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Abstract

Ayash and Rastad (2017) express several concerns about our 2014 analysis of private equity buyouts. We welcome their interest in our work but think their criticisms are off the mark. Some of their claims reflect a misunderstanding of the Census Bureau's Longitudinal Business Database (LBD) and its underlying data inputs. Because the LBD has emerged as a major laboratory for empirical studies in economics and finance, we use this opportunity to reiterate and clarify some of its important features. In a similar spirit, we elaborate on steps taken to develop our large sample of private equity buyouts. We also address Ayash and Rastad's remarks about the empirical design of our establishment-level analysis, our methods for distinguishing between leveraged buyouts (LBOs) and other private equity transactions, bankruptcy rates among firms acquired in LBOs, their assertion that we undercount large public-to-private LBOs, and other matters.

I. Introductory Remarks

Ayash and Rastad (2017) critique our *American Economic Review* article titled "Private Equity, Jobs, and Productivity" (Davis et al., 2014). We welcome their interest in our work but think their criticisms are off the mark. We explain why in this response.

Some of their criticisms reflect a misunderstanding of the Census Bureau's Longitudinal Business Database (LBD), an essential source of data in our study. The LBD has emerged as a major laboratory for empirical research in economics and finance. Thus, we think it is especially important to reiterate and clarify key features of the LBD and its underlying data inputs. Specifically, the LBD is a longitudinal version of the Census Business Register (BR), which derives its core data from IRS quarterly payroll reports – mandatory filings for all employers with at least one paid employee in the U.S. private, non-farm sector. The BR and LBD are supplemented by mandatory business surveys, including the Economic Censuses and the Company Organization Survey. The employment data in the BR and LBD derive directly from payroll tax filings, e.g., Form 941 Quarterly Payroll Tax Filing. Ayash and Rastad repeatedly state (incorrectly) that a limitation of our analysis is that the Census data are "voluntary." They are not. We return to this matter below.

In a similar spirit, we elaborate on steps taken to develop our large sample of private equity buyouts. We highlight challenges in identifying these transactions, and how our approach deals with them. We also address Ayash and Rastad's remarks about the empirical design of our establishment-level analysis, our methods for distinguishing between leveraged buyouts (LBOs) and other private equity transactions, bankruptcy rates among firms acquired in LBOs, their assertion that we undercount large public-to-private LBOs, and other matters.

II. Responses

Our Post-Buyout Analysis Window

Regarding our firm-level analysis, Ayash and Rastad argue that the "period of analysis is too short … and that drawing conclusions is premature in such a setting..." While it would be worthwhile to extend the post-buyout evaluation period, our two-year period is clearly long enough to document a large increase in within-firm reallocation at buyout targets (relative to controls), and to show that this reallocation spurt yields sizable TFP gains in target firms. Moreover, our two-year window allows for more accurate tracking of firms than a longer window, as we discuss in our article.

Distinguishing between LBOs and Other Transactions

Ayash and Rastad claim that the standard approach to distinguishing between LBOs and growth equity buyouts is to impose a transaction value cutoff, because "data service providers have difficulty differentiating between leverage buyouts and growth equity buyouts." In contrast, we identify these transactions directly through extensive research.

The cutoff approach yields two types of errors: (*a*) in deals larger than the cutoff, the improper inclusion of **all** growth equity buyouts, and (*b*) in deals smaller than the cutoff, the improper exclusion of **all** leveraged buyouts. Since these errors are hard-wired under the cutoff approach, it is hard to see its appeal. As an illustration, consider two transactions completed by private equity funds in 2015. In one, KKR agreed to buy roughly one-quarter of Arbor Pharmaceuticals, a specialty drug maker, for over $250 million. In another, AUA Private Equity Partners LLC acquired the entirety of Tijuana Flats Holdings, a small chain of Tex-Mex restaurants, for about $25 million.[1] The latter is the type of transaction we seek to study, while the former is not. The cutoff approach would misclassify both transactions.

We also disagree with the claim that the standard approach in the buyout literature is to distinguish between leveraged buyouts and growth transactions using a size cutoff rather than research into the nature of transactions. While some early papers use size cut-offs, they did so due to a lack of data on smaller transactions, not a desire to exclude growth buyouts (e.g., Kaplan, 1989, footnote 3). Most of the major papers in the recent literature on private equity rely on classification methodologies resembling ours to a greater or lesser extent. Examples include Axelson et al. (2013), Faccio and Hsu (2017), Fang et al. (2013) and Ivashina and Kovner (2011).

