity portfolio.

	Overall			Early-Stage VC Funds			Later-Stage VC Funds			Buyout Funds		
	N	Fund IRR (%)	Weighted IRR (%)	*N*	Fund IRR (%)	Weighted IRR (%)	*N*	Fund IRR (%)	Weighted IRR (%)	*N*	Fund IRR (%)	Weighted IRR (%)
Public pension funds	281	8.2	4.4	18	22.5	5.5	83	23.6	16.4	180	−0.3	−1.3
Corporate pension funds	98	6.8	6.7	7	28.2	27.6	27	27.2	26.5	64	−4.2	−3.9
Endowments	134	14.6	13.1	14	2.6	−1.2	50	38.6	34.5	70	−0.1	0.6
Advisors	145	7.3	5.2	10	20.3	19.3	59	20.4	13.8	76	−4.6	−3.4
Insurance companies	58	4.0	2.7	6	3.9	6.4	18	21.3	18.0	34	−5.2	−6.1
Banks and finance companies	72	−0.2	−0.4	3	7.1	7.1	21	5.5	5.3	48	−3.1	−3.3
Other investors	17	−1.3	−1.3	1	−3.8	−3.8	7	6.8	6.8	9	−7.4	−7.3
Overall	805	7.7	5.6	59	15.0	8.9	265	24.0	19.3	481	−2.2	−2.4

Table VII

Investments in Over/Undersubscribed and Fast/Slow-to-Raise Funds

This table reports the results of analyses of investments in funds that were under- or oversubscribed, and in fast- and slow-to-raise funds. Panel A shows the number of funds and LP-fund pairs in the sample for which over/undersubscription data are available from *The Private Equity Analyst*. Panel B shows the number of funds and LP-fund pairs in the sample for which data on the time taken for the fund to be raised are available from *The Private Equity Analyst*. Panel C shows regression results following the specifications in Table III, for subsamples consisting of undersubscribed and just-subscribed funds, and for slow-to-raise funds. For the Heckit regressions in columns (4) and (8) of Panel C, the first-stage probit regression is:

$$Prob(Fund_{ij} \text{ has observable IRR data} \mid \text{independent variables}) = \Phi\left(\begin{array}{l}\gamma_0 + \sum_k \gamma_{1k} DummyLP_{jk} + \gamma_2 FundSize_i + \sum_m \gamma_{3m} GPRegion_{im} \\ + \sum_n \gamma_{4n} FundCategory_{in} + \sum_o \gamma_{5o} FundVintageYear_{io}\end{array}\right),$$

where $FundSize_i$ is the natural logarithm of the total closing amount of fund i in MM$, and $GPRegion_{im}$ is a dummy variable for the geographical location of the GP managing fund i. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Funds	LP-Fund Pairs
Panel A: Over/Undersubscribed Funds		
Undersubscribed (total closing < target close)	103	314
Just subscribed (total closing = target close)	48	254
Oversubscribed (total closing > target close)	356	2,867
Total observations with nonmissing data	507	3,435
Panel B: Fast/Slow-to-Raise Funds		
Slow to raise (% raised in first year < 100%)	245	1,568
Fast to raise (fully raised within first year)	226	1,678
Total observations with nonmissing data	471	3,246
Total sample	838	4,618

(continued)

restrict the sample to (1) only those funds that either were undersubscribed or closed at their target size,[23] and (2) a second subsample consisting only of slow-to-raise funds. We find that endowments still outperform all other LP classes. (The results are similar in unreported univariate comparisons.)

Overall, these results suggest that some of the differences in the performance of LPs (in particular, endowments and public pension funds) might be attributable to preferential access of those LPs that have been investing in the industry for a long time. Over time, they may have developed good relationships

[23] Ideally, we would also have liked to conduct this exercise on the subsample consisting only of funds that were undersubscribed; however, the number of observations in those regressions would be very low, which would make interpretation of the results difficult.

Table VII—*Continued*

Panel C: Fund Performance Regressions for Subsamples Consisting of Undersubscribed and Just-Subscribed Funds, and Slow-to-Raise Funds

	Subsample Consisting of Undersubscribed and Just-Subscribed Funds				Subsample Consisting of Slow-to-Raise Funds			
Dependent variable: Fund IRR	(1)	(2)	(3)	Heckit (4)	(5)	(6)	(7)	Heckit (8)
Dummy for LP class: (comparison category is public pension funds)								
Corporate pension funds	0.29	2.94	3.49	6.76	−4.21	−3.05	−2.27	−3.63
	(7.26)	(9.41)	(12.94)	(9.13)	(6.94)	(8.37)	(9.21)	(7.28)
Endowments	31.37***	26.17*	25.42	35.44**	19.12*	10.90	12.01*	19.71*
	(10.52)	(14.04)	(16.84)	(11.29)	(9.94)	(7.24)	(7.12)	(10.29)
Advisors	12.09	−7.96	−33.29	17.76	3.31	8.06	12.05	3.51
	(8.64)	(20.05)	(24.75)	(11.51)	(4.85)	(9.63)	(10.94)	(4.96)
Insurance companies	5.87	6.92	7.81	23.94	−15.36*	−8.43	−10.67	−12.79
	(10.28)	(11.31)	(12.15)	(19.90)	(7.81)	(9.15)	(11.03)	(9.81)
Banks	−1.78	−8.14	−8.54	9.59	−10.74*	−5.98	−5.71	−8.79
	(7.02)	(14.52)	(14.50)	(12.84)	(6.46)	(8.47)	(8.62)	(8.10)
Other LPs	−24.88	−47.07	−24.20	−6.17	0.11	−15.09	−13.79	2.38
	(20.44)	(34.54)	(20.46)	(20.86)	(6.18)	(11.35)	(10.60)	(8.17)
LP and GP in same region		2.77	3.68			−8.32*	−7.90*	
		(7.08)	(8.01)			(4.38)	(4.43)	
LP vintage		0.00	0.44			1.04*	1.85	
		(0.51)	(0.68)			(0.62)	(1.18)	
LP size (log of total commitments to private equity)		−0.66	−0.88			1.26	2.11	
		(1.91)	(2.17)			(1.18)	(1.28)	
Interaction effects:								
Corporate pension funds * LP vintage			1.04				−0.91	
			(2.23)				(1.33)	
Endowments * LP vintage			−0.09				−1.57	
			(1.99)				(1.30)	
Advisors * LP vintage			−5.05*				0.43	
			(2.79)				(1.58)	
Insurance companies * LP vintage			−1.61				−2.13	
			(2.17)				(2.02)	
Banks * LP vintage			0.90				−0.98	
			(1.93)				(1.77)	
Other LPs * LP vintage			−9.99**				−2.51	
			(4.79)				(1.95)	
Heckit Lambda				−22.18				−6.03
				(19.40)				(8.75)
Fund category fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LP region dummies	No	Yes	Yes	No	No	Yes	Yes	No
R-squared	52.3%	50.9%	52.3%	n/a	28.9%	20.2%	20.6%	n/a
Number of observations	276	161	161	566	990	569	569	1,555

with established and successful GPs in the industry.[24] However, the results regarding young and undersubscribed funds suggest that more than preferential access is at work.

[24] We note that it might be optimal for established LPs to invest in a number of younger funds even if the expected returns on these funds are initially low. The goal of this strategy could be the need to generate information about new classes of funds (e.g., Chinese VC) and to create a pipeline of a new generation of GPs with whom they will have preferential relationships going forward. This, in turn, could bias our results on the returns of young funds downwards and make the differences between LP classes less pronounced than they might otherwise be.

V. Conclusion

While the differences between institutional and individual investors have attracted growing attention by financial economists, the diversity of performance and investment strategies *across* the various classes of institutional investors has been less scrutinized. This paper examines the experience of various institutional investors in private equity funds.

We document dramatic differences in the performance of investments by different institutions: Endowments realize an annual return that is approximately 21% better than that of other institutions, while funds selected by banks perform particularly poorly. These differences remain present when we employ a variety of controls and specifications. Moreover, funds in which endowments decide to reinvest show much higher performance going forward than those in which endowments decide not to reinvest. This suggests that endowments proactively use the private information they gain from being an inside investor, while other LPs seem less willing or able to use information they obtain as an existing fund investor.

We also explore the possibility that the superior performance of endowments can be explained in alternative ways. First, we show that the funds selected by endowments seem no riskier than those chosen by other LPs. Second, we show that differing objectives explain only a portion of the difference between LPs; for instance, the inferior performance of public pensions cannot be entirely explained by the lower returns garnered by in-state investments. Finally, we examine whether, through their early experience as LPs, endowments may have greater access to established, high-performing funds. When conditioning on young private equity funds (those raised after 1990), undersubscribed funds, and slow-to-raise funds, we still find a significant gap between endowments and other investors.

These findings can potentially shed light on some of the previously documented puzzles in the private equity market (see, e.g., Gompers and Lerner (1998) and Kaplan and Schoar (2005)). The presence of unsophisticated or performance-insensitive LPs allows poorly performing GPs to raise new funds and thus makes exit less effective as a governance mechanism of sophisticated LPs. Unsophisticated LPs also contribute to the persistence of performance in private equity, in particular at the lower end.

This paper poses a number of interesting follow-on questions that we leave for further research. First, better understanding the sources of the performance puzzle is an important challenge. For instance, what specific agency problems have led to the poorer selection of funds by corporate pensions, investment advisors, and banks? While we can speculate on some of the weaknesses of certain institutional investors, such as weak incentive compensation for many investment advisors and the frequent rotation of employees in corporate pension staffs, clearly more work is needed to understand these issues.

Second, it would be interesting to explore the generality of our results. We suggest above that the extreme information problems associated with assessing the companies in venture capitalists' portfolios are what have led to the dramatic disparities in the performance of VC investments across investors.

These extreme information gaps may or may not be duplicated in other asset classes. Do the same patterns arise, for instance, in the returns of hedge fund and public equity managers? If so, it may be interesting to explore the broader consequences of the changing mixture of institutional investors.

APPENDIX

Table A.I

Fund Performance Regressions Using Excess IRR as the Dependent Variable

This table reports the results of pooled regressions of fund excess IRR on dummy variables for LP classes and control variables. The sample consists of 4,618 investments by 352 LPs in 838 funds closed between 1991 and 1998, as compiled by Asset Alternatives. Several versions of the following pooled regression are run and coefficient estimates and standard errors are reported by columns in the table:

$$ExcessIRR_{ij} = \beta_0 + \sum_k \beta_{0k} DummyLP_{jk} + \sum_k \beta_{1k} DummyLP_{jk} \times FundInflow_{ij}$$
$$+ \sum_k \beta_{2k} DummyLP_{jk} \times LPvintage_j + \beta_3 D\_sameregion_{ij} + controls.$$

$ExcessIRR_{ij}$ is the IRR of fund i in % minus the median IRR of the portfolio formed for each fund category every year. Six dummy variables identify the class of LP for each LP-fund pair, with $DummyLP_{jk}$ taking a value of one for each observation consisting of an investment in fund i by LP j belonging to LP class k, and zero otherwise. "Public pension funds" is the base LP class, with zero values for all LP dummy variables. $FundInflow_i$ is the year-over-year change in the amount of fund inflows into VC in the country-year of the closing of fund i, and is a proxy for market conditions. $LPvintage_j$ is the year of establishment of the private equity program at LP j relative to that of the median LP in the sample, which began its private equity program in 1987. $D\_sameregion_{ij}$ is a dummy variable that equals one if both LP j and the private equity firm managing fund i are headquartered in the same region in the U.S. (Midwest (includes Illinois and Ohio), Northeast (includes Massachusetts, New York, and Pennsylvania), South (includes Texas), and West (includes California)), and zero otherwise. Robust standard errors allowing for data clustering by funds in all the regressions are shown in brackets below the coefficient estimate. Intercepts are not reported.

For the Heckit regression in column (6), the first-stage probit regression is

$$Prob(Fund_{ij}\, has\ observable\ excess\ IRR\ data \mid independent\ variables)$$
$$= \Phi\left(\gamma_0 + \sum_k \gamma_{1k} DummyLP_{jk} + \gamma_2 FundSize_i + \sum_m \gamma_{3m} GPRegion_{im}\right),$$

where $FundSize_i$ is the natural logarithm of the total closing amount of fund i in MM$, and $GPRegion_{im}$ is a dummy variable for the geographical location of the GP managing fund i. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Dependent variable: Excess IRR	(1)	(2)	(3)	(4)	(5)	Heckit (6)
Dummy for investor type: (comparison category is public pension funds)						
Corporate pension funds	−4.77*	−8.61*	−11.45***	−10.23***	−9.63	−5.11
	(2.61)	(3.66)	(4.22)	(3.71)	(6.24)	(3.49)
Endowments	21.70***	15.69***	15.49***	16.60***	26.79***	23.04***
	(6.83)	(4.57)	(4.51)	(4.72)	(8.32)	(7.66)
Advisors	7.63**	11.36**	11.14**	11.52**	27.90***	9.01**
	(3.40)	(4.92)	(5.52)	(4.85)	(10.15)	(4.08)

(continued)

