Version: March 2013

The Real Effects of Hedge Fund Activism: Productivity, Asset Allocation, and Product Market Competition

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Abstract

This paper studies the long-term effect of hedge fund activism on the productivity of target firms using plant-level information from the U.S. Census Bureau. A typical target firm improves its production efficiency by about 10% of a standard deviation within three years after the intervention, and this improvement is concentrated in industries with low product market concentration. By following plants that were sold post-intervention we also find that efficient capital redeployment is an important channel via which activists create value. Additional tests refute alternative explanations that attribute the improvement to self-cure, management's voluntary reform, industry consolidation shocks, and hedge funds' stock picking. The overall evidence suggests a real long-term effect of hedge fund intervention on target firms' fundamentals.

JEL Classification: G12, G23, G34
Keywords: Hedge fund activism, Governance, Productivity

The authors have benefited from comments from and discussions with Christa Bouwman, Sandy Klasa, April Klein, Dalida Kadyrzhanova, Vikram Nanda, Michael Raith, Adriano Rampini, and seminar and conference participants at the University of Amsterdam, Boston College, Duke, Drexel, Emory, Erasmus, Fordham, Fudan, HEC Paris, INSEAD, Interdisciplinary Center at Herzlyia, London Business School, Oregon, Rotterdam School of Management, Rutgers, Tel Aviv University, Temple, University of Washington, Yale, the Annual Corporate Governance Conference at Drexel University, Western Finance Association Annual Meeting, the International Conference on Corporate Governance at Tsinghua University, the Annual Financial Intermediation Society Conference, Jackson Hole Finance Conference, the SFS Finance Cavalcade, and the Annual Conference on Corporate Finance at Washington University in St Louis. We also thank Jin Xu and Yinghua Li for help with data collection at an early stage of the paper and Bryan Oh for excellent research assistance. Alon Brav can be reached at phone: (919) 660-2908, email: brav@duke.edu. Wei Jiang can be reached at phone: (212) 854-9002, email: wj2006@columbia.edu. Hyunseob Kim can be reached at phone: (607) 255-8335, email: hk722@cornell.edu. Kim gratefully acknowledges financial support from the Kwanjeong Educational Foundation. Any opinions and conclusions expressed herein are those of the author(s) and do not necessarily represent the views of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential information is disclosed.

1. Introduction

A growing literature on hedge fund activism identifies a significant stock price reaction (5-10 percent abnormal return) for targeted companies with the announcement of activism.[1] The range of short-term price impact is highly consistent across different studies and different markets. A subset of this literature also documents significant operating performance improvement in the period following the hedge fund intervention. We validate and summarize this pattern using return on assets (ROA) as the performance measure with our sample of close to 2,000 activism events in the U.S. from 1994 to 2007. Figure 1 plots the target firms' average ROA in excess of that of control group (in the same three-digit SIC industry and year, and adjust for firm size and age) from three years before to three years after the public announcement of activism. There is a clear "V" shape centered on the year of targeting, and the level in the third year post targeting is significantly higher than that during the year of intervention or the year before.

While the evidence regarding both stock returns and firm operating performance speaks favorably for the impact of hedge funds activism several important related questions have not been addressed to date. First, the existing findings have not explicitly identified the underlying sources of value creation by hedge fund activists. As a result, little is known about the precise mechanism via which activists are able to improve efficiency and increase shareholder value. In fact, opponents of hedge fund activism often blame hedge fund activists as "short-term focused" and "financial engineering oriented," denying any meaningful real and long-term impact.[2] Moreover, performance measures at the firm level such as ROA do not reveal the underlying channels of improvement, that is, they cannot isolate production efficiency gains of existing assets from gains due to capital reallocation such as divestiture of underperforming assets and refocusing.

[1] See Brav, Jiang, Partnoy, and Thomas (2008a), Klein and Zur (2009), Clifford (2008), Greenwood and Schor (2009) for U.S. companies; and Becht, Franks, Mayer, and Rossi (2009), Becht, Franks, and Grant (2010) for non-U.S. markets.
[2] See, for example, "Democracy for investors has its limits," *International Herald Tribune*, February 27, 2013.

Second, current research, which is based on databases such as Compustat that cover only public companies at the firm level, cannot address the potential for survivorship bias in the post-intervention period. Within two years of activists' intervention close to 25% of companies targeted by activists disappear from the Compustat database (either acquired or delisted), 60% higher than the normal attrition rate of the Compustat universe. As a result, researchers have not been able to assess the post-targeting performance based on an unbiased sample or trace out the performance of assets subsequent to ownership changes.

The limitation of previous research is due both to the novelty of the topic, and hence the lack of a large sample of post-intervention data, and the reliance on firm-level data of public companies. This paper addresses these important impediments by exploring the longitudinal data of manufacturing establishments (i.e., plants) from the Census of Manufacturers (CMF) and the Annual Survey of Manufacturers (ASM) databases maintained by the U.S. Census Bureau. By matching these plant observations to hedge fund activism events from 1994 to 2007, we examine the dynamics of production efficiency for firms targeted by activists, measured by total factor productivity (TFP), and assess the relative importance of the gains in efficiency among assets in place and those due to reallocation of target firms' plants.

The following key findings on the long-term real effect of hedge fund activism arise from our analyses. First, the productivity of plants owned by firms targeted by activists shows a "V"-shaped pattern around the year of the intervention, echoing the dynamics of ROA shown in Figure 1. Three years prior to the intervention, the productivity of target firms' plants is significantly higher than their control plants with similar size and age in a given industry and year. Target firms' productivity deteriorates thereafter to a level similar to that of the control plants when intervention occurs, but then rebounds to the pre-activism level three years post-activism. Second, we find that the improvement in production efficiency associated with hedge fund activism is more pronounced in industries with low product market concentration — presumably in more competitive industries.

Third, one channel through which activists create value is by facilitating efficient reallocation of corporate assets. Focusing on the subsample of plants that were sold after hedge fund intervention, we find that these plants exhibit lower productivity compared to plants in the control sample prior to the sale but then experience a greater improvement in the hands of the new owners. Moreover, the improvement is significantly greater than that of plants that are sold without the involvement of hedge funds. This evidence suggests that the hedge funds' presence is necessary for the matching of plants to new owners who can operate the underperforming plants more efficiently. An industry with more players (or potential buyers and sellers) offers better chances for a good match, justifying the greater improvement of target firms in industries with low product market concentration.

The combined evidence so far refutes the assertion that the effects of hedge fund activism are purely financial (such as extracting payouts to shareholders through leverage) as argued by some policy makers and the popular press. Moreover, the plant observations in our Census data survive changes in ownership (i.e., plant sales) or firm delisting from the exchanges and are thus not subject to a potential selection due to both asset sale and firm attrition. Hence, our estimates of higher plant productivity for the targets of hedge fund activism are more accurate than performance analyses based on the Compustat data.

An important question remains: Given the nonrandom selection of target firms by hedge funds, to what extent are the effects that we document causal? Some unobservable and omitted plant or firm characteristics may be correlated with both the decision to intervene and the targets' future performance. It may also be argued that activists are able to anticipate significant industry-level shocks to the structure of the product market and thus anticipate the implications of such changes on target firms. The observed improvement in target firm's performance post-intervention may therefore just reflect the consequences of these shocks independent of the presence of the activists.

We believe that these concerns are justified although it is important to emphasize that the growing literature on activism has shown that many of the changes associated with hedge fund activism are unlikely to have occurred absent activists' actions (see the review in Section 5.1). We therefore conduct additional tests to identify the effects from hedge fund intervention, vis-à-vis several counterfactuals including self-cure due to deterioration in performance, voluntary reform by management, and hedge funds' ability to pick winners in the process of industry consolidation. Our tests refute all the alternative explanations. To address the ultimate controversy between hedge funds' stock picking and intervention, we resort to a legal feature in ownership disclosure as the source of identification by comparing the performance of firms for whom hedge funds switched from a 13G to 13D filing,[3] which indicates no change in ownership but a change from a passive to an activist stance. The 199 such cases in the sample provide an ideal setting to test the incremental effect of intervention over stock picking. The significant performance improvement of these firms after hedge funds' decision to switch their filing combined with results from the other identification tests, suggests that the performance improvement among target firms would not have occurred had the hedge funds been mere passive investors.

The paper proceeds as follows. Section 2 presents the construction of the data and sample used in the analysis. In particular, we describe how we form our measure of production efficiency and match the Census data to the hedge fund activism event data. Section 3 presents the main results on the real effect of activism on the productivity of plants owned by the target firms. Another focus of this section is the interactive effect of product market competition with corporate governance in the form of hedge fund activism. In Section 4 we document the extent to which hedge fund activists create value through efficient reallocation of target firms' assets by examining the dynamics of productivity of plants sold post-activism. This section also examines the extent to which the estimate of the real effect of activism based on Compustat is biased due to sample attrition from the database. Section 5 runs a battery of identification tests. We conclude in Section 6.

[3] A shareholder who acquires more than 5% beneficial ownership is required to disclose in the Schedule 13D within 10 days of crossing 5% if it intends to influence control. If the investment intention is purely passive, the disclosure requirement is a less stringent 13G form. Section 5.5 provides a more detailed discussion of these filing requirements with the SEC.

2. Data and Key Variables

2.1 Data Sources and Sample Construction

2.1.1 Plant-level data

We obtain data on manufacturing establishments (i.e., plants) from two types of databases maintained by the U.S. Census Bureau. The first data source includes the Census of Manufacturers (CMF) and the Annual Survey of Manufacturers (ASM) which provide plant-level information to compute our measures of productivity and product market competition. The CMF covers all manufacturing plants in the U.S. for years ending '2' or '7' (the "Census years"), resulting in roughly 300,000 plants in each census. The ASM covers about 50,000 plants for the "non-Census years." Plants with more than 250 employees are always included in the ASM, while those with fewer employees are sampled randomly with the probability increasing in size. Both sources provide operating information at the plant level including total value of shipments, capital stock and investment, labor hours, and material and energy costs. Even though it is called a "Survey," reporting is mandatory if selected and misreporting is subject to legal penalty and fines.

The CMF and ASM data have a few critical advantages over standard firm-level databases of public firms such as Compustat. First, since these databases cover plants owned by private firms as well as public firms, they allow us to track the performance of target firms even if they disappear from Compustat due to acquisitions or delistings. Since such events tend to occur more often among firms targeted by hedge fund activists this feature of the Census data helps us minimize the potential for attrition bias in estimating the effect of activism. Second, accurate estimation of productivity as well as industry benchmarking requires a reasonable uniformity of production functions, a property that applies to plants well but not necessarily at the firm level. Thus, the CMF and ASM data allows us to identify the efficiency gain in the production process associated with activism which is beyond the reach of analyses relying on databases of publicly traded companies.

The second data source is the Longitudinal Business Database (LBD) from which we obtain unique longitudinal identifiers for plants and information on ownership changes. The LBD tracks more than five million (both manufacturing and non-manufacturing) establishments every year, essentially covering the entire U.S. economy. The variables available in the database include the number of employees, annual payroll, industry classifications, geographical location, and ownership status.