When we developed our buyout dataset in conjunction with the World Economic Forum effort on the long-term impact of private equity, we intended to use it for papers on the demographics of private equity (published as Kaplan and Stromberg, 2009) and private equity and innovation (published as Lerner, Sorensen, and Stromberg, 2009). Rather than using an arbitrary cutoff rule, we made major efforts to ensure the integrity of the sample, including thousands of research assistant hours by teams in Boston and Stockholm. More specifically, we undertook a two-part effort, as documented in Stromberg (2008). We began with the CapitalIQ database and constructed a base sample of leveraged buyout transactions. We first selected all M&A transactions in CapitalIQ

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[1] These examples draw from press accounts and private equity databases.

classified as "leveraged buyout," "management buyout," and "JV/LBO" and announced between January 1, 1970 and June 30, 2007. To this sample, we added all M&A transactions undertaken by a financial sponsor classified as investing in "buyouts."

CapitalIQ started its data service in 1999 and its coverage has improved over time. Although CapitalIQ has been back-filling their data using various sources, their coverage is likely to be incomplete in the early years of our sample. In light of this concern, our sample development and assessment efforts included comparisons to transaction lists for the 1980s compiled by other researchers and the business press. We determined that the largest source of discrepancies between our sample and these lists was due to pure "management buyouts" – LBO transactions not sponsored by a buyout fund or other financial institution but, instead, undertaken by management itself. These management buyouts are not the object of our study, as we remark in our article (pages 3958 and 3960, fn. 3).

The overlap with our CapitalIQ-based sample is better, not surprisingly, when focusing on LBOs with a financial sponsor. For instance, 62 of the 74 transactions in Kaplan's (1991) hand-selected sample of LBOs are captured by CapitalIQ, which implies a coverage rate of 84% during the 1979-1986 period. We added the 12 missing transactions to our sample, as we did for other private equity buyouts we identified using various sources of data in addition to CapitalIQ.

In the course of our investigations, we also discovered that CapitalIQ classifies certain buyout fund transactions as "private placements" rather than acquisitions. In most cases, these private placements involve minority stakes or follow-on investments and, hence, are not suitable for inclusion in our sample. Still, the distinction between buyouts and private placements is not always clear. In addition, some transactions reported as LBO deals reflect venture capital investments, which are not the object of our study. We sought to err on the side of excluding non-LBO transactions but, as a result, we may exclude some bona fide LBOs.

For the purposes of our study in Davis et al. (2014), we also excluded acquisitions that were announced but not yet completed, acquisitions of non-control stakes (typically associated with growth and venture transactions, not classic buyouts), acquisitions of stakes in public companies that remained publicly traded (PIPES), and other misclassified transactions. We identified these problematic transactions through a careful review of text fields in CapitalIQ records and our own detailed research using other databases and media accounts. That left us with a total sample of approximately eleven thousand PE-led leveraged buyout transactions worldwide over the period

January 1, 1980 to December 31, 2005. Dropping transactions that involve firms with foreign headquarters left about 5,000 U.S. target firms acquired in private equity buyouts between 1980 and 2005. We matched these firms to the LBD using name and address matching, as described at length in our article.

Our Inclusion of Secondary Buyouts

Ayash and Rastad suggest it is inappropriate to include secondary buyouts in our sample. We do not see why. Secondary buyouts are effectively treated twice by two different buyout firms, and that fact is reflected in how we handle them in our analysis. In any event, we also note (p. 3979) that "There are good reasons to think employment effects vary by type of buyout." We then show (Table 7) how the estimated employment and excess job reallocation responses differ by type of buyout, including a separate column for secondary buyouts. Thus, the interested reader can easily assess how the inclusion/exclusion of secondary buyouts in our sample affects the results. Reflecting this logic, many published articles in the recent literature on private equity include secondary buyouts in their samples. Examples include Axelson et al. (2013), Bernstein and Sheen (2016), Lerner et al. (2011), and many others.

The Empirical Design of Our Establishment-Level Analysis

Regarding the empirical design in our establishment-level analysis, Ayash and Rastad remark: "Traditionally, such empirical tests only include the target firms while *treated*, or controlled by the private equity fund." This mischaracterizes the literature. Consider, for instance, such classic papers as Kaplan (1989), Kaplan and Stein (1993), and Andrade and Kaplan (1998). These and many other works consider the evolution of the PE-acquired firm over fixed time horizons after the buyout, regardless of the continuing involvement of the private equity investor.

