Do Short-Term Incentives Affect Long-Term Productivity?*

Heitor Almeida
University of Illinois at Urbana-Champaign and NBER

Nuri Ersahin
Michigan State University

Vyacheslav Fos
Boston College, CEPR, and ECGI

Rustom M. Irani
University of Illinois at Urbana-Champaign and CEPR

Mathias Kronlund
Tulane University

August 2021

*Heitor Almeida is at the University of Illinois at Urbana-Champaign; 1206 South Sixth Street; Champaign, IL, 61820; U.S.A.; Email: halmeida@illinois.edu. Phone: (217) 333-2704. Nuri Ersahin is at Michigan State University; 632 Bogue St; East Lansing, MI 48824; U.S.A.; Email: ersahin2@msu.edu. Phone: (517) 353-1705. Vyacheslav Fos is at Boston College; 140 Commonwealth Avenue; Chestnut Hill, MA 02467; U.S.A.; Email: fos@bc.edu. Phone: (617) 552-1536. Rustom M. Irani is at the University of Illinois at Urbana-Champaign; 1206 South Sixth Street; Champaign, IL, 61820; U.S.A.; Email: rirani@illinois.edu. Phone: (217) 300-0472. Mathias Kronlund is at Tulane University; 7 McAlister Drive; New Orleans, LA, 70118; U.S.A.; Email: mkronlund@tulane.edu. Phone: (504) 865-6557. The research in this article was conducted while the authors were Special Sworn Status researchers of the U.S. Census Bureau at the Chicago Census Research Data Center. We are also very grateful to Frank Limehouse and Lanwei Yang at the Chicago and UIUC Census Research Data Centers, respectively, for their ongoing assistance. Any opinions and conclusions expressed herein are those of the authors 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.

Do Short-Term Incentives Affect Long-Term Productivity?

August 2021

Abstract

Previous research shows that short-term incentives lead the firm to increase stock buybacks, reducing investments in capital and employment. It is natural to expect that such firms will cut their less productive projects first, with little or even a positive effect on firm-level productivity. Yet, using detailed plant-level Census data, we find that firms make cuts across the board irrespective of each plant's productivity in response to short-term incentives. Unionization of the labor force drives these results by preventing firms from doing efficient downsizing, suggesting that stakeholders can amplify negative consequences of corporate short-termism.

JEL Classification: G32, G35, J23

Keywords: Productivity, Employment, Labor unions, Investment, Short-termism, Agency Frictions, Earnings per Share, Share repurchases

1 Introduction

Are U.S. public firms short-termist? Several recent papers identify actions that are suggestive of short-termism, such as reductions in firms' long-term investments around the time when managers' equity incentives vest (Edmans, Fang, and Lewellen, 2017; Ladika and Sautner, 2020) or reductions in R&D that allow firms to meet earnings forecasts (Terry, 2017). For a short-termism label to apply, it also implies that these actions will end up hurting firms and the economy in the long run. However, there is yet little evidence on how short-term incentives to boost current earnings affect firms' outcomes in the long run, for example, in terms of their effects on firms' future productivity.[1] This paper attempts to fill this gap.

Specifically, we study the long-term effects on future resource allocation and productivity that result from an incentive to engage in share repurchases in order to meet short-term performance targets ("EPS-motivated repurchases"). Hribar, Jenkins, and Johnson (2006) and Almeida, Fos, and Kronlund (2016) show that firms that are just about to miss the consensus earnings-per-share (EPS) forecast have significantly higher repurchases than firms that just meet the EPS forecast without conducting repurchases. Almeida et al. (2016) also show a similar discontinuity whereby these firms that have an incentive to conduct share repurchases end up cutting capital expenditures, employment, and R&D, which suggests that managers are willing to trade-off investments in labor or capital for stock repurchases that allow them to meet analyst EPS forecasts. While this behavior is indicative of short-termism, it is not clear whether it leads to a deterioration of firms' productivity in the long run. On the contrary, one might expect firms to cut their less productive investments first, thus avoiding any adverse effects on firms' overall productivity.

[1] Two recent papers study the long-term effects of short-term incentives (Almeida, Fos, Hsu, Kronlund, and Tseng, 2020; Edmans, Fang, and Huang, 2020). We discuss at the end of the introduction how our paper relates to these two contemporaneous papers.

To shed further light on this question, it is crucial to look more closely into the nature of the specific investments that get cut because of incentives to engage in EPS-motivated repurchases. Are firms making indiscriminate cuts "across the board," or are firms cutting less productive investments first? To answer this question, we use census data. These data allow us to examine changes in resource allocation and productivity at the plant level, and study how pre-existing plant characteristics are related to cuts due to these short-term incentives. Census data is crucial for our purposes, as firm-level data do not allow us to study within-firm changes in resource allocations.

Our identification strategy follows that in Almeida et al. (2016), which exploits a discontinuity in incentives to engage in repurchases when managers expect to 'just-miss' the analyst consensus EPS forecast. The key identification assumption behind this empirical strategy is that in the absence of a discontinuous jump in the incentive to repurchase around zero (pre-repurchase) EPS surprises, there are no other discontinuous changes in firm policies that directly affect outcome variables such as investments and long-term productivity (see section 2 for further discussion). Under this identification assumption, our tests measure the effect of incentives to engage in stock repurchases to meet short-term performance targets on investments and productivity.

We begin by using plant-level census data for manufacturing firms to replicate the main results in Almeida et al. (2016). Consistent with that paper, which instead analyzed firm-level outcomes based on Compustat data, we observe a significant decline in investment and employment expenditures in plants that belong to firms with incentives to allocate resources towards boosting short-term performance measures using buybacks (i.e., those just to the left of the zero pre-repurchase EPS surprise threshold).

Next, we focus on changes in total factor productivity (TFP), measured using the difference in TFP from one year before to three years after the "focal year" (i.e., the year in which we measure incentives to engage in EPS-motivated repurchases). We find that firms

with stronger incentives to engage in EPS-motivated repurchases experience a significant deterioration in average productivity across their plants. Overall, firm-level TFP falls by about 1.3%. Thus, we find that stronger incentives to boost short-term performance lead to cuts in investments and employment, which is accompanied by a drop in future firm-level productivity.

We then investigate the reasons why EPS-motivated repurchases may lead to a drop in firm-level TFP. To shed more light on this question, we study whether the cuts in investment and employment associated with EPS-motivated repurchases depend on each plant's pre-existing level of productivity. It would be natural to expect that cuts in investment and employment should be concentrated in relatively unproductive plants. However, we find evidence that firms make cuts uniformly across the board, irrespective of whether a plant is productive or unproductive.[2] This effect is crucially different from how firms are allocating investments across their plants in other times when they are not subject to short-term pressures: in that case, unproductive plants experience significantly lower growth in investment and employment.

Why is this relatively inefficient allocation of resources happening? To help explain that puzzle, we find evidence that this apparently inefficient reallocation of resources is at least partly a consequence of frictions in the labor market. Specifically, we investigate whether firm-level productivity changes after firms are faced with these short-term incentives depend on the fraction of the firm's business that is located in right-to-work (RTW) vs. non-RTW states. The mechanism we have in mind is that the labor force tends to be relatively more unionized and unions are thus more powerful in states that do not have RTW works, and that unions may act as a force that prevents firms from downsizing in the economically most efficient manner. Our results show that the reduction in TFP is concentrated only among

[2]In our main tests, plants are classified as "productive" and "unproductive" based on their within-firm TFP ranking. In robustness tests, we also consider within-industry rankings of plants plus several nonparametric measures of productivity.

firms with relatively more plants in non-RTW states, where the labor force is relatively more unionized. By contrast, firms primarily located in states with weaker labor unions tend not to experience any significant deterioration in firm productivity.

To better examine what drives these differences, we then investigate whether the nature of the cuts in employment and investment also differ across plants located in states that have adopted RTW legislation vs. those states that have not. Labor unions may act as an impediment to doing relatively more efficient cuts, at least to the extent that labor rules constrain a firm's actions (e.g., Bloom et al., 2019; Chava et al., 2020; Serfling, 2016). Consistent with this hypothesis, we find that plants located in RTW states (where the labor force is less organized), tend not to experience any cuts to either capital or employment. Firms with plants in RTW states that downsize tend to instead do so by selling or closing their unproductive plants. By contrast, the relatively indiscriminate cuts that we observe on average happening to productive and unproductive plants are concentrated in plants located in non-RTW states. These results show that possible frictions related to the unionization of the labor force appear to prevent firms from downsizing efficiently, and firms in more unionized states instead engage in less discriminate "across-the-board" cuts.

This paper is related to multiple strands of literature. First, this paper contributes to the growing literature on the real effects of short-termism. Philippon and Gutierrez (2017) suggest that short-termism—resulting from increases in both institutional ownership and investor activism—has contributed to a drop in long-term investment after the early 2000s. However, other empirical evidence does not support the claim that investor activism leads to short-termism. Instead, evidence by Brav et al. (2015), Brav et al. (2018), and Bebchuk et al. (2015) broadly suggests that activists increase long-term performance at the companies that they target. Kaplan (2018) also notes that there is little evidence that short-termism has affected long-term profits. In contrast to these results, our paper shows evidence that short-term incentives can result in the misallocation of resources and lower long-term

productivity. We also find that allocating more power to a stakeholder (e.g., labor) can amplify the consequences of corporate short-termism, while the absence of such frictions can mitigate these effects.

Second, our results are consistent with other recent papers that identify actions that indicate short-termism, such as a reduction in long-term investment due to vesting equity (Edmans et al., 2017; Ladika and Sautner, 2020), and a reduction in R&D growth that allows firms to meet earnings forecasts (Terry, 2017). This literature also suggests that short-term incentives can lead to long-term underperformance. Ladika and Sautner (2020) find that firms that accelerate option vesting following FAS-123R decrease their investment rates significantly, which can be detrimental to their long-term growth. Using a standard growth model, Terry (2017) shows that the increase in R&D volatility due to EPS targets can lead to lower growth and welfare losses of similar magnitude as those engendered by business cycles. However, much less is known about the productivity consequences of these cuts to investments since they can be productivity-enhancing (or -reducing) depending on whether the cuts are directed at relatively less (or more) productive units. Therefore, identifying which types of investments are cut is critical to evaluate the long-term effects of short-term incentives. To advance the literature along this dimension, our paper looks closely into the nature of the investments that are cut and presents direct evidence that short-termism affects firm-level productivity, and we identify conditions under which this effect occurs.

Two contemporaneous papers examine the real effects of short-term incentives. Almeida et al. (2020) focus on the effect of short-term incentives to increase a firm's earnings per share (EPS) on innovation outcomes, and Edmans et al. (2020) focus on the effects of short-term incentives created by vesting equity on repurchases, M&A investments, and returns. Our paper differs in two important ways. First, we focus on the resource allocation choices *within* firms and the resulting long-term productivity changes by exploiting plant-level census data. Second, our results show that short-term incentives not only affect the level of investment

or R&D but also distort the way resources are allocated within the boundaries of the firm. We believe that the findings in these papers complement each other. Edmans et al. (2020) also find evidence consistent with negative long-term consequences of short-term incentives; similar to the role of labor contracting frictions in our paper, their evidence appears to be driven by inefficient contracting between CEOs and firms. In contrast, Almeida et al. (2020) find a positive effect of short-term incentives on innovation outcomes. It is likely that innovation activity is less subject to the labor market frictions (e.g., unionization) that underlie the decline in productivity among the manufacturing firms in this paper.

