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When Vultures Bring Blessings:

Employment Growth in Japanese Businesses under Private Equity Ownership

Georg D. Blind* and Stefani Lottanti von Mandach*

Abstract

We track the effect of private equity ownership on employment in a methodological setup uniting descriptive analysis with checks for selection effects and causality. Applying this approach to buyout investments in Japan, we find evidence that employment grows significantly under private equity ownership, a finding that is in stark contrast with earlier studies on Anglo-Saxon economies; that the increase is not attributable to selection effects; and that growth rates during the holding period are significantly higher than under the previous ownership. We seek an explanation in the institutional setup and explore the relevance of reputation in transactions with revealed identities.

Keywords: employment adjustments; private equity; buyout investments; Japan

JEL codes: J23; J63; G34; M51

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Essentially, buyout funds are trading in second-hand businesses where gains entirely depend on the difference between the buying and the selling price. With valuation of businesses predominantly resulting as multiples of gross profits, any measures increasing revenues or cutting costs will positively impact on a fund's margin. For simple accounting reasons, cost-cutting measures come with a much stronger lever as any cost reduction directly contributes to bottom-line profits. In contrast, an increase in revenues only contributes to profits via the gross margin. For instance, in a business with high variable cost – say 80% of sales price, a mere 10% reduction of the cost base increases profits about as much as a 50% increase in sales. Moreover, increasing sales typically requires more time than cost reductions. Accordingly, we may expect buyout funds to concentrate on cost reductions whenever possible.

Eventually, the buyout industry has come in for severe criticism in the media for excesses in reducing labor costs in the course of the last decade. Buyout investors, suspected of increasing profitability at the expense of employees and employment, have been cynically labeled “locusts”, “vultures” or “gluttons” (Lutz and Achleitner 2009; Froud and Williams 2007). While industry associations never tire of emphasizing the buyout industry's social gains in terms of employment preservation and creation, most of the independent scholarly research with significant findings on the effects on employment of buyout investments documents substantial average losses, broadly in line with the almost universally negative public perception of these activities.

For Japan it is particularly difficult to predict the net outcome on employment. In a country with a strong tradition of long-term commitments to life-time employment, we might expect significantly smaller employment losses than in other economies, as such institutional environment may limit the grip of investors on one of the major levers in cutting cost. On the other hand, the relative absence of economic growth in a country characterized by economic malaise for the better part of the last two decades may also restrict opportunities to increase firm value via growth strategies.

Building on a unique proprietary dataset covering the full population of fund-led buyout transactions (n=794) from the nascence of the Japanese buyout industry in 1998 to September 2015, our research finds the first ever evidence of the impact of buyout

investments on employment levels in Japan. In addition to these descriptive accounts, our methodological setup rules out selection effects. Our analysis also checks on causality: do fund managers cause a significant differential in net employment growth? Finally, we address the question of whether the employment changes observed are any different from general labor market trends and industry-specific developments. This question has rarely been addressed in previous studies, mainly due to the adoption of a different methodological approach.

Our analysis yields the following main results. First, we find that Japanese businesses subject to buyout transactions by private equity funds exhibit average organic growth in standard employment of 12.3% during a median holding period of 3.92 years and a mean holding period of 4.31 years. Second, when corrected for industry effects and labor market trends this figure shifts to 13.4%. Third, fund managers investing in Japanese private businesses do not merely buy into existing trends, but create a significant and positive employment growth differential pre- and post-buyout. And fourth, building on one and two, it becomes obvious that buyout fund managers in Japan invest predominantly in industries with negative employment growth.

For triangulating these rather surprising results, which indicate no shortage of growth opportunities (contrary to common perceptions of the general state of the Japanese economy), we conducted a series of interviews with domestic and foreign buyout investors. The interviews revealed that fund managers in Japan tend to focus on growth strategies and to refrain from affecting the “Holy Grail” of employment as a rational response to this particular social norm. Beyond this interpretation of the intercept found in a multivariate regression of compound annual growth rates of employment, we find length of holding period, familiarity of the fund manager with the industry of the investee company, as well as size and timing of the investment to exert significant influence on the slope of employment growth.

The paper is structured as follows. Section I reviews related research. Section II describes the dataset. Section III presents descriptive results. Section IV explores an explanatory model of employment changes and presents a number of robustness checks. Section V discusses findings and proposes potential explanations. Section VI concludes.

I. Related Literature¹: Findings and Issues

The studies reviewed here have all documented significant changes in employment under private equity ownership when compared to pre-buyout employment levels and/or control groups.² We review them in relation to our four research questions: (1) What is the impact of buyout investments on employment? (2) Has the study identified or ruled out selection effects? (3) Does the study tackle the question of causality? (4) How does the sample compare to the overall population?

Liebeskind, Wiersema, and Hansen (1992) analyze 33 buyouts that occurred in the largest 1,500 US corporations between 1980 and 1984. Post buyout, the median number of employees declined in both buyout targets and matched control firms, but significantly more in the former. The authors control for pre-buyout differences, which rules out selection effects. They further find that buyout managers downsized corporate operations significantly more than in their set of publicly listed control firms, evidencing a causal involvement of funds. They do not, however, address the question of how the buyout sample compares to industry-specific labor market trends.

