# Common Ownership, Incentives & Productivity

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**Abstract**

We collect uniquely comprehensive ownership data for the universe of U.S. public firms and examine whether ownership helps explain productivity. We find that common ownership of product market rivals is negatively associated with total factor productivity. Using index additions of competitors as shocks to common ownership, we find evidence of an indirect causal effect: common ownership affects productivity through a reduction in performance-sensitive managerial compensation, which in turn causes lower productivity.

Keywords: corporate governance, common ownership, incentives, total factor productivity

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# 1 Introduction

Investors who own shares in multiple firms competing in the same industry have been termed "common owners". Common ownership has been on the rise, among other things due to consolidation in asset management, the centralization of votes across funds within investment advisers, and deliberate decisions by active shareholders to concentrate their portfolio in product market rivals (Amel-Zadeh et al., 2022). The level of common ownership is now so large that it cannot be ignored for purposes of studying firm behavior: the largest three asset managers control about a quarter of votes of S&P500 single-class firms, and an even greater fraction of votes actually voted. In theory, such common ownership can soften incentives to compete, either because companies "looking out for their investors" maximize their most influential owners' aggregate portfolio returns (Rotemberg, 1984) or because common owners have reduced incentives to manage agency problems (Anton et al., 2022), thus enabling managers to lead a "quiet life" (Hicks, 1935).

Both theories predict higher product prices due to common ownership – but for different reasons, and the two theories make different predictions for common ownership's effects on productivity. The former theory assumes that ownership does not affect costs and predicts that less vigorous competition leads to increased profit margins; the latter theory predicts that common ownership exacerbates agency problems, thus reduces productivity, and *thus* raises product prices. Contrasting that latter prediction, other theories allow for the possibility that common ownership incentivitizes cost-reducing innovation (Lopez and Vives, 2019; Anton et al., 2021), leading to the opposite prediction for productivity. Contrasting the wealth of extant theoretical literature on the relationship between common ownership and productivity, empiricists have studied effects of common ownership on product prices, R&D investments, and patenting, but not on productivity.

We empirically investigate whether and how common ownership affects firm-level productivity. This appears an even more important question to address than whether common ownership affects product prices. Common ownership in a model in the tradition of Rotemberg (1984) does not affect the supply curve. Welfare effects are thus limited to the usual deadweight loss. The welfare effect of reduced competition due to an inward shift in the supply curve as in Anton et al. (2022), by contrast, can be orders of magnitude greater. For that

reason alone, distinguishing between these theories' predictions and studying productivity effects of common ownership appears of first-order importance.

A second reason why distinguishing between the theories is important is because the results may give cues as to the optimal policy response. Policy proposals that would weaken institutional investors' ability to shape firm objectives may *strengthen* competition in Rotemberg-style models. However, the same policies might *weaken* competition further in cases in which mismanaged agency conflicts and reduced productivity are at the core of the problem.

To answer the question whether common ownership affects productivity, we first construct a uniquely comprehensive data set of ownership and control structures of all public U.S. firms, which we make freely available to researchers with the present publication. In contrast to thus far commonly used data sources, this data set includes not only institutional investors' holdings, but also those of large individual shareholders and corporate insiders, which are often the most influential or even controlling shareholders of U.S. public firms. Expanding on our previous work (Amel-Zadeh et al., 2022), this data set includes all U.S. publicly traded firms and accounts for differential voting and cash flow rights in multi-class stock corporations. We calculate the extent to which the most influential shareholders in one firm hold financial stakes in product market rivals, and relate this measure of common ownership to measures of managerial incentives taken from Edmans and Gabaix (2016) and estimates of total factor productivity derived from balance sheet data as per De Loecker and Syverson (2021).

We first establish a link between common ownership and productivity in panel regressions. To address potential reverse causality and the endogenous relationship of common ownership and productivity due to unobserved omitted variables, we include year-by-industry fixed effects. Our results remain robust to various alternative specifications and to using alternative measures of common ownership and productivity.

To test whether the panel correlations likely have a causal interpretation, we next limit the identifying variation we use to variation in ownership caused by index additions of competitors (Boller and Scott-Morton, 2020; Anton et al., 2022). Index additions do not directly affect the ownership of incumbents, but introduce variation in the extent to which index incumbents' shareholders have financial interests in rivals. To test for a direct effect of

common ownership on productivity, we measure whether these shocks to common ownership alter treated firms' productivity more than untreated firms'. To test for an indirect effect, we use shocks to common ownership from index additions to instrument the proportionate change in managerial incentives, and estimate how the instrumented change in incentives affects productivity.

Our panel results indicate that common ownership is correlated with reduced productivity. Controlling for managerial incentives marginally reduces the size and significance of these results. This finding is consistent with the idea that at least part of any effect of common ownership on productivity might be indirect, via the governance channel. Quantitatively, an increase in common ownership by one standard deviation, corresponding to an 15% higher interest in competitor profits, decreases total factor productivity (TFP) by about 1%. The effect is concentrated among firms in the manufacturing and services industries, in the last third of our sample time (2014-2019) and among medium-sized firms with limited insider ownership and higher wealth performance sensitivity.

The addition of a firm to the S&P 500 index increases common ownership between the added firm and index incumbents in the same industry, while the common ownership of the index incumbent firms in other industries is mostly unaffected. Ongoing portfolio rebalancing is likely to mute the overall increase in common ownership, especially over a longer time-horizon, but we find a significant treatment effect on index incumbent competitors. Controlling for a multitude of firm and ownership attributes, the index-driven increase in common ownership is related to productivity with a negative point estimate of about 1.2%, although insignificantly so. One possible interpretation of this finding is that the effects are heterogeneous over time or across industries, leading to limited power when estimating the average treatment effect. Another possible interpretation is that firms that are added to the index improve their efficiency or reduce their cost of capital, and this increases competitive pressure causing a marginal increase in the productivity of product market rivals. This positive effect may partially cancel out a concurrent direct negative effect of common ownership on productivity.

Based on existing empirical findings, we assume the confounding effect of more competitive index additions is insignificant (Afego, 2017). To address the possibility of heterogeneous treatment effects and weak statistical power, we use the change in managerial incentives that

is caused by index additions of competitors as a test of a specific mechanism linking common ownership to productivity. The first stage difference-in-difference analysis produces a highly significant 16% reduction in managerial incentives due to the index addition, in line with prior literature (Anton et al., 2022). Regressing productivity on the instrumented variation in managerial incentives, we find that the local average treatment effect on productivity is positive and significant. We thus conclude that reduced managerial incentives due to a positive shock to common ownership lead to a reduction in annual productivity of about 2.1% for the four years post-treatment. Concurrently, via the same mechanism, we find that markups decrease as common ownership increases. These findings point towards a theory in which common ownership exacerbates agency problems, rather than that it changes firm objectives. Namely, common ownership reduces the performance-sensitivity of managerial compensation, and subsequently leads to lower corporate productivity.

We contribute to the literature on the effects of common ownership on corporate outcomes by studying the perhaps most important potential outcome variable, corporate productivity, as well as the channel by which common ownership may have an impact on it. Most closely related to our work is Anton et al. (2022), who develop a theoretical mechanism of how common ownership can affect productivity via managerial incentives. Their empirical exercise is focused on the relation between common ownership and managerial incentives, leaving the effect on productivity untested. Our analysis fills this gap and provides empirical evidence for the causal impact of common ownership on productivity through its reduction of managerial incentives. Additionally, we show that the causal effect of common ownership on managerial incentives holds when accounting for corporate insider ownership and dual class stock structures, which were missing in the Anton et al. data.

To the best of our knowledge, this paper is the first to study the relationship between common ownership and productivity for a broad cross-section of U.S. firms. Bas et al. (2023) study common ownership and productivity but merely their "correlations [...] across countries". They do not test for a particular mechanism or causal link. For the U.S., but limited to a much smaller sample of S&P 500 firms only, Khoo (2024) investigates the relationship between common ownership and markups using similar methods to ours. Different from our average findings, Khoo finds a positive panel association between common ownership and productivity. The underlying data sample gives rise to this contrast: whereas

we look at firms of all sizes, Khoo's data set is limited to only the largest firms in our sample. For these firms we also find a slightly positive association between common ownership and markups, harmonizing the results.

The prior empirical literature has studied the relationship between common ownership and competition-related outcomes, such as prices, innovation and R&D investments (Aslan, 2019; Anton et al., 2021; Lopez and Vives, 2019), and between common ownership and financial returns (Boller and Scott-Morton, 2020; Lewellen and Lowry, 2021), either for a broad cross-section of firms or a single industry (Backus et al., 2021a; Azar et al., 2018). A recent review of these papers is provided by Schmalz (2021). We add a novel dimension to the older literature on the relationship between corporate ownership attributes, incentives and productivity. Existing work studies the effect of managerial, institutional and family ownership on incentives and productivity (Hill and Snell, 1989; Hartzell and Starks, 2003; Amit and Villalonga, 2020). In response to the well-established concerns about the endogeneity of ownership and productivity (Demsetz and Lehn, 1985), we adopt a methodology developed by Boller and Scott-Morton (2020) that provides reasonably exogenous variation in common ownership, and improve on it by applying the staggered treatment techniques from the econometrics literature (Baker et al., 2022).

We also contribute to the applied-microeconomics literature investigating the reasons for the cross-sectional and time-series variation in firm-level productivity. For a comprehensive summary of previously analyzed drivers of productivity dispersion we refer the reader to De Loecker and Syverson (2021). Closely related to Backus (2020), who argues that competition is a key driver of varying productivity, our findings suggest that differences in common ownership cause productivity dispersion mainly via managerial incentives. The link between managerial incentives and productivity was first proposed by Hicks (1935) and empirically investigated by Bertrand and Mullainathan (2003). The difference between Bertrand and Mullainathan (2003) and our work is the cause of the variation in managerial incentives.

We do not directly address the question whether changes in ownership of U.S. corporations may help explain the declining trend in U.S. productivity (Sprague, 2021); our focus is instead on understanding the cross-sectional variation (Syverson, 2011).

Finally, we contribute to the literature and academic profession by making the underlying comprehensive data set of corporate ownership structures for *all* publicly listed U.S. firms,

including multi-class firms, freely available to other researchers. We thereby hope to make research into corporate governance and shareholdings more accessible to researchers globally, and to improve the robustness of findings by providing a comprehensive and accurate ownership database.

# 2 Data & variable construction

The analysis in this paper is based on an expanded novel data set of the complete institutional and insider ownership structure of U.S. publicly traded firms. Below we describe the data construction at a cursory level. For a detailed account of the cleaning, consolidation and correction steps we refer the reader to our online appendix and Amel-Zadeh et al. (2022), where we describe the construction of a small subset of this ownership data comprising only S&P 500 single-class firms. The total factor productivity and markup estimates are derived from Compustat balance sheet data using the standard procedures employed in the applied microeconomics literature (De Loecker and Syverson, 2021). We focus our analysis on the estimates based on the cost shares approach. Methodological discussions and findings for total factor productivity estimates based on the proxy (Ackerberg et al., 2015) and dynamic panel (Blundell and Bond, 2000) production function approach are provided in the robustness section and in the online appendix. Finally we follow Anton et al. (2022) and use estimates for CEO wealth-performance sensitivity derived and provided by Edmans and Gabaix (2016) as proxy for managerial incentives.

## 2.1 Ownership data and measures

The ownership data underlying this project is unique in its comprehensiveness, accuracy and time-coverage. With an extensive, automated routine coded in python we scrape, parse and consolidate all available ownership reports filed with the Securities Exchange Commission (SEC) between 1999 and 2020. This combines the holdings of asset managers, blockholders and corporate insiders in one, accessible database for the first time. Different from our previous work (Amel-Zadeh et al., 2022), we do not limit the analysis to single-class, S&P 500 constituent firms. Instead, we include all firms with significant ownership and balance sheet data, with single and multiple class stock structures, between 2000 and 2020. Since we

are interested in both cash flow and control rights, we adjust the voting rights of shareholders in firms with multiple classes of common stock to reflect the actual number of votes held.

The types of ownership filings consolidated in our data set are 13-F reports of institutional asset managers, SC 13-D and SC 13-G reports submitted by blockholders with or without an intention to engage a given firm, and Form 3/4/5 reports by corporate insiders, such as C-Suite executives, board members or other employees with stock holdings in their own firms. We provide more detail on the filing requirements and formats in Amel-Zadeh et al. (2022). From each filing type we parse all relevant information to identify securities, holding sizes and owners. The filings differ substantially in the information contained and their formatting, thus we apply a filing-specific pipeline to the 13-F, the SC 13-D/G, and the Form 3/4/5 reports, respectively, in order to clean and capture the reported ownership. Each pipeline is extensively spot-checked, and captures more comprehensive and cleaner ownership data across a wider range of firms than the commonly used, publicly available ownership data until now (Backus et al., 2021b; Hadlock and Schwartz-Ziv, 2019).

The resulting three data sets for institutional ownership, blockholder ownership and insider ownership provide information on the shares and votes held at the security-owner-quarter level with gaps. Blockholder and insider filings are required only when stock holdings change, implying that in quarters without trades we do not observe an ownership report. Before merging the data of the three investor types, we interpolate the holdings of blockholders and insiders to have consistent quarterly filings. This step relies on the assumption that investors file according to the requirements whenever they change their holdings and thus stakes do not change between two reports. An additional complication arises where blockholders or insiders file non-zero stock holdings in their final ownership report. The filing requirements imply that such investors have become silent holders of stock who continue to hold the same number of shares indefinitely. However, this may not be true in the long run as investors die or transfer their holdings without a proper filing notice. To account for silent blockholders but limit indefinite ownership, we extrapolate final non-zero holdings of insiders and blockholders for twelve quarters.

Within each pipeline we separate firms with a single class of common stock outstanding from those with multiple classes of common stock, in order to attribute the correct number of votes to each share holding. For that purpose, a separate data set on publicly traded firms

with multiple classes of common stock, their respective voting rights and quarterly number of shares outstanding of less commonly traded classes is compiled manually, extending and expanding on existing data by Gao et al. (2014) and Gompers et al. (2010). We merge this security level information on votes and shares outstanding on the holdings data based on unique security identifiers (see Appendix).

Combining the ownership of different investor types can create a substantial amount of duplication at the security-owner group-quarter level as we observe different filing types reporting the same number of shares owned in the same firm and in the same quarter by the same underlying investor, but owner names or identifiers may differ. This can occur because the institution holding the shares reports them and the controlling shareholder of the institution (for example, Warren Buffett for Berkshire Hathaway or Carl Icahn for Icahn Enterprises) also reports this ownership. Duplication also arises across Form 4 and SC 13-D/G filings, for example if a family member sits on the corporation's board and files an insider report, while the family's trust holding all the shares reports the same shares among others (as is the case for Walton family members and the Walton Family Trust in Walmart, for example). We consolidate the holdings of such investor groups first using a manually compiled list of "investor groups", where the shares and votes owned are likely under the control of one individual or where we assume that the group will act as one entity in most voting choices. For example the different BlackRock entities filing separate 13-F reports are all aggregate to one BlackRock Family. Analogously, the different ownership stakes of the Walton siblings are mainly held in the Family Trust, such that we keep the Trust as the main owner but do not add the individually reported stakes to it. For controlling owners of asset managers, such as Warren Buffett for Berkshire Hathaway, we assume that the individual is in control of how the owned votes are cast and therefore attribute the ownership to the individual. In a second step to removing duplicate ownership we drop exact matches in share holdings of a certain size at the security-quarter level. Spot-checking some of these suggests we capture a non-negligible amount of duplicate filings not yet accounted for in our "investor group" data.

