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# Private Equity Buyouts in Japan: Effects on Employment Numbers

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## Abstract (169 words)

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We examine the effects of majority buyout investments on employment numbers in unlisted firms or businesses taken private in Japan between 1998 and 2015. In our sample (n=184), regular employment grows at an uncorrected average of 12.3% during a mean holding period of 4.3 years, and an annual growth differential of +1.7pp relative to a matched-pairs sample.

Evidence from field interviews indicates that the positive growth differential primarily results from funds not cutting jobs beyond a certain threshold for reputational concerns. These concerns moderate the decisions of funds at two stages in the investment process: funds tend to avoid investing in businesses with major restructuring needs, and to prefer growth-centred to cost-cutting strategies. The effect on employment differs neither between foreign and domestic investors, nor between domestic funds owned by management and domestic funds owned by large financial or corporate groups, when controlling for other influencing factors (e.g., size, deal type, length of holding period, year-fixed effects). This implies that the reputational concerns outweigh differences in ownership and origin.

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**SER Keywords:** private equity; alternative investments; employment; Japan

**JEL codes:** J23; J63; G34; M51

## 1. Introduction

There has been considerable research on the effects of buyout investments on employment numbers, with most studies finding either significantly negative effects or no significant effects at all. Earlier research has predominantly focused on the most mature buyout markets, the USA and the UK. Our study complements the body of research with insights into the employment outcomes of buyout investments in Japan.

Buyout investing through private equity funds (hereafter PEBO) started with a corresponding regulatory change in 1998.<sup>1</sup> In the very same year, both established foreign and new domestic players became active in the Japanese market (Kubo 2014). By 2010 cumulative PEBO investments amounted to an estimated USD 57 billion, six times the figure for Australia, and formed by far the largest market in the Asia-Pacific (Fleming 2018). The advent of this new type of investor caused considerable concern. For instance, in one of the earliest articles on the Japanese buyout market, Fuchita (1999) anticipated that buyouts would take firm root as an alternative corporate restructuring tool – potentially with the same negative consequences for employment as observed in more mature buyout markets like the US, or the UK. While anecdotal evidence suggests that buyout funds in Japan have rarely engaged in large-scale cutbacks in the workforce (Yeh 2012), no systematic study exists to date.

Our study presents the first comprehensive evidence of the effect of PEBO on employment for Japan. Using a dataset of fund-led majority buyout transactions concluded in private and publicly listed, subsequently privatized businesses between 1998 and September 2015, we document substantial increases in standard employment during the holding period. We find employment grows at an uncorrected average of 12.3% during a mean holding period of 4.31 years. Comparing compound annual growth rates (CAGR) to a matched sample, we find a difference of +1.73% p.a. in employment growth relative to the matched sample. Event studies comparing growth rates before and after the buyout transactions (differences in differences) show a significant growth differential, indicating that fund managers are not merely buying trends, but are bringing about the observed increases in employment through their managerial choices.

Our data also enables testing for differences in employment effects, first, between domestic funds and their foreign (mainly Anglo-Saxon) competitors operating in Japan, and second, between domestic funds owned by management (hereafter independent funds) and domestic funds owned by large financial

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<sup>1</sup> In 1998, “The Law Concerning Investment Trust and Investment Corporation” entered into force and allowed the establishment and operation of corporation-type investment vehicles, such as hedge funds, REITs, venture capital and buyout funds. See also Hamao and Matos (2018).

or corporate groups (hereafter dependent funds). Contrary to our hypotheses, we do not find significant differences between the three types of investors: neither between foreign fund managers (mainly from Anglo-Saxon countries) and their domestic competitors, nor between independent and dependent domestic funds. Based on a series of interviews, we conclude that the relative scarcity of investment targets and the ensuing intensified competition for deals motivates all three types of investors to minimize reputational risks linked to the Japanese institutional environment, namely to the norm of employment protection. As we find, these reputational concerns moderate managerial decisions of all three types of investors.

Our efforts directly complement recent studies by Nose and Ito (2012) and Hamao and Matos (2018). Nose and Ito (2012) also study, among other aspects, employment effects of buyout investments, but in public companies (private investment in public equity; PIPE) with systematically low price-to-book ratios. They find that employment decreases by 29.3pp in the second year after investment. Thus, in contrast to our study, they document a significant reduction in employment for this particular segment of buyout investments.

Hamao and Matos' study (2018; in this journal) on activist investors in Japan inquires yet another segment of alternative investments. As in our study, Hamao and Matos do not find a substantial difference between foreign and domestic investors (2018: Table 4, Panel A on p.43). At the same time our study differs in two important aspects. Firstly, as Nose and Ito (2012), Hamao and Matos exclusively inquire minority investments into publicly listed companies. Secondly, in contrast to their analysis of stock market returns and operational performance measures, we use the number of regular employees during the holding period (i.e., when a buyout fund is in control of the target firm) to measure the impact of fund ownership.<sup>2</sup> Our choice of impact measure was motivated by the interest in the employment effects of buyout investing, and proved apt to counter the paucity of publicly available data on unlisted companies.

This paper is structured as follows. Section 2 reviews existing relevant research. Section 3 develops a set of hypotheses for the Japanese context. Section 4 describes the population, sample, and methodology. Section 5 presents quantitative results. Section 6 discusses quantitative findings and provides contextualization through qualitative evidence from a series of interviews. Section 7 concludes.

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<sup>2</sup> Because the vast majority of regular employees in Japan work full-time, their number approximates full-time equivalents (FTE).

## 2. Related studies

The effects of PEBO on employment growth have been predominantly analysed through the lens of agency theory. Buyout investments are regarded as vehicles that increase efficiency by reducing agency problems, such as the pursuit of size over profit, empire building, and operational inefficiencies, potentially leading to a reduction in employment. By acquiring a controlling equity stake, buyout funds temporarily become the dominant shareholder with an incentive to act (Berle and Means 1932; Williamson 1964). As new and temporary investors, they may be less reluctant to break implicit contracts with stakeholders than long-term shareholders or incumbent management (Shleifer and Summers 1988; Schaefer 1998).

The entrepreneurial view, on the other hand, argues that for a successful exit, investee companies must display a history of strong growth. Buyout funds can facilitate growth in a number of ways, eventually leading to increased employment (Wright, Hoskisson, and Busenitz 2001). Managers can be freed from restrictions on growth opportunities such as divisions of large integrated bureaucratically run firms in a weak competitive position, state-owned enterprises, or privately-owned firms with succession problems. Access to finance can be provided or financial constraints reduced (Engel and Stiebale 2014). A strong entrepreneurial spirit can be instilled among management (Houlden 1990; Beaver 2001), or entrepreneurial management practices developed (Bruining, Verwaal, and Wright 2013).

Comprehensive overviews and reviews of existing studies of the effects of PEBO on employment can be found in Bacon, Wright and Demina (2004), Wright, Gilligan, and Amess (2009), Lutz and Achleitner (2009), Cressy, Munari, and Malipiero (2011), Tåg (2012), and Guery, Stevenot, Wood, and Brewster (2017).<sup>3</sup> Only two studies support the entrepreneurial view with significant positive effects of PEBO on employment. Boucly, Sraer, and Thesmar (2011) report an 18% absolute increase in French PEBO businesses over an average of four years, and the global study by Bernstein, Lerner, Sorensen, and Strömberg (2016) finds a 0.6% positive annual growth differential. The majority of studies with significant results, however, document negative effects, thus supporting the ‘efficiency buyout’ relevance of agency theory. Harris, Siegel, and Wright (2005) analyse a sample of 979 buyouts and 4,877 manufacturing plants in the UK during the years 1994–1998; by comparing the mean value of post-buyout to pre-buyout employment level, they find a 61% (weighted) job loss at manufacturing plants. Cressy, Munari, and

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<sup>3</sup> We do not examine studies from buyout groups, industry associations, and labor organizations, due to the possibility of vested interests.

Malipiero (2011) study buyouts in the UK 1995–2000 and find that employment at investee companies falls by 7% as early as the first year post buyout, peaking at a 23% loss in the fourth year. Goergen, O'Sullivan, and Wood (2011) find a significant negative growth differential in the year immediately after the completion of the buyout. Davis et al. (2014) track employment by 3,200 firms in 150,000 establishments in the USA that were subject to buyouts between 1980 and 2005. At the establishment level, employment shrinks by 3% relative to controls in the two-year period post buyout and by 6% over five years, resulting in a cumulative differential of 10 percentage points relative to controls. At firm level, target firms create new jobs in newly opened establishments at a faster pace than control firms, but correcting for the purchase and sale of establishments, the growth differential is less than -1% of initial employment over two years. Therefore, the authors conclude that buyouts lead to modest net job losses, but large increases in gross job creation (entrepreneurial strategies) and job abolition (efficiency strategies).