Third, this paper contributes to the labor and finance literature. This literature has extensively documented that various characteristics of a firm's labor force can explain various aspects of firm policies, including capital structure (e.g., Agrawal and Matsa, 2013; Matsa, 2010; Serfling, 2016; Simintzi et al., 2014), cost of capital and firm value (e.g., Chen et al., 2011; Lee and Mas, 2012), and investments and sales growth (e.g., Bai et al., 2019). Our paper contributes to this literature by showing that allocating more power to labor can result in an adverse interplay with incentives for corporate short-termism.

Finally, our paper is consistent with earlier literature that suggests that chasing EPS targets is a driver of short-termism. Graham et al. (2005), for example, report survey evidence that CFOs are willing to suffer long-term negative consequences by cutting investments in order to meet short-term EPS targets. Our paper provides large-scale empirical evidence that such actions can adversely affect the efficiency of firms' resource allocation choices and their productivity.

2 Empirical strategy

This paper studies how the incentive to engage in EPS-motivated share repurchases affects future resource allocation and firm productivity. Two important empirical challenges

are involved when studying this question. First, identifying and measuring short-termist behavior is challenging; in other words, how might an outside observer know if a particular action taken by the firm is motivated by short-termist pressures? Second, firms' actions to respond to short-termist pressures, such as EPS-motivated repurchases, may be endogenous and thus confounded by omitted variables or selection. Identifying the counterfactual of what would have happened to the firm in the absence of such actions can thus be difficult. The setting of EPS-motivated share repurchases offers a compelling setting precisely because spending money on buybacks in order to just meet or beat an EPS target represents a clear and identifiable example of such incentives.

Our baseline approach of identifying plausibly causal effects on firms' productivity and resource allocation follows the "fuzzy regression discontinuity" framework of Almeida et al. (2016). The basic idea is that firms have a strong incentive to meet or beat their quarterly EPS consensus, and firms can use stock repurchases to raise their EPS to do so. We start by constructing a variable, pre-repurchase EPS surprise, which captures what the firm's EPS *would have been* if it did not engage in any buybacks, and show that firms who fall just below the zero-threshold are more likely to engage in share repurchases.[3] We then use this discontinuity in the incentive to engage in EPS-motivated repurchases to study the effects on future firm outcomes.

To understand the discontinuity, consider the following example. Suppose that the analyst EPS consensus forecast is \$3.00 a share and that the company has one billion shares outstanding. A manager learns that the actual reported EPS number is going to be \$2.99 a share. The manager can meet the forecast by doing share repurchases. For example, using \$600 million to repurchase stock at an assumed price of \$60 per share would reduce shares outstanding to 990 million. The company's earnings would also tend to decrease because the company forgoes interest payments on its cash holdings. Assuming, for example, that the

[3]This discontinuity is originally documented by Hribar et al. (2006).

interest rate is 5%, the firm's marginal tax rate is 30%, and the company forgoes one quarter of interest, the forgone interest is 1.25%*(1-30%)*\$600 million = \$5.25 million. Thus, total earnings would decrease from \$2.99 billion to \$2.98475 billion, resulting in a new EPS equal to \$3.01 (rounded to the nearest cent). This example illustrates how firms can move from a pre-repurchase EPS of \$2.99 to an actual EPS of \$3.01, or equivalently, moving the EPS surprise (relative to the analyst consensus) from -1 cent to +1 cent. It is important to note that the required repurchases are economically meaningful: changing EPS by just two cents involves spending cash representing 1% of the firm's equity value—this is more than four times larger than firms' average quarterly repurchases in our sample.

To capture the effect of the incentive to engage in EPS-motivated stock repurchases on outcome variables, we follow the regression-discontinuity framework and estimate the following regression:

$$\begin{aligned} Y_{i,t+} - Y_{i,t-} &= \beta_1 I_{Negative\ Sue_{adj},it} + \beta_2 Sue_{adj,it} + \beta_3 Sue_{adj,it} I_{Negative\ Sue_{adj},it} \\ &+ \beta_4 X_{it} + \eta + \epsilon_{it}. \end{aligned} \quad (1)$$

Y represents the outcome variables of interest for firm i (or plant i in our plant-level analysis); when we study the effects on future firm/plant outcomes, we define the dependent variable as the *change* in the variable of interest (firm or plant productivity), defined as the difference from the year before $(t-1)$ to the three-year average (average of $t+1$, $t+2$, and $t+3$) after the focal year t. Sue_{adj} is the pre-repurchase EPS surprise, $I_{Negative\ Sue_{adj}}$ is an indicator of having a negative pre-repurchase EPS surprise, X is a vector of controls, and η is a set of fixed effects. In firm-level regressions, we include year fixed effects, η_t. In plant-level regressions, X includes plant age and size, and η_{jt} and η_{st} are industry-times-year fixed effects and state-times-year fixed effects. State-times-year fixed effects allow us to control for the location of the plant, and any changes to local economic conditions that might affect

the outcome variables of interest. Finally, we cluster standard errors at the firm level to account for correlation across plants belonging to the same firm. We find similar results when cluster standard errors at the state where plants are located.

We calculate the pre-repurchase EPS surprise in two steps. First, we calculate the quarterly pre-repurchase EPS surprise, which is the difference between the repurchase-adjusted ("pre-repurchase") earnings per share (EPS) and the median EPS forecast at the end of the quarter; this difference is normalized by the end-of-quarter stock price. The pre-repurchase EPS is calculated as follows: $EPS_{adj} = E_{adj}/S_{adj} = (E + I)/(S + \Delta S)$, where E is reported earnings, I is the estimated forgone interest due to the repurchase, S is the number of shares at the end of the quarter, and ΔS is the estimated number of shares repurchased (the repurchase amount divided by the average daily share price). The foregone interest is the after-tax interest that would be earned on funds equal to that used to repurchase shares if it were instead invested in a 3-month T-bill. To isolate any differences around the threshold, we limit the sample to a small window around a zero pre-repurchase EPS surprise, $-0.003 \leq Sue_{adj} \leq 0.003$.

Second, because the outcome variables on employment, investment, and productivity involve annual data (discussed in 3), while earnings surprises can also be calculated at the quarterly level, we limit our analysis to the fourth quarter of each firm's fiscal year when analyzing the pre-repurchase EPS surprise.[4]

The key identification assumption behind this empirical strategy is as follows: in the absence of a discontinuous jump in the incentive to repurchase around zero pre-repurchase EPS surprises, there are no other discontinuous changes in firm policies around zero pre-repurchase

[4]In general, fourth-quarter earnings tend to be more influential. We obtain qualitatively similar results if we alternatively aggregate the quarterly pre-repurchase EPS surprise to an annual frequency by setting $I_{Negative\ Sue_{adj}}$ to 1 for that year if the firm's quarterly pre-repurchase EPS surprise is negative for at least two quarters in that year (in that case, the continuous variable $Sue_{adj,it}$ for that year is set to be the minimum of the pre-repurchase EPS surprises across negative surprise quarters, or conversely, when we set $I_{Negative\ Sue_{adj}}$ to 0 for that year, the continuous variable $Sue_{adj,it}$ for that year is set to be the minimum of quarterly pre-repurchase EPS surprises across positive surprise quarters.)

EPS surprises that directly affect our outcome variables. Our specification controls for time-invariant observable or unobservable characteristics, as our main variables are defined using differences between future and lagged outcomes. Because we control for the earnings surprise level, our test set-up also addresses the possibility that earnings surprises may proxy for stronger future economic fundamentals. A violation of the identification assumption would not only require an unobservable time-varying characteristic that independently predicts the outcome, but also a *discontinuity* in such a characteristic around the zero pre-repurchase EPS surprise threshold.

Because the census data is limited to manufacturing firms and thus more limited than the sample in Almeida et al. (2016), we begin by verifying whether there exists—also within our subsample of manufacturing firms—a discontinuity in the level of repurchases around the zero pre-repurchase EPS surprise threshold. To do so, we estimate equation (1) with $Repurchases_{it}$ as an outcome variable. $Repurchases_{it}$ are dollars of accretive repurchases, normalized by lagged assets.[5] Table A.1 and Figure A.1 in the Appendix show that the relation between the discontinuity in the pre-repurchase EPS and firms engaging in share buybacks is strong. That is, firms engage in significant additional buybacks if they would have missed their earnings estimates absent such buybacks.

In section 6, we will further show that firms that fall just to the right and the left of the pre-repurchase EPS surprise display similar characteristics and trends in the years leading up to the focal year (the year t when the pre-repurchase EPS surprise is measured). This supports the use of $I_{Negative\ Sue_{adj},it}$ (i.e., having a negative pre-repurchase EPS surprise) to identify the effect of the incentive to engage in EPS-motivated share repurchases on firms' future outcomes using a fuzzy regression discontinuity (RD) framework.

[5]Net repurchases are measured following Fama and French (2001), i.e., as the increase in common Treasury stock if Treasury stock is not zero or missing. If Treasury stock is zero in the current and prior quarters, we measure repurchases as the difference between stock purchases and stock issuances from the statement of cash flows. If either of these amounts is negative, repurchases are set to zero. We define an *accretive* share repurchase as a repurchase that increases the EPS by at least one cent, following Hribar et al. (2006).

Since firms use various techniques to manage earnings, a potential concern with our identification strategy is that firms around the discontinuity threshold may use other earnings management tools to meet analyst EPS forecasts (other than repurchases). Suppose firms use several earnings management tools *in response* to the discontinuity. In that case, our identification assumption still holds because, in the absence of a discontinuous jump in the incentive to increase EPS, there would be no discontinuous changes in firm policies around zero pre-repurchase EPS surprises. Instead, the principal concern is that firms just to the right and to the left of the discontinuity have taken steps to manage earnings.

We provide two pieces of evidence to mitigate this concern. First, firms could use accruals and changes in guidance to influence earnings surprises, although these would not necessarily involve influencing any real variables. While it is thus not clear how this possibility may confound our results, Almeida et al. (2016) show that the effect of short-term incentives on repurchases remains after controlling for accruals and changes in guidance. Further, there is no evidence that accruals or guidance are different for firms just to the right vs. the left of the discontinuity. Thus usage of such tools is unlikely to affect our results.

Second, we check if there is any discontinuity in the distribution of Sue_{adj} around the discontinuity. If firms used other earnings management tools to meet analyst EPS forecasts (other than repurchases), we would expect to see a higher density of just to the right of the discontinuity. In contrast, if firms do not use other earnings management tools around the discontinuity, we expect to see a smooth density around the discontinuity. Figure A.2 shows the distribution of Sue_{adj} around the discontinuity, indicating that the density is smooth around the discontinuity, suggesting that firms do not use other earnings management tools around the discontinuity.[6]

[6]The absence of aggressive usage of other earnings management tools is not surprising because stock repurchases are uniquely suitable to manage EPS, rather than an absolute measure of performance (e.g., net income). In contrast to other earnings management tools that shift performance over time, stock repurchases have a permanent impact on EPS. Once the number of shares outstanding is reduced, EPS would shift to a higher trajectory.

3 Data

This section describes our data and variable construction.

Establishment-level data are provided by the U.S. Census Bureau. Our primary data sources are the Census of Manufactures (CMF) and the Annual Survey of Manufactures (ASM). These two data products provide highly granular information on the economic activity of manufacturing establishments ("plants"). Manufacturing plants have NAICS codes between 3111 and 3399. The CMF is a survey conducted every five years (years ending in 2 and 7) and consists of all manufacturing establishments in the United States with at least one paid employee. The ASM is another survey conducted in non-census years (i.e., when the CMF is not conducted) for a subset of these manufacturing plants. This includes all plants with more than 250 employees, plus smaller plants with fewer employees that are selected with a probability positively correlated with their size. Reporting for both of these surveys is mandatory and misreporting is penalized, so the data is of the highest quality. Both the CMF and ASM include information on location, industry, corporate affiliation, output (total value of shipments), employment, capital expenditures, and material inputs of each plant. The detail of these manufacturing data sets helps us measure factor inputs and construct various productivity measures for each manufacturing plant.

