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

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Abstract

Previous research shows that incentives to meet short-term earnings targets can cause firms to increase share buybacks, leading to cuts in investments and employment. Using plant-level Census data, we find that incentives to engage in EPS-motivated buybacks result in lower future productivity at both the plant and firm level. We attribute this productivity drop to two mechanisms: reduced investment in productivity-augmenting technology, and inefficient allocation of resources across a firm's plants. We identify several sources of friction—including labor unions, financial constraints, agency problems, and adjustment costs—that can constrain efficient reallocations across plants and thus exacerbate the consequences of firms' short-term incentives.

JEL Classification: G32, G35, J23

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1 Introduction

Are U.S. public firms short-term oriented? Several recent papers have shown that firms often cut spending on investments and R&D to achieve short-term goals, such as when they seek to meet earnings forecasts (Terry, 2023) or around the time when managers' equity incentives vest (Edmans, Fang, and Lewellen, 2017; Ladika and Sautner, 2020). However, we know less about the longer-run consequences of corporate short-termism. On the one hand, many observers contend that actions that firms take to boost short-term performance end up being harmful. For example, Jamie Dimon and Warren Buffett caution about an "unhealthy focus on short-term profits at the expense of long-term strategy, growth, and sustainability."[1] On the other hand, if firms cut spending in a way that primarily decreases investment in their least profitable projects (e.g., those near zero NPV), the eventual consequences for overall profitability and productivity might be neutral. Indeed, Kaplan (2018) argues that little evidence exists that short-termism has adversely affected long-term profits.

This paper aims to fill this gap in the literature by studying how incentives to boost current earnings per share (EPS) using share buybacks affect firms' future productivity and the within-firm allocation of resources. Specifically, we study changes in firms' productivity and resource allocation that result from their incentives to use share buybacks to raise EPS just above analysts' earnings forecasts. Hribar, Jenkins, and Johnson (2006) show that firms just about to miss the consensus EPS forecast do significantly more buybacks than firms that "just meet" the EPS forecast without conducting repurchases. These buybacks—which we refer to as "EPS-motivated repurchases"—tend to be expensive and can starve firms of cash, even if they only move EPS by around one cent. Almeida, Fos, and Kronlund (2016) find that firms that have an incentive to spend money on buying back shares when they would otherwise "just miss" the forecast, on average end up cutting capital expenditures

[1] Op-ed, Wall Street Journal, June 6, 2018.

and employment in the short run (i.e, over the next four quarters).

EPS-motivated buybacks are, of course, only one kind of short-term action that firms can engage in. Still, this setting has especially high real-world relevance because share buybacks represent one of the most popular targets for critics of corporate short-termism (e.g., Gutierrez and Philippon, 2017)—especially when they are done to boost EPS.[2] Firms care about their quarterly EPS since it is the short-term performance measure that tends to matter the most to investors (Graham et al., 2005). Yet, it remains an open question how these types of incentives to boost short-run performance affect firms' productivity.

The effects on productivity will mostly depend on how resources are allocated across a firm's projects and plants. On the one hand, if firms principally reduce investments in their least productive plants and projects, we expect little or even a positive effect on firm-level productivity. On the other hand, if firms face frictions that prevent them from allocating resources efficiently across projects or if they reduce particular types of investments that support long-run productivity (e.g., information technology), then productivity may suffer.

To study the effects of incentives to engage in EPS-motivated buybacks on future productivity and within-firm resource allocation, our analysis exploits detailed plant-level data from the U.S. Census Bureau. These data have several advantages when addressing these questions. First, compared with measures of productivity that can be inferred from financial statements, they allow for better estimates of firms' total factor productivity (TFP) by incorporating detailed data on input factors and outputs. Second, these data allow us to examine productivity changes both at the firm level and at the level of each plant. Third, we can examine specific subcategories of investments that are particularly important for maintaining productivity, such as those in new machinery and information technology. Fourth, these data allow for studying the *within-firm* resource allocation across a firm's different

[2] See, for example, Thomas Frank, *CNBC*, March 2, 2021: "Elizabeth Warren rips stock buybacks as nothing but paper manipulation." https://www.cnbc.com/2021/03/02/elizabeth-warren-rips-stock-buybacks-as-nothing-but-paper-manipulation.html

plants; this feature is particularly important as it allows us to examine whether any cuts to investments or employment are concentrated within only less-productive plants. Finally, these data allow us to exploit variation in the spatial distribution of a firm's plants across U.S. states. However, one limitation is that these data cover only manufacturing firms, which reduces the sample, although a benefit of such a constrained sample is that it ensures that productivity measures can be applied consistently across firms.

Our empirical strategy follows the "fuzzy regression discontinuity" framework in Almeida et al. (2016) by exploiting a discontinuity in a firm's incentives to engage in repurchases when managers expect to 'just-miss' the analyst consensus EPS forecast. The key identifying 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 around this threshold that directly affect the outcome variables.

We begin by analyzing the effect of incentives to do EPS-motivated buybacks on *firm-level* total factor productivity (TFP). We measure this effect as the change in TFP from one year before to three years after the "focal year," which is the year in which we measure whether the firm has an incentive to engage in EPS-motivated repurchases. Our first results show that TFP decreases by about 1.3% in the three years after the focal year among those firms with an incentive to engage in EPS-motivated buybacks when compared with other firms without such an incentive (but with otherwise almost-identical EPS). Also consistent with Almeida et al. (2016), we find significant cuts to investment and employment within our sample of manufacturing firms.

To dig deeper into the mechanisms behind the firm-level drop in productivity, we then examine the role of two possible (not mutually exclusive) drivers behind this effect: (i) changes to productivity at the level of individual plants, and (ii) shifts in the allocation of resources *across* a firm's different plants. We first show that the results are primarily driven by drops in productivity at the plant level. This result, in turn, raises the question of how plants become less productive. One possible mechanism is that there is something special about certain kinds of investment in terms of their effects on productivity. That is, capital can vary in the degree of technological progress it embodies (Syverson, 2011). Previous research has pointed out that investments in new equipment/machines and information technology (IT) have an outsized role in supporting productivity (Cummins and Violante, 2002; Sakellaris and Wilson, 2004).[3] In line with a mechanism that underscores the importance of "productivity-enhancing" new technologies, we find that companies with incentives to engage in EPS-motivated buybacks subsequently make cuts to investments in new machinery and IT. These results indicate that plants require sustained investment in new equipment and technologies to maintain their level of productivity and that cuts to such investments that are motivated by incentives to boost short-run performance can harm firms' long-run productivity.

The second mechanism we investigate hinges on firms' allocation of resources across their different plants. Stein (1997) shows that firms can create value through efficient resource reallocation, i.e., through reallocating funds from one division to another.[4] To test how incentives to do EPS-motivated repurchases affect across-plant resource allocation, we examine whether firms distribute the overall cuts in investment and employment in a way that efficiently distinguishes between high- vs. low-productivity plants.[5] The best-case scenario would be that firms would mostly cut investment in their less productive projects and plants, which would mitigate or even reverse any adverse effects on productivity. However, our find-

[3]Previous research has shown that IT-related investments allow hedge funds and private equity firms to raise the productivity of acquired firms and their employees (Agrawal and Tambe, 2016; Brav et al., 2015).

[4]Giroud and Mueller (2015) directly test the "winner-picking hypothesis" (e.g., Stein, 1997) and show that following a positive shock to investment opportunities at one plant, headquarters reallocates capital and labor from plants with relatively low TFP. Bai (2021) compares how conglomerates and stand-alone firms respond to trade shocks, finding that conglomerates restructure more efficiently by closing unproductive plants. Finally, Ersahin et al. (2021) show that following covenant violations, firms create value by reallocating resources away from unproductive plants.

[5]Maksimovic and Phillips (2002), Matvos and Seru (2014), and Dai et al. (2022) argue that division or segment productivity is the key metric determining internal resource allocation.

ings show that firms direct these investment cuts *indiscriminately* across plants, irrespective of whether a particular plant is productive or unproductive.[6] This pattern differs from how firms allocate resources "in normal times"—i.e., when they are not subject to short-term pressures from incentives to engage in EPS-motivated buybacks—when they allocate less investment and hiring towards their less productive plants.

Why do firms not distribute investment cuts in a better way across plants? One possibility is that firms face frictions that prevent them from efficiently (re)allocating resources across plants. To shed more light on such channels, we first examine the role of labor-related frictions. Unions commonly impose constraints that prevent firms from making concentrated cuts to investment and employment in select plants. Such union rules may naturally help some of the unionized employees, but these rules nevertheless tend to be rigid. They thus may not support efficient reallocation in response to sudden shocks in the overall availability of resources (e.g., Bloom et al., 2019; Chava et al., 2020; Serfling, 2016).[7] To measure the extent to which firms are differentially subject to friction from bargaining with labor unions, we split the sample of firms in our sample based on the fraction of each firm's plants located in right-to-work (RTW) states. The underlying idea is that the labor force tends to be more unionized in states that do not have RTW laws and that firms with more of their plants in non-RTW states thus need to negotiate more with unions around any allocation of resources, especially when it comes to cutting investment and employment. We find that the drop in TFP that follows in the years after firms have incentives to do EPS-motivated buybacks is concentrated only among firms with relatively more plants in non-RTW states, where the labor force is relatively more unionized. By contrast, firms with more plants located in states with weaker union power tend not to experience significant deterioration in their

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

[7]Other research has shown that labor frictions influence a range of corporate decisions and outcomes, including sales growth (e.g., Bai et al., 2019), capital structure (e.g., Agrawal and Matsa, 2013; Matsa, 2010; Serfling, 2016; Simintzi et al., 2014), and valuations (e.g., Chen et al., 2011; Lee and Mas, 2012).

productivity. We further tie this differential effect on productivity to differences in how firms allocate resources. Specifically, we find that the relatively "indiscriminate" cuts across both productive and unproductive plants are concentrated in plants in non-RTW (i.e., more unionized) states, and that the negative effects on productivity are stronger for firms that cut the net number of employees compared with firms that merely reduce the level of new hiring.[8] These results show that frictions stemming from organized labor can prevent firms from (re)allocating investments efficiently across plants.

Other sources of friction can similarly affect the efficient allocation of resources and thus amplify the effects of short-term incentives on firms' productivity. To explore this possibility, we also consider the roles of financial constraints, agency problems, and adjustment costs. First, using the Whited and Wu (2006) index, or whether the firm has a credit rating, as two different measures for financial constraints, we show that constrained firms experience a larger decrease in TFP when faced with incentives to engage in EPS-motivated buybacks. In contrast, we find no effects on TFP for financially unconstrained firms. Consistent with these differential effects on TFP that depend on the level of firms' financial constraints, only the more constrained firms appear to make inefficient cuts to investment and employment.

We next study the role of agency problems, including conflicts based on heterogeneity across firms' shareholder base and those rooted in managerial compensation contracts. Using Bushee's classification of 13F investors (Bushee, 1998, 2001), we show that having institutional investors with longer horizons mitigates the negative TFP and resource allocation effects of short-term incentives. This finding is consistent with the literature investigating how institutional investors' horizon affects agency costs (Chen et al., 2007; Gaspar et al., 2005; Harford et al., 2018). Moreover, using detailed data on CEO contracts from ISS Incentive Lab, derived from proxy statements (DEF14A), we show that the negative consequences for TFP are concentrated only in firms where the CEO's pay explicitly depends on meet-

[8] These results are consistent with a "dark side" view on internal capital markets, whereby all plants receive a similar allocation of capital and labor as a form of "corporate socialism" (e.g., Dai et al., 2022).

ing EPS targets. Thus, managers with stronger incentives to boost EPS respond to these incentives at the cost of harming firm productivity.