Harris, Siegel, and Wright (2005) report massive job losses after analyzing a large 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 levels, they find a staggering 61% (weighted) job loss at manufacturing plants. The authors report that funds are investing in plants with below average productivity pointing to a selection effect. However, as in the study of Liebeskind, Wiersema, and Hansen (1992), the question of how the buyout sample compares to the overall economy in terms of employment changes is not addressed.

Boucly, Sraer, and Thesmar (2011) examine an almost equally large sample of 839

¹ Our review excludes studies sponsored by industry associations and labor unions (due to the possibility of vested interests), and research not verified in peer-review processes (for quality concerns).

² The following studies report non-significant employment effects of buyout investments: Kaplan (1989), examining 48 buyouts of public companies; Smith (1990), considering 58 buyouts of public companies in the US completed between 1977 and 1986; Bruining et al. (2005), conducting questionnaire-based surveys in the UK (145 buyouts between 1994 and 1997) and the Netherlands (45 buyouts between 1992 and 1998); Amess and Wright (2007), analyzing 1,350 buyouts in the UK executed between 1994 and 1998; Amess and Wright (2012), researching 544 buyouts in the UK between 1993 and 2004; and Amess, Girma, and Wright (2014), studying 232 buyouts in the UK between 1996 and 2006.

buyouts in France completed between 1994 and 2004. They find that between the four years preceding the transaction and the four subsequent years, employment growth at buyout targets is significantly higher than at their control firms (cumulative 18%), and in the four years after buyout employment grows by approximately 15%. The magnitude of cumulative employment growth is comparable to our findings and there is indicative evidence of some qualitative similarities. While the authors do not comment on it explicitly, their Figure 3 suggests that there may be a selection effect, as fund managers seem to prefer investing in businesses with larger employment growth than matched controls. Post-buyout employment growth remains significantly higher even after controlling for pre-buyout differences, indicating that fund managers do make a difference. Their study does not, however, include a comparison between employment developments in their sample and industry-specific employment trends in French businesses.

Cressy, Munari, and Malipiero (2011) analyze 57 buyouts in the UK over the period 1995–2000. Employment at investee companies falls by 7% relative to controls (83 firms) as early as the first year post buyout, peaking at a 23% per annum loss in the fourth year. Finding a significant difference post buyout compared to their matched companies, but no such difference in the buyout year itself, the authors argue that there was no selection effect and that fund managers did have a significant impact on employment levels at sample firms. A comparison of employment changes in the sample and the overall labor market is missing.

Goergen, O’Sullivan, and Wood (2011) study 73 buyouts of UK public companies completed in the years 2000–2006 and find that employment in acquired firms reduces significantly in the year immediately after the completion of the transaction compared to the matched sample of non-acquired firms. Selection effects can be ruled out because they find no significant differences in medians between sample companies and control companies pre buyout, and fund managers’ impact on employment levels post buyout seems significant. A comparison of employment changes in the sample and industry-specific labor market trends is also missing.

Davis et al. (2014) track employment at 3,200 firms and their 150,000 establishments in the US 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. Gross job loss at target establishments outpaces the losses at controls by a cumulative 10 percentage points over five years post buyout. At firm level, target firms create new jobs in newly opened establishments at a faster pace than control firms. However, accounting for the purchase and sale of establishments as well, the target–control 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 and abolition. They rule out selection effects by controlling for pre-buyout growth history and find post-buyout employment growth rates at target firms significantly different from controls. As in other studies relying on a matched-pairs approach, it remains unclear how investee companies compare to general labor market trends.

Finally, Bernstein et al. (2016) analyzed 11,735 country-industry-year observations between 1986 and 2009 in a global study. Employment in buyout industries (i.e., industries where private equity funds invest) grows significantly faster than in non-buyout industries. This result seems at least partly driven by spillover effects from buyout target firms to other firms in the industry. Bernstein et al. rule out a selection effect because buyout funds do not seem to select industries that are growing faster. They include country-industry and industry-year fixed effects to measure the impact of buyout investments relative to the average performance in a given country, industry, and year, but equally do not indicate how their sample compares to the overall economy.

Summing up this review of prior research, cost efficiency measures – including reductions in headcount – seem to balance or even outweigh growth strategies in the vast majority of cases. Beyond these material findings, the studies reviewed identify a number of methodological challenges in estimating the impact of buyout investments on employment (see for example Davis et al. 2014). Firstly, reliability of studies is at risk through measurement errors, namely through (a) an unwarranted inclusion of cases with acquisitions or divestments of business units during the holding period, or through (b) definitional issues such as using gross headcount instead of full-time equivalent, which leaves changes in employment practices unaccounted for (for example, increasing share of part-time work). Secondly, there is substantial risk of biased samples, namely through (a)

the dropping of buyouts that involve the sale or acquisition of business units (in an attempt at avoiding measurements errors), and (b) when samples are drawn from unknown populations that cannot be checked for representativeness.