A substantial number of studies do not find any significant net effect on employment. These include Kaplan (1989), Amess and Wright (2007), Bergström, Grubb, and Johnsson (2007), Amess and Wright (2012), and Amess, Girma, and Wright (2014). This may be due to the offsetting of efficiency and entrepreneurial approaches as documented in the study of Davis et al. (2014), or due to approaches that have no substantial bearing on employment.<sup>4</sup>

Literature has identified a number of factors potentially moderating the impact of PEBO on employment. For instance, the host country's institutional environment shapes the business activities of multinational firms in general, and of PEBO in particular (Bruining, Boselie, Wright, and Bacon 2005; Engelen, Konings, and Fernandez 2008; Lutz and Achleitner 2009; Bacon, Wright, Meuleman, and Scholes 2012; Bedu and Montalban 2013; Mingo, Junkunc, and Morales 2018). Boucly, Sraer, and Thesmar (2011) attribute the strong cumulative employment increase in French PEBO between 1999 and 2004 (+18% between the four years preceding the transaction and the four years following it) to the alleviation of credit constraints. They conclude that in countries with less mature capital markets, the role of PEBO may be to complement the public capital market and provide companies with access to otherwise unavailable external growth finance.

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<sup>4</sup> Such as financial arbitrage, which does not affect the underlying business, but creates value purely from differences in firm valuation between acquisition and divestment.

There has been considerably less work on the country-of-origin effect in buyout investing. This view, rooted in the literature of comparative capitalism, explains differences in behaviour between foreign and local firms by the differing institutional regimes of the home country: in liberal market economies (such as the US and UK), shareholders or firm owners enjoy extensive rights; in coordinated market economies (such as Germany, France, or Japan), employees enjoy greater rights, and employment is more secure (Jackson 2003; 2005). In line with this argument, a recent study by Guery, Stevenot, Wood, and Brewster (2017) finds foreign buyout investors in France significantly more likely to cut jobs than French investors.

Adding to the former two effects, there is evidence that *within-country* heterogeneity in institutional contexts may moderate buyout investing. Pe'er and Gottschalg (2011) find for the USA that the strategic choices of PEBO funds differ according to whether the state government is Republican or Democrat. Using election results as proxies for the institutional context they show how this affects the decision making of funds. In concrete terms, norms guiding the behaviour of firms towards employees, unions, the environment, and community issues are more firmly rooted in Democrat-led states, which implies higher transaction costs related to restructuring-oriented buyout strategies.

Finally, the literature points to a number of further potential influencing factors. For instance, employment effects may vary by deal type. Amess and Wright (2007) observe that wage and employment growth differ between management buyouts and management buy-ins. Boucly, Sraer, and Thesmar (2011) find that in France employment growth mainly comes from private-to-private transactions. The same study equally points to differences between industries. In addition, the literature implies size that is negatively related to the size of target firms (Wright, Gilligan, and Amess 2009).

### **3. Research questions and hypotheses**

We focus on the following two core aspects derived from previous research (see section 2). First, what is the net effect of PEBO on employment numbers? Second, does fund ownership moderate this effect? – In developing our hypotheses, it is helpful to briefly illustrate the Japanese context as documented in the literature. Japanese corporate culture has been likened to the ideal of a ‘company family’ (Bhappu 2000). Company unionism is found to continuously counter efforts at downsizing even after becoming slightly more accommodating after 1997 (Noda and Hirano 2013). Japanese labour law, as interpreted by the courts, is not only perceived as employee friendly, but also as effectively preventing layoffs other than in cases

of near-bankruptcy (Araki 2005). Anecdotal evidence (Yeh 2012) supports this view. This limits the cost-cutting potential through the reduction of regular employment and increases the importance of (organic) growth strategies relative to other value-creating strategies.<sup>5</sup> The Japanese contexts thus suggests:

H1: The distribution of employment growth rates in PEBO is left truncated.<sup>6</sup>

The literature further suggests that PEBO investors may be less reluctant to break implicit contracts with stakeholders than long-term shareholders or incumbent management (Shleifer and Summers 1988; Schaefer 1998), a view that can also be derived from Abe and Shimizutani's (2007) finding that outside directors in Japanese firms are more inclined to implement layoffs and voluntary or early retirement, while inside directors are more likely to decrease new hiring and protect incumbent employees. Taken together this suggests:

H2: Entry of a PEBO investor causes a decline in employment growth.

Turning to the question of how fund ownership moderates the employment effects of PEBO, we can chiefly distinguish between three categories of investors: between foreign and domestic, and – within the latter – between independent and dependent funds.<sup>7</sup> Pertaining to the former distinction, we expect foreign funds (mainly headquartered in Anglo-Saxon countries) to be harsher on employment than local funds, as evidenced for PEBO in France by Guery, Stevenot, Wood, and Brewster (2017) as well as for listed Japanese businesses with foreign shareholders (Ahmadjian and Robbins 2005; Noda 2012), or subjected to cross-border M&A (Fukuda 2020). Because foreign funds have weaker ties within business and society we may thus expect that they can abrogate implicit contracts such as the lifetime employment promise more easily. Secondly, as we learn from Ahmadjian and Robbins 2005, the negative effect of foreign investors on employment was significant only in businesses that are weakly embedded into domestic institutional networks (e.g., main banks and large investors). Against that background, the very fact of receiving PEBO funding implies that investee companies already are weakly embedded or are at least

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<sup>5</sup> Note that we exclude non-organic strategies such as selling off business units or M&A.

<sup>6</sup> Left truncation implies the existence of a lower negative boundary larger than the theoretical minimum of  $-1$  (cases in which operations are entirely discontinued within one year).

<sup>7</sup> By “foreign” we mean offices or subsidiaries of foreign private equity firms that are active in the Japanese buyout market. By “independent” we refer to domestic funds owned by management, whereas by “dependent” we refer to domestic funds owned by large financial or corporate groups.

willing to cut ties with local institutional stakeholder. This, in turn, implies that they are more susceptible to the effects of foreign ownership. Taken together these two considerations suggest:

H3: Employment growth is smaller under foreign than under domestic fund ownership.

Pertaining to the difference between independent and dependent domestic funds, we argue that (1) choices of individuals are dependent upon their social context (Okuno-Fujiwara 2002); and that (2) these two types of funds constitute such differing social contexts. Dependent funds belong to large firms that are deeply embedded in the Japanese stakeholder system. In these firms, long-term employment of regular employees is generally more prevalent, particularly so in very large companies. Reportedly, these companies strive to protect their regular employees even in times of recession such as after the global financial crisis of 2008 (Ono 2010; Takahashi 2018).

Thus, the social context of fund managers may influence their behaviour as investors and may moderate their choice of value-capturing strategies. Accordingly, we argue that managers in funds associated with large financial groups are more likely than independent fund managers to espouse values honouring regular employment and may be more determined to avoid lay-offs in portfolio firms. Taken together, this suggests:

H4: Employment growth is smaller under independent than under dependent fund ownership.

#### **4. Data and methodology**

Our dataset, assembled and cross-evaluated using government reports, fund websites, press searches, and data provided by a Japanese fund-of-funds, approximates the raw population of majority buyout transactions conducted by funds registered in Japan between 1998 and September 2015 as  $N_{raw} = 794$ . Deducting 234 deals with no exit confirmed by September 2015 (i.e., effects on employment still ongoing), 111 minority transactions (implying limited control over employment), 31 property-centred deals, late-stage VC transactions (7), and cases of investments outside Japan (7) gives us an effective population of  $N_{eff} = 404$ . Employment figures for 224 investee companies were obtained from Teikoku Databank, and funds were also asked to provide employment figures on their investee companies. In order to avoid ‘cherry picking’, we made full data disclosure a prerequisite for participation. Eventually, seven funds provided data on 77 exited portfolio companies. With data on 45 companies received from both sources,

size of the raw sample was  $N_{raw} = 256$ .

In adjusting the timing of employment data to entry and exit dates, we reached a mean deviation of less than one month (-10.9 days for entries, and -25.9 days for exits). Because the effective date for the bulk of employment adjustments in Japan is 1 April, we excluded 17 cases (6.64% of raw sample) with employment data given for a point in time later than the following 31 March after entry of the PEBO investor.<sup>8</sup> This reduced the raw sample to 239 cases (204 firms covered by Teikoku Databank, 66 by fund manager data, and 31 by both sources). Employment levels at entry and exit were approximated by linear interpolation and extrapolation respectively. TABLE A (appended) documents the properties of these operations.