We focus on manufacturing plant-year observations in the CMF and ASM from 1990 to 2009 (the last year of the data coverage). The starting year is determined by the sample period of the hedge fund activism database (1994-2007) and the fact that we examine plant performance beginning three years prior to the intervention. We exclude 'miscellaneous manufacturing industries' (i.e., three-digit SIC=399) as this category does not represent a group of plants that share a common production function. We also require each plant observation to have the variables necessary to estimate total factor productivity (TFP), including SIC codes,[4] total value of shipments, production worker equivalent hours, beginning-year capital stock, and material and energy costs. Appendix A provides details on the construction of these variables, including adjustments for changes in prices of inputs and outputs, and depreciation. This sample selection procedure yields 787,758 plant-years in our sample. Henceforth, we will refer to the collection of sources described in this section the "Census data."

2.1.2. Hedge fund activism data

The database of hedge fund activism events, covering the period of 1994-2007, is an extended sample used in Brav, Jiang, and Kim (2010) based on the same sampling criteria. These events are identified mainly through Schedule 13D fillings to the SEC in which hedge funds disclose ownership exceeding 5% with intention to influence corporate control, supplemented by news searches to identify activist events targeted at mid- to large-cap companies (above $1 billion) with ownership stake between 2% and 5%. We collect detailed information on key aspects of each event from the initial and amended 13D filings via the SEC's EDGAR system and by news searches.

[4] The ASM and CMF provide SIC codes until 2002 and provide NAICS codes only thereafter. We follow Giroud (2011) and impute the SIC codes after 2002.

The target firm-year pairs are then matched to (potentially multiple) plant-year observations in the Census data using a bridge file created by the Census Bureau. Panel A of Table 1 shows that for 368 (out of a total of 1,987) activism events from 1994 to 2007 we are able to find at least one matched plant-year in the Census data with adequate information for estimating TFP, resulting in 14,923 total number of plant-year observations. This match rate is somewhat lower than those typically reported in previous research due to two factors. First, close to 70% of the hedge fund activism targets in our sample are in non-manufacturing sectors. In fact, the match rate is much higher at 44% for activism targets in the manufacturing sector. Second, activism targets tend to be smaller than sample firms examined in previous research using the Census data (e.g., LBO and M&A targets).[5]

[Insert Table 1 here.]

Both the full sample of events and those matched to the Census data are more concentrated in the 2000s period compared to the 1990s, reflecting the rise of activist intervention as an investment strategy among hedge funds from the early 2000s. Out of 368 activist events matched to the Census data, 245 took place in or after year 2000. The number of plan-year observations maintains a similar proportion.

Given that not all targets of hedge fund activists are matched to the Census files, it is necessary to examine if the matched activism events are representative of the entire sample to ensure that our findings have general implications beyond the manufacturing industry. The distributions of stated objectives and success rates (including partial successes) of the full sample and matched sample, reported in Table 1 Panel B, indicate that the matched events appear to be nearly identical to the full sample of events along these two important dimensions. For example, the success rates (i.e., the proportion of events in which hedge funds at least partially attained their stated goals) for both samples are roughly two-thirds.

[5] For comparison, Lichtenberg and Siegel (1990) report a matching rate of about 50% for their LBO target firms with the Census data. Note that target firms classified as "non-manufacturing" based on the SIC code from Compustat might own manufacturing establishments, and thus could also be matched to the Census data.

2.2 Key Variables

2.2.1 Productivity

Our main measure of plant performance is total factor productivity (TFP), which is defined as the difference between the actual and predicted output given inputs. In order to compute the predicted output for each plant, we follow the literature (e.g., Lichtenberg and Siegel (1990), Lichtenberg (1992), Schoar (2002), Bertrand and Mullainathan (2003), and Giroud (2011)) and estimate a log-linear Cobb-Douglas production function using Ordinary Least Squares (OLS) regressions by three-digit SIC industry and year:

$$\ln(Y_{ijt}) = \alpha_{jt} + \beta_{jt}^{K} \ln(K_{ijt}) + \beta_{jt}^{L} \ln(L_{ijt}) + \beta_{jt}^{M} \ln(M_{ijt}) + \varepsilon_{ijt}, \tag{1}$$

where α_{jt} is industry-year specific intercept, Y_{ijt} is output, K_{ijt} is net capital stock, L_{ijt} is labor input, M_{ijt} represents material costs. ε_{ijt} is the residual and the estimate of the TFP for plant *i*, in industry *j* in year *t*. The coefficients in (1) carry *(j,t)* subscripts, which allows for factor intensities that are industry-year specific. In addition, given that TFP is the estimated residual of the industry-year specific regressions, we can interpret TFP of a given plant as a relative productivity rank of the plant within a given industry and year. Finally, following Maksimovic, Phillips, and Yang (2010), we "standardize" the TFP measure from (1) by dividing it by its cross-sectional standard deviation for a given industry-year. Essentially, this adjustment accounts for differences in the precision of TFP estimates among industry-years. As expected, using the non-standardized measure yields qualitatively similar but noisier results.[6]

Though (1) is the common method adopted in the finance literature to analyze productivity at the micro-unit level, it is subject to the criticism that it is a regression residual and could therefore be contaminated if ε_{ijt} in (1) is positively correlated with one or more inputs. The current state-of-the-art remedy to this issue that has been proposed by Levinsohn and Petrin (2003). It controls for unobserved shocks in productivity using an observable intermediate input (in this case, materials) based on the

[6] Lichtenberg and Siegel (1990) point out that the measure of TFP could reflect pricing power as well as efficiency in less-than-perfect competitive markets. As we show later, the gains in efficiency associated with activism are actually driven by target plants in less concentrated—presumably more competitive—industries where the measure for TFP is more accurate.

assumption that the intermediate inputs' demand function is monotonic in productivity as long as the market for the input is more or less competitive. The Levinsohn and Petrin (2003; LP) method, though econometrically more justifiable, comes with cost. It requires a long panel of plant-year observations to estimate production functions in equation (1) because it relies on estimated within-plant persistent productivity shocks. For reliable estimation of the parameters, we use 20 years of data for each industry-year panel. As a result, the implementation of the LP method requires much more computing power and time relative to OLS. We apply the Levinsohn and Petrin (2003) method as a sensitivity check.

2.2.2 Product market concentration

Our main measure of the degree of product market concentration is the Herfindahl-Hirschman Index (HHI). Specifically, we compute the HHI using the Census data as follows:[7]

$$HHI_{jt} = \sum_{f=1}^{N_j} S_{fjt}^2, \tag{2}$$

where HHI_{jt} is the Herfindahl-Hirschman index for industry *j* in year *t*, and S^2_{fjt} is the squared market share of firm *f* in industry *j* in year *t*. Market shares are measured using total value of shipments aggregated at the firm level (i.e., sales), and industry is defined at the three-digit SIC level.

It is worth noting that the HHI measure constructed in our study includes both public and private firms. At the industry level, the correlation between the HHI measure herein and that constructed using the Compustat data alone is 0.17. The modest level of correlation indicates that using only public firm information does not capture the full reality of product market concentration.

2.2.3 Descriptive statistics

Table 2 reports the descriptive statistics comparing the characteristics of target plants during the year of intervention with all Census plant-year observations used in our analyses and plant-year observations belonging to public firms (from Compustat).

[7] The CMF has a more comprehensive coverage but the ASM provides more consistent time series. We compute the HHI using the CMF data for comprehensiveness, and use a given Census year's HHI for two years before to two years after the Census year (Ali, Klasa, and Yeung (2009)). Our results are qualitatively robust if we construct the HHI using the ASM instead, and impute the Census year's value of the HHI for the latest non-Census year's HHI.

[Insert Table 2 here.]

On average, plants owned by target firms from four years before to three years after a hedge fund's intervention have a total value of shipments (TVS) of $78m and real net capital stock of $41m (in 2005 dollars), which are slightly larger than the respective values for the full Census sample but considerably smaller than the average of plants affiliated with publically traded firms. Since our main measure of production efficiency, standardized TFP, is constructed as the residual of a production function regression scaled by its standard deviation, it has a mean of zero and a standard deviation close to 1.00 by construction for the full sample.[8] In comparison, target plants have a positive mean TFP indicating that they are more productive than the average plant in the full sample. Similarly, target plants show a higher operating profit margin than the full sample of plants, on average (but not much different from the average of plants affiliated with public firms). Finally, the industry concentration as measured by the HHI using the Census data for targeted plans is identical to that of plants of all public firms, indicating that hedge fund activists do not have a clear preference for more or less concentrated industries.

Next, we compare target firms in the latest year prior to intervention matched to the Census sample with all target firms and then all public firms (the Compustat universe) for the 1994-2007 period. The summary statistics are reported in Table 3. First, Census-matched target firms are similar to all target firms in terms of size (measured by market equity and book assets) and leverage. However, targets matched with the Census data tend to hold less cash, pay more dividends, have lower valuation ratios (i.e., q), lower sales growth rates, spend less on R&D, and are more profitable than the full sample of activism target firms. These characteristics suggest that firms that are matched to the Census databases generally have worse growth opportunities but enjoy better cash flows. These differences are mostly due to the fact that a great majority of the Census-matched firms are concentrated in the manufacturing sector whereas less than 30% of all targets are in the sector. The comparison between target firms and the full Compustat Universe are consistent with the findings in Brav et al. (2008).

[8] Due to the winsorization at 1% tails the standard deviation is slightly less than 1.00.

[Insert Table 3 here.]

3. Productivity and Product Market Concentration

3.1 Plant and Firm Productivity before and after Intervention

As a first step, we examine the impact of hedge fund activism on target firms' productivity at the plant level. Our main dependent variable is plant-level total factor productivity (TFP) computed as the estimated residual from a log-linear Cobb-Douglas production function regression at the SIC three-digit industry-year level as in equation (1).[9] Our TFP measure can be understood as the relative productivity rank of a plant within its industry-year. By construction, the TFP of an industry in a given year, averaged over all plants, is zero. The resulting regression specification is as follows:

$$y_{it} = \sum_{k=-3}^{3} \gamma_k d_{it}[t+k] + \lambda Control_{it} + \alpha_j + \alpha_t + \varepsilon_{it}. \tag{3}$$

The key independent variables in equation (3) are a set of year-plant dummy variables, *d[t-3],…, d[t+3],* corresponding to plan-year observations from three years before to three years after a firm, to which the plant belongs to, is targeted by a hedge fund activist. That is, we require that the plant is owned by the target in year $t+k$, $0 \leq |k| \leq 3$ for *d[t+k]* to be coded one. Hence, this specification analyzes the dynamics of performance of plants that remain in the hands of the target companies before and after hedge fund targeting. The effect of ownership changes on productivity is an important but separate question which we examine in Section 4.

Control variables include segment and firm size, measured by the log number of plants in a given industry segment of a given firm and the log number of all plants of a given firm, respectively. Plant age is defined as the number of years since a plant's first appearance in the CMF or ASM database. The starting year is censored in 1972 when the coverage of the Census databases begins. This set of control variables is standard among research that analyzes plant-level performance using the CMF and ASM data (e.g., Schoar (2002), Giroud (2011)). Finally the estimation takes into account firm/plant and year fixed

[9] Our main results are robust to a translog functional form, a less popular measure used in the literature.

effects (α_j and α_t). Industry fixed effects are not appropriate given that the dependent variable TFP is already an industry-level residual. When a regression does not include a firm/plant fix effect (which subsumes industry effects) we demean the control variables at the industry-year level so that they are commensurate with the dependent variable.