Our firm-level data comes from CRSP/Compustat, and analyst forecasts used for calculated earnings surprises are from IBES. These databases include stock prices, balance sheets, and income statement data for publicly traded U.S. corporations, which are the focus of this study. We calculate repurchase, stock price, and earnings surprise data required to calculate the repurchase-adjusted earnings per share, as described in section 2. We use the Compustat-SSEL bridge maintained by the Census to match each Compustat firm to its manufacturing plants. The Compustat-SSEL bridge ends in 2011, so we extend the match to 2013 using employer characteristics, including name, address, and employer identification number.

We capture how firms allocate resources using employment and investment data from the CMF/ASM. The change in employment is measured as the average of the firm's employment expenditures (salaries and wages, i.e., payroll) in the three years after the focal year minus the value in the year before the focal year, normalized by the lagged plant-level capital stock. The plant-level capital stock is estimated using the perpetual inventory method following Brav et al. (2015), and described in detail in Ersahin (2020) and Giroud (2013). When we measure employment outcomes at the firm level, we sum the employment expenditures across all of the firm's plants.

For robustness, we also consider two alternative measures of employment. First, we also use the change in the natural logarithm of the number of employees. Second, we use the symmetric growth rate of employment, calculated by dividing the three-year average change in the number of employees by the average of the current and lagged number of employees. This measure accommodates both entry and exit and limits the effects of extreme values (Davis et al., 1998).

Alongside employment, we also analyze changes in investment. We calculate investment as the three-year average change in plant-level capital expenditures scaled by the lagged plant-level capital stock. For robustness, we also consider plant-level expenditures on machinery equipment. As with employment, firm-level investment is calculated by aggregating across plants.

We measure plant productivity as the natural logarithm of total factor productivity (TFP) following the methodology of Foster et al. (2016). In particular, TFP is given by:

$$TFP_{it} \;=\; lnQ_{it} \;-\; \alpha_{kt} lnK_{it} \;-\; \alpha_{lt} lnL_{it} \;-\; \alpha_{mt} lnM_{it}, \tag{2}$$

a where i and t index plants and years, respectively. The variables TFP, Q, K, L, M, and α represent total factor productivity, real output, capital stock, labor input, cost of materials

and parts, and factor elasticities. We measure output as the sum of the plant's total value of shipments and the change in inventories for finished goods and work-in-progress. We obtain real output by deflating output using industry-level prices provided by the NBER-CES Manufacturing Industry Database.

As alternative productivity measures, we also analyze operating margins and the productivities of labor and capital separately (Bai, 2021; Giroud, 2013). The operating margin is measured by scaling the total value of shipments minus labor and material costs by the total value of shipments. The advantage of operating margin is that it does not require any structural assumptions, such as a Cobb-Douglas production function. We measure labor productivity as the total value of shipments minus material and energy costs divided by total employment expenditures. Capital productivity is measured by return on capital (ROC), calculated as the total value of shipments minus labor, material, and energy costs scaled by capital stock. All inputs are measured in 1997 dollars.

Our final sample contains 3,300 firm-year observations covering approximately 35,000 plant-years for the period from 1988 until 2013. Table I presents summary statistics for the full sample. This table also separately reports statistics for subsamples based on whether firms have slightly positive or negative pre-repurchase EPS surprises (Panel A) and depending on the firm's presence in states with right-to-work laws in place (Panel B).[7]

[Insert Table I here]

4 Results

This section estimates the effect of incentives to invest resources in boosting short-term performance measures on firms' resource allocation and productivity, employing a fuzzy regression discontinuity (RD) framework as described in section 2.

[7]As per Census disclosure requirements, we round off the number of observations in each table and quantile values are not reported in any summary statistics table.

4.1 Overall effects on investment, employment, and productivity

We begin our analysis by estimating regression (1) using firm-level data on investment and employment. The results are reported in Panel A of Table II. We find that firms with incentives to spend current resources on repurchases to boost EPS experience future decreases in both employment expenditures and capital investments (columns 1 and 2). For instance, firms invest on average 3.6% (measured as a fraction of their capital stock) less in employment expenditures, representing a reduction of around 6.5% of the mean firm-level employment expenditures.

[Insert Table II here]

These firm-level changes to employment and investment could capture either cuts within existing plants, or alternatively, the separation of plants from the firm because of closures or sales of entire establishments. In column 3, we find little evidence of more separations around these events, measured as the fraction of a firm's plants that are separated over the three years after the focal year. This result suggests that the bulk of the reduction to employment/investment occurs within existing plants (intensive margin), rather than on the extensive margin by selling off or closing plants.

In Panel B of Table II, we next study the plant-level effects on employment and investment. These plant-level results are broadly similar to the firm-level findings in Panel A. We observe significant cuts in both investment and employment in plants that belong to firms with incentives to invest resources in boosting short-term performance measures.

Overall, both the firm-level and plant-level results in Table II are consistent with the headline results of Almeida et al. (2016) and suggest that firms' incentives to spend resources on stock repurchases to boost EPS result in changes in spending on both labor and capital.

We next consider the relation between EPS-motivated repurchases and changes in productivity, measured as changes to Total Factor Productivity ($\Delta\, TFP$). In plant-level analysis,

we measure ΔTFP as the difference between the three-year average future productivity of plant j in firm i and the lagged productivity of that plant. In firm-level analysis, TFP is the capital-weighted average of the individual plant-level TFPs (Giroud and Mueller, 2015; Schoar, 2002). ΔTFP is the difference between the three-year average future productivity of firm i and the lagged productivity of that firm. The results are reported in Table III.

[Insert Table III here]

We find that firms with stronger incentives to engage in EPS-motivated repurchases experience a significant deterioration in productivity. The economic magnitude of these effects is also sizable. Specifically, firm-level productivity falls by 0.025, representing a 1.3% drop compared to the average level of TFP. Plant-level productivity also drops by a similar magnitude. Overall, we find that stronger incentives to boost short-term performance result in cuts to investments and employment and that these cuts also take place alongside a drop in productivity. Figure 1 shows visual evidence that there is a drop in productivity right at the point at which there is a jump in short-term incentives (the zero pre-repurchase EPS surprise threshold).

[Insert Figure 1 here]

4.2 Resource allocation between productive vs. unproductive plants

Next, we dig deeper into the relationship between investment and employment cuts and productivity. One of the key advantages of using plant-level data is that it enables us to take a closer look at the nature of investments and employment that are cut. In line with this idea, we study whether these cuts in investment and employment vary across plants within a firm depending on each plant's pre-existing level of productivity. If any cuts in investment and employment primarily take place in less productive plants, that would suggest that the

allocation of resources within firms is relatively efficient even in the light of short-termist incentives. In contrast, if cuts occur in productive plants, that would be consistent with relatively indiscriminate and thus less efficient allocation of resources driven by such short-termist incentives.

To address this question, we estimate regression (1) using plant-level data and interact $I_{Negative\ Sue_{adj}}$ with two indicator variables, $Productive_{t-1}$ and $Unproductive_{t-1}$. $Productive_{t-1}$ is a dummy variable equal to one if the plant has an above-median within-firm total factor productivity in the year before the focal year t when the firm has an incentive to spend on buybacks. $Unproductive_{t-1}$ is defined analogously. The results are reported in Table IV.

[Insert Table IV here]

The first row of Table IV shows that, in general (i.e., when the firm is not motivated by short-termism), unproductive plants tend to experience larger cuts to both employment and investment. In other words, most of the time, unproductive plants experience proportionately greater cuts than productive ones.

The next rows show how productive and unproductive plants differ specifically when the firm is subject to short-termist pressures, and here, the results show evidence of less efficient cuts. Specifically, estimated coefficients on *Negative Pre-Repurchase EPS Surprise*$\times Productive_{t-1}$ and *Negative Pre-Repurchase EPS Surprise*$\times Unproductive_{t-1}$ show that cuts in employment and investment appear roughly equally large and are also statistically indistinguishable between unproductive plants and productive plants when firms make cuts motivated by short-termism. The regression coefficient suggests that the cuts to employment are perhaps even larger in productive plants, although the difference in the coefficients between the productive and unproductive plants is not statistically significant (p-value of 0.58 for the difference). These results are similar when analyzing investment outcomes. Column (3) shows that neither productive nor unproductive plants are more likely to be separated

from the firm.

Taken together, results in Table IV show that short-termist incentives lead firms to make indiscriminate investment and employment cuts across the board, irrespective of whether a plant is productive or unproductive.

4.3 The dynamics of the consequences for productivity and resource allocation

In Table V, we next examine the dynamic progression of the measured effects. Our main results examine the average productivity and growth in employment and investment over three years after the focal year compared to the year before the focal year t (the year when we measure the pre-repurchase EPS surprise and some firms have an incentive to engage in buybacks). However, we would expect changes to employment not to be immediate but may take some time to show up strongly in the data. It is also possible that the significant cuts to investment that we find may not be immediate as firms may react with some lag, for example, due to frictions that make it difficult to make sudden changes to such investments.

[Insert Table V here]

Panel A of Table V studies the dynamics of these effects at the *firm-level*. These results show that the effects on productivity are fairly immediate but continue to persist over all three years after the focal year (columns 1–3). By contrast, employment changes grow uniformly over time, consistent with employment adjustments taking more time to be enacted (columns 4–6). This is consistent with high adjustment costs of labor. The cuts to investment are fairly immediate, and most of the effect is realized in the first year, although there continue to be relatively smaller cuts also in the second and third year (columns 7–9). In economic terms, the effect on employment grows by almost three times between the first and third year, and by around 50% for investment over the same period.

In Panel B of Table V, we examine the dynamic trends of resource allocation at the plant level, where we can further interact these effects by whether a plant is relatively productive or unproductive. The table shows that the employment cuts follow similar patterns across both productive and unproductive plants, as both types of plants experience monotonically increasing effects, growing by 3–4 times from the first year to the third year. The cuts to unproductive plants represent the most immediate effects for investment; this seems to be evidence of relatively more efficient cuts, supporting higher productivity. Still, this difference is short-lived, as, by the second year, the cuts to productive plants are equally large as those in unproductive plants.

5 Why don't firms reallocate more efficiently? The role of labor frictions

The previous finding that productivity drops in the long run and that firms do significant cuts also to their productive plants raises the question of why firms do not allocate their resources more efficiently to mitigate the negative long-run impact of short-term pressures. To that end, we hypothesize that firms might face frictions that prevent them from allocating resources efficiently or otherwise distort their incentives.

This section investigates one such friction: whether labor bargaining power affects resource allocation within the firm when faced with short-termist incentives. Following Chava et al. (2020) and Bloom et al. (2019), we use right-to-work (RTW) laws as source of variation in labor bargaining power. These laws prohibit any agreement between employers and unions requiring employees to contribute to the unions, thus weakening union power. Our analysis examines whether cuts in employment and investment are similar in states that have adopted RTW laws vs. those that have not. Labor unions often criticize firms for being short-term-oriented. On the other hand, labor unions may themselves act as an impediment

to doing relatively more efficient reallocation of resources, to the extent that labor rules constrain a firm's actions. Therefore, it is interesting to examine whether companies with incentives to boost short-term performance measures through EPS-motivated repurchases engage in a relatively more or less efficient allocation of resources depending on whether unions have more or less bargaining power.

To study this question, we start by measuring each firm's overall exposure to non-RTW vs. RTW states by calculating the fraction of plants located in RTW states and the fraction of employees located in RTW states. We then investigate whether the firm-level productivity changes or the efficiency of resource allocation depend on the fraction of the firm's business located in RTW states, by splitting firms into two groups based on whether their exposure to RTW states is above or below the sample median.

