Governance through Shame and Aspiration: Index Creation and Corporate Behavior

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Abstract

After decades of both de-prioritizing shareholders' economic interests and low corporate profitability, Japan introduced the JPX-Nikkei400 in 2014. The index highlighted the country's "best-run" companies by annually selecting the 400 most profitable among Japan's large and liquid firms. We find that managers competed to be included in the index by significantly increasing ROE, at least in part due to managers' reputational or status concerns. The ROE increase was predominantly driven by improvements in margins, which was partially driven by cutting R&D intensity-a potentially unintended consequence. Our findings suggest that indexes can affect managerial behavior through reputational or status incentives.

Keywords: JPX-Nikkei 400 index, Corporate governance, Index inclusion, Reputation incentives, Status incentives, Return on equity, Capital efficiency, Social norms

JEL Classifications: G18, G34, G38, G41, L51, M14, M52

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1 Introduction

How can persistent economic behavior be changed? The standard approach studied in economics is the use of formal contracts or pecuniary rewards to incentivize the desired behavior. Another is the use of non-pecuniary strategies or informal incentives. This approach has increasingly attracted the attention of scholars in economics and adjacent fields (e.g., Goode, 1978; Masulis and Mobbs, 2014), and could be especially valuable in settings that impose constraints on formal incentives or their effectiveness.

A novel and increasingly salient mechanism for influencing the norms of corporate behavior is stock indexes. Over the last decade, stock indexes whose membership depends on social, environmental, and corporate-governance attributes have proliferated. Perhaps most prominent were the decisions of the S&P500 Dow Jones and the FTSE Russell, two of the world's largest index providers, in mid-2017 to exclude certain firms with classified shares—that deviate from the "one share, one vote" principle—from their main indexes, an apparent attempt to reverse the trend toward adoption of such structures prior to initial public offerings (Bebchuk and Kastiel, 2017).

But little is known about whether, how, or how effectively stock indexes can influence corporate behavior. One theory is that stock indexes intensify the explicit incentives provided by formal contracts by offering capital-market benefits—greater salience to investors, higher liquidity, and lower cost of capital. Alternatively, by functioning as clubs of virtuous firms, thematic stock indexes could promote certain behavior as a value or an ideal (e.g., Guiso, Sapienza, and Zingales, 2015b), thereby acting as an alternate governance mechanism which leverages corporate managers' reputational or status incentives.

We study a setting in which a central government deployed a stock index as a policy tool to solve a longstanding and fundamental economic problem. Japan's low corporate profitability and capital efficiency—specifically, low return on equity (ROE)—became a primary target of Prime Minister Shinzo Abe's "Abenomics" policies and his administration's efforts to revitalize the economy and boost capital markets. (e.g., Ito, 2014, states that "ROE improvement can be regarded as the core of the third arrow of Abenomics.")[1] Formal incentives have limited effectiveness in this setting, due both to a longstanding culture of de-

[1] The report (e.g., Chart 3 of Ito, 2014) cites a 2012 average ROE of 5.3% among Japanese firms in the TOPIX500, or roughly one-quarter of average ROE at U.S. S&P500 firms and one-third of average ROE at the firms in the Bloomberg European 500.

prioritizing shareholders' economic interests relative to those of stakeholders (e.g., customers, suppliers and employees) and to strong corporate norms of relatively low executive compensation and pay-to-performance sensitivities (Buchanan, Chai, and Deakin, 2013). In 2014, the JPX-Nikei Index 400 (JPX400) was launched by the Japan Exchange Group, the parent of Japan's two largest stock exchanges (Tokyo Stock Exchange and Osaka Stock Exchange). Considered "the shiniest toy in the Abenomics box" (Lewis, 2017), this index showcased the 400 large and liquid Japanese firms that performed best in terms of profitability, capital efficiency, and (by extension) corporate governance. Membership was considered highly prestigious, a status attained in part due both to formal endorsement of the index by the Government Pension Investment Fund (GPIF) and to intense media coverage of its membership churn. The JPX400's status as the gauge of Japan's best-run companies was evident in its colloquial nickname, "the shame index," in reference to the experience of excluded firms.

In this paper, we study whether Japanese managers responded to the *ex ante* JPX400-inclusion incentives by improving corporate capital efficiency and, if so, why. Our identification strategy is made possible by a unique feature of the JPX400: membership is determined each year by a quantitative score based on operating income, ROE, and market capitalization. The transparency of this ranking algorithm allows managers (and researchers) to assess firms' relative likelihood of index inclusion, even though the Japan Exchange Group does not disclose the official scores. Indeed, using publicly available information, we are able to generate synthetic JPX400 rankings that predict actual membership with a high degree of accuracy and that explain variation across firms in the likelihood of index inclusion.

If managers were motivated to attain JPX400 membership, these incentives would be expected to be strongest at firms ranked near the margin of index inclusion; all else equal, managers at those firms would be most likely to see their inclusion status change as a function of their effort and performance. Thus, to study the effects on corporate performance generated by the launch of the JPX400, we exploit variation in firms' distance from the threshold of index inclusion. Our analysis focuses on ROE as the main outcome of interest because, among the components of the JPX400 composite score, it is the most heavily weighted determinant, the most directly controllable by managers, and the most directly related to policy makers' goal.

Using synthetic JPX400 ranks, we employ a difference-in-differences (DID) design to study the index-inclusion effects; "treated" firms are defined as those with synthetic ranks near the inclusion threshold (ranks 301–500 in our main specification), and "control" firms are similar firms with a much lower probability of gaining inclusion (firms ranked 501–800). Our DID compares the difference in outcomes between these two groups in the post-period (selection years 2014–2016) with their difference in the pre-period (2010–2012).[2] A unique feature of this design is that, unlike traditional DID designs, a firm's treatment status varies over time: its ranking, and thus its distance from the index-inclusion threshold, varies year by year. No single manager has full control over a firm's rank, which depends on the stochastic nature of the firm's performance and on the performance of *other* JPX400-eligible firms. Therefore, our research design in effect combines multiple "experiments" in the post-JPX400 period with multiple placebo "experiments" in the pre-period to infer the effect of JPX400-inclusion incentives.

We document three main empirical findings. First, firms closest to the JPX400-inclusion threshold achieved differential and economically significant improvement in ROE. We estimate that these firms increased ROE by an economically significant 2.4 percentage-points on average. A battery of tests supports the hypothesis that this effect is driven by index-inclusion incentives—that is, by the firms' efforts to improve ROE in order to be included in (or to avoid exclusion from) the index. We document that the ROE improvements: (1) do not appear to be driven by differential trends; (2) are declining in a firm's distance from the threshold of inclusion; and (3) are not a realized consequence of index inclusion.

Second, we show that managers responded in part due to status or reputational concerns. We do not find that firms realized significant *ex post* capital-market, product-market, or compensation benefits from inclusion in the JPX400 *per se*. However, we do find that firms who are added to or booted from the index received significantly greater media attention. To distinguish whether firms were motivated by *ex ante* expectations of capital- or product-market benefits or by reputational concerns, we exploit the differences between Nikkei225 and non-Nikkei225 firms. By virtue of belonging to a closely tracked and salient market index, Nikkei225 firms already enjoyed the highest liquidity in the Japanese market; thus the *incremental* capital-market benefits of inclusion in the JPX400 would presumably be more important to non-Nikkei225 firms. But Nikkei225 firms would probably also be more

[2]We exclude the year 2013 because firms lacked sufficient time to respond in the first year of the index's implementation. Nevertheless, including this year does not change our results qualitatively. The index and its implementation are described in detail in Section 3 and in Appendix B.

sensitive to reputational concerns, specifically fear of loss of status if they were not included in the new gauge of Japan's best-run corporations. Consistent with a reputation concerns story playing a role among the Nikkei225 firms, we find that the media-attention effect for added or booted firms was greater for Nikkei225 firms; moreover, we document that these firms exhibited significant larger responses compared to non-Nikkei225 firms.

Third, we show that while the index appears to have driven firms towards the broader policy goal of ROE improvement, it might have also induced potentially undesirable behavior. Firms on average improved ROE predominantly through improved margins, and we do not find evidence that index-inclusion incentives drove firms to engage in accruals earnings management, cutting capital investments, or reducing employment or average employee pay. However, we do find evidence that firms improved margins in part by cutting discretionary expenses like research and development, a potentially undesirable effect. Nevertheless, overall we find that the firms' improvements in ROE resulting from index-inclusion incentives, rather than inclusion in the index *per se*, led to higher market valuations.

Our work makes several significant contributions. First, we contribute to the literature on the effect of indexes. There has been abundant research on how index constituents behave and perform *in response to inclusion* in an index, including the capital-market consequences thereof (e.g., Shleifer, 1986; Harris and Gurel, 1986; Chen, Noronha, and Singal, 2004; Greenwood, 2008; Doh, Howton, Howton, and Siegel, 2009; Boone and White, 2015; Appel, Gormley, and Keim, 2015). By contrast, our study provides large-sample evidence that the desire for inclusion (or to avoid exclusion) can motivate changes in firm behavior.

Second, by showing that indexes can affect managerial behavior through managers' status or reputational concerns, we contribute to a small and growing empirical literature on the role of reputation in corporate governance, and in capital markets in general (e.g., Avery, Chevalier, and Schaefer, 1998; Dyck, Volchkova, and Zingales, 2008; Malmendier and Tate, 2009; Masulis and Mobbs, 2014; Dai, Parwada, and Zhang, 2015; Raff and Siming, 2017; Focke, Maug, and Niessen-Ruenzi, 2017). Of these, the papers closest to ours are Dyck et al. (2008) who examine the role of the media and reputation concerns in influencing corporate governance in Russia, and Masulis and Mobbs (2014), who examine the effects of firm prestige on independent directors' performance, and infer that reputational incentives are powerful for directors. We extend these papers by documenting, in a setting with relatively strong empirical identification, how a manager's status concerns can be actively employed in a novel governance tool to influence the behavior of certain firms.

Lastly, our findings also speak to a broad literature in economics and sociology on how persistent behaviors or norms can be changed (Goode, 1978; Guiso, Sapienza, and Zingales, 2015a; Guiso et al., 2015b), in particular on how the allocation of status can affect the resource-allocation process (Zingales, 2015). We highlight a novel mechanism—a stock index—for instigating changes in corporate behavior. Overall, insights from our analyses could be valuable to policy makers seeking alternative channels of influence on corporate managers, which may be particularly relevant in the context of increasing pressures, in the United States and abroad, to limit executive compensation, and in developing-market contexts where there may be greater constraints to contracting.

2 Background

This section discusses the confluence of factors that make the JPX400 a compelling setting for studying the role of indexes in changing longstanding corporate behavior: (1) Japanese corporations' orientation toward "stakeholders"; (2) corporate-governance reform efforts and Japan's persistent corporate capital efficiency problem; and (3) institutional features that made the JPX400 index a credible instrument of reform.

2.1 Governance, Reform, and Corporate Culture in Japan

Today's Japanese corporate culture emerged in a post-war period characterized by government-led reconstruction and centralized industrial policy. Even early in the 21st century, coordination between Japanese corporations and government ministries remained close, and managers saw themselves as guardians of collective "corporate value" rather than of shareholder value (Aoki, Jackson, and Miyajima, 2007). Thus the interests of employees, suppliers, customers, and strategic stakeholders were prioritized over those of shareholders (e.g., Yoshimori, 1995; Ito, 2014). Concurrently, strong norms of relatively low managerial compensation and of low sensitivity of compensation to shareholder returns also took hold.

Although over the years the Japanese legal system conveyed increasing legal rights to shareholders, shareholders' *de facto* power to influence management remained weak. Many domestic investors appeared unwilling to exercise their rights in a manner that could create conflict with corporate management, limiting the role of the market for corporate control—takeovers, private equity, and shareholder activism—in resolving agency problems at Japanese firms.[3] In 2014 Japanese managers rated shareholders as their companies' fourth most important stakeholders, behind customers, employees, and suppliers.[4]

Many observers, including Japanese policy makers, have pointed to the country's stakeholder-oriented culture as an explanation for its lagging corporate capital efficiency—its low return on equity and low return on assets—and for decades of economic stagnation since the bubble economy of the 1980s (Ito, 2014). By 2013, many policy makers had come to believe that improving Japanese corporations' capital efficiency was vital to reviving the economy—an urgent concern in light of Japan's looming problems. That year, the Ministry of Economy, Trade, and Industry commissioned a systematic review of "competitiveness and incentives for sustainable growth"; the result was the influential Ito Report (Ito, 2014), which notes:

> Japan faces a rapidly aging and declining population and a decreasing stock of labor and household financial assets. Japan has no room to waste its limited resources and capital. Japan must effectively leverage the resources. . . . In other words, increasing capital efficiency in the broadest sense is crucial from the perspective of Japan's survival. Japanese companies—as a critical source of value creation—must strive to increase capital efficiency through their dialogue with investors, and contribute to the accumulation of a broad range of capital stock that will serve as the foundation for future economic prosperity.

These goals and concerns were embodied in the Abe's administration's "third arrow" of structural reforms—considered the most important component of Abenomics.[5] As Ito

[3]For example, in cases where activists or prospective acquirers turned hostile, they could rarely secure the votes of domestic retail, strategic, and institutional investors, who were typically unwilling to break the norm of siding with management (Puchniak and Nakahigashi, 2017), even when doing so was likely to convey economic benefits (Buchanan et al., 2013). As Buchanan et al. (2013) document, activist shareholders in the early 2000s were frequently able to compel open shareholder votes on proposals that cash-rich Japanese firms increase their payouts. They faced no formal structural barriers (such as from corporate charters, classified shareholders, or Japanese courts), but their proposals were simply voted down by investors loyal to management.

[4]"Change for the Better: Corporate Governance in Japan," Schroders TalkingPoint, April 22, 2014, Figure 1, accessed December 5, 2014.

