# A Comment on Bai, Li, Xue, and Zhang (2019)[1]

*Does Costly Reversibility Matter for U.S. Public Firms?*

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We recently became aware of the above-referenced working paper co-authored by Hang Bai (University of Connecticut), Erica Li (Cheung Kong Graduate School of Business), Chen Xue (University of Cincinnati), and Lu Zhang (Ohio State University) (Bai et al. (2019)), which challenges results in Clementi and Palazzo (2019), previously circulated as NBER working paper #21064.

In pages 1 through 5 of this document, we demonstrate that BLXZ's challenge to our results has no merit. In an ideal world, we would have happily provided those clarifications to BLXZ via standard means of inter-personal communication. Unfortunately, we were never asked.

In pages 6 through 8, we comment on BLXZ's own empirical results. We point out severe discrepancies between their own estimates and results that are very well established in the literature. In page 9, we briefly consider BLXZ's simulation results.

## On the claims by Bai, Li, Xue, and Zhang (BLXZ)

Citing directly from their paper, BLXZ claim that our evidence

*"originates from the combination of a data error that arises from an internal inconsistency between their measures of net investment and depreciation rates, non standard sample criteria that curb the right tail of the investment rate distribution, and a highly questionable research practice of cutting off the right tail of the quarterly investment rate distribution at 0.2"*

**Our response.** We reassure the interested reader that

1. There is no inconsistency between our measures of net investment rate and depreciation rate;
2. Our sample criteria are entirely standard;
3. The "cutting off" of the right tail of the investment rate distribution is nothing but the imposition of symmetric bounds on a frequency distribution, for the mere purpose of representing it graphically. The sample used in our analysis does not impose this graphical cut-off.

[1] The views expressed are those of the authors and do not necessarily reflect those of the Federal Reserve Board or the Federal Reserve System.

[2] New York University and NBER.

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## On claim #1

In page 13 of their paper, BLXZ write:

*"The Clementi-Palazzo investment rate measure is flawed. Net PPE and the depreciation rate in the gross investment rate are not independent of each other. The accounting depreciation rates in Compustat (not BEA's depreciation rates) are embedded in net PPE via perpetual inventory method, which implements the capital accumulation equation. As noted, most U.S. firms use the straight-line depreciation method for financial reporting purposes. In contrast, BEA estimate geometric depreciation rates (Hulten and Wykoff 1981; Fraumeni 1997). As such, the Clementi-Palazzo investment rate violates the capital accumulation equation for every period and for every firm."*

For the purpose of testing the theory, we need to measure in the data two related phenomena: The evolution of the capital stock and the cash-flow from investment/disinvestment activity. They are related, because to the extent that there is no private component to the value of capital, $k_{t+1} - k_t = I_t - D_t$. That is, the only difference between the evolution of the capital stock $(k_{t+1} - k_t)$ and the cash flow from investment/disinvestment $(I_t)$, is economic depreciation $D_t$.

Compustat data provides us with Net PPE, which we are going to denote with $\hat{k}$, and accounting depreciation $\hat{D}_t$. According to GAAP's guidelines, companies are supposed to make sure that

$$\hat{k}_{t+1} = \hat{k}_t + \hat{I}_t - \hat{D}_t, \tag{1}$$

where $\hat{I}_t$ is the expenditure incurred in the purchase of new assets.

One could argue that, no matter the significance she attaches to $\hat{k}$ and $\hat{D}$, she can always recover the investment $\hat{I}_t$, which equals the gross addition to the capital stock and also the investment cash-flow.

Figuring out the evolution of the actual capital stock is a lot harder, as one needs to know that initial economic value $k_t$ (with $k_t \neq \hat{k}_t$ in general) and the actual economic depreciation of the assets $D_t$ (with $D_t \neq \hat{D}_t$ in general). With those two variables in hand, one can compute $k_{t+1}$ as follows:

$$k_{t+1} = k_t + \hat{I}_t - D_t.$$

BLXZ assume that $k_t \equiv \hat{k}_t$ and, importantly $D_t = \hat{D}_t$ – economic depreciation equals accounting depreciation – at all times. This methodological choice leads to estimates of the moments of the investment rate's distribution that are very far from any other published estimate. See page 6 for a detailed meta-analysis.

The main reason why we decided against recovering the cash flow from gross investment using (1), is that it always provides a downward-biased representation of the cash-flow deriving from the disposition of assets. When blindly adopting equation (1), the sale of an asset that was depreciated completely (from an accounting perspective) always generates zero cash inflow, no matter the actual proceeds from the sale. This is particularly problematic when testing the neoclassical model, since to a large extent the riskiness of value firms depends upon their ability to generate cash by selling assets when hit by a negative shock.