We start by studying the effects on productivity. The results are reported in Table VI and Figure 2. In Panel A, we see that the entirety of the full-sample negative productivity effects come from firms that are more likely to be unionized, i.e., those with a below-median fraction of plants (or production hours) in RTW-states. By contrast, the effects on productivity are close to zero in firms with an above-median fraction of plants in RTW-states and thus are less subject to a unionized labor force.[8] Figure 2 shows graphically that the drop in productivity associated with short-term incentives happens only at plants located in non-RTW-states where the labor force is more likely to be unionized.

[Insert Table VI here]

[Insert Figure 2 here]

These results support a conjecture whereby firms with stronger labor unions undertake less efficient cuts in order to meet short-term EPS goals. These firms consequently experience significant deterioration in firm productivity. In contrast, firms with weaker labor

[8] As of 2013, the last year in our sample, the unionization membership in non-RTW states is 13.9%, and the rate in RTW states is 6.7%. The correlation coefficient between non-RTW and unionization is 0.67.

unions appear to undertake more efficient cuts and consequently experience no significant deterioration in firm productivity.

To further collaborate the results on productivity in Panel A, in Panel B, we next study the effects on employment and investment; for this analysis, we consider two sub-samples of plants: plants located in states that have adopted RTW legislation and plants located in states that have not adopted RTW legislation.

We observe striking differences across labor unionization levels in how firms respond to having short-term incentives to repurchase shares. Columns 1 and 2 of Panel B in Table VI show that for plants located in RTW states—where the labor force is less organized—we observe cuts to neither employment nor investment. However, on the extensive margin, we see that firms with plants in these states are more likely to sell or close their unproductive plants (column 3). Aggregating the intensive and extensive margins points to an overall cut to unproductive plants, which indicates that firms located in RTW states respond relatively efficiently to the pressure to boost short-term performance measures.

By contrast, we observe very different responses for plants located in non-RTW states. Columns 4 and 5 of Panel B in Table VI show that these plants experience significant cuts in employment not only for unproductive plants but also for productive plants. Moreover, also the cuts to investment are similar across both productive and unproductive plants.

Overall, our findings suggest that plants located in non-RTW states experience cuts that end up hurting their productivity compared to plants located in RTW states, indicating that union power adversely affects the efficient reallocation of resources in response to short-term pressures.

6 Robustness

In this section, we discuss several robustness tests for our results.

We first examine the extent to which our results could be sensitive to alternative ways of measuring our key variables. Panels A and B of Table VII show first that our baseline results from Tables II and III are robust to alternative measures of changes to productivity and resource allocation. As to productivity, a concern with our TFP measure is that it relies on structural assumptions, including the Cobb-Douglas production function (Giroud, 2013). We consider alternative measures that do not involve structural assumptions, including operating margin, labor productivity, and return on capital (ROC). We find that our productivity (TFP) results continue to hold if we examine changes to operating margin, labor productivity, and return on capital (ROC), in columns 1 to 3.

Table VII further shows that the baseline cuts in investments when firms are faced with an incentive to engage in EPS-motivated buybacks continue to hold if we limit the investments to only those in machinery (column 4), defined as plant-level machinery expenditures scaled by the lagged machinery stock. Similarly, our employment results are similar if we examine the change in log employment (rather than employment expenditures scaled by lagged capital as in the baseline results) or use a measure based on 'symmetric employment growth' (columns 5 and 6).[9] Further, these results with alternative measures hold both at the firm-level (Panel A) and the plant-level (Panel B).

In Panel C of Table VII we re-examine the results from Table IV using alternative ways of defining the splits between productive and unproductive plants. In columns 1 and 3, we use a within-industry split on productivity. In columns 2 and 4, we use a split on the within-firm marginal productivity of labor and within-firm return on capital for our results on changes to employment and investments, respectively (Ersahin et al., 2021).[10] The results are very similar to the baseline results: We observe significant cuts to both employment and

[9]The symmetric growth rate of employment is calculated by dividing the three-year average change in the number of employees by the average of the current and lagged number of employees. This measure accommodates both entry and exit and limits the effects of extreme values (Davis et al., 1998).

[10]This compares to our use of a within-firm split on productivity in our baseline results in Table IV.

investments in capital that appear similar across both productive as well as unproductive plants.

Table VIII presents several robustness tests for our regression discontinuity framework. Panel A shows that our baseline results from Tables II and III are not sensitive to using a smaller bandwidth (of 0.001 instead of 0.003 in the baseline analysis), or to using a third-degree polynomial control for the level of the pre-repurchase EPS surprise.

Panel B of Table VIII further shows that firms in our sample that fall on either side of the pre-repurchase EPS surprise are similar to each other in terms of changes to the outcome variables during the years immediately before the focal year t, i.e., they follow parallel trends, which is consistent with our main identification assumption as was discussed in section 2. This supports the use of the regression discontinuity framework. Specifically, we find no systematic pre-existing differences in either the changes to productivity, employment, or capital expenditures on either side of the zero pre-repurchase EPS threshold in either of two years before the focal year.

Finally, to further support the parallel trends assumption, and as a type of "placebo" test, Panel C in Table VIII reports results where we consider defining the "negative pre-repurchase EPS" variable, but where this variable is *shifted* by three years from the true event period. That is, we assume a firm was treated (in the sense of having a negative pre-repurchase EPS surprise) three years before it actually was, and then study changes to resource allocation and productivity for productive and unproductive plants like Table VI. We find no results around this "placebo event". This is also consistent with our identification assumption.

7 Conclusion

This paper studies the long-term productivity effects resulting from incentives to engage in share buybacks to meet short-term performance targets ('EPS-motivated repurchases'). We do so using census data, which allows us to examine changes in resource allocation and productivity at the plant level, and study how pre-existing plant characteristics relate to these changes.

Our evidence suggests that short-term incentives lead to lower long-term productivity, but only if additional frictions prevent firms from downsizing efficiently. Specifically, we find that firms with incentives to conduct EPS-motivated repurchases subsequently experience drops in their future productivity following reductions in their investment and employment. Consistent with inefficient downsizing, we find that the cuts that firms make to their employment and investment appear indiscriminate in that these cuts are evenly distributed across both their productive and unproductive plants.

The apparent puzzle of why reallocation appears relatively inefficient can be partly explained through labor frictions that firms face. In particular, we find that the adverse productivity consequences and the related indiscriminate cuts to employment and investment tend to be concentrated among firms and plants in non-right-to-work states, which are characterized by higher rates of unionization.

Our main contribution is to provide a novel measure of the long-term consequences of short-term incentives that focuses on within-firm reallocation of capital and labor. We focus on short-term incentives to conduct stock repurchases. Future research could focus on other actions that have been suggested to indicate short-termism, such as an increase in the volatility of R&D (Terry, 2017), or investment cuts due to vesting equity (Edmans et al., 2017; Ladika and Sautner, 2020). While we focus on frictions arising from unionization, other frictions may also be important to understand the long-term effects of short-term incentives.

That is another promising avenue for future literature.

References

Agrawal, Ashwini K., and David A. Matsa, 2013, Labor unemployment risk and corporate financing decisions, *Journal of Financial Economics* 108, 449–470.

Almeida, Heitor, Vyacheslav Fos, Po-Hsuan Hsu, Mathias Kronlund, and Kevin Tseng, 2020, Do short-term incentives hurt innovation?, working paper.

Almeida, Heitor, Vyacheslav Fos, and Mathias Kronlund, 2016, The real effects of share repurchases, *Journal of Financial Economics* 119, 168–185.

Bai, John (Jianqiu), 2021, Organizational form and trade liberalization: Plant-level evidence, *Management Science, forthcoming* .

Bai, John (Jianqiu), Douglas Fairhurst, and Matthew Serfling, 2019, Employment protection, investment, and firm growth, *Review of Financial Studies* 33, 644–688.

Bebchuk, Lucian A, Alon Brav, and Wei Jiang, 2015, The long-term effects of hedge funds activism, *Colum. L. Rev.* 115, 1085.

Bloom, Nicholas, Erik Brynjolfsson, Lucia Foster, Ron Jarmin, Megha Patnaik, Itay Saporta-Eksten, and John Van Reenen, 2019, What drives differences in management practices?, *American Economic Review* 109, 1648–83.

Brav, Alon, Wei Jiang, and Hyunseob Kim, 2015, The real effects of hedge fund activism: Productivity, asset allocation, and labor outcomes, *Review of Financial Studies* 28, 2723–2769.

Brav, Alon, Wei Jiang, Song Ma, and Xuan Tian, 2018, How does hedge fund activism reshape corporate innovation?, *Journal of Financial Economics* 130, 237–264.

Chava, Sudheer, Andras Danis, and Alex Hsu, 2020, The impact of right-to-work laws on worker wages: Evidence from collective bargaining agreements, *Journal of Financial Economics* 137, 451–469.

Chen, Huafeng Jason, Marcin Kacperczyk, and Hernán Ortiz-Molina, 2011, Labor unions, operating flexibility, and the cost of equity, *Journal of Financial and Quantitative Analysis* 46, 25–58.

Davis, Steven J, John C Haltiwanger, Scott Schuh, et al., 1998, Job creation and destruction, *MIT Press Books* 1.

Edmans, Alex, Vivian Fang, and Allen Huang, 2020, The long-term consequences of short-term incentives, working paper.

Edmans, Alex, Vivian W Fang, and Katharina A Lewellen, 2017, Equity vesting and investment, *Review of Financial Studies* 30, 2229–2271.

Ersahin, Nuri, 2020, Creditor rights, technology adoption, and productivity: Plant-level evidence, *Review of Financial Studies* 33, 5784–5820.

Ersahin, Nuri, Rustom M. Irani, and Hanh Le, 2021, Creditor control rights and resource allocation within firms, *Journal of Financial Economics* 139, 186–208.

Fama, Eugene F, and Kenneth R French, 2001, Disappearing dividends: changing firm characteristics or lower propensity to pay?, *Journal of Financial Economics* 60, 3–43.

Foster, Lucia, Cheryl Grim, and John Haltiwanger, 2016, Reallocation in the great recession: Cleansing or not?, *Journal of Labor Economics* 34, S293–S331.

Giroud, Xavier, 2013, Proximity and investment: Evidence from plant-level data, *Quarterly Journal of Economics* 128, 861–915.

Giroud, Xavier, and Holger M. Mueller, 2015, Capital and labor reallocation within firms, *Journal of Finance* 70, 1767–1804.

Graham, John R, Campbell R Harvey, and Shiva Rajgopal, 2005, The economic implications of corporate financial reporting, *Journal of Accounting and Economics* 40, 3–73.

Hribar, Paul, Nicole Thorne Jenkins, and W. Bruce Johnson, 2006, Stock repurchases as an earnings management device, *Journal of Accounting and Economics* 41, 3–27.

Kaplan, Steven N, 2018, Are US companies too short-term oriented? some thoughts, *Journal of Applied Corporate Finance* 30, 8–18.

Ladika, Tomislav, and Zacharias Sautner, 2020, Managerial short-termism and investment: Evidence from accelerated option vesting, *Review of Finance* 24, 305–344.

Lee, David S., and Alexandre Mas, 2012, Long-run impacts of unions on firms: New evidence from financial markets, 1961–1999, *Quarterly Journal of Economics* 127, 333–378.

Matsa, David, 2010, Capital structure as a strategic variable: Evidence from collective bargaining, *Journal of Finance* 65, 1197–1232.

Philippon, Thomas, and German Gutierrez, 2017, Investment-less growth: An empirical investigation, *Brookings Papers on Economic Activity* 89–190.