In the next step we used 62 valid pairs of firm/year observations received from both sources to test for a potential reporting bias by fund managers. As neither one- nor two-sided tests could confirm a statistically significant difference, the data obtained from fund managers was integrated.<sup>9</sup> Notably, the difference in the mean correction factors of employment at entry and exit contributes 1.68 percentage points to a conservative estimate of employment growth under fund ownership. Finally, the following were also excluded: 35 cases with insufficient employment data on either entry or exit; seven cases with less than ten regular employees at time of investment<sup>10</sup>; and 13 cases with holding periods of up to one year.<sup>11</sup> The effective sample therefore contains  $N_{eff} = 184$  cases (or 45.5% of the population).

A number of particular methodological challenges for empirical inquiries into employment effects have been identified in the literature. Wright, Gilligan, and Amess (2009) point out that using gross headcount instead of full-time equivalents does not account for changes in employment practices, such as an increasing share of part-time work. Cressy, Munari, and Malipiero (2011) and Davis et al. (2014) highlight the risk of measurement errors, such as wrongly counting the effects of the purchase or sale of a business unit as organic growth. To this is added the risk of bias caused by selective or restricted samples. Taken

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<sup>8</sup> Dropping these observations was a judgment call between (a) a potential bias from mis-classifying employment adjustments PEBO investors as “done by previous owners”, and (b) a loss of precision through reduced sample size. Because we could not tax the potential risk of bias and given that excluding these 17 cases with poor timing of employment data and entry of PE investors only reduced the raw sample by 6.64%, we opted for (b).

<sup>9</sup> This test, however, does not preclude the possibility of self-selection among participating funds (see discussion of robustness in Section 6).

<sup>10</sup> For the smallest businesses with about 10 regular employees the minimum adjustment of 1 headcount translates into as much as a 10% change. While this implies that growth rates in smaller businesses are subject to more variation, it does not create any systematic up- or downward bias.

<sup>11</sup> Deals with holding periods < 1 year known as ‘flip-deals’. Buyout funds merely act as business brokers aiming to identify a strategic buyer for the acquired business, but do not systematically engage in value-creating efforts.

together, we have:

1. Measurement errors:

- (a) Use of gross headcount instead of full-time equivalents.
- (b) Unwarranted inclusion of cases with acquisitions or divestments of business units during the holding period.

2. Sampling issues:

- (a) Excluding buyouts that involve the sale or acquisition of business units (in an attempt to avoid measurement error 1(a) above).
- (b) Missing checks for representativeness owing to unknown populations.

Because full-time equivalents are not available, we address 1(a) by relying on the regular full-time employee headcount. Given the fact that between 1998 and 2015, the number of non-standard employees in Japan increased from 11.76 to 19.79 million (headcount; CAGR 3.35%), while standard employment was decreasing from 37.94 to 32.77 million (headcount excluding executives; CAGR -0.86%), the regular employee headcount can be considered a strongly conservative measure of total employment. For 1(b) we check all transactions in the sample for acquisitions and divestments of business units during the holding period and identify 17 cases (or 6.64% of the raw sample). While dropping these cases may imply a risk in terms of 2(a), we were able to safely do so because our dataset enables a comparison of the sample and its subsets with the population. Here we found the sample well-balanced with regard to an important number of dimensions such as its distribution over time (App. Table B), the size of investee companies and duration of holding period (App. Table C), and the sample composition by deal type, fund category, and industries (App. Tables D and E). Accordingly, we believe that our case is not subject to any major issue of type 2(b).

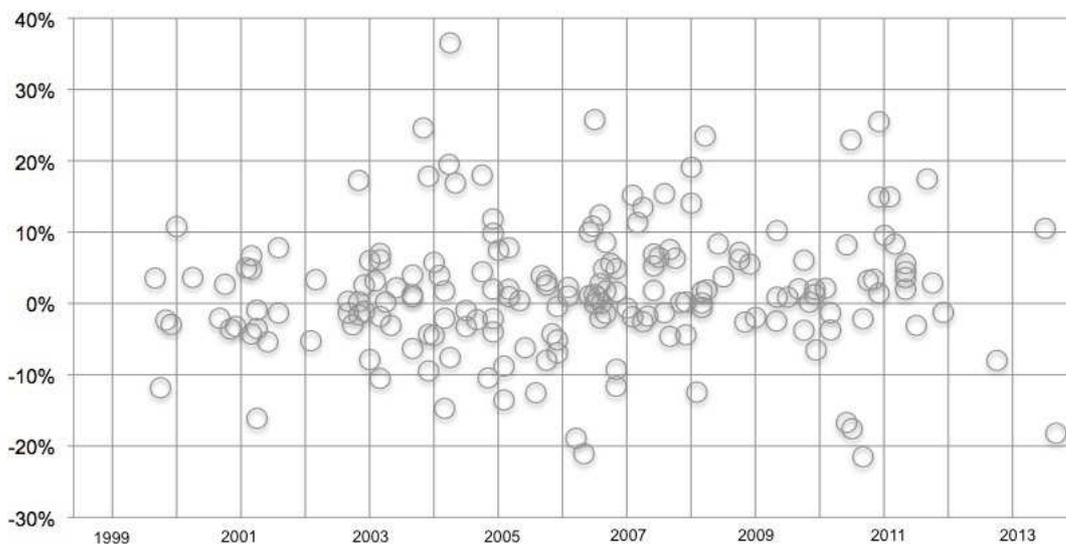
## 5. Results

We start with the absolute effect of PEBO on employment numbers. We first compare standard (regular) employment levels at entry and exit of fund investors and find a positive bottom-line effect with a mean increase of 12.26% during an average holding period of 4.31 years. In the sample subset consisting of data obtained from Teikoku Databank ( $n = 137$ ) this figure reduces to a still impressive 10.44% (cf. Table 1, section I; overleaf).

This mean increase, however, says nothing about the distribution of growth rates in the firms

subjected to PEBO transactions. Our H1 suggests that it might be left truncated, because compliance with the institutional framework should imply a lower boundary to employment adjustments. In order to be able to test for this distributional characteristic, we standardize growth rates in our sample by using CAGR. As can be seen in Figure A (below), we find hardly any values below  $-20\%$ , a reduction in employment that can be attained using a combination of natural fluctuation (around  $10\%$  for small and medium businesses), and voluntary retirement schemes.

FIGURE 1: COMPOUND ANNUAL GROWTH RATES OF STANDARD EMPLOYMENT DURING HOLDING PERIOD BY ENTRY YEAR



Recent literature finds the distribution of growth rates in economic variables most closely follows a Cauchy distribution (Williams, Pinto, Brijesh, and Park 2015). Using a maximum likelihood estimator<sup>12</sup> we fit our data to a Cauchy distribution.<sup>13</sup> As the Cauchy distribution is symmetric, its location parameter should coincide with the sample mean in the absence of truncation. If left truncated, this means that the sample mean is larger than the location parameter. In order to determine whether the estimated location parameter of  $1.041\%$  (SE  $0.455$ ) may reasonably coincide with the sample mean ( $1.775$ ), we determine a  $90\%$  one-sided interval for the location parameter from standard errors, which yields a critical value ( $1.623$ ) below the sample mean. We further verify this finding by using a bootstrap function<sup>14</sup>, which yields a

<sup>12</sup> R package 'fitdistrplus'; see: <https://cran.r-project.org/web/packages/fitdistrplus/fitdistrplus.pdf>

<sup>13</sup> None of Kolmogorov-Smirnov, Cramér-von Mises, and Anderson-Darling statistics suggest rejection.

<sup>14</sup> 'boot.ci' function of R's package 'boot'; see: <https://cran.r-project.org/web/packages/boot/boot.pdf>

more conservative threshold (1.710), still below the sample mean. We also use the estimated Cauchy distribution to calculate the probability of obtaining all 184 realizations larger than the minimum value of  $-0.215$  in our sample. This yields a probability of only 0.02% even when excluding the interval  $[-\infty, -1]$  for theoretical reasons (businesses cannot have a negative number of staff). Taken together these tests confirm that the left part of our sample contains significantly fewer realizations than the estimated Cauchy distribution, i.e., confirming the hints drawn from Figure A. This evidences that funds are abstaining from large-scale dismissals of staff and corroborates H1.