After cleaning and consolidating the complete data to a unique security-owner-quarter level, we merge it with CRSP-provided information on shares outstanding for publicly traded stocks and manually collected shares outstanding for infrequently traded multiple stocks

firms. We adjust interpolations for stock splits and calculate the fraction of ownership and voting rights held at the security-owner-quarter level. Finally, the stock and vote ownership has to be summed across different stock classes to create a unique firm-owner-quarter data set that can be merged with firm-level information on incentives and productivity.

For our analyses, we focus our attention on companies for which the reported share holdings account for more than 30% of the outstanding shares, and do not exceed 150% of outstanding stock. Both restrictions seem necessary to ascertain that we remove firms with erroneous ownership data from our analysis and that we focus on firms for which we can identify a meaningful subset of owners. Aggregate reported ownership in excess of 100% of shares outstanding is a common feature even in the best commercial ownership databases, such as Capital IQ and Factset. It is driven by the extensive share lending of large asset managers and hedge funds. Where we capture less than 30% of aggregate ownership, this may be due to a limited float of common stock or widely dispersed ownership, especially prevalent in very small firms.

The resulting data set contains quarterly holdings of all investors required to report their investments to the SEC for most publicly traded companies from 2000-Q4 until 2019-Q4. The sample consists of more than 6000 firms in total, with a declining number of unique firms per quarter, starting at more than 4000 and ending closer to 2500. The total number of investors has been roughly constant at about 120,000 since 2004, when a change in SEC regulation began mandating corporate insider reports. Before 2004 insiders of significant size were required to report as blockholders, hence the more meaningful subset of all insiders is also contained in our earlier sample. Over time, the number of blockholders and corporate insiders (i.e. filers of SC 13-D, SC 13-G or Form 3/4/5 reports) has been declining gradually, while the number of institutional investment managers required to file 13-F reports has been increasing steadily.

Common ownership interests reflected in our ownership data are primarily measured by the so called "profit weights" following the literature beginning with Rotemberg (1984) and Backus et al. (2021b) and since then widely used in the economics literature on the topic. The key underlying theory is based on a firm's objective to maximize shareholder value, taking into account the portfolio interests of its diversified and undiversified shareholders. Backus et al. (2021b) clarify the assumptions under which the objective function $Q_f$ of firm

$f$ as a function of its shareholders' portfolios $x$ can be re-written as the sum of own firm profits and weighted competitors' profits. The weight on competitor $g$'s profits is referred to as profit weight, and corresponds to the sum of vote-weighted cash flow interests in firm $g$ relative to the sum of vote-weighted cash flow interests in firm $f$ across all shareholders of firm $f$. This profit weight is also referred to as "Edgeworth coefficient of effective sympathy" or "$\kappa_{fg}$" and is frequently used in the applied industrial organization and common ownership literature (Azar and Vives, 2019; Anton et al., 2022).

$$Q_{ft}(x_f, x_{-f}) \approx \pi_{ft} + \sum_{g \neq f} \underbrace{\left( \frac{\sum_i \gamma_{ift} \beta_{igt}}{\sum_i \gamma_{ift} \beta_{ift}} \right)}_{\kappa_{fgt}} \pi_{gt} \tag{1}$$

In the above equation $\beta_{if}$ and $\gamma_{if}$ are the fraction of shares of firm $f$ and the fraction of votes of firm $f$ owned by shareholder $i$, respectively. In our analysis the fraction of votes is the number of votes held according to the number and class of shares owned, divided by the total number of votes outstanding for the firm. For all single class firms we assume the one share-one vote proportional control structure, implying $\beta_{if} = \gamma_{if}$.

The calculated common ownership profit weights are unique and directed at the firm-pair level. So for each period $t$, firm $f$ has a $\kappa_{fg}$ for its competitor $g$ and a different $\kappa_{fh}$ for its competitor $h$, whilst firms $g$ and $h$ again have different profit weights for each other and firm $f$. To investigate the relationship between common ownership and productivity at the firm-year level we measure for each firm the equally-weighted $\kappa_f$ on all its competitors, corresponding to the average weight investors in firm $f$ attribute to competitors' profits relative to firm $f$'s profits. Competitors in our main specification are firms in the same historical NAICS industry at the 3-digit level[1]. This level of granularity is chosen to correspond to the level of production function estimates. Alternative weightings of profit weights based on sales shares, as well as alternative competitor classifications based on current NAICS-3 industries and historical SIC-3 industries are considered in our robustness analysis.

[1]We are aware that these broad industry classifications are no perfect measure of horizontal competition. However, they are persistently available for all sample firms, bi-directional and firms in the same NAICS-3 industry will compete at least in some product markets, fulfilling conditions for common ownership incentives to affect firm behavior. Our goal is to contribute novel evidence of a link between common ownership and productivity to a literature that chiefly relies on NAICS and SIC industry classifications. To distinguish this contribution from findings based on alternative market definitions we follow the existing literature for now and look forward to future research to identify competitors more robustly

While profit weights are the key measure of common ownership used in this paper. We also employ two alternative measures, to ascertain the robustness of our results and to simplify their interpretation. The first alternative measure is the cosine similarity of the vectors describing cash flow ownership in firms $f$ and $g$. Higher similarity of ownership structures, due to the same investor base and comparable invested stake sizes, corresponds to a smaller angle between the two ownership vectors and the cosine value of the angle nearing 1. Less similarity of ownership structures, in the extreme case two completely different owners holding 100% of one firm each, increases the angle between ownership vectors and reduces the cosine value to 0. Analogous to the profit weight measure, each firm pair has a unique cosine similarity value. To measure the average similarity between a firm's ownership structure and its competitors' ownership structures we average across the cosine similarity values with all competitors.

$$cosinesim_{ft} = \sum_{g \in C} \frac{1}{C} cos(\beta_{ft}, \beta_{gt}) \tag{2}$$

Backus et al. show that under the assumption that cash flow and voting rights are the same ($\beta = \gamma$), profit weights can be decomposed into this cosine similarity measure and a measure of relative investor concentration. In addition to being more-readily interpretable than profit weights, cosine similarity of ownership provides an insight into whether similarity in cash flow interests is related to productivity outcomes, regardless of investor concentration. We do not investigate how productivity effects may be differently related to ownership similarity and relative investor concentration trends, as the decomposition does not work for our sample including multi-class firms ($\beta \neq \gamma$). However, given the slight difference between findings based on profit weights and cosine similarity, we note space for future research that aims to understand the relevance of investor concentration vis-a-vis investor overlap.

$$\kappa_{fgt} = \underbrace{cos(\beta_{ft}, \beta_{gt})}_{\text{cosine similarity}} * \frac{\sum_i \beta_{igt}^2}{\sum_i \beta_{ift}^2} \tag{3}$$

A third measure of common ownership we employ is the aggregate voting fraction held by "common owners" ($aggCO_f$). "Common owners" are defined as investors in a firm, who simultaneously hold shares of a competitor in the same NAICS-3 industry. This measure

is most easily interpreted, but it does not account for the influence or common ownership incentives of individual common owners.

$$aggCO_{ft} = \sum_{i_{co} \in i} \gamma_{ift} \tag{4}$$

## 2.2 Productivity estimates

In this paper we consider Hicks-neutral total factor productivity residuals as the key measure of corporate productivity. Total factor productivity, henceforth TFP or $\Omega$, measures the unexplained variation in output once the amount of inputs (materials $M$, overhead $A$ and capital $K$) are combined using the firm-specific technology or production function $f(\cdot)$. The following paragraphs describe our measures of inputs and outputs, and how we estimate the production function parameters.

$$Output_{ft} = \Omega_{ft} * f(M, A, K) \quad \Rightarrow \quad \omega_{ft} = output_{ft} - f(m, a, k) \tag{5}$$

For our estimation we use publicly available balance sheet data from Compustat for the universe of U.S. firms between 1995 and 2020. Data on sales revenue, cost of goods sold, property plant & equipment, administrative expenses and labor expenditures is cleaned as in De Loecker et al. (2020), removing missing values, trimming outliers at the 1st and 99th percentile, and deflating the values using average annual GDP deflator data provided by FRED. To allow for variation in technology (TFP) when firms merge, acquire or divest a substantial amount of assets, we artificially split the firm into two entities before and after the acquisition/divestiture event. This only affects TFP estimates based on the dynamic panel and proxy approach. Finally, we drop firms that change historical NAICS-3 sectors more than four times in our sample period, firms that have negative net sales (sales minus cost of goods sold minus administrative expenses) for more than 50% of their existence in our data set, and remove financial firms as well as public utility firms due to fundamental differences in their production functions, accounting practices, and regulatory constraints.

We assume output and revenue are generated with a Cobb-Douglas production function using three inputs: materials $m$, overhead $a$ and capital $k$. The equation below describes the log-linearized Cobb-Douglas production with three inputs, where each input has a firm-

and year-specific output elasticity $\alpha$.

$$\begin{aligned} output_{ft} &= \omega_{ft} + \alpha^m_{ft} m_{ft} + \alpha^a_{ft} a_{ft} + \alpha^k_{ft} k_{ft} + \epsilon_{ft} \\ \omega_{ft} &= output_{ft} - \alpha^m_{ft} m_{ft} - \alpha^a_{ft} a_{ft} - \alpha^k_{ft} k_{ft} - \epsilon_{ft} \end{aligned} \tag{6}$$

Since we do not have access the input quantity data from the U.S. census, we approximate these quantities with input costs from balance sheet data, using cost of goods sold for materials, sales and administrative expenses for overhead and imputed capital expenses for capital[2].

The unit of our analysis is the producer level, disregarding multi-product firms, treating all firms of an industry as if they were producing identical, homogeneous goods.

To estimate the TFP residual of this production function we follow the empirical industrial organization literature, and approximate the inputs' output elasticities $\alpha$ using the factor cost shares approach. Alternative approaches, namely the proxy approach (Ackerberg et al. (2015) and the dynamic panel approach Blundell and Bond (2000) require estimating the production function using an inverted law of motion for a persistent input or a pre-defined dynamic process for productivity and the use of lagged first-differences of inputs as instruments. The former approach has been shown by Bond et al. (2021) to lack identification in settings with limited input variation. Since we are unable to differentiate between input and input cost variation we cannot ascertain if we are subject to this critique. The latter is requires an extensive number of lagged and first-differenced observations for each industry and input, leading to a significant reduction in the remaining variation across producers and very noisy estimates. Since the incomplete Compustat balance sheet data and limited managerial incentives data already curtail our sample, we are weary that regression analysis based on these estimates will not be able to capture any meaningful relationships due to small sample size and excessive noise. We do provide regression results of productivity estimates based on these alternative approaches in the appendix, but choose the factor shares approach as our main specification.

Under this approach the factor elasticities of the variable inputs are approximated by their respective total cost share. This approach is generally valid for both perfectly competitive

[2]Capital expenditures are inferred using net property plant and equipment and an estimate of the user cost of capital based on short-run interest rates, inflation and depreciation, following De Loecker et al. (2020)

and oligopolistic market structures, with homogeneous and differentiated products, whilst cost shares can be computed and we assume the production function is the same across all products (De Loecker and Syverson, 2021). The inputs' cost shares are informative about their respective output elasticities conditional on assuming constant returns to scale, no adjustment costs and each producer statically minimizing his costs each period. Constant returns to scale are a ubiquitous assumption in productivity estimation, and we are not aware of any analysis that credibly refutes this assumption. Inputs are possibly not perfectly flexible across all firms and industries in our sample, however, on average we believe that firms can alter input costs to a certain degree in response to shocks. A method to address variable adjustment costs averages cost shares over time or producers, to remove idiosyncratic adjustment-cost-driven misalignments and accept that this average will not always equal the true elasticity (De Loecker and Syverson, 2021). We repeat our analysis using productivity estimates based on the median cost shares across all firms within an industry and year in the robustness section. However, the smooth cost shares also impose a common technology across all firms and reduce the remaining variation in the productivity estimates, limiting the power of our tests and imposing more structure on the highly differentiated (likely multi-product) firms in our sample than we want to. Another reason to use the idiosyncratic cost shares is that markups estimated as a secondary outcome variable (discussed below) would be reduced to the ratio of cost of goods sold to sales, a mere margin measure, and therefore significantly less meaningful for our analysis.

A more detailed discussion of the assumptions and applicability of the factor shares, proxy and dynamic panel approaches is provided in De Loecker and Syverson (2021).

$$\hat{\alpha}^m_{ft} = \frac{COGS_{ft}}{TotalCost_{ft}} \qquad \hat{\alpha}^a_{ft} = \frac{XSGA_{ft}}{TotalCost_{ft}} \qquad \hat{\alpha}^k_{ft} = 1 - \hat{\alpha}^m_{ft} - \hat{\alpha}^a_{ft}$$
$$TotalCost_{ft} = COGS_{ft} + XSGA_{ft} + usercost * PPENT_{ft} \tag{7}$$

Finally, we compute TFP estimates as the difference between sales revenues and the hypothetical product value based on input costs and estimated factor shares.

$$\widetilde{\omega}_{ft} = revenue_{ft} - \hat{\alpha}^m_{ft} cogs_{ft} - \hat{\alpha}^a_{ft} xsga_{ft} - \hat{\alpha}^k_{ft} kexp_{ft} - \epsilon_{ft} \tag{8}$$

A significant caveat to whichever estimation approach we use, is that all estimates rely

on input cost and sales revenue data from Compustat, instead of using input and output quantities. The resulting TFP residual therefore does not measure productivity exactly.

$$\omega_{ft} = output_{ft} - f(m, a, k) \quad \neq \quad revenue_{ft} - f(cogs, xsga, kexp) = \widetilde{\omega}_{ft} \tag{9}$$

Instead $\widetilde{\omega}$ ("revenue productivity" henceforth) conflates quantities with prices, implying that estimating a negative effect of common ownership on revenue productivity, could arise because of a negative effect on prices (markups) or on actual productivity $\omega$. Lacking a firm-specific price deflator, price variation due to quality differences or market power is incorporated in our TFP residual.

$$\begin{aligned} revenue_{ft} - f(cogs, xsga, kexp) &= (p_{ft} + output_{ft}) - f(p^m m + m, p^a + a, p^k + k) \\ \widetilde{\omega}_{ft} &\approx prices_{ft} + \omega_{ft} \end{aligned} \tag{10}$$

Given our data availability, we cannot address this concern to estimate actual productivity separately from revenue productivity. However, we can estimate markups, $\mu$, and separately investigate how common ownership affects revenue productivity and markups. Following De Loecker et al., markups are estimated as the product of a variable input's factor elasticity, which we proxy with cost shares, and the inverse sales ratio of that input. The underlying assumptions to approximate the factor elasticity are the same as above (for more detail see De Loecker and Syverson (2021)).

$$\mu_{ft} = \hat{\alpha}^m_{ft} * \frac{sales_{ft}}{m_{ft}} \tag{11}$$

Knowing how both are affected, allows us to disentangle the directional effects on actual productivity and to impute bounds on how large the effect could be. Based on Equation 10, concurrently finding a negative relationship of common ownership with revenue productivity and a positive or insignificant relationship with markups, would imply a negative relationship between common ownership and actual productivity. On the other hand, if markups are also negatively correlated with common ownership, it becomes a question of magnitudes. A larger negative correlation with revenue productivity would also imply a negative relationship with actual productivity. We will use these interrelations to provide an idea of magnitudes in the analysis section.