[5]Since his election in December 2012, Prime Minister Shinzo Abe has advocated an economic policy, dubbed "Abenomics" by economists and the media, that consists of three main components ("three arrows"): the "first arrow" of monetary easing, the "second arrow" of fiscal stimulus, and the "third arrow" of structural reforms.

(2014) summarized, “ROE improvement can be regarded as the core of the third arrow of Abenomics.” As part of its policy efforts, Abe’s administration hoped to increase corporate managers’ dialogue with and focus on shareholders.[6] In other words, in its efforts to boost capital markets and revitalize the economy, Japan sought to change the longstanding corporate norms that de-prioritized shareholders’ economic interests.

2.2 The JPX-Nikkei 400 Index

In 2014, partly in response to these calls for reform, the Japan Exchange Group (JPX)—the parent of Japan’s two largest stock exchanges (Tokyo Stock Exchange and Osaka Stock Exchange)—and Nikkei Inc— Japan’s largest media company—launched a new stock index, the JPX-Nikkei 400 (JPX400), designed to showcase the best 400 large and liquid public firms in Japan in terms of profitability, capital efficiency, and, by extension, good corporate governance. (See Section 3 for a full and exact description of the algorithm.) The index quickly became the new prestige index in the Japanese equity markets, largely due to a formal endorsement by the Government Pension Investment Fund (GPIF)—the world’s largest pension fund, run by the Japanese government—which decided to use the JPX400 as an equity benchmark for its passive investments in 2014. Historically, the GPIF’s investment approach was conservative and fixed-income-focused; thus its choice of the JPX400—the only “thematic” equity index chosen as a benchmark—came as a huge surprise to market participants and attracted substantial attention. The choice signaled that the fund would be paying much more attention to the governance aspects of companies that it invested in, consistent with the goals of Abe’s “third arrow,” and also boosted the status of the JPX400 in the eyes of domestic institutional investors and the media as a gauge of “well-run” companies.

Contributing to the elevation of the JPX400’s status was the subsequent media coverage. Each summer, the JPX reconstitutes its membership, dismissing firms that no longer make the cut and adding firms that have improved their performance. Although the index *per*

[6] Two 2014 reforms by the Financial Services Agency were designed to encourage manager-shareholder dialogue: the Stewardship Code and the Corporate Governance Code. The Stewardship Code encourages institutional investors to pursue long-term returns and to engage companies in constructive dialogue. The Corporate Governance Code, a “bill of rights” for shareholders (Gow, Wang, Jinjo, and Sato, 2017), urges companies to respect shareholder rights, improve capital efficiency, engage investors in dialogue on a regular basis, and appoint at least two external directors to their boards. Neither code is legally binding: neither institutional investors nor companies subject to the codes are required to abide by all of their principles; instead they are required to either comply or explain.

se offers no direct financial benefits to its constituent firms or their managers in the form of a direct monetary award, the index's creation and its churn each August have generated substantial excitement and attention in the media. Its colloquial nickname, "the shame index," refers to the experience of firms that are excluded.

A unique feature of JPX400's selection criterion is that its algorithm is explicit, transparent, and, with the exception of a small number of "qualitative adjustments," based on publicly available financial data. Although the JPX publishes the names of the constituent companies but not the underlying rankings, the JPX400 rankings are highly replicable. These features are in sharp contrast with the Nikkei225, Japan's traditional leading stock index and the oldest stock index in Asia, whose selection criteria are opaque and determined by Nihon Keizai Shimbun, Japan's top financial publication and owned by Nikkei Inc. The other major stock index tracked by institutional investors, the TOPIX, consists of the largest firms listed on the Tokyo Stock Exchange (i.e., those belonging to the "First Section" of the Exchange). Thus, unlike the traditional indexes, on whose composition corporate managers exert little or no influence, the transparency and replicability of the JPX400 selection algorithm increases managers' ability to influence the probability of their companies' inclusion. Anecdotal evidence, news reports, and interviews with top managers all suggest that many managers—both at firms that were initially included and at those initially excluded—were motivated to improve their firms' capital efficiency. Many excluded firms aspired to gain entry; many included firms feared future expulsion.[7]

The JPX400 index can be viewed as an effort to influence the norms of Japanese corporate behavior by incentivizing efforts at inclusion in an index based on performance measures of interest to policy makers and shareholders. This initiative was consistent with Japanese policy makers' belief that improved competitiveness and capital efficiency would help revive the country's economy. Indeed, the index was widely seen as a vital component of the "third arrow" governance reforms of the Abe administration (Economist, 2014): *The Financial Times* called it the "by far the shiniest toy in the Abenomics box." (Lewis, 2017).

The JPX400 is notable as the first instance of a central government deploying a stock

[7]When Amada, a well-established 68-year-old toolmaker and member of the Nikkei225 index, was excluded from the inaugural JPX400 index in 2014, company president Mitsuo Okamoto announced that the company intended to improve its capital efficiency and shareholder returns and to appoint independent directors in order to gain membership. Similarly, some firms that were included in the index, such as Unicharm, announced measures aimed at securing their membership by further improving ROE (McLannahan, 2014).

index as a primary policy tool to change a persistent behavioral outcome (i.e., low corporate capital efficiency). Moreover, as Japan is a setting with constraints on formal incentives, it constitutes a unique laboratory to study the potential incentive effects of this new alternate governance mechanism. The remainder of the paper empirically examines whether, how effectively, and via what incentive channels the JPX400 influenced Japanese corporate behavior.

3 Empirical Design

We study how the JPX400's index-inclusion incentives affect firm performance. Firms nearer to the inclusion threshold are most likely to see their inclusion status change as a function of their performance. Thus we hypothesize that, all else equal, firms closer to the threshold of inclusion will differentially improve ROE, the pertinent performance metric that is most readily controllable by managers.[8]

Our empirical examination requires measurements of firms' index-inclusion incentives *ex ante*. Our maintained assumption is that, though the JPX does not publish its rankings of firms, because of the transparency and replicability of the JPX400 selection algorithm, managers are aware (at least roughly) of their relative rankings and of their firms' proximity to the threshold of inclusion.

Thus our empirical strategy is anchored in a synthetic replication of the JPX400's rankings of eligible firms. We first validate these synthetic rankings, then use them as the basis of a difference-in-differences design to infer how index-inclusion incentives affect firms' subsequent ROE. These steps are described in the following subsections.

3.1 Synthetic JPX400 Ranks and Sample Construction

We obtain Worldscope data on annual fundamentals (including but not limited to all the underlying variables listed in Appendix A) and Datastream data on monthly prices, volume, outstanding shares, and returns, for a comprehensive list of Japanese securities. We omit observations that are missing returns, have an empty "data date" field for fundamentals

[8]The intuition is similar to the theoretical and empirical finding in Casas-Arce and Martínez-Jerez (2009) that, in a multi-period tournament, agents who are either far ahead of or far behind a reward cutoff reduce their effort relative to that of agents closer to the margin between winning and losing the prize.

data, or are duplicated on their Datastream identifier and relevant time indicator. We merge in executive compensation data from Toyo Kezai, information on "full articles" in the media from Ravenpack, an indicator for Nikkei225 membership constructed using historical updates on constituent firms archived on the Nikkei website, and an indicator for JPX400 membership constructed using historical updates on constituent firms archived on the Japan Exchange Group website.[9]

We then replicate the algorithm used by the Japan Exchange Group to construct the JPX400 index, and employ the resulting synthetic ranks to design our empirical analyses. As Appendix B shows, the JPX selects a new crop of JPX400 members each year, on the last day of June, using available price and volume data and financial-statement data released prior to April.[10] We use the same information used by the JPX to construct our synthetic ranking.

The JPX400 selection algorithm begins by filtering TSE-listed companies (i.e., those listed on the First, Second, and Mothers Sections of the Tokyo Stock Exchange and those listed on the JASDAQ) on several criteria. First, it excludes all companies that (1) have been listed on the TSE fewer than three consecutive years, (2) have had negative book value in *any* of the past three years, (3) have had negative operating income in *all* of the past three years, or (4) are in the process of being de-listed. From this pool of eligible firms, the JPX then selects the top 1,200 stocks by "trading value" (price times volume, or the total value of transactions in the stock over the preceding year). Finally, the JPX winnows down these 1,200 stocks to the top 1,000 stocks by market capitalization.

These 1,000 firms, which we refer to collectively as "the ranked set," are then ranked

[9]The set of current constituents and historical changes in the Nikkei225 index can be obtained at https://indexes.nikkei.co.jp/en/nkave/index/component. We first construct an annual dataset consisting of all Nikkei225 firms in each year, then match those firms to our baseline sample using their four-digit tickers (Datastream's "Local Offering Code"). Finally, we define a Nikkei225 indicator that equals 1 if a firm belonged to the Nikkei225 on the date when the JPX400 index was announced. We achieve a complete match for Nikkei225 members in all years. The set of current JPX 400 members can be obtained at http://www.jpx.co.jp/english/markets/indices/JPX-Nikkei400/. We constructed the JPX400 member indicator variable by downloading then-current members as of July 2017, and then reversing the additions and deletions archived in the "Periodic Reviews" (available at the same web address) to construct prior years' members. We then match that indicator to our dataset using the four-digit ticker code, which maps to Datastream's "Local Offering Code."

[10]As Appendix B shows, the inaugural year was an exception to this rule. As we explain below, our empirical design excludes the first year (2013) of index selection.

using the following composite score ($Total\_Score$):

$$Total\_Score_{i,t} = .4 \times ROE\_Rank_{i,t} + .4 \times Op\Pi\_Rank_{i,t} + .2 \times MCap\_Rank_{i,t}, \qquad (1)$$

where $ROE\_Rank_{i,t}$, $Op\Pi\_Rank_{i,t}$, and $MCap\_Rank_{i,t}$ are firm i's ranks in the ranked set on three-year average ROE, three-year total operating profit, and market capitalization respectively.

Each year's index constituents are chosen on the basis of the highest $Total\_Score$, with one caveat: the JPX reserves the right to make up to ten "qualitative adjustments" per year based on corporate-governance and disclosure-related factors. These qualitative adjustments are not determined by observable factors, but, according to our interview with representatives of Japan Exchange Group (and empirical evidence presented in the next subsection), they are insignificant. For our purposes, we treat qualitative adjustments as random noise in the index-inclusion rule. We follow the JPX400 selection algorithm precisely, with the exception of the qualitative adjustments, to create synthetic JPX400 ranks for each year from 1994 through 2016.

3.2 Research Design

We utilize these synthetic rankings to test how index-inclusion incentives affect firm behavior. Our main dependent variable of interest is one-year-ahead ("forward") ROE. Of the three components of the index-selection score (equation 1), ROE is the only scaled variable; thus its ranking is the most controllable by managers. The other two components, market capitalization and operating income, are unscaled; thus their variation is largely driven by firm size. Managers might be able to increase firm size via seasoned equity offerings or acquisitions, but these actions would be likely to generate a competing effect on a firm's rankings for the JPX400 by increasing the equity base and, *ceteris paribus*, decreasing ROE. Thus we expect to observe the incentive effects of the JPX400 most cleanly in ROE.

Our main tests are difference-in-differences (DID) regressions. The first difference compares the ROE of the firms closest to the threshold of JPX400 inclusion—those with synthetic ranks of 301–500, our treatment group—to that of firms outside the threshold of inclusion—those with synthetic ranks of 501–800, our main controls, in the period after introduction of the JPX400 (2014–2016). We exclude 2013 because the JPX400's inaugural constituents were announced at the end of that year, affording firms only three months to respond to index-inclusion incentives. The DID estimator effectively benchmarks the first difference, between treatment and controls in the post-period, against the baseline difference between firms with synthetic ranks of 301–500 and those with synthetic ranks of 501–800 in our pre period (the years 2010–2012, prior to introduction of the JPX400). This second difference accounts for any baseline differences between our treatment and control group that are associated with rank but stable over time. Unlike in traditional DID designs, in our setting a firm's treatment status varies over time: its rank, and thus its distance from the index-inclusion threshold, varies year by year. Moreover, managers do not have full control over these rankings, which depend on the stochastic nature of the firm's own performance as well as the performance of other JPX400-eligible firms. Therefore our research design in effect combines multiple "experiments" in the post-JPX400 period with multiple placebo "experiments" in the pre-period to infer the effect of inclusion incentives.

Our basic research design is summarized in the following DID specification:

$$ROE_{i,t+1} = \alpha + \beta_1 Treat_{i,t} \times Post_t + \beta_2 Treat_{i,t} + \gamma X_{i,t} + f_t + \epsilon_{i,t}, \tag{2}$$

where $Treat_{i,t}$ is an indicator equaling 1 for firms ranked 301–500 and 0 for firms ranked 501–800 in a given selection year t; $ROE_{i,t+1}$ is a firm's return on equity in the following fiscal year; $Post_t$ is an indicator for the period after introduction of the inaugural JPX400 constituents, equaling 1 for years 2014–2016 and 0 for years 2010–2012; $X_{i,t}$ is a vector of contemporaneous firm controls; and f_t represents time-fixed.

The main coefficient of interest, β_1, captures the mean ROE differences between the treatment and control firms in the post-JPX400 period, relative to the differences between placebo treatment and control firms in the pre-JPX400 period. Our identifying assumption is that any counterfactual differences in future ROE—that is, differences that would exist in the absence of JPX400-inclusion incentives—between firms near the inclusion threshold (treated) and those further from the threshold (controls) are stable over time and are thus accounted for by pre-period differences between placebo treatment and control firms.

We believe that this assumption is most defensible when conditioned on contemporaneous ROE, an important predictor of future ROE, because the distribution of contemporaneous ROE can change after introduction of the JPX400. Thus, our most robust specification—the specification on which we rely most heavily throughout our empirical analysis—includes contemporaneous ROE (or the lagged dependent variable more generally) as a control. Another way to interpret our main specification is that its DID estimate (β_1) identifies the treatment effect by comparing the mean differences in firm-level *changes* in ROE between the treatment and control firms in the post-JPX400 period to the mean differences in firm-level *changes* between placebo treatment and control firms in the pre-JPX400 period.

