Governance through Shame and Aspiration: Index Creation and Corporate Behavior

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Abstract

After decades of 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 pro table of its large and liquid firms. We find that managers competed for inclusion in the index by significantly increasing ROE, and they did so at least in part due to their reputational or status concerns. The ROE increase was predominantly driven by improvements in margins, which were in turn partially driven by cutting R&D intensity. 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 approach—the use of non-pecuniary strategies or implicit incentives—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 on 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 the most prominent examples are 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—firms 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), thus acting as an alternate governance mechanism that 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-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. 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 attributable both to formal endorsement by the Government Pension Investment Fund (GPIF) and to intense media coverage of its annual 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 and their managers.

This paper studies whether Japanese managers responded to *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 enables managers (and researchers) to assess firms'

[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.

relative likelihood of inclusion, even though the Japan Exchange Group does not disclose the official scores. 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 in the likelihood of index inclusion.

If managers are indeed motivated to attain JPX400 membership, these incentives can be expected to be strongest at firms ranked near the margin of inclusion; all else equal, managers at those firms are 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 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 effects of index-inclusion incentives: "treated" firms are defined as those with synthetic ranks near the inclusion threshold (ranks 301–500 in our main specification); "control" firms are similar firms with a much lower probability of 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

[2]For consistency, we use this as our main treatment and control definition throughout. But our result is not sensitive to this choice. It remains stable when we use as a control group 200 or 300 firms on either side of the treatment group, or on both sides, and it survives and varies predictably as we vary the "bandwidth" of our treatment group (e.g., becomes stronger if we use firms ranked 351–450 as treatment). 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.

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 its performance and on the performance of *other* eligible firms. In effect, our research design thus combines multiple "experiments" in the post-JPX400 period with multiple placebo "experiments" in the pre-period to infer the effect of inclusion incentives.

We document three main empirical findings. First, the firms closest to the inclusion threshold achieved differential and economically significant improvement in ROE. We estimate that, on average, these firms increased ROE by an economically significant 2.4 percentage points. A battery of tests supports the hypothesis that this effect is driven by 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 were motivated in part by 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*. We do find, however, that firms who are added to or booted from the index attracted 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 their membership in 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 valuable to non-Nikkei225 firms. But Nikkei225 firms would probably be more sensitive to reputational concerns, and specifically to loss of status if they were excluded from the new gauge of Japan's best-run corporations. Consistent with this reasoning, we find that media attention to added or booted firms was greater for Nikkei225 firms; we also document that those firms exhibited significantly larger ROE responses than non-Nikkei225 firms.

Third, we find that firms on average improved ROE predominantly through improved margins, in part by cutting discretionary expenses like research and development. Index-inclusion incentives did not appear to lead firms to engage in accruals earnings management, cut capital investments, or reduce employment or average employee pay. Overall, we find that treated firms' market valuations improved, suggesting that market participants viewed the managerial activities that improved ROE as valuable to shareholders.

Our work makes several significant contributions. First, we contribute to the literature on the effect of indexes. There has been abundant research on the performance and outcomes of firms *in response to inclusion* in an index, including capital-market consequences (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, 2016). 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 those of Dyck et al. (2008), who examine the influence of the media and of reputation concerns on 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 this work by documenting, in a setting with relatively strong empirical identification, how managers' status concerns can be actively employed in a novel governance tool to influence certain firms' behavior.

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), and in particular on how the allocation of status can affect the resource-allocation process (Zingales, 2015). Insights from our analyses could be valuable to policy makers seeking alternative channels of influence on corporate managers; they may be particularly relevant in light 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 factors that make the JPX400 a compelling setting in which to study the role of indexes in changing longstanding corporate behavior.

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; 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 the Japanese legal system conferred increasing legal rights on shareholders over the years, their *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 management, restricting 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 (see, e.g., Figure 1 of Toyoda, 2014).

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 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 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 asserts:

> Japan faces a rapidly aging and declining population and a decreasing stock of

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

> 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 concerns and goals were embodied in the Abe administration's "third arrow" of structural reforms—considered the most important component of Abenomics—which aimed to increase corporate managers' dialogue with and focus on shareholders.[4] In its efforts to boost capital markets and revitalize the economy, in short, Japan sought to change long-standing 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 (the Tokyo Stock Exchange and the 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 400 large and liquid public firms deemed best in terms of profitability, capital efficiency, and, by extension, corporate governance. (See Section 3 for a full description of the algorithm.)

The index quickly became the new prestige index in Japanese equity markets, largely due

[4]Since his election in December 2012, Prime Minister Shinzo Abe has advocated an economic policy, dubbed "Abenomics" by economists and the media, with three main components ("three arrows"): the "first arrow" of monetary easing, the "second arrow" of fiscal stimulus, and the "third arrow" of structural reforms. Two 2014 reforms by the Financial Services Agency were designed to encourage manager–shareholder dialogue. 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 institutional investors nor companies are legally required to abide by all of the codes' principles; they are required to comply or explain.

to formal endorsement by the Government Pension Investment Fund (GPIF)—the world's largest pension fund, run by the Japanese government—which decided in 2014 to use the JPX400 as an equity benchmark for its passive investments. 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—stunned market participants and attracted substantial attention. The choice signaled that the fund would be paying closer attention to the governance of the companies it invested in, consistent with the goals of Abe's "third arrow." It also boosted the status of the JPX400 as a gauge of "well-run" companies in the eyes of domestic institutional investors and the media.