Table A.I—*Continued*

Dependent variable: Excess IRR	(1)	(2)	(3)	(4)	(5)	Heckit (6)
Insurance companies	−0.57	2.08	1.10	0.84	12.68	−0.17
	(3.73)	(4.37)	(4.68)	(4.51)	(9.24)	(6.32)
Banks	−9.11***	−3.55	−2.09	−4.16	−10.18	−9.31*
	(2.88)	(3.85)	(6.29)	(3.83)	(7.78)	(4.89)
Other LPs	−2.51	−39.34***	−39.26***	−36.98***	−53.85***	−2.91
	(5.20)	(8.92)	(8.21)	(9.76)	(9.31)	(6.30)
LP and GP in same region		−8.90***	−8.64***	−8.74***	−8.34***	
		(2.57)	(2.55)	(2.57)	(2.52)	
LP vintage		0.26	0.89*	0.35	0.87*	
		(0.23)	(0.45)	(0.23)	(0.46)	
LP size (log of total commitments to private equity)		−1.42*	−1.10	−1.12	−0.86	
		(0.78)	(0.75)	(0.78)	(0.75)	
Total private equity fund inflow				−13.58**	−7.28	
				(6.03)	(5.67)	
Interaction effects:						
Corporate pension funds * LP vintage			−1.55**		−1.01	
			(0.72)		(0.68)	
Endowments * LP vintage			−0.97		−0.81	
			(0.65)		(0.66)	
Advisors * LP vintage			−0.67		−0.57	
			(0.88)		(0.87)	
Insurance companies * LP vintage			−1.00		−0.70	
			(0.69)		(0.79)	
Banks * LP vintage			−0.03		−0.62	
			(1.46)		(1.16)	
Other LPs * LP vintage			−0.60		−1.23*	
			(0.76)		(0.69)	
Corporate pension funds * inflow					−4.00	
					(9.35)	
Endowments * inflow					−21.79*	
					(12.44)	
Advisors * inflow					−33.75**	
					(14.52)	
Insurance companies * inflow					−27.00**	
					(13.65)	
Banks * inflow					11.67	
					(10.37)	
Other LPs * inflow					36.91***	
					(8.71)	
Heckit Lambda						−1.21
						(10.09)
Fund category fixed effects	No	No	No	No	No	No
Year fixed effects	No	No	No	No	No	No
LP region dummies	No	Yes	Yes	Yes	Yes	No
R-squared	3.9%	5.2%	5.6%	5.9%	6.8%	n/a
Number of observations	2,684	1,541	1,541	1,491	1,491	4,096

Table A.II

Fund Performance Regressions Using Individual Funds as Observations

This table reports the results of regressions of fund IRR on numbers of LPs in each LP class that invested in the funds, and control variables. The sample consists of 838 funds that were closed between 1991 and 1998 and for which data are available to run the following ordinary least squares regressions:

$$FundIRR_i = \beta_0 + \sum_k \beta_{1k} NumLP_{ik} + \beta_2 FundSize_i + controls.$$

$FundIRR_i$ is the IRR of fund i, as reported by *Private Equity Performance Monitor*. $NumLP_{ik}$ is the number of LPs of class k that invested in fund i. $FundSize_i$ is the natural logarithm of the total closing amount of fund i in MM$. For the Heckit regression in column (4), the first-stage probit regression is

$$Prob(Fund_{ij}\ has\ observable\ IRR\ data \mid independent\ variables) = \Phi\left(\begin{array}{l}\gamma_0 + \sum_k \gamma_{1k} NumLP_{ik} + \gamma_2 FundSize_i + \sum_m \gamma_{3m} GPRegion_{im} \\ + \sum_n \gamma_{4n} FundCategory_{in} + \sum_o \gamma_{5o} FundVintageYear_{io}\end{array}\right),$$

where $GPRegion_{im}$ is a dummy variable for the geographical location of the GP managing fund i. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Dependent variable: Fund IRR	(1)	(2)	(3)	Heckit (4)
Number of public pension funds investing in fund	0.25	−0.17	−0.25	−0.17
	(1.28)	(1.26)	(1.26)	(1.50)
Number of corporate pension funds	−4.58*	−4.92*	−5.60**	−6.10**
	(2.75)	(2.68)	(2.82)	(2.99)
Number of endowments	4.10***	4.14***	4.14***	3.96***
	(1.25)	(1.22)	(1.26)	(1.32)
Number of advisors	1.23	2.47	0.99	1.90
	(2.09)	(2.08)	(1.98)	(2.24)
Number of insurance companies	−3.72	−2.13	−1.79	−3.61
	(3.65)	(3.57)	(3.63)	(3.93)
Number of banks	−6.37*	−5.15	−6.89*	−7.18*
	(3.70)	(3.68)	(3.81)	(3.96)
Number of other classes of investors	−7.06	−9.38	−8.85	−6.80
	(7.17)	(7.09)	(7.36)	(7.61)
Log(size of fund)	3.34	6.20	8.18*	6.16
	(3.94)	(4.12)	(4.29)	(4.53)
Average vintage of LPs that invest in fund		1.55*	1.25	
		(0.88)	(0.90)	
Average total private equity commitments of LPs that invest in fund		−0.18*	−0.19*	
		(0.10)	(0.11)	
Total inflows into private equity			−32.78***	
			(10.73)	
Heckit Lambda				−1.64
				(10.70)
Fund category fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	No	Yes
Adjusted R-squared	19.1%	20.2%	18.5%	n/a
Number of observations	341	324	309	672

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Private Equity, Jobs, and Productivity†

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Private equity critics claim that leveraged buyouts bring huge job losses and few gains in operating performance. To evaluate these claims, we construct and analyze a new dataset that covers US buyouts from 1980 to 2005. We track 3,200 target firms and their 150,000 establishments before and after acquisition, comparing to controls defined by industry, size, age, and prior growth. Buyouts lead to modest net job losses but large increases in gross job creation and destruction. Buyouts also bring TFP gains at target firms, mainly through accelerated exit of less productive establishments and greater entry of highly productive ones. (*JEL* D24, G24, G32, G34, J23, J63, L25)

Leveraged buyouts by private equity firms arouse intense concern and strongly held views. For instance, former Danish Prime Minister Poul Rasmussen—architect of the European Commission's Alternative Investment Fund Managers Directive—contends that "'leveraged buy-outs' leave the company saddled with debt and interest payments, its workers are laid off, and its assets are sold, … benefiting neither workers nor the real economy" (Rasmussen 2008, p. 132). The Service Employees International Union, prominent critic of private equity on both sides of the Atlantic, offers this assessment: "Typically it's easier to decrease costs quickly by cutting heads, which is why buyouts have typically been accompanied by layoffs."[1]

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† Go to http://dx.doi.org/10.1257/aer.104.12.3956 to visit the article page for additional materials and author disclosure statement(s).

[1] Wong, Grace. 2007. "Private Equity and the Job Cut Myth." *CNNMoney.com*, May 2. http://money.cnn.com/2007/05/02/markets/pe_jobs/index.htm (accessed August 25, 2011). Remarks attributed to John Adler.

Responding to similar contentions, several industry-sponsored studies claim positive employment and other effects of private equity. Examples include European Private Equity and Venture Capital Association (2005), British Private Equity and Venture Capital Association (2006), A.T. Kearney (2007), and Shapiro and Pham (2008).

Efforts to bring data to these issues are highly welcome, but these studies have several limitations. First, they rely on surveys with incomplete and perhaps selective responses, raising doubts about representativeness and accuracy. Second, the underlying data offer little scope to control for concurrent changes at comparable firms. When a firm backed by private equity sheds 5 percent of employment, the interpretation depends greatly on whether comparable firms grow by 3 percent or shrink by 10 percent. Third, these studies do not distinguish cleanly between changes at firms backed by venture capital and firms backed by other forms of private equity. Both are interesting, but the controversy involves buyouts, not venture capital. Fourth, these studies face major challenges in measuring organic job growth because they lack establishment-level data. As a result, it is hard to disentangle organic changes from the acquisition and sale of particular facilities and business units. Fifth, the lack of establishment-level data also precludes a breakdown of firm-level employment changes into job creation and job destruction components, i.e., gains and losses at the establishment level. As we show, private equity buyouts have quite different effects on these two margins of employment change.[2]

In this study, we construct and analyze a dataset that overcomes these limitations and, at the same time, encompasses a much larger set of private equity buyouts and controls. We rely on the Longitudinal Business Database (LBD) at the US Census Bureau to track employment and earnings before and after buyouts at firms *and* establishments—i.e., specific factories, offices, retail outlets, and other distinct physical locations where business takes place. The LBD covers the entire nonfarm private sector and contains annual data for about five million firms and six million establishments. To obtain high-quality productivity measures, we turn to the Annual Survey of Manufactures (ASM) and the Census of Manufactures (CM). In addition to their other strengths, the establishment-level information in the LBD, ASM, and CM enables us to explore important aspects of within-firm restructuring activity in the wake of private equity buyouts.

We combine the LBD, ASM, and CM with data from CapitalIQ and other sources to identify and characterize private equity transactions. The resulting matched sample contains about 3,200 US firms acquired in buyouts from 1980 to 2005 ("target firms") and 150,000 US establishments operated by these firms as of the buyout year ("target establishments"). We match each target firm to other firms that are comparable in terms of industry, age, size, and single/multi-establishment status, and then follow targets and matched controls over time. We take a similar approach to controls for target establishments.

[2] See Service Employees International Union (2007) and Hall (2007) for other critiques. We discuss the broader academic literature on the economic effects of private equity in Section I. Few academic studies of private equity focus on employment outcomes, and the main exceptions consider data for France and the United Kingdom. To our knowledge, no previous studies exploit linked firm-level and establishment-level data to examine the within-firm reallocation effects of private equity buyouts and their relationship to productivity gains at target firms.

To clarify the scope of our study, we consider later-stage changes in ownership and control executed and partly financed by private equity firms. In these transactions, the (lead) private equity firm acquires a controlling stake in the target firm and retains a significant oversight role until it "exits" by selling its stake. The buyout event typically involves a shift toward greater leverage in the capital structure of the target firm and, sometimes, a change in its management. We exclude management-led buyouts that do not involve an acquisition by a private equity firm. We also exclude startup firms backed by venture capitalists.

Our establishment-level analysis yields three main findings: First, employment shrinks more rapidly, on average, at target establishments than at controls after private equity buyouts. The average cumulative difference in favor of controls is about 3 percent of initial employment over two years and 6 percent over five years. Second, the larger post-buyout employment losses at target establishments entirely reflect higher rates of job destruction at shrinking and exiting establishments. In fact, targets exhibit *greater* post-buyout creation of new jobs at expanding establishments. Adding controls for pre-buyout growth history shrinks the estimated employment responses to private equity buyouts but does not change the overall pattern. Third, earnings per worker at continuing target establishments fall by an average of 2.4 percent relative to controls over two years post buyout.

The establishment-level analysis misses job creation at newly opened establishments, whether by target or control firms. To capture this activity, we move to a firm-level analysis and identify new establishments opened post buyout. The combination of firm and establishment data in the LBD is what enables us to isolate and quantify "greenfield" job creation at facilities opened post buyout. For this part of our analysis, we shorten the time window to two years post buyout. Lengthening the window involves a greater incidence and complexity of ownership changes, threatening the integrity of our firm-level longitudinal linkages or forcing us to narrow the sample. We find that target firms engage in more greenfield job creation than control firms, with a cumulative two-year difference of nearly 2 percent of initial employment. That is, greater greenfield job creation partly offsets the relative employment drop at target establishments. Our firm-level analysis also yields another interesting result: Private equity targets engage in more acquisitions and more divestitures than controls. Summing over job creation and destruction at continuing establishments, job losses at shuttered establishments, job gains at greenfield establishments, and contributions of acquisitions and divestitures, employment shrinks by less than 1 percent at target firms relative to controls in the first two years after private equity buyouts.

We uncover a much larger response in the pace of job reallocation. Specifically, over the first two years post buyout, establishment-level job gains and losses at target firms exceed gains and losses at controls by 14 percent of initial employment. This extra job creation and destruction activity amounts to 25 percent of baseline job reallocation at control firms. A more rapid pace of organic job creation and destruction accounts for 45 percent of the extra reallocation activity at target firms, and greater acquisitions and divestitures account for the rest. These results indicate that private equity buyouts catalyze the creative destruction process, as measured by job creation and destruction and by the transfer of production units between firms.

Our productivity findings reinforce this view. Specifically, compared to control firms, target firms more aggressively close manufacturing plants in the lower part of the total factor productivity (TFP) distribution. They also open new plants in the upper part of the TFP distribution at nearly twice the rate of control firms, and they are much less likely to open low productivity establishments. On average, over the first two years post buyout, we estimate that private equity buyouts raise TFP by 2.1 log points. (Baseline TFP growth for controls is slightly negative.) Three quarters of the post-buyout TFP gains reaped by target firms reflect a greater propensity to close low productivity plants and to open new, high productivity plants. In short, buyouts improve productivity mainly through the directed reallocation of resources across units within target firms. These TFP results and our results on worker earnings imply that private equity buyouts materially improve operating margins at target firms.

The next section briefly reviews related research and offers additional motivation for our study. Section II describes the construction of our analysis datasets, and Section III explains our empirical methods. Sections IV and V present our main establishment-level and firm-level analyses of employment and job reallocation effects. Section VI considers effects on TFP and earnings per worker, and Section VII offers concluding remarks.

I. Related Work and Additional Motivation

Economists hold a longstanding interest in how ownership changes affect productivity and employment. Examples include Siegel and Lichtenberg (1987); Long and Ravenscraft (1993); McGuckin and Nguyen (2001); and Harris, Siegel, and Wright (2005). One ownership change that attracts particular attention is the acquisition of firms by professional private equity firms. Jensen (1989) and Shleifer and Summers (1988), among others, discuss the economic effects of private equity buyouts based largely on case study evidence. Kaplan and Strömberg (2009) provide a useful overview of research on the economic effects of private equity.

Few previous studies focus on the employment effects of private equity, and the exceptions typically rely on small samples dictated by data availability. Kaplan (1989a) considers 76 public-to-private leveraged buyouts (LBOs) during the 1980s. He finds that the median firm lost 12 percent of employment on an industry-adjusted basis from the end of the fiscal year prior to the private equity transaction to the end of the fiscal year after the transaction. After dropping target firms with asset sales or purchases that exceed 10 percent of total value, the adjusted employment decline is 6.2 percent for the remaining 24 firms. Muscarella and Vetsuypens (1990) consider 72 firms that completed an initial public offering (IPO) after an LBO between 1983 and 1987. For the 26 firms they can track, employment declines by an average of 0.6 percent between the LBO and the IPO.