A remaining limitation of our TFP estimates is that we do not account for selection bias introduced by firm survival over time. Lacking data on private firms we are not able to control for the selection bias inherent in this estimation and leave this adjustment to future research which has access to more comprehensive firm data.

## 2.3 Sample Description

The key variables in our final data sample are firm-specific productivity estimates, henceforth TFP, and measures of common ownership at the firm-year frequency. Common ownership measures are based on ownership in the fourth quarter of every year. To test the mechanism via managerial incentives, we approximate the performance-sensitivity of managerial compensation with wealth-performance sensitivity estimates from Edmans and Gabaix, henceforth WPS. Merging the three different data sets implies a substantial loss of data, as productivity estimates are not available for all firm-years for which we have ownership data, and the wealth-performance sensitivity estimates are available for even fewer firm-years. The resulting sample covers a total of 3,700 firms with an average of 1,600 firms per quarter, yielding 34,100 firm-year observations. All key variables have been extensively cleaned during the estimation process (see above paragraphs), hence we do not further curtail the sample at this point.

We add controls for ownership structure attributes and firm characteristics to this data sample. Categorizing shareholders into groups for corporate insiders, activists (as classified by Brav et al. (2008)), engaged activists who currently have a campaign ongoing in a given firm (based on activist, target firm and campaign start dates compiled by Brav et al. (2008)), the Big Three asset managers (Vanguard, BlackRock & StateStreet), undiversified or universal investors (owning >95% of all firms in a given year), we include the aggregate fraction of votes held by each group and a dummy for firms which have a controlling owner (>50% of votes held by one investor) or have a dual (multi) class stock structure as firm-level controls. The rationale is that the existence or non-existence of influential insiders or activists may influence corporate productivity and will not be fully captured by our common ownership measure. As our ownership data does not provide information on shareholdings of so called retail investors, with small portfolios or small investments, and the fraction of shares held by retail investors varies across firms, we also account for the overall fraction

of share and vote vote ownership captured. Finally, we are mindful of varying degrees of investor concentration across firms and include a variable called "Investor HHI", reflecting the Herfindahl-Hirschmann Index of individual investors' fractional shareholdings. In terms of firm characteristics we control for market capitalization, market share by market capitalization (at the same industry level used to calculate profit weights) and firm age (years since first listing). Summary statistics of all key variables are provided in Table 1.

# 3 Analysis & Results

The key question to be answered by the following analysis is whether common ownership affects firm productivity. This question is central to an ongoing debate among academics and policy-makers whether common owners have anti-competitive effects. Theory predicts common owners stand to benefit if commonly owned competitors soften competition and reap higher profits, and therefore have an incentive to govern in a way that transfers these incentives to managers. Anton et al. (2022) provide a mechanism how common owners are able to affect softer competition by setting less performance-sensitive incentives for managers and thereby lowering productivity. Based on similar data and a comparable sample to ours, the authors find empirical evidence for a broad-based negative effect of common ownership on performance-sensitive compensation. However, the study does not further investigate the follow-on effect on corporate productivity and competition.

The prior literature also analyses industry-specific links between common ownership, competition and productivity. However, findings vary across industries. Using our broad sample with data for most publicly traded firms in the U.S., we are able to understand the relationship between common ownership, incentives and productivity estimates for a larger cross-section of the economy than previous studies.

Figure 1 and especially the cross-sectional patterns in our data provide a first glance at the relationship of interest. Scattering productivity estimates on common ownership profit weights for all firm-year observations, and estimating the linear effect of common ownership on productivity by common ownership quintile, we observe an interesting non-linear relationship. For the observations with low levels of common ownership the estimated effect seems negative, while it turns positive for observations with high levels of common

owners. In the following we will test if this non-linear relationship remains significant when controlling for firm-, industry-, and time attributes.

A pairwise correlation analysis with results in Table 2 substantiates the graphical trend. Across the whole data set and without controls, the correlation between common ownership and productivity is positive and statistically significant, although economically small. To remove confounding effects from this raw correlation coefficient and to establish a causal link, we first employ a saturated panel analysis, regressing productivity on measures of common ownership. As the panel regressions are still subject to concerns about reverse causality, we follow Boller and Scott-Morton (2020) and use S&P 500 index additions as an exogenous shock to the common ownership of index incumbent competitors, employing a staggered difference-in-difference regression framework to test for its effect on productivity.

## 3.1 Panel regressions

Covering 20 years and about 3,700 firms across 90 industries, our data allows a detailed investigation of the relationship between common ownership and TFP using panel regressions. To control for potential bias introduced by unobserved industry or time-period characteristics that are correlated with both productivity estimates and measures of common ownership, we include industry-by-year fixed effects. We cluster standard errors by year and NAICS-3 industry as our common ownership measures are based on averages within industry by year implying that common ownership across firms in a given industry in a given year is not independently distributed. Future research that has the computational capacity to study firm-pair directed common ownership incentives (underlying our average profit weight measures) and firm-specific price-deflated productivity estimates could potentially exploit more variation to enhance the robustness of this analysis.

Our main specification relates the firm-level log-normalized productivity estimate ($\widetilde{\omega}_{ft}$) to the equally-weighted average profit weight ($\kappa_{ft}$) placed by firm $f$ on its competitors, or alternative measures of common ownership. In addition to the industry-by-year fixed effects, we control for firm size (log market capitalization), firm age, firm market share (by market capitalization) and dummies for dual class firms, controlled firms and ownership structure characteristics such as the aggregate fraction of vote ownership captured, the aggregate vote fraction held by corporate insiders, activists, engaged activists, universal owners and

ownership concentration. As intuition suggests that low levels of common ownership will have little impact on corporate outcomes, whilst high common ownership may have more pronounced effects, we control for non-linearities in the relationship between outcomes and profit weights using a squared common ownership term.

$$\begin{aligned} \widetilde{\omega}_{ft} &= \beta_1 * \kappa_{ft} + \Phi * X_{ft} + \alpha_{st} \\ \widetilde{\omega}_{ft} &= \beta_1 * \kappa_{ft} + \beta_2 * \kappa_{ft}^2 + \Phi * X_{fts} + \alpha_{st} \end{aligned} \tag{12}$$

Table 3 reports the findings for profit weights as key independent variable and measure of common ownership. The estimation results in Columns (1)-(2) suggest that profit weights have a significantly negative and non-linear impact on TFP. An increase in common ownership profit weights by one standard deviation (corresponding to 15% more weight on competitors' profits) is associated with an average decrease of 1.1% in TFP. All control variables have the expected relationship with productivity. Large firms and younger firms have significantly higher productivity, while firms constituting a larger fraction of market capitalization and with high activist ownership have lower TFP, justified by activists investing in underperforming firms. Activist engagement increases TFP, however insignificantly in our sample.

Controlling for the quadratic of profit weights increases the significance of the level regressor significantly and doubles the coefficient size. To interpret the non-linearity of the estimated effects, Figure 2(A) plots the marginal effect of profit weights on productivity at different points of the common ownership distribution. For more than half of our sample with an average profit weight below 0.3, increases in common ownership have a significant negative effect on productivity. Whereas the average estimated effect of a unit change in common ownership of TFP is 7.16%, this effect grows to 15% for the least commonly owned firms and becomes negligible for firms with initial average profit weights of more than 0.3. Figure 2(B) depicts predicted productivity outcomes for varying levels of profit weights. Across the whole distribution, the lowest predicted productivity is associated with firms weighting their average competitor's profits at about 50%. Beyond that the predicted productivity levels increase again.

The non-linearity at high levels of common ownership is in part attributable to our productivity estimate, which confounds price and efficiency dynamics. As the below discussion

of the association between markups and common ownership demonstrates, very high common ownership has a positive effect on markups and may therefore be associated with higher revenue productivity. At low levels of common ownership, however, the non-linearity is relatively intuitive in an example: We test for the relationship between a firm's productivity and that same firms' investors' interest in competitors' profits. If a firm is initially not commonly owned, while its two competitors are already commonly owned, then according to theory, the two commonly owned competitors are producing with lower efficiency, while the maverick competitor produces highly efficiently. When common ownership incentives at the two already commonly owned firms rise (regardless of an increase in common ownership between these firms or with the maverick firm), this has little impact on their productivity, as it is already low. Instead, if the maverick competitor becomes commonly owned as its investors invest in the other competitors, its productivity is reduced from much higher starting levels to a level comparable with the commonly owned competitors', to limit the external price pressure.

Our working hypothesis is that common ownership reduces productivity by lowering managerial incentives. We include managerial incentives, approximated by wealth-performance sensitivity (WPS), in the panel regression to test if the relationship between productivity and common ownership is absorbed by the effect of incentives on productivity. If $\beta_1$ is affected but remains economically and statistically significant despite including incentive proxies, then other mechanisms must also play a role in the association between common ownership and productivity.

$$\widetilde{\omega}_{ft} = \beta_1 * \kappa_{ft} + \beta_2 * \kappa_{ft}^2 + \beta_3 * wps_{ft} + \Phi * X_{ft} + \alpha_{st} \tag{13}$$

Results in Columns (3)-(4) of Table 3 suggest that the link via managerial incentives does not account for the total common ownership effect on productivity. The significant negative and non-linear profit weight coefficients hold up to the added control, however, the coefficient size decreases and without controlling for the quadratic term, the effect becomes insignificant. Performance incentives of managers seem to play an important role in the link between common ownership and productivity, although other channels may co-exist.

We run the same regression as in 12 with markups as dependent variable, to discern the underlying price and efficiency dynamics. Table 7 in the appendix provides the corre-

sponding results and Figure 9 showcases the main effects graphically. The level relationship between common ownership and markups is not significant, however, including a quadratic term renders the relation significantly negative. Including a control for managerial incentives reduces the coefficients in absolute size but does not change their significance. We also perform an inter-model Hausmann test of coefficient equality between the regression of productivity on profit weights and markups on profit weights. The corresponding $\chi^2$ statistic is 17.33. With a p-value below 0.001 we can therefore robustly reject the null hypothesis that common ownership affects both outcome variables equally.

Even if our productivity measure confounds price and efficiency dynamics, the limited significance and smaller coefficient estimates indicate that common ownership has a negative, non-linear effect on actual underlying productivity. Although we cannot regress actual productivity estimates on common ownership given data limitations, we attempt to put bounds on the potential effect based on our available estimates.

Profit weights are a theoretically sound measure of common ownership, however they are often difficult to interpret in practice. Therefore, we extend our analysis to two more readily interpretable measures: the cosine vector similarity between cash flow ownership structures and the aggregate fraction of votes held by common owners. The regression specification, fixed effects and standard error clustering are the same as before.

The relationship between productivity and the aggregate fraction of votes held by common owners behaves analogous to the findings for profit weights (see Table 9). If common owners increase their vote ownership by 10%, TFP is decreased by 1%, the effect is markedly larger for less commonly owned firms, and overall more significant than based on profit weights. As before, the association between aggregate common owner control and markups is less significant and smaller in absolute size than for productivity, substantiating our argument for a meaningful impact on actual efficiency.

Using the similarity of cash flow ownership vectors, we find similar but less non-linear results, see Table 8. The cosine similarity between two firms is higher if the firms' owner base contains mostly the same investors and if the most influential investors are the same across firms. A one standard deviation more similar ownership structure with a firm's competitors reduces its productivity highly significantly by 2%. This relationship is twice as strong as for profit weights and more linear. Controlling for the quadratic term of ownership similarity

reduces the coefficient on the level regressor and renders it insignificant.

Following Backus et al. (2021b), profit weights can be mathematically constructed from the product of ownership similarity and of relative investor concentration between firms under the assumption of one share-one vote. We cannot perform this separation in our data, since in contrast to Backus et al. (2021b) the sample contains firms with multiple class of differential voting right stock. However, it seems that ownership similarity is the key driver of the negative relationship between common ownership and productivity, whilst relative investor concentration (contained in profit weights and also relevant to the aggregate common owner control measure) is responsible for the non-linearity of the relationship.

Mirroring previous results and supporting the case for an effect on underlying productivity, the relationship between markups and owner similarity is smaller and less significant in levels. In contrast to the productivity results, the level coefficient remains significant when controlling for a squared similarity term, suggesting that markups are truly affected differently, for different degrees of common ownership.

Summing up the overall cross-sectional panel evidence, we find a significant negative effect of common ownership similarity on productivity. This effect is stronger at lower levels of common ownership, possibly related to varying relative investor concentration. Additional evidence for a significantly smaller effect on markups suggests that actual underlying efficiency is affected negatively by common ownership.

To understand how this relationship between common ownership and productivity is distributed across the sample, Figure 3 depicts for each historical NAICS-1 digit industry and year the coefficient on profit weights in a regression of log TFP on profit weights in levels, managerial incentives, controls and fixed effects. Each sub-figure pertains to a different industry, where NAICS-1 industry 1 (Agriculture) and 8 (Public Services) had to be dropped for lack of sufficient annual observations. Each bar within the sub-figure represents the year-specific coefficient on profit weights, bar height corresponding to coefficient size and color suggesting significance (gray being insignificant, light, medium and navy blue representing a p-value of 10%, 5% and 1%, respectively). The most persistent, significant negative effect is observed for NAICS-1 industries 3 and 5, corresponding to Manufacturing and Services, and during the last four years of the sample period. All other NAICS-1 industries have either erratically positive and negative estimates, of which only a couple are significant, or overall

coefficients close to zero.

The concentration of significant effects towards the later years in our sample may be justified by the sustained increase in common ownership over our sample period (Backus et al., 2021b; Amel-Zadeh et al., 2022), leading to meaningful levels of common ownership building up over the first two thirds of the sample and having a delayed impact on productivity levels.[3]

Manufacturing and Services are the NAICS-1 industries with the largest number of constituent firms per year, suggesting that we have more power to estimate these annual regressions. However, the significant negative relationship between common ownership and productivity in these two broad industries can be rationalized by two other, non-mechanical arguments: First, manufacturing and services are the most competitive industries, with many firms, high entry and exit over time, where distortions to managerial incentives can have the most significant impact on efficiency. Second, in both industries most value is added within the US, and with our data we can therefore measure more variation in the differences between inputs and outputs, resulting in meaningful productivity estimates. For example, if most of the difference between input costs and sales revenue is only due to marketing, as all relevant inputs are imported and not further differentiable, the total factor productivity residual we capture is mainly due to more or less successful sales marketing. Instead, if the costly inputs are substantially altered, differentiated or enhanced by US firms, then TFP residuals of different size capture relevant efficiency dispersion.