Subsequent media coverage further elevated the JPX400's status. Each summer, the JPX reconstitutes its membership, booting firms that no longer make the cut and adding firms that improved their (relative) performance. Although the index *per se* offers no direct financial benefits to its constituent firms or their managers in the form of a direct monetary award, its creation and its churn each August have generated substantial excitement and media attention. Its colloquial nickname, "the shame index," captures 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 performance data. Although the JPX does not publish the underlying rankings, they are highly replicable. These features sharply distinguish the JPX400 from the Nikkei225—Japan's traditional leading stock index and the oldest stock index in Asia—whose selection criteria are opaque and determined by Japan's top financial publication, 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. 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.[5]

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. The index is also notable as the first instance of a central government deploying a stock index as a primary policy tool to change a persistent behavioral outcome. It thus constitutes a unique laboratory in which to study the potential incentive effects of this new alternate governance mechanism.

3 Empirical Design

We study how firms' index-inclusion incentives affect their performance. We hypothesize that, all else equal, firms closer to the threshold of inclusion will differentially improve ROE.[6] Our empirical strategy exploits the variation in index-inclusion incentives, which is approximated by the variation in firms' rankings. Our maintained assumption is that,

[5]When Amada, a 68-year-old toolmaker and member of the Nikkei225 index, was excluded from the inaugural JPX400 index in 2014, its president 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 initially included in the index, such as Unicharm, announced measures aimed at securing their membership by further improving ROE (McLannahan, 2014).

[6]Firms nearer to the inclusion threshold are most likely to see their inclusion status change as a function of their performance. This intuition resembles the theoretical and empirical finding of 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 expend less effort than do agents closer to the margin between winning and losing.

though the JPX does not publish its rankings of firms, the transparency and replicability of its selection algorithm make managers aware of (at least roughly) their firms' relative rankings and 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, and then use them as the basis of a difference-in-differences design to infer how index-inclusion incentives affect firms' subsequent ROE.

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 data, or have duplicate Datastream identifiers in the same year. We merge in executive-compensation data from Toyo Kezai, information on media coverage from Ravenpack, and indicators for Nikkei225 and JPX400 membership constructed using information obtained from the indices' websites.

We then employ the algorithm used by the Japan Exchange Group to replicate firms' JPX400 rankings. The selection algorithm begins by filtering companies listed on the Tokyo Stock Exchange (TSE) 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 preceding 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, these 1,200 stocks are winnowed down to the top 1,000 by market capitalization.

These 1,000 firms, which we refer to collectively as "the ranked set," are then ranked using the following composite score ($Total\_Score$):

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

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 members are chosen on the basis of the highest $Total\_Score$, with one caveat: the JPX can make up to ten "qualitative adjustments" per year based on corporate-governance and disclosure-related factors. These adjustments are not determined by observable factors, but, according to our interviews with representatives of the Japan Exchange Group (and empirical evidence presented below), they are insignificant. We treat these 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, the outcome of interest to policy makers. As the only scaled measure, moreover, ROE is the most controllable by managers.[7]

[7]Market capitalization and operating income are unscaled and thus largely driven by firm size. Managers might be able to increase firm size via seasoned equity offerings or acquisitions, but such actions would be

We implement a difference-in-differences (DID) design using the synthetic rankings, where "treated" firms are those close to the threshold of inclusion and "control" firms are those further from the threshold. Our main tests estimate the following:

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

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 members, 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 effects.

Note that the first difference compares the ROE of the firms closest to the threshold of inclusion to that of firms outside the threshold in the three years after introduction of the JPX400. (We exclude 2013 because the JPX400's inaugural members were announced at the end of that year, affording firms only three months to respond to index-inclusion incentives.) The main coefficient of interest, β_1, effectively benchmarks this first difference against the baseline difference between similarly ranked firms in the three years prior to introduction of the JPX400. This second difference accounts for baseline differences between our treatment and control groups 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 lack full control over these rankings, which depend on the stochastic nature of a firm's own performance and the performance of other JPX400-eligible firms.

likely to generate a competing effect on a firm's rankings 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.

To infer the effect of inclusion incentives, therefore, our research design in effect combines multiple "experiments" in the post-JPX400 period with multiple placebo "experiments" in the pre-period.

Our identifying assumption is that any counterfactual differences in future ROE—differences that would exist in the absence of JPX400-inclusion incentives—between treated firms and controls are stable over time and thus accounted for by pre-period differences between placebo treatment and control firms. We believe that this assumption is most defensible when conditioned on current ROE, an important predictor of future ROE, because the distribution of ROE can change after introduction of the JPX400. Thus, our most robust specification—on which we rely most heavily throughout our empirical analysis—includes current ROE (or the lagged dependent variable more generally) as a control.

3.3 Validation of Synthetic Ranks

Because our research design relies on a synthetic replication of JPX400 ranks, we validate them before proceeding to the DID analysis by examining how well they explain variation in the likelihood of index inclusion. To ensure that this analysis includes all false negatives, our analysis sample includes all firms in the top 2,000 by trading value, for the selection years 2013–2016.

Table 1 reports the results of an OLS regression of *JPX400 Member*—an indicator for actual JPX400 membership—on ranges of firms' synthetic ranks. Column 1 shows that a synthetic rank of 400 or less (*Above Cutoff*) is highly predictive of actual index inclusion. This univariate regression generates an R^2 of 87%. 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 JPX400's weighting toward large firms; thus 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. (We omit an indicator for ranks 601–800, which would induce perfect collinearity with the other indicators.) They show that the probability of actual inclusion decreases monotonically with our synthetic ranks, and, once again, that the only firm-level control that additionally predicts inclusion is size. Collectively, these results show that our synthetic rankings are highly valid, and that 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 from a test of their mean 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 would accentuate 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. We will utilize the holdout sample of firms later, however, in robustness tests to help rule out alternative hypotheses.

4 Empirical Analysis

This section presents empirical tests that examine whether, how effectively, and through what incentive channels the JPX400 influenced corporate behavior.