A second reason is that the "Depreciation and amortization" Cash-Flow variable in Compustat includes items that have nothing to do with the depreciation of fixed assets, even after removing the amortization of intangibles (as BLXZ do). For example: amortization of deferred charges, amortization of tools and dies, amortization or depreciation of asset accounts reported as a positive number, and debt issuance costs.

We decided instead to proceed as much of the literature and take the series of Net PPE as our series of capital stock, i.e. $k_t \equiv \hat{k}_t$. Belo and Lin (2012), Liu and Zhang (2014), Dangl and Wu (2016), and Murray and Shen (2016) are only a few of the recent papers that made the same assumption.

Then we imposed that economic depreciation is given by the sector-specific rates ($\delta$) estimated by the NBER, and recovered investment residually:

$$I_t = \hat{k}_{t+1} - \hat{k}_t + \delta.$$

We found it extremely reassuring that the unconditional cross-sectional moments of the process we recover are close to those reported in other Compustat-based investment studies. In particular, our mean annual investment rate of 15.6% is close to the values reported by Gomes (2001), Barnett and Sakellaris (1998), and Kogan et al. (2018): 14.5%, 16%, and 17.5%, respectively.

Contrary to what BLXZ state, there is no inconsistency in our procedure. Faced with severe data limitations as all other scholars that try to reliably estimate investment rates, we made informed and well motivated methodological choices, which we carefully illustrated in the paper for the sake of the research community.

As is well known to scholars in the field, and likely transpires from the meta-analysis to follow, estimating moments of the distribution of investment rates is not trivial, in particular when the only available information consists of accounting data. It is not surprising then, that economists have adopted different methodologies. Lu Zhang, a member of the BLXZ team, has used at least three different methods in his research, as can be garnered by reading Liu et al. (2009), Belo et al. (2013), and Goncalves et al. (2019).

## On claim #2

In page 15 of their paper, BLXZ write:

*Our replication sample contains 463,426 firm-quarter observations, in contrast to Clementi and Palazzo's (2019) 296,218. The crux is that we adopt sample criteria that are more standard in empirical finance. In contrast, Clementi and Palazzo impose substantially more stringent sample criteria: (i) Excluding financial firms, utilities, and unclassified firms (SIC codes $\geq$ 9000); (ii) dropping firms with fewer than 12 past quarterly investment rates (i.e., dropping the first 12 quarterly investment rate observations); (iii) dropping firm-quarter observations associated with acquisitions larger than 5% of total assets; (iv) discarding firm-quarter observations in the top and bottom 0.5% of the pooled distribution of quarterly investment rates; and (v) dropping firm-quarter observations with missing values of investment rates or book-to-market. In particular, criterion (ii) and (iii) largely explain the substantial differences between our replication sample and their original* *sample.*

Below is a comprehensive list of the criteria we have followed when forming our sample, along with a (often obvious) justification:

- **On excluding financial firms, utilities, and unclassified firms (SIC codes $\geq$ 9000).** Excluding financial firms and utilities is entirely standard practice (e.g., Belo et al. (2014)). Excluding unclassified firms and firms with government links (SIC $\geq$ 9000) is also rather common – see for example the paper by Claudia Custodio "Mergers and Acquisitions Accounting and the Diversification Discount" published in the Journal of Finance in 2014 (Custodio (2014)). However, their number is so small that results are unaffected by their exclusion. In short, we adopted the criteria by Li et al. (2016), who wrote: *"Following the literature, we remove all regulated utilities (SIC 4900-4999), financial firms (SIC 6000-6999), and quasi-governmental and nonprofit firms (SIC 9000-9999)"*.

- **On dropping firms with fewer than 12 past quarterly investment rates (i.e., dropping the first 12 quarterly investment rate observations).** This is not an obvious point. Our version of the neoclassical investment model (and BLXZ's too, as far as we can tell) admits an ergodic and stationary distribution. Our analysis is conducted on that distribution. This means, in particular, that the simulation results do not account for the life-cycle behavior of firms that enter the stock market at a very early age – well outside the ergodic distribution, so to speak. By dropping the first 3 years, we are effectively mitigating this problem, bridging the distance between the model and the evidence we want to confront it with. That is, we restrict our sample to the firms that are closer to their "long-run" scale. Keeping the first three years of observations would have been justified only if our model could account for the firm life-cycle of firms that went public early. We decided against the latter option, to present a model as close as possible to that in Zhang (2005).