Taking another perspective to look at the distribution of the effect of common ownership on productivity, Table 12 provides results for regressions of productivity on profit weights, separating the sample of firms according to their market capitalization into quintiles. The relationship between common ownership and productivity is significant and most meaningful economically in the 3rd and 4th quintile of firm size, and smallest for the 5th quintile containing the largest firms. This links back to the non-linearity of the baseline regression in Table 3, as the largest firms on average also have the largest profit weights, associated with a smaller effect on productivity. However, it is not the smallest firms that are most affected by common ownership, but medium-sized firms, which have less insider ownership,

[3]The period 2000-2009 saw significant consolidation in the asset management industry (BlackRock-MGIM and BlackRock-BGI mergers) and an increasing availability of highly diversified, passive investment tools, which fueled the growth in common ownership.

more sensitive managerial incentives, and matter for competition due to larger market shares, than smaller competitors. In such firms, an increase in common ownership by one standard deviation leads to an average decrease in productivity by 3%.

## 3.2 Diff-in-Diff analysis of index additions

The panel evidence gathered so far associates various measures of common ownership with lower productivity, especially among services and manufacturing firms of medium size. However, despite including fixed effects, clustered standard errors and control variables, we cannot infer a causal link, yet. It may be that the least productive firms attract mostly activist investors that are already shareholders in the industry and who are experienced activists in this industry compared to outsiders. This story would imply reverse causality where lower productivity causes higher common ownership. To test that causality runs from common ownership to productivity we need an exogenous shock to common ownership, that affects productivity only via its impact on common ownership incentives.

Following Boller and Scott-Morton (2020), we use the addition of a firm to the S&P 500 index to shock the common ownership incentives of index incumbent competitors and run a difference-in-difference analysis on the productivity changes in treated versus untreated index incumbents. Intuitively, as a firm is added to the index, the competing index incumbent's ownership structure itself does not change. However the profit weight the incumbent places on the new index member increases, as index funds increase their exposure to the new index constituent. This increases the within-industry common ownership of index incumbent competitors, but should not equally affect common ownership of index incumbent firms in other industries.

Our complete ownership data at the quarterly level suggests that ownership by large institutional asset managers changes frequently and sometimes markedly, in response to index events and a multitude of other events. Thus the "shock" to common ownership of index incumbent competitors versus non-competitors is likely only measurable in a relatively short event window. However, once added to the index, firms mostly stay in the index for a longer time, hence the higher level of common ownership should be maintained on average and we therefore try to identify the longer run impacts of initial treatment on the index incumbent firms. Table 4 provides the results for regressions of changes in common

ownership on a treatment dummy, that is 1 for index incumbent competitors and 0 for index firms in other industries, controlling for various firm and ownership characteristics measured the year before addition, comprising industry- and year-fixed effects and standard errors clustered by industry and year. Profit weights are significantly more positively affected for treated firms than for control firms, while ownership similarity increases insignificantly for treated firms, suggesting there exists significant exogenous variation in common ownership in the short term. Over a longer time horizon the significance vanishes.

Industries are defined by their historical NAICS-3 segments and our event window around index addition is 4 years. Given our data sample the treatment events must happen between 2005-2015 to allow for 4 observation years before and after treatment.[4] In this time period we have 140 index additions, where an index incumbent in the same NAICS-3 industry exists. Of those only 90 additions are "true additions", meaning the added firm was not a member of the S&P 400 Mid-Cap index previously (see Boller and Scott-Morton (2020)). Firms which are in the year of index addition already in the S&P 500, in the same historical NAICS-3 industry as the added firm, and do not get dropped from the S&P 500 in the following year are classified as "treated" firms. "Control" firms are members of the S&P 500 in the addition year, but in a NAICS-3 industry without an addition in that year, or the previous and following year. For our 90 true treatment events we end up with 264 treated and 223 control firms.

We improve on the econometric methods employed by Boller and Scott-Morton (2020) and Anton et al. (2022) in this setting following the literature on staggered treatment events (Baker et al., 2022). As firms are added to the S&P 500 index every year, incumbent firms get treated at different points in time and because index ownership of different added firms can vary, the treatment effect is likely heterogeneous across firms. If a firm is treated the previous year and then used as a control firm in the following year, estimates of treatment effects over a four year horizon would be biased due to a "bad comparison". For a more detailed description of the issues arising in this setting see Baker et al. (2022). To address these concerns we run the difference-in-difference regressions on repeated cross-sections of treated and control firms, including treatyear-by-year and treatyear-by-industry fixed effects, and taking care to exclude control firms treated the previous or following year. In the robustness section

[4]Treatment year 2004 is dropped for lack of sufficient observations.

we perform the analysis on an even more restricted sample, excluding all firms that were treated in the previous or following four years as controls. The results are qualitatively and quantitatively similar, but the F-stat and power of the model is limited due to the severely limited sample.

Using a difference-in-difference analysis, we compare the change in productivity estimates of treated firms to control firms, before and after the treatment (addition) event. The outcome variable is regressed on a "treatment" dummy, which is 1 for treated and 0 for control firms, a "post" dummy, which is 1 for all firms in the year of treatment and subsequent years but 0 before, and the interaction of the treatment and post dummies. The interaction is 1 for treated firms from the year of addition onward and otherwise 0. We include firm-specific controls such as market capitalization, firm age, dual class structure, market share, and ownership structure controls for ownership concentration and aggregate vote ownership captured, of insiders, activists and universal owners. All control variables are measured with a one year lag. Since the trends of these controls may be affected by the treatment, we interact the controls with the "post" dummy. To control for treatment year specific variation and industry level persistence, we add treatyear-by-industry and treatyear-by-year fixed effects and cluster standard errors by year and NAICS-3 industry.

$$\widetilde{\omega}_{ft} = Post_{ft} * Treat_{ft} + Post_{ft} + Treat_{ft} + \Phi_1 X_{ft} + \Phi_2 Post_{ft} * X_{ft} + \alpha_t + \alpha_s \qquad (14)$$

Table 5 presents the regression results for this analysis and Figure 4 plots the average annual effect for treated firms' productivity relative to untreated firms. The interacted *post#treat* coefficient suggests a negative 1.2% effect of index additions on treated index incumbents' productivity, however this is statistically insignificant.

There are two rationales to explain the insignificance result. First, countervailing effects of the index addition may make added firms more competitive, through lower costs of capital or higher operating efficiency. The increased competitive pressure on index incumbents may influence their productivity positively and thereby nullifying the negative influence of increased common ownership. However, reductions in added firms' cost of capital will be very small and based on existing empirical findings (Afego, 2017) there is little evidence that index additions lead to significant changes in competitive pressure. Thus, it is unlikely that countervailing effects explain the economically meaningful but statistically insignificant

point estimate.

The second rationale is based on a lack of statistical power when estimating an average treatment effect that may be heterogeneous across time and firms, among the largest US firms (which the S&P 500 firms are) for whom panel regressions show a smaller negative effect of common ownership on productivity. To address this concern we estimate a local average treatment effect (Angrist and Imbens, 1994), employing the index addition shock to instrument variation in managerial incentives and relating this instrumented change in incentives to productivity outcomes. This implicitly re-weights the sample towards firms with more marked decreases in managerial incentives, for whom the theory of Anton et al. (2022) predicts a larger impact on productivity.

## 3.3 The mechanism: managerial incentives

Following the theoretical mechanism proposed by Anton et al. (2022), our hypothesis is that common owners cause lower productivity by setting weaker incentives for managers, and managerial slack leads to a reduction in productivity. This link between weak managerial incentives and lower productivity, which is also referred to as "enjoying the quiet life", has been supposed theoretically by Hicks (1935) and empirically established since Bertrand and Mullainathan (2003).

Initial empirical evidence on this mechanism comes from Anton et al. who find a significant negative and causal effect of common owners on managerial incentives. Their study examines the link between common ownership and managerial incentives on a restricted data set ignoring insider ownership for the sample period 2000-2017. Using a common ownership measures that is based on complete ownership data and extending the sample period, we validate their findings. Panel results in Table 14 and results of an analogous difference-in-difference regression to above summarized in Table 5, suggest common ownership significantly reduces the wealth-performance sensitivity of managerial pay. The index addition of a competitor leads to a 16% reduction in managerial wealth-performance sensitivity.

To test if common owners reduce productivity via their impact on managerial incentives, we extend the difference-in-difference analysis one step further. We use the shock to common ownership due to index addition of a competitor to estimate the effect on managerial incentives. Using this as first stage, we regress productivity on the instrumented variation

in incentives. The event window is unchanged at 4 years pre- and post-treatment and we include treatyear-by-industry and treatyear-by-year fixed effects, cluster standard errors by year and NAICS-3 industry and have the same controls for firm and ownership characteristics as before.

Anton et al. (2022) and our validation of their results show that the index addition shock is a valid instrument, as it causes significant variation in managerial incentives. Furthermore, the exclusion condition, that the shock to common ownership only affects productivity via its effect on managerial incentives, is fulfilled, assuming that our prior argument for insignificant countervailing effects via competitive pressures holds.

$$wps_{ft} = Post_{ft} * Treat_{ft} + Post_{ft} + Treat_{ft} + \Phi X_{ft} + Treatft * X_{ft} + \alpha_t + \alpha_s \tag{15}$$

$$\widetilde{\omega}_{ft} = \widehat{wps}_{ft} + \Phi X_{ft} + \alpha_t + \alpha_s \tag{16}$$

The results in Table 6 provide the first and second stage estimated coefficients. The treatment has, as expected, a significantly negative impact on incentives. We check the statistical evidence for instrument validity and the exclusion criterion. Kleibergen-Paap statistics for underidentification (weak instruments) in the first stage are rejected, while Sargan-Hansen tests for the validity of over-identifying restrictions (i.e. instrument validity) are not rejected at the 5% significant level, supporting our argument for validity and exclusivity. The instrumented variation in wealth-performance sensitivity has a highly significant (at the 1% level) positive effect on productivity. Knowing from the first stage regression, that the treatment effect on incentives is negative, the positive local average treatment effect in the second stage implies an average negative effect on productivity of $\Delta logTFP = (-0.164) * 0.125 = -2.05\%$.

Comparing the difference-in-difference results from Table 5 to the instrumented results above, the coefficient size for the average treatment effect of a shock to common ownership on productivity is slightly smaller than the local average treatment effect via managerial incentives. Analogous to the "randomized intention to treat" example in Angrist and Imbens (1994), employing managerial incentives allows for heterogeneous treatment effects in our setting, which are clearly present.

To further ascertain whether the effect of common ownership on productivity via managerial incentives we document is, in fact, an efficiency effect and not solely measuring

price adjustments, we investigate the causal impact on markups using the local average treatment effect estimation. The effect of common ownership on markups is significantly negative and marginally larger in magnitude than for productivity. The index addition of a competitor reduces markups on average by about 1.2% (Table 5), while the variation in managerial incentives caused by common ownership shocks reduces markups by $\Delta markups = (-0.164) * 0.130 = -2.13\%$ (Table 6).

Overall these results suggest that some of the estimated negative effect of common ownership on our productivity estimates is driven by the reduction in markups, but we cannot cleanly identify the proportions. At a maximum, index additions reduce actual productivity over a four year horizon by 2% via less performance-sensitive managerial incentives. At the other extreme the effect of index additions on S&P 500 incumbents' productivity is negligible. Such non-effects may be a characteristic of the sample, comprising only the largest US public firms, and may look very different in a different exercise that comprises medium sized firms. However, due to limited sources of exogenous variation in common ownership incentives we are not able to test for causal effects among such firms.

# 4 Robustness

The results thus far have been based on the cost shares approach to estimating total factor productivity. Alternative strategies to derive productivity estimates are based on the proxy approach (Ackerberg et al., 2015) and the dynamic panel approach (Blundell and Bond, 2000), which estimate the production function to arrive at factor elasticity estimates. Cost shares are likely to yield the least biased estimates of TFP in our setting and therefore are the focus of our analysis.

For the proxy approach we do not re-estimate the output elasticities of individual inputs but use the estimates derived and kindly shared by De Loecker et al.. The production function estimation following the Blundell-Bond dynamic panel data approach (Blundell and Bond, 2000) is performed on our data at the sector level and on a rolling window of 5 years. NAICS-2 or -3 digit industries are classified as individual sectors depending on their size, in order to maintain enough observations at the sector-year to estimate the production function under this approach. The elasticity estimates of both approaches are unique at

the industry- or sector-year level. Analogous to the cost shares approach we subtract the firm-specific input costs weighted by the estimated elasticities from sales to arrive at the TFP estimates.

Using these alternative estimation methods, we find that our panel and difference-in-difference results hold up qualitatively and in significance for the proxy approach-based estimates (Columns 1-4 in Table 18). For the dynamic panel approach based estimates all coefficients are rendered insignificant (Columns 5-8 in Table 18), which indicates that productivity estimates are too noisy due to limited sample size and large number of first-differences and lags used as instruments in estimation.

Another robustness exercise considers TFP and markup estimates based on industry-median cost shares, which impose the same technology on all firms in an industry and remove some idiosyncratic variation from the outcome estimates. The panel results for industry-adjusted outcome variables on profit weights in Table 19 are reduced to null and complete insignificance, likely because the variation remaining in the data after industry-adjustments, industry-by-year fixed effects and two-way clustering of standard errors is too limited to estimate any relevant associations. An important caveat to this approach for the markup estimates is that the only remaining variation in markups using industry-level cost shares and industry-by-year fixed effects comes from the firm-specific COGS-to-Sales ratio (see equation 11).

The difference-in-difference results for index addition effects on alternative measures of productivity in Table 20, are as insignificant as in our main specification, despite sizable negative coefficients. The local average treatment effects of instrumented incentives on alternative productivity estimates differ significantly (Table 21). While productivity estimates based on the proxy approach are insignificantly negatively affected by the instrumented variation in incentives, productivity based on the dynamic panel approach is insignificantly positively affected and industry-adjusted cost shares productivity is highly significantly positively affected. Industry-adjusted cost shares markups are also insignificantly positively affected possibly. Overall, these estimates offer very little evidence towards any local average treatment effect on productivity, because of extremely large standard errors that are likely driven by highly volatile productivity estimates under the proxy and dynamic panel approaches, especially.

The staggered nature of our treatment events gives rise to concerns about "good comparisons" between treated and never treated, not year treated or not currently treated firms in our difference-in-difference setting. Because true S&P500 additions offer a very limited set of treatment events and the number of control firms in the index is small by construction, we opt for the loose constraint of using not currently treated firms as controls in our main specification. This sample is just large enough to offer enough statistical power to draw relevant conclusions from it, while providing reasonable comparisons. Limiting the control sample to firms not treated at all during the treatment window of four years pre- and post-event reduces the number of control firms to 88. Results of the difference-in-difference regression and local average treatment effects estimated based on this data are reported in Tables 25 and 26. Estimated coefficient sizes and significance seem similar to the main results. However, the model validity declines markedly, as can be seen comparing the F-statistics, and the Kleibergen-Paap test for underidentification fails due to insufficient power of the first stage.

