Does capital market myopia affect plant productivity? Evidence from "going private" transactions*

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Abstract

One influential criticism of the stock market oriented U.S. financial system is that its excessive focus on short term quarterly earnings forces public firms to behave in a myopic manner. We hypothesize that if capital markets pressure listed firms to be myopic in a way that impacts efficiency, then going private (when myopia is eliminated) should cause U.S. firms to improve their establishment level productivity relative to a peer control groups of firms. We find no evidence that this is the case. Our key finding is that while there is evidence for substantial within-establishment increases in productivity after going private, there is little evidence of difference-in-differences efficiency gains relative to peer groups of establishments constructed to control for industry, age, size at the time of going private, and the endogeneity of the going private decision effects. Also, we do not find evidence that myopic markets lead to under-investment at the establishment level. On the contrary, we find that after going private, firms shrink capital and employment, and close plants more quickly, relative to peer groups. Our findings cast doubt on the view that public markets cause listed firms to make sub-optimal, productivity-decreasing choices, or under-invest at the establishment level.

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1 Introduction

An influential criticism of the stock market oriented U.S. financial system is that it provides strong incentives to corporate managers to behave in a myopic manner. Porter (1992) argues that the U.S. system first and foremost advances the goals of shareholders interested in near-term appreciation of their shares even at the expense of the long-term performance of American companies. In September 2009, prominent business leaders such as John Bogle (of Vanguard), Warren Buffett (of Berkshire Hathaway) and Lou Gerstner (ex-CEO, IBM) joined with the Aspen institute in a call to end the focus on value-destroying short-termism in U.S. financial markets.[1] The nexus of stock market analysts, traders and fund managers with their excessive focus on quarterly earnings and other short-term metrics is thought to harm the interests of shareholders seeking long-term growth and sustainable earnings. In turn, if managers and boards pursue strategies simply to satisfy those short-term investors they may unwittingly put a corporation's future at risk.

Perhaps the most widely cited evidence in favor of these claims come from a survey of Corporate CFOs published in Graham, Harvey and Rajagopal (2005). In this survey, the authors find that managers would rather take economic actions that would have negative long term consequences and sacrifice value in order to meet short term quarterly earnings benchmarks. However, though they can be very informative, surveys measure beliefs, which may not always coincide with the actions of managers. Indeed, Shleifer and Vishny (1997) in their review of corporate governance conclude that the theories and arguments in favor of the view that U.S. companies are relatively short sighted are remarkably short of empirical support. The goal of this paper is to develop direct evidence that either confirms or refutes this view.

One common solution advocated to halt the encumbrance of meeting quarterly results and the short term focus imposed on managers by financial markets is to delist the companies and take them private. In February 2010, Burlington Northern Santa Fe, North America's second largest railroad company prepared to delist from the public markets following its takeover by Warren Buffet's Berkshire Hathaway in a deal valued at U.S. $44 billion. Matthew Rose, chief executive of Burlington Northern Santa Fe, said it could be frustrating dealing with quarterly earnings announcements when the company's investments were far longer term and criticized the market's excessive focus on short-term results and the lack of a long term perspective. He further suggested that the management team will always be very performance-driven and looking to put up great numbers in terms of running these assets but by going private will now be freed from worrying about an individual quarter.[2]

[1]Source: Overcoming Short-termism: A Call for a More Responsible Approach to Investment and Business Management, Aspen Institute dated September 9, 2009.

[2]Source: Financial Times, February 12, 2010

To investigate if the stock market induces myopia, we examine changes in the plant-level efficiency of firms that opted out of public markets by going private, and contrast these firms' plants against a peer group of 'matched' plants. The starting point of our paper is the following hypothesis: if U.S. private firms/managers are less myopic than U.S. public firms/managers because of the absence of pressure from public markets, then episodes of going private transactions should cause firms and their establishments to improve their productivity relative to peers. We find no evidence that this is the case. Our key finding is that while there is evidence of substantial within establishment (before-after going private) increases in productivity of about 3% to 6%, there is little evidence of difference-in-differences efficiency gains relative to a peer group constructed to control for industry, age and size at the time of going private. These results continue to hold when we address endogeneity concerns about the going private decision by creating a "propensity to go private" matched control group using information of all the firms at the time of their IPO (which is on average 13 years before the going private decision). Bharath and Dittmar (2010) show that firm characteristics at the time of the IPO predict the ultimate decision to go private with a 71% accuracy rate. The propensity matched results are similar to those from using the industry-age-size matched control group. However, because the peer group in this test consists only of publicly listed firms, these results show that private firm establishments supposedly freed from the tyranny of "short termism" perform no better than their public counterparts. One may argue that the pressures of a myopic market have the strongest effect on very long term investment. For instance, if financial market pressures lead to managers not investing optimally in R&D expenditures, we expect these to ultimately show up in our productivity differences tests at least towards the latter part of the 6 year post going-private period included in our analysis. None of our findings suggest that operational efficiency of establishments that went private are differentially enhanced even six years after going private, thus casting doubt on the view that the U.S. stock market's excessive focus on short term results is affecting their long-run performance.

Most models of market myopia (Stein (1989)) and also some empirical work (Bhojraj et. al.(2009)), suggest that stock market short-termism could lead managers to boost current earnings, at the expense of forgoing longer term investments. To test this implication of myopic behavior, we examine establishment-level capital stock, employment and plant closures. If market myopia leads to under-investment when the firms are listed, we would expect to see a relative increase in capital stock, commensurate increases in employment, and a greater patience with (potentially short-term) under-performance and hence a relatively lower propensity to shutdown plants, after going private. We find no evidence that firms increase establishment-level investment after going private. In fact, relative to the two control groups discussed above, we find that if anything, firms shrink in terms of capital stock and employment after going private. We then examine the exit propensity of establishments after going private and find that going private firms close plants more quickly than the industry-age-initial size matched control group (particularly in the short-run after going private). These results are largely robust to using the propensity (to go private) matched control group of public firms. Taken together, the results on establishment level capital stock and exit propensity do not suggest either under-investment by going-private firms while they are public relative to after going private. Some models of market myopia (Bebchuk and Stole (1993) and Bizjak, Brickley and Coles (1993)) predict "overinvestment" by public firms. While we do find that firms appear to downsize after going private, the fact that we find no DID (difference-in-differences) changes in productivity between the public and private firm establishments suggests that myopia, if any, is not impacting the operational performance of listed firms. In other words, the productivity results rule out "under" or "over" investment induced by market myopia insofar as these terms are defined in the context of optimal operational efficiency.

Since there are multiple ways a firm can go private, our sample includes transactions that are driven by private equity firms, management, and private operating firms. In the main analysis, we treat these deals uniformly. However, it is possible that the productivity dynamics differ by the parties involved in the transaction. To evaluate this possibility, we classify transactions into sub-groups by collecting data on the parties that drive each of these transactions, and compare changes in these groups relative to a industry-age-initial size matched control group of establishments. There is very weak evidence of a long run increase in productivity of establishments acquired by private operating firms compared to the control establishments.[3] Otherwise, changes in productivity of establishments of all going private groups are no different than the control group of establishments. Further, all three groups cut capital sharply (between 10% to 20%) after going private, which is statistically significant at the 1% level. Firms taken private by private equity firms and/or management also experience a statistically significant drop in employment between 6% to 7% lower than control firms. In terms of exit decisions, firms taken private by operating and private equity firms have 25% to 30% increased hazards of plant exit relative to controls, while there is no effect observed in the management buy out deals. We find some evidence of skill by private equity firms in that they are able to successfully and differentially target establishments with low labor and total factor productivity for closure. We find that the private operating firms are also more likely to close establishments with low labor productivity. Similar to the earlier results, these results do not support the hypothesis that public market myopia lead firms to under invest or operate less efficiently. However, these results do shed light on the similarities and differences based on who takes the firm private.

We undertook a number of checks and additional analysis (see discussion in Section 6). We

[3]Productivity of establishments acquired by private operating firms is about 3.1% higher with a p-value of 0.072. However, this result which uses Solow TFP as the productivity measure is not robust and disappears using other methods of calculating TFP. For instance, the OLS method estimates a productivity increase of 1.7%, however, only with a p value of 0.319.

analyzed outcomes at the acquirer firm to check if changes here offset, and hence affect the interpretation of, results for the going private establishments. In particular, we examined and found no difference-in-differences increases in capital or employment, or productivity measures at acquirer firm establishments, and also no increase in new establishment openings at acquirer firms. Thus, we conclude that there is no evidence for changes at the acquirer firm establishments offsetting (or complicating interpretation of) changes documented at the going-private establishments. Other key checks address concerns relating to possible measurement error in productivity measures, explore changes in outcomes over different time periods, investigate motives behind establishment closures, and extend analysis of employment and exit propensity to non-manufacturing establishments.

The remainder of the paper is organized as follows. We describe our hypotheses and related literature in Section 2. Section 3 describes the data and productivity measures. In Section 4 we discuss methodology and present results on productivity changes. Section 5 discusses the analyses and results from examining other outcomes (capital, employment, and plant exit). Section 6 discusses the results and related additional robustness checks. Section 7 concludes.

2 Myopic nature of managers, the going-private decision, and efficiency implications

Most theoretical models that focus on the myopic nature of managers (especially in the U.S.) are driven by asymmetric information where the firm's managers typically have better information than the market. Stein (1989) shows that in such a setting, even with a perfectly rational stock market, managers are cornered into myopic behavior. The key problem is that if short-run earnings are poor, the market is unable to determine whether this is caused by poor management or by prudent long term investments, resulting in underinvestment. The market partly uses current earnings to forecast future earnings and knowing this, the managers attempt to manipulate the information available to shareholders by increasing current earnings, potentially at the expense of long-term investments, resulting in underinvestment. Even if the markets were to assume no myopia on the part of managers, the latter have an incentive to deviate from no myopic behavior in order to fool the market by manipulating current earnings. Manipulation in earnings could be accounting actions or even real actions (such as forgoing investments). Bebchuk and Stole (1993) and Bizjak, Brickley and Coles (1993) point out that the relationship between managerial short-term objectives, imperfect information and firm investment behavior can result in either under- or over-investment and depends on the observability of investment. For our purposes, the key implication of these models is that the capital market's obsession with meeting short-term expectations, coupled with asymmetric information problems, too often hinders corporate managers from focusing on long-term value creation. Severing the link between capital markets and firms (by going private) will therefore solve the managerial myopia problem. We should then expect to see a (long term) increase in productivity due to managers focussing on the value enhancing long term projects.[4]

Both survey and empirical evidence seem to be consistent with the view of managerial myopia induced by capital markets. Graham, Harvey and Rajagopal (2005) report in a survey of more than 400 financial executives, that 80 percent of the respondents indicated that they would decrease discretionary spending on such areas as research and development, advertising, maintenance, and hiring in order to meet short-term earnings targets and more than 55 percent said they would delay new projects, even if it meant a small sacrifice in value creation. Assuming these survey responses reflect the actual intent and behavior of executives, these results indicate that myopia is a larger issue than companies simply using accounting actions to meet quarterly earnings expectations from financial markets. There are also real actions such as asset sales and forgone strategic investments that corporate managers use to meet the forecasted quarterly earnings number. In a related empirical study, Bhojraj et al (2009) document that using accruals or discretionary expenditures (such as R&D expenditure) to meet or beat analyst forecasts results in short-term positive impact on firm performance, but long-term underperformance relative to firms that do not manage earnings to meet forecasts. These results confirm managerial myopia due to capital market pressures in an empirical setting if one further assumes that firms that manage earnings are most likely to engage in myopic behavior relative to their control sample that do not manage earnings. Although the creation of long-term company value is widely accepted as management's primary responsibility, these results suggest that managing predominantly for the market's short-term earnings expectations often impairs a manager's ability to deliver value. Our study contributes to this literature by examining if there are operational improvements to be obtained by removing the hypothesized myopic pressures imposed by capital markets; thus, comparing firms when they are public to when they are private.

Our paper is also related to the long literature that examines productivity changes around corporate events. Lichtenberg and Siegel (1991) find that TFP (total factor productivity) increases after a LBO (Leveraged buy out) using a sample of 131 firms that conducted an LBO in 1983-1986. Our sample of firms includes LBOs and MBOs but has a very large number of private operating firms buying public firms and taking them private. Davis et. al. (2008a and 2008b) study changes after a private equity deal and point out that productivity and employment relationships uncovered in earlier studies may not hold because of the tremendous changes in the private equity industry due

[4]However, Carmel (2008) argues that the myopia findings in models relative to a first-best standard that ignore risk aversion are often reversed when evaluated relative to the relevant standard of optimal contracting. He further shows that results purported to be myopia in the previous literature often are not and instead have excessive emphasis on the long-term. He also solves in closed-form, for the region in parameter space which gives rise to the reversal of findings, and shows that this region can be arbitrarily large.

to increased competition for transactions. They also observe that fundraising (inflation-adjusted dollars) by U.S. private equity groups is 100 times greater in 2006 than in 1985 and is a primary driver of these changes. Davis et al (2008a and 2008b) focus on the entire universe of private equity firm deals, the vast majority of which are private to private transactions. Alternatively, our study focuses only on public-to-private transactions of manufacturing firms, and studies TFP and employment changes. Based on public 13E-3 filings with the SEC, the public to private deals account for only about 157 out of more than 5,000 deals done by private equity firms from 1980-2005. Thus, we examine the impact of market myopia by focusing on going private deals and they investigate the role of private equity. Kaplan (1989a, 1989b, and 1991) also examines the benefits of going private using a sample of LBOs and highlights the importance of tax and incentive improvements due to the high leverage in these transactions. Maksimovic, Phillips and Prabhala (2009) examine productivity changes and purchase decisions by acquirers after mergers and acquisitions to understand how firms redraw their boundaries. Maksimovic and Phillips (2008) find that plants acquired by conglomerate firms increase in productivity and conclude that organizational forms' comparative advantages differ across industry conditions.