In any event, the implied criticism is off the mark. It is well understood in other settings that we care about treatment effects that persist beyond the treatment period and effects that manifest after the treatment period ends. There are, for example, vast literatures on how maternal nutrition and behavior affect child health outcomes (e.g., Black et al., 2008) and on how the early childhood environment affects child, adolescent and adult achievement (e.g., Heckman, 2006). Prominent recent work by Chetty et al. (2016) studies how moving to a better neighborhood as a child affects college and adult outcomes. In short, the practice of limiting attention to treatment effects during the treatment period would be a weakness, not a strength. Similarly, there are numerous studies in the finance literature on the long-run consequences of financial intervention, even when the

treatment has quite limited duration. Ritter's (1991) study of IPOs is a famous example, as is Bethel et al.'s (1998) study of a purchase stake in a firm by an activist investor.

Ayash and Rastad appear to misunderstand the nature and purpose of our establishment-level analysis. On page 3 and again on page 10, they characterize our establishment-level analysis of employment dynamics as encompassing continuers, divestitures and deaths. That is incorrect. Our establishment-level analysis starts with the set of establishments operated by target firms as of the buyout year, and it then tracks those establishments forward (and backward) in time from the event year. Ownership transfers, including divestitures, do not enter into our tracking of establishments. In particular, if a bankrupt firm sells some of its establishments to another party, that divestiture makes no contribution to our quantification of establishment-level employment dynamics except insofar as the establishments expand (creation) or contract (destruction) employment.

Ayash and Rastad also misunderstand and mischaracterize how the Census Bureau distinguishes between acquisitions and births and between divestitures and deaths. They state on page 14: "Given that census data is voluntary, it is not possible to determine with certainty whether a new establishment is an *acquisition* or a *birth.* Due to the voluntary nature of census data, a birth could simply be the acquisition of an establishment that was not previously reported to census by the controlling firm…." This comment again reflects the misperception that the Census data are voluntary. The mandatory payroll tax filings that feed into the BR and LBD capture information about ownership changes. Moreover, the longitudinal establishment identifiers in the LBD avoid classifying such events as spurious births or deaths. More details on tracking establishments and firms in the LBD are available in Davis et al. (2007) and Haltiwanger et al. (2013).

LBOs and Bankruptcies

As our remarks above make clear, Ayash and Rastad's concern related to bankruptcies is a red herring. Still, it's worth noting that their factual claim about high post-buyout bankruptcy rates is not supported in the empirical literature. They point to Andrade and Kaplan (1998), who consider a sample of 136 Highly Leveraged Transactions (HLTs) from the late 1980s. That period is unrepresentative of our larger sample, due to the intense use of leverage in transactions in 1986 and 1987 (Kaplan and Stein, 1992). In fact, Kaplan and Stromberg (2009), after extensive research into transaction outcomes, conclude that ***only 6% of private equity transactions end in bankruptcy***. This figure reflects bankruptcies while the target firm was under PE control. Moreover, Andrade and

Kaplan (1998) conclude that the economic costs of financial distress are small, which suggests that bankruptcies have little impact on the economic outcomes that we study in our *AER* article.

Ayash and Rastad also point to Ayash and Shutte (2016), who limit attention to public-to-private LBOs. For reasons of data availability, Ayash and Shutte drop most public-to-private LBOs and work with a sample of only 183 LBOs. As they discuss, their sample is selected in ways that are correlated with financial and operating performance. That makes it a questionable source of guidance about the frequency of post-buyout bankruptcy rates.

They also point to a *Wall Street Journal* article by Marymount (2012), which states that of the "77 businesses Bain invested in while Mr. Romney led the firm," 22% either filed for bankruptcy reorganization or closed their doors by the end of the 8th year after Bain first invested." Aside from the narrow focus on one PE group under one person's leadership, it's unclear whether the 77 businesses are restricted to LBOs. In particular, Bain in its early years executed numerous venture capital transactions, many of which failed, consistent with the performance of venture investments more generally. The statistics in the *Wall Street Journal* article include bankruptcies **after** the firms were no longer under Bain's ownership and control. Only 5 of the 17 bankruptcies (among the 77) clearly happened under Bain control. That is, only about 6% of the 77 clearly involved bankruptcy while under Bain control. Finally, we note that the *Wall Street Journal* article offers another potential explanation for why Bain companies might have experienced high bankruptcy rates: Bain's preponderance of investments in small companies, which tend to fail at a higher rate.