The final set of frictions we consider focus on adjustment costs. Since the cost of adjusting a firm's investment policies tends to be convex in the size of the adjustment, conducting a large cut at one plant can be costlier than cutting resources across multiple plants (e.g., Eisner and Strotz, 1963).[9] To test this hypothesis, we construct a firm-level measure of the "concentration" of investment cuts and find that—consistent with large, concentrated cuts being costlier—as the cuts become more concentrated, the negative effect on firm-level TFP worsens. Overall, the evidence indicates that labor frictions, financial constraints, agency problems, and adjustment costs can amplify the negative effect of short-term incentives on firm productivity.

This paper is related to multiple strands of literature. First, we contribute to the growing literature on the real effects of short-termism. Gutierrez and Philippon (2017) suggest that short-termism—which they trace to increases in institutional ownership and investor activism—has contributed to a drop in long-term investment after the early 2000s. But while investor activism has often been depicted as a force driving short-term corporate behavior, other empirical evidence does not support a view that activism hurts firms in the long run. On the contrary, Brav et al. (2015), Brav et al. (2018), and Bebchuk et al. (2015) find that activist-targeted firms experience higher long-term performance. Our paper contributes to this literature by showing that short-term incentives to engage in EPS-motivated buybacks can harm long-term productivity.

Furthermore, we conduct a comprehensive analysis of the mechanisms for *how* short-term incentives can lead to lower long-term productivity. First, we highlight the role of cuts to technology-intensive investments and an inefficient allocation of resources. Our findings illustrate that not merely the level but also the composition of investments are critical when

[9] Khan and Thomas (2018) provide a review of the academic literature on adjustment costs.

evaluating the long-term effects of corporate short-termism. Second, we explore mechanisms of why firms do not (re)allocate resources more efficiently around cuts to investments and employment by highlighting the role of labor frictions, financial constraints, agency problems, and adjustment costs. This systematic exploration of underlying mechanisms allows us to go beyond the question of whether short-termism harms firms by providing a more complete picture of the economic forces at play.

Two contemporaneous papers examine related aspects of future consequences that can result from short-term incentives. Edmans et al. (2022) focus on the effects of short-term incentives created by vesting equity on future returns, and Almeida et al. (2022) focus on the effect of incentives to engage in EPS-motivated buybacks on firm's future patenting outputs. One important difference in the current paper is that we exploit detailed plant-level data, which allows for studying the allocation of spending within firms across different types of investments and plants. Another important difference relative to Edmans et al. (2022) is that we focus on EPS-motivated buybacks, which have been the focus of much of the debate about short-termism. Similar to our findings, Edmans et al. (2022) find evidence consistent with negative long-term consequences (which they measure as low returns) from short-term incentives, and they propose a mechanism based on inefficient investment due to agency frictions arising from vesting equity. This mechanism is complementary to the role of labor contracting frictions, financial constraints, and adjustment costs in our paper. Almeida et al. (2022) find a positive effect of short-term incentives on innovation outcomes, highlighting that short-term incentives do not need to be harmful absent other frictions but depend on firms' ability to restructure efficiently. One reason why innovation outputs and manufacturing productivity may be affected differently is that innovation activities tend to be less subject to labor-market frictions than manufacturing activities. Jointly, these papers highlight the critical role of underlying frictions and incentives that can either moderate or amplify the consequences of short-termism.

Finally, our paper is consistent with earlier literature that suggests that chasing EPS targets is a driver of short-term behavior. For example, Graham et al. (2005) report survey evidence that CFOs are willing to suffer long-term negative consequences by cutting investments to meet short-term EPS targets. Still, while managers say they are willing to suffer such consequences, limited evidence exists on the extent and nature of such long-run costs. Our paper provides large-scale empirical evidence that such actions can adversely affect the efficiency of firms' resource allocation choices and, ultimately, their productivity.

2 Empirical strategy

We face two principal empirical challenges when studying how short-term incentives affect firms' future resource allocations and productivity. 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 likely motivated by short-termist pressures? Second, firms' actions in responding to short-term pressures—including whether to engage in EPS-motivated buybacks or other types of earnings management—may be confounded by omitted variables or selection.

Our approach to identifying plausibly causal effects on firms' productivity and resource allocation follows the "fuzzy regression discontinuity" framework of Almeida et al. (2016). Following this framework, we start by constructing a variable, *pre-repurchase EPS surprise*, which captures what the firm's EPS surprise (i.e., its EPS less the consensus analyst forecast) *would have been* for a given quarter if it did not engage in any buybacks. As in Hribar et al. (2006), we find that firms that fall just below the zero threshold (which means they would "just miss" without a buyback) are discontinuously more likely to engage in accretive share repurchases that raise their EPS.[10] We then use this discontinuity in the incentive to engage

[10]This discontinuity is originally documented by Hribar et al. (2006). Hribar et al. (2006) and Almeida et al. (2016) describe additional detail on calculating the pre-repurchase EPS surprise.

in such 'EPS-motivated repurchases' to study the effects on the firm's future outcomes, such as its productivity and resource allocation.

To better understand the discontinuity, consider the following example. Suppose a firm has a stock price of $60 and 1 billion shares outstanding, and the analyst EPS consensus forecast for a particular quarter is $3.00 a share, but the manager learns that the actual EPS number is about to be $2.99 a share. The manager can meet the forecast by doing share repurchases: For example, spending $600 million to repurchase stock at the price of $60 per share would reduce shares outstanding to 990 million. The company's earnings would decrease because it forgoes interest payments on its cash holdings. If the interest rate is 5%, the firm's marginal tax rate is 30%, and the company forgoes one quarter of interest, the forgone interest is 5%/4*(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).[11] 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. Note that the money required for repurchases to move the EPS by even just a small amount 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.[12]

To estimate the effects of these incentives to engage in EPS-motivated stock repurchases on firms' future outcomes, we estimate the following regression, which represents a fuzzy

[11]In this example, we are assuming that the repurchase takes place at the beginning of the quarter. In our empirical analysis, we assume that repurchases are evenly divided over each day of a quarter, which symmetrically affects both the forgone interest and the shares outstanding for calculating EPS.

[12]In our analysis, we measure all (pre-repurchase) EPS surprises relative to the share price. The average share price is around $25, so a 1 cent move is equivalent to around a 0.01/25 = 0.0004 unit move in price-normalized units. For a firm with a P/E of 20 (i.e., quarterly earnings of around 31 cents), such a move would require buying back up to 6% of shares outstanding if interest rates are close to zero and the repurchase to place in the middle of the quarter, or potentially less if the EPS is close to being rounded up to the nearest cent.

regression discontinuity (in the reduced form):

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

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 (e.g., productivity), the dependent variable is defined as the *change* in the variable of interest, calculated 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. The focal year is when we measure whether the firm is incentivized to engage in EPS-motivated repurchases. Sue_{adj} is the pre-repurchase EPS surprise—which we describe in more detail below—and $I_{Negative\ Sue_{adj}}$ 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.

The assignment variable in the fuzzy RD, the pre-repurchase EPS surprise (Sue_{adj}), is the difference between the repurchase-adjusted ("pre-repurchase") earnings per share (EPS) and the median EPS forecast at the end of the quarter.[13] This difference is normalized by the end-of-quarter stock price. The main independent variable is the indicator of whether the pre-repurchase EPS surprise is negative ($I_{Negative\ Sue_{adj}}$); this variable captures the discontinuous effect of whether a firm would just-miss the EPS consensus in the absence of doing a buyback. In other words, the empirical specification tests whether those firms that would narrowly miss their EPS forecast without doing a repurchase exhibit future changes to their resource allocations and productivity that are different from those firms that just meet their consensus

[13]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. Note that we assume that repurchases are evenly distributed over the quarter, which affects both the forgone interest and the shares outstanding for calculating EPS

analyst forecast without engaging in buybacks. The idea is that firms that would otherwise miss the forecast on average tend to do more buybacks; but crucially, we do not condition any part of the sample on firms actually doing (or not doing) a buyback. That is, on both sides of the zero pre-repurchase EPS threshold, there are some firms that don't do buybacks and some that do. The identification comes from the fact that firms, on average, do more buybacks just to the left of the threshold (both in number of firms and dollars); these additional firms being those that do a buyback only because they seek to beat the threshold.[14]

Since the dependent variables are defined as differences between future and lagged outcomes, this accounts for any time-invariant differences in productivity or other outcomes across firms and plants. In firm-level regressions, we include year fixed effects (η_t), which account for any possible economy-wide confounders over time. In plant-level regressions, we additionally include controls for plant age and size as well as industry-by-year and state-by-year fixed effects. State-times-year and industry-times-year fixed effects control for any changes over time that are common within a region or industry. To better isolate differences around the threshold, we limit the sample in the regressions to observations (firm-years) that fall within a small window around the zero pre-repurchase EPS surprise threshold, $-0.003 \leq Sue_{adj} \leq 0.003$. Moreover, because our census data involve an annual frequency (we describe the data in more in section 3), while earnings surprises can be calculated at the quarterly level, we limit our analysis to the fourth quarter of each firm's fiscal year when calculating the pre-repurchase EPS surprise.[15] We winsorize all variables at the 1% level.

[14]There exist firms on both sides of the threshold that do buybacks for other reasons, such as reasons related to payout or taxes, but the identification assumption is that there is no discontinuity around the zero pre-repurchase EPS threshold that's related to those other reasons.

[15]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.)

Across all regressions—regardless if the data is at the plant level or firm level—we cluster standard errors at the firm level to account for correlations across plants belonging to the same firm and over time for the same firm/plant.

The key identification assumption behind this empirical strategy is the following: In the absence of a discontinuous jump in the incentive to repurchase around zero pre-repurchase EPS surprise threshold, there are no other discontinuous changes in firm policies around the same threshold that directly affect our outcome variables. By controlling for the (continuously measured) earnings surprise level, we account for the possibility that higher earnings surprises may proxy for stronger future economic fundamentals. A violation of the identification assumption would require an unobservable time-varying characteristic that independently affects the outcome variables and, moreover, a *discontinuity* in such a characteristic around the zero pre-repurchase EPS surprise threshold.

Since firms can also use various other techniques to manage earnings beyond buybacks (e.g., accruals-based or real earnings management), one potential concern with our empirical strategy is that firms on different sides of the discontinuity threshold may use alternative earnings management tools to meet analyst EPS forecasts. The principal concern here is that firms just to the right and to the left of the discontinuity have acted differently using other earnings management tools and that those tools, in turn, independently affect future productivity and resource allocation. We provide two pieces of evidence to mitigate this concern. First, Almeida et al. (2016) show that the effect of short-term incentives on repurchases remains after controlling for accruals and changes in guidance, suggesting that discontinuous differential usage of alternative earnings management tools around the threshold is unlikely to affect our results. It is also hard to imagine that accruals management could affect future real variables such as productivity or employment. Second, we empirically test the extent to which real earnings management variables, measured as changes to R&D and SG&A, differ discontinuously around the threshold. The results, reported in Appendix

Table A.1, show that firms on either side of the threshold appear similar on these dimensions. That is, while many firms may also be engaging in other forms of earnings management, we nevertheless do not observe differential use of these on either side of the threshold.

To further support the identification assumption, we examine the distribution of Sue_{adj} around the discontinuity. Figure A.2 shows that the distribution of Sue_{adj} is quite smooth around the discontinuity.[16] Finally, in section 6, we present robustness tests showing 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).