4.1 The JPX400 Inclusion Threshold and Future ROE

Table 3 reports the results from estimating various versions of equation (2). Column 1 estimates a basic DID specification, without time- or industry-fixed effects and without any firm-level controls. The DID estimate suggest that, compared to lower-ranked and less affected firms, the firms most affected by index-inclusion incentives experienced a statistically significant (at the 1% level) relative increase in ROE of 2.8 percentage points.

Noteworthy is the statistically insignificant coefficient (at the 10% level) on *Treat*, suggesting that the treated and control firms are similar in mean *Forward ROE* during the pre period. Also noteworthy is the positive and significant (at the 1% level) coefficient on *Post*, an estimate of the secular trend in ROE, suggesting that control-group firms experienced a 1.8-percentage-point-higher ROE in the post period. One interpretation of this coefficient is that it captures the effects of the other contemporaneous governance reforms, such as the Corporate Governance Code and the Stewardship Code. If so, our DID estimate suggests that the incremental effect on ROE of being close to the inclusion threshold is at least as large as the effects of all other contemporaneous reform efforts. Alternatively, to the extent that the *Post* coefficient reflects in part the effects of the 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.

Most notably, the coefficients on *Treat*×*Post* remain similar in magnitude and in 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 concern 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 as the contemporaneous control. The DID coefficient remains statistically significant at the 1% level, suggesting that our effects are not driven by a secular proportional increase in ROE in the post-period.

We offer several ways of interpreting the magnitude of these documented effects, and in particular of 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 in their probability of inclusion. This is the case in part because an improvement in ROE would affect firms' rankings along all dimensions—operating profits and market capitalization. We find that, for firms included in the index and also in our treatment group, a reduction in profitability of this magnitude (i.e., dropping to what the firm's profitability would be absent the estimated average treatment effect of 2.4% in ROE) would cost an average of 83 ranks, holding other firms' behavior fixed.[8] Another benchmark is the pre-period treatment-group mean, against which the main effect of Column 5 indicates a 40% proportional increase. Although this finding suggests a very large effect relative to the Japanese benchmark, it reflects the relatively low ROE and relatively large amount of slack in Japan. Put differently, relative to the U.S. benchmark cited in the Ito report (Ito, 2014), this main effect would close 15% of the ROE gap for our treatment firms.

4.2 Testing the Identifying Assumptions

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 (that is, that they follow parallel trends). We provide two tests of this 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. 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

[8] We calculate this quantity by estimating a firm-level profitability treatment effect (2.4% times contemporaneous common equity) and then recalculating 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 recalculate 3-year average ROE, subtract it after tax adjustment to recalculate 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.

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 on 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 do not find evidence of differential pre-period trends, 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 rankings and 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 rankings *after* 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 (and statistically indistinguishable from) each other in magnitude, and both statistically significant at the 5% level.

Column 3 provides an alternative test using firms ranked 1–300 as contemporaneous placebo treatments. Again, we find that the coefficient on *Rank 1–300*×*Post* is (1) statistically no different from zero at the 10% level, and (2) statistically different, at the 1% level, from the DID coefficient. Thus, firms ranked 1–300 exhibit no differential response in *ROE* as compared to firms ranked 501–800.

Column 4 provides another alternative cross-sectional placebo test using firms outside of the "ranked set" and thus ineligible for JPX400 selection. We rank these firms according to equation 1, construct placebo treatment and control groups using these ranks—among the ineligible firms, those ranked 301–500 are considered treated and those ranked 501–800 controls—and re-run our main test. The estimated coefficient on *Placebo Group Treat*×*Post* is statistically no different from 0 at the 10% level. Consistent with the findings in Columns 2 and 3, and supportive of the parallel-trends assumption, these findings suggest that it is unlikely that some omitted factor correlated with JPX400 rankings 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 the threshold of index inclusion. In particular, in Columns 2 and 3 we show that firms on either side of the threshold of inclusion improved ROE by virtually identical magnitudes. Moreover, firms further below the threshold (ranked 501–800) and those ranked highest (1–300) exhibit similar ROE improvement, both significantly lower than the improvement at firms near the threshold of inclusion.

Next, 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 win, or to avoid losing, membership in the index.

4.3 Consequences of Index Inclusion

We next turn to examining *why* managers may have been motivated to pursue or maintain membership in the JPX400. We begin by studying the consequences of realized inclusion in the JPX400 in terms of firms' *ROE*, *Sales Growth*, *Executive Compensation*, *Employee Compensation*, *Illiquidity*, and *BM*.

Our empirical tests are motivated by the empirical design in Appel et al. (2016) but tailored to our setting. In particular, we regress the outcome of interest on *JPX400 Member*, an indicator of firms' actual JPX400 membership status in a given year; *Centered Rank*, the firm's total rank minus 400; *Log Market Cap*; *ROE*; and time and industry dummies. We restrict our analysis to 50 firms on each side of the threshold for inclusion, i.e., firms ranked 351–450.[9] Our identifying assumption is that, after controlling for the major determinants of index inclusion (synthetic rank, Market Cap, and ROE), variation in JPX400 status within this narrow bandwidth is plausibly exogenous to the outcome variables we examine, and consequently that the coefficient on *JPX400 Member* identifies the effect of inclusion in the index *per se*.

Table 5 tabulates the results of this analysis. Column 1 indicates that firms did not enjoy a statistically measurable improvement in ROE simply as a result of index inclusion, a finding consistent with the main ROE effect being a consequence of firms' *ex ante* inclusion incentives. Columns 2–6 all additionally report null results: there is no statistically measurable benefit of inclusion in sales, executive compensation, average employee pay, liquidity, or book-to-market ratio.[10]

That firms and managers do not appear to derive any measurable benefits on those dimensions from JPX400 inclusion may be surprising, but these results can be explained by institutional factors or by recent work on the capital-market effects of ETFs. Though the JPX400 is seen as highly prestigious by the Japanese business, finance, and political communities, it is less salient for ordinary Japanese consumers. Thus it is not clear that mere inclusion in the index would generate significant product-market benefits (such as pricing-power and brand-value benefits) that could drive improvements in *Sales Growth* and

[9]In one year, two firms are tied for rank 450 (each assigned the rank 450.5), and thus dropped by our filter, yielding 299 observations for each specification.