Table 4 reports results from a variety of specifications to ensure robustness. Columns (1) through (3) adopt a combination of firm/plant fixed effects with the dependent variable being the normalized TFP. The dependent variable in column (4) is the non-standardized TFP to validate that our results are not driven by normalization of TFP scales. In column (5), the TFP measure is obtained using the Levinshon and Petrin (2003) GMM estimation to address the issue that the residuals and the inputs are potentially correlated in equation (1). Finally, column (6) reports results at the firm level by aggregating plants belonging to the same firms.

[Insert Table 4 here.]

All six specifications in the table demonstrate a "V"-shaped dynamics of performance for plants owned by firms targeted by activists. That is, the productivity of targets of hedge fund activism experiences a deterioration prior to intervention and then rebounds steadily afterwards. Formal tests, reported at the bottom of Table 4, indicate that the improvement in productivity from the year of targeting to three years afterwards is statistically significant at the 5% level throughout all specifications. And in half of specifications the improvement is significant beginning in year *t+2*. The economic magnitude of the improvement in plant-level TFP associated with activism is sizeable: a typical target plant experiences an increase in TFP of 7.7%-10.8% of the standard deviation from years *t* to *t+3* using the first three specifications where the dependent variable is constructed to be of unit standard deviation. A formal test of the significance of the "V" shape, which amounts to an *F* test for the joint inequality of coefficients on *d[t]* and *d[t-3]*, and that of coefficients on *d[t+3]* and *d[t]*, rejects joint equality at the 5% (10%) level for four (two) specifications.

Interestingly, both the pattern and the magnitude of TFP dynamics around hedge fund intervention echo the findings of improved ROA at target firms after the intervention, as shown in Figure 1. The three-year ROA improvement from the trough in year *t* is about 3 percentage points, which is about 10% of the standard deviation of ROA (with the same winsorization at the 1% extremes as we conducted on the raw TFP estimates) during our sample period.

In addition, the positive coefficients on the targeting dummies (in the specifications without firm/plant fixed effects) suggest that plants owned by target firms are generally more productive than their industry-size-age matched peers. This evidence is consistent with Brav et al.'s (2008a) finding that hedge funds tend to be target mature firms with relatively strong business fundamentals but may be subject to agency problems of free cash flows. These firms experience deterioration due to bad governance or mismanagement such as poor adaptation to market changes. The deterioration triggers activist targeting, and is more or less reversed within the 2-3 year period post targeting. The dynamics of plant-level productivity is hard evidence for changes in the fundamental value of firms associated with hedge fund targeting. In addition, it refutes the assertion that the positive returns to hedge fund activism can be attributed solely to financial gains (such as extracting payouts to shareholders through leverage).[10]

3.2. Interaction with Product Market Concentration

A growing body of recent work highlights the interactive effects of product market concentration (often viewed as a proxy for competition) and corporate governance. Bauer, Braun, and Viehs (2010) show that the lack of industry competition in combination with managerial entrenchment increases the likelihood of activist shareholder proposals. Kadyrzhanova and Rhodes-Kropf's (2010) theoretical model concludes that industry concentration affects the trade-offs of governance for shareholders. Most related to our work are papers that assess the substitutability of product market competition for corporate governance. Giroud and Mueller (2010, 2011) show that anti-takeover laws have a more negative impact on shareholder value in non-competitive industries; accordingly, takeover pressure and product market

[10] See, for example, "Hedge Fund Activists Set for Comeback," *Financial Times,* December 8, 2009.

competition seem to work as substitutes. Chhaochharia, Grinstein, Grullon, and Michaely (2012) find another form of substitution documenting that firms in more concentrated industries benefit more from the Sarbanes Oxley Law in 2003 which was designed to enforce stricter internal governance.

Hedge fund activism is distinct from the other two forms of governance discussed above in that it is a non-control driven (instead of takeover oriented) and market based (instead of internal) form of governance. A priori its relation to product market competition is unclear. It is worth noting that the theory in this context is also ambiguous. While competition requires high effort to avert failure (Schmidt, 1997) and leads to strong managerial incentives because outcomes are more informative (Hart, 2003), it also reduces profits which makes effort less attractive (Schmidt, 1997; Raith, 2003). Moreover, these theoretical papers predict the relation between competition and incentives but do not offer a direct prediction on the interactive effects of competition and governance (shareholder monitoring in our context) on performance.

To address the question empirically, we conduct regression analysis in the form of equation (3) but interact all regressors with *High_HHI* and *Low_HHI,* dummy variables for the SIC three-digit industries being in the top and bottom quartiles of the Herfindahl-Hirschman Index (*HHI*) as described in equation (2), a direct measure for product market concentration used by a large literature as a proxy for product market competition.

$$\begin{aligned} y_{it} = {} & High\_HHI \cdot \left(\sum\nolimits_{k=-3}^{3} \gamma_k^{High\,HHI} d_{it}[t+k] + \lambda^{High\,HHI} Control_{it}\right) + \\ & Low\_HHI \cdot \left(\sum\nolimits_{k=-3}^{3} \gamma_k^{Low\,HHI} d_{it}[t+k] + \lambda^{Low\,HHI} Control_{it}\right) + \alpha_t + \varepsilon_{it} \end{aligned} \tag{4}$$

The two sets of coefficients $\{\gamma_k^{High\,HHI}, \lambda^{High\,HHI}\}$, $\{\gamma_k^{Low\,HHI}, \lambda^{Low\,HHI}\}$ are reported in Table 5.[11]

[Insert Table 5 here.]

The key message from Table 5 is that the post-intervention improvement in TFP is more pronounced among less concentrated industries. The magnitude of a change from year *t* to year *t*+3 is 2.8

[11] This regression is equivalent to running regression (3) separately on the top and bottom HHI quartile subsamples. We adopted the specification in (4) due to restrictions on data disclosure from the Census Bureau. The same regression specification is adopted for all subsample analyses in the rest of the paper.

times larger in the least concentrated industries compared to the most concentrated ones. If low concentration is related to more competition, this relation suggests that product market competition and outside shareholder monitoring are potential complements.

One natural question that arises from this result is why do hedge fund activists target firms in concentrated industries given that the effect of activism on productivity appears to generate insignificant efficiency gains? In fact, and perhaps surprisingly, we find that hedge funds target firms in more or less concentrated industries with roughly equal probabilities. This relation stands using Compustat as well as the Census data (see also Giroud and Mueller (2011) for similar evidence). One plausible explanation for this result is that hedge fund activists create value in different ways in dispersed versus concentrated industries. In particular, Raith's (2003) theoretical model shows that the benefit of improved efficiency (due to better governance or incentives) is higher in competitive industries in which the firm-level demand function is relatively elastic, and thus a marginal improvement in efficiency leads to a large increase in output and profits ("business stealing effect"). Therefore, activists might want to focus on improving productivity in these industries. In concentrated industries, however, the benefit of productivity gains is not as large due to relatively inelastic demand curves, and so activist hedge funds can instead focus on allocational, financial, and governance-related improvements.

We provide two pieces of evidence that support this hypothesis. First, output expands in competitive industries but shrinks in concentrated industries post hedge fund intervention, consistent with the "business stealing" story in competitive industries. Controlling for industry and year fixed effects, we find that output expands by about 13.6% among plants in industries whose HHI is in the lowest quartile; in contrast, output shrinks by roughly 3.1% for targeted plants in industries in the top HHI quartile. The difference is not significant. The same pattern is observed for all inputs: labor, capital, and materials all expand (shrink) in how (high) HHI industries.

Second, firm-level data from Compustat reveals that hedge funds are more likely to focus on fixing the free cash flow problems in concentrated industries, where firms tend to be more profitable (even though less productive). Table 6 shows that, in concentrated industries, hedge fund activism is associated with significant increases in leverage, dividend payout, and CEO turnover rates, and a significant decrease in capital expenditure post intervention compared to pre-targeting levels. All these changes support the hypothesis that activists attempt to correct agency problems associated with free cash flows and entrenched management, and are fully consistent with the disciplinary effect of proxy contests documented by Fos (2012). Interestingly, the same effect is largely absent in non-concentrated industries where activists are more effective at improving real efficiency. This contrast supports the view that activists optimally focus on other aspects of target firms than production efficiency, such as capital structure and corporate governance, in concentrated industries.

Our findings also highlight the difference between hedge fund activism, a non-control driven form of external (or market-based) governance and two other forms of governance: control driven external governance (i.e., takeovers, analyzed by Giroud and Mueller (2010)) and internal governance (through boards and compliance with regulations, studied by Chhaochharia, Grinstein, Grullon, and Michaely (2012)). Hedge fund activism interacts with product market competition in ways that are critically different from the work of alternative forms for the following reasons. First, takeover defenses (which underlie common governance measures) do not shield entrenched management from hedge fund activism because activists typically aim for strictly minority ownership. The inter-quartile range of ownership is 5.3% to 8.8%, and in 95% of the cases the ownership stake is below 20%. Even when hedge fund activism escalates to proxy contests, activists tend to seek a short slate of board representation with rare exceptions. As a result, the most powerful takeover defenses such as poison pills and staggered boards are nonbinding constraints for activists. In fact, firms with more of these defenses stand a significantly higher chance of being targeted by hedge fund activists (Brav, Jiang, and Kim, 2010).

Second, hedge fund activism is also distinct from internal monitoring which laws like Sarbanes-Oxley were designed to promote. Hedge fund activists seek to invest in underperforming firms and hope to profit from the improvement which is different from activism by traditional institutional investors (e.g., pension funds) whose aim is to limit the damage to their portfolio firms that turn out to underperform. By being "offensive" rather than "defensive" activists, hedge funds accumulate the critical mass of their stakes within a short period of time, often within a quarter (Collin-Dufresne and Fos (2012), Gantchev and Jotikasthira (2012)). As a result, hedge fund activists monitor and influence firm decisions as outsiders, and their job is made easier in industries where a target firm has many peers to compare performance to and to share best practices with. The next section further shows that capital reallocation is an important way for activists to add value, and the strategy works better when there are more potential buyers and sellers of similar assets.

4. Capital Reallocation and Attrition Analyses

4.1. Gains Due to Reallocation of Assets: New Insights from the Census Data

To the extent that hedge fund activists help enhance the production efficiency of the targeted firms an equally important question is whether such improvements are accomplished through improving the efficiency of assets in place or through capital reallocation, or both. In fact, efficient redeployment of capital is a commonly stated goal of activist hedge funds. In addition to about 20% of the events in which hedge funds explicitly demand sales of the entire target company, in another 15% of the events the activists push for the divestiture of under-performing or non-core assets in order to strengthen and refocus the companies' on their core line of business. The case of Pershing Square engagement with Fortune Brands, described in Appendix B, also points to capital reallocation as an important mechanism for the value added by the activist hedge funds.

Prior literature has offered some indirect evidence on the extent of the gain from capital reallocation. For example, Brav et al. (2008a) and Greenwood and Schor (2009) show that announcement returns of hedge fund activism are largest among events in which the stated goal is to push for the sale of the target. The scope of these previous findings, however, has been limited by data from CRSP/Compustat since it is impossible to isolate the role of asset reallocation. First, performance measures computed using firm-level data (such as ROA) do not separate organic improvement (i.e., productivity gains of existing assets) from re-allocational gains (i.e., due to acquisition/disposition of better/worse performing assets). The Census data, which is recorded at the plant level and hence survives ownership changes and firm delisting, allows us to separate the two effects by tracing out the performance of plants that change ownership post targeting (i.e., are spun off).