II. Population and Sample

Our proprietary dataset, assembled and cross-evaluated using government reports, fund websites, press searches and data provided by an independent Japanese advisor, includes the population of 794 buyout transactions conducted by all private equity funds active in Japan since 1998. To obtain the subset relevant to our assessment of the employment effects of fund ownership on investee companies, 234 deals not confirmed as concluded by September 2015 are excluded. From the remaining 560 deals, 111 minority deals are excluded (89 investments in listed companies and 22 minority investments in private businesses). This is because changes in employment cannot be fully attributed to fund activity in these cases due to insufficient control rights. A further 45 transactions are excluded where changes in employment do not offer substantial leverage (31 real estate-driven deals), are biased by investment type (7 late-stage venture capital transactions), or pertain to investee companies located outside Japan (7). This leaves us with a relevant population of $n=404$.

Employment figures for 224 investee companies were obtained from Teikoku Databank. Funds themselves were also asked to provide employment figures on their investee companies. In order to avoid “cherry picking”, we made full data disclosure (i.e., for the entire completed part of their portfolios) 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 $n=256$.

For addressing the risks to reliability discussed in section I, we checked all transactions in the raw sample for acquisitions and divestments of business units during the holding period and 17 corresponding cases (or 6.64% of the raw sample) were

identified.³ In light of this relatively low figure, we decided to trade in a good deal of reliability for a small – if any – loss in validity by eliminating these distorted cases. As full time equivalents were not available we opted for headcount of regular full-time employees. Given the strong general trend in replacing regular employees by non-standard employment observed for Japan since the 1990s (Blind and Lottanti 2015), headcount of regular employees can be considered a strongly conservative measure of total employment.

To match employment data to the entry and exit dates, a maximum deviation of 365 days was allowed. As the effective date for most employment adjustments in Japan is April 1, cases with post-entry data beyond March 31 were not included reducing the raw sample to 239 cases (204 firms covered by Teikoku Databank, 66 by fund manager data, and 31 by both sources).

The next step required 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.⁴ Employment levels at entry and exit were approximated by linear interpolation and extrapolation respectively. Appended TABLE A documents properties of data matching and corresponding inter/extrapolation. 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; 7 cases with less than 10 regular employees at time of investment; and 13 cases with holding periods of up to one year.⁵ The final sample therefore contains n=184 cases (or 45.5% of the population of completed transactions).

As our dataset covers the full population, we are able to assess the sample and its subsets for representativeness. Eventually, we found the sample well-balanced with regard to an

³ This small share is due to the fact that roll-ups are very rare in the Japanese buyout market.

⁴ This test, however, does not preclude the possibility of self-selection among participating funds (see discussion of robustness in section 5).

⁵ See also sections IV and V. Deals with holding periods < 1 year known as “flip-deals”. Buyout funds act as business brokers aiming to identify a strategic buyer for the acquired business.

important number of dimensions such as its distribution over time (appended TABLE B), the size of investee companies and duration of holding period (appended TABLE C), as well as sample composition by deal type, fund category, and industries (appended TABLE D and E).

III. Descriptive Results

We use an industry-adjustment approach as used by Kaplan (1989), Smith (1990) or Sousa and Jenkinson (2013) in the buyout industry context, and by Allen and Philipps (2000) or Hsu et al. (2015) in other empirical studies. We also analyze whether the observed employment changes are any different from general labor market trends and industry-specific developments, because we believe that such information is critical for policy making. This question has rarely been addressed in previous studies mainly due to the adoption of a different methodological approach (matched-pairs analysis) and it remains largely unclear whether fund managers select businesses with above- or below-average employment growth as compared to the overall economy.⁶ Addressing these latter questions, we equally succeed in providing evidence on the same questions as addressed by matched-pairs studies, including employment levels, selection effects and causality.

A simple comparison of standard (regular) employment at entry and exit of PE investors provides cues to our first question on employment changes during fund ownership. There was an average increase of 12.3% in standard employment during the holding period with a one-sided confidence threshold of 7.99% at the 90% level ($CI_{90\%} = [6.78; 17.75]$). Using only the data from Teikoku Databank ($n=137$) reduces these figures to 10.44% with a 90% threshold of 5.61%. This shows that the PE industry is making a positive contribution to employment growth in Japan, which is in stark contrast to most findings from other countries. Owing to the dispersion of holding periods, however, the measure of total growth during holding period is subject to quite substantial variance, which unnecessarily

⁶ The matched-pairs analysis is more appropriate in studies directed towards understanding mechanisms of change in firms (see for example O'Farrell and Hitchens 1988, and Liebeskind, Wiersema, and Hansen 1993).

complicates further statistical treatment.

Therefore, we use compound annual growth rates as our test statistic going forward. TABLE 1 shows the test statistic for the entire sample as well as for subsets ‘TDB’ (data obtained from Teikoku Databank) and ‘Fund’ (data obtained from fund managers). Businesses under fund ownership show a positive trend in regular employment with significant estimates confirmed at the 90% level in one-sided t -tests for the full sample as well as for both subsets. Mean growth rates are substantially higher for subset ‘Fund’, which may indicate a self-selection bias (likelihood of fund participation in our survey potentially conditional upon their track record of employment creation). We will revert to this issue in the robustness part of the discussion (see section IV).