Lichtenberg (1990) uses US Census Bureau data to examine changes in employment at the manufacturing plants of 131 firms undergoing buyouts between 1981 and 1986. On an industry-adjusted basis, employment falls by 1.2 percent per year after buyout, as compared to a 1.9 percent rate of decline beforehand. Declines are larger for nonproduction workers than blue-collar workers. Wright, Thompson, and Robbie (1992) and Amess and Wright (2007) similarly find that buyouts in the UK lead to modest employment declines. These studies follow overall employment at a set of firms, and contrast it with aggregate employment at matching firms.[3] Boucly, Sraer, and Thesmar (2011) find that employment grows much more rapidly at target firms than at controls in the wake of French private equity buyouts, a result they attribute to an important role for private equity in relaxing financing constraints.

These studies share certain weaknesses. First, most focus on the company-wide employment of firms backed by private equity. Thus, the sale of a division or other business unit is typically counted as an employment loss, even if the sold business unit continues with the same number of employees under new ownership. Likewise, the acquisition of a division or other business unit is counted as an employment gain, even if there is no employment change at the business unit itself. Several studies attempt to address this issue by dropping buyouts that involve substantial asset sales, but this sample restriction may greatly influence the results, given the extent of "asset shuffling" by firms backed by private equity. The handful of previous US studies that treat establishments as the unit of observation are typically restricted to the manufacturing sector, and even then have limited ability to track establishment or firm closings.

Second, previous studies of US private equity deals rely on highly selected samples—potentially an important source of bias in the findings. The public-to-private buyouts that dominate earlier samples account for less than one-quarter of the employees directly impacted by private equity buyouts and only 12 percent of the deals (Table 2). Most previous US studies consider deals before the 1990s only, but fundraising by US private equity groups rose 36-fold from 1985 to 1998 and more than 100-fold by 2006.[4] The tremendous growth in private equity activity allows us to examine a much larger set of deals. Moreover, the nature of private equity activity has also changed over time—competition for attractive deals has intensified, and many private equity firms now have a strong operational orientation, as opposed to the financial engineering approach that characterized many groups during the 1980s.

Our study overcomes these weaknesses, as we have explained. In addition, we exploit the establishment-level aspect of our data to examine job creation and job destruction outcomes, as well as net employment changes. In this regard, we are motivated in part by previous work that documents a rapid pace of establishment-level job creation and destruction. Davis and Haltiwanger (1999) review work in this area. Earlier empirical work also shows that the reallocation of jobs and workers across establishments plays a major role in medium-term productivity gains. Many important theoretical models also feature distinct roles for the creation and destruction margins of employment adjustment. Caballero (2007) provides an insightful, detailed analysis and extensive references to the relevant literature.

Numerous case studies provide detailed descriptions and analyses of particular private equity deals. By our reading, these studies deliver four sets of insights.

First, private equity groups sometimes generate few or no productivity gains because they fail to achieve their goals for target firms. For instance, when Berkshire

[3] The samples in these UK studies include management-led deals (management buyouts), which need not involve a financial sponsor that acquires a controlling stake in the target firm. Management-led deals potentially differ substantially from the traditional private equity buyouts that we consider.

[4] Using inflation-adjusted dollars and data from Thomson Reuters VentureXpert, https://vx.thomsonib.com/VxComponent/vxhelp/VentureXpert_Fact_Sheet.pdf.

Partners bought Wisconsin Central, it had an ambitious plan to raise productivity. However, technological problems arose soon after the buyout and prevented the deployment of a computerized control system that was crucial for planned cost savings. As a result, the numbers in an ambitious business plan were never met (Jensen, Burkhardt, and Barry 1990). In the Revco transaction, a crippling debt load, management disarray, an inexperienced LBO sponsor, and a disastrous midstream shift in strategy led to a failure to achieve performance goals (Wruck 1991).

Second, the Revco case also points to tax savings as the primary source of private value creation in certain buyouts. Consistent with this view, Kaplan (1989b, p. 611) provides evidence that greater leverage and other organizational shifts imposed by private equity investors can yield substantial tax savings that are "an important source of the wealth gains in leveraged buyouts." If tax savings are the principal motive for buyouts, there is no compelling reason to anticipate positive effects on productivity at target firms.

Third, many case studies find substantial productivity gains at target firms through improvements to existing operations. In the Hertz buyout, for instance, Clayton, Dubilier, and Rice (CDR) addressed operational inefficiencies to increase profitability. Specifically, CDR lowered overhead costs by reducing inefficient labor expenses and cutting noncapital investments to industry standard levels, and more closely aligned managerial incentives with return on capital (Luehrman 2007). Similarly, the buyout of O.M. Scott and Sons led to substantial improvements in the firm's existing operations, partly through powerful incentives offered to management and partly through specific suggestions by the private equity investors (Baker and Wruck 1989). In examples like these, profitability increases and private value creation are likely to go hand in hand with productivity gains.

Finally, in a number of other cases, private equity targets achieved substantial efficiency improvements not by enhancing existing operations, but rather by divesting units. Beatrice had acquired a large number of unrelated businesses as part of a conglomerate strategy, many of which operated in segments in which it had little expertise. Its private equity investor, Kohlberg, Kravis, and Roberts, divested many of these laggard operations (Baker 1992). Similarly, the buyout group that purchased Kaiser Steel shut down its outdated and inefficient steel operations. The group focused its operational attention on Kaiser's coal mines, which it regarded as the firm's "hidden jewel" (Luehrman 1992). Greater profitability and private value creation are also likely to involve productivity gains in these examples, though mainly through productivity-enhancing reallocation rather than operational improvements within continuing units.

These case studies illustrate a wide range of motives for and effects of private equity transactions. Our study can be seen as an effort to determine which of these stories best characterizes the impact of private equity buyouts on average, especially with respect to employment and productivity outcomes.

II. Constructing the Analysis Samples

Our analysis requires a comprehensive database of private equity transactions and the matching of target firms to firm-level and establishment-level records in the LBD, ASM, and CM. This section describes the data construction process and the resulting samples.

A. *Identifying Private Equity Buyout Transactions*

CapitalIQ has specialized in tracking private equity deals on a worldwide basis since 1999 and, through extensive research, backfilled transactions prior to 1999.[5] We consider all recorded transactions in CapitalIQ that closed between January 1980 and December 2005. Integrating the CapitalIQ data with firm-level Census data to track outcomes before and after the transactions requires much care, as we discuss below. Our data integration work, a major undertaking, covers a 26-year sample period and yields a much larger, more comprehensive set of buyout targets and controls than previous studies. It would be interesting in future work to extend our sample to consider deals executed in the buyout boom of 2006–2007 and to examine their performance during and after the financial crisis and recession of 2008–2009.

We impose two main sample requirements. First, we omit transactions that do not involve a financial sponsor, i.e., a private equity firm. Second, we restrict attention to transactions that entail some use of leverage. Many transactions that do not involve leverage are venture capital investments rather than investments in mature firms. Given our focus, we omit transactions not classified by CapitalIQ as "going private," "leveraged buyout," "management buyout," "platform," or a similar term. This approach excludes "growth buyouts" and "expansion capital" investments to purchase a minority stake using little or no leverage. Such transactions may share some characteristics of private equity deals but do not fit the classic profile of leveraged buyouts.

After restricting the sample in these two ways, the resulting database contains about 11,000 transactions worldwide. Dropping transactions that involve firms with foreign headquarters leaves about 5,000 US target firms acquired in private equity buyouts between 1980 and 2005. (We do not consider US establishments operated by foreign targets.) To fill out our information about private equity deals and target firms, we supplement the data drawn from CapitalIQ with data from Dealogic, Thomson Reuters SDC, VentureXpert databases, and news stories. Dealogic, in particular, often contains greater detail about transaction characteristics. Other useful information in the supplementary sources includes alternative names associated with target firms and their later acquisitions and sales. Tables 1 and 2 below report summary statistics on enterprise values, which are available for about half the buyout deals, but we do not use the value data in our main analyses.

B. *Matching to LBD Records*

The LBD derives from the Census Bureau's Business Register, which contains annual data on US businesses with paid employees. The LBD covers the entire nonfarm private sector and contains, in more recent years, over six million establishment records and almost five million firm records per year. We use LBD data from 1976 to 2005. The Business Register and the LBD draw on administrative records

[5] Most data services tracking private equity transactions were not established until the late 1990s. The most comprehensive exception, SDC VentureXpert, mainly focused on capturing venture capital transactions until the mid-1990s. See Strömberg (2007) for a discussion of the completeness of the CapitalIQ database.

TABLE 1—VALUE OF PRIVATE EQUITY BUYOUT TARGETS, TOTAL AND MATCHED BY DECADE
(*$millions*)

	1980–1989	1990–1999	2000–2005
Matched	138,111	212,387	363,395
Total	199,166	280,746	529,595

Note: Enterprise values (debt plus equity) as of the transaction are imputed for about half the buyout targets based on deal type, industry, and transaction year, as detailed in online Appendix A.

TABLE 2—PRIVATE EQUITY BUYOUTS IN THE ANALYSIS SAMPLES

	Number of transactions (target firms)	Value of transactions, millions of 2005 dollars	Target establishments, transaction year	Target employment, transaction year
All, 1980–2005	3,218	713,892	151,529	5,828,532
Private to private	1,350	149,509	59,865	2,224,530
Public to private	390	272,599	36,717	1,371,129
Divisional sales	918	158,580	35,259	1,359,139
Secondary sales	396	108,324	13,455	637,591
Other	164	24,880	6,233	236,143
All, 1980–2003	2,265	530,786	103,671	4,323,558
Excluding EIN cases	1,874	465,657	79,131	3,410,598
Two-year continuers, excluding EIN cases	1,374	333,519	76,271	3,187,171
Private to private	686	91,247	37,283	1,470,447
Public to private	248	133,727	20,380	872,206
Divisional sales	206	45,683	7,922	391,705
Secondary sales	160	47,627	7,957	353,325
Other	74	15,235	2,729	99,488
All, 1980–2000	1,306	395,020	54,729	2,385,163
Private to private	647	99,973	24,593	901,284
Public to private	171	173,500	18,454	854,779
Divisional sales	342	74,487	6,557	416,055
Secondary sales	107	35,742	3,885	161,557
Other	39	11,318	1,240	51,488
Mfg., 1980–2003	539	138,006	9,174	805,328
Multi-unit firms only	427	126,572	9,062	792,864
Multi-unit with TFP	286	82,076	2,053	496,699

Notes: We exclude single-unit matched targets from our analysis of the manufacturing sector. They account for only 112 of 9,744 target manufacturing establishments and less than 1.5 percent of target manufacturing employment. The last row in the table reports data for matched multi-unit targets for which we can obtain data on total factor productivity in the transaction year from the Census of Manufactures or the Annual Survey of Manufactures.

and survey sources for data on firms and their establishments. Core data items include employment, payroll, four-digit Standard Industrial Classification (SIC) or six-digit North American Industrial Classification (NAICS), employer identification numbers, business names, and information about location.[6] Identifiers in the LBD files enable us to compute growth rate measures for establishments and firms and to track their entry and exit and ownership changes. Firms in the LBD are defined

[6] Sales data in the Business Register are available annually from 1994 and once every five years in earlier years.

based on operational control, and all establishments majority owned by a parent firm are included in the parent's activity measures.

To merge data on buyouts into the LBD, we match names and addresses of private equity portfolio firms (i.e., target firms) to LBD name and address records. To cope with timing differences between datasets, we search over a three-year window in the LBD centered on the transaction year for each target firm. We adopt a conservative approach to matching that requires either an exact match on name and address or an approximate match on both name and address according to probability-based matching algorithms. Our procedures match about 65 percent of target firms to the LBD, 71 percent on a value-weighted basis, yielding about 3200 matched target firms.[7] Once matched, firm-establishment links in the LBD serve to identify all establishments owned by target firms as of the private equity buyout year. Matched target firms operate about 150,000 US establishments as of the buyout year. LBD longitudinal links allows us to follow firms and establishments over time. Tracking firms is more challenging, as we discuss below, which influences the design of our firm-level analysis sample.

Given our interest in employment dynamics, the relationship of the LBD employment measure to the timing of private equity transactions requires careful treatment. The LBD reports total employment in the payroll period containing the week of March 12th. Accordingly, for buyout transactions that close before October 1, LBD employment in March of the same calendar year serves as our contemporaneous employment measure. We assign transactions that close on or after October 1 in year t to year $t + 1$ for purposes of our analysis, treating the LBD employment value in March of $t + 1$ as the contemporaneous measure. October is the natural cutoff because it lies midway between March-to-March employment changes in the LBD.

Figure 1 shows the number of US target firms acquired by year and the number matched to the LBD. It is apparent that the number of private equity buyouts grew rapidly beginning in the mid-1990s. Table 1 reports the enterprise value of private equity targets and matched targets by decade. The enterprise value of acquisitions is very large in the later years, reaching 530 billion in the 2000–2005 period. Figure 2 displays employment data for our matched target firms. For example, target firms acquired in 2005 and matched to the LBD account for 0.83 percent of total non-farm business employment in 2005. Given the extent of unmatched targets, the full set of firms that came under private equity control in 2005 accounts for more than 1 percent of private sector employment. Based on our data, we infer that more than 7 percent of private employment came under private equity control at some point in the ten-year period from 1998 to 2007.[8]

According to Figure 1, match rates vary slightly over time: 65 percent in the 1980s, 70 percent in 1990s, and 63 (65) percent from 2000 to 2005 (2003). To explore representativeness of our sample, we compare the characteristics of matched buyouts

[7] Puri and Zarutskie (2012) obtain somewhat higher match rates to the LBD for firms involved in venture capital (VC) deals, even though their matching methods are similar to ours. Matching is easier for VC deals than buyout deals, because buyouts usually involve larger, more complex organizations and deals, and they often involve the sale of divisions rather than whole firms.

[8] We arrive at this inference by summing the employment percentages of matched targets from 1998 to 2005, dividing the sum by 0.7 to account for unmatched targets, and making the assumption (supported by other data sources) that private equity activity continued at record levels in 2006 and the first half of 2007.