Schoar, Antoinette, 2002, Effects of corporate diversification on productivity, *Journal of Finance* 57, 2379–2403.

Serfling, Matthew, 2016, Firing costs and capital structure decisions, *Journal of Finance* 71, 2239–2286.

Simintzi, Elena, Vikrant Vig, and Paolo Volpin, 2014, Labor protection and leverage, *The Review of Financial Studies* 28, 561–591.

Terry, Stephen J., 2017, The macro impact of short-termism, Working Paper.

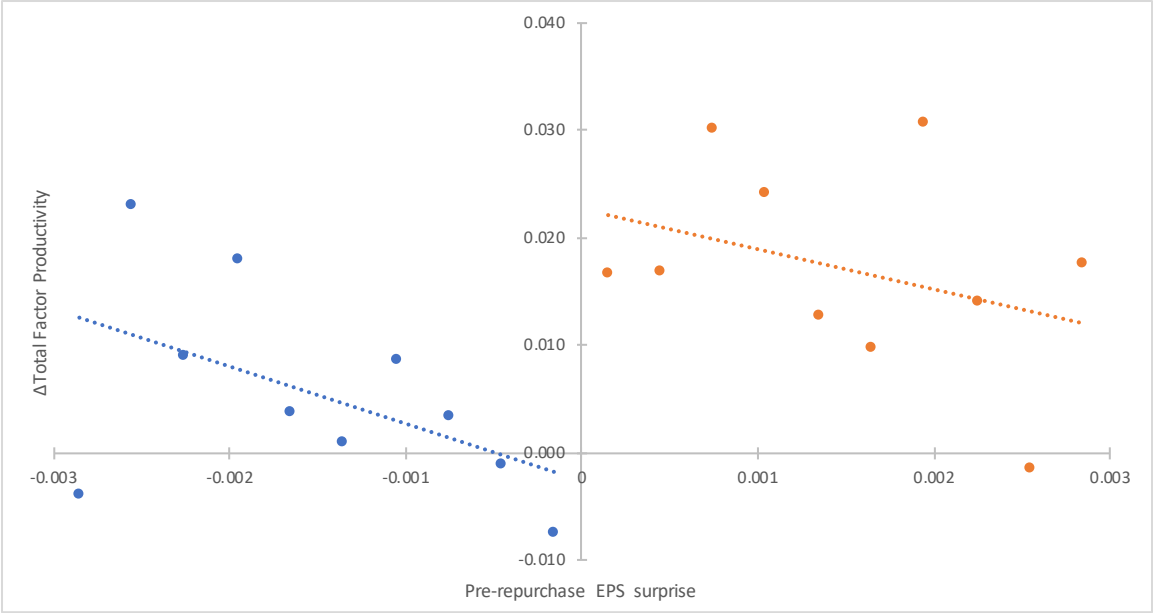
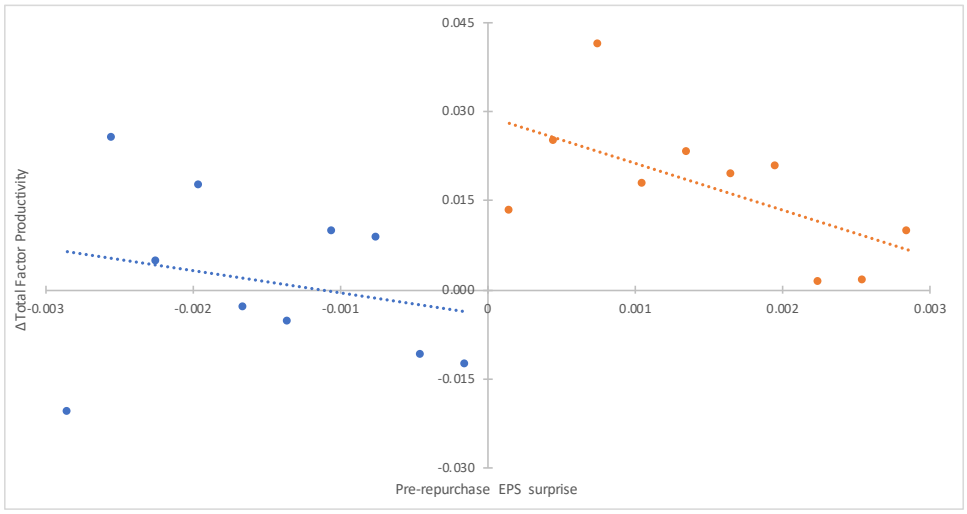
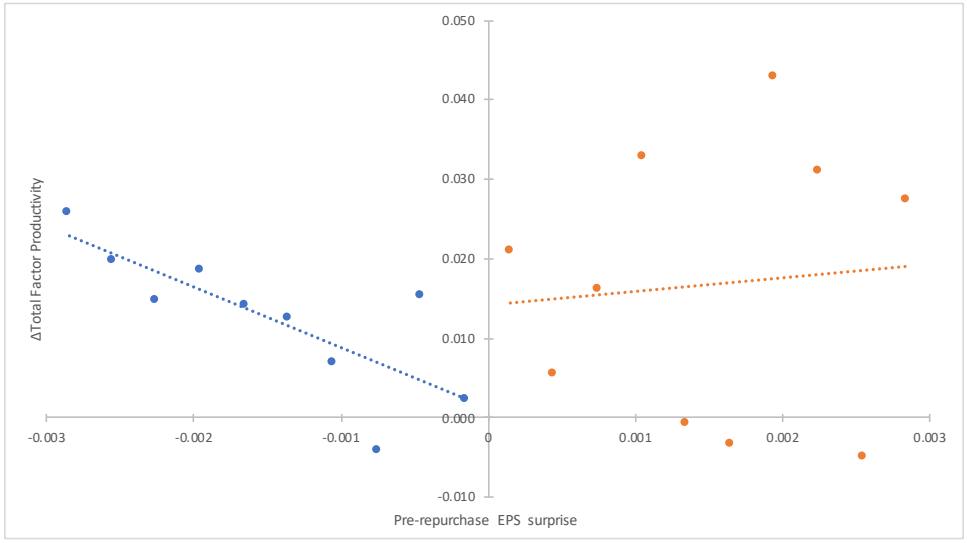


Figure 1: Negative pre-repurchase EPS surprises and change in productivity among manufacturing plants

This figure shows the difference in future productivity depending on the pre-repurchase EPS surprise. The difference in productivity (y-axis) is the average TFP over the following three years less the lagged TFP. The pre-repurchase EPS surprise (x-axis) is the difference between the repurchase-adjusted ("pre-repurchase") EPS and the median end-of-quarter EPS forecast, scaled by the end-of-quarter stock price.



(a) Plants in non-RTW states



(b) Plants in RTW states

Figure 2: Negative pre-repurchase EPS surprises and change in productivity among manufacturing plants: Split samples by right-to-work (RTW) laws

This figure shows the results in Figure 1 for split samples depending on right-to-work (RTW) laws. Panel (a) shows the difference in future productivity depending on the pre-repurchase EPS surprise among plants located in non-right-to-work states. Panel (b) shows the difference in future productivity depending on the pre-repurchase EPS surprise among plants located in right-to-work states.

Table I
Summary statistics

This table provides sample summary statistics. Panel A shows summary statistics split by whether the firm has a slightly negative (from –0.003 to 0) or positive (0 to +0.003) pre-repurchase earnings per share (EPS) surprise. The pre-repurchase EPS surprise is the difference between the repurchase-adjusted ("pre-repurchase") EPS and the median end-of-quarter EPS forecast, scaled by the end-of-quarter stock price. Panel B splits the sample by the firm- and plant-level "exposure" to state-level right-to-work (RTW) laws. Here, firms are sorted according to whether or not they have an above-median number of plants located in RTW states. Plants are sorted according to whether they are or are not located in RTW states. The unit of observation is a firm-year and plant-year for firm-level and plant-level statistics, respectively. All variables are defined in Appendix A. The list of states adopting RTW laws are listed in Appendix B.

Panel A: Summary statistics by (slightly) negative/positive pre-repurchase EPS surprise

	N	Mean	Std.	N	Mean	Std.	N	Mean	Std.
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
				Pre-repurchase EPS surprise					
Firm-level summary statistics	Full sample			Slightly negative [–0.003,0)			Slightly positive [0, +0.003)		
TFP	3,300	1.95	0.55	1,000	1.94	0.54	2,300	1.96	0.565
Capital Productivity (ROC)	3,300	1.47	1.60	1,000	1.41	1.49	2,300	1.50	1.65
Labor Productivity (MPL)	3,300	4.51	4.35	1,000	4.64	4.43	2,300	4.46	4.32
Employment	3,300	0.55	0.41	1,000	0.51	0.38	2,300	0.57	0.42
Log(#Employees)	3,300	7.52	1.45	1,000	7.56	1.45	2,300	7.51	1.46
Symmetric Employment Growth Rate	3,300	0.03	0.52	1,000	0.01	0.50	2,300	0.05	0.53
Investment	3,300	0.10	0.07	1,000	0.09	0.07	2,300	0.10	0.07
Machinery Investment	3,300	0.14	0.09	1,000	0.14	0.09	2,300	0.14	0.09
%Closed	3,300	0.08	0.13	1,000	0.08	0.13	2,300	0.08	0.13
%Sold	3,300	0.04	0.12	1,000	0.05	0.12	2,300	0.04	0.12
Plant-level summary statistics									
TFP	35,000	1.85	0.60	11,500	1.89	0.61	23,500	1.84	0.59
Capital Productivity (ROC)	35,000	1.55	1.96	11,500	1.48	1.85	23,500	1.59	2.01
Labor Productivity (MPL)	35,000	5.11	6.24	11,500	5.25	6.61	23,500	5.04	6.04
Employment	35,000	0.55	0.51	11,500	0.53	0.47	23,500	0.57	0.53
Log(#Employees)	35,000	5.26	1.26	11,500	5.26	1.26	23,500	5.27	1.26
Symmetric Employment Growth Rate	35,000	–0.04	0.37	11,500	–0.04	0.36	23,500	–0.03	0.37
Investment	35,000	0.09	0.09	11,500	0.08	0.09	23,500	0.09	0.09
Machinery Investment	35,000	0.12	0.12	11,500	0.12	0.12	23,500	0.12	0.12
Plant Age	35,000	2.81	0.60	11,500	2.79	0.57	23,500	2.82	0.60
Plant size	35,000	10.82	1.34	11,500	10.77	1.35	23,500	10.84	1.33