TABLE 1: COMPOUND ANNUAL GROWTH RATES (CAGR) OF REGULAR EMPLOYMENT UNDER FUND OWNERSHIP (%)

| | Mean | Confidence (90%) | | | Min | Max |
|------------------------|------------------|------------------|-------|-----------|--------|-------|
| | | lower | upper | one-sided | | |
| Full sample (n=184) | 1.63 (0.0065) | 0.56 | 2.70 | 0.79 | -22.13 | 36.80 |
| Subset TDB (n=137) | 1.12 (0.0072) | -0.06 | 2.29 | 0.20 | -22.13 | 28.36 |
| Subset Fund (n=47) | 3.11 (0.0144) | 0.74 | 5.49 | 1.26 | -16.98 | 36.80 |

The second question – whether these changes are any different from general labor market trends and industry-specific developments – is addressed by adjusting the data for corresponding changes documented in the Ministry of Health, Labor and Welfare’s *Labor Force Survey (Rôdôryoku Chôsa)* (Statistics Bureau of Japan 1998–2015). This adjustment is achieved by expanding the employment figures by the inverse of the relative change in industry-specific absolute employment figures during the holding period. The *Labor Force Survey* provides monthly data from 2002, enabling almost perfect matching of entry and exit dates. Before 2002, where only annual data is available, weighted averages are used.

Re-computing the test statistic using the adjusted employment figures produces qualitatively similar distributional properties with increased significance (see TABLE 2). One-sided tests are now significant at the 95% level for the full sample as well as for both subsets. Employment creation during fund ownership is therefore not attributable to general labor market trends and industry-specific developments.

TABLE 2: COMPOUND ANNUAL GROWTH RATES (CAGR) OF REGULAR EMPLOYMENT UNDER FUND OWNERSHIP CORRECTED FOR INDUSTRY EFFECTS AND LABOR MARKET TRENDS (%)

| | Mean | Confidence (95%) | | | Min | Max |
|-------------------------|------------------|------------------|-------|-----------|--------|-------|
| | | lower | upper | one-sided | | |
| Full sample (n=184) | 1.77 (0.0066) | 0.48 | 3.07 | 0.69 | -21.50 | 36.54 |
| Subset 'TDB' (n=137) | 1.47 (0.0074) | 0.02 | 2.92 | 0.26 | -21.50 | 25.49 |
| Subset 'Fund' (n=47) | 2.65 (0.0144) | -0.16 | 5.47 | 0.29 | -16.12 | 36.54 |

Notes: Subset 'TDB' includes cases exclusively involving data from Teikoku Databank; subset 'Fund' includes all cases involving data obtained from funds.

A comparison of means of the full samples in TABLE 1 and TABLE 2 shows that fund managers in our sample have invested in an industry mix of businesses with negative average employment growth. When applied to the entire holding period, the relative difference between employment figures before and after these adjustments becomes significantly different from zero (mean = 1.82pp, $CI_{90\%} = [0.63; 3.01]$). Thus, investments in industries with negative average employment growth can safely be claimed to extend to the population.

While these findings suggest that industry growth trends do not guide the general investment strategy of fund management, it is still possible that relative growth potential is a selection criterion within industries. To answer our third question – whether funds are buying or creating employment growth – requires a pre-/post-entry comparison of growth rates for the individual companies in the sample (see TABLE 3).

TABLE 3: DIFFERENTIALS OF PRE-/POST-ENTRY EMPLOYMENT GROWTH AFTER
ADJUSTMENTS (PP)

| Reference period (subset) | n (€ 'Funds') | Mean | Confidence (90%) | | | Min | Max |
|--|------------------|-------------------|------------------|-------|-----------|--------|------|
| | | | lower | upper | one-sided | | |
| HP ₁ – HP ₁ (A) | 81 (7) | –0.64 (0.0262) | –4.95 | 3.68 | –3.99 | –112.1 | 50.9 |
| HP ₂ – HP ₁ (B) | 65 (7) | 5.66 (0.0291) | 0.87 | 10.44 | 1.93 | –74.7 | 78.7 |
| HP ₂ – HP ₁ (C) | 58 (0) | 4.68 (0.0312) | –0.45 | 9.81 | 0.68 | –74.7 | 78.7 |

Notes: Composition of subsets A and B result from data availability; subset C corresponds to subset B less the cases involving data obtained from fund management.

Results indicate no significant change in employment growth during the first year of ownership, for which we cannot confirm a positive growth differential. This supports our earlier decision to exclude transactions with holding periods of up to one year from the sample. Results become more clear-cut when the second year of fund ownership is compared to the 12 months preceding entry. Here, a two-sided test is significant at the 90% level. Even after excluding the seven cases involving data from the Funds subset, we still find a one-sided test significant at the 90% level (at 0.68pp).

IV. Understanding Employment Growth

Given the median holding period of 3.92 years, it is unsurprising that earlier growth (12 months prior to investment) is only weakly correlated (0.162) to compound annual employment growth under fund management. Correspondingly, a univariate regression with 106 degrees of freedom yields an R^2 of only 0.026 with a parameter estimate of 0.067 significant at the 90% level. With “acquired growth” only a minor influence, the data available from our set of micro data is used to further explore potential influencing factors.