Claims that We Undercount Large Public-to-Private LBOs

Ayash and Rastad also assert (e.g., p. 6) that we seriously undercount large public-to-private LBOs. This assertion reflects a faulty understanding of the LBD, their reliance on problematic Compustat data, the highly-selected nature of their Compustat sample, and an uninformative sample comparison:

- **Faulty understanding of the LBD**: Ayash and Rastad write that LBD "Employment data is drawn from survey sources and is voluntarily reported." (page 13) In fact, LBD employment data derive from administrative records and mandatory government surveys, as remarked above. Moreover, the LBD contains employment data for the **universe** of tax-paying businesses with paid employees in the United States. Thus, there is no issue (pages 11-12) "regarding the representativeness of voluntarily reported U.S. Census data" on employment in the LBD.
- **Problematic Compustat Data, I:** Compustat employment is the number of company workers reported to shareholders. It may be an average number of employees during the year or a year-end figure. Since the source data from annual reports can be incomplete, some

- Compustat firms have missing employment data even when the firm has positive sales. In addition, Compustat employment includes all employees of the listed firm's consolidated subsidiaries, domestic ***and foreign***. Thus, Compustat employment data are not limited to domestic U.S. operations, the focus of our study.
- **Problematic Compustat Data, II:** Davis et al. (2007) directly compare Compustat-reported employment for listed firms to employment records in the comprehensive LBD, which is built up from tax and other records for U.S. ***domestic*** operations. They report that Compustat employment is about 40% greater than LBD employment for the same set of Compustat firms. In addition to the inflated employment totals in Compustat, the extent of measurement varies greatly across firms in terms of both levels and changes. In this regard, Davis et al. report that the correlation between five-year firm-level growth rates as measured from Compustat and the corresponding employment growth rate in the LBD is 0.64 and only 0.54 on an employment-weighted basis. Given the high quality of the LBD employment data, the inflated employment figures in Compustat and their low correlation with LBD figures reflect errors in the Compustat employment data, at least as a measure of U.S. operations.
- **A Highly-Selected Sample:** Ayash and Rastad's sample restricts attention to LBOs that did not undertake significant divestitures and acquisitions. There's no sound economic reason to exclude LBOs with significant post-buyout acquisitions and divestitures, if one wants to obtain a full picture of how LBOs affect employment (on organic and other margins). In fact, the divestiture and acquisition of operating units are standard features of the tool-kit employed by buyout firms, as many business school case studies attest. Ayash and Rastad's sample also restricts attention to LBOs with publicly available data before and after the buyout event. There's no sound economic reason to impose this restriction either. Much previous work on LBOs imposes one or both of these two restrictions, ***because data limitations left no better alternative.*** These sample restrictions can bias the results in unknown ways. Our work circumvents the need to impose these sample restrictions, which is a major advantage of our approach and our reliance on the LBD.
- **An Uninformative Comparison:** Ayash and Rastad compare the Compustat employment total in their highly-selected sample of 108 Public-to-Private LBOs from 1980 to 2006 to the LBD employment total in our sample of 248 Public-to-Private buyouts from 1980 to 2003. On this basis, they suggest "that larger Public-to-Private LBOs are underrepresented in U.S. Census data." This comparison is tilted and uninformative in several respects: First, Compustat data overstate employment due to the inclusion of foreign operations. Second, their use of a transaction value cutoff in screening for leveraged buyouts involves two types of classification errors, as discussed above; and it virtually ensures that acquired firms with smaller employment levels are screened out of their sample. Third, their sample is highly selected in two other respects – by excluding firms that undertook significant divestitures and acquisitions post buyout, and by restricting attention to firms with publicly available data before and after buyout. Fourth, they do not align their sample period with ours.

For all these reasons, Ayash and Rastad have not provided a sound basis for their assertion that our sample systematically underrepresents larger public-to-private LBOs.

Ayash and Rastad also offer a skewed characterization of how the underrepresentation of larger public-to-private LBOs would affect our analysis, arguing on page 14 that it "would

understate the *Divestiture* and *Deaths* adjustment margins presented in our Figure 1, and potentially increase the impact of leveraged buyouts on job destruction." Possibly, but by the same logic it might also understate acquisitions, job creation and the opening of new facilities.

<u>Timing Details</u>

Ayash and Rastad express concerns (page 10) that we "use October as the cutoff when the U.S. Census data is published every March." They argue for instead assigning all buyout events during calendar year t to employment data for the end of year t (December), as illustrated in their Figure 3, Panel A. This approach treats the December employment value in year t as the event-year 0 outcome for all buyouts in t, regardless of whether they occurred in January, February, etc. The analogous approach when using Census data treats, say, the March 1990 employment value as the event-year 0 outcome for all buyout events that occur from April 1989 to March 1990.