[10]In untabulated tests we find very similar results, both qualitatively and quantitatively, when we use wider bandwidths of 251-550 and 151-650. We also find very similar results using a "fuzzy" regression discontinuity design (FRD), using a larger bandwidth of firms around the rank threshold of 400, and instrumenting for *JPX400 Member* with an indicator for crossing the rank threshold. None of the coefficients on *JPX400 Member* in the FRD are statistically significant at the 10% level. 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 encountering weak-instrument problems that might affect a FRD in a narrow bandwidth (Appel et al., 2016).

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 has been documented (Nakazato, Ramseyer, and Rasmusen, 2011; Kato and Kubo, 2006) and as we confirmed in untabulated tests.[11] Thus, even if shareholders and directors pushed managers to pursue 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, closer examination of the institutional factors helps to explain these findings. In theory, inclusion in a stock index would improve liquidity and valuation multiples to the extent that it significantly changes 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, which 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 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

[11]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 the control group.

cases, the increase in total demand benefits all firms that belong to the TOPIX index, which remains the Japanese index most frequently tracked by institutional investors, and which especially benefits firms that belong to the Nikkei225, the index that the BOJ's program heavily targeted. Although the BOJ and the GPIF both allocated some of their capital to the JPX400, the amounts 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 increased allocation to equity. Also, recent work on ETFs challenges the assertion that inclusion in an index leads to capital-market benefits. For example, 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, 2017), and creates excess co-movement in stock returns (Da and Shive, 2018). Jointly, these patterns could explain why we do not observe empirical evidence of significant capital-market benefits from inclusion in the JPX400 *per se*.

4.4 Media Attention to Inclusion and Exclusion

Given the absence of *immediate* financial or monetary benefits from realized inclusion in the index, why were Japanese managers and firms motivated to pursue membership? One possibility—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.

A long literature in finance and economics (e.g., Dyck et al., 2008; Dai et al., 2015) has documented that media coverage can serve as an alternative mechanism for disciplining managers' behavior by influencing their reputation costs. In Table 6 we examine whether media attention to firms responded to changes in their JPX400 membership status. Our empirical design follows that of Table 5, but here the explanatory variables of interest are *Booted*, an indicator that evaluates to 1 if a firm was included in the index in the previous year and excluded in the current year; *Added*, which evaluates to 1 if a firm was previously excluded and newly added; and *Switch*, which evaluates to 1 if the firm was either booted or added. As in Table 5, the sample consists of firms ranked 351–450 in the years 2014-2016; we include time- and industry-fixed effects as well as linear controls for the primary determinants of JPX membership status: *Centered Rank*, *Log Market Cap*, and *ROE*. (We drop 2013 because the status of all 400 included firms changed, and none were booted, in that year.)

In Columns 1-3, the dependent variable is *No. of Media Articles*, a simple count of unique ("full") articles identified by Ravenpack in which given firm was mentioned between July and September of each year.[12] The positive and significant loading on *Switch* reported in Column

[12]To capture media attention in anticipation of the index turnover, we begin counting articles in July, when all information necessary to predict index inclusion is publicly available. To capture articles that respond to the index turnover, we also count articles in the two months after the August 1 announcement date. To

1 indicates that changes in firms' inclusion status generated greater media attention. 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 2013 mean for firms ranked 351-450. Column 2 reports the results of a specification that replaces *Switch* with separate indicators for *Added* and *Booted*; our results suggest that media coverage of JPX400 inclusion and exclusion respectively is not statistically distinguishable. Finally, Column 3 examines differential media attention to firms in the Nikkei225—historically Japan's leading stock index, closely tracked by institutional investors, and consisting of the largest, best-known, and most liquid firms in 36 industries. This specification interacts *Switch* with an indicator for *Nikkei225* firms and includes a Nikkei-specific intercept. The estimated coefficient on *Nikkei225* $\times$ *Switch* is large—representing 132 additional media mentions—and statistically significant.

Columns 4-6 examine an alternative measure of media attention, *Weighted No. of Media Articles*, which relaxes the relevance criterion and instead weights each article by the relevance score. The results using this alternative measure 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. The media response to *Switch* is notably higher for Nikkei225 firms.[13]

Collectively, these results suggest that inclusion in the JPX400 had implications for the

ensure that we identify articles directly related to the firm in question, we restrict ourselves in Columns 1-3 to articles with a Ravenpack relevance score greater than 75.

[13]In untabulated tests, we verify that these results are not sensitive to the choice of bandwidth and persist when we use samples of firms ranked 251-550 or 151-650. Moreover, using data from 2010-2012 and defining a placebo *Switch* measure, which switches on when firms cross the rank-threshold of 400, we do not find a positive and significant coefficient on Switch from estimating the specification in Column1, Table 6. Similarly, we do not find a positive and significant coefficient on Nikkei225×Switch from estimating the specification in Column 3, Table 6 using the pre-period sample. Placebo test results are again similar when we examine a wider bandwidth of firms ranked 251-550 or 151-650.

reputations of firms and their managers: media outlets reported on which firms were booted out and newly included. This scrutiny was especially pronounced for Nikkei225 firms.

4.5 Exploring Incentive Channels: Nikkei vs. Non-Nikkei

The findings reported in Sections 4.3 and 4.4 suggest that the *ex-post* consequences of JPX400 inclusion were reputation-related. But it remains possible 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. For example, firms may have expected capital-market or product-market benefits to manifest over the longer term, as the index became more popular.