# 5 Conclusion

In this paper, we investigate empirically whether common ownership affects corporate productivity and if changes to managerial incentives are the underlying mechanism. Our measures of common ownership are based on a novel and uniquely comprehensive data set of corporate ownership for U.S. public firms. The estimates of total factor productivity and markups are derived from balance sheet data using the standard cost shares approach in the literature. We examine the relationship between common ownership and corporate outcomes using panel analysis and a difference-in-difference analysis using changes in common ownership and managerial incentives due to the index addition of a competing firm. Both specifications provide evidence of a negative effect of common ownership on total factor productivity. Further we test the theoretical mechanism suggested by Anton et al. in which common owners allow for less performance-sensitive managerial compensation and thereby affect lower productivity. Combining the difference-in-difference analysis with an instrumental variable approach we find that the variation in managerial incentives caused by an exogenous change in common ownership significantly positively affects productivity. This

result corroborates our hypothesis that common ownership negatively affects productivity by reducing managerial incentives.

Our findings contribute to the ongoing debate on anti-competitive effects of common ownership by providing first large-sample evidence on the effect of common ownership on corporate productivity which has thus far been missing. Our findings suggest that common ownership induces lower productivity by setting less performance-sensitive managerial incentives. One limitation of our analysis is that available data only allows us to estimate the effect of common ownership on total factor "revenue" productivity, which confounds changes in actual (quantity) productivity and prices (markups). However, we are able to infer directional effects on actual (quantity) productivity by estimating the effect of common ownership on revenue productivity *and* markups. Doing so, we find that actual productivity is also negatively affected by common ownership via the reduction of managerial incentives. However, we are not able to quantify the exact impact on productivity and how it may compound over time, but invite further research into the real effects of common ownership. While we have made the first step in this direction, much remains to be done.

To encourage future research on the effects of common and universal ownership not only on competition, but also on real economic outcomes such as productivity and long-run growth in greater depth, we are making our ownership data publicly available. We hope this will allow many more researchers to comprehensively investigate the economic effects of common owners and also answer other broader research questions on corporate ownership structures.

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# 6 Figures & Tables

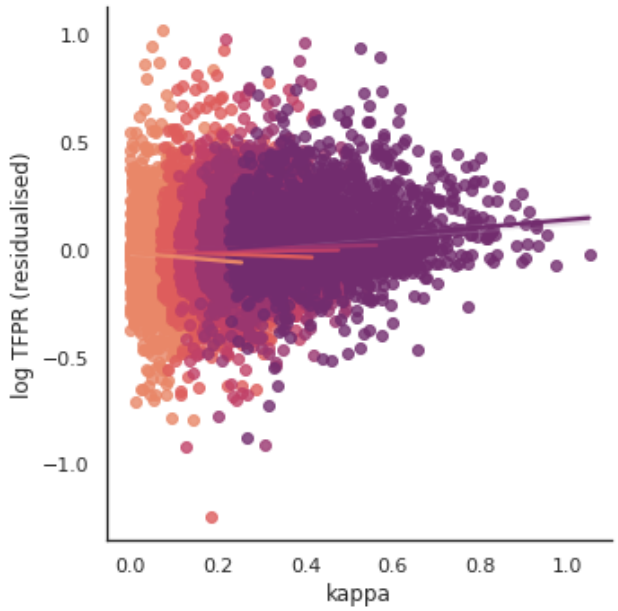
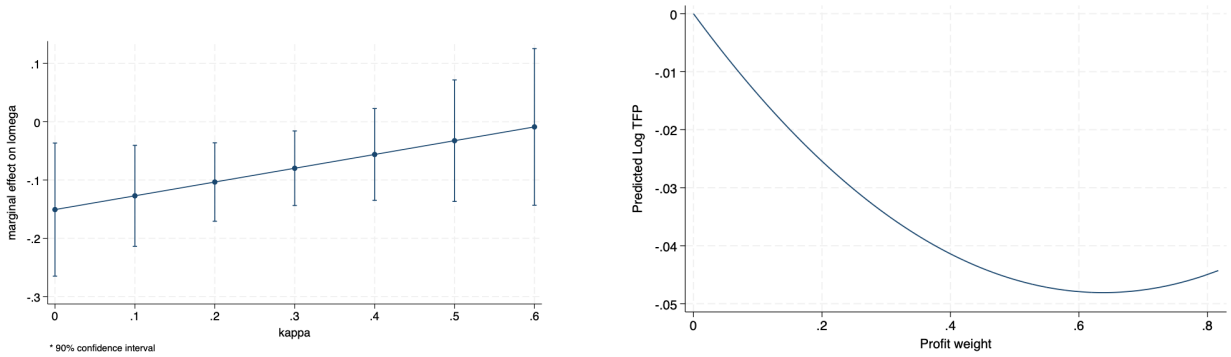



**Figure 1. Evidence on common ownership and productivity.** This figure scatters productivity estimates residualized on industry-by-year fixed effects over common ownership profit weights for all firm-year observations and estimates a linear relationship between the two variables separately by quintile of the profit weight distribution. The sample covers Q4 2000 until Q4 2019.




**(A)** Marginal effect of $\kappa$ on productivity

**(B)** Predict productivity with $\kappa$

**Figure 2. Effect of profit weights ($\kappa$) on productivity.** Figure (a) plots the marginal effect of profit weights on productivity, at discrete levels of profit weights (0-0.7 at every 10 basis points). Figure (b) plots log productivity predicted with profit weights on the right y-axis and a density plot of profit weights on the left y-axis. The underlying regressions control for ownership and firm characteristics, industry#year fixed effects, and cluster standard errors by year and NAICS-3 industry.

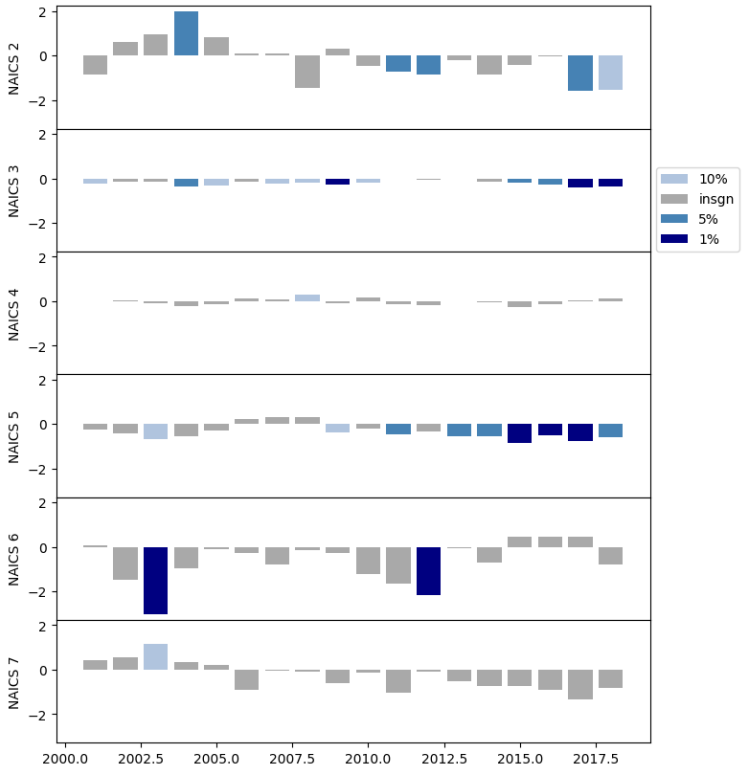



**Figure 3. Relationship profit weights and productivity across years and NAICS-1 industries.** This figure plots the coefficient on profit weights in a panel regression of log TFP for each NAICS-1 industry and year individually. Significance is indicated by colors, with p-values: p<0.1 in light blue, p<0.05 in medium blue and p<0.01 in navy blue. The underlying regressions control for ownership and firm characteristics and cluster standard errors by year and NAICS-3 industry.

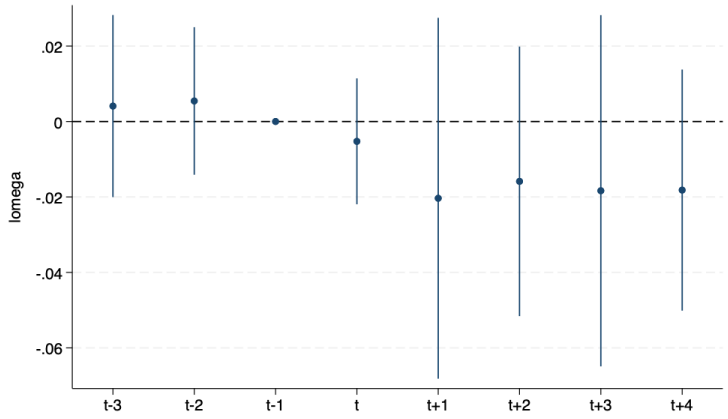



**Figure 4. Treatment effects on productivity.** This figure plots the treatment effect of index addition on treated firms' productivity in the three years pre-treatment, the treatment year itself ($t$) and the 4 years post-treatment. The underlying regression interacts annual dummies with the treatment indicator, and controls for ownership and firm characteristics, industry and year fixed effects, and clusters standard errors by year and NAICS-3 industry. Error bars correspond to the 95% confidence interval.

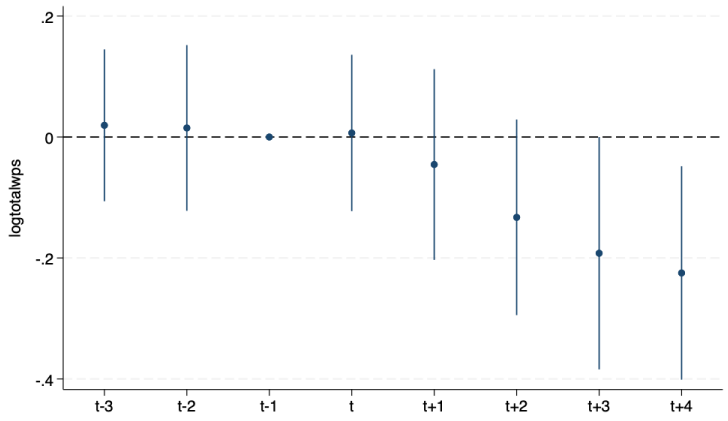



**Figure 5. Treatment effects on managerial incentives.** This figure plots the treatment effect of index addition on treated firms' wealth-performance sensitivity in the three years pre-treatment, the treatment year itself ($t$) and the 4 years post-treatment. The underlying regression interacts annual dummies with the treatment indicator, and controls for ownership and firm characteristics, industry and year fixed effects, and clusters standard errors by year and NAICS-3 industry. Error bars correspond to the 95% confidence interval.

**Table 1. Summary statistics**
This table provides summary statistics, comprising the equally-weighted mean, standard deviation, 5th, 50th and 95th percentile across the whole sample for our key dependent and independent variables, and all control variables. Common ownership measures are firm-level averages and consider firms as competitors if they were classified into the same historical NAICS-3 industry in a given year.

| | Mean | Std dev | 5th | 50th | 95th |
|---|---|---|---|---|---|
| log TFP | 0.779 | 0.220 | 0.412 | 0.785 | 1.148 |
| Markup | 1.149 | 0.143 | 0.981 | 1.123 | 1.423 |
| log TFP (med) | 1.212 | 0.359 | 0.788 | 1.135 | 1.904 |
| Markup (med) | 1.319 | 0.659 | 0.802 | 1.130 | 2.460 |
| log TFP (DL) | -1.849 | 1.825 | -4.197 | -2.036 | 1.076 |
| log TFP (BB) | 0.703 | 1.154 | -0.813 | 0.539 | 3.077 |
| $\kappa$ | 0.238 | 0.147 | 0.029 | 0.219 | 0.509 |
| $\kappa_{vw}$ | 0.012 | 0.026 | 0.000 | 0.003 | 0.056 |
| $\kappa_{sw}$ | 0.011 | 0.024 | 0.000 | 0.003 | 0.051 |
| $cosinesim$ | 0.220 | 0.109 | 0.059 | 0.210 | 0.419 |
| $aggCO$ | 0.840 | 0.269 | 0.305 | 0.880 | 1.215 |
| log WPS | 1.842 | 1.561 | -0.449 | 1.754 | 4.502 |
| log CEO WPS | 1.851 | 1.522 | -0.451 | 1.753 | 4.501 |
| Agg Ownership | 1.002 | 0.235 | 0.592 | 1.020 | 1.360 |
| Insider owned | 0.225 | 0.179 | 0.029 | 0.181 | 0.583 |
| Activist owned | 0.043 | 0.053 | 0.002 | 0.026 | 0.141 |
| Eng Activist owned | 0.004 | 0.023 | 0.000 | 0.000 | 0.005 |
| Universal owned | 0.110 | 0.086 | 0.006 | 0.087 | 0.263 |
| Ownership HHI | 0.060 | 0.086 | 0.014 | 0.040 | 0.169 |
| log Market Cap | 21.443 | 1.713 | 18.854 | 21.295 | 24.462 |
| Age | 25.924 | 20.243 | 3.000 | 21.000 | 75.000 |
| Dual dummy | 0.072 | 0.259 | 0.000 | 0.000 | 1.000 |
| Controlled | 0.025 | 0.156 | 0.000 | 0.000 | 0.000 |
| MCap share | 0.054 | 0.125 | 0.000 | 0.008 | 0.277 |

**Table 2. Correlation table**
This table provides pairwise correlation coefficients of our key independent and dependent variables. Common ownership measures are firm-level averages and consider firms as competitors if they were classified into the same historical NAICS-3 industry in a given year. Significance is indicated by stars, with p-values: ** p<0.05, * p<0.1 .

| | log TFP | Markup | log WPS | $\kappa$ | $\kappa_{sw}$ | *cosinesim* | *aggCO* |
|---|---|---|---|---|---|---|---|
| log TFP | 1.000 | | | | | | |
| Markup | 0.735** | 1.000 | | | | | |
| log WPS | 0.110** | 0.128** | 1.000 | | | | |
| $\kappa$ | 0.038** | 0.141** | -0.131** | 1.000 | | | |
| $\kappa_{sw}$ | -0.078** | -0.033** | 0.005 | 0.471** | 1.000 | | |
| *cosinesim* | -0.031** | 0.084** | -0.184** | 0.873** | 0.371** | 1.000 | |
| *aggCO* | 0.007 | 0.091** | -0.141** | 0.209** | -0.115** | 0.332** | 1.000 |
| $N$ | 16015 | | | | | | |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

**Table 3. Productivity on equal-weighted profit weights**

This table provides results for our main regression specification of firm-level productivity estimates regressed on common ownership. The key independent variables are firm-level profit weights ($\kappa$) averaged across all industry competitors and equally weighted, its squared term and log wealth-performance sensitivity as proxy for managerial incentives. Firms are considered competitors and subject to common ownership if they were classified into the same historical NAICS-3 industry in a given year. Controls include ownership and firm characteristics, industry#year fixed effects. Standard errors are clustered by year and NAICS-3 industry, and significance is indicated by stars, with p-values: *** p<0.01, ** p<0.05, * p<0.1 .