The choice between Ayash and Rastad's approach and our preferred approach trades off two concerns: minimizing the (fractional-year) mistiming of buyout events, and ensuring that no fractional-year pre-event changes are commingled with post-event changes. Our approach minimizes the mean mistiming gap at the cost of some commingling. To see this point, consider the following remarks:

- **Mean mistiming gap:** Suppose a buyout deal closes in January. Then Ayash and Rastad's approach yields a mistiming gap of 11 months from the January buyout event to the December employment observation assigned to that buyout in event-year 0. If buyouts are uniformly distributed over the calendar year, the mean absolute mistiming is 5.5 months. There is no way to eliminate this mistiming gap, absent monthly data. However, we substantially shrink the mean absolute mistiming gap by using an October cutoff (coupled with March Census data) when assigning buyouts to event years.[2] When buyouts are uniformly distributed over the calendar year, our approach yields a mean absolute mistiming gap of 3 months.[3] Our approach minimizes the mean absolute mistiming gap if buyouts are uniformly distributed over the year. As an empirical matter, these transactions are distributed very evenly over the calendar year. Inspecting our sample of transactions between 1980 and

[2] Quoting from page 3964 of our article: "The LBD reports total employment in the payroll period containing the week of March 12th. Accordingly, for buyout transactions that close before October 1, LBD employment in March of the same calendar year serves as our contemporaneous employment measure. We assign transactions that close on or after October 1 in year t to year $t+1$ for purposes of our analysis, treating the LBD employment value in March of $t+1$ as the contemporaneous measure. October is the natural cutoff because it lies midway between March-to-March employment changes in the LBD."

[3] A continuous-time or daily approach to the same calculations reveals an even larger percentage reduction in the mean absolute mistiming gap for our approach.

2013, the closing date falls into the first quarter 23.4% of the time, the second quarter 23.9%, the third quarter 24.2%, and the final quarter 28.4%.

- **Commingling pre- and post-event changes:** Suppose a buyout deal closes in July 1990. Our approach treats the March 1990 LBD employment value as the event-year 0 outcome. Thus, when we calculate the event-year 1 employment change using data from March 1990 and March 1991, we commingle the pre-event change from March to July 1990 with the post-event change from July 1990 to March 1991. This is a potential source of measurement error in our approach.

We see this commingling as a minor matter for our purposes. Concerns about commingling apply with equal force to creation and destruction, because we treat creation and destruction in a fully symmetric manner. So, if we slightly understate event-year 1 creation due to commingling, we understate event-year 1 destruction for the same reason. These two errors have offsetting effects on our estimated net employment response to buyout events.

In any case, the concerned reader can inspect Figures 3 or 4 or Table 3 in our article and easily discern the impact of dropping event-year 1 outcomes. Dropping the event-year 1 outcomes obviates any concerns that commingling has somehow caused us to paint PE buyouts in an overly favorable light. In fact, as a quick look at our Figure 3 or 4 or Table 3 reveals, dropping the event-year 1 outcomes mutes the cumulative net employment losses associated with PE buyouts (relative to controls).

As a footnote to this discussion of timing details, it is worth pointing out that Ayash and Rastad's preferred approach also involves the commingling of pre-event and post-event employment outcomes when using Compustat data. To see this point, recall that some firms report annual average employment to their shareholders (and, thus, to Compustat), not end-of-year employment. Consider a buyout event that occurs in, say, August 2000. When Ayash and Rastad apply the 2000 Compustat employment value for such a firm, they commingle employment outcomes that occurred before the buyout (January-July 2000) with employment outcomes that occurred afterwards (August-December 2000). For firms that report annual average employment values in their regulatory data, Ayash and Rastad's approach involves the same extent of commingling as our approach when buyout events are uniformly distributed over the calendar year.

<u>Our Treatment of Job Creation and Destruction</u>

Ayash and Rastad claim (p. 10) that we attribute "all of the job creation observed in the wake of the buyout (t = 0) to the private equity funds without empirical justification."[4] This statement is misleading and inaccurate in two respects. First, we treat creation and destruction symmetrically in this respect. There is no tipping of the scales by treating creation one way, and destruction another. Second, we omit the event-year 0 outcomes (i.e., changes from the prior year to event-year 0) when characterizing the effects of buyouts. For example, when discussing Figure 3B on page 3968, we write "The differentials cumulate to 3.2 percent of employment in the first two years post buyout and 6.4 percent over five years." We obtain these figures by summing the losses shown in our Figure 3B over event-years 1 and 2 (3.2%) and event-years 1 to 5 (6.4%). Likewise, when discussing Table 3 on page 3972, we write "Implied five-year cumulative employment losses at targets range from -4.7 percent to -6.4 percent …" Here again, we exclude event-year 0 and sum over event-years 1 to 5. For the firm-level analysis later in our article, we stress cumulative results that sum over outcomes in event-years 1 and 2.

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[4] A similar claim appears on page 11.

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