To distinguish between these two possibilities, we examine cross-sectional variation in the treatment response in ROE between Nikkei225 and non-Nikkei225 firms. As we document above, the inclusion status of Nikkei225 firms is likely to elicit greater public scrutiny; thus Nikkei225 firms are more likely to be attuned to status or reputation concerns. On the other hand, because 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.[14]

[14]One way to think about this is that the treatment effect on non-Nikkei225 firms represents an upper-bound estimate of the treatment response from 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 inclusion are probably greater than those of Nikkei225 firms. Conversely, the treatment effect on Nikkei225 firms represents an upper-bound estimate of

We estimate a difference-in-difference-in-differences (DDD) specification to examine the differential responses in ROE between Nikkei225 and Non-Nikkei225 firms:

$$\begin{aligned} ROE_{i,t+1} &= \alpha + \beta_1 T_{i,t} \times Nikkei225_{i,t} \times Post_t + \beta_2 T_{i,t} \times Post_t + \beta_3 T_{i,t} \times Nikkei225_{i,t} \\ &+ \beta_4 Nikkei225_{i,t} \times Post_t + \beta_5 Nikkei225_{i,t} + \beta_6 T_{i,t} + \gamma X_{i,t} + f_t + \epsilon_{i,t}. \end{aligned} \tag{3}$$

Here, $T_{i,t}$ denotes the treatment variable of interest; we use the two treatment-intensity measures in prior analyses, the binary *Treat* measure and the more continuous *Quintile(Closeness)* measure. In this regression, we are interested in the sign and significance of the triple-interaction coefficient. Under the status-incentive hypothesis, β_1 should be positive and statistically significant.

Table 7 reports the results of our empirical tests. Column 1 estimates equation 3 using our original sample (i.e., in Table 3) and treatment and control definitions. We find a positive DDD coefficient of 0.038, but it is not statistically significant at the 10% level (with a p-value of 13%). This is unsurprising given the relatively sparse presence of Nikkei225 firms in the control group (firms ranked 500-800) and the demanding DDD estimation.

To increase the statistical power of our tests, Column 2 enlarges the sample to include firms ranked 1–300 as controls (as in Table 4, Column 3); Column 3 incrementally includes firms ranked 801–1,000 as controls (as in Table 4, Column 5). With the larger estimation samples we obtain a positive and statistically significant (at the 10% and 5% levels) DDD coefficient, suggesting a stronger ROE response for Nikkei225 firms close to the threshold of index inclusion.

Finally, Column 4 repeats the estimation using the more continuous *Quintile(Closeness)*

the treatment response from non-Nikkei225 firms that could have resulted from status concerns alone.

treatment measure and the sample of firms ranked 1-1000 (as in Table 4, Column 5). Again, we find a positive and significant (at the 10% level) coefficient on the triple interaction term, further evidence of a stronger ROE response for Nikkei225 firms due to stronger index-inclusion incentives.

Collectively, our results support the hypothesis that managers' concerns for status and reputation play 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. Columns 1-6 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 by 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 no statistically measurable treatment effect on *Asset Turnover*, suggesting that it 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 on profit margin.

In Column 5, we do not find evidence that financial leverage was impacted by firms' JPX400-inclusion incentives. This finding is not entirely surprising, since the 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—might increase its ROE but would 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 to capture the JPX400's potential effects on firms' capital-structure decisions. Thus we supplement our 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.

Column 6 reports that treated firms increased payouts by 0.59 percentage points. This 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 baseline. 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.

Table 9 further examines whether managers attempted to improve ROE by managing accruals or by cutting productive investments (e.g., Stein, 1989; Healy, 1985). We explore these possibilities by estimating the incentive effects of the JPX400 on six different forward outcome variables of interest: *Accruals to Assets*, *Log NOA*, *R&D to Sales*, *Log Employees*, *Log Average Employee Pay*, and *Log Book to Market*. Each regression in the 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.

We do not find evidence in Column 1 that managers increased accruals in response to their index-inclusion incentives. To the extent that such accrual-based earnings management is expected to reverse, however, 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.

Alternatively, managers may be incentivized to change their real investments, such as by shedding loss-making divisions, increasing market capitalization through acquisitions, or reducing R&D expenses. In Column 2, we do not find evidence that index-inclusion incentives drove managers to change firms' net operating assets. Together with our evidence on net shareholder payouts, firms did not appear to seek increasing market capitalization as a primary means of attaining index inclusion. Column 3 shows, however, that index-inclusion incentives are associated with a decline in R&D. The point estimate 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. We are unable to ascertain whether these cuts represent a form of real earnings management or an improvement in R&D discipline. Nevertheless, cutting discretionary expenses like R&D does not appear to be the main driver of companies' efforts to improve margins and ROE: this effect explains about 15% of the average treatment effect on profit-margin improvement.

Columns 4 and 5 examine firm-level employment and employee pay. We do not find evidence of significant changes in either of these variables as a result of firms' index-inclusion incentives. That managers did not improve ROE by cutting employees or employee compensation is consistent with the stickiness of wages and the tradition of long-term employment in Japan.

Finally, Column 6 examines the market's assessment of the activities that managers engaged in to improve ROE—namely, improving ROA and profit margins, in part by cutting R&D intensity. We do so by estimating the treatment effect on *Log Book to Market*. The point estimate of -.028 represents a 2.8% improvement in book-to-market relative to the pre-period treatment-group mean, and is statistically significant at the 10% level. In conjunction with Table 5, our finding supports the hypothesis that 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. These results also suggest that this revision was not compensated for by a commensurate increase in expectations of firm risk.

5 Discussion

We synthesize and provide a discussion of the empirical results in the paper.

5.1 Overall Evaluation of the Index

We begin with the aggregate 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, which is significant at the 5% level. 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 corporate profits over our post-period.