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| VARIABLES | log TFP | log TFP | log TFP | log TFP |
| | | | | |
| $\kappa$ | -0.0716* | -0.151** | -0.0657 | -0.145* |
| | (0.040) | (0.069) | (0.042) | (0.070) |
| $\kappa^2$ | | 0.118 | | 0.119 |
| | | (0.108) | | (0.107) |
| log WPS | | | 0.00431 | 0.00432 |
| | | | (0.003) | (0.003) |
| Agg ownership | 0.00736 | 0.0106 | 0.0107 | 0.0140 |
| Insider own | -0.0406 | -0.0463 | -0.0462 | -0.0520 |
| Activist own | -0.123** | -0.126** | -0.117** | -0.120** |
| Eng Activist own | 0.160 | 0.157 | 0.172 | 0.170 |
| Universal own | -0.00299 | 0.0121 | 0.00143 | 0.0167 |
| Ownership HHI | -0.0968 | -0.122 | -0.0963 | -0.122 |
| log Market Cap | 0.0478*** | 0.0478*** | 0.0467*** | 0.0466*** |
| Age | -0.000634*** | -0.000635*** | -0.000592*** | -0.000594*** |
| Dual dummy | -0.000487 | 0.000289 | -0.000426 | 0.000357 |
| MCap share | -0.196*** | -0.201*** | -0.198*** | -0.203*** |
| Controlled | 0.0151 | 0.0220 | 0.0185 | 0.0255 |
| | | | | |
| Observations | 15,295 | 15,295 | 15,295 | 15,295 |
| R-squared | 0.414 | 0.415 | 0.415 | 0.415 |
| Industry | naicsh3 | naicsh3 | naicsh3 | naicsh3 |
| Controls | Yes | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes | Yes |
| Industry-Year FE | Yes | Yes | Yes | Yes |

**Table 4. Treatment effect on common ownership measures**
This table provides regression results for the shock to common ownership due to treatment, measured by the percentage change in profit weights and ownership structure similarity between Q4 in the year before addition and Q4 in the year of addition. The treatment window for all treated and control firms is 4 years pre- and post-treatment, implying the sample is restricted to treatments between 2004-2015. The treatment indicator is 1 for all treated firms and 0 for all control firms. Treated firms observe the addition of a NAICS-3 industry competitor being added to the S&P 500 index in a given year, and are not themselves dropped the following year. Control firms do not observe a competitor addition to the index in the respective treatment year, nor in the previous or following year. Controls are omitted but include firm characteristics and ownership concentration in the year before treatment, industry and year fixed effects. Standard errors are clustered by year and NAICS-3 industry, and significance is indicated by stars, with p-values: *** p<0.01, ** p<0.05, * p<0.1 .

| | (1) | (2) |
|---|---|---|
| VARIABLES | $\Delta\ \kappa$ | $\Delta\ cosinesim$ |
| | | |
| treat | 0.0473* | 0.0327 |
| | (0.0249) | (0.0194) |
| | | |
| Observations | 2,152 | 2,152 |
| R-squared | 0.059 | 0.095 |
| Industry | naicsh3 | naicsh3 |
| Cluster | Yes | Yes |
| Industry FE | Yes | Yes |
| Year FE | Yes | Yes |

**Table 5. True Treatment effect on productivity & markups**
This table provides regression results for the treatment effect on log wealth-performance sensitivity, log productivity and markups. The treatment window for all treated and control firms is 4 years pre- and post-treatment, implying the sample is restricted to treatments between 2004-2015. The variable post is a dummy equal to 1 in the treatment year and 4 consecutive years and otherwise 0. The treatment indicator is 1 for all true treated firms (added competitor was not previously part of the S&P 400 Mid-Cap) and 0 for all control firms. Treated firms observe the addition of a NAICS-3 industry competitor being added to the S&P 500 index in a given year, and are not themselves dropped the following year. Control firms do not observe a competitor addition to the index in the respective treatment year, nor in the previous or following year. Controls are omitted but include firm and ownership characteristics in the year before treatment, industry and year fixed effects. Standard errors are clustered by year and NAICS-3 industry, and significance is indicated by stars, with p-values: *** p<0.01, ** p<0.05, * p<0.1 .

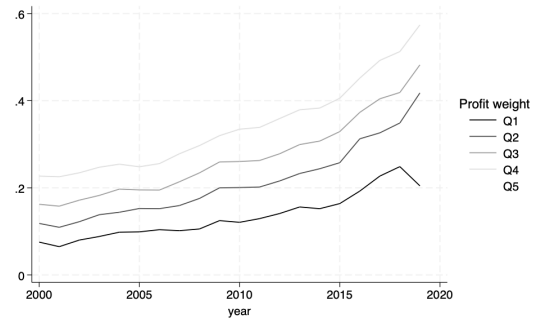
| VARIABLES | (1)<br>log WPS | (2)<br>log TFP | (3)<br>Markup |
|---|---|---|---|
| post#treat | -0.161** | -0.0115 | -0.0113 |
| | (0.069) | (0.016) | (0.014) |
| Observations | 10,715 | 10,090 | 10,090 |
| R-squared | 0.428 | 0.558 | 0.505 |
| Industry | naicsh3 | naicsh3 | naicsh3 |
| True Inclusions | Yes | Yes | Yes |
| Unique Treated Firms | 264 | 264 | 264 |
| Unique Control Firms | 223 | 223 | 223 |
| Controls | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes |
| Treatyear-Industry FE | Yes | Yes | Yes |
| Treatyear-Year FE | Yes | Yes | Yes |

**Table 6. Instrumented effect of incentives on outcomes**
This table provides regression results for the instrumented treatment effect of managerial incentives on log productivity and markups. The treatment window for all treated and control firms is 4 years pre- and post-treatment, implying the sample is restricted to treatments between 2004-2015. The first stage regresses log wealth-performance sensitivity on the treatment#post interaction. The variable post is a dummy equal to 1 in the treatment year and 4 consecutive years and otherwise 0. The treatment indicator is 1 for all true treated firms (added competitor was not previously part of the S&P 400 Mid-Cap) and 0 for all control firms. Treated firms observe the addition of a NAICS-3 industry competitor being added to the S&P 500 index in a given year, and are not themselves dropped the following year. Control firms do not observe a competitor addition to the index in the respective treatment year, nor in the previous or following year. The second stage regresses log productivity and markups (from the 3-input Cobb-Douglas function, columns (1)+(3) or from the 2-input Cobb-Douglas function, columns (2)+(4)) on instrumented wealth-performance sensitivity. Controls are omitted but include firm and ownership characteristics in the year before treatment, industry and year fixed effects. Standard errors are clustered by year and NAICS-3 industry, and significance is indicated by stars, with p-values: *** p<0.01, ** p<0.05, * p<0.1 .

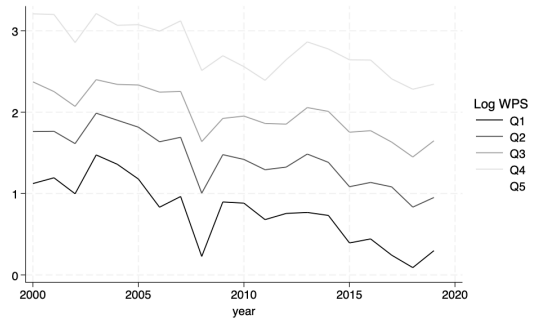
| | (1) | (2) | (3) |
|---|---|---|---|
| VARIABLES | log WPS | log TFP | Markup |
| post#treat | -0.164 *** | | |
| | (0.044) | | |
| $\widehat{logWPS}$ | | 0.125*** | 0.130*** |
| | | (0.039) | (0.033) |
| Observations | 10,029 | 10,029 | 10,029 |
| R-squared | | -0.537 | -1.235 |
| Fstat | 26.48 | 2.632 | 15.85 |
| Industry | naicsh3 | naicsh3 | naicsh3 |
| Controls | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes |
| Treatyear-Industry FE | Yes | Yes | Yes |
| Treatyear-Year FE | Yes | Yes | Yes |

# Internet Appendix to *Common Ownership, Incentives & Productivity*

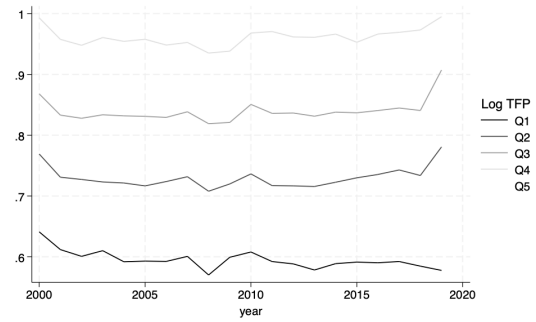
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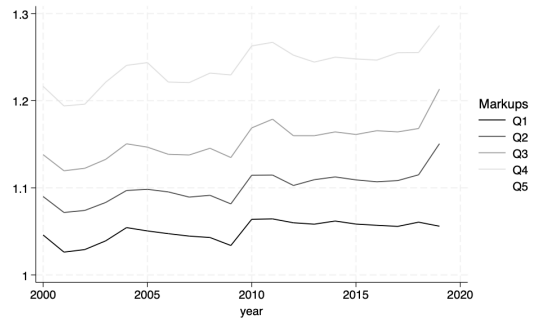

**(A)** Profit weights




**(B)** Managerial incentives

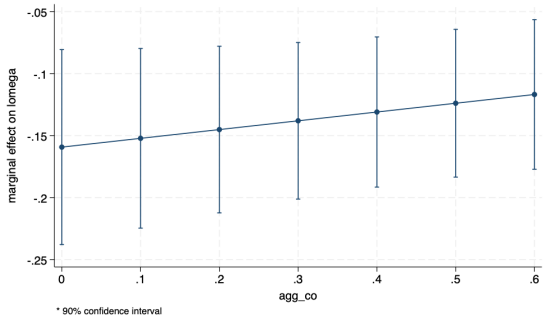



**(C)** Productivity

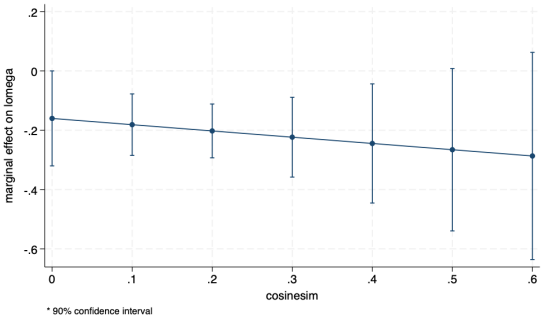



**(D)** Markups

**Figure 6. Time trend in key variable distributions** This figure plots the annual quintiles of profit weights, log wealth performance sensitivity, log TFP and markups across all sample firms. The sample covers Q4 2000 until Q4 2019.




**(A)** Marginal effect of aggCO on log TFP




**(B)** Marginal effect of cosinesim on log TFP

**Figure 7. Effect of aggregate CO control and ownership similarity on productivity.** Figure (a) plots the marginal effect of aggregate common owner control on productivity, at discrete levels of aggregate common owner control (0-1.3 at every 10 basis points). Figure (b) the marginal effect of ownership similarity on productivity, at discrete levels of ownership similarity (0-0.8 at every 10 basis points). The underlying regressions control for ownership and firm characteristics, industry#year fixed effects, and cluster standard errors by year and NAICS-3 industry.

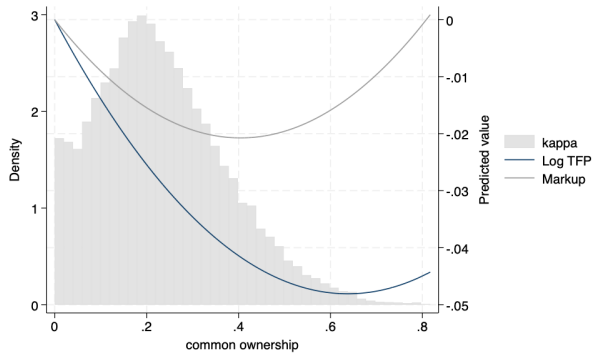



**Figure 8. Predict productivity and markups on profit weights.** This figure plots log productivity and markups predicted by profit weights on the right y-axis and a density plot of profit weights on the left y-axis. The underlying regressions control for ownership and firm characteristics, industry#year fixed effects, and cluster standard errors by year and NAICS-3 industry.

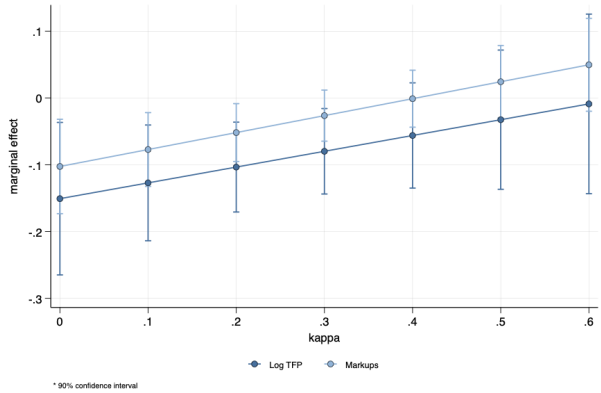
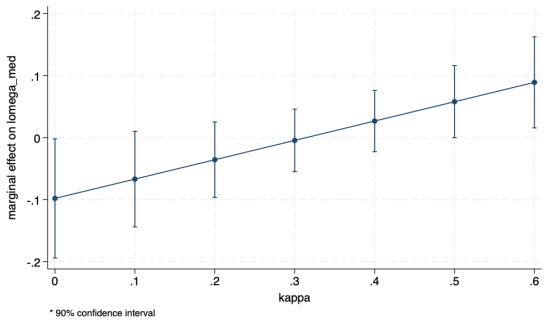
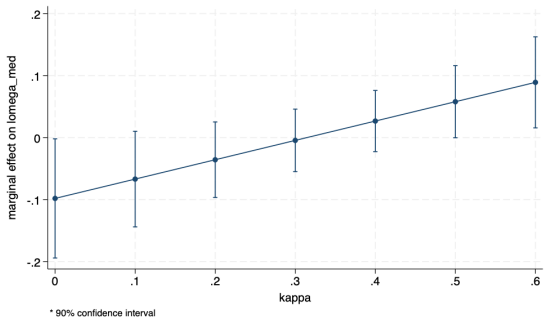


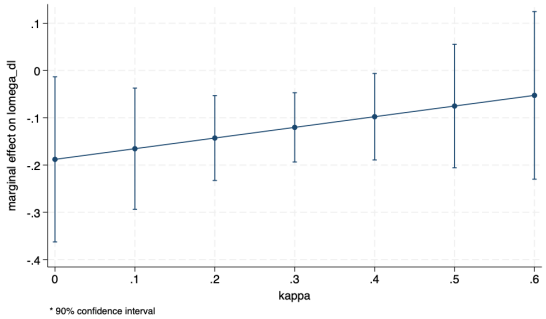

**Figure 9. Marginal effect of profit weights on productivity & markups.** This figure plots the marginal effect of profit weights on log TFP and markups, at discrete levels of profit weights (0-0.7 at every 10 basis points). The underlying regressions control for ownership and firm characteristics, industry#year fixed effects, and cluster standard errors by year and NAICS-3 industry.