3 Data and productivity measures

3.1 Data sources and description

The main sources of data used in this study are the Census of Manufactures (CMF) for the years 1977, 1982, 1987, 1992, 1997 and 2002, and the Annual Survey of Manufactures for inter-census years from 1978 to 2004. This data has been used in previous studies, particularly to study the effects of mergers and acquisitions on productivity. While even early users of census micro data examined the issues related to ownership change and firm performance, prominent recent work include by Lichtenberg (1992) and McGuckin and Nguyen (1995). These studies found that acquired establishments enjoyed above average productivity growth for several years following a change in ownership. Lichtenberg and Siegel (1991) showed that certain types of mergers (leveraged buyouts and management buyouts) resulted in greater productivity improvement than other types of buyouts. Schoar (2002) found that diversifying firms experienced a net reduction in productivity, with the acquiring firm experiencing a decline in productivity, while the acquired plant experienced an increase in productivity.[5]

We also use the longitudinal business database (LBD) to obtain identifiers to link establishments over time, and for data used in exit analysis.[6] Technical details on the cleaning of the data,

[5]Other influential papers that have examined separate issues using Census data include Maksimovic and Phillips (2001 and 2002).

[6]We also check if the employment and exit results for manufacturing firms hold for the full universe of firms using data

as well as the detailed definitions of the key variables used in the study are provided in the data appendix. Detailed descriptions of the productivity variables are provided in the next section (with additional details provided in the data appendix).

To analyze the effect of going-private on firm productivity and other outcome measures, we use a comprehensive sample of firms that went private as detailed in Bharath and Dittmar (2010)[7]. We then match these firms to census databases using the Compustat-SSEL bridge available at the Census using 6-digit CUSIP identifers for the period 1981 to 2005 to identify all establishments owned by the sample of going private firms. In the baseline analysis we create a control sample for each establishment in the going private sample (hereafter 'going private establishment'), by including upto eight establishments (based on data availability) that are closest in size (measured using employment) to the going private establishment in the going-private year, from within the same 3-digit SIC industry, and belonging to the same age quartile.[8] Table 1 presents the summary statistics on the number of establishments in event time for a period of thirteen years with year 0 being the date of going private. Over all, we have 29,788 going private establishment year and 157,391 control firm establishment year observations in the sample. All of our analysis examines outcomes at the establishment level. While the establishment level changes provide a detailed and disaggregated picture of the effects of going-private, aggregating to the firm level is difficult here because the firm identifier for the establishments of the acquired firm will change to that of the acquiring entity, after the going-private event. Thus aggregating to the firm level would potentially conflate the specific effects in the target establishments with the effects in the establishments of the acquirer which is not our focus here. Importantly, McGuckin and Nguyen (1995) show how aggregating to the firm level could mask interesting establishment level changes at the target establishments. There are two additional reasons to focus on establishments. One, many of the firms have multiple establishments operating in multiple industries; thus, forming a suitable control group for a firm that matches the firm's industry composition is more difficult. Two, one aspect of firm behavior we specifically want to look at is the decision to shutdown particular establishments (see section 5.2). The establishment level analysis could mask improvements that occur due to selective closure of inefficient establishments; we address this separately in Section 5.2.2.

from the LBD (see point v in section 6).

[7]Bharath and Dittmar (2010) use all forms of 13e-3 filings to identify going private transactions and require that firms are no longer registered or traded (even over the counter). They also supplement their sample with data from prior studies.

[8]In section 4.3, we use an alternative control group, based on propensity score matching.

3.2 Key productivity measures

In this section, we discuss in detail a number of alternative measures used in our analysis of establishment-level productivity, as well as the variables and methodology used in their definition.

- **Labor productivity**

  Labor productivity is defined as log real value of shipments divided by employment. Value of shipments is simply the sales value deflated using 4-digit SIC industry-specific output deflators. Employment is the total number of employees reported in the ASM-CMF database.

- **Solow residual TFP measure**

  The Solow residual TFP is defined as $TFP_{it}^{Solow} = y_{it} - \beta_m m_{it} - \beta_k k_{it} - \beta_e e_{it} - \beta_n n_{it} - \beta_l l_{it}$, where y_{it} is the log of real value of shipments of establishment i in year t, m is log real materials, k is log of real depreciated capital stock, e is log of real depreciated energy costs, and n is log of white-collar (non-production) employment and l is log of blue-collar (production) employment. Employment is measured in equivalents of production worker hours, and thus adjusts for utilization.[9] The elasticities β_m, β_k, β_e, β_n and β_l are defined equal to the material share, capital share, energy share, white-collar and blue-collar share of total costs in the 4-digit SIC (1987) industry j to which firm i belongs.

- **OLS-FE TFP measure**

  A key issue in the estimation of production functions is the correlation between unobservable productivity shocks and input levels. Profit-maximizing firms respond to positive productivity shocks by expanding output, which requires additional inputs. Negative shocks lead firms to pare back output, decreasing their input usage. This endogeneity / simultaneity (Marschak and Andrews 1944) can be addressed in a variety of ways. One solution is to use panel data transformations with fixed effects assuming that factors that are correlated with input choice (e.g. quality of labor, advantages from location, entrepreneurial quality) are likely to be fixed over time. In this case, the OLS-FE productivity measure is defined as the residual from an OLS establishment-fixed-effects regression of log real value of shipments on log real materials, log real energy costs, log blue-collar employment, log white-collar employment and log real capital.

- **Levinsohn-Petrin TFP measure**

  While the use of OLS-FE estimator attempts to solve the simultaneity problem, it produces two new issues. The first is that the fixed effect transformation imposes a "strict exogeneity" requirement on the residuals rather than just being "predetermined". In other words, we

[9] Specifically, for blue collar (i.e. production worker) employment we use reported total production worker hours. For white collar employment, we divide the total white collar wage bill by the implied production worker wage rate per hour, to obtain production worker hour equivalents of white collar labor use. (Production worker wage rate per hour is obtained by dividing the blue collar wage bill by the total production worker hours.)

require the residual to be uncorrelated with all past and future realizations of the dependent variable. Given the optimizing responses of firms to these unobserved shocks, ordinary least squares (OLS) estimates of production functions are biased and, by implication, lead to biased estimates of productivity, the relevant quantity for the estimation in this context. The second issue is one of implausible parameter estimates in practice, because the within transformation of the variables due to fixed effects magnifies any noise in the data.

Olley and Pakes (1996) develop an estimator that uses investment as a proxy for these unobservable shocks. More recently, Levinsohn and Petrin (2003) (LP) pointed out that the investment proxy is only valid for plants reporting non-zero investment, and in practice this leads to loss of a substantial amount of data. Using intermediate input proxies instead of investment avoids truncating all the zero investment firms. In the census data, firms almost always report positive use of intermediate inputs like electricity or materials. LP show the conditions under which intermediate inputs can also solve this simultaneity problem and provide empirical evidence that these benefits are important. A brief description of our implementation of the LP method is provided in the data appendix.

- **Blundell-Bond system-GMM TFP measure**

  Blundell and Bond (2000) note that while the estimation of simple Cobb-Douglas production functions from plant level panel data attempt to control for unobserved heterogeneity and the simultaneity problems described above using GMM estimators have proved to be unsatisfactory. In particular, GMM estimators which take first differences to eliminate unobserved firm-specific effects and use lagged instruments to correct for simultaneity in the first-differenced equations have suffered from the weak instruments problem. Blundell and Bond (1998) show that weak instruments could cause large finite-sample biases when using the first-differenced GMM procedure to estimate autoregressive models for moderately persistent series from moderately short panels. In the context of production function applications, Blundell and Bond (2000) show that these biases could be dramatically reduced by incorporating more informative moment conditions. This involves the use of lagged first-differences as instruments for equations in levels (which are found to be informative), in addition to the usual lagged levels as instruments for equations in first-differences. Importantly, Bond and Soderbom (2005) show that the Blundell and Blond (2000) estimator addresses a critique of the LP approach put forth by Ackerberg, Caves and Frazer (2006). A brief description of the Blundell-Bond (2000) procedure used by us is provided in the data appendix.

- **Translog TFP measure**

  One drawback of the Cobb-Douglas specification for the production function used in the previous estimations is that the elasticities of output with respect to individual inputs are restricted to be constant, and the elasticity of substitution between inputs is restricted to be equal to one. As an alternative, we consider the following second order translog specification:

$$y_{it} = \sum_j \beta_j X_{it}^j + \beta_{jj}(X_{it}^j)^2 + \sum_{j \neq k} \sum_k \beta_{jk} X_{it}^j X_{it}^k + f_i + \omega_{it} \quad (1)$$

where i indexes plants, t indexes years, j and k index the different inputs. We use log of real materials, log of real energy costs, log of the real depreciated capital stock, log of the number of production (blue collar) employees and the log of the number of non-production (white collar) employees as the inputs. We use the residuals from this translog production function estimated using OLS with establishment fixed effects as TFP measures.

4 Analysis of productivity changes

When examining productivity and other outcomes, we present two sets of results. The first set of "before-after" results summarizes what happened to the key variables of interest within the establishments that belonged to firms that went private, compared to their levels prior to going private. The second set of "difference-in-differences" results presents the changes in the variables of interest relative to changes in a matched control group of establishments. While both of these results use fairly standard methodologies from the literature, we present the specifics of our approach in the following sub-sections.

4.1 The before-after methodology

To examine before-after changes, we retain data for up to 13 years for each establishment belonging to the firms that went private. These include up to 6 years of data before the year of going private, the year of the firm went private, and up to 6 years after the firm went private.[10]

We then use simple regression approaches to summarize the before-after changes in two ways. First we use the following regression specification:

$$\begin{aligned} y_{it} = \beta_{LR\_PRE}\ LR\_PRE + \beta_{SR\_PRE}\ SR\_PRE + \beta_{SR\_POST}\ SR\_POST \\ + \beta_{LR\_POST}\ LR\_POST + f_i + e_{it} \end{aligned} \quad (2)$$

where y_{it} stands for the dependent variable (productivity or other measures), f_i stands for plant fixed effects, and the four dummy variables are defined to capture four distinct time periods as follows: (i) <u>The long-run before going private</u>: LR_PRE is a dummy equal to one for the 3-year period from 6 to 4 years before going private and zero otherwise; (ii) <u>The short-run before going private:</u> SR_PRE is a dummy equal to one for the 3-year period from 3 to 1 years before going private and zero otherwise; (iii) <u>The short-run after going private:</u> SR_POST is a dummy equal

[10]Note that there may be some establishments that were born less than 6 years before the firm went private, and some establishments that exit less than 6 years after the going-private event.

to one for the 4-year period from 0 to 3 years after going private and zero otherwise; and (iv) The long-run after going private: LR_POST is a dummy equal to one for the 3-year period from 4 to 6 years after going private and zero otherwise. The term e_{it} stands for residual error.

The estimates of interest are not the levels of the dependent variables, but rather their changes over time.[11] Specifically, we are interested in the following changes:

(i). Short-run post- versus short-run pre- going private ($\beta_{SR\_POST} - \beta_{SR\_PRE}$): This provides an estimate of the changes in the dependent variable in the short-run after going private, relative to the period just before the going private event. Thus, if the new owners take steps that have immediate effects on the performance of the plant, this should be reflected in this estimate.

(ii). Long-run post- versus short-run pre- going private ($\beta_{LR\_POST} - \beta_{SR\_PRE}$): This provides an estimate of the changes in the dependent variable in the long-run after going private, relative to the period just before the going private event. If the actions of the new owners take some time to have an impact, we may obtain significant estimates here, but not in (i) above.

(iii). Test for prior trend ($\beta_{SR\_PRE} - \beta_{LR\_PRE}$): This provides an estimate of trends in the dependent variable prior to going private. If the establishment was experiencing an increasing (decreasing) trend in the dependent variable, this would manifest as a positive (negative) estimate in this test. Thus, any changes we document in (i) or (ii) above, should be evaluated in the context of the pre-existing trend captured by the estimate here.

Table 2 presents the results of this analysis. For all inferences we compute p-values based on standard errors clustered by establishments. The first column regresses labor productivity while columns 2 through 6 measure TFP (total factor productivity) according to the various methods described in section 3.2. We find that there is a pre-existing improving trend in all the productivity variables (between 3% and 10%) prior to the going private decision. Labor productivity and TFP increase both in the short run (by 6.3% and about 3%, respectively) and the long run (by 7.2% and about 5% respectively) after going private and these differences are statistically significant. The only exception is the Levinsohn-Petrin TFP which increases but this increase is insignificant. However the magnitude of the post going-private changes do not suggest any acceleration relative to the pre-existing trend.

In the above analysis, the year in which the firm goes private is included in the SR_POST dummy as part of the short-run post-going private period. Whether the going private year should

[11] The inclusion of the plant fixed effects implies that one of the time period dummies is not identified. However, our estimation procedure reports the mean for the omitted LR_PRE as the constant term.

be considered part of the post- (and not the pre-) going private period is unclear, but this choice should not have a large impact, as the estimate averages the effects for four years. Nevertheless, in order to allow for a more flexible examination of the year-by-year effects, we examine a standard event study graph, by plotting coefficients on the index dummies from the following regression specification.

$$y_{it} = \sum_{k=-6}^{6} \beta_k D_k + f_i + e_{it} \tag{3}$$

where k indexes the years after the going private event, and correspondingly D_k is a dummy variable equal to one for the year k after the going private event. (Negative values of k correspond to years before going private.) All other variables are as in (2) above. We then plot the β_k coefficients as well as the corresponding confidence intervals, to illustrate the trends in the dependent variable, and the significance of the changes in the trends. Figure 1 shows that there is a statistically significant increase in the productivity measures for the establishments after the firm goes private, for all six of the measures. The improvement appears to be reversed in the LP TFP measure in the longer term but not for the other measures. Further, consistent with the results in Table 3, there appears to be a strong pre-exiting increasing trend in almost all of the measures, though the figure suggest a short-run slowing down of productivity before going private and a short-run acceleration after going private.

4.2 The difference-in-differences methodology

The before-after analysis simply summarizes the trends in the variables of interest in the plants belonging to the firms that went private. However, these changes could simply be driven by factors specific to the industry, or age-related changes (as plants are increasing in age over the period of our analysis). Changes may also be driven by factors related to the initial size of the establishment, e.g. if going-private firms' establishments were relatively large, and if all large establishments experienced relatively different patterns of productivity change.