Second, a Compustat firm will drop out of the database if it is acquired by another company (public or private), or is delisted (i.e., going private). Within two years after being targeted by hedge funds, 25.5% of the targets in our sample cease to be covered by Compustat, a rate that almost doubles the average attrition rate of a typical Compustat firm. Therefore, addressing the potential delisting bias is challenging, particularly given that the direction and magnitude of the bias is a priori unclear. Firm delisting is usually associated with negative reasons (Shumway (1997)). Accordingly, analyses based on the surviving sample tend to carry a positive bias. However, such an intuition might not apply to the hedge fund activism target firms because attrition from the sample may actually represent a successful outcome for the following reasons. First, targeted companies tend to have stronger fundamentals (higher productivity, ROA, and liquidity, as shown by Brav et al. (2008a) and Table 4 of this paper), and hence the subsequent attrition is less likely due to distress compared to firms delisted without the intervention of hedge fund activists. Moreover, the "sale of the company" objective category experiences the highest attrition rate (31.0%), where the ex post sale of a target firm reflects a successful execution of the stated goal of the hedge fund. Indeed, 70% of the target firms that disappear from Compustat within two years post intervention are acquired. Using analyst coverage and trading liquidity as instruments, Brav, Jiang, and Kim (2010) uncover a negative survivorship bias due to delisting from Compustat. That is, firms that will experience greater improvement in performance post intervention are also more likely to disappear from the Compustat database conditional on observable characteristics.

The Census data allows us to pin down the direction and magnitude of the attrition bias by following targeted plants regardless of the listing status of the firms they are affiliated with. The analyses that follow provide direct evidence consistent with a negative survivorship bias. That is, plants belonging to firms that were delisted from Compustat post targeting experience greater productivity gains than those that remain in the database.

4.2 Ownership Change of Target Firms' Plants

By focusing on plants that belong to targeted companies prior to activism but were later spun off, we attempt to identify gains in efficiency via asset redeployment facilitated by the activists. In our sample, about 20% of the plants of the targeted companies were sold between the year of intervention and the third year post intervention. These numbers validate the stated goals of hedge funds in many activism events and generalize the anecdotes regarding hedge fund strategies. Appendix B of this paper provides the case of Pershing Square and its role in the spinning off of Fortune Brands' peripheral segments. Another example can be found in Trian Fund Management's engagement with Wendy's/Arby's starting in 2008. The hedge fund pushed the company to jettison the underperforming sandwich chain and to revitalize Wendy's core menu to have a stronger position against rivals McDonald's and Burger King. Interestingly, following these developments several other fast-food companies openly announced that they were seeking to reposition themselves. Notably Yum Brands Inc. sought to spin off its Long John Silver's and A&W All-American Food Restaurants in order to focus on Pizza Hut, Taco Bell and KFC.

To formally assess the impact of asset reallocation, we first analyze the determinants of plant sale, in particular in relation to hedge fund intervention. In Table 7 Panel A, we report results from probit regressions at the plan-year level where the dependent variable is a dummy variable for plant sale in a given year. The most important plant characteristics are TFP and the importance of the segment that the plant belongs to for the firm (as measured by the share of the segment in a firm's total shipments). As expected, both are significantly negatively associated with the probability of plant sale. Related to hedge fund activism, we find the following significant (at the 5% level) results: plants belonging to targeted firms are more likely to be sold after, but not before, intervention. Moreover, low productivity plants are far more likely to be sold post intervention. Finally, the probability of being sold increases significantly post-targeting for plants in non-concentrated industries, but not for plants in concentrated industries.

[Insert Table 7 here.]

Panel A provides a clear message that hedge funds are associated with the sale of low productivity plants and more so in non-concentrated industries.

Next, we ask whether the productivity improvement occurs among plants that were sold plants (and now in the hands of new owners). Panel B of Table 7 presents results that address this question. First, we re-run the regression presented in equation (3) but do not restrict the ownership of plants by the targeted companies in the three years before and after targeting. Instead, the dummy variable $d[t+k]$, $k = -3,\ldots,+3$, assumes the value of one as long as the plant is owned by the target company during the year of targeting (year t).[12] Column (1) shows that the post-targeting performance change for these broadly defined event plants is less impressive than those reported in Table 4 for plants owned by target firms in each year from $t-3$ to $t+3$. This key difference is due to the inclusion of plants that were sold over the two years subsequent to the intervention and is consistent with the fact that worse-performing plants were more likely to be sold after hedge fund intervention (shown in Panel A of Table 7).

A mere divestiture of a negative NPV business unit creates value for a firm; yet the efficiency gain argument in favor of hedge fund intervention could be further strengthened if the performance of sold plants improves in the hands of new owners. To test this hypothesis, we re-run the TFP regression (3) but redefine an event as the sale of a plant by a firm that was targeted by hedge fund activists in the year of activism or within two subsequent years (i.e., from t to $t+2$). The second column of Panel B shows that plants that are sold post-activism exhibit a sharp "V"-shaped pattern of performance around their sales. In particular, those plants had productivity that is statistically equivalent to that of their industry-size-age benchmarked peers three years before their sale, but were sold right after their trough in terms of

[12] In contrast, the analysis in Table 4 requires the ownership of the plant by the target company in *each* of the years for the corresponding plant-year event dummy to be coded as one.

performance. Subsequently, the change in TFP from years *t* to *t+3* amounts to 22% of a standard deviation in TFP of the peer group, which is statistically significant at the 10% level.

A question remains as to whether the TFP improvement subsequent to the sale of the plant is unique among targeted firms or is equally prevalent among plants sold in the absence of hedge fund intervention. The third column in Panel B addresses this issue through what is essentially a placebo test. When we examine all sales of plants that do not belong to firms ever targeted by hedge funds in our sample, we find the improvement from years *t* to *t+3* is 0.037 (statistically significant due to a much larger sample of plant sales), or one-sixth of the magnitude experienced by sales associated with hedge fund activism.

Finally, we examine the interaction between the change in performance subsequent to the plant sale and product market concentration and whether it is consistent with the discussion in Section 3.2. The increase in TFP documented in column (2) of Table 7 Panel B is most pronounced among the least concentrated industries (results not tabulated). For the subsample of industries whose HHI measures rank among the bottom quartile, the TFP change from years *t* to *t+3* is 0.443 (t-statistic = 3.32). The same figure for the top-quartile HHI industries is 0.112 (t-statistic = 0.72).

Overall, results in Table 7 illustrate the relative importance of TFP improvement on the intensive margin (i.e., gain in efficiency for assets retained by the target firms post intervention) and that on the extensive margin (i.e., gain in efficiency due to assets' improved match to new owners). Hedge funds overall seem to be more effective on the extensive margin by facilitating assets reallocation. Such a role is natural given that hedge funds are outsider investors who may not possess detailed knowledge about the operation of a firm, but may have a comparative advantage in sharing industry-wide best practice and managing asset portfolios at the industry level. Moreover, industries with lower concentration tend to offer more opportunities for asset redeployment. The combination serves to explain why hedge fund activism appears to be a complement to product market competition as a form of corporate governance. It is worth noting that this explanation does not hinge on the use of product market concentration as a proxy for competition, which also sets this study apart from Giroud and Mueller (2010, 2011) and Chhaochharia, Grinstein, Grullon, and Michaely (2012).

4.3 Delisting from Compustat

Our Census sample includes plants belonging to 368 companies that were targeted by hedge funds between 1994 and 2007. Within this sample, 91 companies disappear from Compustat within two years after being targeted because they were sold, taken private, or liquidated. Among this sample we are able to follow 261 plants owned by 53 firms that delisted from Compustat post-targeting. These additional observations from the Census data allow us to assess the sign as well as the magnitude of the attrition bias associated with using the Compustat data. We will discuss the remaining bias due to plant liquidation later in this section.

[Insert Table 8 here.]

In Table 8, we report results from regressions that interact the dummy variables $d[t+k], -3 \leq k \leq 3$ with an indicator variable, *Attrition (Non-attrition),* which is set equal to one if a plant belongs to a company that is targeted by hedge funds and then delisted from (remains in) the Compustat database by the end of year $t+1$. On the side of the table we report the *t*-tests for improvement in performance among the plants of companies remaining in and disappearing from the Compustat database. Interestingly, when we focus on the plants that belong to companies that were delisted from Compustat during the one-year post-targeting period (*Attrition* = *1*), we find a positive improvement in two (three) years in the magnitude of 0.109 (0.239). The improvement from years t to $t+3$ is significant at the 10% level. In comparison, the magnitude of improvement for remaining firms (*Non-attrition* = *1*) is reduced to about half. The statistical significance for the improvement is higher for the remaining firms due to a much larger sample.

We thus find no support for the conventional positive survivorship bias. The relative magnitude actually suggests an unusual negative survivorship bias. That is, restricting the measurement of performance to the sample of surviving firms in Compustat tends to *underestimate* the change in performance of target firms. This result is direct evidence supporting the findings in Brav, Jiang and Kim (2010) using an instrumental variable approach, and good news to the existing literature using firm-level data: the performance (such as ROA) improvement documented therein is on the conservative side.

Needless to say, the Census data have their own attrition issues. About 43% of the plants that appear in our sample before hedge fund targeting disappear afterwards. There are two reasons for the attrition. First, "small" plants (with fewer than 250 employees) are not sampled every year in the ASM (but, all operating plants are sampled in the CMF for the years ending '2' and '7') so that they might disappear from the sample (possibly temporarily), but in fact could still operate. This attrition bias is due to random sampling and therefore should not bias results in either direction. Second, the plants that are liquidated drop out of the sample simply because they cease to exist. A formal test (not tabulated) shows that plants belonging to target firms stand somewhat lower (not significant) probability of closure after intervention compared to before. If we believe that plan liquidation is more likely to be distress-related, then there is no evidence that the distress risk increases post hedge fund intervention.

5. Causality

5.1. Overview

The evidence reported so far is consistent with but does not "prove" a treatment effect by the hedge funds on the plants of the targeted companies. Before delving into the causality tests, we would like highlight two different aspects of a treatment effect in our context. The first question is the following: If hedge fund activists were randomly assigned to target firms (i.e., if targeting per se is exogenous to future firm performance), would they have improved the performance? This addresses the population average treatment effect. The second question asks: would the same changes have occurred in the absence of hedge funds' effort in the firms that they chose to target? This notion represents the treated effect on the treated.

For the purpose of our research, as well as for relevant policy implications, we are primarily interested in the second notion of the treatment effect and do not attempt to take a stance on the first. We fully acknowledge that hedge funds do not target firms randomly, along both observable and unobservable dimensions. In fact, picking a target where hedge funds could have the biggest impact is an important part of the activist investing strategy, and no sensible policy should mandate matching of targets to hedge fund activists. As a result, we are most interested in assessing the real effects from activism relative to passive investments. That is, the counterfactual is the outcome that would prevail had the hedge funds picked the same target firms but remained merely as passive investors.

Current research on hedge fund activism has already provided support for the view that hedge fund intervention, beyond stock picking, is necessary for the observed outcomes. Certain changes (notably a significant increase in CEO turnover rate as shown in Table 6) are natural results of confrontation, which are unlikely to have occurred but for the persistence of the activists. In our sample, activists tend to hold concentrated stakes in target firms for an average holding period of two years.[13] We observe an even longer duration of ownership by Pershing Square in the Fortune Brands in the case described in Appendix B. Undiversified positions together with costly engagements, including proxy contests or public campaigns (Gantchev (2012)), cannot be justified by pure stock picking. Moreover, openly hostile activism generates higher announcement returns than non-confrontational ones, and activist stakes, which require the filing of a Schedule 13D, generate higher returns than the revelation of large passive stakes, which can be disclosed at a longer delay on Schedule 13G (see Klein and Zur, (2009), Clifford (2008)).