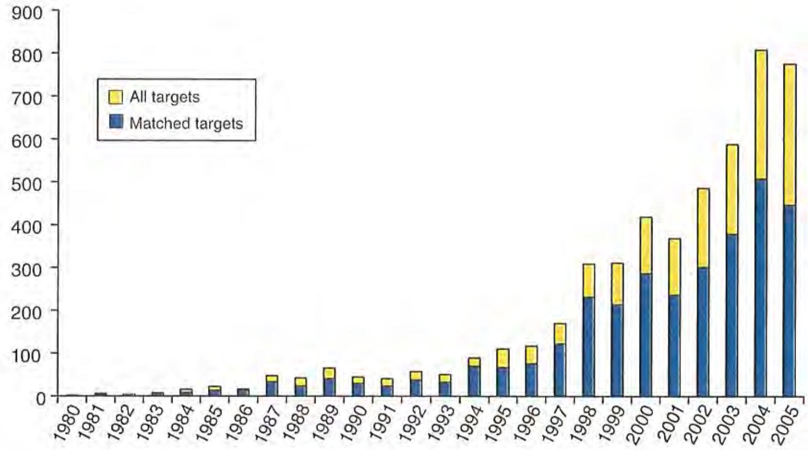

FIGURE 1. NUMBER OF TARGET FIRMS IN US PRIVATE EQUITY BUYOUTS, 1980 TO 2005

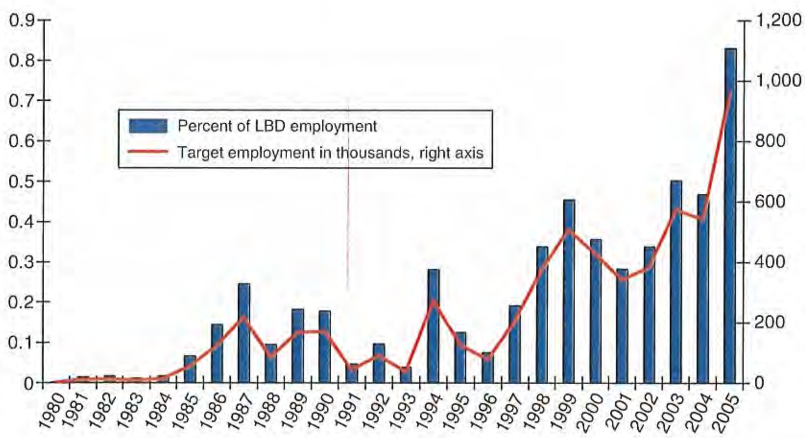

FIGURE 2. EMPLOYMENT AT MATCHED TARGETS AS OF THE BUYOUT YEAR, 1980 TO 2005

to all buyouts along several dimensions. Tables A.1 to A.3 in the online Appendix show that, in each decade, matched buyouts and all buyouts are similar with respect to industry, deal type, and average enterprise value. These results suggest that our sample of matched buyouts is representative of all buyouts.

C. *Analysis Samples*

Our study considers several related analysis samples. For descriptive statistics on the number and volume of private equity buyouts and their distribution by industry and firm characteristics, we consider all matched targets through 2005. For our main establishment-level analysis, we consider buyouts from 1980 to 2000. This sample allows us to track target (and control) establishments for five years before and after the buyout year. For our firm-level analysis sample, we consider buyouts from 1980 to 2003, so that we can track firm-level outcomes for two years post buyout. We also consider various subsamples, the most important of which focuses on buyouts in the manufacturing sector from 1980 to 2003. For the manufacturing subsample, we draw on ASM and CM data to construct plant-level TFP measures. Table 2 reports summary statistics on matched targets for our analysis samples.

III. Empirical Methods

This section describes key methodological choices in our empirical study. The first relates to the unit of analysis. Section IV considers establishments owned by target firms in the buyout year. We track these units over time, irrespective of their ownership in earlier or later years. For example, if the target firm goes public or sells an establishment, we continue to track that establishment and associate it with the buyout event. Section V takes a different approach, treating the firm as the unit of analysis, which lets us capture greenfield job creation and the acquisition and sale of establishments after the buyout event.

The second key choice relates to controls. We need suitable controls because the distribution of private equity buyouts across industries and business characteristics is not random. For example, practitioner accounts often suggest a concentration of transactions in industries undergoing significant restructurings due to regulatory action, foreign competition, or technological change. Target firms in our data are disproportionately concentrated in manufacturing, information services, and accommodation and food services, as shown in Figure B.1 of the online Appendix. Target firms are also considerably larger and older than the average firm, as shown in online Appendix Figure B.2.[9] The literature on firm dynamics concludes that growth and volatility vary systematically with firm size and age. See, for example, Caves (1998); Davis et al. (2007); and Haltiwanger, Jarmin, and Miranda (2013). Thus, it is important to control for these characteristics when evaluating the reallocation and other effects of private equity buyouts.

The huge number of firms and establishments in the LBD allows us to control for a full set of interactions among industry, size, age, multi-unit status, and year of the buyout transaction. We sort target firms into cells defined by the cross product of these characteristics.[10] We then identify all firms in the LBD not backed by private equity that fall into the same cell as a given target firm, and we treat those firms as controls. Specifically, we control for the interaction of 72 two-digit industries, 10 firm size categories, 6 firm age categories, a dummy for firms with multiple establishments, and 24 distinct transaction years. The cross product of these categorical variables yields over 8,000 control cells per year. Of course, many cells are unpopulated, but the richness of our controls is evident. In our regression analysis, we also control for pre-buyout employment growth histories. We follow the same approach in the establishment-level analysis. To obtain controls for a given target establishment, we select all establishments in the same control cell from among the set of active establishments in the transaction year, excluding establishments owned by a firm under private equity control.

[9] Firms with 500 or more employees account for 96 percent of employment at matched targets, as compared to 51 percent of all LBD employment in the 1980–2005 period. Firms 10 years or older account for 91 percent of employment at matched targets as compared to 64 percent in the LBD.

[10] We define industry for multi-unit firms based on the modal industry of their establishments, computed on an employment-weighted basis.

A related choice involves our statistical approach to estimating the effects of buyouts on employment outcomes. We consider nonparametric comparisons that control for the cross-product of our categorical variables, semi-parametric regressions that include additional controls, and propensity score methods. Ideally, we seek to estimate the average treatment effect on the treated, i.e., the average effect of buyouts on target firms. As discussed in Woolridge (2002, ch. 18), consistent estimation of average treatment effects requires conditional mean independence: conditional on the controls and the treatment indicator, outcomes for the treated and nontreated are independently distributed. Compared to previous research, our rich set of controls lends greater plausibility to this identifying assumption. Even if one questions the conditional mean independence assumption, our study yields a rich set of new findings about outcomes at private equity targets. These findings throw light on alternative views about the economic role of private equity, as we discuss below. Our findings also provide useful evidence for formulating and evaluating theoretical models of private equity behavior and effects.

A fourth choice relates to the time window around private equity transactions. Our establishment-level analysis considers employment outcomes for five years on either side of a private equity transaction. Five years is a typical holding period for target firms (Strömberg 2007). For our firm-level analysis, we must confront the reorganization of firms through mergers, ownership changes, partial divestitures, and acquisitions of establishments from other firms. Because it tracks both firms and establishments over time and contemporaneously links establishments to firms, the LBD offers greater scope for identifying these changes than most other business-level datasets. Nevertheless, some private equity targets undergo complex post-buyout restructurings that challenge the maintenance of high-integrity longitudinal links. We deal with this challenge in two ways. First, our firm-level analysis considers a relatively short window of two years after each buyout transaction, thereby limiting the linkage issues that arise from complex firm-level reorganizations. Second, we use our establishment-level data to assess the impact of potential sample selection bias in our firm-level analysis.

Before proceeding, we define our employment and growth rate measures. Let E_{it} be employment at establishment or firm i in year t; i.e., the number of workers on the payroll in the pay period covering March 12. The employment growth rate is $g_{it} = (E_{it} - E_{it-1})/X_{it}$, where $X_{it} = 0.5 \times (E_{it} - E_{it-1})$.[11] Employment growth at any higher level of aggregation is the weighted mean of establishment or firm growth rates given by $g_t = \sum_i (X_{it}/X_t) g_{it}$, where $X_t = \sum_i X_{it}$. We consider the contributions of expanding and shrinking establishments, establishment entry and exit, and acquisitions and divestitures to firm-level employment changes, and compare outcomes between targets and controls on each of these adjustment margins.

[11] This growth rate measure has become standard in analyses of establishment and firm dynamics, because it shares some useful properties of log differences while also accommodating entry and exit. See Davis, Haltiwanger, and Schuh (1996) and Törnqvist, Vartia, and Vartia (1985) for discussions.

IV. Establishment-Level Analysis of Employment Outcomes

A. *Nonparametric Comparisons*

We begin with an "event study" that compares employment outcomes at target establishments to outcomes at control establishments. To encompass a window of five years before and after buyouts, we consider transactions in the 1980–2000 period. As discussed above, we construct control cells as the cross product of industry, size of parent firm, age of parent firm, multi-unit status, and buyout year. Our firm size categories are 1–4, 5–9, 10–19, 20–49, 50–99, 100–249, 250–499, 500–999, 1,000–2,499, 2,500–4,999, 5,000–9,999, and 10,000 or more employees. Our firm age categories are 0–5 years, 6–10, 11–15, 16–20, and 21 or more years.[12] We use firm size and age measures to facilitate comparisons to our firm-level analysis below. Replacing firm size and age measures with measures based on establishment size and age yields similar results.

The solid curve in Figure 3A shows the employment path of target establishments around the buyout year. Establishments that came under private equity ownership between 1980 and 2000 employed 2.3 million workers as of the buyout year. The dashed curve shows the counterfactual path of employment at targets had they grown at the same rate as controls. To construct this counterfactual, we first rescale the employment of controls to match that of targets cell by cell in the buyout year. We then apply the actual growth rates of the controls to generate the dashed curve.[13] Comparing the solid and dashed curves highlights the critical need to evaluate target outcomes relative to controls. In particular, a simple comparison of outcomes at target firms before and after buyout events would produce a highly misleading impression about the employment effects of private equity.

Figure 3B tracks mean employment growth rate differences between target and control establishments from 5 years before to 5 years after the buyout year. Perhaps surprisingly, Figure 3B shows no systematic pattern of slower job growth at targets in the years leading up to buyouts. In the buyout year itself, employment growth at targets is actually 2 percentage points higher than at controls. However, there is a clear pattern of slower growth at targets post buyout, with growth differentials ranging from 0.5 percent to 2 percent per year. The differentials cumulate to 3.2 percent of employment in the first two years post buyout and 6.4 percent over five years. These results accommodate heterogeneous treatment effects over the cross product of industry, firm size, firm age, multi-unit status, and year of buyout. They recover the average treatment effect on the treated under the assumption of conditional mean independence, as we discussed above.[14]

[12] Following Davis et al. (2009), when a firm first appears in the LBD, we assign it the age of its oldest establishment. We then increment the firm's age by one year for each year it continues as a legal entity in the LBD. In this way, we avoid arbitrary increases or decreases in firm age due to the sale and purchase of establishments.

[13] To be precise, we calculate the weighted mean growth rate over cells using the weights defined at the end of Section III. The cell-level weights evolve over time in line with the growth experiences of targets (solid curve) and controls (dashed curve). For cells with multiple controls, each control receives equal weight.

[14] To be sure, consistent estimation of treatment effects also rests on the stable unit treatment value assumption (SUTVA): applying the treatment to one unit has no effect on outcomes at other units. This assumption fails if, for example, treatment effects on targets systematically alter equilibrium output and employment at controls. Given that controls greatly outnumber targets in our setting, equilibrium effects of this sort are unlikely to matter much. Moreover, the productivity effects we estimate below work to offset any output changes implied by

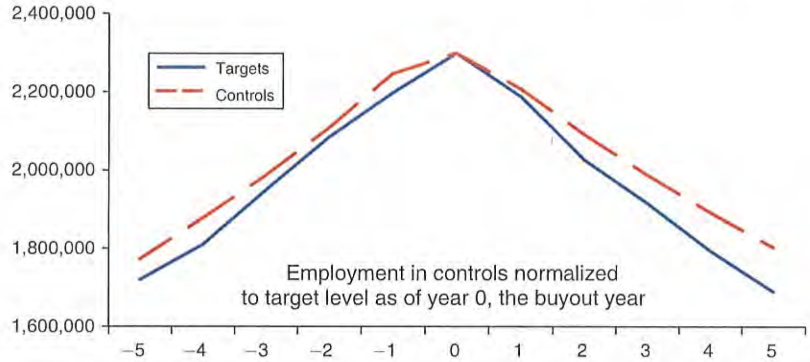

Figure 3A. Comparison of Employment Trajectory for Target Establishments to Controls, Buyouts from 1980 to 2000

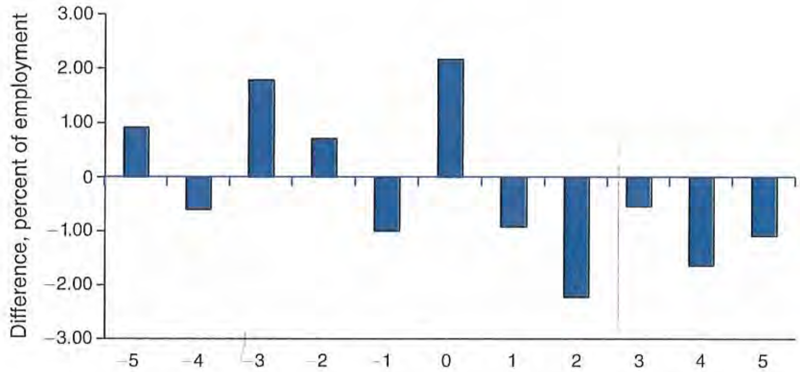

Figure 3B. Employment Growth Rate Differences Before and After the Buyout Year, Target Establishments Minus Controls, Buyouts from 1980 to 2000

Previous research finds large gross job flows relative to net employment changes (Davis and Haltiwanger 1999), raising the question of how employment responds to private equity buyouts on job creation and destruction margins. Figures 4A and 4B tell an important story in this regard: Slower employment growth at private equity targets post buyout entirely reflects a greater pace of job destruction. Indeed, gross job creation rates are greater at target establishments in the wake of buyouts. These results are interesting for at least two reasons. First, they indicate that buyouts accelerate the pace of employment change on destruction *and* creation margins, a theme we return to below. Second, Figure 4B confirms that jobs at target establishments are at greater risk post buyout than jobs at controls. As seen in Figure 5, about half of this greater risk reflects a higher post-buyout shutdown propensity at target establishments.

the estimated employment effects on target firms, further lessening the scope for equilibrium employment effects on controls.