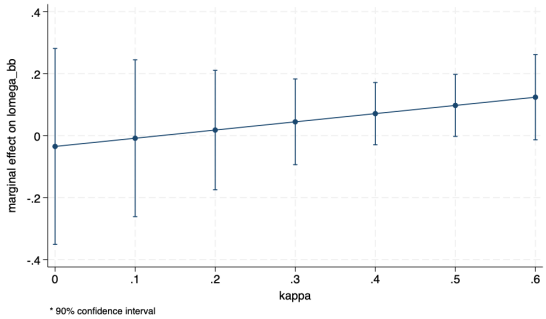

**(A)** Productivity (industry-specific cost shares).




**(B)** Markups (industry-specific cost shares).




**(C)** Productivity (proxy approach).




**(D)** Productivity (dynamic panel approach).

**Figure 10. Marginal effect of profit weights on alternative TFP estimates.** This figure plots the marginal effect of profit weights on log TFP and markup estimates based on the cost shares approach but using industry-specific input elasticities in (a) and (b), respectively, and on log TFP estimates based on the proxy approach following (De Loecker et al., 2020) in (c) or the panel approach following (Bond et al., 2021) in (d), at discrete levels of profit weights (0-0.7 at every 10 basis points). The underlying regressions control for ownership and firm characteristics, industry#year fixed effects, and cluster standard errors by year and NAICS-3 industry.

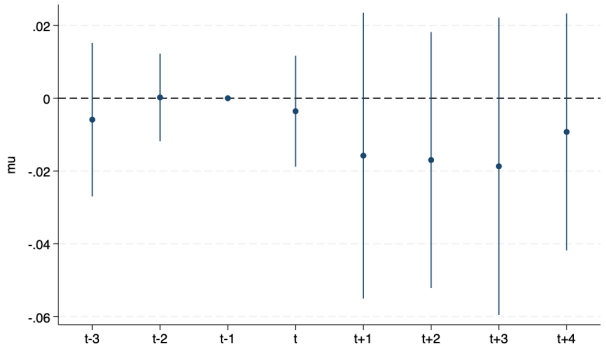
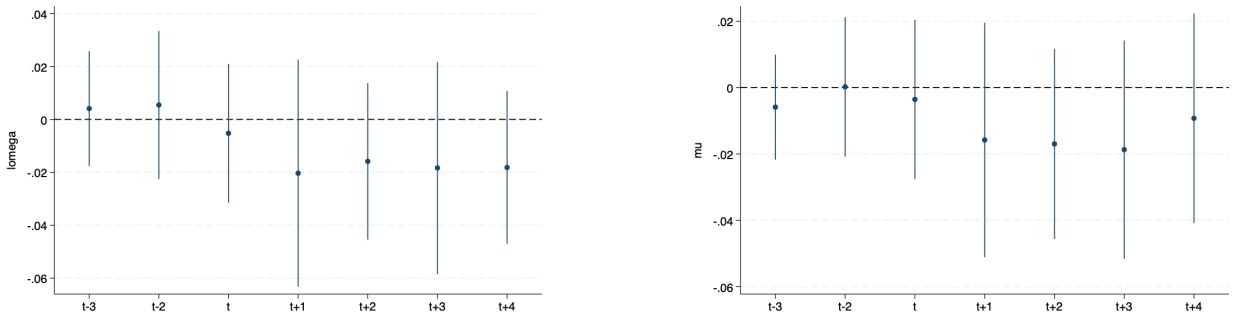



**Figure 11. Treatment effect on markups.** This figure plots the treatment effect of index addition on treated firms' markups in the three years pre-treatment, the treatment year itself ($t$) and the 4 years post-treatment. The underlying regression interacts annual dummies with the treatment indicator, and controls for ownership and firm characteristics, industry and year fixed effects, and clusters standard errors by year and NAICS-3 industry.




**(A)** Treatment effect on log TFP.

**(B)** Treatment effect on markups.

**Figure 12. Treatment effects on log TFP and markups - only year clustered.** This figure plots the treatment effect of index addition on treated firms' median-cleaned productivity in the three years pre-treatment, the treatment year itself ($t$) and the 4 years post-treatment. The underlying regression interacts annual dummies with the treatment indicator, and controls for ownership and firm characteristics, industry and year fixed effects, and clusters standard errors by year.

**Table 7. Markups on equal-weighted profit weights**

This table provides results for our main regression specification of firm-level markup estimates regressed on common ownership. The key independent variables are firm-level profit weights ($\kappa$) averaged across all industry competitors and equally weighted, its squared term and log wealth-performance sensitivity as proxy for managerial incentives. Firms are considered competitors and subject to common ownership if they were classified into the same historical NAICS-3 industry in a given year. Controls include ownership and firm characteristics, industry#year fixed effects. Standard errors are clustered by year and NAICS-3 industry, and significance is indicated by stars, with p-values: *** p<0.01, ** p<0.05, * p<0.1 .

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| VARIABLES | Markups | Markups | Markups | Markups |
| $\kappa$ | -0.0176 | -0.103** | -0.0109 | -0.0966** |
| | (0.023) | (0.043) | (0.025) | (0.043) |
| $\kappa^2$ | | 0.127** | | 0.128** |
| | | (0.060) | | (0.058) |
| log WPS | | | 0.00486** | 0.00487*** |
| | | | (0.002) | (0.002) |
| agggamma | 0.0351* | 0.0386* | 0.0389* | 0.0424** |
| Insider own | -0.0314* | -0.0376** | -0.0378** | -0.0441** |
| Activist own | -0.0817** | -0.0854** | -0.0748** | -0.0785** |
| Eng Activist own | -0.0462 | -0.0490 | -0.0323 | -0.0352 |
| Universal own | 0.0126 | 0.0288 | 0.0176 | 0.0339 |
| Ownership HHI | 0.000151 | -0.0270 | 0.000734 | -0.0267 |
| log Market Cap | 0.0441*** | 0.0441*** | 0.0428*** | 0.0428*** |
| age | -0.000755*** | -0.000756*** | -0.000708*** | -0.000709*** |
| dual | -0.0110 | -0.0101 | -0.0109 | -0.0101 |
| MCap share | -0.164*** | -0.170*** | -0.167*** | -0.172*** |
| controlled | -0.00402 | 0.00335 | -0.000146 | 0.00729 |
| Observations | 15,295 | 15,295 | 15,295 | 15,295 |
| R-squared | 0.398 | 0.399 | 0.401 | 0.401 |
| Industry | naicsh3 | naicsh3 | naicsh3 | naicsh3 |
| Controls | Yes | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes | Yes |
| Industry-Year FE | Yes | Yes | Yes | Yes |

**Table 8. Productivity & markups on equal-weighted ownership structure similarity**
This table provides regression coefficients for our main panel regression specification. The dependent variables are log productivity in columns (1)-(4) and markups in columns (5)-(8), the key independent variables are firm-level average ownership structure similarity with competitor firms, their squares and log wealth performance sensitivity. Firms are considered competitors and subject to common ownership if they were classified into the same historical NAICS-3 industry in a given year. Controls are omitted but include ownership and firm characteristics, industry#year fixed effects. Standard errors are clustered by year and NAICS-3 industry, and significance is indicated by stars, with p-values: *** p<0.01, ** p<0.05, * p<0.1 .

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| VARIABLES | log TFP | log TFP | log TFP | log TFP | Markups | Markups | Markups | Markups |
| *cosinesim* | -0.209*** | -0.160 | -0.200*** | -0.152 | -0.107** | -0.160* | -0.0961** | -0.150* |
| | (0.061) | (0.097) | (0.063) | (0.099) | (0.039) | (0.077) | (0.041) | (0.075) |
| $cosinesim^2$ | | -0.106 | | -0.104 | | 0.115 | | 0.117 |
| | | (0.239) | | (0.239) | | (0.149) | | (0.144) |
| log WPS | | | 0.00411 | 0.00410 | | | 0.00467** | 0.00468** |
| | | | (0.003) | (0.003) | | | (0.002) | (0.002) |
| Observations | 15,295 | 15,295 | 15,295 | 15,295 | 15,295 | 15,295 | 15,295 | 15,295 |
| R-squared | 0.416 | 0.416 | 0.416 | 0.416 | 0.399 | 0.400 | 0.401 | 0.401 |
| Industry | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry-Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

**Table 9. Productivity & markups on aggregate common owner control**
This table provides regression coefficients for our main panel regression specification. The dependent variables are log productivity in columns (1)-(4) and markups in columns (5)-(8), the key independent variables are firm-level average aggregate control fraction of common owners, their squares and log wealth performance sensitivity. Firms are considered competitors and subject to common ownership if they were classified into the same historical NAICS-3 industry in a given year. Controls are omitted but include ownership and firm characteristics, industry#year fixed effects. Standard errors are clustered by year and NAICS-3 industry, and significance is indicated by stars, with p-values: *** p<0.01, ** p<0.05, * p<0.1 .

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| VARIABLES | log TFP | log TFP | log TFP | log TFP | Markups | Markups | Markups | Markups |
| *aggCO* | -0.0992** | -0.159*** | -0.0935** | -0.156*** | -0.0383** | -0.0767 | -0.0316* | -0.0723 |
| | (0.042) | (0.048) | (0.044) | (0.048) | (0.017) | (0.045) | (0.018) | (0.045) |
| $aggCO^2$ | | 0.0354 | | 0.0366 | | 0.0226 | | 0.0240 |
| | | (0.031) | | (0.030) | | (0.025) | | (0.025) |
| log WPS | | | 0.00388 | 0.00392 | | | 0.00460** | 0.00463** |
| | | | (0.003) | (0.003) | | | (0.002) | (0.002) |
| Observations | 15,347 | 15,347 | 15,347 | 15,347 | 15,347 | 15,347 | 15,347 | 15,347 |
| R-squared | 0.418 | 0.418 | 0.419 | 0.419 | 0.402 | 0.402 | 0.403 | 0.404 |
| Industry | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry-Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

**Table 10. Productivity & markups on sales-weighted profit weights**
This table provides regression coefficients for key outcome variables regressed on sales-weighted profit weights. The dependent variables are log productivity in columns (1)-(4), and markups in columns (5)-(8). The key independent variables are firm-level profit weights averaged across all industry competitors and weighted with the sales share of each respective competitor, its squared term and log wealth performance sensitivity. Firms are considered competitors and subject to common ownership if they were classified into the same historical NAICS-3 industry in a given year. Controls include ownership and firm characteristics, industry#year fixed effects. Standard errors are clustered by year and NAICS-3 industry, and significance is indicated by stars, with p-values: *** p<0.01, ** p<0.05, * p<0.1 .

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| VARIABLES | log TFP | log TFP | log TFP | log TFP | Markups | Markups | Markups | Markups |
| $\kappa_{sw}$ | -2.016*** | -4.100*** | -1.990*** | -4.059*** | -0.804*** | -1.777*** | -0.770*** | -1.720*** |
| | (0.452) | (0.724) | (0.453) | (0.724) | (0.184) | (0.379) | (0.182) | (0.376) |
| $\kappa_{sw}^2$ | | 10.83*** | | 10.74*** | | 5.057** | | 4.927** |
| | | (3.162) | | (3.129) | | (1.996) | | (1.950) |
| log WPS | | | 0.00357 | 0.00312 | | | 0.00451** | 0.00431** |
| | | | (0.003) | (0.003) | | | (0.002) | (0.002) |
| Observations | 15,295 | 15,295 | 15,295 | 15,295 | 15,295 | 15,295 | 15,295 | 15,295 |
| R-squared | 0.425 | 0.431 | 0.425 | 0.431 | 0.402 | 0.406 | 0.404 | 0.407 |
| Industry | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry-Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

**Table 11. Productivity & markups on profit weights - separately for quintiles of profit weights**
This table provides regression coefficients for log TFP regressed on equal-weighted profit weights, separately for each quintile of the profit weight distribution, beginning with firms in the lowest quintile of profit weights in column (1) and ending firms in the top quintile of profit weights in column (5).The key independent variable are firm-level profit weights averaged across all industry competitors and equally weighted. Firms are considered competitors and subject to common ownership if they were classified into the same historical NAICS-3 industry in a given year. Controls include ownership and firm characteristics, industry#year fixed effects. Standard errors are clustered by year and NAICS-3 industry, and significance is indicated by stars, with p-values: *** p<0.01, ** p<0.05, * p<0.1 .

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | log TFP | log TFP | log TFP | log TFP | log TFP |
| $\kappa$ | -0.193 | -0.204 | -0.515** | -0.0867 | 0.0706 |
| | (0.218) | (0.301) | (0.246) | (0.131) | (0.085) |
| log WPS | -0.000380 | 0.00799** | 0.0111* | 0.00639 | 0.0108* |
| | (0.003) | (0.004) | (0.005) | (0.008) | (0.005) |
| Observations | 2,796 | 2,776 | 2,788 | 2,837 | 2,770 |
| R-squared | 0.448 | 0.428 | 0.459 | 0.486 | 0.564 |
| Industry | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 |
| Controls | Yes | Yes | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes | Yes | Yes |
| Industry-Year FE | Yes | Yes | Yes | Yes | Yes |
| kappa Quantile | 1 | 2 | 3 | 4 | 5 |

**Table 12. Productivity & markups on profit weights - separately for quintiles of market capitalization**

This table provides regression coefficients for log TFP regressed on equal-weighted profit weights, separately for each quintile of the market capitalization distribution, beginning with firms in the lowest quintile of log market cap in column (1) and ending firms in the top quintile of log market cap in column (5).The key independent variable are firm-level profit weights averaged across all industry competitors and equally weighted. Firms are considered competitors and subject to common ownership if they were classified into the same historical NAICS-3 industry in a given year. Controls include ownership and firm characteristics, industry#year fixed effects. Standard errors are clustered by year and NAICS-3 industry, and significance is indicated by stars, with p-values: *** p<0.01, ** p<0.05, * p<0.1 .