As has become obvious from our comparison of pre-/post-entry employment growth, strategies aimed at boosting sales require time to play out in terms of employment. Adding to this initial time lag, standard economic theory also assumes that marginal effects are decreasing. We capture this compound non-linear relationship in a categorical

variable that regroups deals with a rounded measure of years under fund management (base 1, 2, ..., 6 and 7+ years).

Although the dependent variable is corrected for general industry effects, the ability of fund managers to select growth potential may vary by industry. As the vast majority of fund managers hail from the financial sector, they may be at a relative advantage when it comes to making judgments about businesses in the financial sector. Accordingly, a 'Finance and Insurance' dummy is included.

Further controls are added for size (growth opportunities expected to be marginally decreasing), year fixed effects (owing to the time series nature of the dataset), "turnaround" deals (with more substantial job losses expected) and *keiretsu*-type (base) funds versus non-*keiretsu* and foreign (with employment protection assumed to be strongest within *keiretsu* firms and of the least importance to foreign funds). Importantly, deals involving data obtained from fund management are also controlled for, in order to test for potential selection bias. TABLE 4 documents estimation output.

TABLE 4: ESTIMATION OUTPUT FOR EMPLOYMENT GROWTH CORRECTED
FOR GENERAL INDUSTRY AND LABOR MARKET TRENDS

| Variable | Estimate | t |
|----------------------------|------------|--------|
| (Intercept) | -0.0082 | -0.191 |
| HPr2 | 0.0600* | 1.846 |
| HPr3 | 0.0686** | 2.348 |
| HPr4 | 0.0817*** | 2.713 |
| HPr5 | 0.0939*** | 2.887 |
| HPr6 | 0.0747** | 2.151 |
| HPr7plus | 0.0562* | 1.767 |
| Sector finance | 0.0757** | 2.388 |
| log(Staff _{t=0}) | -0.0162*** | -2.937 |
| FE2001 | -0.0050 | -0.129 |
| FE2002 | 0.0310 | 0.807 |
| FE2003 | 0.0416 | 1.254 |
| FE2004 | 0.0681** | 2.125 |
| FE2005 | -0.0020 | -0.059 |
| FE2006 | 0.0354 | 1.091 |
| FE2007 | 0.0528 | 1.593 |
| FE2008 | 0.0482 | 1.389 |
| FE2009 | 0.0255 | 0.680 |
| FE2010 | 0.0461 | 1.283 |
| FE2011 | 0.0867** | 2.150 |
| Turnaround | -0.0242 | -1.255 |
| Foreign fund | -0.0060 | -0.295 |
| Non-keiretsu | -0.0021 | -0.131 |
| Fund data | 0.0293* | 1.729 |

Notes: Multiple R²: 0.1906, Adjusted R²: 0.0720, F-statistic: 1.607 on 23 and 157 DF, *p*-value: 0.0480

The overall model is significant at the 95% level. In spite of several categorical variables transformed into dummies, individual variance inflation factors are all well below 4 (the most conservative of the frequently suggested thresholds of 10, 5 and 4). Residual analytics (residuals vs. fitted, residuals vs. leverage, normal q-q, scale location and heteroscedasticity) did not indicate any major concerns.⁷

The dummy-coded categorical variable capturing the non-linear relationship between length of holding period and employment growth produced significant estimates

⁷ Three cases of potential outliers were re-examined, but no measurement errors were found.

for the entire spectrum (HPr2 to HPr7plus). More importantly, parameter estimates reproduce the hypothesized inverse U-shaped relationship: short-term fund involvement causes less employment growth than mid-term investments, and long-term holding periods show a depletion of improvement opportunities. Equally, familiarity of fund managers with their own industry helps to tap a significant positive growth differential of 7.57pp for investee companies from the financial sector. Size equally proved to be relevant control, with an estimated negative differential of 1.62pp for every tenfold increase in size of investee companies. As regards year fixed effects⁸, years 2004 and 2011 produce significant parameter estimates at the 95% level and years 2007 and 2008 are not very far off a significant estimate. These findings suggest that most deals concluded in 2004 and 2011 benefited disproportionately from phases of general economic recovery (until 2008, chiefly under Prime Minister Koizumi, and until 2015 under Prime Minister Abe respectively). In turn, financing committed for years ahead may have helped a substantial number of deals concluded in 2007 and 2008 to fare significantly above the industry average considering the challenges faced in the post-Lehman years. Among the remaining controls, turnaround and fund types loaded with the expected signs, but did not produce significant estimates. In contrast, cases involving data provided by fund management were obviously subject to a (self-)selection bias with a positive premium of 2.93pp significant at the 90% level.

Robustness of Descriptive Results

In view of the selection bias identified above, our evidence needs to be reconfirmed based on sample subsets not involving data provided by funds. Accordingly, we assess the composition of subsets TDB and C as used for evidencing our second and third research questions (Tables 2 and 3) against the population. We do so by using the variables identified as significant in our explanatory model (Table 4) for estimating the baseline impact on subset means. Appended Table F provides the corresponding estimates. As it turns out, the estimated impact on sample means does not challenge our initial finding of

⁸ Pre-2001 and post-2011 cases were relegated to the base category to keep variance inflation at a reasonable level (each with <7 cases; see appended Table A).

positive employment growth in businesses under fund management.⁹ Neither does it challenge findings on our second question – whether the growth identified is any different from general labor market trends.¹⁰ Equally, subset C providing evidence on our third question – whether funds are buying or creating employment growth – still holds.