Finally, we note that the DID coefficients produced by this research design represent a conservative estimate of the JPX400-inclusion incentive effect for the treated group of firms. This is the case because our research design uses as controls those firms that are less influenced by index-inclusion incentives (those ranked 501–800)—effectively assuming that they are unaffected by those incentives. To the extent that these firms do respond to some degree to the incentives of JPX400, our DID estimates would be downward-biased.

3.3 Validation of Synthetic Ranks

Because the JPX publishes the index's constituents, but not the underlying rankings, our research design relies on a synthetic replication of JPX400 ranks. Thus, before proceeding to our DID analysis, we empirically validate our synthetic rankings. We estimate the following OLS regression:

$$JPX\ Member_{i,t} = \alpha + \beta Above\ Cutoff_{i,t} + \gamma X_{i,t} + \epsilon_{i,t}, \tag{3}$$

where $JPX\ Member_{i,t}$ is an indicator for actual JPX400 membership, $Above\ Cutoff_{i,t}$ is an indicator that takes a value of 1 for all firm-year observations for which our synthetic JPX400 rank is is 400 or less and zero otherwise, and $X_{i,t}$ is a vector of contemporaneous firm-level controls. To ensure that this analysis includes all false negatives, the regression sample includes all firms in the top 2,000 by trading value, for the selection years 2013–2016.

Table 1 reports the results of this analysis. Column 1 shows that synthetic rank of 400 or less is highly predictive of actual index inclusion. Column 2 adds firm-level controls to assess the extent to which our prediction errors, such as those due to "qualitative adjustments," are systematically correlated with firms' fundamentals. We find that *Log Market Cap* alone exhibits a significant relationship with the likelihood of JPX400 inclusion; no other firm attribute in this regression does. The loading on *Log Market Cap* is likely explained by the fact that the JPX400 is weighted toward large firms, so our false negatives are on average much larger than the full sample of 2,000 firms. Columns 3 and 4 use more granular synthetic rank ranges—1–200, 201–400, 401–600, and over 800—as the main predictors of interest. They show that the probability of actual inclusion decreases monotonically with our synthetic ranks, and, once again, the only firm-level control that additionally predicts inclusion is size. Collectively, these results show that our synthetic JPX400 rankings are highly valid, and our prediction errors are unlikely to cause systematic biases in our main results.

3.4 Summary Statistics

Table 2 reports pre-period summary statistics for the sample of firms used in our main analyses (firms with synthetic ranks of 301–800). The first five columns report the distributional statistics (quartiles, mean, and standard deviation) of covariates for the full sample; the rightmost three columns report the means for the treatment group (ranked 301–500) and the control group (ranked 501–800) and the t-statistics of their differences. Definitions of covariates appear in Appendix A.

It is noteworthy that our treated and non-treated firms differ in their means on several variables related to profitability and size, such as *ROA*, *Asset Turnover*, *Log NOA*, and *Log Market Cap*. This pattern is to be expected, since the treatment firms are ranked higher than the control firms, and the synthetic ranks are driven by size and profitability. These differences motivate our inclusion of linear controls for firm characteristics.

Note that, though we could have included firms ranked 1–300 or 801–1,000 in our control group, untabulated tests revealed that doing so accentuates the degree of covariate imbalance. Thus our baseline design combines a pre-estimation matching of similar firms with linear controls to account for local differences. As we explain below, we will utilize the holdout sample of firms later, in robustness tests to help rule out alternative hypotheses.

4 Empirical Analysis

This section presents empirical tests examining whether, how effectively, and through what incentive channels the JPX400 influenced Japanese corporate behavior. We also provide back-of-the-envelope estimates for the overall effect of the index on the Japanese equity market.

4.1 The JPX400 Inclusion Threshold and Future ROE

To test the hypothesis that firms closer to the threshold of JPX400 inclusion differentially improve their ROE, we estimate equation (2). Table 3 reports estimation results for various specifications of this DID regression, which compares the forward ROE of firms near the threshold of inclusion (the treated group of firms, ranked 301–500) to that of firms under the threshold of inclusion (the control group of firms, ranked 501–800).

Table 3, column 1, estimates a basic DID specification, without time- or industry-fixed effects and without any firm-level controls. The DID estimate of the treatment effect—the coefficient on $Treat \times Post$—of 2.8 percentage points is statistically significant at the 1% level, and represents a 48% increase in ROE relative to the pre-period treatment-group mean ROE of 5.85%.

Also noteworthy is the positive and significant (at the 1% level) coefficient on *Post*, an estimate of the secular trend in ROE. The point estimate of 0.018 implies that firms assigned to the control group had a 1.8-percentage-point-higher ROE in the post period, or a 35% increase relative to the pre-period control-group mean of 5.12%. In the context of this economically significant *Post* coefficient, the DID estimate can be interpreted in two ways. At one extreme, we can attribute all of the secular trend to the effects of the other contemporaneous governance reforms, such as the Corporate Governance Code and the Stewardship Code. If that attribution is correct, this DID estimate suggests that the incremental effect on ROE of being close to the index-inclusion threshold is at least as large as the effects of all other contemporaneous reform efforts. On the other hand, to the extent that the *Post* coefficient reflects in part the effects of the JPX400-inclusion incentives on firms ranked 501–800, our DID coefficient would be downward-biased.

Columns 2-5 present estimates from increasingly robust DID specifications relative to column 1. Column 2 replaces the *Post* indicator with time-fixed effects; column 3 adds industry-fixed effects; column 4 adds linear controls for contemporaneous firm attributes that can also potentially explain future ROE, specifically *Log Market Cap*, *Log Book to Market*, *Sales Growth*, *LT Debt to Equity*, and *Cash to Equity*; and column 5 includes contemporaneous ROE as an additional firm-level control. By including contemporaneous

ROE as a control, the treatment effect identified in column 5 is the effect of the index incentives on the *change* in future ROE.

Most notably, the coefficients on $Treat \times Post$ remain similar in magnitude and statistical significance across columns 2-5. By contrast, the adjusted R^2 of the regression increases from 2.19% in column 2 to 30.31% in column 5, mitigating concerns about omitted variable biases (Oster, 2017).

Column 6 examines an alternative specification that substitutes the log of gross ROE for ROE, both as the forward outcome variable of interest and the contemporaneous control. The DID coefficient remain statistically significant at the 1% level, suggesting that our effects are not driven by a secular proportional increase in ROE in the post-period, and is robust to alternate specifications.

We offer several ways of interpreting the magnitude of these documented effects, in particular the main effect documented in column 5—the most robust specification. First, in simulations, we find that this level of ROE improvement would make a significant difference in marginal firms' ranks and thus their probability of inclusion. This is in part because an improvement in ROE would affect firms' rankings along all dimensions—operating profits and market capitalization. We find that for firms that were included in the index and also in our treatment group, a reduction in profitability of this size (i.e., down to what the firm's profitability would be absent the estimated average treatment effect of 2.4% in ROE) would cost them an average of 83 ranks, holding other firms' behavior fixed.[11] Another benchmark is the pre-period treatment-group mean, against which the main effect of column 5 indicates a 40% proportional increase. Although this suggests a very large effect relative to the Japanese benchmark, we note that this likely reflects the relatively low ROE and the relatively large amount of slack in Japan. Put differently, relative to the U.S. benchmark cited in the (Ito, 2014) report, this main effect would close 15% of the ROE gap for our treatment firms.

[11] We calculate this quantity by estimating a firm-level profitability treatment effect (2.4% times contemporaneous common equity), and then re-calculating the firm's ranking after appropriately adjusting all of the components of the overall rank. Specifically, we subtract the estimated profitability effect from contemporaneous net income to re-calculate 3-year average ROE, subtract it after tax-adjustment to re-calculate 3-year total operating income, and adjust the company's market capitalization assuming its P/E ratio is held constant. We then re-rank each firm on each dimension and the composite score, holding all other companies' attributes fixed, and calculate and save each firm's change in overall rank.

4.2 Testing the Identifying Assumptions

As Section 3 explains, our empirical design relies on the identifying assumption that any counter-factual differences in future ROE between the treated and control firms are stable over time (i.e., follow parallel trends). Table 4 and Figure 1 empirically test this assumption central to our design.

We begin with two tests of the parallel-trends assumption. The first test assesses whether there is evidence of differential pre-treatment trends in ROE between the firms ranked 301–500 and those ranked 501–800, the presence of which would challenge the validity of assuming parallel trends *after* JPX400's introduction. Table 4, column 1, reports a specification that augments the specification in Table 3, column 5, with the following additional interaction variables: $Treat \times (Year = 2011)$ and $Treat \times (Year = 2012)$, where $Treat$ is defined as in equation 2, and $Year = 2011$ and $Year = 2012$ are indicators for the selection years 2011 and 2012. These interaction coefficients are insignificant both economically and statistically (at the 10% level), suggesting no evidence of differential pre-treatment trends and consistent with the parallel-trends assumption.

We also test for evidence of differential pre-treatment trends by implementing placebo tests using synthetically reconstructed ranks going back to 1994. Figure 1 graphs the results of five placebo DID estimates for five sets of seven-year sample periods prior to introduction of the JPX400. Following the precise setup and implementation of our main empirical test (see Table 3, column 5), in each year we rank firms based on the JPX400 selection algorithm and the composite score of equation (1), and create placebo treatment and control indicators as in our main tests. Then we take seven-year samples, drop year 4 from the analysis, and define the final three years of the sample as the post period; finally, we estimate the DID specification. Under the identifying assumption, we expect to find placebo DID estimates that are statistically no different from 0. Indeed, Figure 1 shows that none of our placebo DID estimates are statistically different from 0, and that most of the point estimates are close to 0, providing further support for the parallel-trends assumption.

Although we find evidence that the differences in ROE between those firms ranked 301-500 and 501-800 were stable in the years prior to the introduction of the JPX400, it is possible in theory that the counter-factual trends between the treated and control firms diverged *after* introduction of the index. For example, if an economy-wide shock in 2013 causes a secular proportional increase in ROE in subsequent years, firms with higher pre-period ROE (i.e., higher JPX400 rank) would experience a larger increase in the level of ROE in the post period. More generally, after 2013 some omitted variables could have changed in ways that are correlated with the JPX400 ranks and could thus explain the relative increase in ROE we document in treated firms.

We note first that Table 3, column 6, shows that our results also hold for forward log of gross ROE, suggesting that our findings are not driven by proportional secular increase in ROE. To address this possibility more generally, Table 4, column 2, reports the estimates from a specification in which we split our treatment indicator in two—one indicator for the higher-ranked firms (with ranks within 301–400) and the other for those ranked lower (401–500)–and compare the DID coefficients. If the treatment effects we document were due to differential trends that were correlated with the JPX400 ranks *after* the introduction of the index, we would expect the treatment effect to be higher for those firms ranked 301–400. We find, however, that the estimated DID coefficients for the two types of treatment firms are nearly identical to each other—0.027 for those ranked 301–400 and 0.024 for those ranked 401–500—and to our baseline DID estimates in Table 3. Both coefficients are statistically significant at the 5% level; they are not statistically different from each other.

Column 3 reports the results of an alternative specification further addressing the possibility that some omitted factor, correlated with the firms' rank, could be driving our main result. In this specification, we also include firms ranked 1–300 as contemporaneous placebo treatments. Again, if the treatment effects in ROE that we document are driven by differential trends that are correlated with the JPX400 ranks *after* the introduction of the index, we would expect to see a coefficient on *Rank 1–300* $\times$ *Post* that is positive, significant, and larger in magnitude than the coefficient on $Treat \times Post$. However, we find that the coefficient on *Rank 1–300* $\times$ *Post* is (1) statistically no different from zero at the 10% level, and (2) statistically different, at the 1% level, from the coefficient on $Treat \times Post$, which remains at 0.023. Thus, firms ranked 1–300 exhibit no differential response in ROE as compared to firms ranked 501–800.[12]

In column 4 we conduct an alternative cross-sectional placebo test. Recall that the JPX400 selection algorithm begins by filtering down to the top 1,000 firms in Japan by

[12] In untabulated tests, we considered a specification in which firms ranked 1–300 are also included as controls. This alternate specification yields an estimated treatment effect of 0.019, which is significant at the 1% level.

market capitalization and liquidity. Thus we can estimate synthetic ranks and implement a placebo test on the ineligible firms outside of this cutoff—those ranked 1,001 to 2,000 by market capitalization. We rank these firms according to equation 1, construct placebo treatment and control groups using these ranks (those ranked 301–500 are considered treated, those ranked 501–800 controls) and re-run our main test. If some omitted factor associated with the algorithm's rank-ordering were driving our results, we would expect the effect to appear in this placebo sample of smaller firms. But the estimated coefficient on *Placebo Group Treat*×*Post* in column 4 provides no such evidence, consistent with the findings in columns 2 and 3 and supportive of the parallel-trends assumption. We thus conclude that it is unlikely that some omitted factor correlated with JPX400 ranks in the post period confounds our main results.

These results also constitute evidence in support of a second critical assumption behind our empirical strategy: that firms experience stronger incentives to improve ROE when they are nearer to the threshold of index inclusion. In particular, in column 2 and 3 we show that firms on either side of the threshold of inclusion improved ROE by virtually identical magnitudes. Moreover, they show that firms further below the threshold (ranked 501–800) and those ranked highest in the index (ranked 1–300) exhibit similar ROE improvement, which is significantly lower than the improvement experienced at firms near the threshold of inclusion.

We provide a final empirical test to examine how index-inclusion incentives ("treatment intensity") vary with firms' distance from the threshold of inclusion. We create a more continuous measure of treatment intensity by sorting the 1,000 "ranked set" firms into five quintiles, based on the negative of the absolute value of their distance from the rank-400 cutoff. The resulting variable, *Quintile(Closeness)*, ranges from 0 to 4 and is *increasing* in proximity to the JPX400 cutoff: higher values represent more intense index-inclusion incentives. Column 5 reports the results of our estimates using the entire ranked set and this alternative treatment measure. We find a positive and significant coefficient (at the 5% level) on *Quintile(Closeness)*×*Post*. Together with the results in columns 1-4, these findings are consistent with the observed effects on ROE being driven by firms' desire to gain, or to avoid losing, membership in the index.