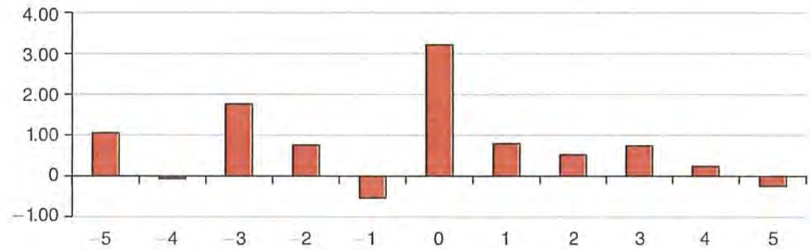

Figure 4A. Job Creation Rates before and after Buyout Year, Target Establishments Minus Controls, Buyouts from 1980 to 2000

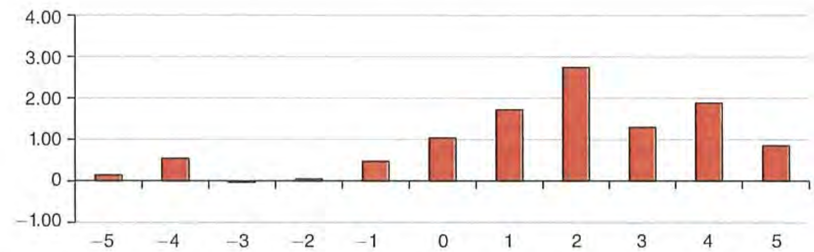

Figure 4B. Job Destruction Rates before and after Buyout Year, Target Establishments Minus Controls, Buyouts from 1980 to 2000

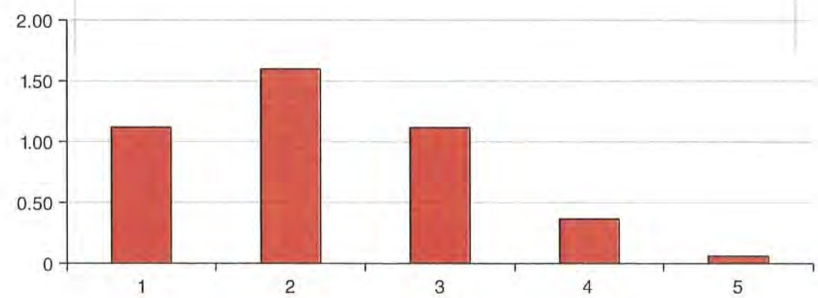

Figure 5. Employment-Weighted Establishment Exit Rates Post Buyout, Targets Minus Controls, Buyouts from 1980 to 2000

B. *Regression Analysis*

We turn now to a regression analysis that allows for additional controls and an easy calculation of standard errors in the estimated effects of private equity buyouts. Table 3 reports regression results for the buyout year and five subsequent years. Each regression involves the matched target establishments in buyouts from 1980 to 2000 and their corresponding control establishments. The dependent variable is the employment growth rate in the indicated year following the buyout. The first column in Table 3 reports the mean growth rate differentials from Figure 3B. The second and third columns report results for semi-parametric regressions that include controls for the pre-buyout growth history of parent and target firms.

TABLE 3—POST-BUYOUT EMPLOYMENT GROWTH RATES AT TARGET ESTABLISHMENTS RELATIVE TO CONTROLS, BUYOUTS FROM 1980 TO 2000

	Nonparametric comparison from Figure 3B	Semi-parametric regressions	
		ATE=ATE1	ATE1 heterogeneous
Buyout year	2.17	2.08*** (0.17)	2.28*** (0.17)
Buyout year +1	−0.93	−0.72*** (0.20)	−1.15*** (0.20)
+2	−2.23	−1.74*** (0.20)	−1.76*** (0.21)
+3	−0.55	0.00 (0.21)	0.08 (0.21)
+4	−1.64	−1.31*** (0.22)	−1.16*** (0.22)
+5	−1.09	−0.95*** (0.22)	−1.23*** (0.23)

Notes: Table entries report estimated employment growth rate differences between targets and controls in the buyout year and following years. For example, the entries for "Buyout year +2" report the estimated growth rate difference from Year 1 to Year 2 following the buyout. Each reported coefficient is for a different nonparametric comparison or regression. Standard errors are in parentheses. They are computed by the delta method in the "ATE1 heterogeneous" regression. The nonparametric comparison reflects the patterns displayed in Figure 3b. As explained in the text, this comparison allows for heterogeneous treatment effects and controls for the fully interacted cross product of 72 industries, 10 firm size categories, 6 firm age groups, multi-unit status, and buyout year. The semi-parametric regression specifications include fully interacted industry, year, firm age, firm size, and multi-unit effects plus additional controls for pre-buyout growth history. The "ATE=ATE1" specification imposes a uniform treatment effect, while the "ATE1 heterogeneous" specification allows the treatment effect to vary by firm size, firm age, and the two measures of pre-buyout growth history. The average number of establishment-level observations in each regression or nonparametric comparison is about 4.9 million. The observation count falls with each successive year following the transaction year because of target deaths and deleted observations for the corresponding control establishments.

***Significant at the 1 percent level.

We include two variables to control for pre-buyout history. One variable considers the set of establishments owned by the target firm as of year 0 (the buyout year). We set the value of this variable to the employment growth rate of these establishments from year −3 to year −1. A second variable considers the parent firm that owned these establishments in year −3. If ownership was split across multiple firms in year −3, we select the firm with the largest share of employment among these establishments. We then set the second variable to the employment growth rate of that firm from year −3 to year −1. Often, but not always, these two control variables take on the same value.

The Table 3 regressions contain a large battery of additional controls. The column headed "ATE=ATE1" includes a fully interacted set of controls for two-digit industry, firm size, firm age, multi-unit status, and year. This specification posits a common treatment effect, given by the coefficient on an indicator variable for target establishments in private equity buyouts. The column headed "ATE1 heterogeneous" includes the same set of controls, but relaxes the assumption of uniform treatment effects by interacting the private equity indicator with the six firm age categories, ten firm size categories, and the two measures of pre-buyout growth history.[15] This specification is more restrictive than the nonparametric specification in some respects but less restrictive in the inclusion of controls for pre-buyout growth history and in allowing the treatment effect to vary with pre-buyout employment growth. To recover the average treatment effect on the treated in this case, we compute a weighted average of the heterogeneous estimated treatment effects, using cell-level employment weights of targets in the transaction year. We calculate standard errors by the Delta method.

As seen in Table 3, the nonparametric and semi-parametric specifications deliver similar results. The two semi-parametric regressions also yield small standard errors and tightly estimated effects of private equity buyouts. Implied five-year cumulative employment losses at targets range from −4.7 percent to −6.4 percent, depending on specification, with somewhat smaller losses in the semi-parametric specifications.[16] In short, the evidence says that target establishments experience deeper job losses after private equity buyouts than control establishments.

V. Firm-Level Analysis of Employment Outcomes

A. *Tracking Firms*

Section IV considers outcomes for establishments owned by target firms at the time of the buyout deal. We now shift to a firm-level analysis to capture new establishments opened after the deal as well as post-buyout acquisitions and divestitures. By necessity, we restrict attention to target firms that we can track post buyout. While we can readily track establishments over time in the LBD, tracking firms is more challenging for two reasons: the disappearance of firm identifiers (IDs) in some circumstances, and irregularities in Census Bureau tracking of private equity targets involved in divisional sales.

The disappearance of a firm identifier (ID) in the LBD can occur for various reasons. One is the death of a firm and closure of its establishments. Firm death in this sense presents no problem for our analysis, and we capture such events whether they involve target or control firms. A more difficult situation involves a firm ID in year 0 that disappears in later years, even though some of the establishments owned by the firm in year 0 continue to operate. This situation can arise because of a merger or complex reorganization (e.g., different components of the original firm are bought by multiple existing firms). It is inherently difficult to define and measure firm growth when the original legal entity ceases to exist, and we exclude these observations in our firm-level analysis. To reduce the number of observations lost for this reason, we limit our firm-level analysis to years 1 and 2 after the buyout.

In the course of our data development and analysis, we discovered that the Census Bureau did not accurately track firm IDs in certain private equity transactions. Inaccuracies sometimes arose when a private equity group acquired one or

[15] See the online Appendix for explicit statements of the regression specifications in mathematical form.

[16] Smaller losses in the semi-parametric specifications point to a modest tendency for private equity to target firms with weaker employment growth prospects, which differs somewhat from the inference suggested by the pre-buyout comparison in Figure 3B. Recall that Figure 3 involves a comparison of growth rates between target and control *establishments*. In contrast, the semi-parametric regressions reported in Table 3 contain controls for the pre-buyout growth history of *parent firms*.

more divisions of a corporate entity, but not the whole firm. In principle, the Annual Company Organization Survey (sent to all large multi-unit companies) lets Census track these divisional sales. However, we identified divisional sales in which the firm ID of the (new) target firm remained the same as the ID of the selling firm. This problem did not affect the establishment-level analysis in Section IV, because we could rely on an alternative identifier—the Employer Identification Number (EIN)—to accurately identify, as of the transaction year, establishments involved in divisional sales. Unfortunately, EINs are unsuitable for tracking firms because new and acquired establishments may obtain new EINs. We therefore exclude divisional cases from our firm-level analysis for those cases when the LBD does not have an accurate ID for the target firm.[17]

For the firm-level analysis, we expand the sample period of buyout events to run through 2003. (A firm-level analysis for the period running from 1980 to 2000 yields similar results.) As reported in Table 2, our full matched sample contains 2,265 target firms from 1980 to 2003. They account for about 4.3 million workers and 104,000 establishments as of the buyout year. Excluding the divisional EIN cases that lack accurate firm IDs yields 1,874 target firms with about 3.4 million workers and 79,000 establishments.[18] Further restricting attention to firms that we can track for two years after the buyout year, including deaths, yields a sample of 1,374 firms and 3.2 million workers. This sample represents 73 percent of the matched sample with accurate firm IDs and 93 percent of their employment. The latter statistic is more relevant given our focus on employment-weighted outcomes.

B. *Firm-Level Employment Results*

Our firm-level analysis considers the same type of semi-parametric regression specifications as in Table 3. Now, however, we explore employment responses on several adjustment margins, including the entry of new establishments post buyout. As before, the regressions include the pre-buyout growth variables and the cross product of industry, firm size categories, firm age categories, multi-unit status, and buyout year as controls. We weight observations by employment, as before. To obtain the effect of interest, we rely on indicator variables for target firms.

Table 4 presents firm-level regression results for cumulative responses over the first two years post buyout. Again, we report results for an ATE=ATE1 specification that posits a uniform treatment effect, and for an ATE1 heterogeneous specification that allows treatment effects to vary with pre-buyout history and across firm age and size categories. The top row in Table 4 says that target firms shrink more rapidly than controls in the two-year period after buyouts—by 0.88 percentage points in the ATE=ATE1 specification and 0.65 percentage points in the ATE1 heterogeneous

[17] We more fully discuss tracking issues related to divisional sales and our use of EINs in the online Appendix. Online Appendix Table A.4 repeats the Section IV analysis excluding establishments owned by divisional targets with inaccurate IDs, yielding results similar to Table 3 in Section IV, but with somewhat smaller relative employment losses at targets. The similarity of establishment-based results for the full sample and the subsample suggests that our firm-level analysis is not seriously distorted by the inability to accurately track firm IDs for some divisional sales.

[18] Although our firm-level analysis sample excludes some transactions covered by the establishment-level analysis, extending the sample period through 2003 captures a large number of more recent buyouts, as seen in Figures 1 and 2. As a result, the firm-level analysis sample actually covers more employment.

TABLE 4—BUYOUT EFFECTS ON EMPLOYMENT GROWTH RATE AT TARGET FIRMS RELATIVE TO CONTROLS, BUYOUTS FROM 1980 TO 2003

	Regression specification			
Dependent variable:	ATE=ATE1		ATE1 heterogeneous	
	Buyout effect	R^2	Buyout effect	R^2
Firm-level employment growth rate from buyout year t to $t+2$	−0.88 (0.18)	0.07	−0.65 (0.16)	0.07
By adjustment margin				
Continuers	−1.57 (0.12)	0.09	−1.36 (0.11)	0.09
Creation	−1.07 (0.08)	0.16	−0.93 (0.08)	0.16
Destruction	0.71 (0.07)	0.09	0.64 (0.07)	0.09
Deaths	4.12 (0.09)	0.06	4.13 (0.08)	0.06
Births	1.80 (0.05)	0.22	1.87 (0.05)	0.22
Acquisitions	5.62 (0.05)	0.12	5.56 (0.05)	0.13
Divestitures	2.77 (0.05)	0.06	2.75 (0.04)	0.06

Notes: Employment-weighted regressions on target and control firms, with rates calculated over the two-year horizon from the event year t to $t+2$. Standard errors in parentheses. All reported coefficient values are statistically significant at the 0.01 level. The semi-parametric regression specifications include fully interacted industry, year, firm age, firm size, and multi-unit effects plus additional controls for pre-buyout growth history. The "ATE=ATE1" specification imposes a uniform treatment effect, while the "ATE1 heterogeneous" specification allows the treatment effect to vary by firm size, firm age, and the two measures of pre-buyout growth history. Each regression has 1,985,000 (rounded to nearest 1,000) observations.

specification. These estimated effects are much smaller than the cumulative two-year difference of 2.9 points for both specifications in Table 3. This comparison suggests that the additional adjustment margins captured by the firm-level analysis alter the picture of how private equity buyouts affect employment outcomes.