Because the census data is limited to manufacturing firms and thus more limited than the sample in Almeida et al. (2016), we also verify whether there exists—also within the sample 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.[17] Appendix Table A.2 and Figure A.1 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.

[16]Note, however, that the distribution is right-skewed, suggesting that analysts, on average, underestimate earnings. This fact does not affect our identification strategy as this does not differentially affect firms just to the right or left of the threshold.

[17]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).

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 within the Census 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 that is increasing in their size. Reporting for both surveys is mandatory, and misreporting is penalized, so the data is of the highest quality. The CMF and ASM both include information on location, industry, corporate affiliation, output (total value of shipments), employment, capital expenditures, and material inputs of each plant. The granular level of detail in these manufacturing plant data helps us measure factor inputs and construct productivity measures for each individual plant.

Additional firm-level data come from CRSP/Compustat, and analyst forecasts used for calculating the consensus earnings forecasts are from IBES. 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 plant's/firm's employment expenditures (salaries and wages, i.e., payroll) over three years after the focal year minus the value in the year before the focal year, normalized by the lagged capital stock. Each plant's capital stock is estimated using the perpetual inventory method following Brav et al. (2015), and described in detail in Giroud (2013), Krishnan et al. (2014), and Ersahin (2020). When we measure employment outcomes at the firm level, we sum the employment expenditures across all the firm's plants.[18]

In some tests, we separately examine production and non-production workers. Production workers are employees (up through the working foreman level) engaged in production operations at the plant. Non-production personnel includes supervisors (above the working foreman level) and office employees in sales and marketing, financing, purchasing, professional and technical.

Alongside employment as an outcome variable, we also measure changes to investment. We calculate the change in investment as the three-year average of plant-level capital expenditures after the focal year minus capital expenditures in the year before the focal year, scaled by the lagged plant-level capital stock. We further investments separately for the subcategories "machinery" and "information technology." Firm-level investment is calculated by aggregating across plants.

The most critical object to define and measure is the level of "productivity" for plants and firms—that is, how much output is obtained from a given set of inputs. We measure plant productivity as the natural logarithm of total factor productivity (TFP) following the methodology of Foster et al. (2016). Specifically, TFP is calculated as follows, using the "index approach" of Syverson (2011):

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

[18]For robustness, we also consider two alternative measures of employment: The change in the natural logarithm of the number of employees and the symmetric growth rate of employment. The latter 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, which accommodates both entry and exit and limits the effects of extreme values (Davis et al., 1998).

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 plus 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. For factor elasticities, we follow Foster et al. (2016) and use industry-level total factor cost shares for each plant by averaging factor cost shares at time t and $t-1$.[19]

Our final sample contains 3,300 firm-year observations covering approximately 35,000 plant-years from 1988 until 2013. We define all the variables in Appendix A.

Table I presents summary statistics for the full sample, both at the firm and plant levels. This table also separately reports statistics for subsamples based on whether firms have slightly positive or negative pre-repurchase EPS surprises (columns 4–9).[20]

[Insert Table I here]

4 Results

This section describes how firms' incentives to engage in EPS-motivated buybacks affect their future productivity and resource allocation, using the fuzzy regression discontinuity (RD) framework described in section 2. We begin by estimating the effects on firm-level productivity. We then study how these overall productivity effects are separately driven by (i) plant-level productivity changes versus (ii) reallocations in investment across plants.

To better understand these productivity changes at the plant level, we examine investments specifically in categories of "technology-embedded capital," and how ex-ante pro-

[19] Our results are robust to the use of plant-level factor cost shares.

[20] 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.

ductive and unproductive plants are differentially affected. We also study differences in employment effects across production and non-production workers. Finally, to study the dynamic impacts, we investigate the year-by-year changes in resource allocation patterns and the associated productivity trends.

4.1 Effects on productivity

We begin the main analysis by examining the effects of firms' incentives to engage in EPS-motivated buybacks on firm- and plant-level productivity. We measure productivity changes as changes to a firm's (or plant's) Total Factor Productivity (ΔTFP). In the plant-level analysis, we measure ΔTFP as the difference between the three-year average future productivity of plant j in firm i minus the lagged productivity of that plant. In the firm-level analysis, TFP is the capital-weighted average of the individual plant-level TFPs for firm i (Giroud and Mueller, 2015; Schoar, 2002), and ΔTFP is the difference between the three-year average future productivity of firm i after the focal year less the one-year lagged productivity of that firm.

We start by analyzing the *firm-level* effects. Our null hypothesis is that—while the firms that have an incentive to boost their EPS using buybacks on average scale down their future investments in employment and investment—this does not necessarily harm their productivity (TFP) as each firm will be a slightly smaller version of itself. A firm's productivity may even increase if it mostly scales down its less efficient projects and plants. The alternative hypothesis is that productivity drops if firms make investment cuts that harm their productivity (e.g., by hampering capital-embodied technological progress). To test this hypothesis, we estimate Equation (1), with the change in TFP as the outcome variable. The results are reported in Panel A of Table II.

[Insert Table II here]

These results show that the firms with incentives to boost current EPS using buybacks (those to the left of the zero-pre-repurchase EPS threshold) on average experience a *deterioration* in their productivity over the next three years (compared with firms that are just to the right of the same threshold and that therefore do not have an incentive to engage in these buybacks). 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.[21]

Two different reasons could explain this drop in productivity at the firm level. The first is that productivity could be dropping on average at the plant level, and the other reason is that each plant could maintain its productivity, but firm productivity might decline due to inefficient allocation of resources across plants. We start by examining the first of these hypotheses by investigating the effects on plant-level productivity, and we report the results in Panel B of Table II (in the next section, we examine the effects of across-plant reallocation). In this regression, we control for each plant's location (state fixed effects), which we further interact with time fixed effects, and we additionally include industry-by-time fixed effects as well as controls for plant age and size.

Panel B shows that plant-level productivity also drops by a similar magnitude as we previously found for firm-level productivity; the point estimate of the average effect is a TFP drop of 1%. This result thus suggests that scaling down investment does not merely have a neutral effect on a plant's productivity in these circumstances but rather a negative one. To illustrate this effect graphically, Figure 1 shows visual evidence that there is a sharp drop in plant productivity right at the point at which there is a jump in incentives to engage

[21]As is common in the literature—see, e.g., Krishnan et al. (2014)—we interpret this estimate through the lens of profits as follows: First, note that a 1.3% decrease in TFP corresponds to a 1.3% decrease in revenues using the same set of inputs (i.e., holding costs constant), which in turn depends on the profit margin. Assume that costs are $100 and revenues are $125, which implies the profit margin (profits over revenues) is equal to 20%. Then, one way to think of a TFP decrease of 1.3% would be that revenues decrease to $123.375 (125*0.987) while costs stay constant at $100. This, in turn, causes profits to decrease from $25 to $23.375 (123.375-100)—a decrease of 6.5%. Note that a given decrease in TFP (e.g., 1.3%) thus tends to result in a magnified effect on profits (e.g., 6.5%).

in EPS-boosting buybacks (the zero pre-repurchase EPS surprise threshold).[22]

[Insert Figure 1 here]

Why do plants become less productive? To investigate the underlying mechanisms, we begin by studying the allocation of resources at the plant level (Table III), and in section 4.2, by exploiting heterogeneity across ex-ante more-productive versus less-productive plants.

In Table III, we report results on changes in plant-level resource allocation between firms right around the zero pre-repurchase EPS threshold. We find that both employment expenditures and capital investments fall at the plant level after firms have an incentive to engage in EPS-motivated buybacks (columns 1 and 2). For example, firms invest 2.2% (measured as a fraction of their capital stock) less in employment expenditures, representing a reduction of around 4% of the average firm-level employment expenditures. These plant-level results are consistent with firm-level findings in Almeida et al. (2016). In column 3, we examine the effects on whether plants are closed or sold, which we refer to as 'separation.' We find little evidence of more separations around these events, suggesting that the bulk of the reduction in employment/investment occurs within existing plants (intensive margin) rather than on the extensive margin by selling off or closing plants.

[Insert Table III here]

Beyond these overall cuts to capital, it is also important to examine *which types* of capital these cuts affect. If marginal-value projects were cut, productivity should increase, yet our evidence shows that productivity is decreasing. This suggests that these investment cuts include capital types critical for supporting productivity. One example of such investments that the previous literature has identified is referred to as "technology-embodied"

[22] Appendix Table A.3 reports two-stage-least-squares (2SLS) results, which are consistent with the reduced form results that directly employ the discontinuity as the independent variable. We report these results for both firm-level productivity (Panel A) and plant-level productivity (Panel B).

capital (Syverson, 2011). Johansen (1959), Solow (1960), Cummins and Violante (2002), and Sakellaris and Wilson (2004) argue that more recent vintages of capital embody important technological advances, which means that continued investment is essential to reap benefits from the part of technological progress that is embodied in this capital. In the context of this study, two specific components of investment where we might expect newer vintages of capital to have particularly large spillovers on productivity and that we can separately measure in the Census data are investments in (i) new machinery and (ii) computers/IT equipment.

In Panel B of Table III, we show results for the effects on investments specifically in machinery and computers. Consistent with a hypothesis whereby technology-embodied capital can explain the drop in plant-level productivity, Panel B shows significant cuts to investments in new machinery (column 1) and computers/IT equipment (column 2)—precisely the types of investments that tend to support productivity growth. One possible interpretation of our results is that firms with an incentive to boost EPS using buybacks subsequently scale down productivity-boosting investments and thus are left behind by other firms. These results are also broadly consistent with the findings in Jovanovic and Rob (1997) that any distortions to investment incentives can have a substantially larger effect when technological growth is embodied in equipment.

Why would managers cut IT investments specifically? One possibility is that these types of investments tend to not be "strategic," but instead tend to be more of an incremental nature and thus easier to change. By contrast, managers may be less likely to cut larger strategic investments that have been planned long in advance. Further, IT investments are special in that their effects tend to have *relatively* smaller impact on productivity in the very short run (e.g., Brynjolfsson and Hitt, 2003; Gordon, 2003; Oliner et al., 2007). Thus, managers focused mostly on the short run may be encouraged to neglect these incremental investments.

We next examine differential changes across "blue-collar" vs. "white-collar" employees. While the microdata we use for this study does not explicitly distinguish between blue- and white-collar workers, it provides information about two groups of employees—production workers and non-production personnel—which we use as proxies for blue- and white-collar workers, respectively. Production workers represent employees (up through the working foreman level) engaged in production operations at the plant. Non-production personnel includes supervisors (above foreman level) and office employees in sales and marketing, financing, purchasing, and professional and technical roles. Panel C of Table III presents the results, showing cuts to both production and non-production employees.

4.2 Heterogeneity across ex-ante productive vs. unproductive plants

One of the key advantages of using plant-level data is that it allows for more closely examining the within-firm allocation of investments in new capital and labor. In particular, this enables us to exploit heterogeneity in the effects between plants that are ex-ante more productive versus less productive. To further examine underlying mechanisms behind the firm-level drop in productivity, we test two hypotheses that exploit heterogeneity across ex-ante high- vs. low-productivity plants regarding (i) differential effects for resource allocations across plants and (ii) differential effects on TFP across plants.