4.3 Consequences of Index Inclusion: ROE, Sales Growth, Compensation, and Capital Market Outcomes

Next, we study the consequences of realized inclusion in the JPX400, for two main reasons. First, this analysis allows us to establish more precisely whether our main effect in ROE is indeed driven by index-inclusion incentives or is an outcome of index-inclusion itself. Second, it allows us to better understand *why* managers may have been motivated to pursue or maintain membership in the JPX400. In Table 5 we report the results of regressions estimating the effects of index inclusion *per se* on firms' *ROE*, *Sales Growth*, *Executive Compensation*, *Employee Compensation*, *Illiquidity*, and *BM*.

For these tests, we use a regression specification motivated by the empirical design in Appel et al. (2015) but tailored to our setting. We regress the dependent variable of interest on *JPX Member*, an indicator for whether the firm was actually included in the JPX400 in that year, *Centered Rank*, the firm's total rank minus 400, *Log Market Cap*, and *ROE*, and time and industry dummies. We restrict our analysis to a narrow bandwidth of 50 firms on each side of the rank threshold for inclusion in the JPX400, i.e., firms ranked 350-450.[13] Our identifying assumption is that, after controlling for the major determinants of index inclusion (synthetic rank, Market Cap, and ROE), variation in JPX status for firms within this narrow bandwidth is plausibly exogenous to the outcome variables we examine, and, consequently, the coefficient on *JPX Member* identifies the effect of inclusion in the index *per se*.

Table 5 tabulates the results of this analyses. The result in column 1 indicates that firms did not enjoy a statistically measurable improvement in their ROE simply as a result of index inclusion. Columns 2 through 6 all additionally report null results: There is no statistically measurable benefit in sales, executive compensation, average employee pay, liquidity, or book-to-market ratio.[14]

[13] We lack required data for a total of 2 firm-years, leaving 301 observations for each specification.

[14] In untabulated tests, we find very similar results, both qualitatively and quantitatively, when we examine the consequences of index inclusion in a "fuzzy" regression discontinuity design (FRD), using a larger bandwidth of firms around the rank threshold of 400, and instrumenting for *JPX Member* with an indicator for crossing the rank threshold. All of the coefficients on *JPX Member* in the FRD are not statistically significant at the 10% level, with wider confidence intervals. We choose the relatively simpler approach as our primary design, because it allows us to restrict our test to a narrower bandwidth of firms around the threshold, thus plausibly achieving a closer match between included and excluded firms, without facing the weak instrument problems that might affect a FRD in a narrow bandwidth (Appel, Gormley, and Keim,

That firms and managers do not appear to derive any measurable benefits on these dimensions from JPX400 index-inclusion may at first appear surprising—but these results can be explained by institutional features and by recent work on the capital-market effects of indexes. Though the JPX400 was seen as highly prestigious by the Japanese business, finance, and political communities, it was less salient for ordinary Japanese consumers. Thus, it is not clear that mere inclusion in the index would generate significant product-market benefits (e.g., pricing-power and brand-value benefits) that could drive improvements in *Sales Growth* and *ROE*. That we do not find significant effects on *Executive Compensation* can be explained by the fact that CEO pay-to-performance sensitivity is very low in Japan, as had been documented in prior literature (Nakazato, Ramseyer, and Rasmusen, 2011; Kato and Kubo, 2006) and as we confirmed in untabulated tests.[15] Thus, even if shareholders and directors were pushing managers to achieve index inclusion, we would not expect success to yield significantly higher managerial compensation.

The lack of direct capital-market benefits—in terms of *Illiquidity* and *Book-to-Market*—may appear to be the most puzzling finding, particularly given evidence on the price effects of index inclusion in the United States (e.g., Shleifer, 1986; Chang, Hong, and Liskovich, 2015). Again, a closer examination of the institutional dynamics in the Japanese equity markets help to explain these findings.

In theory, inclusion in a stock index would improve liquidity and valuation multiples to the extent that it significantly changes the relative demand for a stock. However, inclusion in the JPX400 *per se* is unlikely to generate such a significant increase in relative demand. First, since 2013 there has been a large secular increase in total demand for Japanese equities, driven by two factors: (1) the Bank of Japan's (BOJ) quantitative-easing program, in which it significantly increased domestic equity holdings by purchasing index-linked ETFs (in quantities amounting by the end of 2016 to $16 trillion Yen); and (2) the GPIF's decision

2016).

[15]We found that top managers at our treatment firms did not earn significantly more than their counterparts at control firms. In untabulated tests, we estimated a standard pay-to-performance regression using Log Avg Exec Pay as the dependent variable and *Annual Return* as the main explanatory variable, and tested for differences in this sensitivity between time periods and between treated and control firms. We found very low average pay-to-stock-return-performance sensitivity in Japan. Among the control firms in the pre-period, a 100% increase in the stock price is associated with a mere 4.4% increase in pay. Estimating a similar specification using U.S. executive-compensation data, we found a pay-to-stock-return-performance sensitivity of 20%, about 4.5 times as large as in Japan. Finally, we did not find evidence that this sensitivity changed after introduction of the JPX400, for either the treatment or control group of firms.

to increase the proportion of assets under management (more than $130 trillion Yen) allocated to equity investments, from a target of 12% to 25% (Barbon and Gianinazzi, 2017). In both cases, the increase in total demand benefits all firms belonging to the TOPIX index, which remains the most frequently tracked by institutional investors among indexes in Japan, and especially firms belonging to the Nikkei225, the index that the BOJ's program heavily targeted. Although the BOJ and the GPIF both allocated some amount of their capital to the JPX400, they are relatively insignificant. For example, only 4% of the BOJ's purchases flowed to ETFs tracking the JPX400; 53% flowed to ETFs tracking the Nikkei225 and 43% flowed to ETFs tracking the TOPIX (Barbon and Gianinazzi, 2017). Similarly, the GPIF only benchmarked approximately 6% of its domestic equity portfolio to the JPX400 (vs. 74% benchmarked to the TOPIX).

These institutional factors imply insignificant changes in relative demand for JPX400 stocks, in light of the significant rise in total demand for Japanese stocks in the TOPIX (which includes all of the JPX400 firms) and the Nikkei225. In fact, our computations suggest that inclusion in the JPX *per se* contributed only approximately 14% of the total increase in demand due to the BOJ's quantitative easing and the GPIF's increase in allocation to equity. Also, recent work on ETFs challenges the assertion that inclusion in an index leads to capital-market benefits. For example, this research has found that increased ownership due to ETFs reduces liquidity (Hamm, 2014), increases non-fundamental volatility (Ben-David, Franzoni, and Moussawi, 2018), reduces information-production incentives (Israeli, Lee, and Sridharan, 2015), and creates excess co-movement in stock returns (Da and Shive, 2018). Jointly, these factors could explain why we do not observe empirical evidence of significant capital-market benefits from inclusion in the JPX400 *per se*.

4.4 Consequences of Index Inclusion: Media Attention

The results reported in section 4.3 indicate that Japanese firms and managers did not gain statistically measurable *immediate* financial or monetary benefits from realized inclusion in the index. This naturally raises the question of why Japanese managers and firms were motivated to exert effort to attain membership. One possibility—one consistent with our interviews with investors and with the JPX400's nickname (the "shame" index)—is that managers were motivated by reputation concerns: the prestige of inclusion and fear of the shame of exclusion.

We leverage a long literature in finance and economics (e.g., Dyck et al., 2008; Dai et al., 2015), which has documented that media coverage can serve as an alternative mechanism for disciplining managerial behavior by influencing their reputation costs, and we examine whether the media's attention on firms responded to changes in their JPX membership status. Our empirical design is very similar to that in Table 5, except that instead of JPX400 membership, our dependent variables of interest are *Booted*, an indicator that evaluates to 1 if the firm had been included in the index in the previous year and excluded in the current year, *Added*, which evaluates to 1 if the firm was excluded last year and newly added, and *Switch*, which evaluates to 1 if the firm was either booted or added. As in Table 5, we use firms ranked 350-450, in years 2014-2016. (We drop 2013 because all 400 included firms have their inclusion status change, and none are booted out, in that year.) In columns 1-3, the dependent variable is *No. of Media Articles*, a simple count measure of the number of unique "full-articles" identified by Ravenpack in which the firm was mentioned between July and September of each year.[16] To ensure that we identify articles directly related to the firm in question, in columns 1-3 we restrict ourselves to articles with a Ravenpack relevance score of greater than 75. In columns 4-6, we examine an alternative measure of media attention—*Weighted No. of Media Articles*—which relaxes the relevance criterion and instead use a weighted measure, weighting each article by the relevance score. In all columns, we include time and industry fixed effects, and linear controls for the primary determinants of JPX membership status—*Centered Rank*, *Log Market Cap*, and *ROE*. As before, our identifying assumption is that firms ranked 350-450 are, by definition, closely matched on the determinant of index inclusion, and, after adding controls, realized inclusion status is approximately "as if random."

The positive and significant loading on *Switch* reported in column 1 indicates that changes in firms' inclusion status generated greater media mentions. The estimated effect size—an additional 36 articles mentioning the firm in news sources tracked by Ravenpack—is substantial, representing an 82% increase relative to the baseline for firms with simulated ranks 350-450 in 2013, before the index was created. Column 2 reports the results of a specification that

[16] We begin enumerating media articles in July, when all information necessary to predict index inclusion is publicly available, to capture media attention in anticipation of the index turnover. We also enumerate media articles in the three months after the August 1st announcement date to capture articles that respond to the index turnover.

substitutes *Switch* with separate indicators for *Added* and *Booted*. The insignificant F-stat (with an associated p-value of .711) indicates that we cannot statistically distinguish whether firms received more "shame" due to exclusion vs. prestige due to inclusion, as measured by their media mentions. Next, we estimate the heterogeneity in the effect for Nikkei225 firms. The Nikkei225 has long been Japan's leading stock index, closely tracked by institutional investors; it consists of Japan's largest, most-established, best-known, and most liquid firms in 36 industries. Consequently, we hypothesize that the JPX400 inclusion status of Nikkei225 firms is likely to receive greater media attention than that of non-Nikkei225 firms. Column 3 interacts *Switch* with an indicator for *Nikkei225* firms and includes a Nikkei-specific intercept. The estimated coefficient on *Nikkei225 x Switch* is large—representing 132 additional media mentions—and significant: Nikkei firms received markedly more incremental media attention as a result of being added or booted from the JPX400. Columns 4-6 report the results of specifications following columns 1-3, but with the weighted measure as the dependent variable. The results here are qualitatively similar, though statistically stronger. Once again, there is a large, significant coefficient on *Switch*; we cannot statistically distinguish whether being *Booted* or *Added* generated a larger media response; and the media response to *Switch* is notably higher for Nikkei firms than non-Nikkei.

Collectively, these results provide suggestive evidence in favor of the idea that inclusion in the JPX400 had implications for firms' reputations—media institutions paid attention to and reported on which firms were booted out or newly included. They also suggest that this scrutiny was especially pronounced for Nikkei225 firms.

4.5 Exploring the Incentive Channels: Nikkei vs. Non-Nikkei Firms

Overall, our findings in 4.3, and 4.4 suggest that the *ex post* consequences of JPX inclusion were potentially reputation related and did not include significant immediate financial or capital-market benefits. It remains possible, however, that firms coveted membership in the index because they *expected* to realize benefits, such as greater liquidity or lower cost of capital *ex ante* (both realistic expectations, given that the GPIF promised to track the index and that other institutional investors could be expected to follow suit). Moreover, it is plausible that firms expect capital-market or product market benefits to manifest over the longer-term, as the JPX index becomes more popular.

To further disentangle whether firms exerted effort in anticipation of *expected* financial benefits or may have responded to reputation concerns, we examine cross-sectional variation in the treatment response in ROE between Nikkei225 and non-Nikkei225 firms. As we argue, and document, in the previous section, the inclusion status of Nikkei225 firms is likely to face greater public scrutiny, and thus, Nikkei225 firms are more likely to be attuned to status or reputation concerns. On the other hand, since Nikkei225 firms already enjoyed the highest liquidity in the Japanese market, the *incremental* capital-market benefits of inclusion in the JPX400 would presumably be more salient for, and appear greater to, non-Nikkei225 firms. Thus, we would interpret a higher treatment response from Nikkei225 firms as evidence for the "status-incentives" hypothesis; a higher treatment response from non-Nikkei225 firms would be evidence for the "expected capital-market benefits" hypothesis.[17]

Table 7 reports the results of our empirical tests of this hypothesis, using two different specifications and our two treatment-intensity measures (the binary indicator and the *Quintile(Closeness)* measure. The first pair of columns (1 and 2) employs our main DID specification, but subdivides our treatment-group indicator into *Nikkei225* and *non-Nikkei225* subgroups. More formally, we estimate the equation:

$$\begin{aligned} ROE_{i,t+1} &= \alpha + \beta_1 Treat_{i,t} \times Nikkei225_{i,t} \times Post_t \\ &+ \beta_2 Treat_{i,t} \times non\text{-}Nikkei225_{i,t} \times Post_t + \beta_3 Treat_{i,t} \times Nikkei225_{i,t} \\ &+ \beta_4 Treat_{i,t} \times non\text{-}Nikkei225_{i,t} + \gamma X_{i,t} + f_t + \epsilon_{i,t}. \end{aligned} \quad (4)$$

This specification allows for treatment-effect differences but assumes no Nikkei225- and non-Nikkei225-specific effects; that is, it does not allow for the control firms to differ in ROE based on Nikkei225 status. In this regression, we are interested in the sign and significance of the two triple-interaction coefficients and the test for their equality. Under the status-incentive hypothesis, β_1 should be larger in magnitude than, and statistically different from,

[17] One way to think about this is that the treatment effect on non-Nikkei225 firms represents an upper-bound estimate on the treatment response in Nikkei225 firms that could have resulted from expected capital-market benefits alone, because (a) non-Nikkei225 firms probably also have status concerns and (b) non-Nikkei225 firms' expected capital-market benefits from index inclusion are probably greater than those of Nikkei225 firms. Conversely, the treatment effect on Nikkei225 firms represents an upper-bound estimate on the treatment response from non-Nikkei225 firms that could have resulted from status concerns alone.