The remaining rows in Table 4 address the issue directly in the firm-level sample. Focus on the ATE1 heterogeneous specification, and consider first the results for "Continuers" and "Deaths," two adjustment margins captured by the establishment-level analysis.[19] Summing these two components yields a two-year employment growth rate differential of −5.49 percentage points (−1.36 – 4.13) for targets, a large difference. But target firms create more new jobs at new establishments in the first two years after buyouts, a difference of 1.87 points in favor of targets. Combining these three adjustment margins yields a differential of −3.62 percentage points for targets.[20] Finally, bringing in the role of acquisitions

[19] Unlike the establishment-level analysis, however, the firm-level analysis does not encompass post-divestment employment changes at divested continuing establishments.

[20] The two-year differential of −5.49 percent for continuers plus deaths in Table 4 (ATE1 heterogeneous) is larger than the corresponding two-year differential of −2.91 percent in Table 3. Recall that these two tables address different questions and use somewhat different samples. Table 4, unlike Table 3, excludes (i) EIN cases and other firms that we could not track post buyout and (ii) the employment changes of establishments divested in years one and two post buyout. Table 3, however, does not capture employment at post-buyout establishment births. To obtain an estimated target-control growth differential that captures all organic adjustment margins (and only organic

and divestitures reduces this differential to −0.81 points, close to the estimated differential in the top row. Table C.3 in the online Appendix reports similar results when dropping the firm age or size variables. Thus, the overall impact of private equity buyouts on firm-level employment growth is quite modest.

As a robustness check, we also estimate the average treatment effect of private equity buyouts on firm-level employment growth using propensity score methods. We construct propensity scores by fitting logit specifications, one for each buyout year, for the likelihood that a firm becomes a private equity target. The logit specification includes the pre-buyout growth variables and the cross product of industry, firm size categories, firm age categories, and multi-unit status. Our second-stage regression includes an indicator for private equity targets, as before, plus the propensity score measure interacted with year effects. Using this second-stage regression, we estimate that a private equity buyout raises firm-level employment growth by 0.26 percentage points in the first two years post buyout, with a standard error of 0.18 points.[21] Thus, under the propensity score approach, we cannot reject the hypothesis that private equity buyouts have zero net impact on employment growth at target firms.

It is worth stressing that our firm-level and establishment-level regression analyses answer different questions. The establishment-level analysis tells us what happens to employment at establishments owned by target firms as of the buyout year. The firm-level analysis tells us what happens to employment at target firms, overall and on various adjustment margins. In practice, the main difference is that the firm-level analysis picks up large differentials between targets and controls in job creation at newly opened establishments and in employment changes associated with acquisitions and divestitures.

C. *Private Equity: Agents of Reallocation within Firms?*

Table 4 and Figure 4 suggest that buyouts act as catalysts for creative destruction. Target firms exhibit more job destruction in establishment shutdowns and more job creation in establishment births, larger job losses through divestment and larger job gains through acquisition. This evidence is consistent with two distinct views. One view holds that private equity acts as an agent of change—inducing some target firms to expand relative to controls and others to retrench. According to this view, the evidence reflects a combination of (i) upsizing target firms that add establishments and jobs more rapidly than controls and (ii) downsizing target firms that shed jobs and establishments more rapidly than controls. The positive effects of buyouts on job creation and destruction then result by aggregating over upsizing and downsizing cases. A second view holds that private equity acts as an agent of restructuring within target firms, accelerating the reallocation of jobs across establishments

margins), sum the greenfield job creation differential in Table 4 (1.87 percent of initial employment in favor of targets) and the two-year growth differential from Table 3 (2.91 percent in favor of controls). This calculation yields an estimated 1.04 percent employment contraction at targets relative to controls over the first two years post buyout.

[21] The standard error is not adjusted for the first-stage estimation. As Woolridge (2002) notes, an advantage of including controls directly in the main regression is that it simplifies the computation of standard errors. He also points out that propensity score methods often yield similar results to methods that use controls in the main regression. When estimated with a linear probability model, a propensity score approach is equivalent to a one-stage approach that introduces the controls directly into the main regression, as in Table 4.

in these firms and their pace of acquisition and divestment. These two views are not exclusive because private equity could accelerate both types of creative destruction.

To evaluate these views, we now estimate buyout effects on firm-level reallocation measures. A firm's job reallocation is the sum of its gross job gains due to new, expanding, and acquired establishments and gross job losses due to exiting, shrinking, and divested establishments. Its excess reallocation is the difference between job reallocation and the absolute value of its net job growth.[22] If a given firm changes employment in the same direction at all of its establishments, it has zero excess reallocation. To the extent that a firm expands employment at some units and contracts employment at others, it has positive excess reallocation. If the firm adds jobs at some of its establishments and cuts an equal number of jobs at other establishments, excess reallocation equals job reallocation. So, if private equity acts exclusively as agents of change, the entire creative destruction response of target firms involves higher job reallocation but no impact on excess reallocation. At the other extreme, if private equity acts exclusively as agents of restructuring within target firms, firm-level job reallocation and excess reallocation rates respond by the same amount to buyouts.

Table 5 reports regression results for firm-level job reallocation and excess reallocation rates using the same specifications and two-year horizon as in Table 4. In the ATE1 heterogeneous specification, the job reallocation rate is 13.9 percentage points higher at target firms, and the excess reallocation rate is 9.3 points higher. Thus, two-thirds of the extra job reallocation associated with private equity buyouts reflects an accelerated pace of restructuring within target firms. For organic changes, the impact of buyouts on excess reallocation—6.4 percent of initial employment over two years—is actually greater than the impact on total job reallocation.[23] In short, and especially for organic employment changes, Table 5 implies that private equity acts predominantly as an agent of (accelerated) restructuring within target firms.

The regression results in Tables 3, 4, and 5 identify only the differential responses of targets relative to controls. To recover information about the levels of creation and destruction activity, we return to the nonparametric approach of Section IVA and consider a counterfactual exercise along the lines of Figure 3. Specifically, we sort target and control observations in our 1980–2003 firm-level analysis sample into cells defined by the same cross product of industry, size, age, multi-unit status, and buyout year as before. For each cell, we calculate cumulative two-year changes post buyout for each employment adjustment margin. We then generate the weighted average outcomes for targets and controls using the same approach to weighting as in Figure 3. These calculations reveal the extent of creation and destruction activity on each adjustment margin at target firms, and they tell us how target firm activity would differ if targets exhibited the same behavior as controls.

[22] This concept of excess reallocation is used often in the literature on gross job flows to quantify job reallocation within industries, regions and business categories. See Dunne, Roberts, and Samuelson (1989), Davis and Haltiwanger (1992), and, for a review of the literature, Davis and Haltiwanger (1999). Our approach here applies the same concept to the reallocation of jobs across production units within firms.

[23] By definition, overall job reallocation equals or exceeds excess job reallocation for a given firm or group of firms. Our comparison here, however, involves the difference between job reallocation and excess reallocation responses for two distinct sets of firms, targets, and controls.

Table 5—Buyout Effects on Firm-Level Job Reallocation and Excess Reallocation, Buyouts from 1980 to 2003

Estimated responses relative to controls from buyout year t to $t+2$	Regression specification			
	ATE=ATE1		ATE1 heterogeneous	
Dependent variable:	Buyout effect	R^2	Buyout effect	R^2
Firm-level excess reallocation–All adjustment margins	9.25 (0.08)	0.37	9.29 (0.09)	0.37
Firm-level excess reallocation–Births, deaths, and continuers	6.38 (0.08)	0.38	6.40 (0.08)	0.39
Firm-level job reallocation–All adjustment margins	13.81 (0.15)	0.21	13.89 (0.15)	0.21
Firm-level job reallocation–Births, deaths, and continuers	5.47 (0.14)	0.22	5.62 (0.14)	0.22

Notes: Employment-weighted regressions on a sample of target and control firms, with rates calculated over the two-year horizon from the event year t to $t+2$. Standard errors in parentheses. All reported coefficient values are statistically significant at the 0.01 level. The semi-parametric regression specifications include fully interacted industry, year, firm age, firm size, and multi-unit effects plus additional controls for pre-buyout growth history. The "ATE=ATE1" specification imposes a uniform treatment effect, while the "ATE1 heterogeneous" specification allows the treatment effect to vary by firm size, firm age, and the two measures of pre-buyout growth history. Each regression has 1,985,000 (rounded to nearest 1,000) observations.

Table 6 reports the results of these calculations. They show high rates of creation and destruction at target firms in the wake of private equity buyouts. The two-year cumulative job reallocation at target firms is 52 percent of initial employment for organic changes (panel A) and 69 percent inclusive of acquisitions and divestitures (panel B). According to the "Difference" column in panel A of Table 6, buyouts raise job creation, destruction, and reallocation rates by, respectively, 2.0, 4.3, and 6.3 percent of initial employment, which amount to 9, 19, and 14 percent of the base rates at control firms. Panel B shows that the increases in creation, destruction, and reallocation associated with buyouts are considerably larger, in both absolute and relative terms, when including acquisitions and divestitures. To check the consistency of these results with Tables 4 and 5, the two rightmost columns in panels A and B report the semi-parametric regression estimates of target-control differences. The two approaches yield similar estimated differences, and the differences are precisely estimated.

Table 6 also reveals that excess reallocation accounts for more than 95 percent of overall job reallocation for both target and control firms, whether considering organic changes or including acquisitions and divestitures. About two-thirds of excess reallocation occurs within firms for organic employment changes, 56–58 percent when including acquisitions and divestitures. Excess job reallocation within the same firm and county (not shown in the table) accounts for half of all excess job reallocation within control firms, and slightly less within target firms. Putting these results together, the movement of job positions across locations within the same firm and county account for about one-third of all excess job reallocation for organic adjustment margins and about one-quarter when including acquisitions and divestitures.

D. *Differential Employment Responses by Time Period, Industry, and Buyout Type*

The foregoing analyses could mask important differences in private equity effects by time period, industry, or type of buyout. Descriptive accounts suggest that private

Table 6—Cumulative Two-Year Job Reallocation at Target Firms and Controls, Buyouts from 1980 to 2003

Rates expressed as a percent of employment	Target firms	Control firms	Difference	From Tables 4 and 5: Difference	From Tables 4 and 5: Standard error
Panel A. Organic changes, excluding acquisitions and divestitures					
Job creation	24.96	22.96	2.00	0.94	(0.10)
Continuers	11.51	11.74	−0.22	−0.93	(0.07)
Births (entrants)	13.44	11.22	2.22	1.87	(0.05)
Job destruction	26.89	22.62	4.27	4.69	(0.11)
Continuers	13.28	12.65	0.63	0.64	(0.06)
Deaths (exits)	13.60	9.96	3.64	4.13	(0.08)
Employment growth	−1.93	0.34	−2.27	−3.75	(0.16)
Job reallocation	51.85	45.58	6.27	5.62	(0.14)
Excess reallocation	49.91	45.23	4.68		
Within-firm	33.09	27.02	6.06	6.40	(0.08)
Panel B. All adjustment margins, including acquisitions and divestitures					
Job creation	35.86	28.42	7.44	6.25	(0.11)
Job destruction	33.21	26.67	6.53	7.03	(0.13)
Employment growth	2.65	1.75	0.90	−0.65	(0.16)
Job reallocation	69.07	55.10	13.97	13.89	(0.15)
Excess reallocation	66.41	53.35	13.07		
Within-firm	38.82	29.82	9.01	9.29	(0.09)

Notes: For Target firms and Control firms, we aggregate over cells using the employment shares of targets. For cells with multiple controls, each control receives equal weight.

equity groups shifted to a more operational orientation over time, which could lead to time-varying effects on target employment. The scale of private buyout activity also increased enormously over time, which could alter the character of the marginal target and its post-buyout performance. Motivated by these observations, Figure C.2 in the online Appendix displays the evolution of target-control growth rate differences for buyouts that took place in the 1980s, 1990–1994, and 1995–2000. In each period, employment contracts more rapidly at targets than at controls in the years following private equity buyouts.

Some accounts of private equity paint a picture of aggressive cost cutting through layoffs. This characterization suggests a potential for greater post-buyout job destruction rates in labor-intensive industries, reflecting a view that cost cutters focus on the largest cost sources. More generally, there are major differences in factor cost shares, market structure, demand conditions, and labor relations that might lead to important industry differences in the responses to private equity buyouts. Motivated by these ideas, online Appendix Figure C.3 displays the evolution of target-control growth differences for three industry sectors that cover most private equity buyouts. Employment falls modestly at target establishments relative to controls post buyout in Manufacturing. Retail Trade exhibits a markedly different response pattern. In the years leading up to buyout transactions, controls and targets in Retail Trade exhibit similar growth rates. Post buyout, however, employment at target establishments falls by nearly 12 percent relative to controls over five years. The Service sector exhibits yet a different pattern. Targets grow more rapidly than controls before the buyout year but more slowly afterwards. These large industry differences serve as a caution against painting with an overly broad brush when characterizing employment outcomes in the wake of private equity buyouts.