To estimate total shareholder-wealth 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 overall market capitalization in 2014. A less conservative incremental P/E ratio would be the treatment firms' cash-adjusted P/E ratio: $(MCap - Cash)/NetIncome$.[15]

[15] 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 unlikely 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

This would imply a valuation impact of JPY61.45 trillion, or approximately 13.10% of total market capitalization in June 2014. The midpoint between those two estimates, 6.94%, would account for about 20% of 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.

However, our findings also suggest possible flaws or tradeoffs in the design of the JPX400 index. 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 (see Table 3, Column 5), and that, using firms ranked 501–800 as a benchmark, "safe" included firms (ranked 1–300) generated 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, more firms might have exerted effort to improve their performance. (Although widening the "incentive bandwidth" in this way could also generate undesirable tradeoffs.) Second, although the market-valuation results show that the market did not perceive treated firms' overall efforts as shareholder-value-destructive, competition for inclusion over a longer time horizon might induce firms to seek undesirable channels to boost ROE as they exhaust relatively easy avenues for improving profitability. In other words, although the index appears to be effective at incentivizing managers to change behavior, the focus on ROE as a performance metric could ultimately lead managers to respond in value destructive ways.

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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.

5.2 External Validity of Findings

The JPX400 appears to have been effective at motivating managers to improve ROE, but the extent to which these findings apply to other indices and to non-Japanese settings remains an open question. The notion that a stock index serving as a gauge of virtuous firms could incentivize managers to change behavior is, we believe, at minimum applicable to East Asian economies, such as China, Korea, Singapore, or Taiwan, that share cultural roots with Japan (as well as patterns of low capital efficiency and weak *de facto* shareholder rights).

Whether such incentive effects can be expected in Western economies is more speculative. Decisions by the S&P Dow Jones and the FTSE Russell in mid-2017 to exclude certain firms with classified shares from their main indexes, and the increasing number of stock indexes based on Environmental, Social, and Governance themes, suggest a hope that the promise of index inclusion—when it can be influenced by direct managerial action—can motivate managers to change their behavior. And in the West, as in Japan, status and reputation could be important reasons why managers do so. In an interview with one of us, David Blitzer—the head of the Index Committee at S&P Dow Jones—cited two main reasons why he believes U.S. managers might be willing to change corporate policies to gain S&P500 membership: "managerial ego" and "a stable shareholder base."

Our finding that managers respond to status incentives is, we believe, broadly generalizeable. This idea is not new: Adam Smith famously wrote that "rank among our equals, is, perhaps, the strongest of all our desires" (Smith, 1759). Recent empirical evidence confirms that this desire explains significant managerial decisions. For example, examining a sample of U.S. executives who engaged in large acquisitions between 1986 to 1988, Avery et al. (1998) suggests that CEOs' acquisitions are rewarded not with compensation gains but with enhanced prestige and standing in the business community. The more recent work of Focke et al. (2017) documents that U.S. CEOs working at more prestigious firms (firms on *Fortune*'s most admired companies list) receive on average 8% lower compensation, and suggests that U.S. CEOs are willing to trade off monetary compensation for the status of working for a publicly admired company. Masulis and Mobbs (2014) shows that independent directors care more about and pay greater attention to their more prestigious directorships. This evidence, to which we contribute, implies that status incentives can be *actively* employed to influence the behavior of corporate managers.

6 Conclusion

This paper provides evidence that the JPX400 was effective at changing certain firms' behavior. The index—thanks to the certification provided by the GPIF and reinforced by the media—came to be seen as a virtuous club of Japan's best-run companies. And managers competed to be included by improving their profitability and capital efficiency, due in part to reputational concerns: the prestige of inclusion and fear of the shame of exclusion.

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 possibility is that, by incentivizing some of the most respected firms in the Japanese market (e.g., those in the Nikkei225) to change their behavior, the JPX400 could instigate a broader shift in corporate norms toward more shareholder-friendly policies and improved capital efficiency. Another possibility is that the index's focus on ROE could lead to perverse and myopic behavior that harms aggregate economic growth in the long run.

But the surprising efficacy of the index to date points to the potential of this alternative corporate-governance mechanism. Given growing calls in the United States and Europe to limit executive compensation, a move that could constrain formal incentives, it may become increasingly important to understand and develop novel mechanisms for shaping corporate behavior. We look forward to further research in this area.

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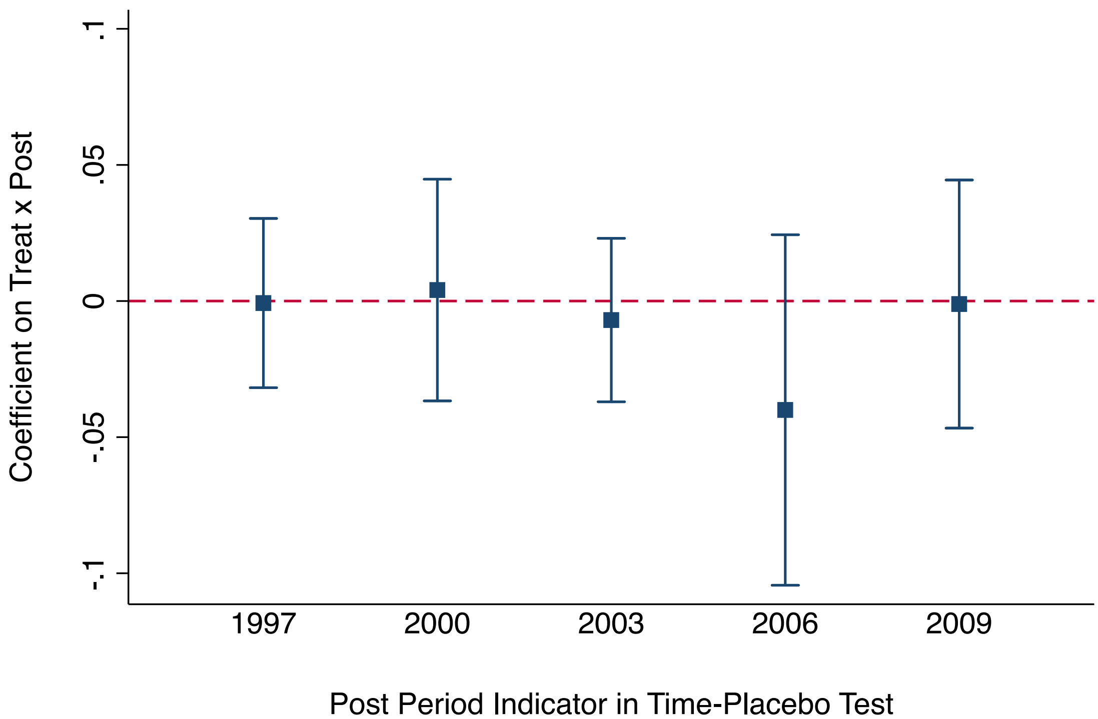
Fig. 1. Placebo Tests by Time