β_2.

The next pair of columns (3 and 4) employs a difference-in-difference-in-differences (DDD) design, once again implemented using both our binary treatment indicator and the *Quintile(Closeness)* measure. Specifically, we estimate the following equation:

$$\begin{aligned} ROE_{i,t+1} \;=\; & \alpha + \beta_1 Treat_{i,t} \times Nikkei225_{i,t} \times Post_t + \beta_2 Nikkei225_{i,t} \times Post_t \\ & + \beta_3 Treat_{i,t} \times Post_t + \beta_4 Nikkei225_{i,t} + \beta_5 Treat_{i,t} + \gamma X_{i,t} + f_t + \epsilon_{i,t}. \quad (5) \end{aligned}$$

This test is more general because it allows for Nikkei225- and non-Nikkei225-specific effects. In this regression, we are interested in the sign and significance of the triple-interaction coefficient. Under the prestige-incentive hypothesis, β_1 should be positive and statistically significant.

In these tests, we use the expanded data set spanning the entire ranked set of 1,000 firms, for two main reasons: first, the *Quintile(Closeness)* treatment measure was originally defined on the full set of ranked firms (1–1,000); second, the expanded sample enhances the statistical power with which we estimate the parameters of (and conduct hypothesis testing using) the relatively more demanding triple-interaction models, particularly given the relatively sparse presence of Nikkei225 firms in the original control group, firms ranked 500–800.

Columns 1 and 2 of Table 7 report the results of estimating equation 4 using our binary treatment measure and the *Quintile(Closeness)* treatment measure respectively. In column 1, the estimated coefficient of 0.066 on *Treat*×*Nikkei225*×*Post* is six times the estimated coefficient of 0.011 on *Treat*×*non-Nikkei225*×*Post* (a difference that is statistically significant at the 5% level), consistent with prestige incentives being the primary driver of the overall ROE effects. Similarly, in column 2, the estimated treatment response of Nikkei225 firms is larger in magnitude—over three times as large—and statistically different from that of non-Nikkei225 firms at the 5% level.

Columns 3 and 4 report coefficient estimates from equation 5. In column 3, using our binary treatment measure, we report a coefficient of 0.050 on the triple interaction, *Treat*×*Nikkei225*×*Post*; the DDD estimate is both economically and statistically significant. The point estimate indicates that the treatment response for the Nikkei225 firms is more than 5 percentage points higher than that of the non-Nikkei225 firms; moreover, these results suggest that the DID estimate for Nikkei225 firms is statistically different from the DID estimate for non-Nikkei225 firms at the 5% level. Column 4 repeats this exercise using the *Quintile(Closeness)* treatment measure. We find a positive and significant (at the 10% level) coefficient on the triple interaction, *Quintile(Closeness)*×*Nikkei225*×*Post*, further evidence of a stronger treatment response for Nikkei225 firms.

Collectively, our results provides evidence for managers' concerns for status and reputation playing an important role in explaining why certain managers (e.g., those in Nikkei225 firms) were incentivized to obtain JPX400 membership. However, our findings do not necessarily rule out the expected capital market benefit hypothesis, in particular for explaining why other managers (e.g., in non-Nikkei225 firms) may have been motivated to get into the index. We believe this could be a fruitful avenue of research as data on long-run consequences of JPX400 inclusion become available.

4.6 Drivers of the ROE Response

Next we study which levers managers pull to generate the improvement in ROE that we document. Table 8 reports the results of regressions examining the behavior of the major drivers of ROE. In columns 1-6, we replicate our main baseline specification (Table 3, column 5), but use forward *Sales Growth*, *ROA*, *Profit Margin*, *Asset Turnover*, *Leverage*, and *Shareholder Payouts*, respectively, as dependent variables.

The results in Table 8 suggest that improvement in ROE is predominantly driven by improvements in operational efficiency (as measured by ROA) rather than changes in capital structure. Improvements in ROA, in turn, appear to be on average driven by increased margins rather than asset turnover or sales growth. Column 1, shows no significant treatment effect (at the 10% level) on *Sales Growth*. Column 2 reports a statistically significant (at the 5% level) treatment effect on *ROA* of 0.53 percentage points, a 17% increase over the treatment group's pre-period mean of 3.1%. Column 3 suggests that this increase in ROA is primarily driven by improved *Profit Margin*: the statistically significant (at the 5% level) treatment effect estimate of 0.0074 constitutes a 15.3% increase in margins relative to their pre-period average of 4.9%. Column 4 reports that there is no statistically measurable treatment effect on *Asset Turnover*, suggesting that this was not a dominant channel for the documented improvement in ROA. In untabulated tests, however, we find that treatment firms with more "slack" in this dimension—those with below-median asset turnover—did improve their efficiency. But even for this subsample, the effect on asset efficiency is much smaller than the treatment effect in profit margin.

As indicated by the statistically insignificant coefficient in Column 5, we find no evidence that financial leverage was impacted by firms' JPX400-inclusion incentives. This finding is not entirely surprising, since JPX400's ranking algorithm does not necessarily incentivize firms to increase leverage *per se*. Under Modigliani and Miller (1958) assumptions, a profitable firm making a simple *ceteris paribus* increase in financial leverage—such as by issuing debt and repurchasing shares—would increase the company's ROE but might decrease its total market capitalization, a component of the selection algorithm.

It is also possible that our measure of financial leverage (net debt over equity) is too noisy a measure to capture JPX400's potential effects on firms' capital-structure decisions. Therefore, we complement the analysis of financial leverage by examining payout policy, using shareholder payout ratio (i.e., dividends plus repurchases divided by shareholder's equity) as the dependent variable of interest. Treated firms could increase shareholder payouts to boost their ROE, by reducing retained earnings and the book value of shareholders' equity, particularly if doing so leads shareholders to increase valuation multiples (thus dampening the potential tradeoff in market capitalization). In this way we can interpret shareholder payouts as a financing choice that firms can employ to boost ROE, but where we are more likely to find a statistically measurable effect than in leverage. This variable is also of interest given that, as Ito (2014) asserts, Japanese policy makers were eager to change Japanese corporations' cash-hoarding cultures.

As reported in column 6, we find a statistically significant effect (at the 5% level) on shareholder payouts overall. This effect size—0.59 percentage points—is large relative to the baseline, representing approximately a 21% increase relative to the pre-period treatment-group mean. Relative to total cash on treatment firms' balance sheets, however, the magnitude of the effect is quite small, representing only 1.5% of the treatment-group pre-period cash-to-equity ratio. This finding might explain why an effect driven through the shareholder-payouts channel is statistically difficult to detect by examining financial leverage. Another possibility is that the JPX400's positive effects on earnings had a countervailing and positive effect on total cash, lowering financial leverage.

Jointly, these findings provide further support for the thesis that firms improved ROE in response to index-inclusion incentives, and shed light on the channels through which managers sought ROE improvements. The analysis in Table 8 shows that the overall effects on ROE are driven in part by operating changes—improvements in ROA, predominantly driven by improvements in margins—and in part by increasing shareholder payouts.

4.7 Accruals, Investments, R&D, and Employee Outcomes

We further examine whether the index-inclusion incentives have produced potentially undesirable effects. For example, managers could have improved accounting earnings by manipulating accruals or by cutting productive investments (e.g., Stein, 1989; Healy, 1985), contrary to the JPX400's broader goals of improving capital efficiency and economic improvement.

Table 9 explores these possibilities by estimating the incentive effects of JPX400 inclusion on six different forward outcome variables of interest: *Accruals to Assets*, *Log NOA*, *R&D to Sales*, *Log Employees*, and *Log Average Employee Pay*. Each regression in this table employs the main specification from Table 3, column 5: DID in the dependent variable of interest with firm-level controls and a control for the lagged dependent variable.

Column 1 reports the results of the specification with *Accruals to Assets* as the dependent variable. We use total income-statement accruals as a proxy for earnings management. The coefficient on $Treat \times Post$ is not statistically significantly different from zero, suggesting that the increase in ROE is not driven by accruals-based earnings management. To the extent that such accruals earnings management is expected to reverse, we would not necessarily expect a manager to boost ROE in this way if the ultimate objective is to maximize the duration of the company's JPX400 membership.

An alternative may be for managers to change their real investments, for example by shedding loss-making assets, increasing market capitalization through acquisitions, or by reducing R&D expenses. We begin by examining the behavior of net operating assets. Column 2 reports the results of the specification with *Log NOA* as the dependent variable. Once again, we find no statistically significant treatment effect on *Log NOA*, suggesting that the JPX400 did not drive a statistically measurable change in investments in net operating assets. This suggests that managers were not on average cutting investments in operating assets. In addition, together with our evidence on net shareholder payouts, this result also suggests that firms did not seek to increase market capitalization as a primary means of attaining index inclusion.

Column 3 examines how firms altered R&D intensity (R&D-expense-to-sales ratio) in response to JPX400-inclusion incentives. R&D is often seen as having positive externalities and long-term benefits that are not captured by the accounting system, but managers might cut R&D—as a discretionary expense item—in order to boost reported earnings (e.g., Roychowdhury, 2006). R&D expenditures could thus potentially entail a tradeoff between ROE and the macroeconomic goals of Japanese policy makers. The point estimate on $Treat \times Post$ in column 3 of -.0011 is significant at the 10% level and indicates a 7.05% reduction in R&D intensity relative to the treatment group's pre-period mean of 1.56%. This effect also explains about 15% of the average treatment effect on profit margin improvement.

Next we examine how firm-level employment responds to the incentives generated by the JPX400. Japanese firms are well known for having historically had an implicit system of lifetime employment. This system has been weakened in recent years, but Japanese firms remain more reluctant than Western firms to downsize employment. Some policy makers view these employment norms as a barrier to dynamism and growth; others see them as providing social-welfare benefits to Japanese workers. Either way, any employment effects attributable to JPX400-inclusion incentives are policy-relevant. Column 4 reports the result of the main DID specification, with *Log Employees* as the dependent variable. We find no evidence of statistically or economically significant changes in firm-level employment, suggesting that our main ROE result is unlikely to be driven by employee downsizing.

Finally, column 5 examines the impact of the index on average employee pay. The coefficient on $Treat \times Post$ is insignificant, suggesting that our main effects on ROE are unlikely to be driven by a cut in mean employee compensation at treated firms. Collectively, the results in Table 9 suggest that the main ROE effect we document is not driven by significant accounting-based earnings management or by significant cuts to capital investments in operating assets, employment, or employee compensation. The ROE effects are in small part due to cutting discretionary expenses such as R&D. We are unable to ascertain, however, whether these cuts represent a form of real earnings management or an improvement in R&D discipline.

Overall, our evidence suggests that although the JPX400 may have been effective in changing certain firms' behaviors towards the policy goals of improving ROE, it also produced potentially undesirable consequences. Although cutting discretionary expenses such as R&D does not appear to be the main driver of our documented effects in ROE, it will be interesting to examine how managerial strategies for improving ROE evolve in the long run, particularly as the slack in operations tightens.

4.8 Market Valuation

In light of the finding that JPX400-inclusion incentives drove firms to improve ROE in part by engaging in potentially undesirable behaviors, our final analysis focuses on the market's assessment of these improvements. Table 10 reports the results of our main DID specification using *Log Book to Market* as the outcome variable of interest. Column 1 reports the results of a reduced-form OLS DID with firm-level controls and time-fixed. The negative, statistically significant coefficient on $Treat \times Post$ in this column indicates that treatment firms experienced a relative improvement in their valuation multiples. The point estimate of -.038 represents a 3.7% decrease in book-to-market relative to the pre-period treatment-group mean of 1.018. In conjunction with our analysis in Table 5, our findings suggest that the improvement in ROE for treatment firms, not inclusion in the index *per se*, led to an upward revision in the market's expectations about their future cash flows, in spite of the decline in R&D intensity. Moreover, these results suggest that this revision was not compensated for by a commensurate increase in expectations of firm risk.

Columns 2 and 3 supplement this analysis by implementing a two-stage least-squares estimation of the effect of *ROE* on *Log Book to Market*, using the DID interaction as the instrument for *ROE*. Column 2 reports the results of the first stage of the estimation, which is similar to our main result in Table 3. Column 3 reports the results of the second stage, which in effect regresses *Log Book to Market* on the predicted values of *ROE*. The negative, statistically significant coefficient on *ROE* indicates that a 1-percentage-point increase in ROE due to the JPX400-inclusion incentives is estimated to yield a decrease in *Log Book to Market* of -1.823. In other words, a 2.4-percentage-point increase in *Forward ROE*—our most robust DID estimates in Table 3—is expected to yield a decrease in *Forward Book to Market* of 0.0438, which is a 4.3% decrease relative to the pre-period treatment-firm mean and is consistent with the reduced-form estimates in column 1. Overall, our evidence suggests that firms enjoyed an expansion in multiples as a result of their on-average increase in ROE due to the JPX400.