The first hypothesis we test is whether firms mitigate the impact on productivity at the *firm-level* by directing more of the cuts in investments and employment towards the ex-ante less productive plants. On the one hand, if cuts to investment and employment primarily take place in less productive plants, that would suggest that firms' responses in terms of their within-firm allocation of resources are relatively efficient (Giroud and Mueller, 2015). In that case—if firms allocate relatively fewer cuts to their productive plants (or perhaps do not make any cuts at all in these plants)—they can improve the overall capital allocation across their plants and counteract the plant-level decline in productivity. On the other hand, if firms also cut investment and employment in their more productive plants, that would suggest a less efficient process whereby firms allocate resources.

To examine these hypotheses, we estimate regression (1) using plant-level data and further interact $I_{Negative\ Sue_{adj}}$ with two indicator variables for $Productive_{t-1}$ and $Unproductive_{t-1}$, respectively. $Productive_{t-1}$ 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. $Unproductive_{t-1}$ is defined analogously. Table IV reports the results.

[Insert Table IV here]

In Panel A, we study the differential effects on changes in employment and capital investment across high- vs. low-productivity plants. The first row of Panel A of Table IV shows that in normal times (i.e., when the firm does not face the incentives to do EPS-motivated buybacks), unproductive plants tend to see decreased investment and employment compared with productive plants. However, the next rows (2 and 3) show how productive and unproductive plants differ in the effects on investment and employment in the years after firms have had the incentive to engage in EPS-motivated buybacks. Here, the results show evidence of more inefficient cuts. Specifically, estimated coefficients for *Negative Pre-Repurchase EPS Surprise*$\times$*Productive*$_{t-1}$ and *Negative Pre-Repurchase EPS Surprise*$\times$*Unproductive*$_{t-1}$ show that cuts in employment (column 1) appear roughly equally large, and these coefficients are also statistically indistinguishable from each other. These results are similar when analyzing investment outcomes (column 2).[23]

In Panel B of Table IV, we also study the differences in investments in machinery and computers across productive and unproductive plants. Even for these investments, we cannot

[23] Column (3) shows that neither productive nor unproductive plants are more likely to be "separated" from the firm, i.e., closed or sold.

reject the hypothesis that the cuts in response to short-term incentives are similar "across-the-board" regardless of each plant's productivity.

Next, in Panel C of Table IV, we study differential changes in employment changes separately for production workers and non-production personnel in high- vs. low-productivity plants. We find that production workers are reduced across both productive and unproductive plants. However, for non-production personnel, these cuts happen only in productive plants and significantly more so than in unproductive plants. These results imply important heterogeneity in how different types of employees are affected by corporate short-termism. Second, these results suggest that cuts to white-collar employees (especially those in relatively productive plants) likely play an important role in explaining the deterioration in long-term firm-level productivity.

Next, in Table V, we similarly study the differential effects on productivity changes for ex-ante productive and unproductive plants. To the extent that the effects on average productivity that we identified in the previous section are related to a decline in investments in technology-embedded capital, we expect that plants that are ex-ante more productive might suffer relatively more from a drop in new investment (Giroud and Mueller, 2015). The idea is that more productive plants require a higher level of continued investment to remain near the technology frontier. In contrast, unproductive firms might be less affected from scaling down investment. To test this hypothesis, we employ a similar regression as in the previous test by interacting $I_{Negative\ Sue_{adj}}$ with two indicator variables, $Productive_{t-1}$ and $Unproductive_{t-1}$, and examining productivity changes as the left-hand side variable.

[Insert Table V here]

The results in Table V show that the drop in productivity appears more pronounced in ex-ante productive plants and is only significant in this group.[24] However, we cannot reject that

[24]It is also interesting to note that the coefficient on *Unproductive* is negative, which means that unproductive plants, on average, tend to become even less productive over time—that is, plants tend not to revert to some mean productivity level but instead continue to diverge.

these changes in productivity are the same across both productive and unproductive plants (the p-value of the difference is 0.48). This evidence nevertheless adds further support to the hypothesis that the cuts that firms make do not exclusively affect only the unproductive plants—that is, if firms were doing efficient reallocations, then we might have expected to see that the unproductive plants would be more adversely affected.

4.3 The dynamics of consequences for productivity and resource allocation

Our previous results examined the average changes in productivity, employment, and investment over three years after the focal year compared to the year before the focal year t (when we measure the pre-repurchase EPS surprise). This section examines the dynamic progression of the changes to investment, employment, and productivity.

In particular, we might expect the effects on employment not to occur immediately but to take some time to show up strongly in the data. The significant cuts to investment that we find may also take time 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 VI here]

Table VI studies the year-by-year dynamics of the effects on employment, investment, and productivity at the firm level. These results show that the cuts to investment happen fairly immediately but persist over all three years after the focal year (columns 1–3). By contrast, employment cuts tend not to happen all at once but grow larger over time (columns 4–6). In economic terms, the effect on employment grows almost three times between the first and third years. The differences in these dynamics between changes to labor and capital are consistent with a hypothesis that labor adjustment costs tend to be relatively higher, thus resulting in employment adjustments taking place over a relatively longer period.

The adverse consequences for productivity also become evident in the data by the first year and persist into the later years (columns 7–9). These results suggest that investment cuts that follow EPS-motivated buybacks can have a fairly immediate effect on productivity.[25] The timing of these effects is consistent with the symmetrically opposite result in Giroud (2013), who identifies increases in investment that positively affect TFP within six months.[26]

5 Potential mechanisms

The previous findings that firms make significant cuts to their high-productivity plants raises the following question: Why don't firms allocate their resources more efficiently (e.g., by cutting relatively more in their less efficient plants) to mitigate the negative long-run impact of short-term pressures? The patterns in resource allocation and the adverse consequences on productivity suggest that firms face frictions or agency problems that stand in the way of more efficient allocation. In this section, we analyze such potential mechanisms by considering the role of labor market frictions, financial constraints, agency conflicts, and adjustment costs.

5.1 Labor frictions

We start by examining the role of frictions that can arise from constraints when bargaining with labor unions. Unions seek to protect workers, and thus, we would expect the presence of unions to be associated with smaller reductions in employment, although unions might not completely stop employment cuts from happening. More critically for the reallocation process, labor unions often impose rules on companies that determine how employees and resources can be allocated both within and across a firm's plants. Such constraints include

[25] For the investment cuts, we can use Compustat level to investigate (at the firm level) at what point during the first year these cuts take place. We find significant cuts already in the first and second quarters after the focal time, which may help explain why they affect TFP already during the first year.

[26] Syverson (2011) also describes the fairly rapid effects of investments on productivity growth.

seniority rules and rules around how firms can reassign tasks across workers. Such a relative lack of flexibility means that a dollar cut in a unionized plant could have worse overall consequences for productivity than a dollar cut from a non-unionized plant.

To test whether unions can help explain our baseline results, we exploit cross-sectional variation in the extent of union power across firms and plants to examine if those firms and plants that are likely to be more constrained by union power tend to make less efficient downsizing decisions than firms that are less constrained.

Following Bloom et al. (2019) and Chava et al. (2020), we use state-level right-to-work (RTW) laws as a source of variation in labor bargaining power. RTW laws weaken union power by prohibiting any agreement between employers and unions that requires employees to contribute to the unions. We measure each firm's exposure to union power by calculating the fraction of plants (or, as an alternative measure, the fraction of employees' production hours) that are located in RTW states. Note that the higher this fraction, the *less* exposed firms are to union power. We use this measure to split firms into two groups based on whether their exposure to RTW states is above or below the sample median. We then investigate whether firm-level productivity or the efficiency of resource allocation differs depending on the fraction of the firm's business located in RTW states.

We start by studying the effects on productivity. The results are reported in Table VII. In Panel A, we see that the full-sample negative productivity effects come from firms that are more likely to be exposed to unions, 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.[27]

[Insert Table VII here]

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

Exposure to labor unions should matter more when firms are reducing the total number of employees rather than merely reducing the level of new hiring. To test this hypothesis, we interact the indicator for *Negative Pre-Repurchase EPS Surprise* with *Employment Cut*, which is an indicator variable equaling one if the total number of employees decreases from time $t-1$ to time $t+1$. Panel B in Table VII reports the results. In columns 2 and 4, we see that the estimated coefficients on the interaction variable are negative and statistically significant, which shows that the negative effect on TFP that we report in Panel A stems from firms in non-RTW states that reduce the overall number of employees. These results further support the hypothesis that constraints around the reallocation of labor play an important role in how firms react to incentives to boost short-term performance.

To further corroborate the differential productivity results that depend on firms' exposure to union power, in Panel C, we next study the effects on employment and investment across productive vs. unproductive plants. We examine whether the cuts in employment and investment are similar in states that have adopted RTW laws vs. those that have not. Since this analysis uses plant-level data, we can directly compare plants in states that have adopted RTW legislation and those in states that have not (instead of splitting firms based on the fraction of plants across these two groups).

We observe striking differences in how firms respond depending on whether a plant is in a RTW or non-RTW state. Columns 1 and 2 of Panel C in Table VII show that for plants located in RTW states—where the labor force is less organized—we observe no cuts to either employment or investment. By contrast, on the 'extensive' margin, the results show that plants in RTW states are more likely to be sold or closed if they are unproductive (column 3).[28] Conversely, plants in non-RTW states are less likely to be sold or closed (column 6), consistent with previous research showing that unions impede takeovers (Dessaint et al., 2017; Tian and Wang, 2021). However, plants in non-RTW do experience significant reductions

[28]This result is consistent with Bai and Mkrtchyan (2023) who show that outside CEOs improve firm performance by divesting low-TFP plants.

in employment, regardless of whether these plants are productive or not (columns 4–5); this finding is somewhat surprising since it shows that unions do not, on average, seem to protect total employment in remaining plants.[29] The cuts to investment are likewise similar across both productive and unproductive plants in the non-RTW states (that are more likely to have greater union power).

Overall, our findings suggest that firms that have plants in non-RTW states make cuts that end up hurting their overall productivity, indicating that union power adversely affects the efficient reallocation of resources in response to short-term pressures: Firms that have plants in right-to-work states tend to sell or close plants when they are unproductive, while plants in non-RTW states exhibit more indiscriminate reductions in both employment and investment across plants. A labor power channel may thus help explain why firms are not better at mitigating the decline in TFP by favoring productive plants over less productive ones.

5.2 Financial constraints

Next, we investigate the role of financial constraints. We posit that financially unconstrained firms may have enough capital to both invest, hire, and at the same time finance additional buybacks. This suggests that we should see inefficient cuts in investment and employment, mostly among financially constrained firms. We use two different measures of firm-level financial constraints. First, we use the Whited and Wu (2006) index, and we classify firms as financially constrained (unconstrained) if their Whited-Wu value is above (below) the median. Second, we use bond ratings: we classify firms without a bond rating as financially constrained and unconstrained if they have one.

The results are reported in Table VIII. Panel A shows results for changes to firm-level

[29] However, we do not have individual employee data such as seniority, so we cannot directly test whether unions are associated with fewer cuts among more full-time senior workers and more cuts among part-time junior workers.

productivity. Columns 1 and 3 indicate that the decreases in TFP are concentrated among the financially constrained firms. By contrast, columns 2 and 4 show no effects on productivity for financially unconstrained firms. This differential finding holds across both measures of financial constraints.

In Panels B and C, we report plant-level results on how financial constraints are related to changes to employment and investment and how these changes differ across productive vs. unproductive plants using the Whited and Wu (2006) and bond ratings measures, respectively. In line with the hypothesis that financially constrained firms are less likely to mitigate negative consequences from an incentive to focus on short-term performance, only the financially constrained firms make significant cuts to investment and employment, which apply across both productive and unproductive plants.