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])

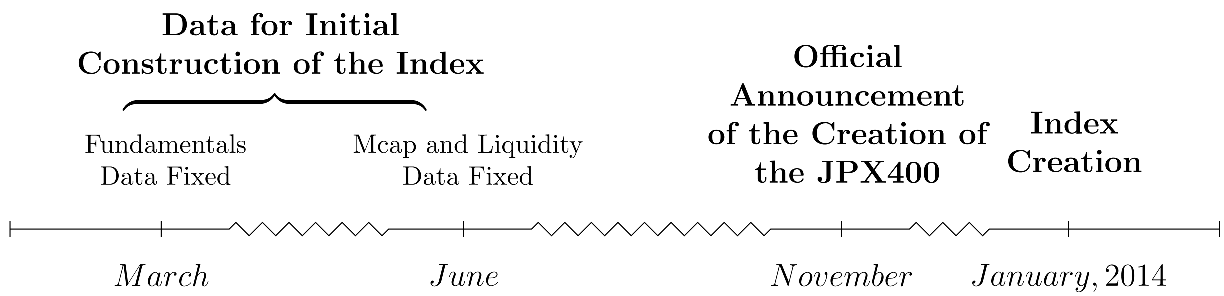
Appendix A Continued

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		Unique number of "full articles" in Ravenpack with a *Relevance* of more than 25 (as coded by Ravenpack) weighted by the *Relevance*

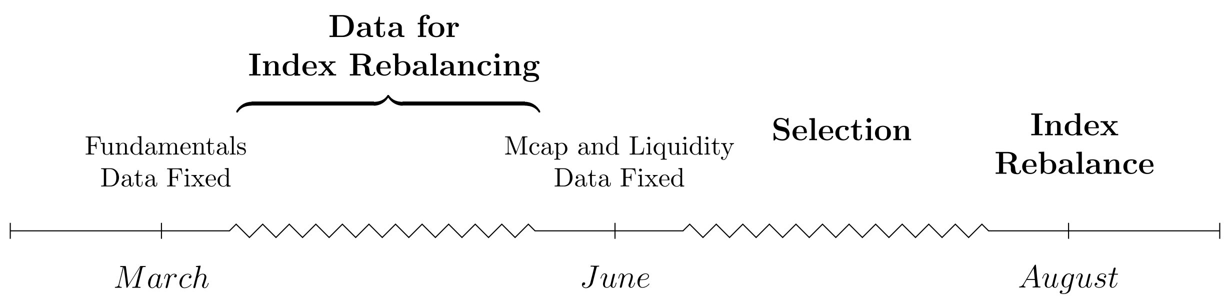
Appendix B Timeline, Selection of JPX400 Index Constituents

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.

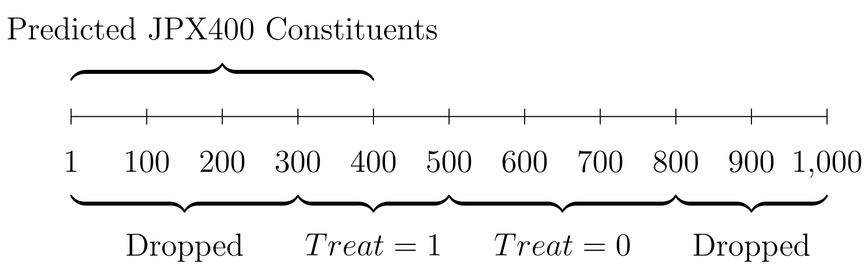


Table 1. Predicting JPX400 Membership

Columns 1 and 2 report estimates from OLS regressions of *JPX400 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. Two other indicators—for ranks within 401–600 and over 800—are also included in these regressions. Columns 2 and 4 include other specified firm-level controls. The sample consists of firms with synthetic ranks of 1–2,000 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) *JPX400 Member*	(2) *JPX400 Member*	(3) *JPX400 Member*	(4) *JPX400 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 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	0.874	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) *Forward ROE*	(2) *Forward ROE*	(3) *Forward ROE*	(4) *Forward ROE*	(5) *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 351-450. 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 *JPX400 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*
JPX400 Member	-0.003	0.020	0.077	0.006	0.064	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	0.270***	0.065***	-1.111***	-0.217***
	(0.00)	(0.01)	(0.04)	(0.02)	(0.10)	(0.05)
ROE	0.871***	0.172	0.180	-0.228	-1.098	-6.029***
	(0.09)	(0.23)	(0.66)	(0.26)	(1.21)	(0.93)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Ind FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	299	299	299	299	299	299
R^2	0.4914	0.2171	0.2182	0.2411	0.6013	0.4421