We conduct several additional tests to complement the evidence summarized above. Each test addresses a particular alternative hypothesis on the possibility that the same changes would have occurred even if hedge funds were mere passive investors.

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[13] The holding period is measured as the length of time between the filing of the initial Schedule 13D, and the last amendment to the 13D that indicates a drop of the stake to below the 5% level. This measure provides a lower-bound for a hedge fund holding period of a significant stake.

5.2. Specific Alternative Hypotheses

5.2. 1. "Self-cure"

The first alternative hypothesis is "self-cure." That is, the target companies/plants tend to experience deterioration before hedge fund intervention. These units might recover on their own, perhaps just by the force of mean-reversion. To address this concern, we conduct a placebo test where for each of the plants belonging to a target firm, a "pseudo event" is assigned to a plan-year observation where the plant does not belong to a firm targeted by hedge funds but experiences similar deterioration (in the same quintile) with the targeted plant. We run the same regression as in Table 4 and plot the coefficients on the $d[t+k]$, $k=-3,\ldots,+3$, dummies, and plot them in Figure 2, on top of the coefficients from the main regression (for "true" events).

[Insert Figure 2 here.]

By construction, the target plants and the pseudo event plants share similar paths in their productivities from year $t-3$ to year t. Importantly, the paths diverge right after intervention: the placebo plants keep their deterioration trend while the target plants turn around for improvement.

5.2.2. Voluntary reform by target

The second alternative hypothesis is that hedge funds select companies where management was about to implement changes even without influence or pressure from the hedge funds. To entertain this possibility, we focus on the subsample of openly confrontational events where the hostile nature of hedge fund activism due to management's resistance to hedge fund agenda is publicly known. We classify an event as one in which activists maintain a hostile stance if the activist tactics involve actual or threatened proxy contests or law suits, or shareholder campaigns of confrontational nature (such as publically denouncing the management, or shareholder proposals aiming at ousting the CEO). Such events account for about one quarter of our sample. Note that our classification algorithm is conservative: while we might miss events that were hostile behind closed doors, the selected subsample should consist exclusively of hostile events. Results are reported in the first two columns of Table 9 Panel A.

[Insert Table 9 here.]

Repeating the same regression as in Table 4 but restricting event observations to those involved in hostile events, column (1) reveals the same pattern of TFP: deterioration before and improvement after the intervention. For comparison purpose, coefficients associated with non-hostile events are shown in column (2). Interestingly, TFP improvement between years t and $t+3$ is comparable between hostile and non-hostile events (0.127 vs. 0.097) both of which are significant at the 10% level. For the hostile event subsample, it is difficult to attribute these changes to management's voluntary and planned reform, as we know that in these cases management resisted the actions demanded by the activists. A further partition of this subsample into events where hedge fund accomplished their stated goals versus those that did not does not yield significantly different results. It could be due to a loss of statistical power, or due to the fact that hedge fund activism exerts disciplinary pressure on target firms that does not solely depend on the attainment of the activists' stated goals.

5.2.3. Industry shocks

The third alternative hypothesis posits that hedge funds are sophisticated stock pickers and target players are best positioned in an industry shock (such as winners from consolidation). This hypothesis is highly pertinent in view of our finding that productivity improvement tends to be more pronounced in less concentrated industries (hence more potential for consolidation). Under this hypothesis, however, the real effects associated with hedge fund activism should concentrate in plants belonging to the primary industries (which were the reason for targeting) but not in plants belonging to non-primary industries of target firms.

To assess this possibility we examine target firms that have plants in both the primary industry it belongs to and non-primary industries. Following Maksimovic and Phillips (2002), we define a three-digit SIC segment of a target firm as "core" ("peripheral") if the combined shipments of the industry segment is more (less) than 25% of total shipments of the firm. In columns (3) and (4) of Table 9 Panel A, we report the coefficients separately for events that involve plants that are part of the core segments of targeted firms, and those that are peripheral. We find that improvements in plants in non-primary industries are just as strong as their primary-industry counterparts. The three-year post intervention TFP improvement is 0.138 (t-statistic = 2.59) for peripheral plants and 0.087 (t-statistic = 1.90) for core plants, and the two numbers are not statistically different from each other. Therefore, riding-the-industry-shock alone cannot explain our main results about productivity improvement in targeted plants.

5.3. A General Alternative: Stock Selection vs. Intervention

It is difficult to exhaust all specific alternative explanations to our findings. We thus conduct a summary test that aims at separating hedge fund stock picking from intervention. In our setting, a "treatment" is a public statement of hedge fund intervention, which necessarily builds on hedge funds' block holding. The challenge is therefore to separate hedge funds' skills in picking stocks and the anticipation of positive changes in the target firm from hedge funds' intervention that causes or facilitates these changes. Such a separation can be derived from cases where activists' change of investment stance from passive to activist without material ownership changes in the target firm. It turns out that a legal feature in the SEC's ownership disclosure rules allows such identification.

Investors with beneficial ownership of more than 5% (but below 20%) for purely "investment purpose" without an intention to exert control are usually eligible to file a shorter form 13G (under Exchange Act Section 13(g) and Regulation 13D-G). To equate 13D (13G) filing to activist (passive) stance for identification purpose we must establish that (i) an investor with an activist intention (including actively "communicating" with the management regarding firm strategies) cannot file 13G, and (ii) an investor with a passive stance does not want to file 13D. It turns out that (i) is required by law and (ii) is incentive compatible. Regarding (ii), the 13G form not only requires less information disclosed but also allows for a longer delay in ownership disclosure.[14] Moreover, 13D filings entail more legal obligations.[15] As such, a true passive investor should not find it appealing to file a Schedule 13D.

On the surface, changes in firm performance subsequent to the hedge fund's filings of Schedule 13D (which involves both stock picking and potential intervention) vs. post 13G filing (stock picking only), should allow us to filter out the treatment effect. However, hedge funds choose to take activist or passive positions in different firms which might not be comparable even if we control for all observable characteristics. Hence, our identification comes narrowly from the same hedge fund-firm paring, that is, when a hedge fund switches from "G" to "D." A switch is required by law if a formerly passive investor decides that it may now want to take actions to influence control. Importantly, a switch usually does not come with significant ownership changes. The only major change at the switch is the investment stance of the hedge fund from passivity to activism.

There are 299 events in our sample where activism was initiated by activists' switch of 13G to 13D filings. Due the relatively small sample of switching events and the loss of event observations in matching to Census,[16] we conduct the test both at the plant level using the Census data and at the firm level using Compustat data. Given that the previous sections establish that target plants' productivity follows similar patterns as target firms' ROA (Figure 1 and Table 4), and that the attrition of Compustat firms does not introduce a positive survivorship bias for target firms (Table 8), we believe the analysis of firm-level operating performance is informative of the performance of underlying business units.

We construct a new sample where a plant-year or firm-year observation is included if at least one of our 319 sample hedge funds have a 5% or more passive ownership disclosed in a Schedule 13G (the "G-stayers") and those observations where hedge funds have Schedule 13D filings that are switched from 13G (the "switchers"). A plant-year or firm-year observation becomes an "event" if during that year, the

[14] Passive blocks of more than 5% require disclosure in Schedule 13G within 45 days after the end of the calendar year.
[15] Such legal obligations include instant filing of an amendment if there is any "material" change in the action including ownership change of 1% of more in either direction.
[16] Recall that we are able to match about one-sixth of the activism event firms to the Census data.

13G filing was switched to a 13D. We call the event "*G to D switch*." Such a sample encompasses 2,983 plan-year observations or 3,954 firm-year observations (including 199 event observations). We then run the following regression:

$$\Delta Performance_{i,t \to t+3} = \beta \cdot G\ to\ D\ switch_{i,t} + \gamma \cdot Control_{i,t} + \alpha_f + \alpha_t + \alpha_{SIC3} + \varepsilon_{i,t}\ , \tag{5}$$

where $\Delta Performance_{i,t \to t+3}$ is the change in TFP or ROA during the three year period post switch (if there is a "G to D switch" in year *t*) or just a three-year period (for non-events). $G\ to\ D\ switch_{i,t}$ is a dummy variable equal to one if in year *t* a hedge fund switched a 13G filing in firm *i* (or plant that belongs to firm *i*) to a 13D filing. $Control_{i,t}$ represents the same control variables used in previous plant-level regressions, or includes firm market cap and firm age in the CRSP database for firm-level regressions. α_f, α_t, and α_{SIC3} are fund, year, and three-digit SIC fixed effects.

Results, reported in Table 9 Panel B, are encouraging despite the small sample of events that contribute to the identification. Compared to the "G-stayers," the "switchers" experience TFP change amounting to 0.089-0.132 and ROA change that is 2.5-3.3 percentage points higher during the three-year period post switch after controlling for year fixed effects. The second specification with fund fixed effect is particularly informative as it controls for fund-specific stock-picking ability. The key coefficients are significant at the 10% (5%) level using plant (firm) regressions. If we further add industry fixed effects, the coefficients are rendered insignificant although the magnitude remains comparable. Due to the small number of switches in the sample, the loss of statistical power is expected with multiple layers of fixed effects.

Table 9 demonstrates that firm performance improves after a passive hedge fund blockholder turns active. Given that the only addition at the switching point is the activist stance and not ownership, we believe the test provides a clean identification of intervention from stock picking. Importantly, the coefficients on *G to D switch* are of comparable magnitude to the overall improvement in TFP and ROA of all target plants/firms (see the differences in the coefficients on *d[t+3]* and *d[t]* as reported in Table 4 and plotted in Figure 1), suggesting that the "treatment effect" (conditional on hedge fund stock picking) underlies the association between hedge fund targeting and firm performance improvement.

It is important to emphasize that we do not claim that the same improvement would arise if a *randomly* chosen 13G filer is forced to switch to 13D. Our results support a causal effect of intervention among the firms that the hedge funds choose to intervene. In other words, if the hedge funds were disallowed to engage in activism, then the improvement we observe would not have materialized even if the same hedge funds picked the same firms for the purpose of passive investment.

6. Conclusions

Using mostly plant-level observations from the U.S. Census Bureau we show that hedge fund intervention is associated with both productivity and profitability gains at the plants of the targeted companies and that this effect is stronger in less concentrated industries. We also measure the performance of plants that were sold subsequent to the intervention and find that they were among the worst performing plants at the time of divesture but later experience a substantial improvement in the hands of new owners relative to a matched sample. These results support the view that hedge fund activists facilitate improvements in terms of both production efficiency in kept plants and capital re-allocation. Overall, the evidence provided in the paper highlights the real and fundamental effects brought about by hedge fund activists to their target firms.

Appendix A – Construction of Variables to Estimate Production Function

This appendix describes the construction of variables required to estimate the production function described in Section 2.2 using variables in the CMF and ASM databases. Output is computed as the sum of total value of shipments (TVS) and the net increase in inventories of finished goods and works in progress. To account for industry-level changes in output price, we deflate output using the four-digit SIC level output price deflator from the NBER-CES manufacturing database constructed by Bartelsman, Becker, and Gray (2000).