While the left-truncation of the Cauchy distribution evidences the existence of a lower boundary of employment growth, it remains unclear whether these growth rates are any different from growth rates in a sample of firms similar in terms of size, industry, and time span, that have *not* been subject to PEBO investments. We use a matched-pairs analysis to answer this question and find evidence of a significant positive contribution of PEBO investments to employment growth relative to the development in the matched-pairs sample. We create a matched sample from the approximately 5,000 businesses included in the Toyo Keizai Unlisted Company Edition (1998–2015) using entry and exit dates, industry classification, and number of employees at the time of investment (entry).<sup>15</sup> Ideally, we would have been able to use performance measures for this matching in order to account for earlier trends. For example, if PE investors had invested into businesses with above-average performance, this would have given them a head-start in terms of employment growth. However, this would have meant to substantially reduce our effective sample to  $n = 132$ , for which useful performance data is available. To tax a potential bias from this, we cross-checked the performance distribution (after-tax return on sales<sup>16</sup>) for these 132 cases against the performance expected for a random sample of businesses with the same sizes, industries, and entry years as in our sample. Doing so we found a mean difference of only +0.14pp (details on this are in App. Table I). Accordingly, we believe that the risk of a systematic bias is very limited.

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<sup>15</sup> The matching of entry and exit timing cancels out year effects. Relying on unlisted businesses helps to account for systematic differences in corporate governance in general (Fukuda et al. 2018), and for employment practices in particular (Konzelmann et al. 2006). Industries are subject to differences in life- and business cycles and are thus taken account of in a great many of studies (e.g., Guery et al. 2017). Size (number of employees) is relevant because we find that growth declines with size (as documented in our Table 3). We achieved a 100% match for the first three categories and were able to keep average deviation of employment numbers in the matched sample at only 7% above our original sample (SD = 0.49). Details on industry classifications for matching process are documented in the Notes to appended Table E.

The estimated impact of size is around +1.5% p.a. per every tenfold increase in regular headcount for the three models used. Accordingly, the estimated loss of precision from our matching is negligible at about 0.07pp (from SD), and even more so for the mean bias at about 0.035pp (from mean difference).

<sup>16</sup> Operating margins would have provided a clearer picture, but the only performance data available for our cases (from Teikoku Databank) are after-tax profits, and sales.

TABLE 1: STANDARD EMPLOYMENT GROWTH UNDER FUND OWNERSHIP (%)

	Original sample					Sample 50+ (n=159)	
	mean	p-value	Confidence (90%)			mean	p-value
			lower	upper	one-sided		
<i>(I) Total growth during holding period</i>							
Full sample (n=184)	<b>12.26</b>	0.00	6.78	17.75	7.99	<b>11.55</b>	0.00
Subset 'Teikoku' (n=137)	<b>10.44</b>	0.01	4.24	16.64	5.61		
Subset 'Fund' (n=47)	<b>17.57</b>	0.00	5.93	29.20	8.50		
<i>(II) Compound annual growth rates</i>							
Full sample (n=184)	<b>1.63</b>	0.01	0.56	2.70	0.79	<b>1.62</b>	0.02
Subset 'Teikoku' (n=137)	1.12	0.12	-0.06	2.29	0.20		
Subset 'Fund' (n=47)	<b>3.11</b>	0.04	0.74	5.49	1.26		
<i>(III) Differences in compound annual growth rates relative to matched sample</i>							
Full sample (n=184)	<b>1.70</b>	0.02	0.30	3.10	0.53	<b>1.63</b>	0.03
Subset 'Teikoku' (n=137)	<b>1.62</b>	0.04	0.07	3.16	0.32		
Subset 'Fund' (n=47)	1.94	0.23	-1.21	5.09	-0.70		
<i>(IV) Compound annual growth rates corrected for industry effects and labour market trends</i>							
Full sample (n=184)	<b>1.77</b>	0.01	0.48	3.07	0.69	<b>1.78</b>	0.01
Subset 'Teikoku' (n=137)	<b>1.47</b>	0.05	0.02	2.92	0.26		
Subset 'Fund' (n=47)	<b>2.65</b>	0.07	-0.16	5.47	0.29		

*Notes:* Subset 'Teikoku' includes cases exclusively involving data from Teikoku Databank; subset 'Fund' includes all cases involving data obtained from funds. Bold font indicates significance of 90% or better. For (IV) sample industries Nr. 1 through 13 as in App. Table E attributing to the following categories of the *Labour Force Survey*: 1 to "Medical, health care and welfare" (from 2003) and to "Services" (until 2002); 2 to "Construction"; 3, 4, 6 and 8 to "Manufacturing"; 5 to "Finance & Insurance" (from 2003) and to "Financing & insurance, real estate" (until 2002); 7 to "Information & communications" (from 2003) and to "Transport & communication" (until 2002); 9 to "Information & communications" (from 2003) and to "Transport & communication" (until 2002); 11 to "Wholesale & retail trade" (from 2003) and to "Wholesale & retail trade, eating & drinking places" (until 2002); 12 to "Services, N.E.C." (from 2003) and to "Services" (until 2002); 13 to "Transport & postal activities" (from 2003) and to "Transport & communication" (until 2002).

Comparing our sample to its matched pairs yields a +1.7pp annual growth differential (cf. Table 1, section III). To test the robustness of this analysis, we finally compute our test statistic using an industry-year adjustment as used by Kaplan (1989), Smith (1990), and Sousa and Jenkinson (2013) in the buyout

industry context.<sup>17</sup> Results from this comparison of growth rates by industries and years are qualitatively similar, confirming our matched-pairs analysis. Results are summarized below in Table 1, section IV.

While Table 1 provides substantial evidence of positive employment growth during PEBO ownership compared to a matched sample, it leaves causation largely unresolved, because buyout funds may simply make smart choices and buy into existing growth trends. To address this problem, we conduct event studies comparing growth rates before and after PEBO investment applying a differences-in-differences approach and calculate the change in growth differentials between our sample and its matched pairs. When comparing growth rates during the first year after investment to growth rates during the last year prior to investment, we do not find any significant difference (c.f. Table 2(I), subset A). However, shifting the focus to the second year under fund management produces a significant positive growth differential, a finding also robust to excluding data obtained from fund management (c.f. Table 2(I), subsets B and C). To check the robustness of our matched-pairs analysis, we use growth differentials between industry-year averages (as above) to compare with the differentials observed in our sample. Again, this yields qualitatively similar results (c.f. Table 2(II)). Evidence from these event studies, therefore, confirms that funds create a positive growth differential in investee companies. Results further indicate that growth strategies of PEBO investors become effective already from the second year after their initial investment. This implies rejection of H2, which suggests that the entry of a PEBO investor causes a decline in employment growth.

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<sup>17</sup> We use the *Labour Force Survey* (Statistics Bureau of Japan 1998–2015) to adjust our data for the relevant industry-year-specific changes. We do so by expanding our employment figures by the inverse of the relative change in industry-specific employment figures during the holding period. The *Labour Force Survey* has monthly data from 2002, enabling almost perfect matching of entry and exit dates. As only annual data is available before 2002, we use weighted averages for this earlier part of the sample. Our data includes public-sector employment and are not corrected for size. Yet, these two potential biases are minor and conservative. Total annual employment growth between 2000 and 2015 including public sector is only marginally smaller than if excluded (CAGR 0.09%). Also, our sample contains larger businesses (median staff = 200) than the *Labour Force Survey*. As documented in Table 3, every tenfold increase in size implies around 1.5% less employment growth. Accordingly, the net effect of the two biases is towards the conservative.

TABLE 2: DIFFERENCES IN DIFFERENCES FOR EMPLOYMENT GROWTH (PP)

Reference period <sub>year</sub> (subset)	n (€'Funds')	Mean (p-value)	Confidence (90%)			Min	Max
			lower	upper	one-sided		
<i>(I) Sample vs. matched pairs</i>							
Entry <sub>1</sub> – Entry <sub>-1</sub> (A)	81 (10)	-0.85 (0.77)	-5.54	3.83	-4.50	-127.5	48.02
Entry <sub>2</sub> – Entry <sub>-1</sub> (B)	65 (7)	<b>7.91</b> (0.01)	2.86	12.95	3.97	-70.86	83.28
Entry <sub>2</sub> – Entry <sub>-1</sub> (C)	58 (0)	<b>5.70</b> (0.06)	0.64	10.76	1.76	-70.86	71.21
<i>(II) Sample vs. industry-year-adjusted values</i>							
Entry <sub>1</sub> – Entry <sub>-1</sub> (A)	81 (10)	0.41 (0.89)	-4.54	5.36	-3.45	-143.53	62.91
Entry <sub>2</sub> – Entry <sub>-1</sub> (B)	65 (7)	<b>7.14</b> (0.02)	2.14	12.13	3.25	-70.91	67.20
Entry <sub>2</sub> – Entry <sub>-1</sub> (C)	58 (0)	<b>6.27</b> (0.06)	0.93	11.61	2.11	-70.91	67.20

*Notes:* Composition of subsets A and B results from data availability; subset C corresponds to subset B minus the cases involving data obtained from fund management. Bold font indicates significance of 90%, or better.