- **On dropping firm-quarter observations associated with acquisitions larger than 5% of total assets.** This choice is justified by the fact that the standard neoclassical investment model is not designed to account for mergers and acquisitions. For the same reason,Whited (1992) eliminates from her sample any firm that *"that has been involved in a merger accounting for more than 15% of its assets"*. In fact, there is an active literature on how to model in this framework the synergies that drive most M&A activity. See Dimopoulos and Sacchetto (2017). It turns out that our view is also shared by Erica X. N. Li and Chen Xue, the second and third component (in alphabetical order) of the BLXZ team. On page 12 of their paper "Intangible Assets and Cross-Sectional Stock Returns: Evidence from Structural Estimation," co-authored with Liu (Li et al. (2014)), one reads: *"Finally, we delete firm year observations when a firm went through a major restructuring defined as acquisition amount (Compustat item AQC) is more than fifteen percent of the book value of asset."*

- **On discarding firm-quarter observations in the top and bottom 0.5% of the pooled distribution of quarterly investment rates.** This is standard procedure to avoid undue influence of outliers on investment rate moments.

- **On dropping firm-quarter observations with missing values of investment rates or book-to-market.** Since our paper is about the relation between investment activity and value premium, it appears legitimate to require the investment rate and the book-to-market ratio to be available for all firms in our sample.

## On claim #3

In page 15 of their paper, BLXZ write:

*"The differences between Panel B of Figure 4, which is a direct reproduction of Clementi and Palazzo's (2019) Figure 1, and their original Figure 1 raise a disconcerting question. The only way we can fully reproduce their Figure 1 is to cut off the right tail of the firm-level investment rate distribution at 0.2. Nowhere in their paper do Clementi and Palazzo admit that such a problematic procedure has been performed. However, their original Figure 1 displays a probability mass at 0.2 that is clearly higher and thicker than the mass at -0.2. Because Clementi and Palazzo conclude "no sign of irreversibility (p. 289)," yet a long right tail of the investment rate distribution is the "smoking gun" evidence in support of costly reversibility (Cooper and Haltiwanger 2006), cutting off the right tail is, at a minimum, a highly questionable research practice (Simmons, Nelson, and Simonsohn 2011)."*

The intent of Figure 1 in our paper is simply to illustrate the substantial mass of firms with negative investment rate. Obviously, the moments' calculation includes <u>all</u> observations in our sample, and in particular those associated with investment rates greater than 20%. None of our empirical results are affected by the way we represent graphically the frequency distribution of the investment rate. It is also worth noting that in Table 1 of our paper we report the fraction of observations associated with negative and positive investment spikes.

## The estimates in BLXZ

BLXZ's empirics generates an estimated **mean cross-sectional gross investment rate of 38%** and a **standard deviation of 58%**. How do such results compare with alternative estimates? Are they consistent with other empirical evidence?

We selected 15 papers that calculate gross investment $I_t$ and physical capital $K_{t-1}$ following a variety of methodologies. We purposely excluded papers that use total book assets as a measure of $K_t$. The means and standard deviations of $I_t/K_{t-1}$ across all of these studies are reported in Table 1 and plotted in Figure 1. At the bottom of the table, we also report the mean and standard deviation of both statistics. It is rather clear that the moments obtained by BLXZ are wide outliers.