Capital stock is constructed using a recursive perpetual inventory formula (Lichtenberg (1992), Kovneck and Phillips (1997)). First, we obtain the initial value of nominal capital stock for each plant when the plant is born (identified using the LBD) or first appears in the CMF or ASM. Second, we translate this initial *historical* value of *gross* capital stock into a *constant* value of *net* capital stock using a NAICS-based industry-level capital stock deflator from the Bureau of Economic Analysis (BEA). Third, we account for changes in the price of capital by deflating the computed real, net capital stock using the four-digit SIC level investment deflator from the NBER-CES manufacturing database. Fourth, beginning with the constructed initial net capital stock in constant dollars for each plant, we accumulate capital stock going forward using the following recursive formula:

$$K_{it} = K_{it-1} \times (1 - \delta_{it}) + I_{it}, \tag{A-1}$$

where K_{it} is net capital stock, δ_{it} is a two-digit SIC level depreciation rate from the BEA, and I_{it} is investment for plant i in year t. The measure of investment is deflated using the four-digit SIC level investment deflator from the NBER-CES manufacturing database. Before 1997, variables for investment were available separately for equipment and structure, and we thus construct capital stock separately for each category and then sum the two capital stock measures to obtain total capital stock. After 1997, only variables for total capital are available, and so we only construct total capital stock.

We use "production-worker equivalent hours" as our measure of labor input. Specifically, labor input is constructed as the total production worker hours times total wage bills divided by wage bills for production workers. The underlying assumption to construct this measure of labor hour is that the per-hour wage rates for production and non-production workers are similar. Lastly, material costs are computed as the costs of materials and parts plus the costs of fuel and electricity.

Appendix B: Case Study: *Pershing Square Capital Management and Fortune Brands*

On October 8, 2010, Pershing Square filed a Schedule 13D with the SEC indicating that it owned 10.9% of Fortune Brands shares and that it also had exposure to cash-settled total return swaps arrangements increasing its economic exposure to a total of 11.3%. At the time Fortune Brands, a conglomerate, ran three divisions: a home and security business, a spirits business, and a golf related business. With scarce evidence for synergies across the divisions it was believed that the company would be worth more if one or two of the parts were sold or spun off.

On October 28, 2010, during the conference call for the third quarter earnings results, the CEO, Bruce Carbonari, said that the company was open to constructive talks with all shareholders including Pershing Square. He proceeded, however, to defend the conglomerate's business structure. Shortly afterwards the company reported that Credit Suisse and Centerview Partners were hired for the negotiations with Pershing Square.[17] It is important to note that since the filing of the Schedule 13D Pershing Square had kept private their plan for the firm as well as the negotiations with management.

In mid-November 2010, the *Wall Street Journal* reported that "Several parties could be interested in the different businesses of Fortune and some have expressed an interest already."[18] The article speculated on the identity of Fortune Brands' competitors who might want to acquire its spirits and golf assets and the possibility that the remaining home and security business could be sold to private equity firms. On December 8th, 2010, Fortune Brands said it would spin off its golf and home and security businesses and retain its higher growth spirits business to be renamed Beam Inc. By then the company's stock price had risen by 18% since the initial filing by Pershing Square.

In the ensuing period Pershing Square did not reduce its stake in Fortune Brands. In fact, on August 8, 2011, it was reported that it increased its direct ownership stake to 13.5% (and an economic exposure of 14.8% including the total return swaps). Pershing Square remained the largest shareholders of the spun-off building products business, named Fortune Brands Home and Security, and the spirits business, Beam. In the letter to investors later in November 2011, the fund described Beam's strong competitive position and high growth reflecting "a very scarce asset" with "many strategic alternatives available to the company, including a sale of the business, a merger with another spirits company, and the acquisition of other brands." The fund also described its holding in Fortune Brands Home and Security as an investment that is well-positioned to benefit from an improvement in the housing market.

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[17] The transcript of the earnings conference call is available at www.SeekingAlpha.com. See also the article in *Reuters*, "Fortune Brands' biggest foe: the Tax Man," October 29, 2010.
[18] "Fortune May Cooperate With Ackman," *Wall Street Journal* November 13, 2010.

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Figure 1: Target Firm Return on Assets (ROA) Before and After Activists' Intervention

This figure plots the coefficients β_k , k=-3,…, +3, from the following regression at the firm (i) – year (t) level:

$$ROA_{i,t} = \sum_{k=-3}^{+3} \beta_k \; d[t+k]_{i,t} + \gamma \; Control_{i,t} + \alpha_{SIC3} + \alpha_t + \varepsilon_{i,t} \; ,$$

where $ROA_{i,t}$ is return on assets, defined as the ratio of earnings before interests and taxes to total assets. $d[t+k]_{i,t}$, $k = -3,\ldots,+3$ is a dummy variable equal to one if firm i was or will be targeted by hedge funds in year $t+j$ years. $Control_{i,t}$ are control variables including the logarithm of firm market cap and firm age (proxied by the number of years since first appearance in CRSP). α_{SIC3} and α_t are SIC three-digit and year fixed effects. The solid line plots the coefficients on $d[t+k]$ dummies which represent industry-year adjusted ROA. The dotted lines are 95% confidence intervals.

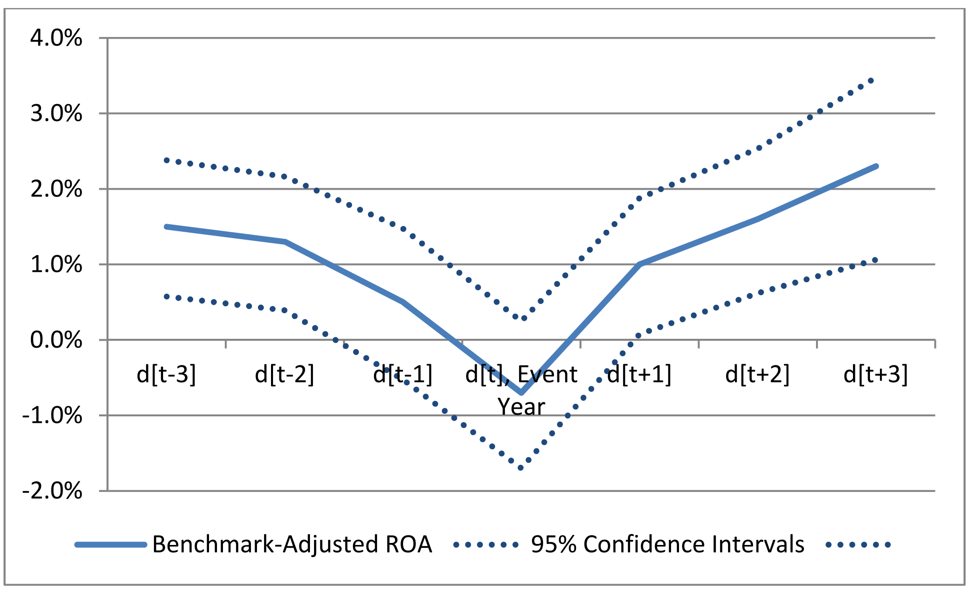

Figure 2: Placebo Test - Target Firms vs. Firms Matched on Pre-Targeting Deterioration

This figure plots two sets of estimated coefficients on *d[t+k]*, $k = -3, \ldots, +3$, and *t* is the year of targeting, from regression (5). The two graphs represent plants in targeted firms and non-event plants matched by similar decline in TFP from *t-3* to *t-1*. The dotted lines in the figure represent 95% confidence intervals.

[The publication of this figure is pending data clearance from the Census Bureau.]

Table 1: Sample Construction and Descriptive Statistics on the Census-matched Activism Events

Panel A shows the numbers of all hedge fund activism events and the events matched to the Census of Manufacturers (CMF) and Annual Survey of Manufacturers (ASM) databases from 1994 to 2007, separately for manufacturing and non-manufacturing target firms based on the Compustat SIC code. The panel also shows the number of plant-year observations for the Census-matched events. Panel B shows the distribution of activists' stated objectives, the percentage among the sample, and success rates for the full sample (columns 1-3) and the Census-matched sample (columns 4-6) of events from 1994 to 2007. Columns 1, 2 and 4, 5 report the number of events, and the percentage among all events, of each category. Columns 3 and 6 list the rate of success (including partial success). Percentages sum up to more than 100% since one event can have multiple objectives (However, the first category and the other four categories are mutually exclusive.) An event is classified as successful if the hedge fund achieves its main stated goal and a partial success if the hedge fund and the company reach some settlement through negotiation that partially meets the fund's original goal.

Panel A: Sample Selection for Activism Events Matched to Census Data

Events	Nun. events	Num. plant-years
1. All activism events	1987	-
a. Manufacturing targets	640	-
b. Non-manufacturing targets	1347	-
2. Matched to Census data with TFP	368	14923
a. Manufacturing targets	281	12631
b. Non-manufacturing targets	87	2292

Panel B: Summary of Activism Events by Stated Objectives

	Census-matched			All		
Stated Objectives	N events (1)	% of Sample (2)	% Success (3)	N events (4)	% of Sample (5)	% Success (6)
1. General	237	64.4%	N/A	1212	61.0%	N/A
2. Capital Structure	51	13.9%	64.7%	263	13.2%	62.0%
3. Business Strategy	56	15.2%	58.9%	293	14.7%	58.4%
4. Sales of Target	61	16.6%	65.6%	375	18.9%	62.7%
5. Governance	119	32.3%	73.9%	631	31.8%	72.4%
Specific – Sum [2 to 5]	131	35.6%	64.9%	775	39.0%	65.0%
Total – Sum [1 to 5]	368	-	-	1987	-	-

Table 2: Summary Statistics on Plant Observations from the CMF and ASM Sample

This table presents descriptive statistics on the plant-year observations targeted by activists (column "Targets"), all plant-year observations used in the analysis (column "Universe"), and plant-year observations matched to public firms from Compustat (column "Universe-Public") from the CMF and ASM databases for the period 1990-2009. We require each observation in both samples to have all variables necessary to compute total factor productivity (TFP). "Total value of shipments" is TVS in the CMF and ASM databases and a measure of sales from plants in million dollars; "Capital stock" is the sum of real net stock of equipment and structures in 2005 constant million dollars. It is constructed using a perpetual inventory formula following the procedure described in Appendix A; "Total wage" is the sum of wages for production and non-production workers in thousand dollars; "Total employees" is the number of total employees; "Average wage" is computed as total wage divided by total employees; "Wage per hour (production workers) is total production worker wage divided by total production hour; "Plants per segment" is the number of plants in a given industry segment (defined at the three-digit SIC level) of a given firm; "Plants per firm" is the total number of plants of a given firm; "Plant age" is the number of years since a plant first appears in the CMF or ASM database; "TFP (Standardized)" is total factor productivity computed by estimating a log-linear Cobb-Douglas production function by three-digit SIC industry and year, divided by its within-industry standard deviation; "Operating margin" is defined as (output – labor costs – material costs) / output; "HHI (Census)" is the Herfindahl–Hirschman Index computed at the three-digit SIC level using all observations with positive total value of shipments in the ASM database. "Num. industries (SIC3)" is the number of three-digit SIC industries represented in the sample; "Observations" is the number of plant or firm observations.