Hypotheses 3 and 4 pertain to the moderating effect of fund ownership on the employment impact. Ownership type includes domestic funds associated with large financial groups, domestic independent funds, and foreign (mainly Anglo-Saxon) funds. We have suggested that foreign funds generate less (H3), and domestic funds associated with large financial groups more employment growth (H4), both relative to independent domestic investors. We first estimate growth differentials of our matched-pairs analysis, controlling for other potential influencing factors, namely earlier growth, length of holding period, deal type, size, informational advantages of fund managers, and a flag for data obtained from funds (compare Table 3 for estimation output, and variable definitions). For robustness concerns we estimate two more variants of the dependent variable: employment growth by the industry-year approach; and an average of the former two. The evidence is clear: none of the variants produces a significant estimate for foreign-owned funds, nor for the contrast between domestic dependent and domestic independent funds. Accordingly, we reject H3 and H4.

TABLE 3: ESTIMATION OUTPUT FOR COMPOUND ANNUAL EMPLOYMENT GROWTH

Coefficients (%) (SE)	Full sample (n = 184)			Sample 50+ (n = 159)		
	MPD	IYA	AVG	MPD	IYA	AVG
<i>Fund ownership</i>						
- Foreign	0.261 (2.171)	-0.834 (2.084)	-0.277 (2.038)	1.028 (2.302)	0.515 (2.219)	-0.770 (2.173)
- Independent	1.163 (1.728)	0.141 (1.659)	0.668 (1.623)	1.565 (1.931)	0.777 (1.865)	1.219 (1.825)
<i>Controls</i>						
Pre-entry growth	<b>14.911</b> (5.670)	7.704 (5.268)	<b>11.580</b> (5.475)	<b>11.216</b> (6.218)	3.575 (5.682)	7.653 (5.970)
<i>Holding period</i>						
- 3 years	3.548 (2.223)	2.842 (2.139)	3.163 (2.089)	3.940 (2.419)	<b>3.911</b> (2.347)	3.874 (2.292)
- 4 years	<b>4.277</b> (2.349)	<b>3.992</b> (2.267)	<b>4.056</b> (2.209)	<b>4.497</b> (2.489)	<b>5.551</b> (2.425)	<b>4.926</b> (2.361)
- 5 years	3.413 (2.684)	<b>4.964</b> (2.591)	4.130 (2.528)	3.787 (2.992)	<b>6.164</b> (2.909)	<b>4.896</b> (2.841)
- 6 years	3.928 (3.007)	3.334 (2.983)	3.566 (2.826)	2.521 (3.288)	2.461 (3.183)	2.355 (3.106)
- 7+ years	0.737 (2.634)	1.710 (2.520)	1.146 (2.467)	0.534 (2.909)	2.554 (2.801)	1.465 (2.743)
<i>Deal type</i>						
- Private-to-private	2.900 (2.232)	2.234 (2.146)	2.545 (2.098)	3.445 (2.559)	2.313 (2.467)	2.864 (2.416)
- Public-to-private (TP2)	<b>3.958</b> (2.157)	2.671 (2.068)	3.296 (2.025)	3.259 (2.375)	1.735 (2.283)	2.499 (2.240)
- Secondary	4.900 (3.193)	4.208 (3.061)	4.587 (2.996)	3.573 (3.462)	2.141 (3.333)	2.929 (3.265)
Log(size)	<b>-1.321</b> (0.591)	<b>-1.540</b> (0.566)	<b>-1.435</b> (0.554)	<b>-1.977</b> (0.806)	<b>-2.385</b> (0.780)	<b>-2.171</b> (0.763)
Financials	<b>6.597</b> (3.324)	<b>6.155</b> (3.189)	<b>6.374</b> (3.121)	<b>7.024</b> (3.822)	<b>6.099</b> (3.677)	<b>6.556</b> (3.607)
<i>Year-fixed effects</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Flags</i>						
Fund Data	2.658 (1.813)	2.770 (1.726)	2.704 (1.696)	2.328 (1.851)	2.885 (1.774)	2.599 (1.744)

Notes: Bold font indicates significance of 90% or better. F-statistics are significant at 95% or above for all set-ups but IYA (about 90%) and variance inflation factors are all well below 3. Standard residual analytics (residuals vs. fitted, residuals vs. leverage, normal q-q, scale location, and heteroscedasticity) did not indicate any major concerns. Three cases of potential outliers were re-examined, but no measurement errors were found.

Abbreviations: MPD = matched-pairs differences; IYA = industry-year approach; AVG = average of MPD and IYA.

Variables: "Foreign" and "Independent" contrasts with the base category of funds associated with large financial groups.

"Pre-entry growth" controls for prior growth and is employment growth during the 12 months prior to investment;

"Holding period" contrasts deals with particular lengths (3, 4, 5, 6, 7+ years) to the base category of up to 2 years;

"Deal type" contrasts private-to-private, public-to-private, and secondary deals to turnaround deals (base);

"Size" corrects for decreasing growth opportunities and is log of number of regular employees at entry;

"Financials" is an industry dummy and corrects for informational advantages of fund managers;

"Year fixed effects" (entry) account for business cycle fluctuations; base category includes pre-2001 and post-2011 cases to keep variance inflation at a reasonable level (each <7 cases; compare App. Table A). "Fund data" marks data obtained from funds to account for potential self-selection in survey participation.

Critically, data provided by fund management were evidently subject to a self-selection bias (compare the close-to-significant estimates of around 2.7pp for flag 'Fund Data' in Table 3). This implies a need to assess the representativeness of sample subsets used earlier for assessing the overall impact of PEBO investments (Table 1), and for our event studies (Table 2). We do so by approximating the baseline impact

on subset means as the sum-product of the structural differences between the sample and its subsets, and the estimated influence on employment growth. Our approximation of the implied biases challenges neither our finding of positive employment growth in businesses under fund ownership, nor that of funds as bringing about a positive growth differential (c.f. App. Table F). As a further robustness check we also run a model with a more granular version of the deal type control (turnaround vs. business successions, MBOs, divestments, and secondaries), which yields comparable results (see App. Table G).

For very small businesses in our sample, even a headcount change of one employee translates into substantial relative change (e.g., changing headcount by one in a business of just ten regular employees). While as a general implication such heteroscedasticity only means that OLS loses efficiency, but remains unbiased, chances are that a few influential observations cause log of size to become a significant moderator (compare Table 3). For testing this possibility, we build a sample “50+” that excludes all business with less than 50 employees at entry of the PEBO investor. As it turns out, we obtain very similar results not only for our estimates of growth rates (Table 1), but also for our multivariate models (Table 3). Notably, in the latter, the control for log(size) remains significant, which implies that the convex shape of this relationship is not a random result from larger variation at the lower end of the distribution.

## **6. Discussion**

Our main findings can be summarized as follows. First, we observe significant positive average employment growth in businesses under fund ownership and are able to evidence a left truncation of employment growth rates, i.e., the existence of a lower boundary to employment reductions (H1). Second, employment growth is significantly higher than in comparable firms, and event studies show that funds cause a positive growth differential from their second year of ownership (see Table 2). This implies rejection of H2. Third, the effect on employment in businesses under Anglo-Saxon fund ownership is no different from that of their domestic competitors, implying rejection of H3. Fourth, domestic independent funds do not create less employment than domestic dependent funds. Hence, we reject H4.

The most important difference between our results for Japan and earlier research is the significant positive effects on employment growth. For Japan, this seemingly contrasts with Nose and Ito’s (2012) finding of substantial job cuts in Japanese PIPE investments. However, they focus on another segment of PE

investments, which defies any direct comparison.<sup>18</sup> But even among the few studies on other economies that allow for such direct comparison, only the study Boucly, Sraer, and Thesmar's (2011) reports employment growth of a similar magnitude for buyout investments in France. In their sample, employment growth is explained by funds alleviating credit constraints for medium-sized unlisted companies. In contrast, this cannot possibly explain the positive PEBO effects for Japan, because apart from the period 1997–2003 access to finance in Japan has been intact (Koo 2009:46), a finding even extending into the years after the 'Lehman shock' (2009/10).