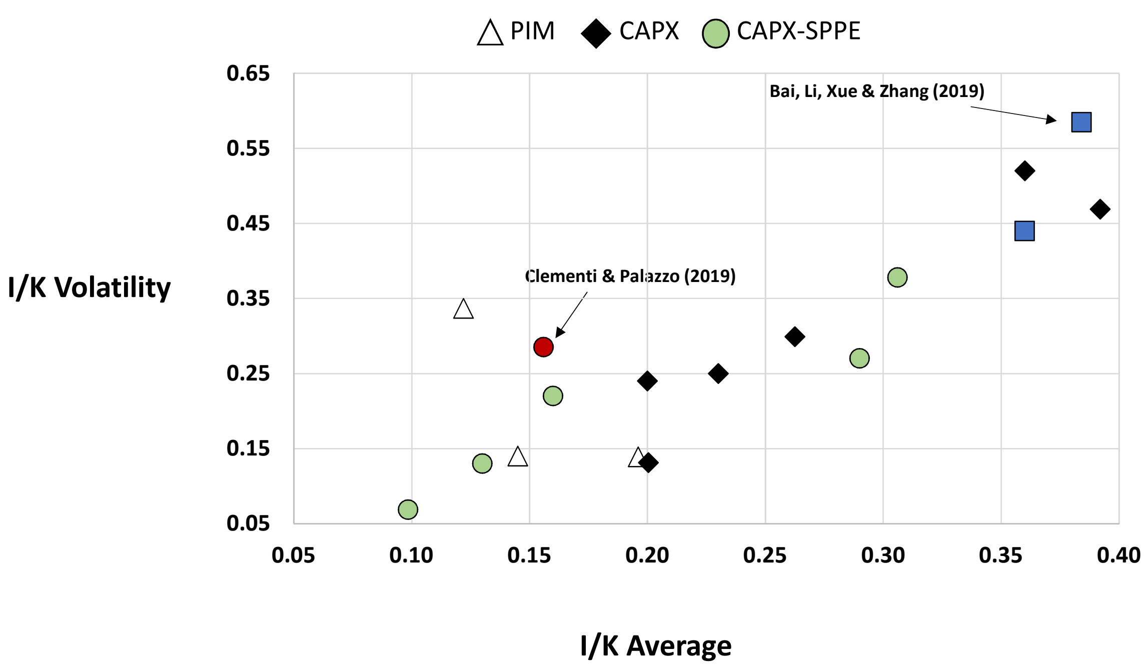


Figure 1: **Investment rate moments across recent studies.** Every marker is associated with one of the studies listed in Table 1. A white triangle indicates studies that use the perpetual inventory method (PIM). A black diamond indicates studies that measure gross investment using CAPX. A green dot indicates studies that measure gross investment using the difference between CAPX and SPPE. The red dot indicates the values in Clementi and Palazzo (2019). The blue squares indicate studies that follow the methodology in BLXZ.

An alternative way to put BLXZ's results in context is to benchmark their mean investment rate and mean depreciation rate against the aggregate measures implied by the data at the Bureau of Economic Analysis. Of course, there is no reason to expect that the cross-sectional mean investment rate equals the aggregate value, since a very high cross-sectional mean could result from a large number of small fast-growing companies. However, it is worth recalling that investment rates were consistently found to be declining in firm age and that the average age of publicly traded firms is substantially greater than

Table 1: **Investment rate average and volatility.** This table reports the average and the volatility of the investment rate calculated by 15 papers over the period 1998–2019. For each paper, we also report the publication outlet (WP if not published), the publication year, and the sample period. We purposely excluded from the list papers that use the total book value of asset a measure of $K_t$. The bottom rows report the average and the standard deviation of the first and second moment of the investment rate. All of the studies except Cooper and Haltiwanger (2006) calculate the investment rate moments using U.S. publicly listed firms. Cooper and Haltiwanger (2006) use a *"balanced panel from the LRD consisting of approximately 7000 large, manufacturing plants that were continually in operation between 1972 and 1988"*.

| Authors | Mean | Volatility | Journal | Sample | |
|---|---|---|---|---|---|
| | | | | start | end |
| Barnett and Sakellaris (1998) | 0.16 | 0.22 | JME | 1959 | 1987 |
| Barnett and Sakellaris (1998) | 0.20 | 0.24 | JME | 1959 | 1987 |
| Gomes (2001) | 0.15 | 0.14 | AER | 1979 | 1998 |
| Malmendier and Tate (2005) | 0.23 | 0.25 | JF | 1980 | 1994 |
| Cooper and Haltiwanger (2006) | 0.12 | 0.34 | RESTUD | 1972 | 1988 |
| Polk and Sapienza (2009) | 0.26 | 0.30 | RFS | 1963 | 2000 |
| Almeida et al. (2010) | 0.20 | 0.13 | RFS | 1970 | 2005 |
| Belo and Lin (2012) | 0.29 | 0.27 | JPE | 1965 | 2009 |
| Foucault and Fresard (2014) | 0.39 | 0.47 | JFE | 1996 | 2008 |
| Bustamante (2015) | 0.36 | 0.52 | RFS | 1968 | 2008 |
| Murray and Shen (2016) | 0.31 | 0.38 | JFE | 1955 | 2011 |
| Bustamante (1998) | 0.20 | 0.14 | JFQA | 1980 | 2014 |
| Wu (2018) | 0.10 | 0.07 | RFS | 1992 | 2011 |
| Clementi and Palazzo (2019) | 0.16 | 0.29 | JF | 1975 | 2016 |
| Goncalves et al. (2019) | 0.36 | 0.44 | RFS | 1967 | 2016 |
| Bai et al. (2019) | 0.38 | 0.58 | WP | 1962 | 2018 |
| Bianco and Gamba (2019) | 0.13 | 0.13 | JCFS | 1969 | 2014 |
| | | | | | |
| Average | 0.23 | 0.29 | | | |
| Std Dev | 0.10 | 0.15 | | | |

the population's.