	Mean	STD	Mean	STD	Mean	STD
	Universe		Universe-Public		Targets	
Total value of shipment ($m)	74.15	340.50	145.32	529.62	78.17	142.81
Capital stock ($m) – real, net	39.33	193.95	83.16	318.49	40.61	102.37
Total wage ($m)	10.38	34.45	19.54	56.97	12.10	17.40
Total employees	226.00	545.00	385.00	872.00	265.00	324.00
Average wage ($000)	41.00	15.13	44.22	15.45	44.12	14.16
Wage per hour (production workers)	17.21	6.81	18.85	7.18	18.82	6.73
plants per segment (SIC3)	6.52	13.56	12.43	18.02	9.23	12.57
plants per firm	18.30	33.58	41.66	43.18	28.23	29.23
Plant age	19.93	8.99	20.77	8.55	23.30	8.99
TFP (Standardized)	0.001	0.900	0.112	0.934	0.086	0.908
Operating margin	0.229	0.278	0.240	0.312	0.247	0.271
HHI (Census)	0.030	0.039	0.038	0.044	0.038	0.045
Num. Industries (SIC3)	134	-	133	-	119	-
Observations (plant-year)	787758	-	238846	-	14923	-
Observations (unique plant)	125112	-	31005	-	2900	-
Observations (firm-year)	406747	-	29391	-	1902	-
Observations (unique firm)	85552	-	3702	-	304	-

Table 3: Summary Statistics on Firm Observations from the Compustat Sample

This table presents descriptive statistics on targets of hedge fund activists matched to the Census plant-level data (column "Census Sample") and all target firms (column "All Target Firms"), benchmarked with the full sample of Compustat firms (column "Full Compustat Sample") for the event period 1994-2007. All variables are retrieved from years prior to the event year. "MV" is market capitalization in millions of dollars; "Assets" is total book value of assets in millions of dollars; Leverage is defined as debt/(debt + book value of equity); "Cash" is defined as (cash + cash equivalents)/assets; "Div Yld %" is dividend yield, defined as (common dividend + preferred dividends)/(market value of common stocks + book value of preferred); "q" is defined as (book value of debt + market value of equity)/(book value of debt + book value of equity); "Sales growth" is the growth rate of sales over the previous year; "Cash flow" is defined as (net income + depreciation and amortization)/lagged assets; "R&D" is R&D scaled by lagged assets; "Firm age" is the number of years since a firm's first appearance in Compustat; "HHI" is the Herfindahl-Hirschman index of industry competition defined as the industry-level (SIC3) squared sum of firm market shares measured by sales; "Capx %" is capital expenditures scaled by lagged assets; "Total Payout Yld %" is defined as the sum of common dividends and common share repurchases, scaled by the lagged market capitalization; "CEO Turnover" is equal to one if the name of the current CEO is different than that of previous year's CEO, and zero otherwise; "Altman (Ex. Leverage)" is Altman's Z-Score computed excluding the leverage ratio.

	Census Sample (#obs = 368)		All Target Firms (#obs = 1,575)		Full Compustat Sample	
	Mean	Std	Mean	Std	Mean	Std
MV	800.50	2071.36	657.81	1554.44	1677.3	5156.96
Assets	1090.27	2694.02	1128.22	3498.62	2555.98	8420.64
Leverage	0.288	0.251	0.26	0.259	0.284	0.298
Cash	0.109	0.149	0.173	0.219	0.18	0.231
DivYld %	0.950	1.620	0.751	1.751	1.111	2.295
Q	1.671	1.393	2.066	1.986	3.86	8.072
Sales Growth	0.082	0.296	0.242	0.905	0.261	0.711
Cash flow	0.044	0.165	0.009	0.238	-0.134	0.78
R&D	0.038	0.062	0.048	0.117	0.064	0.164
Firm Age	21.42	17.81	12.77	13.89	12.14	13.73
HHI (Compustat)	0.182	0.164	0.15	0.14	0.14	0.14
Capx %	5.010	4.959	5.54	7.06	5.78	7.55
Total Payout Yld %	2.341	4.538	2.21	4.62	2.18	4.29
CEO Turnover	0.205	0.405	0.13	0.34	0.09	0.29
Altman (Ex. Leverage)	1.52	1.67	-0.19	3.97	-1.55	5.33

Table 4: Hedge Fund Activism and Productivity

This table examines the impact of hedge fund activism on the productivity of plants owned by target firms in each year from three years before to three years after the hedge fund's intervention. The dependent variable is measures of productivity, which is the standardized total factor productivity (TFP) as defined in Table 2 in columns 1 to 3 and 6. In column 1, all control variables are demeaned at the industry-year level. Column 4 uses the nonstandardized TFP as dependent variable, and column 5 uses standardized TFP based on Levinsohn and Petrin (2003) GMM estimates of production functions. *d[t+k]* (*k=-3,…,+3*) is a dummy variable equals to one if the plants belongs to a firm that is targeted in year *t+k*. Year *t* is the event year. "log(plants per segment)," "log(plants per firm)" and "Plant age (/ 100)" are defined in Table 2. The unit of observation is the plant except for column 6, in which plant-level TFP is aggregated at the firm level using beginning-year capital stock as a weight and the number of plants per segment is the average across segments for a given firm. Year fixed effects are included in all regressions. Columns 2 and 3 include additionally firm and plant fixed effects, respectively. The *t*-statistics based on standard errors adjusted for sample clustering at the firm level are reported below the coefficient estimates. At the bottom of the table we report differences in the coefficients before and after the event year and the associated t-statistics, as well as the statistics from an *F*-test for joint inequality.

	(1)	(2)	(3)	(4)	(5)	(6)
Unit	Plant	Plant	Plant	Plant	Plant	Firm
Dep. Var.	TFP	TFP	TFP	Raw TFP	LP TFP	TFP
d[t-3]	0.069	0.001	-0.012	0.007	0.052	0.125
	1.95	0.03	-0.62	0.60	1.66	2.51
d[t-2]	0.072	0.001	-0.013	0.007	0.083	0.069
	1.76	0.03	-0.50	0.49	1.85	1.47
d[t-1]	0.056	-0.026	-0.036	-0.001	0.056	0.074
	1.73	-0.84	-1.40	-0.11	1.77	1.60
d[t]	0.053	-0.032	-0.045	-0.001	0.035	0.020
	1.38	-0.94	-1.60	-0.04	0.93	0.42
d[t+1]	0.075	-0.028	-0.044	0.007	0.026	0.076
	2.01	-0.77	-1.45	0.52	0.62	1.53
d[t+2]	0.116	0.011	-0.005	0.020	0.097	0.151
	3.30	0.29	-0.16	1.48	2.66	2.91
d[t+3]	0.165	0.046	0.032	0.035	0.155	0.177
	4.15	1.30	1.06	2.62	3.80	2.72
log(plant per segment)	0.001	0.026	0.007	0.000	0.006	-0.018
	0.74	3.33	1.35	0.12	0.67	-1.47
log(plant per firm)	0.002	-0.062	0.004	0.021	0.045	0.066
	4.29	-6.12	1.11	9.01	9.39	7.45

Unit Dep. Var.	(1) Plant TFP	(2) Plant TFP	(3) Plant TFP	(4) Plant Raw TFP	(5) Plant LP TFP	(6) Firm TFP
Plant age (/100)	-0.005	-0.788	-	-0.207	-0.810	-0.446
	-14.05	-18.36	-	-15.52	-20.81	-13.83
Year fixed effects	N	Y	Y	Y	Y	Y
Firm fixed effects	N	Y	N	N	N	N
Plant fixed effects	N	N	Y	N	N	N
Observations	787758	787758	787758	787758	787758	407020
R-squared	1.14%	33.20%	55.29%	1.02%	1.09%	0.32%
Differences and t-statistics						
d[t] – d[t-3]	-0.014	-0.032	-0.033	-0.008	-0.017	-0.106
	0.49	1.05	1.23	0.69	0.49	1.82
d[t+2] – d[t]	0.056	0.042	0.040	0.020	0.062	0.131
	1.99	1.40	1.39	1.82	1.54	2.88
d[t+3] – d[t]	0.108	0.077	0.077	0.036	0.119	0.158
	3.08	2.24	2.21	2.69	2.48	2.57
F test (d[t] – d[t-3] = 0) & (d[t+3] – d[t]=0)	4.84	2.57	2.47	3.66	3.18	3.72
(p-value for F-test)	0.01	0.08	0.08	0.03	0.04	0.02

Table 5: Hedge Fund Activism, Product Market Concentration, and Productivity

This table presents the interactive effect of hedge fund activism with product market concentration on the productivity of plants owned by target firms in each year from the three years before to three years after the hedge fund's intervention. Our measure of product market concentration is the Herfindahl–Hirschman Index (HHI) defined in Table 2 lagged by one year relative to the dependent variable. We estimate the effect of hedge fund activism for industries with the HHI in the first quartile ("Low HHI") and for industries with the HHI in the fourth quartile ("High HHI"). TFP is estimated using the specification described in Table 2. All other independent variables are defined in Table 4. Year fixed effects are included in both regressions. The t-statistics based on standard errors adjusted for sample clustering at the firm level are reported below the coefficient estimates. At the bottom of the table we report differences in the coefficients before and after the event year and the associated t-statistics.

	(1)	(2)
Dep. Var.	TFP	
Sample	Low HHI	High HHI
d[t-3]	-0.108	-0.039
	-1.79	-0.72
d[t-2]	-0.018	-0.083
	-0.31	-1.68
d[t-1]	-0.088	-0.096
	-1.66	-2.20
d[t]	-0.081	-0.101
	-1.26	-1.71
d[t+1]	-0.095	0.006
	-1.10	0.11
d[t+2]	0.028	-0.004
	0.33	-0.07
d[t+3]	0.161	-0.014
	2.11	-0.22
log(plant per segment)	0.004	0.041
	0.28	3.33
log(plant per firm)	0.072	0.036
	9.40	4.85
Plant age (/100)	-0.453	-0.596
	-9.48	-10.38
Observations	787758	
R2	1.25%	
Differences and t-statistics:		
d[t] – d[t-3]	0.027	-0.062
	0.37	1.33
d[t+2] – d[t]	0.109	0.097
	1.35	1.78
d[t+3] – d[t]	0.242	0.088
	3.08	1.39