Literature further points to size as a moderating factor, with larger businesses less likely to grow their number of staff (Wright, Gilligan, and Amess 2009). Given that the mean size of investee companies in our sample is substantially smaller (418 employees) compared to figures for most Western economies (e.g., 1,811 in the US study by Davis et al. (2014: 3963)), part of the employment growth observed for Japan may be a result of smaller average business size. Using the moderating effect of size as estimated in Table 3 of about –1.4pp per annum for every tenfold increase in size and expanding with the log of the size multiple ( $\log(1811/418) = 0.64$ ) we may tax this effect at around 0.90pp. Accordingly, the moderating effect of smaller business sizes adds up to an annual difference of less than 1pp.

Differing growth rates by deal type might offer some additional explanation. As the dummy for the deal categories in Table 3 essentially tests whether employment growth is higher than for turnaround deals, applying one-sided tests is appropriate. Looking at the corresponding standard errors, we understand that 7 of the 9 estimates in Table 3 actually pass this test at 90% (all but private-to-private in models IYA and AVG). The difference maximizes between turnaround deals and secondary buyouts, with estimates ranging from 4.2 to 4.9pp of employment CAGR. This may relate to the previous fund ownership of secondary buyouts having already sought to improve efficiency (including in employment matters); hence, value creation needs to rely on growth strategies. While these findings are interesting in their own right, it seems that they equally do not suffice to explain the strong absolute growth observed for buyout targets, because secondary transactions are not unique to the Japanese market.

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<sup>18</sup> In our view, the systematic selection of strongly undervalued businesses by PIPE investors arguably explains this finding. By our understanding, Nose and Ito's 2012 employment figures are not differences from a matched-pairs analysis, but absolute values from their original sample. While this means that there is no direct evidence on such selection effect, our view builds on a comparison of the mildly negative pre-entry employment growth of -1.5% in our sample against the median price-to-book ratio of 28.6% in their sample (calculated from Nose and Ito 2012: Table 2).

Yet another route to explaining the magnitude of employment growth might be the realization of untapped potential through the seconding of professional management into investee companies. As Sugiura reports, this particularly applies to smaller businesses and to business succession deals (Sugiura 2018). However, interacting size with "private-to-private" transactions to explore this possibility for the three models in Table 3 only showed that the interaction effect neutralizes the main effect of size.

Finally, funds could alleviate impediments to growth from insufficiently professional management and poor access to human resources (Sugiura 2018), particularly for businesses in rural areas. Seconding management to such investee companies may resolve both issues. However, introducing a dummy for deals involving investments into businesses in rural areas<sup>19</sup> did not produce technically significant estimates for either of the models in Table 3. Interestingly, all estimates came with negative signs (-1.7 to -2.8pp) suggesting that private equity funds cannot compensate for the general lack of opportunities in rural areas.

Taken together, it seems that neither of these aspects can fully explain the magnitude of employment growth during the holding period.

Using event studies, we were also able to evidence a causal effect of PEBO on employment from the second year after their entry. Contrary to our hypothesis H2, the growth differential caused by PEBO investors is positive. This is a rather surprising result in two respects. First, because it contradicts our hypothesis, by which we have expected outsider owners to be less reluctant to cut employment. Yet, taken together with our finding of a left-truncation (lower boundary), and the need of investors to create value added, growth can turn a necessity, and thus displace downsizing. We will revert to this in our discussion of insights obtained from interviews. Second, it seems surprising to find employment adjustments from the second year already in the light of the broadly held view that substantial increases of headcount in Japanese businesses require more time because they strongly depend on the hiring of new graduates (c.f. Kato 2001, Kester 1991). This view, however, strongly focuses on large and very large businesses where mid-career hiring is largely uncommon. However, with median of 200 regular employees, the firms in our sample are predominantly SMEs. Japanese SMEs, in turn, are known to rely on mid-career hiring quite substantially (Takeuchi and Wakabayashi 1998), and can thus more swiftly build up headcount, a trend

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<sup>19</sup> Businesses headquartered outside the greater urban areas of Sapporo, Tokyo, Nagoya, Kyoto-Osaka-Kobe, and Fukuoka-Kitakyushu.

that has reportedly increased since the early 2000s (Zhu and Hennings 2019). Eventually, in our interviews with funds (further details below), respondents confirmed that they regularly use mid-career hiring through specialized agencies.

We now turn to our second question on whether the type ownership matters. We neither find significant differences between foreign and domestic funds (H3), nor between domestic dependent and domestic independent funds (H4). The former contrasts with Guery, Stevenot, Wood, and Brewster's (2017), who find that Anglo-Saxon buyout investors are significantly more likely to cut jobs in France than domestic investors, particularly so in the aftermath of the financial crisis. They attribute this finding to efforts by French authorities aimed at encouraging French funds to mitigate job losses. They also muse that French funds might have had a greater notion of responsibility to their immediate social environment than outside entrants, and a more nuanced understanding of the long-term effects of present practices.<sup>20</sup> Thus, while France and Japan might share a similar value system with regard to employment protection, it is only in France that foreign funds are more likely than their domestic counterparts to resort to employment reductions as a value-creating strategy.

Similarly, we have to reject H4 about dependent investors being more employment-friendly. While, due to an absence of studies, our finding does not challenge any prior empirical research on PEBO investment in Japan, it still defies our initial intuition. To make up for this lack of reference studies, we add this finding to our agenda for a series of field interviews (see below).

Among the controls used to isolate the effect of ownership (Table 3), the dummy-coded categorical variable for length of holding period produced significant positive parameter estimates with maximum values for deals exited within four, and also five years. This is a notable finding as it points to a non-linear relationship between the length of holding period and the effect of PEBO on employment. Tentatively, we may explain this inverse U-shaped relationship as the result of two combined effects. First, strategies aimed at boosting organic growth require time to play out in terms of employment, as documented in our event studies (see Table 2). In contrast, for longer holding periods, we may expect marginal effects of a change in ownership/strategy to decrease. Taken together this would imply smaller

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<sup>20</sup> In contrast, Bacon, Wright, Meuleman, and Scholes (2012) suggest some adaptation of Anglo-Saxon buyout funds to local host country contexts. Adopting a pan-European perspective, they find that firms owned by Anglo-Saxon funds are as likely to introduce high-performance work practices as those owned by non-Anglo-Saxon buyout firms, suggesting some adaptation to the local host country contexts of buy-outs.

growth in deals with very short as well as with very long holding periods relative to deals with holding periods of medium length.

To triangulate our findings, we conducted a total of 30 in-depth, semi-structured interviews in July 2015 and January 2016 with management of investee companies (4), fund managers (17), company labour unions (5), and intermediaries and advisers (4).<sup>21</sup> Interviews were subjected to pilot testing.<sup>22</sup> With regards to the overall positive impact on employment, fund managers cited a number of reasons for the lower boundary on employment reductions (Figure 1). First, large-scale employment reductions are rarely necessary because the majority of deals concerns small to medium-sized companies that tend to have less slack than large companies. Domestic interviewees perceived foreign funds as taking a harsher stance on employment than Japanese funds but noted that their focus is on larger deals. Both explanations are consistent with our finding that size is negatively related to employment growth (see Table 3).

Second, funds tend to consciously avoid deals requiring major employment reductions for concerns about reputation. This directly materializes as improved (or at least ‘intact’) chances of winning future deals. Respondents across all three ownership categories pointed to the dominance of the selling side in the Japanese buyout market (“too much capital chasing too few targets”) as the major reason for their moderation in employment reduction. Firms seeking for or willing to accept PEBO investors may actually choose between a number of competing funds, and selling to a buyout fund that will eventually make drastic cuts in the workforce negatively reflects back on former owners as well. Moreover, fund managers report that listed companies reduce overstaffing by internal transfers before carving out a business in order to prevent negative media attention, a practice that may continue into the holding period if the selling side remains minority shareholder.<sup>23</sup> Reportedly, selling parties may even add employee protection clauses to contractual obligations (see also *The Economist* 2010). Thus, in many cases inefficiencies related to employment have already been resolved prior to divestment or have been made contractually ‘untouchable’. Interviewees further reported that owners of private businesses with visibility

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<sup>21</sup> Most fund managers had previously provided employment data, and some of the access gained to labor unions, investee companies, and intermediaries was through fund managers and advisers. In each category, however, we secured sufficient evidence from interviews arranged on our own initiative to rule out the possibility of selection effects.

<sup>22</sup> During the pilot interviews both researchers actively engaged in the discussion. As a result of the pilot testing, however, the setting was changed to one researcher asking questions and leading the interview, while the second researcher focused on taking extensive handwritten notes, because the interviewees were uncomfortable with the interview being recorded due to the perceived sensitivity of the topic. Interview duration was approximately one hour.