While these checks thus raise no technical concerns with our findings, the most substantial corroboration of robustness is the strongly conservative measure of employment used. In fact, between 1998 and 2015, the number of non-regular employees in Japan increased from 11,760,000 to 19,790,000 (headcount; +61%). During this period, on average 72% of the non-regular employees were part-time employees and temporary workers (*arubaito*), 16% were contract employees and 5% worked for temporary labor agencies. Contract and agency workers usually work full time with a limited time contract, while part-time workers and temporary workers (*arubaito*) work an average of 26.5 hours per week, 66% of a full-time equivalent (FTE) (Ministry of Health, Labor, and Welfare 2011). Thus, in 1998, non-regular workers amounted to 8,585,800 FTE and in 2015 to 14,446,700. This results in a CAGR of 3.35% of non-regular employees in addition to our finding of a mean of 1.77 CAGR for regular employment under fund ownership (see Table 2), making total job creation under private equity ownership well above 4% p.a.¹¹

V. Discussion

Our analysis provides substantial evidence on all four research questions (organic employment growth, selection effects, causality, and comparison to the overall economy). With regard to the first question, we identify a number of variables linked to employment growth in businesses under fund ownership: holding period (in a non-linear fashion); familiarity of the fund manager with the industry of the investee company; and size and timing of investments (vintage years 2004 and 2011).

⁹ The one-sided 90% threshold for total employment growth during the holding period figures a magnitude higher at 7.99pp.

¹⁰ While the 95% threshold for subset TDB given in Table 7 reduces to a mere 3 basis points, an expansion of the measure to the entire holding period brings the 95% threshold to about 3pp.

¹¹ This does not even take into account that growth of non-regular jobs is likely higher in businesses with above-average growth of regular employment, as in the population in this study.

The most important difference between the present results for Japan and earlier research on other economies is the finding of significant *positive* effects on employment growth. The one and only research reporting employment growth of similar magnitude is the study by Boucly, Sraer, and Thesmar (2011) analyzing buyout investments in France. They report that employment grows by a cumulative 18% between the four years preceding the transaction and the four subsequent years, and by approximately 15% in the four years after buyout. Post-buyout employment growth is, however, concentrated among private-to-private transactions, where sellers typically are individuals or the founding families, as opposed to divisional buyouts, secondary buyouts, and public-to-private buyouts. The authors take this as first evidence that private equity funds alleviate credit constraints for medium-sized private companies, allowing investee companies to tap into pre-existing growth opportunities.

To test their hypothesis on our data, we re-run our analysis introducing dummies equivalent to the panels in Boucly, Sraer, and Thesmar (2011). Here we find indications of a similar hierarchical structure, in which “take-private”-type transactions show the lowest gains in employment growth followed by divestments. The most substantial gains are made in private-to-private transactions, including secondary buyouts. Alas, parameter estimates for the corresponding dummy variables are not significant. Thus, Boucly, Sraer, and Thesmar’s (2011) controls may also be relevant for an interpretation of our results. They cannot, however, explain the order-of-magnitude difference to findings from Anglo-Saxon economies.

To further put our findings into perspective, we next consider differences in the institutional environment, particularly the regulatory environment. In the literature, Japanese labor law, as interpreted by the courts, has been regarded not only as employee-friendly, but as effectively hindering the cutting of regular employment in situations other than near-bankruptcy (Araki 2005; Witt 2014). Thus, as a first conjecture, compliance with the regulatory framework may significantly reduce the negative part of the distribution of employment change.

A second conjecture builds on Guiso, Sapienza, and Zingales’ (2006) assumption that social norms are closely linked to preferences, which may lead to biased market decisions. There is an abundance of literature about variations in preferences across

different institutional environments in general, and Japan-specific preferences in particular (see for example Katzner 2008). For instance, Japanese corporate culture has been likened to the ideal of a “company family” (Bhappu 2000). Companies are expected to act “benevolently” towards their employees and to secure their employment, while for their part employees commit themselves to the company (Hofstede and Bond 1988; Hill 1995; Glisby and Holden 2003). Thus, given the strong foothold of this ideal in Japanese society, selling parties might be reluctant to “throw their employees to the vultures” and may eventually not sell their business to the highest bidder, but to the buyer most likely to conform to the social norm of valuing employment. Our second conjecture, thus, understands fund managers as reacting to such preferences when adopting growth strategies in an attempt at reputation building.

Third, growth strategies might be relatively more profitable in Japan than elsewhere owing to differences in restructuring cost given the risks and costs associated with layoffs, which include a negative impact on staff morale and reputational damage to all parties involved.