In order to rigorously address potential bias from these industry, age and initial size related factors, we form a matched control group for each establishment in the going-private sample. Specifically, for each establishment in the going-private sample, we select up to eight matched control establishments in the following way. Using the data for the closest prior-to-going-private year in which the establishment is observed in the ASM-CMF sample, we classify all establishments into 3-digit industry-age quartile groups. Then, we sort by employment within each industry-age quartile group; and we select up to four non-going-private establishments just lower and up to four non-going-private establishments just greater in size than the going-private establishment, for each going-private establishment. There are not always 8 matched controls in cases where the going-private establishment was too close to the largest or smallest establishment within the industry-age quartile. For a very small sample (less than 3%) of establishments, control groups overlap. We dropped all such control group establishments from our analysis, so that control groups are unique to each going-private establishment.

This procedure generates non-overlapping 'cells', with one going-private establishment and up to eight control establishments. We then estimate the following regression specification:

$$y_{ijt} = \beta_0 + \beta_{LR\_PRE}\ LR\_PRE + \beta_{SR\_PRE}\ SR\_PRE + \beta_{SR\_POST}\ SR\_POST + \beta_{LR\_POST}\ LR\_POST + D_{jt} + e_{ijt} \tag{4}$$

where i refers to the plant, j refers to the cell that plant i belongs to, D_{jt} refers to cell-year fixed effect, and the other variables are as defined in the before-after specification (2) above. Note that period dummy variables are defined only for the going private sample – for instance, LR_PRE is a dummy defined equal to one for going private establishments in the 3-year period from 6 to 4 years before going private (zero otherwise). Thus, the intercept term (β_0) captures the overall mean value for the control group of establishments. The inclusion of the cell-year dummies (D_{jt}) implies that the coefficients on the period dummy are estimates of the differences between the going private establishments and the control establishments, for that period.

Accordingly, the differences between the period dummies yield difference-in-differences estimates that control for cell-year, or equivalently, industry-age-size-year effects (where size refers to the pre-going-private size of the establishment). As before, we are interested in the following three estimates, but defined as a difference from the control group:

(i). Short-run post- versus short-run pre- going private ($\beta_{SR\_POST} - \beta_{SR\_PRE}$): This provides an estimate of the changes in differences between the going-private and control group in the short-run post-going private period, relative to the period just before the going private event. If both the going-private as well as their matched controls experienced similar changes in the dependent variable, there would be no changes in the difference between the treated (i.e. going-private) and the control group. The way the control groups are constructed, controls for any effects related to industry-wide changes, or age-related changes or initial size related changes (or any combination of these) are accounted for. In particular, industry-age-size specific year effects are controlled for in this estimation.

(ii). Long-run post- versus short-run pre- going private ($\beta_{LR\_POST} - \beta_{SR\_PRE}$): This provides an estimate of the changes in differences between the going-private and control group in the long-run post-going private period, relative to the period just before the going private event. As explained in (i) above, industry-age-size specific year effects are controlled for in this estimation.

(iii). Test for prior trend ($\beta_{SR\_PRE} - \beta_{LR\_PRE}$): This test examines if the difference between the going-private and the control firms was increasing or decreasing in the pre-going private period. Though we match on age and size characteristics just prior to the going private event, differences between the going private and control groups could exhibit specific trends in the prior period. One particular concern would be that, relative to this matched control group, the efficiency levels of the going-private establishments may have been on an up-trend; that is, the going-private establishments may have been selected based on prior trends. Then, any post-going-private changes may simply be a reflection of these relative trends. Therefore, this test helps to establish whether differential trends in the dependent variable may have been a basis for selection (and hence a source for biasing estimated difference-in-differences changes).

Table 3 presents the results of this analysis. For all inferences, we compute p-values based on standard errors clustered by industry-size-age groups. As in Table 2, the first column regresses labor productivity while columns 2 through 6 examine TFP (total factor productivity) according to the various methods described in section 3.2. We note three important results from this table. First, we find that there is no pre-existing improving trend in any of the productivity variables prior to the going private decision *relative to* the control establishments included in the regression. This suggests that the earlier result of a trend in productivity for the private firms is also mimicked by the control group of establishments, perhaps mirroring industry (or age or initial size) related trends. Second, there is no evidence of a short-run increase in productivity variables for the going private establishments in a difference-in-differences sense when compared with the control group. This indicates that while private firm establishments do have a short-run increase in productivity (Table 2) after exiting the public markets, so do the control group establishments that do not change their public-private status. Thus, we do not see any evidence of the pressures due to the short sighted behavior by the public markets which was postulated to be a drag on their productivity. Indeed, if there were such pressures, the exit from the public markets would have made the private firm establishments see large productive improvements relative to the control group, which do not have any firms undergoing a similar change. Third, we do not find any evidence of a long-run productivity increase in private firm establishments, relative to the control sample. These results question the commonly held belief that the U.S. stock market by its excessive focus on short-term earnings imposes myopic behavior on part of the firm to meet such expectations.

Again, as in the case of the before-after analysis, we also examine a difference-in-differences event study graph, by plotting coefficients on the index dummies from the following regression specification.

$$y_{ijt} = \beta_0 + \sum_{k=-6}^{6} \beta_k D_k + D_{jt} + e_{ijt} \tag{5}$$

where k indexes the years after the going private event, and correspondingly D_k is a dummy variable equal to one for the year k after the going private event, defined only for going-private establishments. Thus, as in specification (4) above, the intercept term captures the overall mean for the control group, and the dummy coefficients (the β_ks) capture the difference between the going-private establishments and their control groups in that index year. The D_{jt} fixed effects are cell-year effects as in (4) above. We then plot (in Figure 2) the β_k coefficients as well as the corresponding confidence intervals, to illustrate the trends in the mean (and standard errors) of the difference between the going-private and control groups.

Figure 2 confirms the results in Table 3. First, the pre-going private trend is flat, confirming that the pre-going private productivity trends are similar for going private group and the control group.[12] Second, there is no statistically significant short-run or long-run improvement in relative productivity for the going-private establishments, compared to the pre-going private productivity levels.

Going private may still have had positive productivity consequences if it was the case that these firms were headed for a relative decline in productivity. In other words, could it be that going private enabled these establishments to match the performance of the control establishments, whereas without that they would have performed relatively worse? We see no evidence for this possibility in Figures 1 and 2. First, Figure 1 shows strong improving trends in all productivity measures for the going-private establishments; so the prior absolute productivity trends do not portend any coming distress on the productivity front. Second, none of the productivity measures in Figure 2 show any significant dip prior to going private; in fact, the trends are remarkably flat for most of the measures from years -2 to 0. Thus, there is no hint that without going private, the going private establishments would have suffered declines in productivity.

4.3 Addressing endogeneity of the going private decision and related selection-bias

One potential concern in our study is that the decision to go private is not random and thus it is important that we control for the endogeneity of the going private decision as it may impact productivity changes. In particular, if the choice of firms to go private were based on some characteristics that predict future improvements in productivity, then the before-after results in section 4.1 are biased by this endogenous selection of going private firms. The difference-in-differences approach in section 4.2 controls for this issue, if the key drivers of future productivity changes are related to one (or a combination of) industry, age, or plant size related factors. The fact that the productivity measures for the going private establishments show increasing trends pre-going private when examined on their own (as shown in Figure 1), but not relative to the industry-age-size

[12] This also suggests that industry-age-initial size may be a good combination of characteristics to match on, as the trends within establishments matched on these are similar in the 'pre-treatment', i.e. pre-going private, period.

matched control group (as shown in Figure 2) suggests that industry, age, and/or firm size-specific factors may indeed be the most important ones to control for correcting potential selection bias.

Nevertheless, in this section we check robustness of the results to an alternative approach to constructing the control group. In particular, we utilize the results in Bharath and Dittmar (2010) to construct a propensity score matched control sample. By matching on the propensity score, we test whether the establishments that went private show an improvement over-and-above the improvement exhibited by firms that had a similar probability of being selected into going private treatment (Rosenbaum and Rubin 1985). Bharath and Dittmar (2010) find that despite the fact that, on average, the private sample firms remain in the public market for over thirteen years, firms that ultimately go private are very different and discernable in information and liquidity considerations, relative to firms that remain public, throughout their public life and even *at the time of the IPO*. They estimate a logit model using explanatory variables only at the year following the IPO to predict if a firm will ultimately go private. The results are striking. They find that firms that are more likely to ultimately go private have less analyst coverage, less institutional holdings, more concentrated ownership, and more mutual fund ownership at the time of the IPO compared to firms that remain public, supporting the importance of information considerations in the choice between being public or private. They also find that firms that go private are more illiquid and have less share turnover, supporting the importance of liquidity issues. Using a ROC analysis, they show that the logit model has a 71% accuracy compared to a bench mark of 50% accuracy with a random guess, reflecting substantial improvement over a naive model.

Motivated by these results, we construct a sample of the closest propensity (to go private) score matched firm(s) that did not go private for each establishment in the going private sample, and use their establishments as controls. We use the firm specific control variables at the time of the IPO to estimate the propensity to go private as in Bharath and Dittmar (2010).).[13] Since these control variables are not available for all firms in our sample, the number of establishments of firms that went private drops from 28,518 in Table 2 to 22,488 in these estimations.[14] Similar to the approach in section 4.2, we include industry-propensity cell-year fixed effects in each regression.[15]

[13]Specifically, we use the estimate from the analysis in column 2 of Table 7 of Bharath and Dittmar (2010) to estimate the propensity to go private.

[14]One noteworthy attribute of this control group (and another reason for the drop in the number of observations) is that all of the firms in the control group are also listed firms, as the propensity model in Bharath and Dittmar (2010) uses stock market related variables. In section 4.2, the control group was establishments of all non-going private firms, which include both listed and unlisted firms.

[15]Here a 'cell' refers to the unique (i.e., non-overlapping) group of establishments comprising one going private establishment and the control group of establishments matched (based on propensity score and industry) to this going private establishment. Then for each of the cells we include cell-year fixed effects in the regressions. Accordingly, as in section 4.2, the estimated effects are the mean of the relative difference between each going-private establishment and its matched

For all inferences we compute p-values based on standard errors clustered by industry-propensity score cells in the table.

The results in Table 4 are qualitatively identical to Table 3. We find no long-run or short-run differential increases in productivity for the going private firm establishments over the public firm establishments even after controlling for the endogenous choice of firms to go private. Also, even relative to this control group, there is no evidence of a statistically significant pre-existing trend in productivity, suggesting that the control establishments were also experiencing improvements in productivity similar to that of the pre-existing trend for the going-private sample (Table 2).

5 Analysis of other outcomes

The previous results establish the fact that while labor and TFP productivity improves for the private firm establishments, it does not improve differentially compared to the public firm establishments. By implication, this analysis strongly suggests that there is no evidence for either 'over' or 'under' investment in listed firms, as sub-optimal investments should result in a negative effect on operational efficiency (and hence lead to improvements in productivity post-going private).

Nevertheless, it is interesting and informative to examine investment directly and thus in this section we examine the change in investment by studying two other sets of outcomes. First, we look at capital and employment changes in establishments belonging to going-private firms. If the capital market's short-term outlook forces listed firms to sacrifice long term growth by reducing investment and limiting expansions, we should expect to see greater investment and corresponding employment expansion at the establishment level after firm's go private.

Second, we examine a firm's propensity to close plants. Again, if market short-termism causes firms to underinvest it is likely to lead to faster closing down of plants in order to improve short-term profits, possibly at the expense of long term investment. In other words, if the market's impatience with short-term poor performance of some new projects or establishments was indeed the hindrance motivating the going-private decision, we would expect to see a greater nurturing of investment in plants after going private, and correspondingly, we could expect to see a lower probability of shutting down plants after going private.

5.1 Analysis of capital and employment

In this section, we examine the capital and employment choices around the going private decision. The regression specifications are identical to that in section 4, except for the change in depen-

control group.

dent variables. Table 5 presents the regression results and Figure 3 summarizes the coefficients in the regression with the confidence intervals. We find that while log deflated capital for the private establishments increase by 2.5% (specification 1a) in the short run, they actually decline by 10% (5.2%) relative to the industry-age-size control group in specification 1b (industry-propensity matched control group in specification 1c) over this same period. Also, while in absolute terms capital increases by 6.4% in the long-run after going private, again relative to the two alternative control groups they show a large and significant decline of about 15%. We also find that there was a statistically significant upward trend in capital in the going private establishments in absolute terms (1.7% in Column 1a), but there was no statistically significant prior trend relative to the control groups (i.e., there is a large positive effect in Column 1c, but this is not statistically significant).

Log employment shows a decline in absolute terms both in the short run (5.8%) and the long term (8.9%) in specification 2a. This seems in line with a prior trend decline of 3.9%. This pattern of declines in employment is also seen relative to the industry-age-size matched control group in specification 2b. In specification 1c, while there are even larger point estimates (particularly in the longer run), these are noisier. What is noteworthy in 2c is that the prior trend in employment, relative to the propensity matched (listed) firm establishments, was positive, large and significant (at 10% level), similar to the pattern for capital (in 1c). Thus, the employment results are consistent with the capital results, and do not suggest any increased expansion of business activity within plants after going private.

Taken together, these results provide no support for an acceleration of investment after going private; thus, public markets do not lead to underinvestment. If anything, the DID results (relative to other listed, propensity score matched firms) suggest that a positive relative trend in capital and employment is reversed in a significant way after the going private event.[16] These results suggest that public markets lead firms to invest more rather than private firms. Thus, these results contradict the commonly help view that market myopia leads to underinvestment. Taken alone, the results could suggest over investment, either because of myopia (Bebchuk and Stole (1993), Bizjak et al (1993), or empire-building in the going private firms when they were listed; however, this interpretation is not consistent with difference-in-differences results showing that firms productivity does not change after going private, detailed in the previous section.

[16]In the context of the productivity results, one relevant question is why the relative downsizing on the input side in capital and employment did not translate into productivity gains; we find that sales declined in line with the decreases in capital and employment, so that the input declines were not TFP enhancing (results are available in a supplementary appendix available online).