<sup>23</sup> Sugiura (2019:155) reports that these cases are “typical” for divestment transactions in Japan.

on the local level would rather let their firm go bankrupt or not find a successor than “throwing one’s employees to the vultures”. In contrast to views commonly found in the literature, legal constraints were not considered a major reason for avoiding job cuts, an assessment shared by labour unions.

Taken together, reputation matters for all funds active in the Japanese market regardless of ownership category. Arguing that the selling side was essentially dominating the Japanese buyout market, fund respondents univocally noted that securing their reputation was the major motivation for mitigating major job cuts. This also explains the concessions on employment and staffing issues reported as equally applying to foreign funds (CNN 2002), and ultimately explains why H3 and H4 do not hold.

## **7. Concluding remarks**

Our research questions were: 1. What is the impact of PEBO on regular employment levels in Japan?  
2. Does fund ownership matter?

In a sample of firms with a median of 200 employees, we find that regular employment grows at an uncorrected average of 12.3% during a mean holding period of 4.31 years, and an annual growth differential of +1.7pp relative to a matched-pairs sample. Event studies show significantly higher growth rates during the holding period than under the previous ownership, which implies a causal relationship. While employment decreases in some deals, the lower limit is around –20%, evidencing that standard employment is not reduced beyond a certain threshold.

Our quantitative analysis further finds employment growth negatively related to the size of target firms, confirming findings from earlier research. We also find compound employment growth with a non-linear relationship to the holding period, where short (less than three years) and very long deals (six and more years) are outperformed by deals closer to the average of four to five years. Tentatively, we may explain the former by the time required until growth strategies take effect, and the latter by decreasing marginal effects of a change in ownership and strategy.

Contrary to our hypotheses, we find no significant differences in the effect on employment numbers neither between Anglo-Saxon and domestic funds, nor between dependent and independent domestic funds. Our series of interviews indicate that reputational concerns relating to the institution of employment security are at the heart of our findings. These concerns moderate the decisions of funds at two stages in the investment process: funds tend to avoid investment in businesses with major restructuring needs, and to prefer growth-centred over cost-cutting strategies. Interviewees report that lay-offs are minimized

because their negative consequences affect the reputation of sellers and acquirers alike. Regardless of their ownership structure, funds risk access to future deals if excessively laying off employees, because the Japanese buyout market is essentially a seller's market with funds competing for deals (not businesses competing for capital). Thus, reputational concerns outweigh differences in ownership and origin. Combined with the small mean size of investee companies (with relatively more growth opportunities), this may explain the significant positive growth differential observed during the holding period.

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## APPENDIX

TABLE A: PROPERTIES OF DATA PROCESSING IN SAMPLE

		<b>Mean</b>	<b>SD</b>	<b>SD</b> (months)	<b>Min</b>	<b>Max</b>
<i>Time adjustment</i>	Entry	-10.9	132	4.39	-358	323
( $t_{\text{employment data}} - t_{\text{transaction}}$ ; in days)	Exit	-25.9	116	3.87	-354	237
<i>Inter-/extrapolation</i>	Entry	0.83	7.54		-33.4	47.3
(given – adjusted staff numbers; in %)	Exit	-0.85	5.56		-61.8	15.3

NOTES: *Timing adjustment* documents our matching of the timing of entry and exit to that of our employment data. *Inter-/extrapolation* notes the resulting deviation of employment numbers relative to the closest data point.

INTERPRETATION: Mean time deviation is negligible at <30 days. Inter-/extrapolation results imply that our growth estimate for the entire holding period of 12.26% may include a conservative margin of some 1.68pp (0.83 – (0.85)).

TABLE B: POPULATION COVERAGE OF SAMPLE BY VINTAGE YEAR

	<b>1999</b>	<b>2000</b>	<b>2001</b>	<b>2002</b>	<b>2003</b>	<b>2004</b>	<b>2005</b>	<b>2006</b>
Sample & Matched Sample (n=184)	0.50	0.40	0.48	0.34	0.40	0.40	0.57	0.45
Subset ‘Teikoku’ (n=137)	0.13	0.27	0.19	0.31	0.33	0.30	0.38	0.32
Subset ‘C’ (n=58)	0.13	0.00	0.10	0.14	0.18	0.11	0.20	0.13
Sample coverage (cont’d)	<b>2007</b>	<b>2008</b>	<b>2009</b>	<b>2010</b>	<b>2011</b>	<b>2012</b>	<b>2013</b>	<b>Total</b>
Sample & Matched Sample	0.44	0.50	0.44	0.58	0.85	0.13	0.50	0.46
Subset ‘Teikoku’	0.33	0.38	0.32	0.54	0.85	0.13	0.40	0.34
Subset ‘C’	0.16	0.28	0.04	0.17	0.23	0.00	0.00	0.14

Notes:  
Matching score was 100% for entry (vintage) year.

TABLE C: DISTRIBUTION OF TRANSACTION VALUE AND HOLDING PERIOD

	<b>N</b>	<b>Mean(log)</b>	<b>SD(log)</b>	<b>Median</b>	<b>Min</b>	<b>Max</b>
<i>Transaction value</i> (bn yen)						
Population	356	1.59	0.68	3.86	0.10	415
Sample	166	1.62	0.66	4.53	0.12	352
Subset 'Teikoku'	119	1.66	0.68	4.58	0.12	352
Subset 'C'	52	1.65	0.70	4.45	0.12	352
<i>Holding period</i> (months)						
Population (HP > 12 months)	383	1.64	0.230	43.4	13.0	224.0
Sample	184	1.66	0.228	45.4	13.0	144.0
Subset 'Teikoku'	137	1.68	0.231	47.7	13.0	144.0
Subset 'C'	58	1.78	0.169	59.9	25	144.0

Notes: Median calculated as  $10^{\wedge}\text{mean}(\log)$ .

TABLE D: COMPOSITION OF SAMPLE AND POPULATION BY TYPE OF FUND AND TYPE OF TRANSACTION AND EXIT

<b>Dimension</b>	<b>Category</b>	<b>Population</b> (N=404)	<b>Sample</b> (n=184)	<b>Subset</b> <b>'Teikoku'</b> (n=137)	<b>Subset 'C'</b> (n=58)
<i>Fund type</i>	Dependent (J)	0.51	0.55	0.47	0.41
	Independent (J)	0.33	0.28	0.30	0.34
	Foreign	0.16	0.17	0.23	0.24
<i>Deal type</i>	Divestment	0.28	0.32	0.28	0.33
	Turnaround	0.23	0.15	0.15	0.17
	MBO	0.16	0.20	0.21	0.24
	PIPE TP*	0.13	0.11	0.12	0.14
	Business succession	0.11	0.14	0.13	0.05
	Secondary buyout	0.07	0.09	0.09	0.07
	NA	0.02	0.01	0.02	0.00

\*Private investments into public equity (PIPE) refer to buyout firms acquiring stock of a listed company. If the acquired company is subsequently delisted, the transaction is referred to as 'take private' (TP).

TABLE E: COMPOSITION OF POPULATION, SAMPLE AND AND MATCHED SAMPLE BY INDUSTRY (%)

<b>Industry</b>	<b>Population (N=404)</b>	<b>Sample (n=184)</b>	<b>Matched Sample (n=184)</b>	<b>Sample Subset 'Teikoku' (n=137)</b>	<b>Sample Subset 'C' (n=58)</b>
1 Bio/health care/medical	5.20	6.52	6.52	5.84	8.62
2 Construction	2.97	3.26	3.26	4.38	0.00
3 Consumer goods & other products	10.15	10.33	10.33	8.76	13.8
4 Electronics/machinery/ automobile	15.10	20.11	20.11	24.1	22.4
5 Finance/insurance	5.69	4.89	4.89	5.11	3.45
6 Food/agriculture	7.67	10.87	10.87	9.49	8.62
7 IT/telecoms/internet	7.18	5.98	5.98	5.84	3.45
8 Materials/chemicals/ metals/mining	6.19	5.43	5.43	5.84	5.17
9 Media/publishing/ contents	5.45	4.35	4.35	3.65	1.72
10 Restaurants	3.71	4.35	4.35	3.65	1.72
11 Retail/wholesale	10.64	7.61	7.61	7.30	8.62
12 Service	16.09	14.67	14.67	13.1	17.2
13 Transportation/ distribution	3.96	1.63	1.63	1.46	1.72