TABLE 7— BUYOUT EFFECTS ON TARGET FIRMS RELATIVE TO CONTROLS BY TYPE OF BUYOUT, 1980 TO 2003

	Type of private equity buyout				
Dependent variable:	Public to private	Independent to private	Divisional buyout	Secondary buyout	Other
Employment growth rate from buyout year t to $t+2$	−10.36 (0.38)	10.51 (0.24)	−1.48 (0.45)	7.15 (0.58)	−6.45 (0.80)
Excess reallocation rate from buyout year t to $t+2$	5.08 (0.21)	4.69 (0.10)	20.32 (0.19)	29.79 (0.27)	6.16 (0.40)
Observations (rounded)	374,000	1,298,000	475,000	169,000	123,000

Notes: Results are based on the semi-parametric ATE1 heterogeneous specifications of Tables 4 and 5, fit separately to target and control observations for each type of private equity buyout. Standard errors in parentheses. All reported coefficient values are statistically significant at the 0.01 level.

There are also good reasons to think employment effects vary by type of buyout. Public-to-private deals may be more likely to involve target firms with a strong need for cost cutting, as in the Beatrice case (Baker 1992). Better funding access and relaxed constraints on capital investment and job creation are more likely to motivate deals for privately held firms. In light of these arguments, Table 7 reports estimated average effects on two-year post-buyout rates of employment growth and excess job reallocation by type of buyout. In public-to-private deals, target employment contracts more than 10 percent relative to controls over two years. As reported in online Appendix Table C.4, target firms in these deals experience much greater job losses due to establishment deaths and divestitures and less job creation through births. Along with the high visibility of public-to-private deals, these results help understand concerns about job loss related to private equity buyouts. In striking contrast, employment at independent targets (private-to-private deals) expands 10 percent relative to controls in the first two years post buyout. More rapid employment growth at independent targets reflects a higher pace of acquisition, consistent with the view that private equity investments facilitate firm-level expansion.[24] Most US buyout transactions involve independent targets, even though public-to-private transactions garner much more attention. In terms of buyout-year employment, independent targets account for about 63 percent more jobs than publicly held targets (Table 2).

One common pattern emerges for all deal types in Table 7: excess reallocation rates are higher at target firms than at controls. The magnitude of the target-control difference in excess reallocation varies greatly by type of transaction, but it is positive and highly statistically significant in all cases.

VI. Effects on Productivity and Worker Earnings

A. *Productivity Measurement and Sample Weights*

Our productivity analysis considers plant-level observations covered by the Census of Manufactures (CM) and the Annual Survey of Manufactures (ASM). The CM

[24] Our evidence for private-to-private deals in the United States is broadly consistent with the evidence on French buyouts and its interpretation in terms of relaxed capital constraints offered by Boucly, Sraer, and Thesmar (2011).

surveys all but the smallest manufacturing plants once every five years. The ASM, a rotating panel of manufacturing plants, samples the largest units with certainty and other units with probabilities increasing in size. Following Baily, Hulten, and Campbell (1992) and others, we compute plant-level log TFP as $\ln TFP_{it} = \ln Q_{it} - \alpha_K \ln K_{it} - \alpha_L \ln L_{it} - \alpha_M \ln M_{it}$, where i and t index plant and year, respectively, Q is real output, K is real capital, L is labor input, M is materials, and α denotes factor elasticities. Operationally, we measure plant output as shipments plus the change in inventories, deflated by industry-level price indices. We measure capital separately for structures and equipment using perpetual inventory methods. Labor is total hours of production and non-production workers. We measure and deflate energy and other materials separately, and we measure the factor elasticities using industry-level cost shares.[25]

Because the CM takes place every five years and the ASM over samples larger plants, our study of productivity outcomes adjusts for the probability that a given observation appears in a given analysis sample. To do so, we first create indicator variables for whether each observation appears in a given analysis sample.[26] We then fit year-by-year logit models to the indicator variables, obtaining estimated sample inclusion probabilities for each observation. Explanatory variables are dummies for industry (4-digit SIC or 6-digit NAICS), payroll size classes, employment size classes, multi-unit status, target status, and interactions between multi-unit status and industry. The LBD contains data on these explanatory variables for every plant-year observation. We set the propensity weight for each observation to the reciprocal of its model-implied sample inclusion probability.[27]

B. *Do Target Firms Direct Reallocation to More Productive Plants?*

Section V shows that private equity buyouts accelerate job reallocation. Much of the extra reallocation involves establishment births and deaths. We now consider whether target firms direct this reallocation activity in ways that affect productivity. To that end, we first sort target plants and controls into terciles of the same-industry/same-year TFP distribution. We then investigate how plant entry and exit probabilities vary by location in the TFP distribution for targets in comparison to controls.

Panel A in Table 8 reports estimated exit probabilities in the first two years post buyout. We obtain these probabilities from a logit model fit to an exit indicator variable, where the explanatory variables are dummies for TFP terciles interacted

[25] See Foster, Grim, and Haltiwanger (2013) for details on our measurement of inputs and outputs. As discussed in Syverson (2011), alternative methods for measuring the factor elasticities tend to yield similar plant-level TFP values even when they produce somewhat different elasticity values.

[26] Our assignment of controls to target plants for the productivity analysis is similar to the approach adopted in Section IV. Starting from our establishment-level analysis sample, we restrict attention to manufacturing plants operated by two-year continuing firms (i.e., two years from the buyout year) and industry-year cells for which the ASM/CM data contain at least one target and one control observation. We drop a few observations for which the ASM/CM employment figure differs from the LBD figure by more than 1,000. We also drop control observations on plants with more than 10,000 employees, of which there are none among target manufacturing plants.

[27] Plant and firm age measures did not improve the logit models for sample inclusion—perhaps not surprisingly, given that Census mainly relies on industry, size, and multi-unit status in the ASM sample design. We checked that the propensity-weighted analysis samples adequately match the firm size, firm age, and industry distributions of employment in the populations of targets and controls, and that they adequately reproduce changes along each adjustment margin—births, deaths, continuers, etc.

TABLE 8—ENTRY AND EXIT OF MANUFACTURING PLANTS BY LOCATION IN OWN-INDUSTRY DISTRIBUTION OF TOTAL FACTOR PRODUCTIVITY, BUYOUTS FROM 1980 TO 2003

Panel A. Plant exit probabilities in the first two years post buyout (logistic specification)

Location in own-industry TFP distribution as of the buyout year t	Probability of plant exit by year $t + 2$		p-value for difference between targets and controls
	Targets	Controls	
Bottom tercile	0.143 (0.023)	0.091 (0.002)	0.025
Middle tercile	0.112 (0.034)	0.062 (0.002)	0.139
Top tercile	0.078 (0.015)	0.067 (0.002)	0.487

Panel B. Plant entry probabilities in the first two years post buyout (logistic specification)

Location in own-industry TFP distribution in $t + 2$, two years after buyout	Probability that a plant operating in $t + 2$ entered in $t + 1$ or $t + 2$		p-value for difference between targets and controls
	Targets	Controls	
Bottom tercile	0.056 (0.015)	0.121 (0.006)	0.000
Middle tercile	0.071 (0.016)	0.078 (0.003)	0.590
Top tercile	0.127 (0.029)	0.072 (0.003)	0.058

Notes: Predicted probabilities implied by logistic regressions, using the propensity weights described in Section VIA. Standard errors are in parentheses. All reported coefficient values are statistically significant at the 0.01 level. The dependent variable in panel A equals 1 if the plant exits in the first or second year after the buyout year, zero otherwise. The dependent variable in panel B equals 1 if the establishment enters in the first or second year post buyout, zero otherwise. Entry and exit outcomes are determined using the full LBD. Explanatory variables are terciles of the own-industry × year TFP distribution interacted with target and control dummies. Terciles are defined based on a plant's position in the same-industry/same-year distribution of total factor productivity, using 4-digit SIC and 6-digit NAICS industry definitions. The panel A regression has about 107,000 observations, and the panel B regression has about 91,000 observations.

with target and control dummies. Two results stand out: First, exit probabilities decline much more steeply with TFP for target plants than for controls. Second, targets exhibit greater exit probabilities in the bottom and middle terciles of the own-industry TFP distribution. The target-control exit differential is 5 percentage points in both terciles and significant at the 2.5 percent level in the bottom tercile.

Panel B provides information about the location of new plants in the TFP distribution, where new plants are those that enter in the first or second year post buyout. We obtain entry probabilities from a logit model fit to an entry indicator variable, where, as before, the explanatory variables are dummies for TFP terciles interacted with target and control dummies. The panel B results uncover a striking contrast between targets and controls in the productivity of new plants: The prevalence of new plants in the bottom TFP tercile is only half as large for targets as controls, and the prevalence of new plants in the top tercile is nearly twice as large for targets. New plants are concentrated in the upper part of the TFP distribution at firms backed by private equity. The opposite pattern holds at control firms.[28]

[28]Previous work by Foster, Haltiwanger, and Krizan (2001), among others, highlights rapid productivity gains among young plants through learning and selection effects. Thus, the concentration of control entrants in the

C. *Quantifying Buyout Effects on Total Factor Productivity*

We can briefly summarize the evidence in Tables 5, 6, 7, and 8 as follows: Private equity buyouts accelerate job reallocation within target firms, and target firms direct reallocation activity in ways that raise TFP. We now develop additional evidence on the relationship of private equity buyouts to TFP. We then pull together several results to quantify and decompose the effects of buyouts on firm-level TFP growth.

Table 9 reports results for propensity-weighted OLS regressions that compare log TFP between target and control plants in the same industry-year. In addition to the full set of industry-year effects, the regressions include an extensive set of firm size and age effects.[29] Panels A and B consider productivity outcomes in buyout years t and $t + 2$, respectively, and panel C considers TFP log changes for continuers. The most striking results involve entrant productivity: target entrants are 18 log points more productive in year $t + 2$ than continuing control plants and 22 log points more productive than control entrants. These large TFP advantages reflect a concentration of target entrants in the upper part of the TFP distribution and the opposite pattern for control entrants (Table 8). Although the effects are smaller and the evidence less powerful, Table 9 also confirms that plant exit patterns raise TFP at target firms, absolutely and relative to control firms. In contrast, we find no evidence that target continuers experience more rapid TFP growth than control continuers. Panel C makes this point directly: the estimated target-control differential for TFP growth is one log point, with a standard error of 11 log points. If private equity buyouts improve relative TFP in continuing plants, the effects are either too small to reliably discern in our sample, or the gains mount too slowly to capture in our two-year tracking interval.[30]

To quantify the overall effect of private equity buyouts on firm-level TFP and assess the role of various adjustment margins, consider the difference-in-difference $\Delta P_t - \Delta\tilde{P}_t$ where

$$\Delta P_t = [S^C_{t+2}P^C_{t+2} - S^C_t P^C_t] + [S^N_{t+2}P^N_{t+2} - S^X_t P^X_t] + [S^A_{t+2}P^A_{t+2} - S^D_t P^D_t]$$

is the average two-year change in TFP among target firms, S denotes an employment share, P denotes a TFP value, and C, N, X, A, and D denote continuers, entrants, exits, acquisitions, and divestitures, respectively. For example, P^C_{t+2} is the average TFP among continuing target plants two years post buyout, where each plant's TFP is expressed as a deviation from mean log TFP in the same industry-year cell. The average two-year TFP differential for controls, $\Delta\tilde{P}_t$, is defined analogously. Now express the TFP terms as deviations about same-year TFP values for control continuers, cancel terms in $\Delta P_t - \Delta\tilde{P}_t$, and rearrange to obtain

bottom tercile of the TFP distribution should not be seen as evidence that entry lowers industry-level TFP over time. Nevertheless, panel B in Table 8 reveals that new plants opened by private equity targets significantly outperform new plants opened by control firms—at least with respect to early-life TFP.

[29] Adding covariates for firm size and age moves us away from an exact matching estimator but, in our view, is preferable to (i) omitting the size and age effects or (ii) a pure matching estimator that uses very coarse size-age-industry-year cells. See chapter 3 in Angrist and Pischke (2009) for a discussion of this issue.

[30] A recent study by Bharath, Dittmar, and Sivadasan (2013) considers 406 publicly held US manufacturing firms that went private in recent decades, including 115 through private equity buyouts. Consistent with our results, they find no evidence that public-to-private transitions generate productivity gains in continuing plants.

TABLE 9—PRODUCTIVITY OF MANUFACTURING PLANTS, TARGETS AND CONTROLS, BUYOUTS FROM 1980 TO 2003

Panel A. TFP in buyout year t by plant status in year t + 2			
Dependent variable: Plant-level log TFP in year t			p-value for difference between targets and controls
Plant status	Targets	Controls	
Continuers	0.016 (0.012)	Omitted group	0.180
Exits	−0.075** (0.035)	−0.032*** (0.008)	0.232
Divestitures	−0.023*** (0.027)	−0.044*** (0.007)	0.462
R^2	0.538		

Panel B. TFP in year t + 2, two years after buyout, by plant status in year t + 2			
Dependent variable: Plant-level log TFP in year $t + 2$			p-value for difference between targets and controls
Plant status	Targets	Controls	
Continuers	0.020* (0.011)	Omitted group	0.076
Entrants	0.182*** (0.055)	−0.039*** (0.011)	0.000
Acquisitions	−0.010 (0.047)	−0.030*** (0.007)	0.668
R^2	0.523		

Panel C. TFP growth at continuing plants, from buyout year t to t + 2			
Dependent variable: Change in plant-level log TFP from buyout year t to $t + 2$			p-value for difference between targets and controls
	Targets	Controls	
Continuers	0.001 (0.011)	Omitted group	0.954
R^2	0.071		

Notes: OLS regressions using the propensity weights described in Section VIA. The omitted group is continuing control plants. All specifications include industry-year effects as well as firm size and age effects. Standard errors in parentheses. There are about 107,000 observations in the panel A regression, 91,000 in the panel B regression, and 62,000 in the panel C regression. On a propensity-weighted basis, 83 percent of the target observations in panel A are continuers, 8 percent are entrants, and 7 percent are divestitures. Continuers account for 86 percent of target observations in panel B, entrants for 8 percent, and acquisitions for 6 percent. Plant status is determined using the full LBD. Since target plants are overwhelmingly (99 percent) part of multi-unit firms, this analysis focuses on targets and controls part of multi-unit firms. By design, this table considers firms that continue from the buyout year t to $t + 2$. If we add firm exits to the sample, the "Exits" row of panel A changes slightly: the coefficient becomes −0.085 (0.034) for Targets and −0.042 (0.007) for Controls.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

$$(1)\quad \Delta P_t - \Delta \tilde{P}_t = S^C_{t+2}\left(P^C_{t+2} - \tilde{P}^C_{t+2}\right) - S^C_t\left(P^C_t - \tilde{P}^C_t\right) + S^N_{t+2}\left(P^N_{t+2} - \tilde{P}^C_{t+2}\right) - \tilde{S}^N_{t+2}\left(\tilde{P}^N_{t+2} - \tilde{P}^C_{t+2}\right) - S^X_t\left(P^X_t - \tilde{P}^C_t\right) + \tilde{S}^X_t\left(\tilde{P}^X_t - \tilde{P}^C_t\right) + S^A_{t+2}\left(P^A_{t+2} - \tilde{P}^C_{t+2}\right) - \tilde{S}^A_{t+2}\left(\tilde{P}^A_{t+2} - \tilde{P}^C_{t+2}\right) - S^D_t\left(P^D_t - \tilde{P}^C_t\right) + \tilde{S}^D_t\left(\tilde{P}^D_t - \tilde{P}^C_t\right).$$

The top line of (1) isolates the contribution of target-control differences among continuing plants, the second and third lines isolate the contribution of plant entry and exit (births and deaths), and the fourth and fifth lines isolate the contribution of acquisitions and divestitures.