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 351–450. 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. Standard errors, clustered by firm, 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	36.327*		3.541	70.255**		19.788
	(19.73)		(7.06)	(32.11)		(20.74)
Added		53.625			85.081	
		(56.02)			(80.07)	
Booted		27.493			62.684	
		(25.16)			(41.78)	
Nikkei225 x *Switch*			132.846*			203.883**
			(69.57)			(101.99)
Nikkei225			2.407			0.065
			(13.72)			(18.82)
Centered Rank	-0.174	-0.121	-0.081	-0.341	-0.296	-0.203
	(0.15)	(0.17)	(0.13)	(0.22)	(0.23)	(0.19)
Log Market Cap	45.450***	44.015**	35.782**	59.784***	58.555**	45.879**
	(16.03)	(17.52)	(15.18)	(21.12)	(22.63)	(19.20)
ROE	137.211	115.584	129.014	153.964	135.428	137.980
	(95.01)	(74.03)	(84.90)	(142.84)	(103.71)	(125.84)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	299	299	299	299	299	299
R^2	0.3404	0.3393	0.3694	0.3203	0.3182	0.3529
F-stat		0.140	3.646		0.049	3.997
p-value		0.709	0.058		0.825	0.047

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

Table 7 reports the results of estimating difference-in-difference-in-differences specifications (equation 3), testing for differential treatment response of Nikkei225 firms. In all columns we include time-fixed effects, firm controls, and contemporaneous ROE (as in our main specification, from Column 5 of Table 3). *T* denotes the treatment variable used for a particular specification. In Column 1, we use our main treatment and control definition, with firms ranked 301–500 as *T* with firms ranked 501–800 included in the estimation sample as controls. In Column 2, we add firms ranked 1–300 to the estimation sample as controls. In Column 3, we additionally add firms ranked 801–1,000 as controls (i.e., use the full "ranked set" as the estimation sample). In Column 4, we use the full ranked set, but use our more continuous measure of treatment intensity (*Quintile(Closeness)*) as *T*. 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 15%, 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)
Dep Var:	*Forward ROE*	*Forward ROE*	*Forward ROE*	*Forward ROE*
T:	*Treat*	*Treat*	*Treat*	*Quintile(Closeness)*
T x *Nikkei225* x *Post*	0.038^{+}	0.042*	0.050**	0.015*
	(0.03)	(0.02)	(0.02)	(0.01)
T x *Post*	0.014**	0.012**	0.012**	0.003
	(0.01)	(0.01)	(0.01)	(0.00)
T x *Nikkei225*	-0.051**	-0.050**	-0.055**	-0.017***
	(0.02)	(0.02)	(0.02)	(0.01)
Nikkei225 x *Post*	0.013	0.010	0.006	-0.018
	(0.02)	(0.01)	(0.01)	(0.02)
Nikkei225	0.006	-0.001	-0.007	0.019+
	(0.01)	(0.00)	(0.01)	(0.01)
T	0.004	-0.005	0.001	0.003+
	(0.01)	(0.00)	(0.00)	(0.00)
ROE	0.380***	0.450***	0.373***	0.366***
	(0.15)	(0.11)	(0.05)	(0.05)
Log Market Cap	-0.011*	-0.003+	0.001	0.000
	(0.01)	(0.00)	(0.00)	(0.00)
Log Book to Market	-0.045***	-0.045***	-0.049***	-0.048***
	(0.01)	(0.01)	(0.01)	(0.01)
Sales Growth	-0.012	-0.012	-0.014	-0.013
	(0.02)	(0.02)	(0.01)	(0.01)
LT Debt to Equity	-0.007	-0.003	-0.003	-0.004
	(0.00)	(0.00)	(0.00)	(0.00)
Cash to Equity	0.001	0.005	0.003	0.003
	(0.01)	(0.00)	(0.00)	(0.00)
Time FE	Yes	Yes	Yes	Yes
Observations	2,783	4,462	5,546	5,546
R^2	0.3084	0.3557	0.2656	0.2663
Sample	301-800	1-800	1-1000	1-1000

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, Investments, and Book-to-Market Multiple

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*, *Log Average Executive Pay*, and *Log Book-to-Market*. 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*	(6) *Forward Log BM*
Treat x *Post*	0.0017	0.0329	-0.0011*	0.0050	0.0045	-0.0281*
	(0.002)	(0.022)	(0.001)	(0.011)	(0.005)	(0.017)
Treat	0.0003	0.0038	0.0011**	0.0044	-0.0117***	0.0114
	(0.002)	(0.014)	(0.000)	(0.009)	(0.004)	(0.012)
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 Book to Market	0.0048***	0.2935***	0.0003	-0.0449***	-0.0063**	0.8617***
	(0.002)	(0.101)	(0.000)	(0.009)	(0.003)	(0.011)
Log Market Cap	-0.0007	0.3007***	-0.0003	0.0060	0.0077***	-0.0152***
	(0.001)	(0.097)	(0.000)	(0.007)	(0.002)	(0.005)
Sales Growth	0.0250***	0.1630***	0.0035***	0.0505**	0.0049	0.2264***
	(0.006)	(0.050)	(0.001)	(0.020)	(0.010)	(0.033)
LT Debt to Equity	0.0003	0.2083***	-0.0001	0.0110**	-0.0024	0.0028
	(0.001)	(0.068)	(0.000)	(0.006)	(0.002)	(0.008)
Cash to Equity	0.0125***	-0.2537***	0.0005	-0.0185**	-0.0036	-0.0399*
	(0.002)	(0.098)	(0.000)	(0.009)	(0.004)	(0.023)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,779	2,710	2,785	2,654	2,560	2,725
R^2	0.2295	0.9618	0.9260	0.9883	0.9209	0.8675

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