*Notes:*

Matching score for industries was 100%. Industry categories assigned by authors and matched to Toyo Keizai's Unlisted Company Edition (1998-2015) as follows: 1 to Pharmaceuticals; 2 to Construction; 3 to Building materials, furniture, textiles, clothing, other manufacturing industries; 4 to Other transportation devices, machinery, metal products, automobiles, precision machinery, precision devices, electrical devices, and transportation devices; 5 to Leasing, banking, commodity futures, securities, trust banks, credit sales and credit cards, life insurance, property and casualty insurance, other finance, money lending and credit cards, investment business, investment trusts, investment advisory, insurance; 6 to Foodstuffs, agriculture, forestry and fisheries; 7 to Systems and software development, information, systems and software, information services and other information; 8 to Glass, soil, rubber, leather, rubber products, pulp, paper, chemicals, mining, oil, coal, petroleum, coal, steel, electricity, gas, electric power and gas, nonferrous metals; 9 to Printing, video and music, advertising, publishing, newspapers, communications and broadcasting; 10 to Eating and Drinking places; 11 to Glass and soil wholesale, supermarkets, convenience stores, other wholesale, pharmaceuticals wholesale, chemical wholesale, machinery wholesale, car sales, retailing, food product wholesale, precision instruments wholesale, petroleum and fuel wholesale, specialty stores, textiles and clothing wholesale, paper wholesale, general wholesale, other retailing, steel and metals wholesale, electrical equipment wholesale, electric apparatus wholesale, department stores, transportation equipment wholesale; 12 to Outsourcing, consulting, hotels, leisure, entertainment, machinery and equipment maintenance, machinery and other repairs, education, architectural design, building management, building security, temporary staffing and contracting, other services, real estate, travel; 13 to Cargo transportation, shipping, aviation, warehousing and wharves, warehousing and logistics-related, rail and bus, land transportation.

TABLE F: POPULATION AND SAMPLE SUBSET COMPOSITION  
AND ESTIMATED BASELINE IMPACT

Dimension	Category	Population <sup>†</sup> (%)	Subset 'Teikoku' (n=137)		Subset 'C' (n=58)	
			Structure (%)	Impact (pp)	Structure (%)	Impact (pp)
<i>Holding period</i> (years)	2	17.49	13.14	-0.26	1.72	-0.95
	3	21.93	22.63	0.05	17.24	-0.32
	4	18.02	17.52	-0.04	25.86	0.64
	5	13.58	14.60	0.10	17.24	0.34
	6	7.57	7.30	-0.02	12.07	0.34
	7p	15.67	18.98	0.19	25.86	0.57
<i>Industry</i>	Finance/ insurance	5.69	5.11	-0.04	3.45	-0.17
<i>Vintage</i>	2004	13.12	11.68	-0.10	10.34	-0.19
	2011	3.22	8.03	0.42	5.17	0.17
<i>Deal size</i> (mean(log(value))) <sup>††</sup>		1.59	1.62	-0.05	1.65	-0.10
<b>Sum of estimated impact</b>				<b>0.23</b>	<b>0.34</b>	

Notes:

Subset 'Teikoku' as used in Table F, subset 'C' as used in Table G.

Impact estimated as product of structural differentials and parameter estimates of Table H.

<sup>†</sup> n = 383 for holding period >12 months, n = 404 for industry and vintage, n = 355 for deal size

<sup>††</sup> n<sub>Teikoku</sub> = 119, n<sub>C</sub> = 52

TABLE G: ESTIMATION OUTPUT FOR COMPOUND ANNUAL EMPLOYMENT GROWTH (PP)

Coefficients (SE)	<b>Full sample (n = 184)</b>		
	MPD	IYA	AVG
<i>Fund ownership</i>			
- Foreign	0.104 (2.182)	-0.959 (2.100)	-0.421 (2.052)
- Independent	1.460 (1.747)	0.266 (1.682)	0.876 (1.643)
<i>Controls</i>			
Pre-entry growth	<b>13.480</b> (5.771)	7.212 (5.356)	<b>10.545</b> (5.578)
<i>Holding period</i>			
- 3 years	3.544 (2.228)	2.806 (2.151)	3.147 (2.098)
- 4years	<b>4.770</b> (2.376)	<b>4.213</b> (2.304)	<b>4.422</b> (2.242)
- 5 years	3.591 (2.684)	<b>5.039</b> (2.599)	<b>4.267</b> (2.534)
- 6 years	4.161 (3.020)	3.391 (2.916)	3.718 (2.844)
- 7+ years	0.785 (2.637)	1.732 (2.531)	1.188 (2.475)
<i>Deal type</i>			
- Bus. succession	0.277 (2.558)	0.971 (2.463)	0.614 (2.406)
- MBO	3.147 (2.319)	1.965 (2.241)	2.530 (2.187)
- Divestment	<b>3.911</b> (2.225)	2.716 (2.130)	3.305 (2.091)
- Secondary	4.276 (3.169)	3.798 (3.044)	4.068 (2.978)
Log(size)	<b>-1.365</b> (0.546)	<b>-1.541</b> (0.567)	<b>-1.456</b> (0.554)
Financials	<b>7.044</b> (3.361)	<b>6.381</b> (3.232)	<b>6.712</b> (3.161)
<i>Year-fixed effects</i>	Yes	Yes	Yes
<i>Flags</i>			
Fund Data	2.671 (1.836)	2.720 (1.751)	2.683 (1.719)

*Notes:*

F-statistics, variance inflation factors, and residual analytics with qualitatively equal outcomes as in Table 3. Bold font indicates significance of 90% or better.

Abbreviations and definitions as in Table 3; deal types contrast to base category “turnaround”.

TABLE H: SIZE DISTRIBUTION OF SAMPLE AND MATCHED SAMPLE (FREQUENCIES)

<b>Number of regular Employees</b>	<b>Sample (n=184)</b>	<b>Matched Sample (n=184)</b>
11-29	0.071	0.071
30-99	0.212	0.207
100-299	0.326	0.332
300-999	0.299	0.299
1000+	0.092	0.092

Notes:

Mean difference (Matched Sample – Sample) is 0.070, SD of that difference is 0.497.

TABLE I: PERFORMANCE DISTRIBUTION IN SUB-SAMPLE AND CORRESPONDING RANDOM MATCH

<b>Sample dimensions</b>	<b>Sub-sample Return on sales (%; A)</b>	<b>Random match Expected RoS (%; B)</b>	<b>Difference (pp; A–B)</b>	<b>n</b>
<i>Firm size I (staff)</i>				
- SME (up to 300)	1.66	1.29	0.37	79
- Large (more than 300)	2.14	2.32	-0.18	53
<i>Firm size II (Paid-in capital; JPY)</i>				
- Less than 500 million	1.15	1.12	0.03	17
- 500 million to less than 10 billion	0.99	2.12	-1.13	37
- More than 10 billion	3.86	3.75	0.11	7
<i>Entry period</i>				
- 1999 through 2004	1.62	1.33	0.29	53
- 2005 through 2007	1.65	2.35	-0.70	44
- 2008 through 2013	2.47	1.47	1.00	35
<i>Grand total</i>	1.85	1.71	0.14	132

Notes:

We define performance as after-tax return on sales. Data is available from Teikoku Databank for entry years of 144 businesses in our sample. We exclude 12 cases outside the interval [–.50; +.50] to account for outliers (e.g., extraordinary gains and losses such as write-offs or earlier unrealized gains), which reduces effective *n* for this comparison to 132.

For the expected RoS of a random match we take reference values from METI's *Chūshōkigyō no kei'ei shihyō* for 1999 through 2002 and from the subsequent *Chūshō kigyō jittai kihon chōsa* for 2003 through 2013. For the 53 businesses with more than 300 employees we use their paid-in capital to reference values from METI's *Kigyō katsudō kihon chōsa (1999-2013)*. We also use the latter for referencing businesses with less than 300 employees for 3 cases of negative performance and 6 cases of industry categories not included in the 1999-2003 *kei'ei shihyō*, the latter including one case of the former. *Firm size II* only includes the cases referenced by paid-in capital.

The 6 cases subjected to *Chūshōkigyō no kei'ei shihyō* reference our industries 4, 6, 8 (as of App. Table E) as “Manufacturing”, and 11 as “Retail”. For the 65 cases referenced by *Chūshō kigyō jittai kihon chōsa* we attribute our 1 to “Manufacturing” (4 cases), and “Services (3 cases); 2 to “Construction”; 3, 4, 6, 8 to “Manufacturing”; 7 and 9 to “Information & Telecoms”; 10 to “Restaurants & Lodging”; 11 to “Retail”, and “Wholesale”, respectively; 12 to “Services”; and 13 to “Transportation”. Finally, in the *Kigyō katsudō kihon chōsa* we reference 1, 5, 7, 9, 12, 13 to “Services”; 2 through 4, 6 and 8 to “Manufacturing”; 10 to “General eating services”, and 11 to “Retail”.