[Insert Table VIII here]

5.3 Agency conflicts and managerial incentives

The next set of frictions we consider sheds light on the role of agency problems and managerial incentives. We consider two empirical proxies for agency/governance frictions. First, we investigate how differences in the investment horizon of a firm's institutional investors can moderate or amplify the effects on the firm's future productivity and allocation of resources (Chen et al., 2007; Gaspar et al., 2005; Harford et al., 2018). We hypothesize that dedicated institutional investors with long horizons monitor managers more effectively by making sure they do not sacrifice long-term goals, such that having more of these investors can mitigate the negative effects of short-term incentives.

To measure the horizon of the firm's institutional investors, we use Bushee's classification of Schedule 13F investors (Bushee, 1998, 2001) to categorize investors into three types: dedicated, quasi-indexer, and transient. The investors classified as "dedicated" have a longer horizon. We then calculate the total ownership by dedicated investors for each firm-year and sort firms into high vs. low long-term investor ownership based on whether share ownership by dedicated investors is below or above the median.

Panel A in Table IX shows that firms with above-median long-term investor ownership show no deterioration in TFP when faced with incentives to engage in EPS-motivated buybacks (column 3). In contrast, firms with relatively more short-term investors experience a significant decrease in TFP (column 2). In Panel B, we investigate plant-level outcomes for employment and investment. Here, we see that only firms with more short-term investors undertake inefficient cuts—specifically, these firms tend to cut investment and employment significantly across both productive and unproductive plants. By contrast, firms with above-median long-term ownership tend to cut investment mostly in unproductive plants, suggesting a more efficient response that is consistent with the lack of a negative effect on firm-level productivity in Panel A.

[Insert Table IX here]

As a second type of agency conflict, we examine the role of managerial contracts. Many executives have pay contracts that explicitly reward meeting specific EPS targets. EPS incentives in managerial contracts can create an agency friction with consequences for how firms differentially react when faced with a short-term incentive to boost EPS using share repurchases. To implement a test along these lines, we use detailed CEO compensation data from ISS Incentive Lab, which is derived from proxy statements (DEF14A). We use these data to create an indicator variable for whether the CEO's pay depends explicitly on EPS.

Panel A of Table X shows that the negative consequences for TFP are concentrated only in firms with EPS-related CEO pay (column 2). Conversely, there is little evidence of any adverse productivity consequences among the firms that do not explicitly use EPS as part of CEO pay, as the coefficient is both economically close to zero and statistically insignificant (column 3). In Panel B, we see that both the unproductive and unproductive plants in firms where the CEO's pay depends explicitly on EPS experience significant employment and investment cuts. The cuts to employment appear especially inefficient since these cuts affect productive plants even more. By contrast, for firms where the CEO's pay does not depend explicitly on EPS, there are no significant cuts to either employment or investment.

[Insert Table X here]

5.4 Adjustment costs

The final set of frictions we consider is focused on adjustment costs. Because the costs to adjust investments tend to be convex in the size of the change, conducting a large cut at one plant can be costlier compared with making several smaller cuts across multiple plants. To test this hypothesis, we construct a firm-level measure of "investment-cut concentration," similar to the Herfindahl-Hirschman (HHI) index of market concentration. According to this measure, if the investment-cutting firm has only one plant, the investment-cut HHI equals one. The investment-cut HHI of a multi-plant firm will be less than one (unless all cuts are within only one plant). For example, suppose a firm with two plants distributes the cuts equally among the two plants, then the investment-cut HHI equals $(1/2)^2+(1/2)^2=1/2$.

Table XI reports the results. In column 1, we regress firm-level TFP on the indicator for a negative pre-repurchase EPS surprise interacted with investment-cut HHI. We find that the estimated coefficient on the interaction term is negative, indicating that—consistent with large, concentrated cuts being costlier—as the cuts become more concentrated, the negative effect on firm-level TFP gets worse.

[Insert Table XI here]

Even though cutting evenly across many plants might be less costly from an adjustment cost perspective, a force running counter to this is that plants may have different productivity levels. That is, a firm that has significant heterogeneity in plant productivity may want to cut investment in its most unproductive plants only, even if these cuts are larger and more expensive. In columns 2 and 3 on Table XI, we conduct a test that can help tease out the convex adjustment cost mechanism from the heterogeneity-in-productivity mechanism. We split firms into those with a high vs. low within-firm standard deviation of productivity and perform the same tests in these subsamples. In column 2, where we hold the within-firm heterogeneity in productivity low, we continue to see a negative and significant coefficient on the interaction term. This alleviates the concern that the negative effect found in column 1 is coming from within-firm heterogeneity in productivity rather than convex adjustment costs.

6 Robustness

In this section, we consider several robustness tests. We first examine how our results could be sensitive to alternative ways of measuring our key variables. Panels A and B of Appendix Table A.4 report results from Tables II and III, where we use alternative measures for productivity and resource allocation. A concern with our TFP measure is that it relies on structural assumptions, including the Cobb-Douglas production function (Giroud, 2013). As an alternative to our baseline method, we use the structural techniques of Olley and Pakes (1996) and Levinsohn and Petrin (2003) to calculate TFP. The results remain similar (columns 1–2). We also consider alternative measures that do not involve structural assumptions, including operating margin, labor productivity, and return on capital (ROC). Our baseline TFP results remain with these alternative measures as well (columns 3–5).

Appendix Table A.4 further shows that 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 6 and 7).[30] Further, these results with alternative measures hold both at the firm level (Panel A) and the plant level (Panel B).

In Panel C of Appendix Table A.4, 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 (rather than within-firm as in our baseline results) 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). The results are similar to the baseline results: We observe significant cuts to employment and capital investments that appear indiscriminate in that the magnitudes are similar across both productive and unproductive plants.[31]

TFP is also highly correlated with stock market performance Schoar (2002) and profitability (Foster et al., 2008). As an additional robustness test, in Appendix Table A.5, we test how incentives to do short-term buybacks affect firm value (Tobin's Q) and profitability (operating income before depreciation divided by assets). The negative effects are consistent with the baseline findings of lower TFP and inefficient resource allocation.

Appendix Table A.6 presents several robustness tests for our regression discontinuity framework. Panel A and Panel B show 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 C and Panel D of Appendix Table A.6 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

[30]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).

[31]A related concern is that we might be misclassifying "core" plants, which could appear to be low-productivity even though they are critical to the firm's operations. This is difficult to completely rule out, although Maksimovic and Phillips (2002) show that core plants are, on average, more productive (higher TFP) than peripheral plants, which suggests that core plants are more likely to be classified as high-productivity. In untabulated results, we also confirm this finding using our data, using a definition of core plants as plants operating in three-digit SIC industries that account for more than 25% of the firm total employment expenditures.

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. Specifically, we find no systematic pre-existing differences in the changes to productivity, employment, or capital expenditures on either side of the zero pre-repurchase EPS threshold in either of the two years before the focal year.

To further support the identification assumption and empirical framework, we consider two "placebo thresholds:" Rather than limiting the sample to firms in the window [–0.003, +0.003] with a threshold at 0, we consider two samples of false thresholds centered on –0.006 and +0.006, respectively, with corresponding windows of [–0.009,–0.003] and [+0.003, +0.009]. We investigate changes to both firm- and plant-level TFP using these windows, and report results in Appendix Table A.7. We find that the estimated coefficients are statistically indistinguishable from zero. These placebo tests provide additional evidence that our benchmark results do not spuriously arise from the underlying empirical framework.

7 Conclusion

This paper studies the long-term productivity effects of firms' 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 resource allocation and productivity changes at the plant level and study how pre-existing plant characteristics relate to these changes.

Overall, our paper suggests that actions to boost short-term profits have negative long-term consequences for manufacturing firms. In particular, our evidence suggests that short-term incentives lead to lower long-term productivity at both the firm and plant levels. The decline in plant-level productivity is, in turn, associated with cuts in productivity-augmenting investments in machinery and IT. Overall, these results suggest that plants require sustained investment to maintain their level of productivity and that cuts made to meet short-term goals can thus have negative consequences. Firms can minimize the negative effects of investment cuts on productivity by efficiently allocating resources across their projects and plants (e.g., Giroud and Mueller, 2015; Stein, 1997). However, we find that firms cut investments indiscriminately ("across the board") irrespective of whether a particular plant is productive or unproductive.

These findings are consistent with the notion that firms face various frictions when allocating resources in response to short-term pressures, which can amplify the adverse consequences of short-termism. Frictions related to bargaining with labor unions appear to act as such a mechanism, as the deterioration in firm productivity and the indiscriminate cuts in investment and employment appear driven by firms whose plants are primarily concentrated in non-right-to-work states. We also find that financial constraints, agency problems, and adjustment costs additionally can help explain why firms reallocate resources inefficiently in response to short-term incentives.

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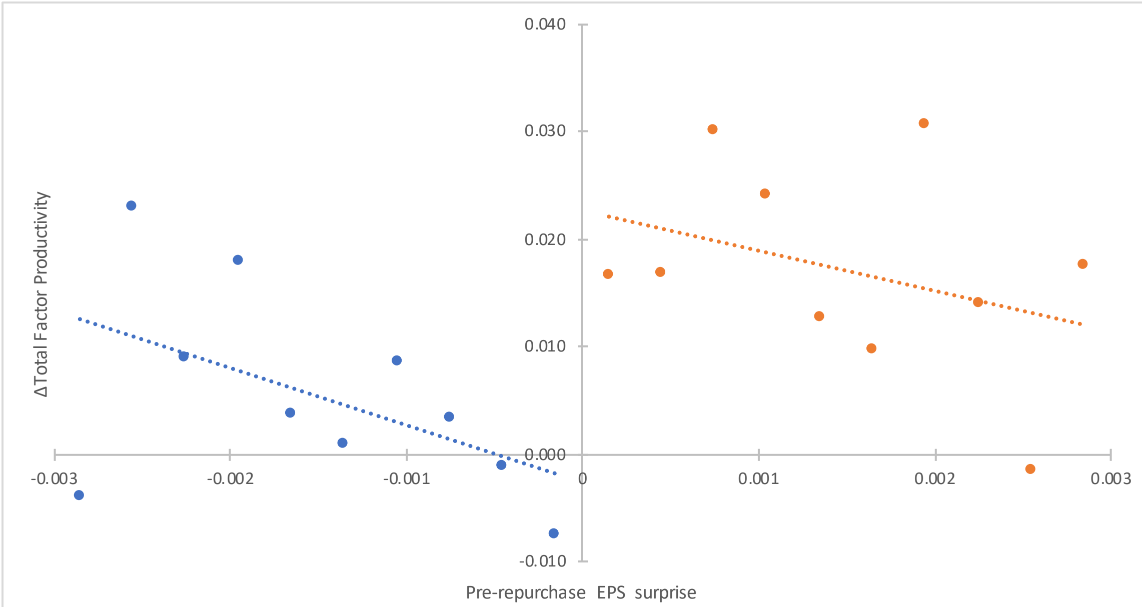


Figure 1: Negative pre-repurchase EPS surprises and change in productivity among manufacturing plants

This figure shows the difference in future productivity depending on the pre-repurchase EPS surprise as a binned scatterplot. The x-axis is the pre-repurchase EPS surprise, divided into 20 separate bins between -0.003 and $+0.003$ (each bin having a range of 0.0003). The y-axis is the difference in productivity measured as the TFP over the following three years less the lagged TFP, averaged across observations within each bin. The lines represented fitted values on each side of the zero threshold. 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.

Table I
Summary statistics

This table provides sample summary statistics at the firm-level and at the plant-level as of each focal year (t) (columns 1–3). Columns 4–9 further show these statistics in split samples based on whether the firm has a 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. All variables are defined in Appendix A.