4.9 Discussion

In this section, we attempt to synthesize our various empirical results into an overall evaluation of the JPX400 as a policy tool. We begin by trying to quantify the aggregate wealth effect of the index. For this purpose, we estimate a simple DID in forward net income using our baseline treatment and control groups, and find an on-average annual firm-level improvement in net income of JPY6.0 billion. Aggregating across the 200 firms in the treatment group implies an aggregate net income effect of JPY1.2 trillion per year. This amount represents an 8.9% increase relative to pre-period aggregate income across all Japanese public firms, and about 16% of the overall increase in Japanese corporate profits over our post-period

To estimate the total wealth or valuation effects, we multiply this estimated net-income effect by a range of incremental price-to-earnings (P/E) multiples. A very conservative ratio on the new profits generated by the JPX400 would be 1. Under this assumption, the total wealth effect for the three years in our post-period would be JPY3.6 trillion, or a 0.77% increase relative to the overall Japanese market capitalization in 2014. A less conservative incremental P/E ratio would be the treatment firms' cash-adjusted P/E ratio: $(MCap - Cash)/NetIncome$.[18] This would imply a valuation impact of JPY61.45 trillion or approximately 13.10% of total market capitalization in June 2014. The midpoint of those two estimates, 6.94%, would account for about 20% of the overall market-capitalization growth in Japan since the introduction of the index. These back-of-the-envelope estimates are assumption-dependent and, as such, only approximations of the magnitude of the JPX400's effects. At face value, they suggest that the creation of the JPX400 had a meaningful aggregate impact.

Our analysis also suggests possible flaws or tradeoffs in the design of the JPX400 index.

[18] Applying an incremental P/E of 1 to the new profits would be appropriate if the markets perceived the earnings boost as completely unsustainable and not likely to generate any internal alpha if reinvested. The alternative, firms' cash-adjusted P/E, is justified under the assumptions that (a) the market values Japanese corporations' cash holdings 1-for-1, such that the cash-adjusted P/E ratio captures the market's valuation of the companies' earnings; and (b) because the market perceives the income effect as sustainable, expected growth of and discount rates on the incremental profitability remain fixed. The average cash-adjusted P/E multiple for treated firms in the post period is 17.07.

First, only a limited number of Japanese firms were "in the mix" and thus incentivized to compete for inclusion each year. We find that firms' efforts to improve ROE decline with their distance from the threshold of inclusion/exclusion (as in Table 3 column 5), and that, using firms ranked 501-800 as a benchmark, "safe" included firms (ranked 1-300) generate no measurable improvement in ROE (Table 3, column 3). If the regulators had designed the index rules so that more firms had a realistic probability of winning or losing inclusion, it is possible that more firms would have exerted effort to improve their performance (though there could be tradeoffs in widening the "incentive bandwidth" in this way).

Second, as we show in Table 9 column 3, treated firms reduced their R&D intensity to gain inclusion into the index. Although doing so may improve short-term ROE, the reduction in R&D may reduce firms' long-term profits and generate externalities that hamper aggregate economic growth (Romer, 1990). Although the market valuation results in Table 10 show that the market did not perceive treated firms' overall efforts to be value destructive to these firms, we cannot rule out the possibility that these foregone R&D investments could have produced externalities that would harm aggregate growth and health of the Japanese economy, the central goals of Abenomics. It is plausible that over a longer time horizon, as firms exhaust relatively easy avenues for improving profitability, competition for inclusion in the JPX400 might induce firms to further seek undesirable channels to boost ROE. Put differently, although the index appears to be effective in incentizing managers to change behavior, the focus on ROE as a performance metric could ultimately lead managers to respond in value destructive ways.

5 Conclusion

In this paper, we provide evidence that the *ex ante* JPX400-inclusion incentives were effective in changing the behavior of Japanese firms close to the margin of index inclusion. Our evidence is consistent with the theory that, in a setting where formal incentives are limited in their ability to influence behavior, alternative governance mechanisms could be effective.

By leveraging the certification provided by the GPIF and reinforced by the media, the JPX400 represented a virtuous club of Japan's best-run companies that could have motivated certain managers to change their behavior to attain membership. Our evidence suggests that one explanation why some managers improved ROE to obtain index membership is due to their concerns for status and reputation. However, it is also possible that other managers were incentivized by their expectations of long-term capital market benefits.

The long-run effects of the JPX400 on Japanese corporations remain to be seen. Goode (1978) observes that changes in social norms tend to be precipitated by a shift in the behavior of a small group of respected elites. Thus one possiblity is that by incentivizing some of the most well-established and respected firms in the Japanese market (e.g., in the Nikkei225) to change their behavior, the JPX400 index mechanism could serve to coordinate a broader shift in corporate norms to be more shareholder friendly. However, our analyses also suggests that, although the index was effective in incentivizing certain firms to improve ROE, firms' responses may not have been entirely consistent with policy goals. Consequently, another possibility is that the index's focus on ROE may increasingly lead to perverse and myopic behavior (e.g., more dramatic cuts in R&D or other forms of real or accruals earnings management) that harms aggregate economic growth in the long run.

While our analysis cannot speak to the long-run efficacy of the JPX400, our findings point to the potential of alternative corporate governance mechanisms. These findings can inform corporate-governance reform efforts in capital markets with similar patterns of low capital efficiency and weak de-facto shareholder rights, such as those of other East Asian economies like China, Korea, Singapore, and Taiwan. More generally, given the growing calls in the United States and Europe to limit executive compensation, understanding alternative mechanisms to elicit meaningful changes in corporate behavior can be useful in the evolving corporate-governance arena.

Indeed, efforts to use stock indexes as governance mechanisms are percolating across global markets. Japan continues its efforts to use stock indexes to induce changes in corporate behavior. In early 2017, TSE and Nikkei announced a new index, the JPX-Nikkei Mid and Small Cap Index, that would extend profitability-based incentives to mid- and small-cap TSE-traded firms. Similarly, MSCI announced the Japan Empowering Women index, backed by the GPIF and designed to shift Japanese norms around gender diversity in the workplace. Similar efforts are evident in the United States and the United Kingdom. In mid-2017, S&P Dow Jones and FTSE Russell announced decisions to exclude certain companies with classified-share structures from their indexes. Globally, the number of ESG-based indexes has been growing over the last decade. Our findings suggest that indexes can leverage managers' reputation incentives to induce changes in corporate behavior. We look forward to further research on the effectiveness of this alternate governance mechanism and the different incentive channels through which it might operate in different contexts.

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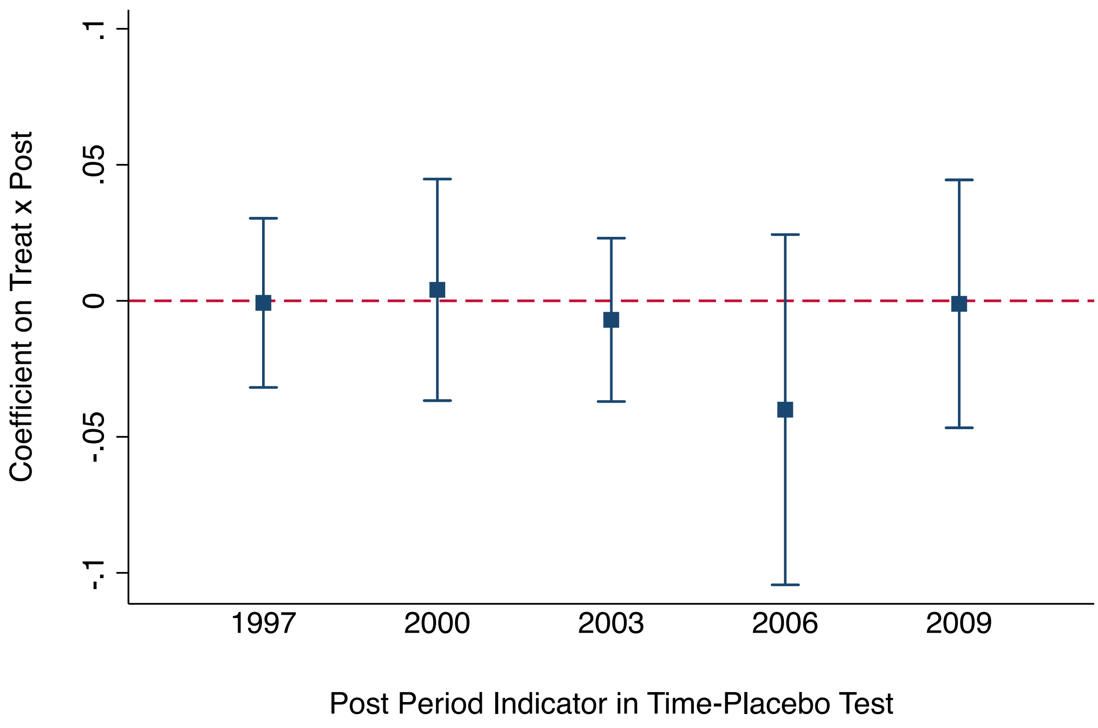
Fig. 1. Placebo Tests by Time

Figure 1 displays the results of placebo versions of our main analysis. We use historical data dating back to 1991 to synthetically create JPX400 ranks and implement our main difference-in-differences test for six sets of years prior to the launch of the JPX400 in 2013. We mimic the temporal structure of our main analyses, and include seven years for each placebo test: three pre-treatment years and three post-treatment years; the treatment year is excluded. The figure reports the point estimate and the 95% confidence intervals for six placebo tests. The y-axis reports the magnitudes of the estimated treatment effect; the x-axis reports the mid-point or the dropout year of the seven-year window around which the treatment effect is estimated. The first placebo test uses data from 1994–2000, with 1994–1996 as the pre-placebo-treatment period, 1998–1999 as the post-placebo-treatment period, and 1997 as the midpoint and dropout year. The remaining placebo tests roll forward the window of examination by three years so that the three-year pre-placebo-treatment windows are non-overlapping.



Appendix A Description of Variables

This table defines variables used in our analysis. Nikkei membership data is taken from https://indexes.nikkei.co.jp/en/nkave/index/component. Average employee compensation and average executive officers and directors compensation are drawn from a proprietary dataset collected by Toyo Kezai. All other data are obtained from the Thomson Reuters Datastream database: Datastream variable codes are specified in brackets in the Computation column.

Variable	Description	Computation
Accruals to Assets	Ratio of total accruals to total assets	(Net Income [WC07250] – Funds from Operations [WC04201]) / Total Assets [WC02999]
Asset Turnover	Asset turnover ratio	Revenues [WC07240] / Total Assets [WC02999]
Cash to Equity	Ratio of cash to total equity	Cash & Short-Term Investments [WC02001] / Total Shareholders' Equity [WC03995]
Dividends to Equity	Ratio of dividends to equity	Cash Dividends Paid [WC04551] / Total Shareholders' Equity [WC03995]
Leverage	Financial leverage	(Long Term Debt [WC03251] - Cash & Short-Term Investments [WC02001]) / Total Shareholders' Equity [WC03995]
Log Avg EE Pay	Natural logarithm of the average employee pay	ln(Average Employee Pay)
Log Avg Exec Pay	Natural logarithm of the average pay for executives and directors	ln(Average Executive Pay)
Log Book to Market	Natural logarithm of book-to-market ratio	ln(Total Shareholders' Equity [WC03995] / Market Value [MV])
Log Employees	Natural logarithm of the number of employees	ln(Employees [WC07011])
Log Gross ROE	Natural logarithm of gross return on equity	ln(1 + ROE)
Log Illiquidity	Natural logarithm of the Amihud (2002) illiquidity measure	ln(Daily Average of Absolute Return [RETURN] / Volume [VO])

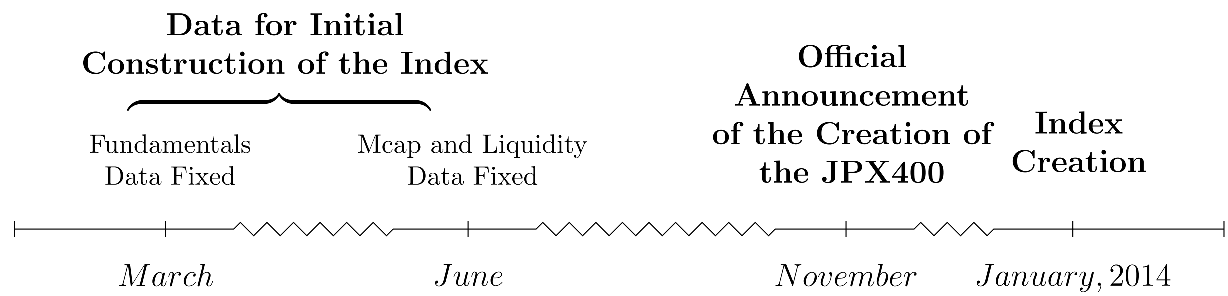
Appendix A Continued

Variable	Description	Computation
Log Market Cap	Natural logarithm of market capitalization	ln(Market Value [MV])
Log NOA	Natural logarithm of net operating assets	ln(Long-Term Debt [WC03251] – Cash & Short-Term Investments [WC02001] + Total Shareholders' Equity [WC03995])
LT Debt to Equity	LT debt leverage	Long-Term Debt [WC03251] / Total Shareholders' Equity [WC03995]
Nikkei225	Nikkei225 indicator	Indicator for a company's inclusion in the Nikkei225 index at the time of the JPX400 selection in the given year
No. of Media Articles	Media mentions from July through September	Unique number of "full articles" in Ravenpack with a *Relevance* of more than 75 (as coded by Ravenpack) in which the firm is indicated as referenced
Profit Margin	Net profit margin	Net income [WC07250] / Revenues [WC07240]
R&D to Sales	R&D intensity	R&D Expenses [WC01201] / Revenues [WC07240]
Repurchases	Estimated repurchases	Funds to Decrease Common or Preferred Stock [WC04751] – Change in Preferred Stock [WC03451]
ROA	Return on assets	Net Income [WC07250] / Total Assets [WC02999]
ROE	Return on equity	Net Income [WC07250] / Total Shareholders' Equity [WC03995]
Sales Growth	Sales growth	Revenues [WC07240] / Lagged Revenues [WC07240]
Shareholder Payouts	Shareholder payout ratio	(Repurchases + Cash Dividends Paid) / Total Shareholders' Equity [WC03995]
Weighted No. of Media Articles		No. of Media Articles (above) weighted by the *Relevance* of that article to the firm, as coded by Ravenpack

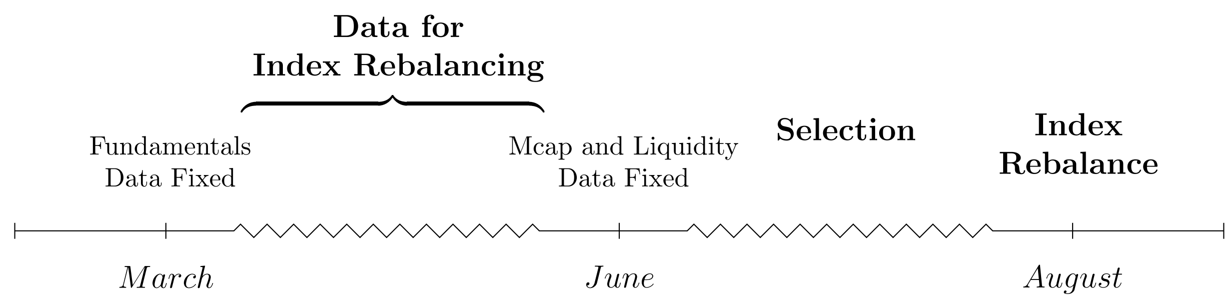
Appendix B Timeline, Selection of JPX400 Index Constituents

This figure is a schematic representation of the timeline for the selection of JPX400's constituent firms, relative to firm-level information, for each year of the index's existence. Panel A traces the initial construction of the index in 2013; Panel B traces the annual rebalancing from 2014 onward. Vertical lines indicate important dates.