This decomposition is new to the literature to our knowledge and has some attractive features. It shows how to combine difference in differences estimates with an accounting decomposition that appears often in the empirical literature on firm-level productivity dynamics. To see this point, note that the expressions in parentheses can be read directly from Table 9. The shares S can be retrieved from the job creation and destruction statistics reported in online Appendix Table C.2, the manufacturing analogue to Table 6. Related, (1) lets us exploit the full LBD to compute the share variables, while relying on the ASM-CM sample to obtain the difference in differences estimates. The decomposition also sidesteps any need to compare TFP across industries or years, because all productivity terms in (1) are based on plant-level TFP deviations about industry-year means.

Table 10 exploits (1) to obtain the average TFP growth differential between target and control firms and its decomposition by margin of adjustment. Target firms outperform controls with respect to post-buyout TFP growth by 2.14 log points over two years, a large gain compared to the change of −0.38 log points among control continuers. Summing over terms in the second line of (1) yields a value of 1.59, implying that plant entry and exit effects account for 74 percent of the superior TFP growth at target firms. This result confirms the importance of the target-control differences on the entry and exit margins documented in Tables 8 and 9.

For additional insight into the nature of the entry and exit effects, we replace S^N and $\tilde{S}^N$ with their average in (1), do the same for S^X and $\tilde{S}^X$, and then recalculate the second line of (1) to obtain a value of 1.56 log points. This calculation corresponds to a counterfactual that turns off target-control differences in the *pace* of job reallocation to isolate the role of differences in its *direction*. The message is clear: The stronger directedness of job reallocation in target firms accounts for almost all of the entry and exit contribution to (1) and, indeed, more than 70 percent of $\Delta P_t - \Delta \tilde{P}_t$.

Two other remarks help put this finding in perspective. First, while directional differences are central to our explanation for superior TFP growth at target firms, they matter because entry and exit involve sizable rates of job creation and destruction. In this respect, both the pace of job reallocation and the target-control directional differences are essential. Second, reallocation rates are considerably higher outside the manufacturing sector, as readily seen by comparing Tables 6 and C.2. This fact has potentially important implications for the TFP effects of buyouts in the private sector as a whole. If we plug private sector share values from Table 6 into (1) alongside difference in differences estimates from Table 9, the implied TFP growth advantage of targets is 3.05 log points, 81 percent of which is due to entry and exit effects.

D. *Effects on Earnings Per Worker*

Tables 8–10 provide strong evidence that, on average, private equity buyouts improve operating performance, at least in the manufacturing sector. To investigate whether buyouts also affect operating margins via unit input costs, we now consider LBD data on annual earnings per worker (EPW) at the establishment level.

TABLE 10—IMPACT OF PRIVATE EQUITY BUYOUTS ON TOTAL FACTOR PRODUCTIVITY IN THE MANUFACTURING SECTOR, BUYOUTS FROM 1980 TO 2003

Estimated average two-year post-buyout change in TFP at target firms relative to controls, log points	
TFP log change differential	2.14
Excluding acquisitions and divestitures	2.01
Share of total TFP two-year change differential by margin of adjustment	
Continuing establishments	0.20
Entry and exit	0.74
Acquisitions and divestitures	0.06

Notes: Table entries are calculated according to equation (1) using difference in differences estimates from Table 9 and share measures retrieved from Table B.1 in the online Appendix, as discussed in the main text. The lower panel in the table reports the shares of the TFP log change differential on the left side of equation (1) accounted for by each term on the right side of the equation. The baseline average two-year TFP change for control firms is an estimated −0.38 log points with an estimated standard error of 0.24. This estimate is obtained from a propensity-weighted regression of the two-year log change in TFP on a constant and target status dummy in the sample of continuing target and control plants, the same sample used for panel C in Table 9.

("Earnings" encompass all taxable forms of compensation.) We follow the same approach to selecting control establishments as in Section IV, and we again exploit the size of the LBD to include an extensive set of controls in the regression specifications. Table 11 reports the results, following the same layout as Table 9. As before, the units of the estimated effects are log deviations about industry-year means.

There are several noteworthy results. First, among plants destined to exit within two years post buyout, average EPW are 9–12 log points lower than at control continuers. This evidence rejects the view that firms backed by private equity tend to close establishments with high EPW. Second, target firms divest establishments with high EPW, whereas controls do not. The EPW difference for divestitures is 22 log points in favor of reduced labor costs for targets, a huge difference. Third, panel C reports that average EPW shrink by 2.4 log points at target continuers relative to control continuers over the first two years post buyout. Fourth, we also applied equation (1) to the difference in differences estimates in Table 11 and reallocation rates in Table 6 to construct the EPW counterpart to Table 10 above. That exercise reveals an overall two-year post-buyout EPW decline of 4.0 log points for target firms (relative to controls).[31] Continuers account for 79 percent of the relative EPW reduction at target firms, acquisitions and divestitures account for 29 percent, and net entry effects actually raise relative EPW at target firms.

Online Appendix Table C.5 reports additional EPW regressions. Continuing establishments operated by target firms experience large post-buyout EPW cuts of 6–8 log points (relative to controls) in Wholesale, Retail, and Services—industries that rely heavily on less skilled labor—while FIRE shows a large EPW gain of 9 log points, and Manufacturing shows virtually no change. EPW at target

[31] Our data do not let us decompose EPW changes into the effects of hours worked and wages per unit time. The wage per unit time could fall because of either wage reductions for workers of a given quality or a shift to less skilled workers who command lower wages. However, if wages decline at target firms because of a shift to lower skill workforces (relative to concurrent changes at controls), then our results understate buyout-driven TFP gains.

TABLE 11—EARNINGS PER WORKER (EPW) AT TARGET AND CONTROL ESTABLISHMENTS, BUYOUTS FROM 1980 TO 2003

Panel A. EPW in buyout year t by establishment status in year t + 2

Dependent variable: Establishment log real EPW in year t			p-values for difference between targets and controls
Establishment status	Targets	Controls	
Continuers	0.011 (0.003)	Omitted group	0.000
Exits	−0.085 (0.006)	−0.115 (0.004)	0.000
Divestitures	0.163 (0.009)	−0.055 (0.006)	0.000
R^2	0.448		

Panel B. EPW in year t + 2, two years after buyout, by establishment status in year t + 2

Dependent variable: Establishment log real EPW in year $t + 2$			p-values for difference between targets and controls
Establishment status	Targets	Controls	
Continuers	−0.031 (0.003)	Omitted group	0.000
Entrants	0.015 (0.006)	−0.011 (0.004)	0.000
Acquisitions	0.010 (0.007)	−0.015 (0.006)	0.000
R^2	0.421		

Panel C. EPW growth at continuing establishments, from buyout year t to t + 2

Dependent variable: Change in establishment log real EPW from buyout year t to $t + 2$			p-values for difference between targets and controls
	Targets	Controls	
Continuers	−0.024 (0.002)	Omitted group	0.000
R^2	0.200		

Notes: All specifications include the full cross product of industry, year, firm size, and firm age effects. Standard errors in parentheses. Reported coefficient values are statistically significant at the 0.01 level. The reported results are for weighted regressions equivalent to a nonparametric matching estimator in panel C and approximately equivalent in panels A and B. The observation for each control establishment is weighted by the ratio of targets to controls in the same industry-year-size-age cell. Similar results obtain with equal weighting of all observations. There are about 1.7 million observations in the panel A regression, 1.8 million in the panel B regression, and 1.3 million in the panel C regression. Like Table 9, this analysis focuses on target and control plants part of multi-unit firms. Results (available upon request) including SU plants are very similar. By design, this table considers firms that continue from the buyout year t to $t + 2$. If we add firm exits to the sample, the "Exits" row of panel A changes slightly: the coefficient remains −0.085 (0.006) for Targets and becomes −0.120 (0.004) for Controls.

continuers decline by 7 log points in public-to-private deals and by 2 points in private-to-private deals. Divestitures contribute to relative EPW reductions at target firms for all private equity deal types, with an especially pronounced divestiture effect in private-to-private deals. In summary, while the details of the EPW results differ somewhat by industry and buyout type, the prevailing pattern is one of reduced EPW at target firms in the wake of private equity buyouts. Related work by Neumark and Sharpe (1996) and Bertrand and Mullainathan (1999), for example, finds mixed results for the effects of hostile takeovers on worker earnings. We believe there is much room for additional research on how ownership changes affect labor costs and worker earnings.

VII. Concluding Remarks

Our study develops new evidence on the responses of employment, job reallocation, productivity, and worker earnings to private equity buyouts. Compared to previous research, we exploit a much larger sample of buyouts, a much more extensive set of controls, and a novel ability to track outcomes at firms and establishments. These advantages enable us to overcome important limitations in previous research and address controversies about the effects of private equity buyouts on jobs and operating performance. We also exploit the strengths of our data to explore new questions about private equity's role in the creative destruction process and its impact on restructuring activity inside target firms.

Our findings support the view that private equity buyouts lead to greater job loss at establishments operated by target firms as of the buyout year. Employment at these establishments shrinks by 3 percent relative to controls in the two-year period post buyout and by 6 percent over five years. Gross job destruction at target establishments outpaces destruction at controls by a cumulative 10 percentage points over five years post buyout. Thus, pre-existing employment positions are at greater risk of loss in the wake of private equity buyouts.

While noteworthy, these results make up only part of a richer story. Using our ability to track each firm's constituent establishments, we examine how jobs respond to buyouts on several adjustment margins, including job creation at greenfield establishments opened post buyout. This aspect of our analysis reveals that target firms create new jobs in newly opened establishments at a faster pace than control firms. Accounting for the purchase and sale of establishments as well, the target-control growth differential is less than 1 percent of initial employment over two years.

Private equity buyouts involve much larger effects on the gross creation and destruction of jobs. The job reallocation rate at target firms exceeds that of controls by 14 percentage points over two years post buyout. About 45 percent of the extra job reallocation reflects a more rapid pace of organic employment adjustments, and the rest reflects acquisitions and divestitures. We find greater job reallocation in the wake of private equity buyouts for public-to-private deals, private-to-private deals, divisional sales, and secondary sales. These novel findings provide evidence that private equity buyouts catalyze the creative destruction process as measured by gross job flows and the purchase and sale of business establishments. Digging deeper, we also address two distinct views about the nature of the increased reallocation activity associated with private equity buyouts. One view sees private equity as agents of change in the sense that buyouts accelerate retrenchments at some target firms, while accelerating expansion at others. Another view sees private equity as agents of restructuring in the sense that buyouts accelerate the reallocation of jobs across establishments within target firms. We show the restructuring effect predominates, and it is the entire story for organic employment changes.

Our investigation into the effects of private equity buyouts on TFP growth in the manufacturing sector yields a highly complementary set of results. Relative to controls, target firms more aggressively close plants with low TFP, and they more aggressively open new plants with high TFP. In other words, target firms direct job reallocation activity on the plant entry and exit margins in ways that raise TFP. On average, target firms outperform control firms with respect to TFP growth by 2.1 log points over two years post buyout. More than 70 percent of the estimated TFP gains arise from private equity influence on the direction of job reallocation on plant entry and exit margins. These results refute the view that the returns to private equity rest entirely on private gains to financial engineering and wealth transfers from other stakeholders.

We also find sizable reductions in earnings per worker in the first two years post buyout. Specifically, we estimate an average two-year post-buyout reduction in earnings per worker of 4 log points at target firms relative to controls, mostly due to reductions at continuing plants. In sum, our evidence indicates that private equity buyouts improve operating margins at target firms by raising productivity and by lowering unit labor costs. The resulting gains in profitability are magnified in their impact on corporate earnings per share by the leveraged capital structures that characterize firms acquired in private equity buyouts.

By identifying a large sample of private equity transactions and linking them to the LBD, ASM, and CM, this paper also sets the stage for new research into buyout effects on capital expenditures, other input costs, profitability, and other outcomes. A rich array of input and outcome measures are available at the firm and establishment level in Census Bureau datasets that can be linked to the LBD and our data on private equity transactions. Our plans for future work also include further investigation into how and why buyout effects differ by industry and type of buyout, and an examination of outcomes in corporations that sell to private equity groups. Many divisional buyouts involve divestitures of underperforming units that may place heavy demands on senior management. Schoar (2002) documents that acquisitions can lead managers to neglect core businesses, what she calls the "new toy" effect. The LBD allows us to investigate whether the same phenomenon operates in reverse when firms sell underperforming or poorly fitting divisions, thereby freeing senior management to focus on core activities.

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