To assess the extent to which these conjectures explain the results of our study (13.4% organic employment growth after correcting for industry effects and labor market trends), we conducted a series of 30 interviews with fund managers active in Japan in July 2014 and January 2015. First, legal constraints are not a major reason for avoiding job cuts. The majority of fund managers indicated that legislation and its application is less strict than usually portrayed (contradicting conjecture 1). Second, to lay off regular employees in order to maximize profits is not socially acceptable, whereas the opposite holds when it happens to save an ailing business. This finding is consistent with conjecture 2. Third, concerns about their reputation cause fund managers to implement strategies leading to employment growth rather than cost-cutting measures.

There are several reasons for this latter aspect. Job cuts may have a highly negative impact on staff morale in the investee company owing to the few exit options available in the rigidly structured Japanese labor market. Similarly, layoffs impact negatively on the image of an investee company, which may translate into a substantial threat to sales growth. Furthermore, and likely most importantly, to be seen not to care about employees of an acquired company will substantially increase the cost of winning future deals for

fund managers. Especially in the case of business successions, owners tend to care about the future of their company and employees, and therefore may even request a legally binding agreement not to lay off employees for two to three years after buyout (evidence for conjecture 2). Also, given the importance attached to the banking system in Japan, financial advisors and commercial banks play a pivotal role in the sourcing of deals. As large layoffs would reflect badly not only on the fund manager, but also on the intermediary, fund managers have an incentive to refrain from layoffs in order not to jeopardize their future deal-sourcing capabilities. Besides the concerns of intermediaries pertaining to their own reputation, baseline profits of main banks directly depend on keeping and growing the investee company as their borrowers (evidence for conjecture 3).

To sum up, growth strategies as rational response to a particular social norm may partly explain the impressive growth rates achieved during the holding period. On the one hand, selling business owners are unlikely to reap any future financial benefit from selecting buyers by their (moderate) stance on employment reduction. This indicates that their behavior might be guided by social preferences. On the other hand, moderation in employment reductions pays off in terms of reputational gains for fund managers, sellers with continued business operations, and intermediaries alike.

The interviews revealed three further cues to understanding the “growth blessings brought by vultures” in Japan. First, consistent with the finding that size is negatively related to employment growth (see TABLE 9), fund managers report that there is often little need to reduce employment. This is because the majority of deals are small to medium-sized companies (median number of employees at entry: 200), which not only tend to have less slack (owing to continuous margin pressure in vertical *keiretsu* groups), but also come with more ample potential for scale effects. However, even a tenfold increase in company size would reduce CAGR by a mere 1.62 pp (see TABLE 9), and thus only explains a minor fraction of the difference in employment growth between our findings and the figures reported for most Anglo-Saxon economies. Second, specific to the case of corporate divestments, the parent company often adjusts employment within the group prior to the transfer of ownership. Doing so potentially increases the sale price by the equivalent of the restructuring costs avoided, including reputational risk incurred by sellers and by funds.

Third, in contrast to our initial conjecture about a relative absence of growth opportunities, potential for sales growth through geographic expansion and productivity improvements are reportedly substantial.

Our fourth finding, of funds investing in an industry-mix with negative average employment growth, is consistent with their business logic as traders in second-hand businesses. With prices predominantly determined as multiples of gross profits, effecting the same absolute profit improvement to a poorly profitable business rather than to a well-performing business translates into a much larger gain for the investing fund. For example, increasing profits from 2 to 4% percent may well suffice to double the value of an investee company, whereas boosting profits from 10 to 12% will lead to a value gain of just about 20%.

VI. Concluding Remarks

This study provides novel evidence on four central issues. First, our descriptive analysis of Japanese businesses subject to a buyout transaction between 1998 and 2015 finds average organic growth in standard employment of 12.3% during holding period of (median: 3.92 years, mean: 4.31 years). Second, when corrected for industry-specific labor market trends, growth amounts to 13.4%. Furthermore, given the massive increase in non-standard employment during the period studied (+61% between 1998 and 2015), our estimate of employment under private equity ownership is highly conservative. Third, the study documents that fund managers investing in Japanese private businesses do not merely buy into existing trends, but cause change by creating a positive differential between pre- and post-buyout employment growth. Fourth, buyout funds in Japan bring about this substantial growth in industries with average negative employment growth, thus substantially contributing to reviving the Japanese economy.

Our explanatory model identifies a number of variables with significant links to employment growth: duration of holding period; familiarity of the fund manager with the industry; size of the investee company; and vintage years 2004 and 2011. However,

neither of these factors can explain why employment growth in Japan under fund ownership is so much higher than in other developed economies.

Whereas evidence from our series of interviews indicates that the regulatory framework does not obstruct job cuts, reputation building was found to be relevant as a reflection on the widely accepted social norm of employment protection. These mechanisms are especially relevant in the case of business successions where it ties in with anecdotal evidence on deals not awarded to the highest bidder. Fund managers who cut costs through employment reductions are risking their future cash flows, as a poor reputation may restrict their access to deals. Consequently, all parties involved have a strategic interest in growing employment – or at least, in avoiding layoffs. This supports our third conjecture on strategic behavior in fund managers: dominant growth strategies are arguably a reflection, less of preferences than of strategic motivation.