Panel A: 2013



Panel B: 2014+



Appendix C Sample Construction

This figure is a schematic representation of the process we use to construct our baseline analysis sample. Steps 1 and 2 mimic the JPX400 selection algorithm, which we follow as closely as possible using the JPX's description of its ranking process. For each sample year, we first identiy the top 1,200 firms by "trading value" (share volume scaled by price per share), and then select the 1,000 largest by market capitalization. We then follow the JPX400 ranking algorithm to compute a synthetic JPX400 rank for each of the 1,000 firms. Step 3 describes how we assign treatment status based on this synthetic JPX400 ranking. The top 400 firms are our predicted JPX400 constituents. For our baseline analysis, we classify firms with ranks of 301–500 as the treatment group, and firms with ranks of 501–800 as the control group. We do not include firms with ranks of 1–300 or 801–1,000 in our baseline analysis but do include them in robustness tests.

Step 1: Each June, select the top 1,000 firms, by market capitalization, from the top 1,200 firms by "trading value."

Step 2: Predict JPX400 ranks using fundamental data available as of March and market data available as of June.

Step 3: Construct baseline treatment and control sample.

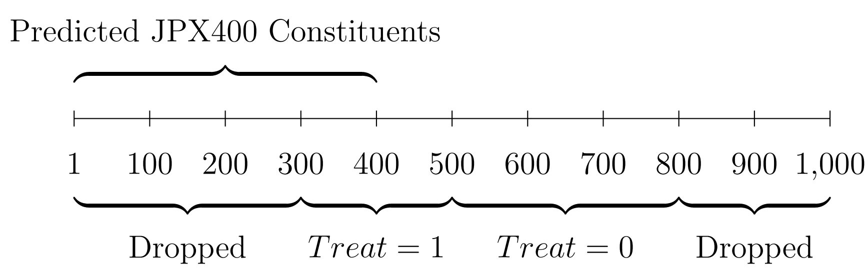


Table 1. Predicting JPX400 Membership

Columns 1 and 2 report estimates from OLS regressions of *JPX Member*—an indicator for whether a firm is selected for the JPX400—on *Above Cutoff*, an indicator for whether a firm's synthetic rank falls between 1 and 400. In columns 3 and 4, *Above Cutoff* is disaggregated into two indicators, which specify whether a firm's synthetic rank falls within 1–200 or within 201–400. Three other indicators—for ranks within 400–600, within 600–800, and over 800—are also included in these regressions. These indicators subsume the constant term in the regression. Columns 2 and 4 include other specified firm-level controls. The sample consists of firms with synthetic ranks of 1–2000 in the years 2013–2016. Standard errors, clustered at the firm level, are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)
	JPX Member	*JPX Member*	*JPX Member*	*JPX Member*
Above Cutoff	0.923***	0.864***		
	(0.01)	(0.01)		
Rank 1–200			0.991***	0.949***
			(0.00)	(0.01)
Rank 201 – 400			0.879***	0.857***
			(0.01)	(0.02)
Rank 401 – 600			0.0542***	0.0457***
			(0.01)	(0.01)
Rank 601 – 800			0	0
			(.)	(.)
Rank outside 800			0.00488***	0.0315***
			(0.00)	(0.01)
Log Market Cap		0.0217***		0.0185***
		(0.00)		(0.00)
Log Book to Market		-0.00180		-0.000153
		(0.00)		(0.00)
Sales Growth		-0.000275		0.00115
		(0.01)		(0.01)
LT Debt to Equity		-0.00120		-0.000453
		(0.00)		(0.00)
Cash to Equity		0.00134		-0.0000954
		(0.00)		(0.00)
Observations	7,110	7,110	7,110	7,110
R^2	0.869	0.872	0.900	0.876

Table 2. Summary Statistics

Table 2 reports summary statistics on the pre-treatment period (2010–2012) for the full sample used for the main regressions (firms synthetically ranked 301–800). The rightmost three columns report the means for the treatment group (ranks 301–500) and the control group (ranks 501–800) and T-statistic from the test of equality in means between the two groups. All continuous variables are winsorized at the top and bottom 0.5% of the cross-sectional distribution. Variable descriptions appear in Appendix A.

	Full Pre-Period Sample							
	p25	*Mean*	*p50*	*p75*	*SD*	*Mean* $Treat = 1$	*Mean* $Treat = 0$	*T-Stat of Difference*
ROE	0.0300	0.0541	0.0500	0.0821	0.1007	0.0585	0.0512	1.3145
ROA	0.0086	0.0284	0.0238	0.0427	0.0382	0.0311	0.0267	2.1998
Profit Margin	0.0136	0.0469	0.0301	0.0605	0.0855	0.0485	0.0459	0.5784
Asset Turnover	0.6064	0.9565	0.8990	1.2423	0.6316	1.0121	0.9197	2.7198
Net Leverage	-0.3175	0.0301	-0.1108	0.2206	0.7299	0.0902	-0.0097	2.4606
Repurchases to Equity	0.0000	0.0062	0.0000	0.0002	0.0270	0.0065	0.0060	0.3315
Dividends to Equity	0.0115	0.0205	0.0170	0.0241	0.0150	0.0213	0.0199	1.8276
Log Book to Market	-0.1845	0.0833	0.1559	0.4247	0.5348	0.0174	0.1269	-3.9403
Lagged Annual Return	-0.1453	0.0355	-0.0091	0.1555	0.3082	0.0321	0.0378	-0.3465
Accruals to Assets	-0.0646	-0.0435	-0.0427	-0.0195	0.0383	-0.0453	-0.0423	-1.5218
Log NOA	17.2469	18.0358	18.0206	18.8100	1.3170	18.3899	17.7994	7.9311
R&D to Sales	0.0000	0.0172	0.0052	0.0254	0.0281	0.0156	0.0182	-1.8103
Log Employees	7.3702	8.1015	8.0467	8.7921	1.2670	8.4527	7.8655	8.6375
Log Market Cap	17.4785	18.0954	17.9597	18.6252	0.8692	18.5053	17.8244	15.3226
Sales Growth	0.9365	1.0127	1.0041	1.0741	0.1711	1.0093	1.0151	-0.6626
LT Debt to Equity	0.0172	0.4040	0.1600	0.4920	0.6759	0.4632	0.3649	2.7037
Cash to Equity	0.1868	0.3918	0.3036	0.4411	0.4078	0.4005	0.3861	0.6322
Nikkei 225	0.0000	0.1440	0.0000	0.0000	0.3512	0.2060	0.1030	5.3076

Table 3. Treatment Effects in ROE

Table 3 reports the estimates of DID regressions using *Forward ROE* as the dependent variable. *Treat* is an indicator variable that evaluates to one for firms ranked 301–500 under our replication of the JPX400 ranking algorithm and to zero for firms ranked 501–800; *Post* is an indicator variable that evaluates to one for the years 2014–2016 and to zero for the pre-treatment period, 2010–2012. Column 1 reports a baseline specification with no controls or fixed effects; column 2 adds year-fixed effects; and column 3 adds industry-fixed effects, where industry is defined by Datastream's Industry Level 3 Name (INDM3). Column 4 adds specified firm-level controls; column 5 adds firm-level contemporaneous ROE as a control; and column 6 substitutes the log of ROE for ROE, both as the forward outcome variable of interest and the contemporaneous control. All firm-level control variables are defined in Appendix A. Standard errors, clustered at the firm level, are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	(1) *Forward ROE*	(2) *Forward ROE*	(3) *Forward ROE*	(4) *Forward ROE*	(5) *Forward ROE*	(6) *Log Gross Forward ROE*
Treat x *Post*	0.028***	0.028***	0.025***	0.025***	0.024***	0.044***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Treat	-0.006	-0.006	-0.005	0.007	-0.005	-0.004
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Post	0.018***					
	(0.01)					
ROE					0.384**	
					(0.15)	
Log Gross ROE						
Log Market Cap				-0.026***	-0.013**	-0.031***
				(0.00)	(0.01)	(0.01)
Log Book to Market				-0.069***	-0.045***	-0.058***
				(0.01)	(0.01)	(0.01)
Sales Growth				0.038**	-0.011	0.034
				(0.02)	(0.02)	(0.03)
LT Debt to Equity				-0.013*	-0.007	-0.045**
				(0.01)	(0.00)	(0.02)
Cash to Equity				-0.016	-0.001	-0.027
				(0.01)	(0.01)	(0.02)
Time FE	No	Yes	Yes	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes	No	No
Observations	2,783	2,783	2,783	2,783	2,783	2,780
R^2	0.0221	0.0219	0.0514	0.2472	0.3031	0.1097

Table 4. Testing the Identifying Assumption

Table 4 reports the results of various regressions testing the identifying assumption of our main tests in Table 3. Column 1 adds two additional interaction terms—*Treat* x *(Year = 2011)* and *Treat* x *(Year = 2012)*—to the main DID specification (Table 3), column 5, to test differences in pre-treatment trends. Column 2 estimates the main DID specification but splits *Treat* into two types—firms ranked 301–400 (*Rank 301–400*) and 401-500 (*Rank 401–500*)—and estimates separate treatment effects. The F-statistic and corresponding p-value for a test of equality of the two DID coefficients are reported in the bottom two rows. Column 3 estimates the main DID specification using an expanded sample that includes firms ranked 1–300 and estimates a separate treatment effect for these firms. The F-statistic, and its corresponding p-value, from a test of equality between the two DID coefficients are reported in the bottom two rows. Column 4 estimates the main DID specification using a sample of firms ranked 1301–1800, where the treatment firms are those ranked 1301–1500 and the control firms are those ranked 1501–1800. Column 5 expands the column 2 sample to include all the top 1000 firms and uses an alternative, more continuous, treatment intensity variable—*Quintile(Closeness)*— indicating which quintile a firm falls into in terms of its closeness to the rank of 400. All firm-level control variables are defined in Appendix A. Standard errors, clustered at the firm level, are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)	(5)
	Forward ROE	*Forward ROE*	*Forward ROE*	*Forward ROE*	*Forward ROE*
Treat x *Post*	0.022**		0.023***		
	(0.01)		(0.01)		
Treat x *(Year = 2011)*	-0.005				
	(0.01)				
Treat x *(Year = 2012)*	-0.000				
	(0.01)				
Treat	-0.003		-0.009		
	(0.01)		(0.01)		
Treat x *Rank 301-400* x *Post*		0.027***			
		(0.01)			
Treat x *Rank 401-500* x *Post*		0.024**			
		(0.01)			
Treat x *Rank 301-400*		0.000			
		(0.01)			
Treat x *Rank 401-500*		-0.008			
		(0.01)			
Rank 1-300 x *Post*			0.006		
			(0.01)		
Rank 1-300			0.014		
			(0.01)		
Placebo Group Treat x *Post*				0.002	
				(0.01)	
Placebo Group Treat				0.020***	
				(0.01)	
Quintile(Closeness) x *Post*					0.006**
					(0.00)
Quintile(Closeness)					-0.000
					(0.00)
ROE	0.384**	0.379**	0.439***	0.171**	0.373***
	(0.15)	(0.15)	(0.12)	(0.07)	(0.05)
Time FE	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes
Observations	2,783	2,783	4,462	2,885	5,546
R^2	0.3026	0.3040	0.3532	0.1196	0.2630
F-stat		0.059	7.719		
p-value		0.808	0.006		

Table 5. Consequences of Index Inclusion: Performance, Compensation, and Capital Market Outcomes

Table 5 reports the results of regressions estimating the effect of realized index inclusion on a variety of monetary and financial outcome variables. For these tests, the sample consists of firms that are tightly ranked around the threshold of inclusion, firms ranked 350-450. Since we are interested in the effects of realized inclusion, and our dependent variables are forward variables measured at an annual horizon, we use only our post-period, years 2014-2016. The independent variable of interest is *JPX Member*, an indicator for whether the firm was actually included in the index in that year. We use the same controls for each regression in this table: *Centered Rank*, which is equal to the firm's synthetic rank minus 400, *Log Market Cap*, and *ROE*. We include time and industry fixed effects throughout. The dependent variables in columns 1 through 6 are *Forward ROE*, *Forward Sales Growth*, *Forward Log Executive Compensation*, *Forward Log Average Employee Compensation*, *Forward Log Illiquidity*, and *Forward Log BM*, respectively. All firm-level controls and outcome variables are defined in Appendix A. Standard errors, clustered at the firm level, are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	(1) *Forward ROE*	(2) *Forward Sales Growth*	(3) *Forward Log Exec Comp*	(4) *Forward Log EE Comp*	(5) *Forward Log Illiquidity*	(6) *Forward Log BM*
JPX Member	-0.003	0.019	0.078	0.005	0.058	0.032
	(0.01)	(0.02)	(0.08)	(0.03)	(0.13)	(0.08)
Centered Rank	-0.000	0.000	-0.000	-0.000	0.001	0.001
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Log Market Cap	0.011***	0.006	0.269***	0.066***	-1.106***	-0.217***
	(0.00)	(0.01)	(0.04)	(0.02)	(0.10)	(0.05)
ROE	0.870***	0.170	0.132	-0.234	-1.141	-6.028***
	(0.09)	(0.23)	(0.67)	(0.25)	(1.21)	(0.93)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Ind FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	301	301	301	301	301	301
R^2	0.4921	0.2169	0.2181	0.2423	0.5983	0.4428