5.2 Analysis of plant shut-down decisions

In this section, we examine if firms are more nurturing of or patient with establishments after going private. Because all of our analysis is based on examining going-private establishments that were operational in the year before going private, the analysis here will essentially examine the exit rate for gone-private establishments, relative to the control group. In other words, we will be looking at differenced means, and we will not be doing a before-after or difference-in-differences analysis, as essentially the sample conditions on no exit in the pre-going private period (so that there is no "before" period, and hence no "difference-in-differences" analysis possible.)

5.2.1 Exit (shutdown) hazard and propensity analysis

To examine the impact of how changes in establishment characteristics and the private firm status impact the probability of establishment exits, we first use a hazard model to investigate if and when a plant is closed down. Specifically, we are interested in the length of time it takes for a plant to shut down from the date of going private, and the influence of different variables on that duration, controlling for the fact that our comparison plants may also have closure decisions at some unobservable time. In the baseline case, we use the Cox proportional hazard model, using the Breslow method for ties. The model to be estimated is:

$$h(t, X) = h(t, 0) \quad exp(\beta' X)) \tag{6}$$

where h(t,X) is the hazard rate at time t for a firm with covariates X. Further, the Cox proportional hazard model does not impose any restriction on h(t,0) the base line hazard; the Cox partial likelihood estimator provides a way of estimating β without estimating h(t,0). A positive coefficient on variable x in the hazard model implies that a higher x is linked to higher hazard rate and thus a lower expected duration. The hazard ratio which is simply exp(β) tells us how much the hazard (i.e., instantaneous risk) of exit increases for a unit change in the independent variable. For robustness, we also consider a parametric model, the exponential model in which we model $h(t, 0) = exp(\beta_0)$.

In each estimation, the sample includes going private establishments and matched controls. As explained in section 4.2 and 4.3, each going private establishment is matched to a unique set control establishments (depending on data availability); each of the matched controls are assigned the same "start/birth" year as the matched going private establishment. We use time invariant characteristics to explain duration, with the main variable of interest being the going-private dummy variable. We include establishment log employment, age, 2-digit industry and year fixed effects as controls. That is, our goal is to understand whether the going private establishments have a higher hazard for exit relative to the control group, controlling for size, age, industry, and common year shocks.

The results of this analysis are presented in Table 6, Panel A. In columns 1a and 1b, the control group is composed of establishments matched on industry, age and initial size (as explained in Section 4.2). In columns 2a and 2b, the control group comprise establishments within the same 3-digit industry matched on the propensity (to go private) score (as explained in Section 4.3). Compared to the industry-age-size matched control group, we find that going private firms have a 21% (the going private dummy hazard ratio is 1.21) and statistically significant higher hazard rate of shutting down establishments. Compared to the propensity-matched control group of listed firms we find a higher hazard rate (about 3.5%), but this is statistically insignificant.

Overall these hazard rate results show no evidence of a lower exit rate for going private firms' plants and after controlling for the propensity to go private the change is not significant. The main strength of the hazard rate model is that it explicitly accounts for the sizeable right censoring that occurs in analysis such as ours (i.e., a sizeable number of establishments survive till the last year of the dataset). However, one weakness is that it is not computationally feasible to use high-dimensional cell-year fixed effects used in the previous sections. In order to check robustness to using higher dimensional fixed effects, we use a linear propensity model in Panel B.[17]

We define 2 exit variables: 2- (4-) year exit dummy is a variable that equals one if the plant exited in the next two (four) years and zero otherwise. These dummy variables are undefined (missing) for time periods after a plant is shut down. Columns 1a and 2a, present exit propensity relative to the industry-age-initial size matched control group cell, while Columns 1c and 2c, present the effects relative to the industry-propensity (to go private) matched control group cell. We find that establishments of the going-private firms have a 1.6% (1.8%) higher propensity than the industry-age-size matched control group to shut down a plant with in the next 2 (4) years, immediately after going private. These results are significantly larger in magnitude when the comparison group is industry-propensity score matched control establishments; establishments of the going-private firms have a 2.1% (2.7%) higher propensity than the industry-propensity score matched control group to shut down a plant with in the next 2 (4) years. However, in the long run, the differential is much smaller, and statistically insignificant in all cases.

These results indicate that, in the short-run after going private, relative to industry-age-size control group going private firms have a higher propensity to close down plants, confirming the hazard model results in Column 1a and 1b in panel A. The results relative to the propensity score matched sample suggest that there is a faster culling of establishments after going private in the short run but not in the longer term. Specifically, there is weak evidence that the establishments

[17]This approach has the drawback that it does not address the right censoring issue; however, our tests dropping plants that survive to the end of the data period yielded very similar results to those reported in Panel B, so we are confident that the qualitative conclusions are not biased by the right-censoring of the data.

that survive past the short-run (0-3 year period) are more likely to survive longer (as reflected in the negative coefficients on LR_POST in columns 1b and 2b of Panel B). Thus the positive but insignificant results in the hazard model results columns 2a and 2b of Panel A seem to hide an interesting and significant acceleration of plant shut-downs in the short-run.

The results from the hazard and propensity models suggest that the going private firms do not decrease and, in the short run, may accelerate plant shut downs after going private. Thus, this evidence does not support a myopia-related hypothesis that the elimination of stock market's short-term focus make it more likely for firms to nurture plants after going private.

5.2.2 Selection of plants for closure

In this subsection, we examine if the going private firms differentially target the poorly performing plants (in a labor productivity and TFP sense) for closure. We want to investigate if the stock market's short term focus leads to different types of shutdown decisions for public firms. If the stock markets are indeed myopic, we would expect to see market pressures leading to faster shut-down of worse performing plants (which may need to be nurtured to achieve greater productivity levels). Thus, we predict a less negative effect of productivity on shutdown decision after the firm goes private, as evidence of market myopia.

To test for differential targeting, we add labor productivity and TFP measures as well as an interaction term between the going private dummy and productivity, to the Cox proportional hazards model specification in Table 6 panel A. In columns 1a, 1b and 1c of table 7, the control group is composed of establishments matched on industry, age and initial size (as explained in section 4.2). In columns 2a, 2b and 2c, the control group comprise establishments within the same 3-digit industry matched on the propensity (to go private) score (as explained in section 4.3).

Two points are noteworthy about the results. First, we find that the coefficient on the TFP measures is negative and highly significant (except for column 2c), which suggests that better performing plants are less likely to be shut down by both public and private firms.[18] This suggests that productivity (as measured here) is indeed informative and guides the shutdown decisions of all firms. Second, and more importantly for our study, the coefficient on the interaction term of the productivity variables with the going private dummy is generally negative, but not statistically significant in any of the specifications. This suggests that private firms are no different from control firms in differentially targeting plants for closure based on their productivity. This finding also indicates that public firms do not seem to be unduly affected by capital market pressures to be

[18]Of course the significance of the productivity terms could be affected by the inclusion of the interaction term; we verified that the productivity terms are highly significant when the interaction term is excluded.

less patient with poorly performing plants; if anything, after going private firms appear somewhat quicker to shut down poorly performing plants.

5.3 Results by acquirer type

There are multiple ways a firm can go private, and our sample includes transactions that are driven by private equity firms, management, and private operating firms. In the main analysis, we treat these deals uniformly. However, it is possible that the productivity dynamics, as well as capital/investment and shutdown choices differ by the parties involved in the transaction. We classify the sample firms that went private into three categories: a buyout by a private operating firm, a buy out by a private equity firm, a buy out by the management. We source the classifications for these deals using news paper reports from Factiva. The residual category is unclassified. The category-types are non-exclusive, so that some deals may involve deals classified under more than one type.

Table 8 panel A shows that there are 772 establishments associated with a management buyout, 944 with a operating firm and 845 with a private equity acquirer. In panels B, C, D and E, we present comparisons relative to a control group composed of establishments matched on industry, age and initial size (as explained in Section 4.2). The same analysis using establishments within 3-digit industry propensity (to go private) score matched control group (described in Section 4.3), yield qualitatively similar results but are omitted here for brevity (and are available on request from the authors).

Table 8 panel B shows that in a DID analysis there is no difference in labor productivity compared to the industry-size-age matched control group for any type of going private deal. The OLS and Solow TFP results by acquirer type are similar to the overall results – there is no differential increase in productivity for the going private firm establishments. There is some weak evidence of a 3.2% increase in Solow TFP productivity (p-value of 7.2%) in the long run for operating firm acquirer deals. However, this result is not robust to changes in methods for calculating productivity. The long run increase using the OLS TFP for operating firm acquirers is an insignificant (p value of 31.9%) 1.7% increase. Table 8 Panel C shows (in the DID specification) that all deals are followed by a long run decline in capital employed, while both management and PE firm deals are followed by long run DID declines in employment.

Table 8 panels D shows that the increased hazards of plant closure are primarily associated with operating and private equity deals. Management buy outs are not associated with increased hazards of plant closure. Finally, tests for differential targeting in Table 8 Panel C reveals that private equity firms have skill in reliably targeting plants with low labor productivity and TFP for closure. The interaction term between the private equity dummy variable and productivity measures is negative and significant suggesting that high productivity plants associated with private equity deals are less likely to be selected for closure.

6 Other robustness checks and discussion of results

In this section, we discuss a number of robustness checks and other tests that help to validate and explain the conclusions from the baseline analyses. Results are available on request from the authors. (Some of the results from these analyses are included in a supplementary appendix available online.)

(i) Examining outcomes at the acquirer firm: The above analyses document that there was no change in productivity, an increased probability of shutdown, and relative declines in employment and capital, at the going private establishments, relative to control groups. We interpret this as evidence against myopia, as myopia would be expected to induce under- (or over-) investment relative to what is optimal for productivity. It is possible that outcomes at the acquiring entity may be different in a way that affects this interpretation. To investigate this possibility, we use firm ownership identification data in the Census LBD dataset to identify the acquirer firm (and its establishments at the time of the going private event).[19] We investigate three sets of outcomes. First, it could be the case that the acquirer firm expands operations in its establishments, offsetting the declines in inputs documented in the going private establishments. We analyzed changes in capital and employment (and sales) at the acquirer firm establishments following the same approach as used for analyzing the going private establishments (using a control group matched on industry, age and size at time of the going private event). Contrary to what would be expected if there was countervailing expansion at acquirer firm plants, we found no difference-in-differences increases in capital or employment (or sales) at the acquirer firm plants (both overall, as well as in a sample restricted to be in the industry of the target going private firm) – in fact in most cases we found significant DID declines in inputs. Second, it could be the case that the acquiring entity opens new establishments in the same industry as the plants shutdown at the target going private firm. We analyzed the propensity to open a new establishment and found that this propensity declines significantly in absolute terms at the acquirer entities, and shows no differential change relative to a control group. Third, we checked whether total factor productivity went up at acquirer establish-

[19] Firm ownership identifiers are not updated every year in the LBD, as documented by Jarmin and Miranda, 2002. Accordingly, all of the owner firm identifiers do not switch to that of the acquirer firm in the year after the going private event. To overcome this limitation within the constraints of the data, for each acquired establishment we identify the first change in firm identifier. We then define as the acquirer firm the modal new firm identifier across all the acquired plants within a going private firm.

ment (say from a transfer of clients or markets that the acquirer's plants were able to serve without increasing inputs proportionately). We found no significant DID changes in any of the six productivity measures at the acquirer firms' establishments, either in the short-term or the long-term. Thus, we conclude that there is no evidence for changes at the acquirer firm establishments offsetting (or complicating interpretation of) changes documented at the going-private establishments.

(ii) Cross-checking validity of productivity measures: One concern may be that the lack of significant productivity improvement after the going private event is driven by measurement error in estimated productivity. As discussed in section 3.2, we use an array of approaches to allay potential endogeneity concerns that arise in all measurements of total factor productivity. Nevertheless, we undertake two additional "external validity" checks for the productivity measures. One, as the results in section 5.2 demonstrate, our productivity measures are very negative and strongly significant in exit propensity regressions – these results hold also in specifications excluding the interaction term presented in 5.2. In other words, establishments with higher levels of our productivity measures are much less likely to be shut down, thus confirming that these measures have meaningful explanatory power. Second, we undertake cross-sectional and within firm regression tests relating our productivity measures to market value of firms. Specifically, we measure the average (over all plants) productivity level for each listed firm for the period 1980 to 2005 and regress it on the market value (based on year-end share price) using two specifications: (a) without firm fixed effects but including industry fixed effects to look at within-industry, across firm relation between measured productivity and market value; and (b) with firm fixed effects to focus on the relationship between within firm changes in measured productivity and market value. We found that our measures were highly significant in explaining the variation of market value both across firms, and importantly, within firm over time as well. These results strongly suggest that both the cross-firm as well as within firm variations in our productivity measures have empirical content and are not dominated by measurement error.

(iii) Using operating profit measures as an alternative to productivity: As a further check to rule out noise in productivity measures as an explanation for the baseline results, we examine two operating profit measures: (i) gross operating profits, defined as sales less sum of materials cost, energy costs, blue collar wage bill and white collar wage bill; and (ii) the ratio of gross operating profits to sales. We find results that are very similar to those in the baseline productivity analysis; neither profitability measures show significant improvement after establishments go private, relative to the two control groups. We also examine the ratio of different cost components (materials, energy, blue collar wage bill and white collar wage bill) to sales, and find no significant DID changes in any of these components.

(iv) Splitting the sample over time: It is possible that the responsiveness of managers to the stock market's short-term focus may have been exacerbated by the increasing use of stock options in executive compensation. To test whether the productivity and investment responses to going private have changed over time, we repeat the analysis in Tables 2, 3, 4 and 5 separately for the going private transactions that occurred before and after 1992. We find no notable differences between these samples; the qualitative conclusions of the analysis are the same for these samples separately as it was for the overall sample.