Panel B: Summary statistics by exposure to right-to-work (RTW) laws

	N	Mean	Std.	N	Mean	Std.	N	Mean	Std.
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
				Presence in RTW states					
Firm-level summary statistics	Full sample			Above median % of plants in RTW states			Below median % of plants in RTW states		
TFP	3,300	1.95	0.55	1,700	1.89	0.52	1,700	2.01	0.57
Capital Productivity (ROC)	3,300	1.47	1.60	1,700	1.42	1.57	1,700	1.52	1.63
Labor Productivity (MPL)	3,300	4.51	4.35	1,700	4.41	3.90	1,700	4.63	4.77
Employment	3,300	0.55	0.41	1,700	0.53	0.41	1,700	0.57	0.40
Log(#Employees)	3,300	7.52	1.45	1,700	7.75	1.51	1,700	7.29	1.35
Symmetric Employment Growth Rate	3,300	0.03	0.52	1,700	0.01	0.50	1,700	0.06	0.54
Investment	3,300	0.10	0.07	1,700	0.09	0.07	1,700	0.10	0.08
Machinery Investment	3,300	0.14	0.09	1,700	0.13	0.08	1,700	0.14	0.10
%Closed	3,300	0.08	0.13	1,700	0.08	0.13	1,700	0.07	0.14
%Sold	3,300	0.04	0.12	1,700	0.05	0.12	1,700	0.04	0.11
Plant-level summary statistics	Full sample			Plants in RTW state			Plants in non-RTW state		
TFP	35,000	1.85	0.60	14,000	1.79	0.59	21,000	1.90	0.59
Capital Productivity (ROC)	35,000	1.55	1.96	14,000	1.57	2.05	21,000	1.53	1.90
Labor Productivity (MPL)	35,000	5.11	6.24	14,000	5.32	6.70	21,000	4.96	5.89
Employment	35,000	0.55	0.51	14,000	0.54	0.50	21,000	0.56	0.51
Log(#Employees)	35,000	5.26	1.26	14,000	5.30	1.26	21,000	5.24	1.26
Symmetric Employment Growth Rate	35,000	–0.04	0.37	14,000	–0.03	0.37	21,000	–0.04	0.37
Investment	35,000	0.09	0.09	14,000	0.09	0.09	21,000	0.08	0.09
Machinery Investment	35,000	0.12	0.12	14,000	0.12	0.12	21,000	0.12	0.12
Plant Age	35,000	2.81	0.60	14,000	2.78	0.61	21,000	2.82	0.59
Plant size	35,000	10.82	1.34	14,000	10.86	1.33	21,000	10.79	1.34

Table II
Short-term incentives and resource allocation

This table shows estimates of the impact of incentives to spend resources on EPS-motivated share repurchases on resource allocation (employment and investment) within firms and plans (intensive margin), and on the decision to sell or close plants (extensive margin). In panel A, the unit of observation in each regression is firm-year. In panel B, the unit of observation in each regression is plant-year. The outcome variables in columns 1–2 are changes in employment expenditures and investment at the firm-year level (Panel A) or plant-year level (Panel B). Changes are measured as the difference from the year before ($t-1$) to the three-year average after ($t+1$ to $t+3$) the focal year (t), scaled by the capital stock in $t-1$. The outcome variables in column 3 of Panel A are the fraction of plants separated from the firm (i.e., either sold or closed) in the three years after the focal year, and in column 3 of Panel B, an indicator for whether the plant was separated. The pre-repurchase earnings surprise is the difference between the repurchase-adjusted ("pre-repurchase") EPS and the median end-of-quarter EPS forecast, scaled by the end-of-quarter stock price. These tests are conducted using only observations within a narrow window around the zero pre-repurchase EPS surprise threshold (between –0.003 and +0.003). All variables are defined in Appendix A. Standard errors (in parentheses) are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Firm-level analysis			
Dependent variable:	Δ*Employment*	Δ*Investment*	*%Separation*
	[1]	[2]	[3]
Negative Pre-Repurchase EPS Surprise	–0.036**	–0.016***	0.004
	(0.018)	(0.006)	(0.007)
Linear control in pre-repurchase EPS surprise	Y	Y	Y
Year fixed effects	Y	Y	Y
Rounded N	3,300	3,300	3,300
R^2	0.034	0.029	0.015

Panel B: Plant-level analysis			
Dependent variable:	Δ*Employment*	Δ*Investment*	*Separation*
	[1]	[2]	[3]
Negative Pre-Repurchase EPS Surprise	–0.022***	–0.006*	–0.005
	(0.006)	(0.003)	(0.012)
Linear control in pre-repurchase EPS surprise	Y	Y	Y
Plant controls	Y	Y	Y
Industry × year fixed effects	Y	Y	Y
State × year fixed effects	Y	Y	Y
Rounded N	35,000	35,000	43,500
R^2	0.111	0.064	0.108

Table III
Short-term incentives and productivity

This table shows estimates of the impact of incentives to spend resources on short-term performance measures of firm- and plant-level productivity. In column 1, the unit of observation in each regression is firm-year. In column 2, the unit of observation in each regression is plant-year. The outcome variable is the change in total factor productivity, measured as the difference from the year before $(t-1)$ to the three-year average (over $t+1$ to $t+3$) after the focal year (t). The pre-repurchase earnings surprise is the difference between the repurchase-adjusted ("pre-repurchase") EPS and the median end-of-quarter EPS forecast, scaled by the end-of-quarter stock price. These tests are conducted using only observations within a narrow window around the zero pre-repurchase EPS surprise threshold (between –0.003 and +0.003). We include linear controls for the pre-repurchase EPS surprise, interacted with the indicator of a negative pre-repurchase EPS surprise, controls for firm age and plant size, and 4-digit NAICS industry-by-year and state-by-year fixed effects. All variables are defined in Appendix A. Standard errors (in parentheses) are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Firm-level analysis	
Dependent variable:	ΔTFP
	[1]
Negative Pre-Repurchase EPS Surprise	–0.025**
	(0.013)
Linear control in pre-repurchase EPS surprise	Y
Year fixed effects	Y
Rounded N	3,300
R^2	0.012

Panel B: Plant-level analysis	
Dependent variable:	ΔTFP
	[1]
Negative Pre-Repurchase EPS Surprise	–0.019***
	(0.009)
Linear control in pre-repurchase EPS surprise	Y
Plant controls	Y
Industry × year fixed effects	Y
State × year fixed effects	Y
Rounded N	35,000
R^2	0.085

Table IV
Plant-level resource allocation effects: High- vs. low-productivity plants

This table shows estimates of the plant-level impact of EPS-motivated share repurchases on resource allocation between ex-ante productive vs. unproductive plants. The unit of observation in each regression is plant-year. Changes are measured as the difference from the year before $(t-1)$ to the three year-average (over $t+1$ to $t+3$) after the focal year (t). The outcome variables in columns 1–2 are plant-level changes in employment expenditures and investment. The outcome variables in column 3 is an indicator for whether a plant was separated (sold or closed) in the three years after the focal year. The pre-repurchase earnings surprise is the difference between the repurchase-adjusted ("pre-repurchase") EPS and the median end-of-quarter EPS forecast, scaled by the end-of-quarter stock price. $Productive_{t-1}$ is a dummy variable equal to one if the plant has an above-median within-firm total factor productivity in $t-1$. $Unproductive_{t-1}$ is defined analogously. These tests are conducted using only observations within a narrow window around the zero pre-repurchase EPS surprise threshold (between –0.003 and +0.003). Each column reports results including linear control in the pre-repurchase EPS surprise, interacted with the indicator of a negative pre-repurchase EPS surprise and the unproductive dummy variable, plant controls (age and size), and 4-digit NAICS industry-by-year and state-by-year fixed effects. All variables are defined in Appendix A. Standard errors (in parentheses) are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	$\Delta Employment$ [1]	$\Delta Investment$ [2]	$Separation$ [3]
$Unproductive_{t-1}$	–0.014*	–0.006**	–0.008
	(0.006)	(0.003)	(0.007)
Negative Pre-Repurchase EPS Surprise × $Productive_{t-1}$	–0.024***	–0.004	–0.013
	(0.007)	(0.004)	(0.012)
Negative Pre-Repurchase EPS Surprise × $Unproductive_{t-1}$	–0.019***	–0.007*	0.003
	(0.007)	(0.004)	(0.014)
Linear control in pre-repurchase EPS surprise	Y	Y	Y
Linear control in pre-repurchase EPS surprise × $Unproductive_{t-1}$	Y	Y	Y
Plant controls	Y	Y	Y
Industry × year fixed effects	Y	Y	Y
State × year fixed effects	Y	Y	Y
Rounded N	35,000	35,000	43,500
R^2	0.112	0.065	0.108
F-stat	0.31	0.25	1.88
p-value of difference between productive and unproductive interaction terms	0.58	0.61	0.17

Table V
Dynamic effects of share repurchases

This table shows estimates of both the firm- and plant-level impacts of EPS-motivated share repurchases on resource allocation and productivity over time. The unit of observation in each regression is firm-year in Panel A and is plant-year in Panel B. The outcome variables are changes in total factor productivity, employment expenditures, and investment. Changes are measured as the difference from the year before $(t-1)$ to the kth year after $(t+k$, as indicated in each column) the focal year (t). The pre-repurchase earnings surprise is the difference between the repurchase-adjusted ("pre-repurchase") EPS and the median end-of-quarter EPS forecast, scaled by the end-of-quarter stock price. These tests are conducted using only observations within a narrow window around the zero pre-repurchase EPS surprise threshold (between –0.003 and +0.003). Plant controls include age and size, and industry (NAICS) fixed effects at the 4-digit level. All variables are defined in Appendix A. Standard errors (in parentheses) are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Firm-level dynamics

Dependent variable:	ΔTFP			$\Delta Employment$			$\Delta Investment$		
	(-1,+1)	(-1,+2)	(-1,+3)	(-1,+1)	(-1,+2)	(-1,+3)	(-1,+1)	(-1,+2)	(-1,+3)
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Negative Pre-Repurchase EPS Surprise	–0.025**	–0.024*	–0.027**	–0.021	–0.044**	–0.059**	–0.013**	–0.015**	–0.019***
	(0.012)	(0.014)	(0.016)	(0.017)	(0.021)	(0.027)	(0.006)	(0.007)	(0.007)
Linear control in pre-repurchase EPS surprise	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Rounded N	3,300	3,300	3,300	3,300	3,300	3,300	3,300	3,300	3,300
R^2	0.013	0.016	0.011	0.034	0.032	0.028	0.028	0.024	0.024

Panel B: Plant-level dynamics

	$\Delta Employment$			$\Delta Investment$		
	(-1,+1)	(-1,+2)	(-1,+3)	(-1,+1)	(-1,+2)	(-1,+3)
	[1]	[2]	[3]	[4]	[5]	[6]
$Unproductive_{t-1}$	–0.009**	–0.014**	–0.019**	–0.002	–0.010***	–0.006
	(0.005)	(0.006)	(0.008)	(0.003)	(0.003)	(0.004)
Negative Pre-Repurchase EPS Surprise × $Productive_{t-1}$	–0.009	–0.023***	–0.038***	0.001	–0.011**	–0.003
	(0.006)	(0.007)	(0.009)	(0.004)	(0.005)	(0.005)
Negative Pre-Repurchase EPS Surprise × $Unproductive_{t-1}$	–0.009	–0.019**	–0.029***	–0.010**	–0.007*	–0.004
	(0.006)	(0.008)	(0.010)	(0.005)	(0.004)	(0.005)
Linear control in pre-repurchase EPS surprise	Y	Y	Y	Y	Y	Y
Linear control in pre-repurchase EPS surprise × $Unproductive_{t-1}$	Y	Y	Y	Y	Y	Y
Plant controls	Y	Y	Y	Y	Y	Y
Industry × year fixed effects	Y	Y	Y	Y	Y	Y
State × year fixed effects	Y	Y	Y	Y	Y	Y
Rounded N	35,000	35,000	35,000	35,000	35,000	35,000
R^2	0.098	0.108	0.108	0.053	0.062	0.064
F-stat	0.00	0.23	0.65	4.66	0.56	0.02
p-value of difference between productive and unproductive interaction terms	0.94	0.63	0.42	0.03	0.46	0.59