Table 6. Consequences of Index Inclusion: Media Attention

Table 6 reports the results of regressions estimating the effect of realized index inclusion on media attention, using the same sample and empirical strategy as in Table 5. For these tests, the sample consists of firms that are tightly ranked around the threshold of inclusion, firms ranked 350-450. Since we are interested in the effects of realized inclusion, and our dependent variables are forward variables measured at an annual horizon, we use only our post-period, years 2014-2016. The independent variables of interest are *Added*, an indicator that evaluates to 1 if the firm is included in the index but had been excluded in the previous year, *Booted*, an indicator that evaluates to 1 if the firm is excluded from the index but had been included in the previous year, and *Switch*, an indicator that evaluates to 1 if the firm has been either *Added* or *Booted*. We estimate heterogeneity in the media response with interacted regressions in which *Switch x Nikkei* is the independent variable of interest (and an indicator for *Nikkei* is added as a control). In Columns 1 through 3, the dependent variable is *No. of Media Articles*; in Columns 4 through 6, the dependent variable is *Weighted No. of Media Articles*. We use the same controls for each regression in this table: *Centered Rank*, which is equal to the firm's synthetic rank minus 400, *Log Market Cap*, and *ROE*. We include time and industry fixed effects throughout. Definitions for all firm-level variables appear in Appendix A. Robust standard errors are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	No. of Media Articles			*Weighted No. of Media Articles*		
	(1)	(2)	(3)	(4)	(5)	(6)
Switch	35.989*		4.125	68.931**		19.685
	(19.30)		(7.01)	(31.39)		(19.96)
Added		51.599			80.656	
		(51.40)			(73.63)	
Booted		27.380			62.464	
		(25.21)			(41.83)	
Nikkei225 x *Switch*			132.369*			203.962**
			(69.62)			(101.91)
Nikkei225			2.718			0.535
			(13.66)			(18.73)
Centered Rank	-0.165	-0.113	-0.080	-0.320	-0.281	-0.193
	(0.15)	(0.17)	(0.12)	(0.21)	(0.23)	(0.18)
Log Market Cap	45.558***	44.297**	35.698**	60.095***	59.149***	45.844**
	(16.04)	(17.31)	(15.13)	(21.15)	(22.41)	(19.14)
ROE	137.763	117.147	130.682	153.975	138.489	139.693
	(94.69)	(74.16)	(84.76)	(142.34)	(104.33)	(125.54)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	301	301	301	301	301	301
R^2	0.3408	0.3396	0.3698	0.3203	0.3182	0.3533
F-stat		0.138	3.615		0.037	4.006
p-value		0.711	0.059		0.848	0.047

Table 7. Treatment Effects in ROE for Nikkei vs. Non-Nikkei

Table 7 examines the differential ROE responses to JPX400-inclusion incentives of Nikkei225 firms and non-Nikkei225 firms. Columns 1 and 2 report the OLS results from an estimation of equation 4; columns 3 and 4 report the results of estimating equation 5. All estimations use the full sample of firms ranked 1–1,000. *Nikkei225* indicates contemporaneous membership in the Nikkei 225 index; *Treat* is an indicator variable assuming a value of one for firms ranked 301–500 under our replication of the JPX400 selection algorithm and zero for all other firms ranked 1–1,000; *Quintile(Closeness)* is a quintile ranking, ranging from the values of 0 to 4, of a firm's distance from the rank of 400, where the highest quintile reflects those firms whose ranks are closest to 400 and the lowest quintile reflects those furthest from 400; and *Post* is an indicator variable assuming a value of one for the years 2014–2016 and zero for the pre-treatment period, 2010–2012. In columns 1 and 2, the last two rows report the F-statistic and the corresponding p-value from a Wald test of equality between the two triple-interaction coefficients. In columns 1–4, we include time-fixed and firm-level controls as in Table 3, column 5, but their coefficient estimates are unreported. Definitions for all firm-level control variables appear in Appendix A. Standard errors, clustered at the firm level, are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	Splitting Treatment		*Triple Diffs*	
	(1) *Forward ROE*	(2) *Forward ROE*	(3) *Forward ROE*	(4) *Forward ROE*
Treat x *Nikkei225* x *Post*	0.066***		0.050**	
	(0.02)		(0.02)	
Treat x *non-Nikkei225* x *Post*	0.011**			
	(0.01)			
Quintile(Closeness) x *Nikkei225* x *Post*		0.013***		0.015*
		(0.00)		(0.01)
Quintile(Closeness) x *non-Nikkei225* x *Post*		0.004		
		(0.00)		
Quintile(Closeness) x *Post*				0.003
				(0.00)
Nikkei225 x *Post*			0.006	-0.018
			(0.01)	(0.02)
Treat x *Post*			0.012**	
			(0.01)	
Treat x *Nikkei225*	-0.059***		-0.055**	
	(0.02)		(0.02)	
Treat x *non-Nikkei225*	0.003			
	(0.00)			
Quintile(Closeness) x *Nikkei225*		-0.009**		-0.017***
		(0.00)		(0.01)
Quintile(Closeness) x *non-Nikkei225*		0.002		
		(0.00)		
Quintile(Closeness)				0.003
				(0.00)
Nikkei225			-0.007	0.019
			(0.01)	(0.01)
Treat			0.001	
			(0.00)	
ROE	0.373***	0.367***	0.373***	0.366***
	(0.05)	(0.05)	(0.05)	(0.05)
Time FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Observations	5,546	5,546	5,546	5,546
R^2	0.2657	0.2661	0.2656	0.2663
F-stat	6.315	6.531		
p-value	0.012	0.011		

Table 8. Drivers of ROE

Table 8 reports the results of DID regressions following the specification in Table 3, column 5, but using as dependent variables drivers of one-year-ahead ROE —*Sales Growth*, *ROA*, *Profit Margin*, *Asset Turnover*, *Leverage*, and *Shareholder Payouts*. As in Table 3, *Treat* is an indicator variable that is set to one for firms ranked 301–500 under our replication of the JPX400 selection algorithm and zero for firms ranked 501–800; *Post* is an indicator variable that is set to one for the years 2014–2016 and zero for the pre-treatment period, 2010–2012. The starting sample for all of these analyses is the one for which all variables used in the baseline analysis—Table 3, columns 3-5—are available. All firm-level control variables are defined in Appendix A. Standard errors, clustered at the firm level, are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	(1) *Forward Sales Growth*	(2) *Forward ROA*	(3) *Forward Profit Margin*	(4) *Forward Asset Turnover*	(5) *Forward Leverage*	(6) *Forward Payout*
Treat x *Post*	0.0065	0.0053**	0.0074**	0.0047	-0.0048	0.0059**
	(0.010)	(0.002)	(0.004)	(0.008)	(0.016)	(0.003)
Treat	-0.0224**	-0.0020	-0.0147***	-0.0161***	0.0077	-0.0039***
	(0.009)	(0.002)	(0.003)	(0.006)	(0.013)	(0.001)
Sales Growth	-0.0017	-0.0167**	-0.0402***	-0.0109	0.0728**	0.0071
	(0.035)	(0.007)	(0.010)	(0.026)	(0.034)	(0.007)
ROA		0.5307***				
		(0.058)				
Profit Margin			0.8153***			
			(0.063)			
Asset Turnover				0.9564***		
				(0.007)		
Repurchases to Equity						0.0147
						(0.033)
Dividends to Equity						0.9491***
						(0.070)
LT Debt to Equity	0.0049	-0.0052***	0.0026	0.0006	0.9456***	-0.0010
	(0.005)	(0.001)	(0.002)	(0.003)	(0.021)	(0.001)
Cash to Equity	-0.0218**	-0.0047**	-0.0013	-0.0090	-0.8277***	0.0051
	(0.009)	(0.002)	(0.004)	(0.007)	(0.032)	(0.003)
Log Market Cap	-0.0019	-0.0026**	0.0058***	0.0058**	0.0173**	-0.0001
	(0.003)	(0.001)	(0.001)	(0.003)	(0.007)	(0.001)
Log Book to Market	-0.0870***	-0.0201***	-0.0175***	-0.0035	0.0331***	-0.0077***
	(0.007)	(0.003)	(0.005)	(0.006)	(0.012)	(0.002)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,785	2,784	2,785	2,784	2,783	2,781
R^2	0.1456	0.5002	0.6261	0.9538	0.8875	0.2611

Table 9. Accruals and Investments

Table 9 reports the results of DID regressions following the specification in Table 3, column 5, but using alternative outcome variables measured in the year following each JPX400 selection: *Accruals to Assets*, *Net Operating Assets*, *R&D to Sales*, *Log Employees*, *Log Average Employee Pay*, and *Log Average Executive Pay*. As in Table 3, *Treat* is an indicator variable that is set to one for firms ranked 301–500 under our replication of the JPX400 selection algorithm and zero for firms ranked 501–800; *Post* is an indicator variable that is set to one for the years 2014–2016 and zero for the pre-treatment period, 2010–2012. The starting sample for all analyses is the one for which all variables used in the baseline analysis—Table 3, columns 3-5—are available. Observations vary across specifications depending on the availability of specific new variables used for the analysis. All firm-level control variables are defined in Appendix A. Standard errors, clustered at the firm level, are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	(1) *Forward Accruals to Assets*	(2) *Forward Log NOA*	(3) *Forward R&D to Sales*	(4) *Forward Log Employees*	(5) *Forward Log Avg EE Pay*
Treat x *Post*	0.0017	0.0329	-0.0011*	0.0050	0.0045
	(0.002)	(0.022)	(0.001)	(0.011)	(0.005)
Treat	0.0003	0.0038	0.0011**	0.0044	-0.0117***
	(0.002)	(0.014)	(0.000)	(0.009)	(0.004)
Accruals to Assets	0.4042***				
	(0.034)				
Log NOA		0.6881***			
		(0.094)			
R&D to Sales			0.9805***		
			(0.018)		
Log Employees				0.9817***	
				(0.005)	
Log Avg EE Pay					0.9515***
					(0.008)
Log Avg Exec Pay					-0.0021
					(0.003)
Log Market Cap	-0.0007	0.3007***	-0.0003	0.0060	0.0077***
	(0.001)	(0.097)	(0.000)	(0.007)	(0.002)
Log Book to Market	0.0048***	0.2935***	0.0003	-0.0449***	-0.0063**
	(0.002)	(0.101)	(0.000)	(0.009)	(0.003)
Sales Growth	0.0250***	0.1630***	0.0035***	0.0505**	0.0049
	(0.006)	(0.050)	(0.001)	(0.020)	(0.010)
LT Debt to Equity	0.0003	0.2083***	-0.0001	0.0110**	-0.0024
	(0.001)	(0.068)	(0.000)	(0.006)	(0.002)
Cash to Equity	0.0125***	-0.2537***	0.0005	-0.0185**	-0.0036
	(0.002)	(0.098)	(0.000)	(0.009)	(0.004)
Time FE	Yes	Yes	Yes	Yes	Yes
Observations	2,779	2,710	2,785	2,654	2,560
R^2	0.2295	0.9618	0.9260	0.9883	0.9209

Table 10. Market Valuation

Table 10 reports the results of reduced-form and instrumental-variables (IV) analyses using *Forward BM* as the dependent variable. The endogenous variable of interest in the IV analysis, *Forward ROE*, is instrumented for by our DID estimator, *Treat* x *Post*. As in Table 3, *Treat* is an indicator variable that is set to one for firms ranked 301–500 under our replication of the JPX400 selection algorithm and to zero for firms ranked 501–800; *Post* is an indicator variable that is set to one for the years 2014–2016 and to zero for the pre-treatment period, 2010–2012. Column 1 reports the results of a reduced-form DID estimate of the effect on *Log Book to Market*, similar to our main DID specification used in Table 3, column 5. Columns 2 and 3 report the first and second stages of the IV regression. The last row in column 2 reports the first-stage F-statistic for the instrument. All firm-level control variables are defined in Appendix A. Standard errors, clustered at the firm level, are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	(1) OLS	(2) IV 1^{st} Stage	(3) IV 2^{nd} Stage
	Forward Log Book to Market	*Forward ROE*	*Forward Log Book to Market*
Forward ROE			-1.823*
			(1.03)
Treat x *Post*	-0.038*	0.022***	
	(0.02)	(0.01)	
Treat	0.029**	-0.004	0.024
	(0.01)	(0.01)	(0.02)
ROE	0.026	0.407***	0.745
	(0.10)	(0.15)	(0.48)
Book to Market		-0.038***	
		(0.01)	
Log Market Cap	-0.024***	-0.013**	-0.047***
	(0.01)	(0.01)	(0.02)
Sales Growth	0.138***	0.009	0.146**
	(0.04)	(0.03)	(0.06)
LT Debt to Equity	0.000	-0.006	-0.013
	(0.01)	(0.00)	(0.01)
Cash to Equity	-0.035	0.012*	-0.020
	(0.02)	(0.01)	(0.02)
Time FE	Yes	Yes	Yes
Observations	2,725	2,725	2,725
R^2	0.8137	0.2845	0.7647
First Stage F		9.4818	

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