(v) Exploration of what drives establishment closure: The results in section 5.2 suggest strongly that after going private, firms close down establishments relatively faster (particularly) in the short-run. However, results in section 5.2.2 suggest that the shutdown decision of going private firms is not differentially targeted based on productivity. This raises the question: what type of establishments are the going private firms shutting down relatively faster? One potential answer could be that these firms shutdown target plants that have valuable assets. To test this explanation, we examine whether closures after going private differentially target plants located in richer communities. In particular, we run specifications similar to that in section 5.2.2 using local (county-level) per capita income and median home value as proxies for resale value of land and building in the locality. We find some (mixed) evidence in support of this explanation; i.e, in many specifications, we find that the coefficient on local per capita income and home value variables for going private firms was more negative, suggesting they are more likely to shut down plants in more expensive neighborhoods. This suggests that going private firms may "harvest" high value assets, and again this does not support the idea that going private may be a way to escape short-term market pressures and nurture projects.

(vi) Employment and exit propensity results for all (including non-manufacturing) firms: All of our analysis is presented using data for manufacturing plants. This is because, due to data limitations, total factor productivity (which is the main focus of our analysis) can be effectively estimated only for manufacturing plants. However, there is data available on employment (and payroll), as well as plant shutdown for all establishments (with at least one employee) in the longitudinal business database (LBD). This allows us to use these measures to check if the results in Section 5.1 and Section 5.2 extend to the universe of all firms that went private, the sample size for which is considerably larger than that for our baseline analysis of manufacturing establishments. Our results show that this is indeed the case. In particular, employment and payroll shows significant declines in establishments after going private, relative to their control group. Further, we also find that the shutdown propensity is significantly higher for establishments belonging to firms that went private, relative to control group establishments.

(vii) Other checks: We perform a number of other checks of the baselines results. These include (a) checking our results to using plant fixed effects and industry-year effects (instead of cell-year) effects in our DID specifications; (b) checking the split-by-acquirer-type results in Table 8 (Panel B and C) using the event time figures (specifications in 3 and 5); (c) redoing the baseline results adjusting for sampling weights (the baseline analysis treats the ASM-CMF sample as an unbalanced panel) to check if sampling systematically affects the going private sample differentially relative to the control groups. We find our results robust to these checks.

(viii) Potential caveat: One important caveat is that, due to data availability reasons, our productivity analysis focuses on manufacturing plants. Thus there is a possibility that the productivity results may be different in other non-manufacturing industries. However, as discussed in point vi above, we verify that the employment and exit results hold for the universe of all firms.

7 Conclusion

An important critique of the stock market oriented U.S. financial system is that its excessive focus on short term quarterly earnings forces public firms to behave in a myopic manner. We hypothesize that if U.S. firms are myopic in a manner that affects operational efficiency, then instances of going private (when myopia is eliminated) should cause U.S. firms to improve their establishment level productivity (by focusing on long-term decisions) relative to peers. We find no evidence that this is the case. Our key finding is that while there is evidence for substantial within establishment increases in productivity (about 3% to 6%) after going private, there is little evidence of difference-in-differences efficiency gains relative to a peer group of establishments constructed to control for industry, age, initial size (at the time of going private) and the endogeneity of the going private decision effects. Contrary to the standard myopia story, we find that going private firms contract activity (decrease capital and employment) and close plants more quickly than peer groups. Our findings cast doubt on the view that stock markets force publicly listed firms to be short-sighted.

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Data Appendix

The Census of Manufactures (CMF) covers all establishments is a quinquennial census that is undertaken in years 1977, 1982, 1987, 1992, 1997 and 2002. For the other years used in our analysis, we use data from the ASM that surveys: (i) All establishments with greater than (or equal to) 250 employees; (ii) All establishments of multi-unit firms; and (iii) a stratified randomized sample of establishments with less than 250 employees. For certain small establishments in the CMF, the employment data is imputed based on reported payroll from administrative records data. Following the practice in the literature, such establishments (which are flagged by an 'Administrative Records' dummy variable) are excluded from our analysis. As very few (less than 1%) of the going-private establishments belong to this category, this exclusion has very little impact on our sample size.

Key variables used in the analysis are as defined below. Deflators used for obtaining real values are taken from the NBER-CES manufacturing industry database (Becker and Gray 2009).

(i). **Output measures**

(a) Log real sales is defined as value of shipments deflated using 4-digit SIC industry-specific output deflators.

(b) Log real value added is defined as log of (real sales - real materials - real energy costs).

(ii). **Input measures**

(a) Log employment is the log of the total number of employees reported in the ASM-CMF database.

(b) Log real materials is the log of the deflated cost of materials used.

(c) Log real energy costs is the log of the deflated cost of fuel, electricity and other energy sources used.

(d) Log real capital is defined as the log the real depreciated capital stock. The real depreciated capital stock is constructed using the perpetual inventory method. The depreciation rates (and deflators) used to construct the plant specific real depreciated structures and equipment stocks were taken from Becker and Gray, 2009.

(iii). **Productivity measures**

Basic definitions of the productivity measures are provided in the text. Here we describe in some detail the specific methodology used to define the Levinsohn-Petrin and the Blundell-Bond productivity measures.

(a) Levinsohn-Petrin TFP measure: To estimate the LP TFP measure, we assume a Cobb-Douglas value-added production function:

$$v_{it}^{j} = \beta_{l}^{j}.l_{it} + \beta_{n}^{j}.n_{it} + \beta_{k}^{j}.k_{it} + \epsilon_{it}^{j} \quad (7)$$

where *v* is the log real value added (gross output net of intermediate outputs), *l* is the log of the number of production (blue collar) employees, *n* is the log of the number of non-production (white collar) employees and *k* is the log of the real capital employed. We allow the coefficients in the production function to vary by (2-digit NIC) industry (indexed by j), by estimating the production function separately for each industry. The index i stands for the plant and t stands for the year. We define total factor productivity as the residual ϵ_{it}.

We assume that the productivity residual has two components (and drop the industry index j from our notation to reduce clutter):

$$\epsilon_{it} = \omega_{it} + \eta_{it}$$

where ω_{it} is the component of the productivity shock that is known to the decision-maker before she makes the choice of inputs (k_{it}, l_{it} and n_{it}), but is unobserved by the econometrician. This "transmitted" component thus leads to a correlation between the input variables (regressors) and the productivity residual (error term), potentially biasing the coefficients estimated using the OLS methodology described above. The transmitted component could arise from correlation in productivity shocks over time, or due to anticipated shocks to productivity. The component η_{it}, which is assumed to be orthogonal to the regressors, captures all other deviations from the hypothesized production function, arising from classical measurement error, optimizing errors, etc. The LP method assumes the demand of the intermediate input (in our case the log of real materials) is a function of the firm's state variables k_{it} and ω_{it}. Making mild assumptions about the firms production technology, Levinsohn and Petrin (2003) show that the intermediate demand function is monotonically increasing in ω_{it}. This allows inversion of the intermediate demand function, so ω_{it} can be written as a function of k_{it} and the intermediate input. Thus, the unobservable productivity term is now expressed solely as a function of two observed inputs. Thus a first stage regression of value added on labor inputs and a polynomial (or semi-parametric) function of capital and materials, allows us to estimate coefficients on labor inputs. To recover the coefficient on capital, the LP methodology relies on two assumptions. One is that the ω_{it} follows a first-order Markov process. Then, assuming that k_{it} is chosen prior to realization of period t shocks, k_{it} is orthogonal to innovations in productivity. Over-identifying moment conditions are available if we assume lagged material and other inputs are orthogonal to the innovation in productivity as well. Further details are available in Levinsohn and Petrin (2003).

(b) Blundell-Bond system-GMM TFP measure: We follow the approach in Blundell and Bond (2000), and we assume a gross output production function with an AR1 component in the productivity term:

$$\begin{aligned} y_{it} &= \beta_l.l_{it} + \beta_n.n_{it} + \beta_k.k_{it} + \beta_m m_{it} + \beta_e e_{it} + \eta_i + \nu_{it} + m_{it} \\ \nu_{it} &= \rho\nu_{it-1} + \epsilon_{it} \qquad |\rho| < 1 \\ \epsilon_{it}, m_{it} &\sim MA(0) \end{aligned}$$

where output and inputs are as defined in section 3.2. The model has a dynamic (common factor representation):

$$\begin{aligned} y_{it} = {} & \pi_l.l_{it} + \pi_2.l_{it-1} + \pi_3.n_{it} + \pi_4.n_{it-1} + \pi_5.k_{it} + \pi_6.k_{it-1} + \pi_7 m_{it} + \pi_8 m_{it-1} \\ & + \pi_9 e_{it} + \pi_{10} e_{it} + \pi_{11} y_{it-1} + \eta_i^* + \omega_{it} \end{aligned}$$

subject to 5 common factor restrictions: $\pi_2 = -\pi_1 * \pi_{11}$, $\pi_4 = -\pi_3 * \pi_{11}$, $\pi_6 = -\pi_5 * \pi_{11}$, $\pi_8 = -\pi_7 * \pi_{11}$ and $\pi_{10} = -\pi_9 * \pi_{11}$, and where $\eta_i^* = \eta_i(1-\rho)$. The standard Arellano-Bond moment (1991) conditions are

$$E[x_{it-j}\Delta\omega_{it}] = 0 \ \text{ where } \ x_{it} = (l_{it}, n_{it}, k_{it}, m_{it}, e_{it}, y_{it})$$

for $j \geq 3$ (assuming $\omega_{it} \sim MA(1)$). This allows the use of suitably lagged levels of the variables as instruments, after the equation has been first differenced to eliminate f_i, the plant specific fixed effects. Blundell and Bond (2000) show that by assuming input and output in first differences depend only on the history of the productivity shock till time t but do not depend on the fixed effect f_i, one can obtain additional moment conditions that use lagged first differences as valid instruments for the equation in levels which greatly improve upon the properties of the estimator. These conditions are:

$$E[\Delta x_{it-j}(\eta_i^* + \omega_{it})] = 0 \ \text{ where } \ x_{it} = (l_{it}, n_{it}, k_{it}, m_{it}, en_{it}, y_{it}) \tag{8}$$

for j=2 (assuming $\omega_{it} \sim MA(1)$). Both sets of moment conditions can be exploited as a linear GMM estimator in a system containing both first-differenced and levels equations. Combining both sets of moment conditions provides the Blundell and Bond (2000) system GMM estimator. The underlying production function parameter estimates are recovered by imposing the common factor restrictions using a minimum distance estimator. Further details on the estimation procedure are available in Blundell and Bond (2003).

Table 1: Sample characteristics, by years around the "going-private" decision

The data used to construct this sample is taken from the Census of Manufactures (CMF) and the Annual Survey of Manufactures (ASM). For each establishment in the going private sample, we include upto eight establishments (based on data availability) that are closest in size (employment) to the going private establishment from within the same 3-digit SIC industry, and belonging to the same age quartile as controls.

Years from going private	Total number of establishments	Number of going-private establishments	Number of matched control establishments	Total number of firms	Number of going-private firms	Number of control firms
-6	13,464	2,285	11,179	7,210	459	6,751
-5	13,905	2,295	11,610	7,284	446	6,838
-4	16,007	2,580	13,427	8,368	424	7,944
-3	16,121	2,517	13,604	8,546	427	8,119
-2	17,600	2,769	14,831	9,411	421	8,990
-1	20,240	2,827	17,413	11,255	382	10,873
0	16,212	2,391	13,821	8,567	407	8,160
1	15,631	2,396	13,235	7,984	442	7,542
2	14,463	2,341	12,122	6,821	459	6,362
3	13,068	2,127	10,941	6,049	462	5,587
4	11,789	1,991	9,798	5,340	434	4,906
5	9,762	1,731	8,031	4,495	389	4,106
6	8,917	1,538	7,379	4,358	373	3,985
Total	**187,179**	**29,788**	**157,391**	**95,688**	**5,525**	**90,163**

Table 2: Changes in productivity measures around the "going-private" decision

This table presents regression results for each of the productivity measures for the sample of establishments of firms that went private. The data used to construct this sample is taken from the Census of Manufactures (CMF) and the Annual Survey of Manufactures (ASM). Dummy variables LR_PRE equals 1 for years -6 to -4 from going private and zero otherwise, SR_PRE equals 1 for years -3 to -1 from going private and zero otherwise, SR_POST equals 1 for years 0 to 3 from going private and zero otherwise, and LR_POST equals 1 for years 4 to 6 from going private and zero otherwise. P-values based on standard errors clustered by establishment are in parentheses.

	Labor productivity	**OLS TFP**	**Solow TFP**	**Levinsohn-Petrin TFP**	**Translog TFP**	**Blundell-Bond TFP**
	1	2	3	4	5	6
LR_PRE	3.237	3.593	1.92	4.015	4.168	2.389
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
SR_PRE	3.342	3.627	1.958	4.0816	4.1933	2.4264
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
SR_POST	3.405	3.659	1.978	4.0897	4.2182	2.4633
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
LR_POST	3.414	3.691	1.986	4.0818	4.2431	2.4773
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
CHANGES RELATIVE TO SR_PRE						
SR_POST - SR_PRE	0.063	0.032	0.020	0.0081	0.0249	0.0369
	(0.000)	(0.000)	(0.001)	(0.548)	(0.000)	(0.000)
LR_POST - SR_PRE	0.072	0.064	0.028	0.0002	0.0498	0.0509
	(0.000)	(0.000)	(0.002)	(0.990)	(0.000)	(0.000)
TEST FOR PRE-EXISTING TREND						
SR_PRE - LR_PRE	0.105	0.034	0.038	0.0666	0.0253	0.0374
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Fixed effects	Plant	Plant	Plant	Plant	Plant	Plant
Number of Observations	28,518	28,518	28,518	28,518	28,518	28,518

Table 3: Changes in productivity measures around the "going-private" decision : Difference-in-Differences (DID) specifications

This table presents difference-in-differences regression results for each of the different productivity measures. For each establishment in the going private sample, we include upto eight establishments (based on data availability) that are closest in size (employment) to the going private establishment from within the same 3-digit SIC industry, and belonging to the same age quartile as controls. The data used to construct this sample is taken from the Census of Manufactures (CMF) and the Annual Survey of Manufactures (ASM). Dummy variables LR_PRE equals 1 for years -6 to -4 from going private and zero otherwise, SR_PRE equals 1 for years -3 to -1 from going private and zero otherwise, SR_POST equals 1 for years 0 to 3 from going private and zero otherwise, and LR_POST equals 1 for years 4 to 6 from going private and zero otherwise. P-values based on standard errors clustered by industry-size-age cells are in parentheses.