Table VI
State-level union power

This table shows estimates of the impact of EPS-motivated share repurchases on firm-level productivity and resource allocation at plants located in states that have and have not adopted right-to-work (RTW) legislation. In panel A, the unit of observation in each regression is firm-year. Firms are partitioned according to whether they have an above- vs. below-median share of plants (or production hours) in states with RTW laws on the books. The outcome variable is the firm-level change in total factor productivity. This change is measured as the difference from year before $(t-1)$ to the three year average (over $t+1$ to $t+3$) following the focal year (t). In panel B, the unit of observation in each regression is plant-year. The outcome variables in columns 1–2 and 4–5 are plant-level changes in employment expenditures and investment. Changes are measured as the difference from the year before $(t-1)$ to the three year average (over $t+1$ to $t+3$) following the focal year (t) scaled by the capital stock in $t-1$. The outcome variable in column 3 and 6 of Panel B is an indicator for whether the plant was separated (closed or sold). The pre-repurchase earnings surprise is the difference between the repurchase-adjusted ("pre-repurchase") EPS and the median end-of-quarter EPS forecast, scaled by the end-of-quarter stock price. $Productive_{t-1}$ is a dummy variable equal to one if the plant has an above-median within-firm total factor productivity in $t-1$. $Unproductive_{t-1}$ is defined analogously. These tests are conducted using only observations within a narrow window around the zero pre-repurchase EPS surprise threshold (between –0.003 and +0.003). All variables are defined in Appendix A. States adopting RTW laws are listed in Appendix B. Standard errors (in parentheses) are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Firm-level productivity growth					
Dependent variable: ΔTFP		% plants in RTW states		% Production hours in RTW states	
Firm splits:	Average effect	Above med.	Below med.	Above med.	Below med.
	[1]	[2]	[3]	[4]	[5]
Negative Pre-Repurchase EPS Surprise	–0.025**	–0.005	–0.047**	–0.009	–0.045**
	(0.013)	(0.016)	(0.020)	(0.016)	(0.021)
Linear control in pre-repurchase EPS surprise	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y
Rounded N	3,300	1,700	1,700	1,700	1,700
R^2	0.012	0.022	0.018	0.028	0.016
p-value of difference		0.09		0.10	

Panel B: Plant-level outcomes

Union power:	RTW state			Non-RTW state		
Dependent variable:	$\Delta Employment$	$\Delta Investment$	*Separation*	$\Delta Employment$	$\Delta Investment$	*Separation*
	[1]	[2]	[3]	[4]	[5]	[6]
$Unproductive_{t-1}$	–0.005	–0.002	–0.006	–0.022***	–0.009***	–0.010
	(0.009)	(0.004)	(0.010)	(0.008)	(0.003)	(0.008)
Negative Pre-Repurchase EPS Surprise × $Productive_{t-1}$	–0.015	0.002	–0.000	–0.027***	–0.007	–0.021
	(0.011)	(0.006)	(0.017)	(0.009)	(0.005)	(0.013)
Negative Pre-Repurchase EPS Surprise × $Unproductive_{t-1}$	–0.002	–0.002	0.033**	–0.029***	–0.010**	–0.017
	(0.011)	(0.005)	(0.017)	(0.008)	(0.005)	(0.016)
Linear control in pre-repurchase EPS surprise	Y	Y	Y	Y	Y	Y
Linear control in pre-repurchase EPS surprise × $Unproductive_{t-1}$	Y	Y	Y	Y	Y	Y
Plant controls	Y	Y	Y	Y	Y	Y
Industry × year fixed effects	Y	Y	Y	Y	Y	Y
State × year fixed effects	Y	Y	Y	Y	Y	Y
Rounded N	14,000	14,000	17,500	21,000	21,000	26,000
R^2	0.139	0.088	0.154	0.116	0.069	0.160

Table VII
Robustness checks: Alternative measurement of key variables

This table considers alternative measurement when estimating the firm- and plant-level impacts of EPS-motivated share repurchases on resource allocation. The unit of observation in each regression is either a firm- or plant-year pair. We examine alternative measures of productivity as outcome variables, including the operating margin and labor and capital productivity. We also examine alternative measures of factor inputs, including machinery investments, the change in the log number of employees, and the symmetric employment growth. Panel C examines alternative measures of ex-ante plant productivity, including the plant's within-industry (4-digit NAICS) TFP ranking, the within-firm labor productivity (MPL) ranking, and the within-firm return on capital (ROC) ranking. The pre-repurchase earnings surprise is the difference between the repurchase-adjusted ("pre-repurchase") EPS and the median end-of-quarter EPS forecast, scaled by the end-of-quarter stock price. These tests are conducted using only observations within a narrow window around the zero pre-repurchase EPS surprise threshold (between –0.003 and +0.003). Plant controls include age and size, and industry (NAICS) fixed effects at the 4-digit level. All variables are defined in Appendix A. Standard errors (in parentheses) are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Firm-level alternative measurement

Alternative outcome:	Productivity			Factor inputs		
Dependent variable:	Δ*Operating Margin*	Δ*Labor Prod.*	Δ*Capital Prod.*	Δ*Machinery Investments*	Δ*Log(#Emp.)*	*Symm. Emp. Growth*
	[1]	[2]	[3]	[4]	[5]	[6]
Negative Pre-Repurchase EPS Surprise	–0.050**	–0.366**	–0.200***	–0.018**	–0.034**	–0.038**
	(0.020)	(0.154)	(0.062)	(0.009)	(0.017)	(0.018)
Linear control in pre-repurchase EPS surprise	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Rounded N	3,300	3,300	3,300	3,300	3,300	3,300
R^2	0.036	0.028	0.024	0.025	0.050	0.051

Panel B: Plant-level alternative measurement

Alternative outcome:	Productivity			Factor inputs		
Dependent variable:	Δ*Operating Margin*	Δ*Labor Prod.*	Δ*Capital Prod.*	Δ*Machinery Investments*	Δ*Log(#Emp.)*	*Symm. Emp. Growth*
	[1]	[2]	[3]	[4]	[5]	[6]
Negative Pre-Repurchase EPS Surprise	–0.015*	–0.263**	–0.108**	–0.009**	–0.016**	–0.027***
	(0.009)	(0.127)	(0.032)	(0.004)	(0.006)	(0.008)
Linear control in pre-repurchase EPS surprise	Y	Y	Y	Y	Y	Y
Plant controls	Y	Y	Y	Y	Y	Y
Industry × year fixed effects	Y	Y	Y	Y	Y	Y
State × year fixed effects	Y	Y	Y	Y	Y	Y
Rounded N	35,000	35,000	35,000	35,000	35,000	35,000
R^2	0.097	0.096	0.085	0.070	0.108	0.104

Panel C: Plant-level alternative measurement for productivity interaction

Dependent variable:	$\Delta Employment$		$\Delta Investment$	
Productivity definition used in interaction:	Within-ind. TFP	Within-firm MPL	Within-ind. TFP	Within-firm ROC
	[1]	[2]	[3]	[4]
$Unproductive_{t-1}$	–0.004	–0.040***	–0.005	–0.023***
	(0.007)	(0.006)	(0.003)	(0.003)
Negative Pre-Repurchase EPS Surprise × $Productive_{t-1}$	–0.022***	–0.021***	–0.005	–0.009**
	(0.007)	(0.007)	(0.004)	(0.004)
Negative Pre-Repurchase EPS Surprise × $Unproductive_{t-1}$	–0.022***	–0.022***	–0.007*	–0.002
	(0.008)	(0.007)	(0.004)	(0.004)
Linear control in pre-repurchase EPS surprise	Y	Y	Y	Y
Linear control in pre-repurchase EPS surprise × $Unproductive_{t-1}$	Y	Y	Y	Y
Plant controls	Y	Y	Y	Y
Industry × year fixed effects	Y	Y	Y	Y
State × year fixed effects	Y	Y	Y	Y
Rounded N	35,000	35,000	35,000	35,000
R^2	0.111	0.116	0.065	0.070
F-stat	0.00	0.05	0.13	1.79
p-value of difference between productive and unproductive interaction terms	0.95	0.83	0.71	0.18

Table VIII
Robustness checks: Specification and falsification tests

This table conducts various specification checks for the impact of EPS-motivated share repurchases on resource allocation and productivity. Panel A considers an alternative bandwidth around the zero pre-repurchase EPS surprise threshold and a third-order polynomial control in the pre-repurchase EPS surprise, which we interact with the indicator of a negative pre-repurchase EPS surprise. Panel B examines whether there are pre-existing trends in outcome variables. Panel C lags the timing of the negative pre-repurchase EPS surprises by three years. Outcome variables and the pre-repurchase earnings surprise are defined in previous tables. The pre-repurchase earnings surprise is the difference between the repurchase-adjusted ("pre-repurchase") EPS and the median end-of-quarter EPS forecast, scaled by the end-of-quarter stock price. These tests are conducted using only observations within a narrow window around the zero pre-repurchase EPS surprise threshold (between –0.003 and +0.003), except where indicated in Panel A. Plant controls include age and size, and industry (NAICS) fixed effects at the 4-digit level. All variables are defined in Appendix A. States that have adopted RTW laws are listed in Appendix B. Standard errors (in parentheses) are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: RDD specification checks

Alternative specification choice:	Bandwidth selection (±0.001)			3rd-degree polynomial		
Dependent variable:	ΔTFP	$\Delta Employment$	$\Delta Investment$	ΔTFP	$\Delta Employment$	$\Delta Investment$
	[1]	[2]	[3]	[4]	[5]	[6]
Negative Pre-Repurchase EPS Surprise	–0.054**	–0.038*	–0.016**	–0.031**	–0.051**	–0.016**
	(0.017)	(0.022)	(0.007)	(0.014)	(0.020)	(0.007)
Linear control in pre-repurchase EPS surprise	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Rounded N	1,900	1,900	1,900	3,300	3,300	3,300
R^2	0.020	0.033	0.030	0.013	0.040	0.029

Panel B: No pre-existing firm-level differences in key outcome variables

Differences in outcomes in:	Changes ($t-3$ to $t-2$)			Changes ($t-2$ to $t-1$)		
Dependent variable:	ΔTFP	$\Delta Employment$	$\Delta Investment$	ΔTFP	$\Delta Employment$	$\Delta Investment$
	[1]	[2]	[3]	[4]	[5]	[6]
Negative Pre-Repurchase EPS Surprise	–0.005	–0.013	–0.007	0.007	–0.014	–0.000
	(0.010)	(0.013)	(0.005)	(0.010)	(0.014)	(0.006)
Linear control in pre-repurchase EPS surprise	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Rounded N	3,200	3,200	3,200	3,300	3,300	3,300
R^2	0.017	0.060	0.018	0.013	0.073	0.024

Panel C: Plant-level placebo test (three-year lagged shock)

Union power:	RTW state		Non-RTW state	
Dependent variable:	$\Delta Employment$	$\Delta Investment$	$\Delta Employment$	$\Delta Investment$
	[1]	[2]	[3]	[4]
$Unproductive_{t-1}$	0.000	–0.006	–0.011	–0.004
	(0.009)	(0.005)	(0.008)	(0.002)
Negative Pre-Repurchase EPS Surprise × $Productive_{t-1}$	0.001	–0.005	–0.013	–0.002
	(0.012)	(0.008)	(0.010)	(0.007)
Negative Pre-Repurchase EPS Surprise × $Unproductive_{t-1}$	–0.010	0.005	–0.006	0.006
	(0.011)	(0.007)	(0.009)	(0.006)
Linear control in pre-repurchase EPS surprise	Y	Y	Y	Y
Linear control in pre-repurchase EPS surprise × $Unproductive_{t-1}$	Y	Y	Y	Y
Plant controls	Y	Y	Y	Y
Industry × year fixed effects	Y	Y	Y	Y
State × year fixed effects	Y	Y	Y	Y
Rounded N	12,500	12,500	18,000	18,000
R^2	0.152	0.090	0.120	0.068

Internet Appendix for "Do Short-Term Incentives Affect Long-Term Productivity"

August 13, 2021

Appendix A: Variable definitions

This appendix presents the definitions for the variables used throughout the paper.