When we measure the business sector's aggregate capital using the current-cost net stock of private non-residential fixed assets[4] and gross investment using private nonresidential fixed investment,[5] we obtain an average investment rate of about 11% over the period 1947-2018. This value is only slightly greater than commonly accepted estimates of the economy-wide investment rate.

Dividing consumption of private nonresidential fixed capital[6] in year $t$ by the capital stock in year $t-1$, we obtain an average depreciation rate over the same period of about 8% per year, versus an estimate by BLXZ of 21%.

Such a high mean depreciation rate obtains from BLXZ's assumption that economic depreciation equals accounting depreciation. It is well known that accounting depreciation consistently overestimates economic depreciation. For example, Kim and Moore (1988) find that for a sample of Canadian firms *"The estimated economic depreciation, which is a function of the rate of utilization and level of maintenance, is about half of that used according to tax (accounting) depreciation."*.

To get a different sense of how implausible a 21% average depreciation rate may be, consider that in a recent study, Li and Hall (2020) estimated that semiconductor and communication equipment – arguably the fastest-depreciating fixed assets – depreciate at an annual rate of 22.6% and 19.2%, respectively (See their Table 2). However, a large portion of property, plant and equipment of U.S. publicly traded companies consists of structures, whose depreciation is much slower. A recent study by Bokhari and Geltner (2019), for example, finds that in the United States the average gross depreciation rate of commercial structures is about 6.6%, while the gross depreciation rate of commercial properties is about 3.4%.

[4] Data source: https://fred.stlouisfed.org/series/K1NTOTL1ES000

[5] Source: https://fred.stlouisfed.org/series/PNFIA

[6] Source:https://fred.stlouisfed.org/series/A753RC1A027NBEA

## On Irreversibility and Risk

It is well known from early contributions by Carlson et al. (2004) and Zhang (2005) that investment irreversibility is necessary for the neoclassical model investment to produce a sizeable risk-based value premium. For example, in Clementi and Palazzo (2019) – see Table C1 in page 318 – we show that, when parameterized to hit the equity premium, the ergodic distribution of capital in Zhang (2005) exhibits almost no variation in capital. Since it is prohibitively costly to disinvest, firms do not invest either.

The main contributions of Clementi and Palazzo (2019) consist in showing that (i) when calibrated to match key investment moments, in fact the neoclassical model does not even get close to match typical estimates of the value premium and (ii) this claim extends to a rather wide class of one-factor models.

The model considered in BLXZ is essentially the same as in Zhang (2005). The capital adjustment cost is given by

$$H(I_{it}, K_{it}) = \frac{\theta_t}{2}\left(\frac{I_{it}}{K_{it}}\right)^2 K_t,$$

where $\theta_t = \theta^+$ if $I_{it} \geq 0$ and $\theta_t = \theta^-$ if $I_{it} < 0$. BLXZ report point estimates of $\theta^- = 508.18$ and $\theta^+ = 0.63$, respectively – a three-orders of magnitude difference. As a comparison, Zhang (2005) – citing evidence in Hall (2001) – set $\theta^-/\theta^+ = 10$.

How does BLXZ produce a sizeable volatility of investment rates when Zhang (2005) could not? The answer is: Large depreciation rates and high volatility of idiosyncratic productivity. In BLXZ, capital depreciates at the rate of 20% per year. Since $\theta^+$ is close to zero, large gross positive investment occurs every time productivity turns out to be high. On the other hand, when productivity is low, gross investment is identically zero.

In BLXZ, the conditional volatility of idiosyncratic productivity is 0.158, a value 58% larger than the one in Zhang (2005). The implied annual conditional volatility is about 0.5, which is way larger than 0.118 and 0.123, the estimates provided by Hennessy and Whited (2007) and Hennessy and Whited (2005), respectively. More recently Catherine et al. (2018) estimate the volatility parameter to be between 0.109 and 0.144 (see their Table 3), depending on the model's specification.

Given the kink in the adjustment cost function (i.e. step in the marginal adjustment cost of capital), it is not surprising that the fraction of inactive firms is 49.13%, as opposed to their own estimates of 1.46%. Essentially, in the ergodic distribution, half of the productivity realizations lead to zero gross investment.

## Conclusion

Clementi and Palazzo (2019) shows that in the one-factor production-based asset pricing model, when the frictions to capital adjustment are shaped to respect the evidence on investment, the model-generated cross-sectional dispersion of returns is only a small fraction of that documented in the data. None of the assertions in BLXZ cast any doubts on the validity of that claim.

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