	Labor productivity	**OLS TFP**	**Solow TFP**	**Levinsohn-Petrin TFP**	**Translog TFP**	**Blundell-Bond TFP**
	1	2	3	4	5	6
LR_PRE	0.066	0.03	-0.001	0.0431	0.0166	0.000
	(0.000)	(0.002)	(0.914)	(0.011)	(0.051)	(0.983)
SR_PRE	0.08	0.037	0.003	0.0429	0.0224	0.005
	(0.000)	(0.000)	(0.678)	(0.004)	(0.005)	(0.543)
SR_POST	0.09	0.027	0.005	0.0685	0.0255	-0.001
	(0.000)	(0.005)	(0.590)	(0.000)	(0.003)	(0.930)
LR_POST	0.085	0.026	0.022	0.073	0.0197	0.006
	(0.000)	(0.028)	(0.048)	(0.001)	(0.073)	(0.592)
CHANGES RELATIVE TO SR_PRE						
SR_POST - SR_PRE	0.011	-0.011	0.002	0.0256	0.0031	-0.0055
	(0.510)	(0.202)	(0.859)	(0.106)	(0.690)	(0.479)
LR_POST - SR_PRE	0.005	-0.011	0.018	0.0301	-0.0027	0.0011
	(0.817)	(0.374)	(0.117)	(0.184)	(0.800)	(0.924)
TEST FOR PRE-EXISTING TREND						
SR_PRE - LR_PRE	0.014	0.008	0.004	-0.0002	0.0058	0.005
	(0.362)	(0.342)	(0.568)	(0.991)	(0.413)	(0.492)
Fixed effects			Industry-size-age-year			
Number of Observations	185,909	185,909	185,909	185,909	185,909	185,909

Table 4: Changes in Productivity measures around the "going-private" decision : Difference-in-Differences specifications using alternative propensity score matched control group

This table presents regression results for each of the productivity measures for the sample of establishments of firms that went private. For each establishment in the going private sample, we construct the closest propensity to go private score matched firm(s) but did not go private, as control firm(s). We use the firm specific control variables at the time of the IPO to estimate the propensity to go private as in Bharath and Dittmar (2010). Since these control variables are not available for all firms (whose establishments we consider in the regression) in our sample, the number of establishments of firms that went private drops to 22,488 (from 28,518 in table 2) in these estimations. The data used to construct this sample is taken from the Census of Manufactures (CMF) and the Annual Survey of Manufactures (ASM). Dummy variables LR_PRE equals 1 for years -6 to -4 from going private and zero otherwise, SR_PRE equals 1 for years -3 to -1 from going private and zero otherwise, SR_POST equals 1 for years 0 to 3 from going private and zero otherwise, and LR_POST equals 1 for years 4 to 6 from going private and zero otherwise. P-values based on standard errors clustered by industry-propensity cells are in parentheses.

	Labor productivity	**OLS TFP**	**Solow TFP**	**Levinsohn-Petrin TFP**	**Translog TFP**	**Blundell-Bond TFP**
	1	2	3	4	5	6
LR_PRE	-0.0519	-0.018	0.0157	-0.0615	-0.009	-0.0017
	(0.238)	(0.489)	(0.495)	(0.162)	(0.695)	(0.942)
SR_PRE	-0.0037	0.0035	0.0217	-0.0124	0.0033	0.0056
	(0.916)	(0.874)	(0.278)	(0.723)	(0.864)	(0.769)
SR_POST	-0.030	-0.026	0.002	-0.025	-0.013	-0.015
	(0.445)	(0.273)	(0.938)	(0.527)	(0.533)	(0.484)
LR_POST	0.0363	0.0327	0.0592	0.0696	0.0424	0.0368
	(0.509)	(0.292)	(0.041)	(0.197)	(0.103)	(0.189)
CHANGES RELATIVE TO SR_PRE						
SR_POST - SR_PRE	-0.0261	-0.0298	-0.0199	-0.0123	-0.0164	-0.021
	(0.497)	(0.187)	(0.363)	(0.755)	(0.425)	(0.310)
LR_POST - SR_PRE	0.040	0.0292	0.0375	0.082	0.0392	0.0312
	(0.488)	(0.366)	(0.224)	(0.158)	(0.163)	(0.292)
TEST FOR PRE-EXISTING TREND						
SR_PRE - LR_PRE	0.0482	0.0215	0.006	0.0491	0.0123	0.0073
	(0.246)	(0.339)	(0.785)	(0.244)	(0.537)	(0.720)
Fixed effects	Industry-propensity cell-year					
Number of Observations	55,358	55,358	55,358	55,358	55,358	55,358

Table 5: Changes in establishment capital and employment around the "going-private" decision : Before-After and Difference-in-Differences Specifications

This table presents difference-in-differences regression results for capital and employment. In DID1, for each establishment in the going private sample, we include upto eight establishments (based on data availability) that are closest in size (employment) to the going private establishment from within the same 3-digit SIC industry, and belonging to the same age quartile as controls. In DID2, we include upto eight establishments within the same 3-digit SIC industry closest in propensity to go private (but whose owner firms did not go private), as control establishments. The data used to construct this sample is taken from the Census of Manufactures (CMF) and the Annual Survey of Manufactures (ASM). Dummy variables LR_PRE equals 1 for years -6 to -4 from going private and zero otherwise, SR_PRE equals 1 for years -3 to -1 from going private and zero otherwise, SR_POST equals 1 for years 0 to 3 from going private and zero otherwise, and LR_POST equals 1 for years 4 to 6 from going private and zero otherwise. P-values based on standard errors clustered by plant establishments (before-after), industry-size-age cells (DID1) and industry-propensity cells (DID2) are in parentheses.

	Log deflated capital			Log employment		
	Before-After	DID1	DID2	Before-After	DID1	DID2
	1a	1b	1c	1a	1b	1c
LR_PRE	8.642	0.181	-0.223	5.033	0.043	-0.134
	(0.000)	(0.000)	(0.012)	(0.000)	(0.000)	(0.033)
SR_PRE	8.659	0.195	-0.124	4.994	0.021	-0.054
	(0.000)	(0.000)	(0.117)	(0.000)	(0.000)	(0.345)
SR_POST	8.684	0.092	-0.176	4.936	0.001	-0.076
	(0.000)	(0.000)	(0.038)	(0.000)	(0.929)	(0.229)
LR_POST	8.722	0.044	-0.278	4.905	-0.011	-0.123
	(0.000)	(0.078)	(0.005)	(0.000)	(0.459)	(0.101)
CHANGES RELATIVE TO SR_PRE						
SR_POST - SR_PRE	0.025	-0.103	-0.052	-0.058	-0.021	-0.022
	(0.001)	(0.000)	(0.405)	(0.000)	(0.032)	(0.650)
LR_POST - SR_PRE	0.064	-0.151	-0.154	-0.089	-0.033	-0.069
	(0.000)	(0.000)	(0.090)	(0.000)	(0.038)	(0.321)
TEST FOR PRE-EXISTING TREND						
SR_PRE - LR_PRE	0.017	0.014	0.099	-0.039	-0.021	0.080
	(0.009)	(0.369)	(0.147)	(0.007)	(0.012)	(0.099)
Fixed effects	Plant	Cell-year	Cell-year	Plant	Cell-year	Cell-year
Number of Observations	28,518	185,909	55,358	28,518	185,909	55,358

Table 6: Establishment exit after the "going-private" decision: Hazard and propensity analysis

This table presents hazard model (Panel A) and exit dummy linear propensity model (Panel B) results. In Panel A, the analysis is done using time-invariant explanatory variables, so the data has one observation for each of the gone private and control establishments. In Panel B, we include all observations for all years; 2- (4-) year exit dummy is a variable that equals one if the plant exited (was shut down) in the next two (four) years and zero otherwise (these variables are undefined (missing) for a plant for time periods after it is shut down). In columns 1a and 1b of Panel A, and columns 1a and 2a of Panel B, for each establishment in the going private sample, we include up to eight establishments that are closest in size (employment) to the going private establishment from within the same 3-digit SIC industry and age quartile as controls. In columns 2a and 2b of Panel A, and columns 1b and 2b of Panel B, we include up to eight establishments within the same 3-digit SIC industry closest in propensity to go private (but whose owner firms did not go private) as controls. The data used to construct this sample is taken from the longitudinal business database (LBD). Dummy variables SR_POST equals 1 for years 0 to 3 from going private and zero otherwise, and LR_POST equals 1 for years 4 to 6 from going private and zero otherwise. P-values based on standard errors clustering by control group cells are in parentheses.

Panel A	Cox proportional hazards model 1a	Exponential model 1b	Cox proportional hazards model 2a	Exponential model 2b
Log employment	-0.189	-0.193	-0.258	-0.256
	(0.000)	(0.000)	(0.000)	(0.000)
Age	-0.019	-0.019	-0.011	-0.01
	(0.000)	(0.000)	(0.000)	(0.001)
Gone-private dummy	0.191	0.194	0.034	0.032
	(0.000)	(0.000)	(0.273)	(0.302)
Gone-private dummy hazard ratio	1.210	1.214	1.035	1.033
Control group	Industry-size-age matched		Industry-propensity score matched	
Fixed Effects	Industry, Year	Industry, Year	Industry, Year	Industry, Year
Observations	34,215	34,215	10,449	10,449

Panel B	2-year exit dummy		4-year exit dummy	
	Differenced mean 1	Differenced mean 2	Differenced mean 1	Differenced mean 2
	1a	1b	2a	2b
SR_POST	0.016	0.021	0.018	0.027
	(0.000)	(0.007)	(0.000)	(0.015)
LR_POST	0.001	-0.008	0.009	-0.015
	(0.707)	(0.400)	(0.138)	(0.258)
Fixed effects	Cell-year	Cell-year	Cell-year	Cell-year
Number of Observations	398,290	119,856	398,290	119,856

Table 7: Exit hazard after the "going-private" decision and productivity: Tests for differential targeting

This table presents Cox proportional hazards model results of the duration to exit measures of the sample of establishments of firms that went private. In columns 1a, 1b and 1c, for each establishment in the going private sample, we include up to eight establishments (based on data availability) that are closest in size (employment) to the going private establishment from within the same 3-digit SIC industry, and belonging to the same age quartile as controls. In columns 2a, 2b and 2c, we include up to eight establishments within the same 3-digit SIC industry closest in propensity to go private (but whose owner firms did not go private) as controls. The data used to construct this sample is taken from the Census of Manufactures (CMF), the Annual Survey of Manufactures (ASM) and the longitudinal business database (LBD). P-values based on standard errors clustered by control group cells are in parentheses.

	1a	1b	1c	2a	2b	2c
Log employment	-0.195	-0.184	-0.204	-0.307	-0.291	-0.298
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age	-0.014	-0.014	-0.014	-0.008	-0.009	-0.010
	(0.000)	(0.000)	(0.000)	(0.110)	(0.072)	(0.046)
Gone-private dummy	0.243	0.386	-0.034	0.161	0.291	0.174
	(0.036)	(0.038)	(0.775)	(0.293)	(0.241)	(0.309)
Labor productivity	-0.159			-0.159		
	(0.000)			(0.000)		
OLS TFP		-0.209			-0.202	
		(0.000)			(0.000)	
Solow TFP			-0.189			-0.029
			(0.000)			(0.623)
Gone-private dummy X Labor productivity	-0.029			-0.064		
	(0.407)			(0.155)		
Gone-private dummy X OLS TFP		-0.069			-0.094	
		(0.185)			(0.167)	
Gone-private dummy X Solow TFP			0.084			-0.106
			(0.155)			(0.191)
Control group	Industry-size-age matched			Industry-propensity score matched		
Fixed effects	Industry, year			Industry, year		
Observations	19,285	19,285	19,285	5,441	5,441	5,441

Table 8 Panel A: Sample characteristics: Breakdown by acquirer type

We classify the sample firms that went private into three categories: buyouts by private operating firms, buy outs by private equity firms, buy outs by management. We source the classifications for these deals using news paper reports from Factiva. The residual category is unclassified. The category-types are non-exclusive, so that some deals may involve deals classified under more than one type. The data used to construct this sample is taken from the Census of Manufactures (CMF) and the Annual Survey of Manufactures (ASM). For each establishment in the going private sample, we include upto eight establishments (based on data availability) that are closest in size (employment) to the going private establishment from within the same 3-digit SIC industry, and belonging to the same age quartile as controls.

Acquirer type	Total number of establishments	Number of going-private establishments	Number of matched control establishments	Total number of firms	Number of going-private firms	Number of control firms
Unclassified	1,585	301	1,284	761	46	715
Management	5,162	772	4,390	2,920	125	2,795
Operating	7,046	944	6,102	3,662	189	3,473
Private equity	5,702	845	4,857	2,941	110	2,831

Table 8 Panel B: Changes in productivity measures around the "going-private" decision : Difference-in-Differences specifications break down by acquirer type

This table presents results for the tests of differences in productivity over time. These tests are based on separate regressions for each acquirer type identified in Table 8, Panel A. For each establishment in the going private sample, we include upto eight establishments (based on data availability) that are closest in size (employment) to the going private establishment from within the same 3-digit SIC industry, and belonging to the same age quartile as controls. The data used to construct this sample is taken from the Census of Manufactures (CMF) and the Annual Survey of Manufactures (ASM). Dummy variables LR_PRE equals 1 for years -6 to -4 from delisting and zero otherwise, SR_PRE is a dummy variable that equals 1 for years -3 to -1 from delisting and zero otherwise, SR_POST equals 1 for years 0 to 3 from delisting and zero otherwise, and LR_POST equals 1 for years 4 to 6 from delisting and zero otherwise. Standard errors clustered by control group cells are used to assess the P-values (in parentheses) for the different tests.