Table 6: Hedge Fund Activism and Firm Policies

This table presents the effects of hedge fund activism on firm financial and governance policies. The dependent variables include *Leverage* (the ratio of net debt to total capital), *Capx* (the ratio of capital expenditure to total assets), *PayoutYld* (the ratio of total payouts including dividends and repurchases to the market value of equity), and *CEOTurnover* (a dummy variable equal to one if there is a CEO turnover during the firm-year). Regressions are conducted separately for firms in industries with the HHI in the first quartile ("Least concentrated") and those in the fourth quartile ("Most concentrated"). All regressions include industry and year fixed effects. The t-statistics based on standard errors adjusted for sample clustering at the firm level are reported below the coefficient estimates.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Least concentrated Industries				Most concentrated Industries			
Dependent variable	Leverage	Capx	PayoutYld	CEOTurnover	Leverage	Capx	Total Payout Yld	CEOTurnover
d[t-3]	0.017	-0.055	-0.254	-0.025	0.003	-0.511**	0.259	0.023
	[1.27]	[-0.18]	[-1.48]	[-0.66]	[0.25]	[-1.99]	[1.00]	[0.64]
d[t-2]	0.029**	-0.023	-0.366**	-0.007	0.013	0.035	-0.153	0.054
	[2.29]	[-0.07]	[-2.16]	[-0.20]	[1.16]	[0.12]	[-0.70]	[1.56]
d[t-1]	0.035***	-0.268	0.081	0.027	0.005	-0.157	0.540*	-0.004
	[2.62]	[-0.97]	[0.39]	[0.70]	[0.46]	[-0.62]	[1.74]	[-0.13]
d[t]	0.025**	-0.440*	0.328	0.032	0.021*	-0.466*	0.559*	0.058*
	[1.99]	[-1.66]	[1.17]	[0.76]	[1.85]	[-1.93]	[1.83]	[1.65]
d[t+1]	0.032**	-0.239	0.440	0.049	0.036***	-0.592**	0.670*	0.089**
	[2.34]	[-0.78]	[1.30]	[1.11]	[2.72]	[-2.18]	[1.80]	[2.22]
d[t+2]	0.010	-0.535*	-0.219	0.055	0.033**	-0.728***	-0.108	0.060*
	[0.68]	[-1.70]	[-0.74]	[1.01]	[2.28]	[-3.14]	[-0.33]	[1.81]
d[t+3]	0.003	-0.321	-0.514*	-0.069**	0.019	-0.744***	0.124	0.067*
	[0.17]	[-0.93]	[-1.70]	[-2.10]	[1.13]	[-2.62]	[0.34]	[1.66]
ln(MV)	0.003**	0.278***	0.167***	-0.011***	-0.008***	0.139***	0.248***	-0.003
	[2.03]	[9.90]	[8.68]	[-4.53]	[-5.17]	[4.70]	[12.60]	[-1.08]
Ln(Firm Age)	0.008***	-0.435***	0.331***	-0.005	0.005	-0.429***	0.351***	-0.015***
	[3.33]	[-7.86]	[9.37]	[-1.15]	[1.57]	[-7.60]	[9.45]	[-3.57]
Observations	38,356	30,729	38,099	9,514	30,434	29,986	30,302	9,416
R-squared	0.259	0.399	0.268	0.025	0.174	0.216	0.103	0.068

Table 7: Determinants of Plant Sale and Performance of Plants Sold After Activism

All regressions include year fixed effects. The t-statistics based on standard errors adjusted for sample clustering at the firm level are reported below the coefficient estimates. At the bottom of the tables we report differences in the coefficients before and after the event year and the associated t-statistics.

Panel A: Determinants of Plant Sale

This panel shows the determinants of plant sales using probit regressions. "Segment share" is the ratio of a given industry segment's shipment to the firm's total shipments. "Before" ("After") is a dummy variable equal to one for years before (after) the targeting by an activist, and zero otherwise. "Competitive" ("Concentrated") is a dummy equal to one if the plant is in the first (fourth) quartile of the HHI distribution.

Dep. Var.	(1) Sale	(2) Sale
TFP	-0.026	-0.025
	-5.58	-5.48
Segment share	-0.302	-0.294
	-13.5	-13.03
After	0.189	0.168
	2.61	1.9
Before	-0.107	-0.157
	-1.81	-2.06
After x TFP	-0.106	-0.106
	-2.45	-2.41
Before x TFP	0.007	0.010
	0.18	0.26
Competitive	-	-0.055
	-	-4.35
Concentrated	-	0.032
	-	2.45
After x Competitive	-	0.096
	-	0.96
Before x Competitive	-	0.081
	-	0.76
After x Concentrated	-	0.003
	-	0.03
Before x Concentrated	-	0.104
	-	0.88
Observations	763130	763130
Pseudo-R2	1.47%	1.52%
Differences and t-statistics:		
After – Before	0.296	0.325
	3.04	2.63
After – Before [Competitive]	-	0.341
	-	2.04
After – Before [Concentrated]	-	0.224
	-	1.49

Panel B: Productivity Change of Plants Owned by Targets in the Event Year and Sold Plants

Column 1 shows the productivity pattern of plants owned by target firms in the year of activism regardless of their owners pre- or post-activism. In the column the event year t is the year of activism. Column 2 shows the productivity pattern of plants owned by target firms prior to activism and then sold to other firms within two years post-activism. Column 3 shows the productivity pattern of plants owned by non-target firms and sold to other firms. In the last two columns, the event year t is the year of plant sale.

	(1)	(2)	(3)
Sample	Targeted Plants	Sold Plants	Non-target Sold
Dep. Var.	TFP	TFP	TFP
d[t-3]	0.063	-0.038	-0.022
	1.91	-0.64	-2.4
d[t-2]	0.071	-0.094	-0.034
	1.97	-1.33	-3.49
d[t-1]	0.040	-0.197	-0.053
	1.16	-2.29	-5.22
d[t]	0.002	-0.089	-0.091
	0.06	-1.28	-8.7
d[t+1]	0.006	-0.072	-0.054
	0.16	-1.01	-5.87
d[t+2]	0.023	-0.028	-0.051
	0.64	-0.32	-5.78
d[t+3]	0.061	0.129	-0.054
	1.67	1.32	-5.72
log(plant per segment)	-0.001	-0.001	0.00
	-0.11	-0.13	-0.3
log(plant per firm)	0.056	0.056	0.06
	9.8	9.91	10.15
Plant age (/100)	-0.561	-0.559	-0.56
	-16.74	-16.71	-17.2
Observations	787446	786324	816546
R2	1.14%	1.14%	1.13%
Differences and t-statistics:			
d[t] – d[t-3]	-0.061	-0.052	-0.069
	2.03	0.53	6.35
d[t+2] – d[t]	0.021	0.061	0.039
	0.74	0.57	3.93
d[t+3] – d[t]	0.059	0.219	0.037
	1.56	1.87	3.61

Table 8: Survivorship Bias due to Sample Attrition from Compustat

This table provides estimates of the extent to which firm attrition from the Compustat database induces biases in the measurement of the effect of hedge fund activism on target firms' performance. "Attrition" ("Non-attrition") is a dummy variable equal to one if the target firm that owns a plant disappears (does not disappear) from Compustat within one year post-activism, and zero otherwise. All variables are defined in Table 4. Year fixed effects are included in all regressions. The t-statistics based on standard errors adjusted for sample clustering at the firm level are reported below the coefficient estimates. On the side of the table we report differences in the coefficients before and after the event year interacted with the attrition status, and the associated t-statistics.

Unit Dep. Var.	(1) Plant TFP
d[t-3] × Attrition	-0.059
	-0.93
d[t-2] × Attrition	-0.107
	-1.54
d[t-1] × Attrition	-0.060
	-0.75
d[t] × Attrition	-0.041
	-0.52
d[t+1] × Attrition	0.050
	0.39
d[t+2] × Attrition	0.067
	0.36
d[t+3] × Attrition	0.198
	1.36
d[t-3] × Non-attrition	0.029
	0.76
d[t-2] × Non-attrition	0.041
	0.92
d[t-1] × Non-attrition	0.017
	0.50
d[t] × Non-attrition	0.008
	0.19
d[t+1] × Non-attrition	0.013
	0.34
d[t+2] × Non-attrition	0.058
	1.61
d[t+3] × Non-attrition	0.103
	2.57
log(plant per segment)	-0.001
	-0.11
log(plant per firm)	0.056
	9.81
Plant age (/100)	-0.561
	-16.74
Observations	787758
R2	1.14%

Differences and t-statistics	
(d[t] – d[t-3]) × Attrition	0.018
	0.28
(d[t+2] – d[t]) × Attrition	0.109
	0.77
(d[t+3] – d[t]) × Attrition	0.239
	1.86
(d[t] – d[t-3]) × Non-attrition	-0.021
	0.66
(d[t+2] – d[t]) × Non-attrition	0.050
	1.61
(d[t+3] – d[t]) × Non-attrition	0.095
	2.52

Table 9: Causality Tests

This table provides evidence for the causal effects of hedge fund activism on productivity of target firms. Year fixed effects are included in all regressions. The t-statistics based on standard errors adjusted for sample clustering at the firm level are reported below the coefficient estimates.

Panel A: Hostile Events and Target Plants in Non-core Segments

Columns 1 and 2 estimate the effect of activism on plant productivity separately for hostile and non-hostile events. Columns 3 and 4 estimate the effect of activism separately for plants in peripheral and core segments of the target firm. We define a three-digit SIC industry of a target firm as "core" ("peripheral") if the combined shipments of the industry segment is more (less) than 25% of total shipments of the firm (Maksimovic and Phillips, 2002). At the bottom of the tables we report differences in the coefficients before and after the event year and the associated t-statistics.

	(1)	(2)	(3)	(4)
Split	Hostile	Non-hostile	Peripheral (<25%)	Core (>= 25%)
Dep. Var.	TFP	TFP	TFP	TFP
d[t-3]	-0.022	0.038	-0.001	0.050
	-0.61	0.93	-0.03	1.26
d[t-2]	-0.056	0.056	0.020	0.046
	-1.07	1.18	0.44	1.06
d[t-1]	-0.008	0.026	-0.034	0.064
	-0.13	0.68	-0.75	1.62
d[t]	-0.027	0.021	-0.027	0.044
	-0.37	0.45	-0.47	0.95
d[t+1]	0.007	0.033	0.003	0.052
	0.08	0.78	0.04	1.26
d[t+2]	0.091	0.056	0.030	0.097
	0.93	1.41	0.45	2.16
d[t+3]	0.100	0.118	0.112	0.131
	1.01	2.63	1.41	2.89
log(plant per segment)	0.011	-0.001	0.009	-0.002
	0.21	-0.12	0.17	-0.25
log(plant per firm)	-0.040	0.056	0.021	0.057
	-0.82	9.81	0.56	9.84
Plant age (/100)	-0.786	-0.561	-1.120	-0.558
	-2.51	-16.74	-4.10	-16.60
Observations	787758		787758	
R2	1.15%		1.15%	
Differences and t-statistics				
d[t] – d[t-3]	-0.005	-0.017	-0.026	-0.006
	0.10	0.57	0.52	0.17
d[t+2] – d[t]	0.118	0.035	0.057	0.053
	1.64	1.12	1.24	1.38
d[t+3] – d[t]	0.127	0.097	0.138	0.087
	1.90	2.27	2.59	1.90

Panel B: 13G to 13D Switchers

This table examines the effects of switches in filing status from Schedule 13D to Schedule 13G. The sample consists of all plant-year or firm-year observations where at least one of our sample hedge funds have 5% or more ownership disclosure in a Schedule 13G and those observations where hedge funds have Schedule 13D filings that are switched from 13G. Columns (1) to (3) conduct regressions at the plant-year level using the Census data with change in TFP being the dependent variable. Columns (4) to (6) run regressions at the firm-year level using the Compustat data with change in ROA being the dependent variable. The change is recorded over a three year period, and for event observations the three year period starts with the event year.

	(1)	(2)	(3)	(4)	(5)	(6)
	Plant level—Census data			Firm level—Compustat data		
Dep. Var.	Change in TFP			Change in ROA		
G to D switch	0.106	0.132	0.089	0.030	0.033	0.025
	0.72	1.68	1.12	1.97	2.15	1.59
Controls?	Y	Y	Y	Y	Y	Y
HF fixed effects	N	Y	Y	N	Y	Y
Industry fixed effects	N	N	Y	N	N	Y
Observations	2983	2983	2983	3, 954	3, 954	3, 954
R2	1.26%	6.23%	12.91%	8.4%	8.9%	15.4%