Variable	Definition	Source
Plant-level definitions		
TFP	Natural logarithm of plant-level total factor productivity based on Foster et al. (2014)	CMF/ASM
OM	Total value of shipments (sales) minus material and energy costs and payroll divided by establishment-level sales	CMF/ASM
Capital Productivity (ROC)	Sales minus material and energy costs and payroll over establishment-level capital stock	CMF/ASM
Labor Productivity (MPL)	Sales minus materials and energy costs divided by total employment expenditures	CMF/ASM
Employment	Total employment expenditures (wages and salaries, i.e., payroll)	CMF/ASM
Log(#Employees)	Natural logarithm of number of employees	CMF/ASM
Symmetric Employment Growth Rate	Annual change in employees divided by the average of current and lagged employees	CMF/ASM
Investment	Total capital expenditures divided by establishment-level capital stock	CMF/ASM
Machinery Investment	Machinery expenditures divided by establishment-level machinery stock	CMF/ASM
Plant Age	Number of years since the plant first appears in the LBD	LBD
Plant Size	Natural logarithm of sales	CMF/ASM
Firm-level definitions		
%Closed	Number of plants closed scaled by the lagged number of plants	CMF/ASM
%Sold	Number of plants sold scaled by the lagged number of plants	CMF/ASM
Negative Pre-Repurchase EPS Surprise	Difference between the repurchase-adjusted ("pre-repurchase") EPS and the median end-of-quarter EPS forecast, scaled by the end-of-quarter stock price. The pre-repurchase EPS is calculated as $EPS_{adj} = \frac{E_{adj}}{S_{adj}} = \frac{E+I}{S+\Delta S}$, where E is reported earnings, I is the estimated forgone interest due to the repurchase, S is the end-of-quarter number of shares, and ΔS is the estimated number of shares repurchased (the repurchase amount divided by the average daily share price). The forgone interest is the after-tax interest that would be earned on an amount of funds equal to that used to repurchase shares if it were invested in a 3-month T-bill.	CRSP/Compustat

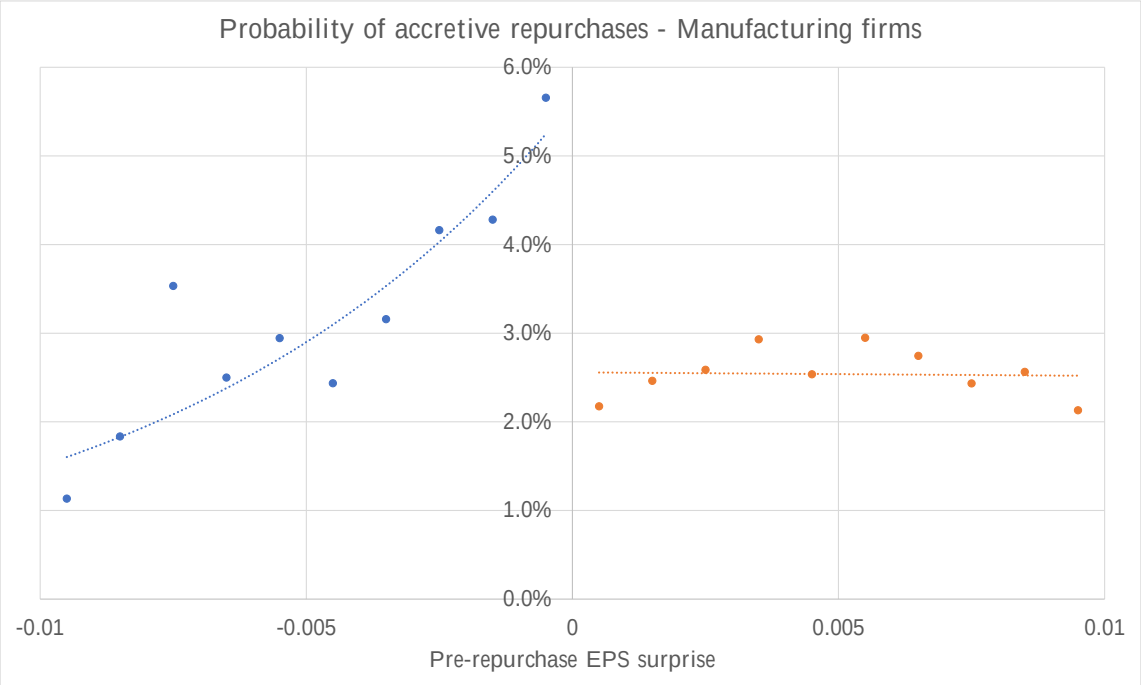
Appendix B: Right-to-work (RTW) laws by state

This table lists the effective year of adoption of right-to-work legislation either by the state constitution or by a statute. These data are provided in Chava et al. (2018).

State	Adopted
AL	1953
AK	
AZ	1947
AR	1947
CA	
CO	
CT	
DE	
DC	
FL	1943
GA	1947
HI	
ID	1986
IL	
IN	2012
IA	1947
KS	1958
KY	2017
LA	1976
ME	
MD	
MA	
MI	2013
MN	
MS	1960
MO	
MT	
NE	1947
NV	1952
NH	
NJ	
NM	
NY	
NC	1947
ND	1947
OH	
OK	2001
OR	
PA	
RI	
SC	1954
SD	1947
TN	1947
TX	1947
UT	1955
VT	
VA	1947
WA	
WV	2016
WI	2015
WY	1963

Figure A.1: Negative pre-repurchase EPS surprises and share repurchases among manufacturing firms

This figure replicates results from Figure 1 in Almeida, Fos, and Kronlund (2016) within the sample of manufacturing firms. Manufacturing firms are defined as firms with 2-digit SIC codes between 20 and 39. The figure plots the probability of doing accretive share repurchases as a function of a pre-repurchase earnings surprise. The dots represent the probability of an accretive share repurchase for every earnings surprise bin—the fraction of firm-quarters with an accretive repurchase out of all firm-quarters in that bin. We define a share repurchase as accretive if it increases EPS by at least one cent. The pre-repurchase earnings surprise is the difference between the repurchase-adjusted ("pre-repurchase") earnings per share (EPS) and the median EPS forecast at the end of the quarter; this difference is normalized by the end-of-quarter stock price. The pre-repurchase EPS is calculated as follows: $EPS_{adj} = E_{adj}/S_{adj} = (E + I)/(S + \Delta S)$, where E is reported earnings, I is the estimated forgone interest due to the repurchase, S is the number of shares at the end of the quarter, and ΔS is the estimated number of shares repurchased (the repurchase amount divided by the average daily share price). The foregone interest is the after-tax interest that would have been earned on the amount that was used to repurchase shares if it were instead invested in a 3-month T-bill.



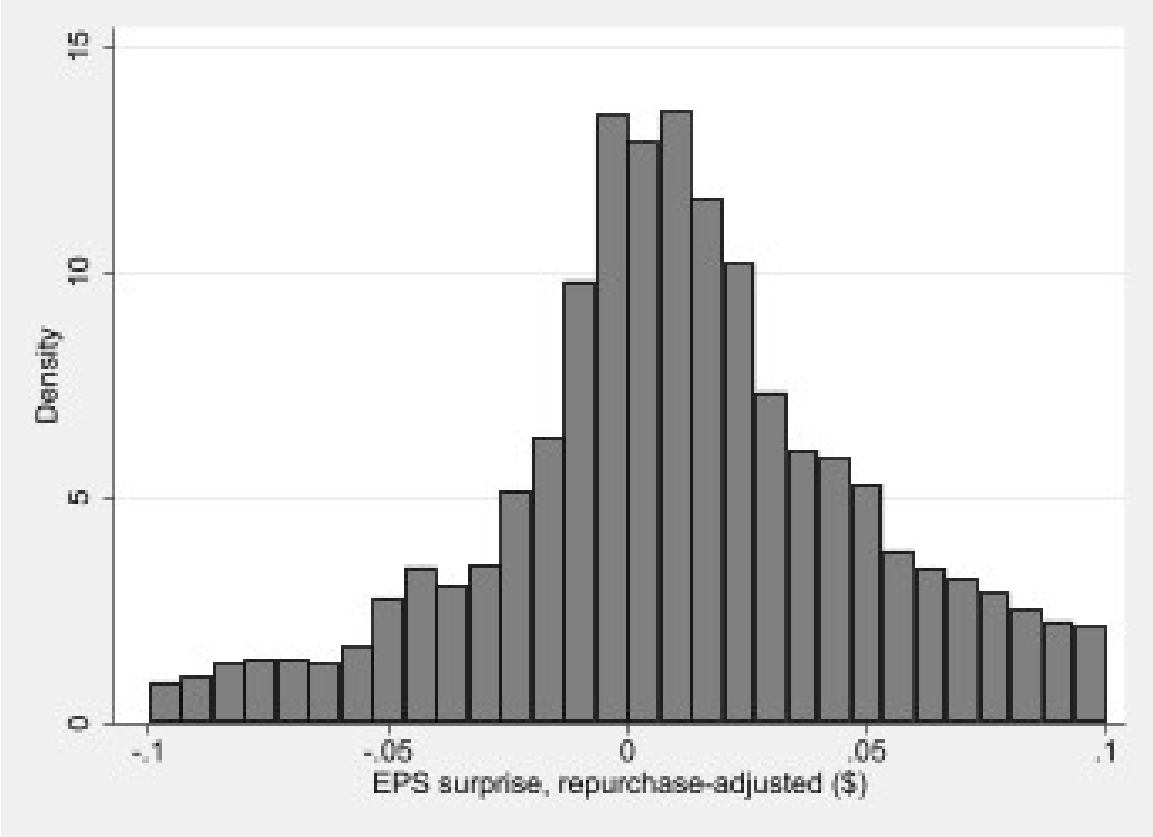


Figure A.2: Histogram of the EPS surprise among manufacturing firms(rounded to nearest cent).

This histogram represents the distribution of the the repurchase-adjusted EPS minus the analyst consensus EPS (rounded to nearest cent). The x-axis is in dollars, which makes the graph be limited to between -10 and +10 cents.

Table A.1: Negative pre-repurchase EPS surprises and share repurchases among manufacturing firms

This table replicates results from Table 3 in Almeida, Fos, and Kronlund (2016) within the sample of manufacturing firms. Manufacturing firms are defined as firms with 2-digit SIC codes between 20 and 39. The table reports the relationship between having a negative pre-repurchase EPS surprise and the probability of doing a share repurchase in a firm-quarter. The calculation of the pre-repurchase EPS surprise is as described in Fig. A.1. Share repurchases are measured as follows: We measure "Net repurchases" following Fama and French (2001), i.e., as the increase in common Treasury stock if Treasury stock is not zero or missing; if Treasury stock is zero in the current and prior quarter, we measure repurchases as the difference between stock purchases and stock issuances from the statement of cash flows. If either of these amounts is negative, repurchases are set to zero. The regressions control for the linear relation between the pre-repurchase EPS surprise and repurchases, interacted with the indicator of a negative pre-repurchase EPS surprise, as well as time fixed effects. We limit the sample to firm-quarters that fall in a small window around the zero pre-repurchase EPS surprise threshold (with a pre-repurchase EPS surprise normalized by share price between -0.003 and 0.003). The dependent variable Column (1) is the amount of net repurchases, normalized by assets. The dependent variable in Column (2) is an indicator variable for whether the firm conducts an accretive share repurchase of at least one cent. t-stats based on standard errors that are robust to heteroskedasticity and clustered at the firm level are reported in parentheses below the coefficient estimates. *, **, and *** represent statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	*Net Repurchases*	*I[Accretive Repurchase]*
	[1]	[2]
Negative Pre-Repurchase EPS Surprise	0.0036***	0.0458***
	(8.23)	(7.09)
Linear control in pre-repurchase EPS surprise	Y	Y
Year fixed effects	Y	Y
Rounded N	23,500	23,500
R^2	0.051	0.028