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | log TFP | log TFP | log TFP | log TFP | log TFP |
| $\kappa$ | -0.0409 | -0.0497 | -0.191* | -0.232*** | -0.0152 |
| | (0.054) | (0.072) | (0.098) | (0.059) | (0.070) |
| log WPS | -0.00260 | 0.00254 | -0.00169 | 0.0210*** | 0.00539 |
| | (0.003) | (0.005) | (0.006) | (0.004) | (0.004) |
| Observations | 2,842 | 2,865 | 2,798 | 2,774 | 2,586 |
| R-squared | 0.403 | 0.413 | 0.440 | 0.461 | 0.529 |
| Industry | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 |
| Controls | Yes | Yes | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes | Yes | Yes |
| Industry-Year FE | Yes | Yes | Yes | Yes | Yes |
| log Market Cap Quantile | 1 | 2 | 3 | 4 | 5 |

**Table 13. Productivity on profit weights interacted with ownership structure attributes**

| VARIABLES | (1)<br>log TFP | (2)<br>log TFP | (3)<br>log TFP | (4)<br>log TFP | (5)<br>log TFP |
|---|---|---|---|---|---|
| $\kappa$ | -0.0657 | -0.0319 | -0.0650 | -0.0649 | -0.0648 |
| | (0.042) | (0.047) | (0.040) | (0.042) | (0.042) |
| $\kappa$#Insider own | | -0.348** | | | |
| | | (0.145) | | | |
| $\kappa$#Activist own | | | -0.0242 | | |
| | | | (0.412) | | |
| $\kappa$#Eng Activist own | | | | -0.727 | |
| | | | | (0.989) | |
| $\kappa$#controlled | | | | | 0.491 |
| | | | | | (0.616) |
| log WPS | 0.00431 | 0.00452 | 0.00431 | 0.00431 | 0.00440 |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Observations | 15,295 | 15,295 | 15,295 | 15,295 | 15,295 |
| R-squared | 0.415 | 0.416 | 0.415 | 0.415 | 0.415 |
| Industry | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 |
| Controls | Yes | Yes | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes | Yes | Yes |
| Industry-Year FE | Yes | Yes | Yes | Yes | Yes |

**Table 14. Managerial incentives on lags of common ownership**

| VARIABLES | (1) log WPS | (2) log WPS | (3) log WPS | (4) log WPS | (5) log WPS | (6) log WPS |
|---|---|---|---|---|---|---|
| $\kappa$ | -0.0101 | -1.559*** | | | | |
| | (0.400) | (0.385) | | | | |
| Lag $\kappa$ | -0.573** | | -1.565*** | | | |
| | (0.229) | | (0.378) | | | |
| Lag2 $\kappa$ | -0.331 | | | -1.502*** | | |
| | (0.224) | | | (0.397) | | |
| Lag3 $\kappa$ | -0.212 | | | | -1.510*** | |
| | (0.244) | | | | (0.385) | |
| Lag4 $\kappa$ | -1.088*** | | | | | -1.735*** |
| | (0.357) | | | | | (0.422) |
| Observations | 9,372 | 15,761 | 13,867 | 12,433 | 11,090 | 9,823 |
| R-squared | 0.233 | 0.228 | 0.229 | 0.229 | 0.229 | 0.230 |
| Industry | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry-Year FE | Yes | Yes | Yes | Yes | Yes | Yes |

**Table 15. Log productivity on lags of managerial incentives**

| VARIABLES | (1) log TFP | (2) log TFP | (3) log TFP | (4) log TFP | (5) log TFP | (6) log TFP |
|---|---|---|---|---|---|---|
| log WPS | 0.000186 | 0.00454 | | | | |
| | (0.002) | (0.003) | | | | |
| Lag log WPS | -0.00220 | | 0.00420 | | | |
| | (0.002) | | (0.003) | | | |
| Lag2 log WPS | 0.000295 | | | 0.00430 | | |
| | (0.002) | | | (0.003) | | |
| Lag3 log WPS | 0.00323 | | | | 0.00445 | |
| | (0.002) | | | | (0.003) | |
| Lag4 log WPS | 0.00361* | | | | | 0.00491 |
| | (0.002) | | | | | (0.003) |
| Observations | 9,168 | 15,347 | 13,534 | 12,142 | 10,837 | 9,610 |
| R-squared | 0.449 | 0.417 | 0.427 | 0.437 | 0.443 | 0.448 |
| Industry | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry-Year FE | Yes | Yes | Yes | Yes | Yes | Yes |

**Table 16. Log productivity on instrumented incentives using profit weights**

| VARIABLES | (1) log WPS | (2) log TFP | (3) Markups |
|---|---|---|---|
| $\kappa$ | -1.382 *** | | |
| | (0.385) | | |
| $\widehat{logWPS}$ | | 0.0518 | 0.0128 |
| | | (0.031) | (0.018) |
| Observations | 15,295 | 15,295 | 15,295 |
| R-squared | | -0.009 | 0.216 |
| Fstat | | 25.34 | 99.80 |
| Industry | naicsh3 | naicsh3 | naicsh3 |
| Controls | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes |
| Industry-Year FE | Yes | Yes | Yes |

**Table 17. Effect on outcomes of instrumented incentives**
This table provides regression results for the instrumented treatment effect of managerial incentives on log productivity and markups. The treatment window for all treated and control firms is 4 years pre- and post-treatment, implying the sample is restricted to treatments between 2004-2015. The first stage regresses log wealth-performance sensitivity on the treatment#post interaction. The variable post is a dummy equal to 1 in the treatment year and 4 consecutive years and otherwise 0. The treatment indicator is 1 for all true treated firms (added competitor was not previously part of the S&P 400 Mid-Cap) and 0 for all control firms. Treated firms observe the addition of a NAICS-3 industry competitor being added to the S&P 500 index in a given year, and are not themselves dropped the following year. Control firms do not observe a competitor addition to the index in the respective treatment year, nor in the previous or following year. The second stage regresses log productivity and markups (from the 3-input Cobb-Douglas function, columns (1)+(3) or from the 2-input Cobb-Douglas function, columns (2)+(4)) on instrumented wealth-performance sensitivity. Controls are omitted but include firm and ownership characteristics in the year before treatment, industry and year fixed effects. Standard errors are clustered by year, and significance is indicated by stars, with p-values: *** p<0.01, ** p<0.05, * p<0.1 .

| | (1) | (2) | (3) |
|---|---|---|---|
| VARIABLES | log WPS | log TFP | Markups |
| post#treat | -0.164 *** | | |
| | (0.044) | | |
| $log\hat{W}PS$ | | 0.125*** | 0.130*** |
| | | (0.018) | (0.018) |
| Observations | 10,029 | 10,029 | 10,029 |
| R-squared | | -0.537 | -1.235 |
| Fstat | 24.53 | 43.79 | 24.47 |
| Industry | naicsh3 | naicsh3 | naicsh3 |
| Controls | Yes | Yes | Yes |
| Cluster | Year | Year | Year |
| Treatyear-Industry FE | Yes | Yes | Yes |
| Treatyear-Year FE | Yes | Yes | Yes |

**Table 18. Alternative productivity estimates on equal-weighted profit weights**

| VARIABLES | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Proxy approach | | | | Panel approach | | | |
| | log TFP | log TFP | log TFP | log TFP | log TFP | log TFP | log TFP | log TFP |
| $\kappa$ | -0.112** | -0.188* | -0.0724 | -0.154 | 0.0535 | -0.0351 | 0.0727 | -0.0187 |
| | (0.047) | (0.106) | (0.048) | (0.113) | (0.073) | (0.192) | (0.080) | (0.197) |
| $\kappa^2$ | | 0.113 | | 0.122 | | 0.132 | | 0.136 |
| | | (0.162) | | (0.170) | | (0.203) | | (0.203) |
| log WPS | | | 0.0257*** | 0.0257*** | | | 0.0123** | 0.0123** |
| | | | (0.004) | (0.004) | | | (0.006) | (0.006) |
| Observations | 15,761 | 15,761 | 15,761 | 15,761 | 15,761 | 15,761 | 15,761 | 15,761 |
| R-squared | 0.981 | 0.981 | 0.982 | 0.982 | 0.825 | 0.825 | 0.825 | 0.825 |
| Industry | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry-Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

**Table 19. Productivity and markup estimates with industry-specific technology on equal-weighted profit weights**

| VARIABLES | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | industry-specific median cost shares | | | | | | | |
| | log TFP | log TFP | log TFP | log TFP | Markups | Markups | Markups | Markups |
| $\kappa$ | 0.00628 | -0.0982 | 0.00958 | -0.0952 | -0.0136 | -0.267 | -0.000713 | -0.255 |
| | (0.030) | (0.058) | (0.030) | (0.060) | (0.112) | (0.358) | (0.111) | (0.356) |
| $\kappa^2$ | | 0.156** | | 0.157** | | 0.378 | | 0.380 |
| | | (0.070) | | (0.070) | | (0.437) | | (0.435) |
| log WPS | | | 0.00238* | 0.00239* | | | 0.00929 | 0.00932 |
| | | | (0.001) | (0.001) | | | (0.008) | (0.008) |
| Observations | 15,295 | 15,295 | 15,295 | 15,295 | 15,295 | 15,295 | 15,295 | 15,295 |
| R-squared | 0.833 | 0.833 | 0.833 | 0.833 | 0.246 | 0.246 | 0.247 | 0.247 |
| Industry | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry-Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

**Table 20. True treatment effect on alternative productivity estimates**

| VARIABLES | (1) log CEO WPS | (2) log WPS | (3) log TFP (proxy) | (4) log TFP (panel) | (5) log TFP (med) | (6) Markups (med) |
|---|---|---|---|---|---|---|
| post#treat | -0.158** | -0.161** | -0.437 | -0.108 | -0.0518 | -0.0585 |
| | (0.068) | (0.069) | (0.289) | (0.137) | (0.043) | (0.044) |
| Observations | 10,713 | 10,715 | 10,781 | 10,781 | 10,090 | 10,090 |
| R-squared | 0.433 | 0.428 | 0.814 | 0.549 | 0.708 | 0.465 |
| Industry | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 |
| True Inclusions | Yes | Yes | Yes | Yes | Yes | Yes |
| Unique Treated Firms | 264 | 264 | 264 | 264 | 264 | 264 |
| Unique Control Firms | 223 | 223 | 223 | 223 | 223 | 223 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes | Yes | Yes | Yes |
| Treatyear-Industry FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Treatyear-Year FE | Yes | Yes | Yes | Yes | Yes | Yes |

**Table 21. Effect on alternative productivity outcomes of incentives instrumented with treatment shock**

| VARIABLES | (1) log WPS | (2) log TFP (proxy) | (3) log TFP (panel) | (4) log TFP (med) | (5) Markups (med) |
|---|---|---|---|---|---|
| post#treat | -0.164 *** | | | | |
| | (0.044) | | | | |
| $\widehat{logWPS}$ | | -0.0555 | 0.0727 | 0.103*** | 0.166 |
| | | (0.089) | (0.152) | (0.036) | (0.125) |
| Observations | 10,715 | 10,715 | 10,715 | 10,029 | 10,029 |
| R-squared | | 0.006 | -0.005 | -0.275 | -0.131 |
| Fstat | | 5.078 | 3.153 | 14.13 | 6.092 |
| Industry | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 |
| Controls | Yes | Yes | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes | Yes | Yes |
| Treatyear-Industry FE | Yes | Yes | Yes | Yes | Yes |
| Treatyear-Year FE | Yes | Yes | Yes | Yes | Yes |

**Table 22. Log productivity on profit weights varying fixed effects**

| VARIABLES | (1) log TFP | (2) log TFP | (3) log TFP | (4) log TFP | (5) log TFP | (6) log TFP | (7) log TFP |
|---|---|---|---|---|---|---|---|
| $\kappa$ | -0.226*** | -0.250*** | -0.0706* | -0.0650* | -0.0657 | -0.0657*** | -0.0657 |
| | (0.045) | (0.052) | (0.034) | (0.037) | (0.042) | (0.019) | (0.040) |
| log WPS | 0.00389 | 0.00449 | 0.00462* | 0.00466* | 0.00431 | 0.00431*** | 0.00431* |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.001) | (0.003) |
| Observations | 15,445 | 15,445 | 15,445 | 15,445 | 15,295 | 15,295 | 15,295 |
| R-squared | 0.131 | 0.135 | 0.374 | 0.377 | 0.415 | 0.415 | 0.415 |
| Industry | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes | Yes | Yes | Year | naicsh3 |
| Year FE | No | Yes | No | Yes | No | No | No |
| Industry FE | No | No | Yes | Yes | No | No | No |
| Industry-Year FE | No | No | No | No | Yes | Yes | Yes |
| Firm FE | No | No | No | No | No | No | No |

**Table 23. Treatment effect on productivity estimates varying fixed effects**

| VARIABLES | (1) log TFP | (2) log TFP | (3) log TFP | (4) log TFP |
|---|---|---|---|---|
| post#treat | -0.0148 | -0.0144 | -0.0110 | -0.0115 |
| | (0.029) | (0.030) | (0.017) | (0.016) |
| Observations | 10,101 | 10,101 | 10,101 | 10,090 |
| R-squared | 0.187 | 0.192 | 0.532 | 0.558 |
| Industry | naicsh3 | naicsh3 | naicsh3 | naicsh3 |
| True Inclusions | Yes | Yes | Yes | Yes |
| Unique Treated Firms | 264 | 264 | 264 | 264 |
| Unique Control Firms | 223 | 223 | 223 | 223 |
| Controls | Yes | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes | Yes |
| Treatyear FE | No | Yes | No | No |
| Industry FE | No | No | Yes | No |
| Year FE | No | No | Yes | No |
| Treatyear-Industry FE | No | No | No | Yes |
| Treatyear-Year FE | No | No | No | Yes |

**Table 24. Effect on productivity of incentives instrumented with treatment shock varying fixed effects**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| 1st stage | log WPS | | | | | | |
| post#treat | -0.079 | -0.097 | -0.126 | -0.124 | -0.178 ** | -0.164 ** | -0.164 *** |
| | (0.119) | (0.107) | (0.083) | (0.076) | (0.081) | (0.065) | (0.044) |
| 2nd stage | log TFP | | | | | | |
| $\widehat{logWPS}$ | 0.0199 | 0.0188 | 0.137*** | 0.137*** | 0.126*** | 0.125*** | 0.125*** |
| | (0.034) | (0.032) | (0.041) | (0.041) | (0.037) | (0.039) | (0.018) |
| Observations | 10,038 | 10,038 | 10,037 | 10,037 | 10,037 | 10,029 | 10,029 |
| R-squared | 0.033 | 0.033 | -0.699 | -0.699 | -0.576 | -0.537 | -0.537 |
| Industry | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 | naicsh3 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes | Yes | Yes | Yes | Year |
| Treatyear FE | No | Yes | No | Yes | No | No | No |
| Year FE | No | No | Yes | Yes | Yes | No | No |
| Industry FE | No | No | Yes | Yes | Yes | No | No |
| Treatyear-Industry FE | No | No | No | No | No | No | No |
| Treatyear-Year FE | No | No | No | No | Yes | Yes | Yes |
| Fstat | 2.755 | 2.802 | 3.148 | 3.171 | 3.164 | 2.632 | 43.79 |

)

**Table 25. Treatment effect on productivity estimates in reduced treatment-control sample**

| VARIABLES | (1) log WPS | (2) log TFP | (3) Markups |
|---|---|---|---|
| post#treat | -0.266** | -0.00386 | -0.00190 |
| | (0.099) | (0.013) | (0.012) |
| Observations | 9,193 | 9,226 | 9,226 |
| R-squared | 0.409 | 0.506 | 0.493 |
| Industry | naicsh3 | naicsh3 | naicsh3 |
| True Inclusions | Yes | Yes | Yes |
| Unique Treated Firms | 236 | 236 | 236 |
| Unique Control Firms | 88 | 88 | 88 |
| Controls | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes |
| Treatyear-Industry FE | Yes | Yes | Yes |
| Treatyear-Year FE | Yes | Yes | Yes |

**Table 26. Effect on productivity of incentives instrumented with treatment shock on reduced treatment-control sample**

| VARIABLES | (1) log WPS | (2) log TFP | (3) Markups |
|---|---|---|---|
| post#treat | -0.272 *** | | |
| | (0.099) | | |
| $\widehat{logWPS}$ | | 0.163** | 0.178*** |
| | | (0.069) | (0.049) |
| Observations | 9,193 | 9,193 | 9,193 |
| R-squared | | -1.013 | -2.477 |
| Fstat | 22.6 | 7.685 | 3.654 |
| Industry | naicsh3 | naicsh3 | naicsh3 |
| Controls | Yes | Yes | Yes |
| Cluster | Yes | Yes | Yes |
| Treatyear-Industry FE | Yes | Yes | Yes |
| Treatyear-Year FE | Yes | Yes | Yes |