	Unclassified	**Management**	**Operating**	**Private Equity**
		Labor productivity		
Short run change (SR_POST - SR_PRE)	-0.003	0.002	0.017	-0.002
	(0.968)	(0.931)	(0.501)	(0.927)
Long run change (LR_POST - SR_PRE)	-0.049	-0.019	0.024	0.003
	(0.582)	(0.651)	(0.473)	(0.932)
Pre-existing trend (SR_PRE - LR_PRE)	0.080	-0.005	0.011	0.008
	(0.143)	(0.856)	(0.628)	(0.785)
		OLS TFP		
Short run change (SR_POST - SR_PRE)	**-0.061**	**-0.022**	0.002	-0.009
	(0.091)	**(0.092)**	(0.859)	(0.468)
Long run change (LR_POST - SR_PRE)	**-0.095**	-0.019	0.017	-0.018
	(0.055)	(0.356)	(0.319)	(0.369)
Pre-existing trend (SR_PRE - LR_PRE)	0.061	-0.008	0.010	0.001
	(0.053)	(0.578)	(0.357)	(0.962)
		Solow TFP		
Short run change (SR_POST - SR_PRE)	-0.009	0.010	-0.009	0.016
	(0.819)	(0.453)	(0.520)	(0.220)
Long run change (LR_POST - SR_PRE)	-0.052	0.025	**0.031**	0.025
	(0.291)	(0.217)	**(0.072)**	(0.201)
Pre-existing trend (SR_PRE - LR_PRE)	0.033	-0.012	0.014	-0.008
	(0.285)	(0.388)	(0.200)	(0.542)
Fixed effects		Industry-size-age-year		
Number of observations	18,683	56,753	84,020	62,795

Table 8 Panel C: Changes in capital and employment around the "going-private" decision : Difference-in-Differences specifications break down by acquirer type

This table presents results for the tests of changes in capital and employment over time. These tests are based on separate regressions for each acquirer type identified in Table 8, Panel A. For each establishment in the going private sample, we include upto eight establishments (based on data availability) that are closest in size (employment) to the going private establishment from within the same 3-digit SIC industry, and belonging to the same age quartile as controls. The data used to construct this sample is taken from the Census of Manufactures (CMF) and the Annual Survey of Manufactures (ASM). Dummy variables LR_PRE equals 1 for years -6 to -4 from going private and zero otherwise, SR_PRE equals 1 for years -3 to -1 from going private and zero otherwise, SR_POST equals 1 for years 0 to 3 from going private and zero otherwise, and LR_POST equals 1 for years 4 to 6 from going private and zero otherwise. Standard errors clustered by industry-size-age groups are used to assess the P-values (in parentheses) for the different tests.

	Unclassified	**Management**	**Operating**	**Private equity**
		Log deflated capital		
Short run change (SR_POST - SR_PRE)	-0.158	**-0.122**	**-0.079**	**-0.110**
	(0.010)	**(0.000)**	**(0.001)**	**(0.000)**
Long run change (LR_POST - SR_PRE)	-0.126	**-0.217**	**-0.116**	**-0.210**
	(0.136)	**(0.000)**	**(0.001)**	**(0.000)**
Pre-existing trend (SR_PRE - LR_PRE)	0.083	0.035	-0.004	0.012
	(0.157)	(0.214)	(0.860)	(0.666)
		Log employment		
Short run change (SR_POST - SR_PRE)	0.019	**-0.047**	-0.018	-0.019
	(0.600)	**(0.004)**	(0.227)	(0.210)
Long run change (LR_POST - SR_PRE)	-0.031	**-0.071**	-0.021	**-0.061**
	(0.563)	**(0.011)**	(0.379)	**(0.017)**
Pre-existing trend (SR_PRE - LR_PRE)	**-0.063**	-0.022	-0.013	-0.021
	(0.041)	(0.128)	(0.280)	(0.136)
Fixed effects		Industry-size-age-year		
Number of observations	18,683	56,753	84,020	62,795

Table 8 Panel D: Establishment exit propensity after the "going-private" decision: breakdown by acquirer type

This table presents hazard models in Panel A, and exit dummy linear propensity models for each acquirer type identified in Table 8, Panel A in Panel B. In Panel A, the analysis is done using time-invariant explanatory variables, so the data has one observation for each of the gone private and control establishments. In Panel B, we include all observations for all years; 2- (4-) year exit dummy is a variable that equals one if the plant exited (was shut down) in the next two (four) years and zero otherwise (these variables are undefined (missing) for a plant for time periods after it is shut down). For each establishment in the going private sample, we include up to eight establishments that are closest in size (employment) to the going private establishment from within the same 3-digit SIC industry and age quartile as controls. The data used to construct this sample is taken from the longitudinal business database (LBD). Dummy variables SR_POST equals 1 for years 0 to 3 from going private and zero otherwise, and LR_POST equals 1 for years 4 to 6 from going private and zero otherwise. P-values based on clustering by control group cells are in parentheses.

Panel D-A	**Cox proportional hazards model**	**Exponential model**
Unclassified acquirers	**0.283**	**0.288**
	(0.000)	**(0.000)**
Management acquirers	-0.063	-0.064
	(0.115)	(0.119)
Operating acquirers	**0.255**	**0.260**
	(0.000)	**(0.000)**
Private equity acquirers	**0.224**	**0.229**
	(0.000)	**(0.000)**
Fixed effects	Industry, Year	Industry, Year
Other controls	Employment, Age	Employment, Age
Observations	34,215	34,215

Panel D-B	**Unclassified**	**Management**	**Operating**	**Private Equity**
		2-year exit dummy		
Short-run (SR_POST)	0.007	0.004	**0.023**	**0.022**
	(0.537)	(0.546)	**(0.000)**	**(0.000)**
Long-run (LR_POST)	-0.004	-0.006	0.006	0.002
	(0.711)	(0.355)	(0.268)	(0.763)
		4-year exit dummy		
Short-run (SR_POST)	0.007	0.006	**0.026**	**0.028**
	(0.707)	(0.543)	**(0.000)**	**(0.001)**
Long-run (LR_POST)	0.030	-0.004	**0.014**	-0.005
	(0.155)	(0.684)	**(0.083)**	(0.618)
Fixed effects		Industry-size-age-year		
Number of observations	45,791	108,256	191,810	118,488

Table 8 Panel E: Exit hazard after the "going-private" decision and productivity : Tests for differential targeting by acquirer type

This table presents hazard model results of the duration to exit measures. For each establishment in the going private sample, we include upto eight establishments (based on data availability) that are closest in size (employment) to the going private establishment from within the same 3-digit SIC industry, and belonging to the same age quartile as controls. The data used to construct this sample is taken from the Census of Manufactures (CMF), the Annual Survey of Manufactures (ASM) and the longitudinal business database (LBD). P-values based on standard errors clustered by industry-size-age groups are in parentheses.

	Cox proportional hazards model			**Exponential model**		
	1	2	3	4	5	6
PROD=	LABOR	OLS	Solow	LABOR	OLS	Solow
Log employment	-0.195	-0.184	-0.204	-0.198	-0.186	-0.206
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age	-0.014	-0.014	-0.014	-0.014	-0.014	-0.015
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Unclassified acquirers	0.148	0.523	0.330	0.151	0.535	0.333
	(0.557)	(0.209)	(0.210)	(0.557)	(0.209)	(0.214)
Management acquirers	-0.381	-0.925	-0.086	-0.402	-0.943	-0.098
	(0.108)	(0.019)	(0.732)	(0.094)	(0.019)	(0.701)
Operating acquirers	0.513	0.625	-0.035	0.530	0.631	-0.040
	(0.001)	(0.015)	(0.845)	(0.001)	(0.016)	(0.826)
Private equity acquirers	0.634	1.228	0.148	0.649	1.242	0.154
	(0.008)	(0.000)	(0.514)	(0.007)	(0.000)	(0.503)
PROD	-0.158	-0.21	-0.186	-0.160	-0.213	-0.188
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Unclassified X PROD	0.047	-0.063	-0.020	0.047	-0.066	-0.020
	(0.489)	(0.577)	(0.868)	(0.496)	(0.569)	(0.870)
Management X PROD	0.105	**0.250**	0.014	0.111	**0.254**	0.018
	(0.145	**(0.023)**	(0.909)	(0.128)	**(0.022)**	(0.886)
Operating X PROD	**-0.095**	-0.120	0.118	**-0.099**	-0.121	0.122
	(0.057)	(0.100)	(0.190)	**(0.057)**	(0.102)	(0.185)
Private equity X PROD	**-0.143**	**-0.300**	0.006	**-0.146**	**-0.303**	0.006
	(0.047)	**(0.002)**	(0.957)	**(0.046)**	**(0.002)**	(0.958)
Fixed effects			Industry, Year			
Observations	19,285	19,285	19,285	19,285	19,285	19,285

Figure 1: Before-after productivity trends in event time

This figure displays the evolution of productivity measures in event time for the going private sample. The confidence intervals are based on standard errors clustered by plant.

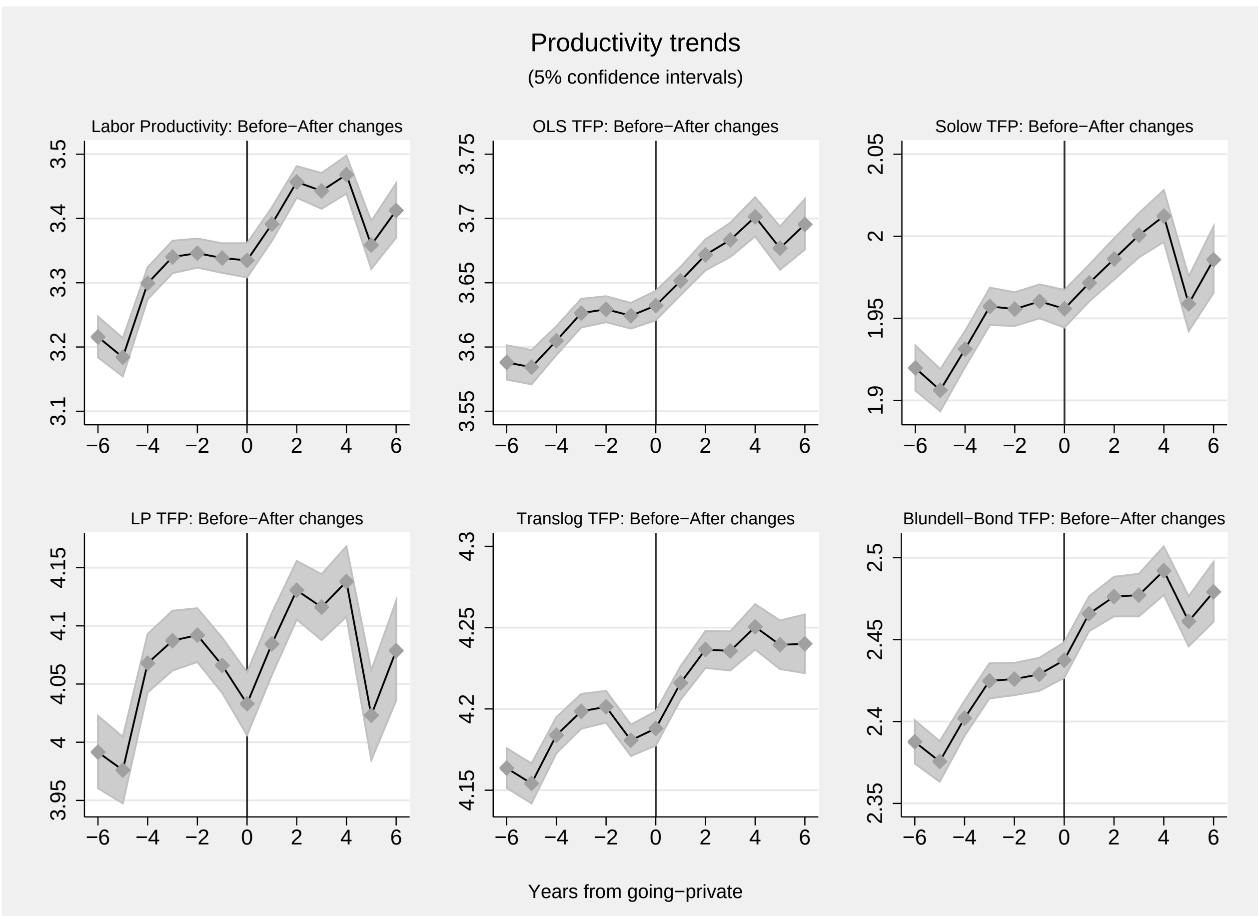


Figure 2: Difference-in-differences productivity trends in event time

This figure displays the evolution of productivity measures in event time for the going private sample, relative to a industry-age-initial size matched control group.The confidence intervals are based on standard errors clustered by control group cells.