	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			Negative [–0.003,0)			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
Production Employment	35,000	0.34	0.34	11,500	0.32	0.30	23,500	0.35	0.35
Non-Production Employment	35,000	0.21	0.27	11,500	0.20	0.26	23,500	0.22	0.28
Investment	35,000	0.09	0.09	11,500	0.08	0.09	23,500	0.09	0.09
Machinery Investment	35,000	0.07	0.08	11,500	0.07	0.07	23,500	0.07	0.08
Computer Investment	14,000	0.01	0.02	4,000	0.01	0.02	10,000	0.01	0.03
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

Table II
Short-term incentives and productivity

This table shows estimates of the impact of incentives to engage in EPS-motivated share repurchases on firm- and plant-level productivity. In Panel A, the unit of observation is a firm-year. In Panel B, the unit of observation is a 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 as indicated. 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 III
Short-term incentives and plant-level resource allocation

This table shows estimates of the impact of incentives to engage in EPS-motivated share repurchases on plant-level resource allocation (intensive margin), and on the decision to sell or close plants (extensive margin). The unit of observation in each regression is a plant-year. The outcome variables in columns 1–2 of Panel A are changes in employment expenditures and investment. 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 variable in column 3 of Panel A is an indicator for whether the plant was separated (i.e., either sold or closed) in the three years after the focal year. In Panel B, we examine the effects on two sub-categories of investment: machinery investment (column 1) and computer investment (column 2). In Panel C, we examine the effects on two sub-categories of employment: production employment (column 1) and non-production employment (column 2). 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: 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

Panel B: Investment Types		
Dependent variable:	Δ*Machinery Investment*	Δ*Computer Investment*
	[1]	[2]
Negative Pre-Repurchase EPS Surprise	–0.004**	–0.001*
	(0.002)	(0.001)
Linear control in pre-repurchase EPS surprise	Y	Y
Plant controls	Y	Y
Industry × year fixed effects	Y	Y
State × year fixed effects	Y	Y
Rounded *N*	35,000	14,000
R^2	0.066	0.201

Panel C: Employment Types		
Dependent variable:	Δ*Production Employment*	Δ*Non-Production Employment*
	[1]	[2]
Negative Pre-Repurchase EPS Surprise	–0.013***	–0.008**
	(0.004)	(0.004)
Linear control in pre-repurchase EPS surprise	Y	Y
Plant controls	Y	Y
Industry × year fixed effects	Y	Y
State × year fixed effects	Y	Y
Rounded *N*	35,000	35,000
R^2	0.113	0.082

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

This table shows estimates of the plant-level impact of incentives to engage in EPS-motivated share repurchases on resource allocation across ex-ante productive vs. unproductive plants. The unit of observation in each regression is a 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), scaled by the capital stock in $(t-1)$. In Panel A, the outcome variables in columns 1–2 are plant-level changes in employment expenditures and investment. The outcome variable in column 3 is an indicator for whether a plant was separated (sold or closed) in the three years after the focal year. In Panel B, the outcome variables represent two sub-categories of investment: machinery investments (column 1) and computer investments (column 2). In Panel C, the outcome variables represent two sub-categories of employment: production employment (column 1) and non-production employment (column 2). $Productive_{t-1}$ 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. The sample and control variables are similar to Panel B of Table II, and we further include a control for the interaction of the linear control in the pre-repurchase EPS surprise with whether a plant is ex-ante unproductive as indicated. 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: Employment, Investment, and Separation			
Dependent variable:	$\Delta Employment$	$\Delta Investment$	*Separation*
	[1]	[2]	[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 prod. and unprod. interaction terms	0.58	0.61	0.17

Panel B: Investment Types		
Dependent variable:	Δ*Machinery Investment*	Δ*Computer Investment*
	[1]	[2]
$Unproductive_{t-1}$	–0.006***	–0.000
	(0.002)	(0.001)
Negative Pre-Repurchase EPS Surprise × $Productive_{t-1}$	–0.004	–0.001**
	(0.003)	(0.001)
Negative Pre-Repurchase EPS Surprise × $Unproductive_{t-1}$	–0.005*	–0.001
	(0.003)	(0.001)
Linear control in pre-repurchase EPS surprise	Y	Y
Linear control in pre-repurchase EPS surprise × $Unproductive_{t-1}$	Y	Y
Plant controls	Y	Y
Industry × year fixed effects	Y	Y
State × year fixed effects	Y	Y
Rounded N	35,000	14,000
R^2	0.067	0.202
F-stat	0.16	1.72
p-value of difference between prod. and unprod. interaction terms	0.69	0.19

Panel C: Employment Types		
Dependent variable:	Δ*Production Employment*	Δ*Non-Production Employment*
	[1]	[2]
$Unproductive_{t-1}$	–0.001	–0.013***
	(0.004)	(0.004)
Negative Pre-Repurchase EPS Surprise × $Productive_{t-1}$	–0.011**	–0.013***
	(0.005)	(0.004)
Negative Pre-Repurchase EPS Surprise × $Unproductive_{t-1}$	–0.016***	–0.003
	(0.005)	(0.005)
Linear control in pre-repurchase EPS surprise	Y	Y
Linear control in pre-repurchase EPS surprise × $Unproductive_{t-1}$	Y	Y
Plant controls	Y	Y
Industry × year fixed effects	Y	Y
State × year fixed effects	Y	Y
Rounded N	35,000	35,000
R^2	0.113	0.084
F-stat	0.77	4.23
p-value of difference between prod. and unprod. interaction terms	0.38	0.04

Table V
Changes to plant-level productivity: Ex-ante high- vs. low-productivity plants

This table shows estimates of the plant-level impact of incentives to engage in EPS-motivated share repurchases on total factory productivity (TFP) across ex-ante productive vs. unproductive plants. The unit of observation is a plant-year. Changes to TFP 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). $Productive_{t-1}$ 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. The sample and control variables are similar to Panel B of Table II, and we further include a control for the interaction of the linear control in the pre-repurchase EPS surprise with whether a plant is ex-ante unproductive as indicated. 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.

Dependent variable:	$\Delta\, TFP$
	[1]
$Unproductive_{t-1}$	–0.175***
	(0.013)
Negative Pre-Repurchase EPS Surprise × $Productive_{t-1}$	–0.023**
	(0.010)
Negative Pre-Repurchase EPS Surprise × $Unproductive_{t-1}$	–0.014
	(0.015)
Linear control in pre-repurchase EPS surprise	Y
Linear control in pre-repurchase EPS surprise × $Unproductive_{t-1}$	Y
Plant controls	Y
Industry × year fixed effects	Y
State × year fixed effects	Y
Rounded N	35,000
R^2	0.149
F-stat	0.51
p-value of difference between prod. and unprod. interaction terms	0.48

Table VI
Dynamics of effects

This table shows estimates of the dynamics over time from the impact of incentives to engage in EPS-motivated share repurchases. The unit of observation in each regression is a firm-year. The outcome variables are changes in investment, employment expenditures, and total factor productivity. 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). 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:	Δ*Investment*			Δ*Employment*			Δ*TFP*		
	(-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	–0.013**	–0.015**	–0.019**	–0.021	–0.044**	–0.059**	–0.025**	–0.024**	–0.027**
	(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

Table VII
Potential mechanisms: State-level union power

This table shows estimates of the impact of incentives to engage in 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 a 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 the 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 is a firm-year. In this panel, the specification from Panel A is further augmented with an interaction with *Employment Cut*, an indicator variable equaling one if the firm's total number of employees decreases from time $t-1$ to time $t+1$. In Panel C, the unit of observation is a 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 C is an indicator for whether the plant was separated (closed or sold). $Productive_{t-1}$ 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 [1]	Above med. [2]	Below med. [3]	Above med. [4]	Below med. [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: Firm-level productivity growth and employment cuts

Dependent variable: ΔTFP	% plants in RTW states		% production hours in RTW states	
Firm splits:	Above med. [1]	Below med. [2]	Above med. [3]	Below med. [4]
Negative Pre-Repurchase EPS Surprise	–0.015	0.041	–0.003	0.035
	(0.021)	(0.025)	(0.020)	(0.024)
Employment Cut	–0.021	–0.021	–0.013	–0.032
	(0.019)	(0.023)	(0.021)	(0.025)
Negative Pre-Repurchase EPS Surprise × *Employment Cut*	0.044	–0.075**	0.009	–0.040*
	(0.033)	(0.037)	(0.034)	(0.023)
Linear control in pre-repurchase EPS surprise	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Rounded N	1,700	1,700	1,700	1,700
R^2	0.023	0.021	0.028	0.017

Panel C: 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 VIII
Potential mechanisms: Financial constraints

This table shows estimates of the impact of incentives to engage in EPS-motivated share repurchases on firm-level productivity and resource allocation at plants depending on the firm's level of financial constraints. In Panel A, the unit of observation in each regression is a firm-year. Firms are partitioned based on the measured level of financial constraints as of $t-1$, using two different measures of constraints from the literature: Whited and Wu (2006) and whether the firm has a bond rating or not. The outcome variable is the firm-level change in total factor productivity. This change is 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). In Panel B and C, the unit of observation in each regression is a plant-year and the measure of financial constraints is Whited and Wu (2006) and bond rating, respectively. The outcome variables in Panel B and Panel C are plant-level changes in employment expenditures and investment, and an indicator for whether the plan was separated (closed or sold). 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 $t-1$. $Productive_{t-1}$ 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. 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	Whited and Wu (2006)		Bond Rating	
Constrained firm?:	Yes	No	Yes	No
	[1]	[2]	[3]	[4]
Negative Pre-Repurchase EPS Surprise	–0.056**	-0.002	–0.051***	–0.002
	(0.022)	(0.017)	(0.018)	(0.017)
Linear control in pre-repurchase EPS surprise	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Rounded N	1,700	1,700	1,500	1,800
R^2	0.027	0.028	0.022	0.018
F-stat	3.62		3.23	
p-value of difference	0.06		0.07	

Panel B: Plant-level outcomes

Financial constraints measure:	Whited and Wu (2006): Constrained			Whited and Wu (2006): Unconstrained		
Dependent variable:	Δ*Employment*	Δ*Investment*	*Separation*	Δ*Employment*	Δ*Investment*	*Separation*
	[1]	[2]	[3]	[4]	[5]	[6]
$Unproductive_{t-1}$	–0.026*	–0.013*	–0.010	–0.013**	–0.005*	—0.004
	(0.014)	(0.007)	(0.017)	(0.007)	(0.003)	(0.010)
Negative Pre-Repurchase EPS Surprise × $Productive_{t-1}$	–0.063**	–0.014*	–0.076**	–0.013	–0.003	–0.019
	(0.026)	(0.007)	(0.032)	(0.009)	(0.004)	(0.018)
Negative Pre-Repurchase EPS Surprise × $Unproductive_{t-1}$	–0.070**	–0.013*	–0.047	–0.009	–0.006	–0.012
	(0.032)	(0.007)	(0.040)	(0.007)	(0.014)	(0.020)
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	6,300	6,300	7,900	29,000	29,000	35,500
R^2	0.272	0.237	0.278	0.118	0.072	0.146
F-stat	0.04	0.00	0.60	0.25	0.64	0.20
p-value of difference between prod. and unprod. interaction terms	0.83	0.95	0.44	0.62	0.51	0.66