APPENDIX

TABLE A: PROPERTIES OF DATA MATCHING AND INTER-/EXTRAPOLATION IN SAMPLE

| | | Mean | SD | SD (months) | Min | Max |
|---|-------|-------|------|----------------|-------|------|
| <i>Matching</i> (days) | Entry | -10.9 | 132 | 4.39 | -358 | 323 |
| | Exit | -25.9 | 116 | 3.87 | -354 | 237 |
| <i>Inter-/extrapolation</i> (per cent) | Entry | 0.83 | 7.54 | | -33.4 | 47.3 |
| | Exit | -0.85 | 5.56 | | -61.8 | 15.3 |

TABLE B: SAMPLE COVERAGE OF POPULATION BY VINTAGE YEAR (YEAR OF ENTRY)

| Sample coverage | 1998 | 1999 | 2000 | 2001 | 2002 | 2003 | 2004 | 2005 | 2006 |
|-----------------|------|------|------|------|------|------|------|------|------|
| Population | 1 | 8 | 15 | 21 | 29 | 45 | 53 | 30 | 53 |
| Sample | 0 | 4 | 6 | 10 | 10 | 18 | 21 | 17 | 24 |
| Coverage | 0.00 | 0.50 | 0.40 | 0.48 | 0.34 | 0.40 | 0.40 | 0.57 | 0.45 |

| Sample coverage (contd) | 2007 | 2008 | 2009 | 2010 | 2011 | 2012 | 2013 | Total |
|-------------------------|------|------|------|------|------|------|------|-------|
| Population | | 43 | 32 | 25 | 24 | 13 | 8 | 404 |
| Sample | | 19 | 16 | 11 | 14 | 11 | 1 | 184 |
| Coverage | | 0.44 | 0.50 | 0.44 | 0.58 | 0.85 | 0.13 | 0.46 |

TABLE C: DISTRIBUTION OF DEAL SIZE AND HOLDING PERIOD IN SAMPLE AND POPULATION

| | N | Mean(log) | SD(log) | Median | Min | Max |
|--------------------------------|-----|-----------|---------|--------|------|-------|
| <i>Deal size</i> (billion yen) | | | | | | |
| Population | 356 | 1.59 | 0.68 | 3.86 | 0.10 | 415 |
| Sample | 166 | 1.62 | 0.66 | 4.53 | 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 |

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

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

| Dimension | Category | Population (n=404) | Sample (n= 184) |
|-----------|-----------------------|--------------------|-----------------|
| Fund type | Dependent (J) | 0.51 | 0.55 |
| | Independent (J) | 0.33 | 0.28 |
| | Foreign | 0.16 | 0.17 |
| Deal type | Divestment | 0.28 | 0.32 |
| | Turnaround | 0.23 | 0.15 |
| | MBO | 0.16 | 0.20 |
| | PIPE TP ¹² | 0.13 | 0.11 |
| | Business succession | 0.11 | 0.14 |
| | Secondary buyout | 0.07 | 0.09 |
| | NA | 0.02 | 0.01 |

TABLE E: COMPOSITION OF SAMPLE AND POPULATION BY INDUSTRY

| Industry | Population (n=404) | Sample (n=184) |
|-----------------------------------|--------------------|----------------|
| Bio/Health Care/Medical | 5.20 | 6.52 |
| Construction | 2.97 | 3.26 |
| Consumer Goods and Other Products | 10.15 | 10.33 |
| Electronics/Machinery/Automobile | 15.10 | 20.11 |
| Finance/Insurance | 5.69 | 4.89 |
| Food/Agriculture | 7.67 | 10.87 |
| IT/Telecom/Internet | 7.18 | 5.98 |
| Material/Chemical/Metal/Mining | 6.19 | 5.43 |
| Media/Publishing/Contents | 5.45 | 4.35 |
| Restaurant | 3.71 | 4.35 |
| Retail/Wholesale | 10.64 | 7.61 |
| Service | 16.09 | 14.67 |
| Transportation/Distribution | 3.96 | 1.63 |

¹² Private Investments into Public Equity (PIPE) occur when a buyout firm acquires stocks of a publicly traded company. If the acquired company is subsequently delisted, the transaction is referred to as “take private” (TP).

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

| Dimension | Category | Population [†] (%) | Subset TDB (n=137) | | Subset C (n=58) | |
|---|-----------------------|--------------------------------|--------------------|----------------|------------------|----------------|
| | | | Structure (%) | Impact (pp) | Structure (%) | Impact (pp) |
| Holding period (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 |
| Industry | Finance/ insurance | 5.69 | 5.11 | -0.04 | 3.45 | -0.17 |
| Vintage | 2004 | 13.12 | 11.68 | -0.10 | 10.34 | -0.19 |
| | 2011 | 3.22 | 8.03 | 0.42 | 5.17 | 0.17 |
| Deal size (mean(log(value))) ^{††} | | 1.59 | 1.62 | -0.05 | 1.65 | -0.10 |
| Sum of estimated impact | | | | 0.23 | 0.34 | |

Notes:

Subset TDB as used in Table 2, subset C as used in Table 3. Impact estimated as product of structural differentials and parameter estimates (Table 4).

[†] n = 383 for holding period >12 months, n = 404 for Industry and Vintage, n = 355 for Deal size

^{††} n_{TDB} = 119, n_C = 52

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