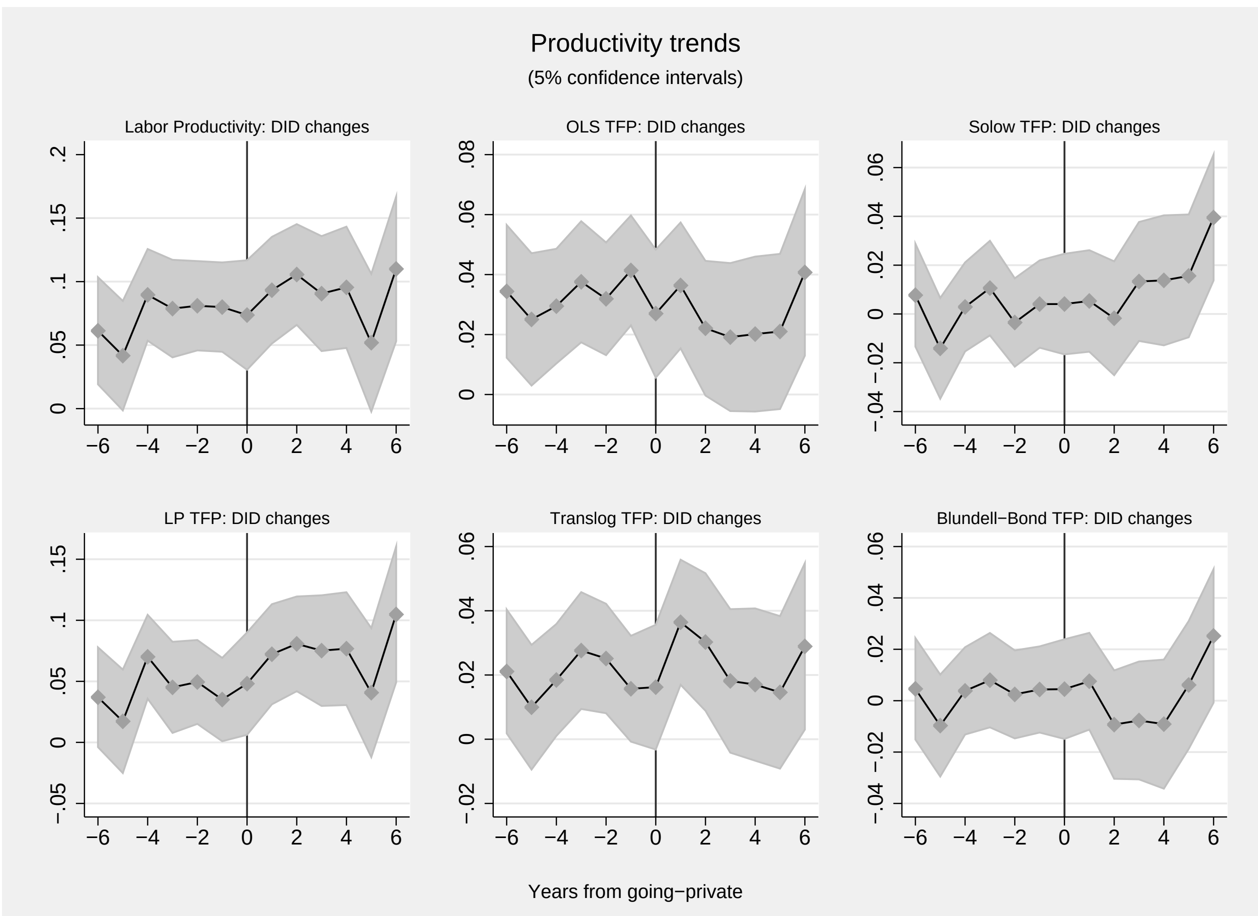
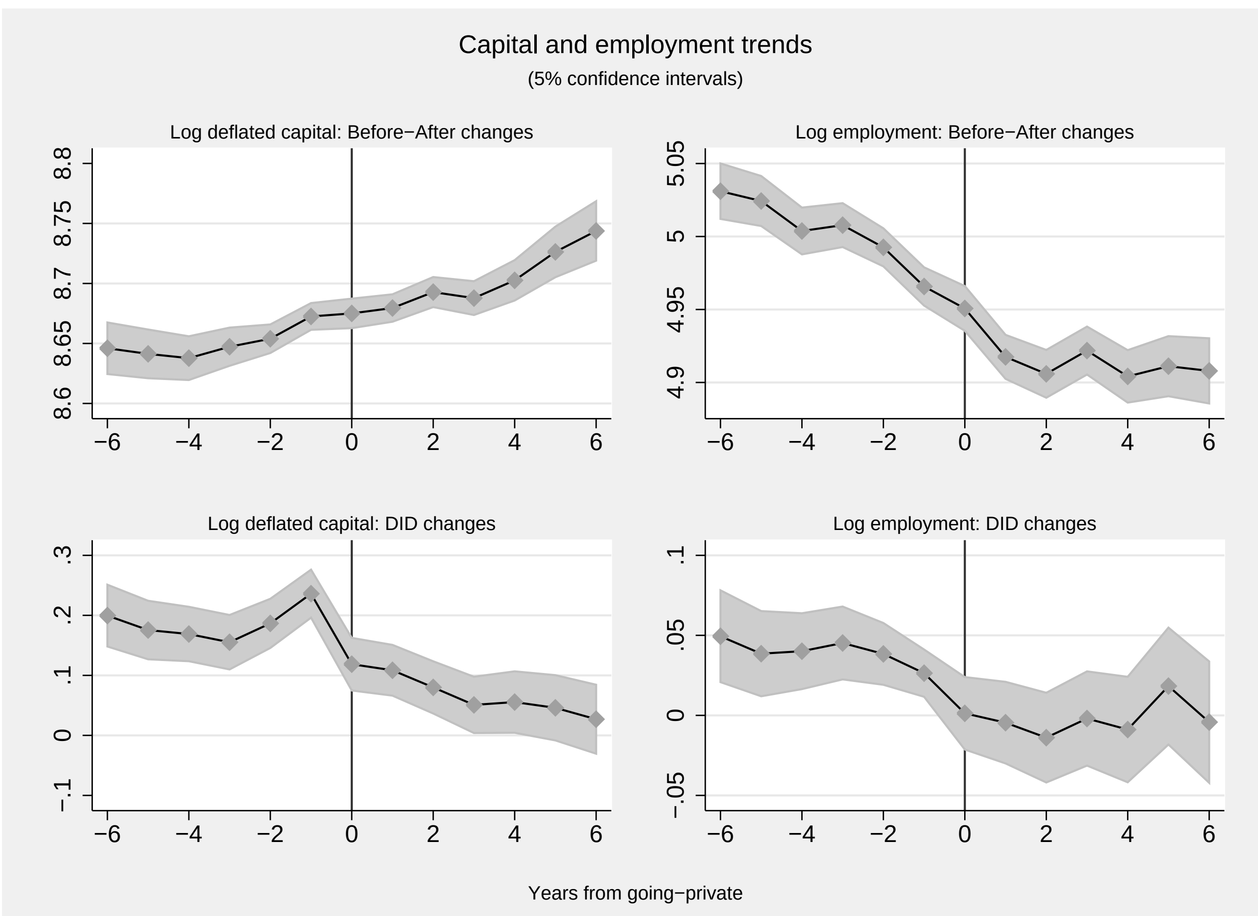


Figure 3: Before-after and difference-in-differences (DID) trends for capital and employment in event time

This figure displays the evolution of capital and employment measures in event time for the going private sample. The before-after figures use only the going private sample, and the DID figures show trends relative to an industry-age-initial size matched control group. The confidence intervals are based on standard errors clustered by plant in the Before-After figures, and by control group cells in the DID figures.



SUPPLEMENTARY APPENDIX TABLES

Table A.1: Changes in establishment profit measures around the "going-private" decision : Before-After and Difference-in-Differences Specifications

This table presents regression results for two profit-related measures. The first is gross profits, defined as sales less sum of (materials cost, energy costs, blue collar wage bill and white collar wage bill). The second is the ratio of gross profits to sales. Both measures are winsorized by 2% on both tails to minimize effects of outliers. In DID1, for each establishment in the going private sample, we include upto eight establishments (based on data availability) that are closest in size (employment) to the going private establishment from within the same 3-digit SIC industry, and belonging to the same age quartile as controls. In DID2, we include upto eight establishments within the same 3-digit SIC industry closest in propensity to go private (but whose owner firms did not go private), as control establishments. The data used to construct this sample is taken from the Census of Manufactures (CMF) and the Annual Survey of Manufactures (ASM). Dummy variables LR_PRE equals 1 for years -6 to -4 from going private and zero otherwise, SR_PRE equals 1 for years -3 to -1 from going private and zero otherwise, SR_POST equals 1 for years 0 to 3 from going private and zero otherwise, and LR_POST equals 1 for years 4 to 6 from going private and zero otherwise. P-values based on standard errors clustered by plant establishments (before-after), industry-size-age cells (DID1) and industry-propensity cells (DID2) are in parentheses.

	Gross Profit			Return on sales		
	Before-After	DID1	DID2	Before-After	DID1	DID2
	1a	1b	1c	1a	1b	1c
LR_PRE	9490.0	34.6	-1160	24.630	0.744	0.732
	(0.000)	(0.909)	(0.277)	(0.000)	(0.056)	(0.446)
SR_PRE	11274.0	195.5	-873.8	26.001	0.874	1.165
	(0.000)	(0.510)	(0.358)	(0.000)	(0.021)	(0.165)
SR_POST	12869.0	301.0	-1332	27.195	1.223	0.547
	(0.000)	(0.407)	(0.285)	(0.000)	(0.002)	(0.581)
LR_POST	14270.0	523.7	-627.4	25.999	0.898	1.620
	(0.000)	(0.324)	(0.706)	(0.000)	(0.090)	(0.223)
CHANGES RELATIVE TO SR_PRE						
SR_POST - SR_PRE	1595.0	105.5	-458.2	1.194	0.349	-0.618
	(0.000)	(0.712)	(0.651)	(0.000)	(0.377)	(0.538)
LR_POST - SR_PRE	2996.0	328.2	246.4	-0.002	0.024	0.455
	(0.000)	(0.490)	(0.875)	(0.996)	(0.967)	(0.749)
TEST FOR PRE-EXISTING TREND						
SR_PRE - LR_PRE	1784.0	160.9	286.2	1.371	0.130	0.433
	(0.000)	(0.537)	(0.743)	(0.000)	(0.734)	(0.664)
Fixed effects	Plant	Cell-year	Cell-year	Plant	Cell-year	Cell-year
Number of observations	28,518	185,909	55,358	28,518	185,909	55,358

Table A.2: Exit hazard after the "going-private" decision: Tests for differential targeting based on local income and property values

This table presents hazard model results of the duration to exit measures. In columns 1a, 1b and 1c, for each establishment in the going private sample, we include up to eight establishments (based on data availability) that are closest in size (employment) to the going private establishment from within the same 3-digit SIC industry, and belonging to the same age quartile as controls. In columns 2a, 2b and 2c, we include up to eight establishments within the same 3-digit SIC industry closest in propensity to go private (but whose owner firms did not go private) as controls. The data used to construct this sample is taken from the longitudinal business database (LBD). The data on county-level per capita income, per capita income growth and median (owner occupied) house value were obtained from the U.S. Census Bureau's American Factfinder website. P-values based on standard errors clustered by control group cells are in parentheses.

	1a	1b	1c	2a	2b	2c
Log employment	-0.183	-0.184	-0.182	-0.252	-0.253	-0.252
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age	-0.019	-0.019	-0.019	-0.011	-0.010	-0.010
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Going private dummy	0.068	0.117	0.149	0.024	0.017	0.010
	(0.157)	(0.017)	(0.000)	(0.735)	(0.805)	(0.816)
County per capita income	0.154			0.134		
	(0.000)			(0.000)		
County per capita income growth (5 yr)	0.000	0.390		0.000	0.340	
		(0.000)			(0.000)	
Median house value			0.012			0.012
			(0.000)			(0.000)
Going private dummy X Income	0.073			0.001		
	(0.004)			(0.977)		
Going private dummy X Income growth		0.169			0.020	
		(0.081)			(0.878)	
Going private dummy X House value			0.005			0.002
			(0.012)			(0.505)
Fixed effects		Industry, Year			Industry, Year	
Number of observations	33,704	33,704	33,704	10,295	10,295	10,295

Table A.3: Changes in establishment employment and payroll around the "going-private" decision for all (including non-manufacturing) establishments: Before-After and DID Specifications

This table presents regression results of each of the productivity measures of the sample of establishments of firms that went private. In DID1, for each establishment in the going private sample, we include upto 2 establishments (based on data availability) that are closest in size (employment) to the going private establishment from within the same 3-digit SIC industry, and belonging to the same age quartile as controls. The data used to construct this sample is taken from the Longitudinal Business Database. Dummy variables LR_PRE equals 1 for years -6 to -4 from going private and zero otherwise, SR_PRE equals 1 for years -3 to -1 from going private and zero otherwise, SR_POST equals 1 for years 0 to 3 from going private and zero otherwise, and LR_POST equals 1 for years 4 to 6 from going private and zero otherwise. P-values based on standard errors clustered by plant establishments (before-after) and industry-size-age cells (DID1) are in parentheses, except in column 1a of Panel B where the underlying standard errors are clustered by plant.

	Log employment		Log payroll	
	Before-After	DID1	Before-After	DID1
	1a	1b	2a	2b
LR_PRE	2.755	0.132	5.766	0.158
	(0.000)	(0.000)	(0.000)	(0.000)
SR_PRE	2.759	0.062	5.693	0.118
	(0.000)	(0.000)	(0.000)	(0.000)
SR_POST	2.738	0.047	5.625	0.096
	(0.000)	(0.000)	(0.000)	(0.000)
LR_POST	2.699	0.036	5.656	0.089
	(0.000)	(0.000)	(0.000)	(0.000)
CHANGES RELATIVE TO SR_PRE				
SR_POST - SR_PRE	-0.021	-0.016	-0.068	-0.022
	(0.000)	(0.000)	(0.000)	(0.000)
LR_POST - SR_PRE	-0.060	-0.027	-0.037	-0.029
	(0.000)	(0.000)	(0.000)	(0.000)
TEST FOR PRE-EXISTING TREND				
SR_PRE - LR_PRE	0.004	-0.070	-0.073	-0.040
	(0.026)	(0.000)	(0.000)	(0.000)
Fixed effects	Plant	Industry-size-age-year	Plant	Industry-size-age-year
Number of observations	832,772	1,961,406	832,772	1,961,406

Table A.4: Exit hazard and propensity after the "going-private" decision: Tests using data on all (manufacturing and non-manufacturing) establishments

This table presents hazard model results of the duration to exit. In all the analysis here, we include up to two establishments (based on data availability) that are closest in size (employment) to the going private establishment from within the same 3-digit SIC industry, and belonging to the same age quartile as controls. The data used to construct this sample is taken from the Longitudinal Business Database (LBD). P-values based on standard errors clustered by control group cells are in parentheses.

Panel A	Cox PH model	Exponential model
	(1)	(2)
Log employment	-0.175	-0.170
	(0.000)	(0.000)
Age	-0.024	-0.024
	(0.000)	(0.000)
Going private dummy	0.153	0.147
	(0.000)	(0.000)
Fixed effects	Industry, year	
Number of observations	203,541	203,541

Panel B	2-year exit dummy	
	Mean	Differenced mean 1
	1a	1b
SR_POST	0.226	0.012
	(0.000)	(0.000)
LR_POST	0.411	0.009
	(0.000)	(0.000)
Fixed effects	Plant	Industry-size-age-year
Number of observations	832,772	1,961,406