Panel C: Plant-level outcomes

Financial constraints measure:	Bond Rating: Constrained			Bond Rating: Unconstrained		
Dependent variable:	Δ*Employment*	Δ*Investment*	*Separation*	Δ*Employment*	Δ*Investment*	*Separation*
	[1]	[2]	[3]	[4]	[5]	[6]
$Unproductive_{t-1}$	–0.023*	–0.013*	–0.010	–0.014**	–0.003	–0.003
	(0.013)	(0.007)	(0.015)	(0.007)	(0.003)	(0.011)
Negative Pre-Repurchase EPS Surprise × $Productive_{t-1}$	–0.040*	–0.011*	–0.011	–0.016	–0.003	–0.028
	(0.023)	(0.007)	(0.035)	(0.017)	(0.004)	(0.019)
Negative Pre-Repurchase EPS Surprise × $Unproductive_{t-1}$	–0.058**	–0.020*	0.017	–0.010	–0.007	–0.019
	(0.028)	(0.011)	(0.035)	(0.007)	(0.014)	(0.021)
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	7,800	7,800	9,600	27,500	27,500	34,000
R^2	0.246	0.199	0.259	0.117	0.072	0.150
F-stat	0.47	0.37	0.99	0.51	0.80	0.29
p-value of difference between prod. and unprod. interaction terms	0.49	0.54	0.32	0.47	0.37	0.59

Table IX
Potential mechanisms: Investor horizon

This table shows estimates of the impact of incentives to engage in EPS-motivated share repurchases on firm-level productivity and resource allocation at plants depending on whether share ownership by long-term investors is below- or above-median. We use Bushee's classification of Schedule 13F investors (Bushee, 1998, 2001) to proxy for share ownership by long-term investors. In Panel A, the unit of observation in each regression is a firm-year. The outcome variable is the firm-level change in total factor productivity. This change is 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). In Panel B, the unit of observation in each regression is a 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. 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		Long-term investors	
Firm splits:	Average effect	Below median	Above median
	[1]	[2]	[3]
Negative Pre-Repurchase EPS Surprise	–0.025**	–0.045**	–0.006
	(0.013)	(0.019)	(0.017)
Year fixed effects	Y	Y	Y
Rounded N	3,300	1,700	1,700
R^2	0.012	0.022	0.013
p-value of difference		0.10	

Panel B: Plant-level outcomes

Long-term investors:	Below median			Above median		
Dependent variable:	Δ*Employment*	Δ*Investment*	*Separation*	Δ*Employment*	Δ*Investment*	*Separation*
	[1]	[2]	[3]	[4]	[5]	[6]
$Unproductive_{t-1}$	–0.016**	–0.010***	–0.000	–0.009	0.002	–0.005
	(0.008)	(0.003)	(0.0010)	(0.009)	(0.004)	(0.017)
Negative Pre-Repurchase EPS Surprise × $Productive_{t-1}$	–0.031***	–0.007*	–0.021	–0.007	0.002	–0.013
	(0.009)	(0.004)	(0.020)	(0.013)	(0.008)	(0.024)
Negative Pre-Repurchase EPS Surprise × $Unproductive_{t-1}$	–0.022**	–0.007*	–0.013	–0.019	–0.011*	–0.006
	(0.010)	(0.004)	(0.021)	(0.013)	(0.007)	(0.029)
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	19,000	19,000	24,000	16,000	16,000	19,000
R^2	0.135	0.088	0.174	0.188	0.129	0.204
F-stat	0.84	0.00	0.30	0.71	1.64	0.08
p-value of difference between prod. and unprod. interaction terms	0.36	0.98	0.58	0.40	0.20	0.78

Table X
Potential mechanisms: Contracting frictions

This table shows estimates of the impact of incentives to engage in EPS-motivated share repurchases on firm-level productivity and resource allocation at plants outcomes depending on whether the CEO's pay depends explicitly on EPS measures. In Panel A, the unit of observation in each regression is a firm-year. The outcome variable is the firm-level change in total factor productivity. This change is 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). In Panel B, the unit of observation in each regression is a 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. 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 outcome			
Dependent variable: ΔTFP		CEO's pay depends on EPS?	
Firm splits:	Average effect	Yes	No
	[1]	[2]	[3]
Negative Pre-Repurchase EPS Surprise	–0.038*	–0.094***	–0.006
	(0.023)	(0.035)	(0.031)
Year fixed effects	Y	Y	Y
Rounded N	1,100	350	700
R^2	0.025	0.072	0.019
F-stat		2.83	
p-value of difference		0.09	

Panel B: Plant-level outcomes

Contract status:	CEO's pay depends on EPS: Yes			CEO's pay depends on EPS: No		
Dependent variable:	$\Delta Employment$	$\Delta Investment$	*Separation*	$\Delta Employment$	$\Delta Investment$	*Separation*
	[1]	[2]	[3]	[4]	[5]	[6]
$Unproductive_{t-1}$	–0.016	–0.001	–0.019	–0.025*	–0.083*	0.010
	(0.011)	(0.006)	(0.018)	(0.014)	(0.008)	(0.027)
Negative Pre-Repurchase EPS Surprise × $Productive_{t-1}$	–0.030**	–0.014*	–0.026	–0.017	0.005	0.014
	(0.014)	(0.008)	(0.028)	(0.016)	(0.008)	(0.027)
Negative Pre-Repurchase EPS Surprise × $Unproductive_{t-1}$	0.007	–0.017*	0.002	–0.014	–0.005	–0.002
	(0.012)	(0.009)	(0.035)	(0.017)	(0.009)	(0.024)
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	5,600	5,600	6,900	8,200	8,200	10,500
R^2	0.160	0.143	0.450	0.175	0.106	0.405
F-stat	5.67	0.07	0.57	0.04	0.83	0.70
p-value of difference between prod. and unprod. interaction terms	0.02	0.79	0.45	0.85	0.36	0.40

Table XI
Potential mechanisms: Convex adjustment costs

This table shows estimates of the impact of incentives to engage in EPS-motivated share repurchases on firm-level productivity depending on the concentration of investment cuts across a firm's plants. The unit of observation in each regression is a firm-year. The outcome variable is the firm-level change in total factor productivity. This change is 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). We augment the baseline specification with an interaction with *Investment cut HHI*, which is a firm-level measure of "investment-cut concentration" and constructed similar to the Herfindahl-Hirschman index (HHI). For example, if a firm with two plants cuts investment equally among its two plants, the investment-cut HHI equals $(1/2)^2+(1/2)^2 = 1/2$. Columns 2 and 3 perform the analysis while splitting firms into those with low vs. high within-firm standard deviation of TFP, respectively. 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.

Dependent variable: ΔTFP		Within-firm Standard deviation of TFP	
Firm splits:		Low	High
	[1]	[2]	[3]
Negative Pre-Repurchase EPS Surprise	0.003	–0.007	0.014
	(0.031)	(0.032)	(0.078)
Investment cut HHI	0.026	0.022	0.032
	(0.021)	(0.026)	(0.033)
Negative Pre-Repurchase EPS Surprise × *Investment cut HHI*	–0.087**	–0.066*	–0.126**
	(0.033)	(0.039)	(0.064)
Linear control in pre-repurchase EPS surprise	Y	Y	Y
Year fixed effects	Y	Y	Y
Rounded N	3,300	1,700	1,700
R^2	0.015	0.021	0.024

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

November 28, 2023

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
TFP-OP	Natural logarithm of plant-level total factor productivity based on Olley and Pakes (1996)	CMF/ASM
TFP-LP	Natural logarithm of plant-level total factor productivity based on Levinsohn and Petrin (2003)	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
Production Employment	Production employment expenditures (payroll)	CMF/ASM
Non-Production Employment	Non-production employment expenditures (payroll)	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
Computer Investment	Capital expenditures for computers divided by establishment-level capital stock	CMF/ASM
Separation	Indicator variable equaling one if the plant is closed or sold	CMF/ASM
Plant Age	Natural logarithm of the 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/IBES
Employment Cut	Indicator variable equaling one if a firm decreases its number of employees from time $t-1$ to time $t+1$	CMF/ASM
Investment Cut HHI	Firm-level concentration of investment cuts constructed similar to the Herfindahl-Hirschman (HHI) index of concentration	CMF/ASM
Tobin's Q	Total assets plus market value of equity minus common equity and deferred taxes scaled by total assets	Compustat
Profitability	Operating income before depreciation divided by assets.	Compustat

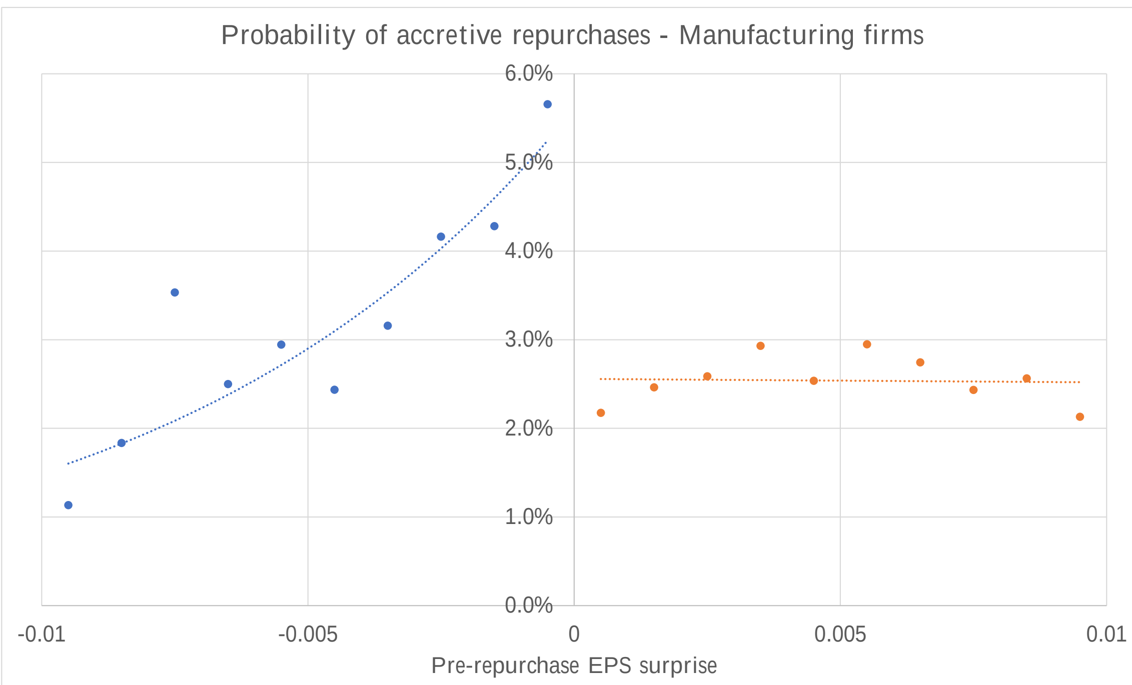
Appendix B: Right-to-work (RTW) laws by state

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.



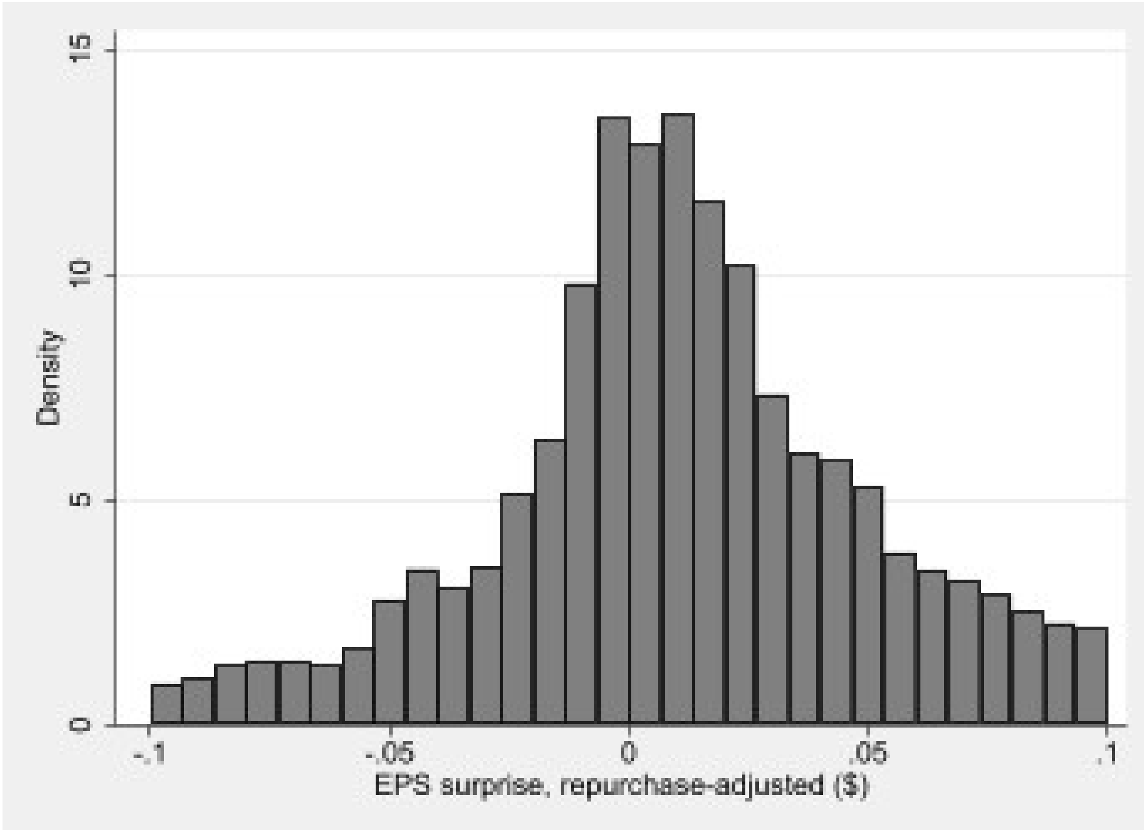


Figure A.2: Histogram of the EPS surprise among manufacturing firms (rounded to nearest cent)

This histogram presents the distribution of the repurchase-adjusted EPS minus the analyst consensus EPS (the consensus is rounded to nearest cent). The sample consists of manufacturing firms (SIC codes 20–39) in Compustat, i.e., not only those firms linked to the Census, but otherwise corresponds to the same sample criteria as in the main analysis.

Table A.1: Concurrent real earnings management

This table shows differences in real earnings management activities around the zero pre-repurchase EPS threshold that could take place concurrently with EPS-motivated buybacks during the focal quarter (t). The sample is limited to fourth quarters, and the data includes all Compustat manufacturing firms (SIC codes 20–39), i.e., not only firms merged with Census data. Real earnings management activities are measured as changes in R&D in Columns (1) and (2), and as changes to selling, general, and administrative expenses (SG&A) in columns (3) and (4). All of these variables are normalized by lagged assets (as of $t = -2$). In columns (2) and (4), we set missing variables to zero. 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, and time (year) fixed effects as indicated. 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.

Dependent variable:	$\Delta R\&D$	$\Delta R\&D$(0 if missing)	$\Delta SG\&A$	$\Delta SG\&A$(0 if missing)
Negative Pre-Repurchase EPS Surprise	-0.0005	0.0014	-0.0005	-0.0004
	(-0.97)	(1.64)	(-0.87)	(-0.68)
Linear control in pre-repurchase EPS surprise	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
R^2	0.017	0.010	0.017	0.018
N	2,217	5,262	5,060	5,262

Table A.2: 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 pre-repurchase EPS surprise, 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. 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). 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

Table A.3: Productivity and IV regressions

This table shows estimates of the impact of incentives to engage in EPS-motivated share repurchases on firm- and plant-level productivity using IV regressions. In Panel A, the unit of observation is a firm-year. In Panel B, the unit of observation is a 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). We instrument the repurchase amount with an indicator of whether the pre-repurchase EPS surprise is negative. 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 as indicated. 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:	ΔTFP
	[1]
Repurchases/Assets (instrumented)	–0.024*
	(0.012)
Linear control in pre-repurchase EPS surprise	Y
Year fixed effects	Y
Rounded N	3,300

Panel B: Plant-level analysis	
Dependent variable:	ΔTFP
	[1]
Repurchases/Assets (instrumented)	–0.023**
	(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

Table A.4: Alternative measurement of key variables

This table considers alternative measurement when estimating the firm- and plant-level impacts of incentives to engage in EPS-motivated share repurchases on resource allocation. The unit of observation in each regression is firm-year (Panel A), or plant-year (Panel B). We examine alternative measures of productivity as outcome variables, including TFP calculated based on Olley and Pakes (1996) and Levinsohn and Petrin (2003), operating margin, labor productivity, and capital productivity. We also examine alternative measures of employment, including 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. 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. 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 alternative measurement

Alternative outcome:	Productivity					Employment	
Dependent variable:	*Δ TFP-OP*	*Δ TFP-LP*	*Δ Operating Margin*	*Δ Labor Prod.*	*Δ Capital Prod.*	*Δ Log(#Emp.)*	*Symm. Emp. Growth*
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Negative Pre-Repurchase EPS Surprise	–0.032***	–0.034***	–0.050**	–0.366**	–0.200***	–0.034**	–0.038**
	(0.012)	(0.013)	(0.020)	(0.154)	(0.062)	(0.017)	(0.018)
Linear control in pre-repurchase EPS surprise	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y
Rounded *N*	3,300	3,300	3,300	3,300	3,300	3,300	3,300
R^2	0.016	0.015	0.036	0.028	0.024	0.050	0.051

Panel B: Plant-level alternative measurement

Alternative outcome:	Productivity					Employment	
Dependent variable:	*Δ TFP-OP*	*Δ TFP-LP*	*Δ Operating Margin*	*Δ Labor Prod.*	*Δ Capital Prod.*	*Δ Log(#Emp.)*	*Symm. Emp. Growth*
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Negative Pre-Repurchase EPS Surprise	–0.020**	–0.021**	–0.015*	–0.263**	–0.108**	–0.016**	–0.027***
	(0.010)	(0.010)	(0.009)	(0.127)	(0.032)	(0.006)	(0.008)
Linear control in pre-repurchase EPS surprise	Y	Y	Y	Y	Y	Y	Y
Plant controls	Y	Y	Y	Y	Y	Y	Y
Industry × year fixed effects	Y	Y	Y	Y	Y	Y	Y
State × year fixed effects	Y	Y	Y	Y	Y	Y	Y
Rounded *N*	35,000	35,000	35,000	35,000	35,000	35,000	35,000
R^2	0.082	0.087	0.097	0.096	0.085	0.108	0.104

Panel C: Plant-level alternative measurement for productivity interaction				
Dependent variable:	Δ*Employment*		Δ*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 prod. and unprod. interaction terms	0.95	0.83	0.71	0.18

Table A.5: Tobin's Q and profitability

This table shows estimates of the impact of incentives to engage in EPS-motivated share repurchases on firm-level Tobin's Q and profitability. The unit of observation is a firm-year. Tobin's Q is defined as total assets plus market value of equity minus common equity and deferred taxes scaled by total assets. Profitability is defined as operating income before depreciation divided by assets. The outcome variable is the change in Tobin's Q and profitability, 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). 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. 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.

Dependent variable:	Δ *Tobin's Q*	Δ *Profitability*
	[1]	[2]
Negative Pre-Repurchase EPS Surprise	–0.090***	–0.025***
	(0.028)	(0.004)
Linear control in pre-repurchase EPS surprise	Y	Y
Year fixed effects	Y	Y
Rounded N	3,300	3,300
R^2	0.055	0.064

Table A.6: Specification and falsification tests

This table conducts various specification checks for the impact of incentives to engage in EPS-motivated share repurchases. In Panel A and Panel C, the unit of observation is a firm-year, whereas in Panel B and Panel D, the unit of observation is a plant-year. Columns 1–3 of Panel A and Panel B consider an alternative bandwidth around the zero pre-repurchase EPS surprise threshold. Columns 4–6 consider a third-order polynomial (instead of linear) control for the pre-repurchase EPS surprise, which we interact with the indicator of a negative pre-repurchase EPS surprise. Panel C and Panel D examine whether there are pre-existing trends in outcome variables. Outcome variables and the pre-repurchase earnings surprise are defined as in previous tables. 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. 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 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: Firm-level 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)
Controls for pre-repurchase EPS surprise	Linear	Linear	Linear	3-degree poly	3-degree poly	3-degree poly
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: Plant-level 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	–0.018*	–0.030***	–0.006*	–0.021*	–0.022***	–0.006*
	(0.010)	(0.008)	(0.003)	(0.012)	(0.007)	(0.003)
Controls for pre-repurchase EPS surprise	Linear	Linear	Linear	3-degree poly	3-degree poly	3-degree poly
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*	22,000	22,000	22,000	35,000	35,000	35,000
R^2	0.105	0.127	0.083	0.085	0.111	0.064

Panel C: 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:	Δ*TFP*	Δ*Employment*	Δ*Investment*	Δ*TFP*	Δ*Employment*	Δ*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 D: No pre-existing plant-level differences in key outcome variables						
Differences in outcomes in:	Changes ($t-3$ to $t-2$)			Changes ($t-2$ to $t-1$)		
Dependent variable:	Δ*TFP*	Δ*Employment*	Δ*Investment*	Δ*TFP*	Δ*Employment*	Δ*Investment*
	[1]	[2]	[3]	[4]	[5]	[6]
Negative Pre-Repurchase EPS Surprise	0.003	–0.002	0.002	–0.002	–0.007	–0.002
	(0.005)	(0.003)	(0.003)	(0.005)	(0.030)	(0.003)
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	0.060	0.086	0.052	0.069	0.088	0.052

Table A.7: Productivity around placebo EPS surprise thresholds

This table shows estimates of the impact of incentives to engage in EPS-motivated share repurchases on firm- and plant-level productivity using falsely assumed EPS surprise thresholds. In Panel A and Panel B, the unit of observation is a firm-year and a plant-year, respectively. In both panels, in column 1 and column 2, observations within the intervals of [–0.009, –0.003] and [0.003, 0.009] are used, respectively. 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). 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 as indicated. 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		
Window:	[–0.009,–0.003]	[0.003, 0.009]
Dependent variable:	ΔTFP	ΔTFP
	[1]	[2]
Negative Pre-Repurchase EPS Surprise	0.572	–0.011
	(0.425)	(0.233)
Linear control in pre-repurchase EPS surprise	Y	Y
Year fixed effects	Y	Y
Rounded N	300	650
R^2	0.056	0.057

Panel B: Plant-level analysis		
Window:	[–0.009,–0.003]	[0.003, 0.009]
Dependent variable:	ΔTFP	ΔTFP
	[1]	[2]
Negative Pre-Repurchase EPS Surprise	0.494	0.015
	(0.308)	(0.137)
Linear control in pre-repurchase EPS surprise	Y	Y
Plant controls	Y	Y
Industry × year fixed effects	Y	Y
State × year fixed effects	Y	Y
Rounded N	1,800	4,200
R^2	0.409	0.338