Governance through Shame and Aspiration: Index Creation and Corporate Behavior

Akash Chattopadhyay
University of Toronto

Matthew D. Shaffer
Harvard Business School

Charles C.Y. Wang*
Harvard Business School

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Abstract

After decades of both de-prioritizing shareholders' economic interests and low corporate profitability, Japan introduced the JPX400 in 2014. The index highlighted the country's "best-run" companies by annually selecting the 400 most profitable among Japan's large and liquid firms. Index-inclusion incentives led firms to increase ROE proportionally by 41%, though firms did not realize significant capital-market or product-market benefits from inclusion. Status incentives contributed to the observed performance improvement. Back-of-the-envelope estimates suggest that JPX400-inclusion incentives accounted for 16% (20%) of the growth in aggregate earnings (market capitalization) over our sample period. Stock indexes can transform longstanding behavior via non-pecuniary channels.

Keywords: JPX-Nikkei 400 index; Corporate governance; Status incentives; Return on equity; Capital efficiency; Social norms

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

*First draft: July 2017. Chattopadhyay (`akash.chattopadhyay@rotman.utoronto.ca`) is an Assistant Professor of Accounting at the University of Toronto. Shaffer (`mashaffer@hbs.edu`) is a doctoral candidate at Harvard Business School. Wang (`charles.cy.wang@hbs.edu`) is the Glenn and Mary Jane Creamer Associate Professor of Business Administration at Harvard Business School. We thank Chad Cecere and Yasunori Nakagami for helpful insights on Japanese corporate governance. We thank Ray Ball, Alma Cohen, Brandon Gipper (FARS discussant), Simon Glossner, Ian Gow (SOAR discussant), Jody Grewal, Paul Healy, Willian C. Johnson, Takuma Kochiyama (TJAR discussant), Ben Mathies, Pedro Matos, V.G. Narayannan, Krishna Palepu, Lee Radebaugh, George Serafeim, Doug Skinner, Eugene Soltes, Cong Wang (SFS discussant), and T.J. Wong as well as seminar participants at Harvard Business School, American University, University of Virginia, Georgetown University, George Washington University, University of Texas Austin, University of Chicago, Shanghai University of Finance and Economics, Fudan University, and Suffolk University, and conference participants at the Financial Accounting and Reporting Section Midyear Meeting, the Hawaii Accounting Research Conference, the Japanese Accounting Review International Conference, the Society for Financial Studies Cavalcade, the SOAR Accounting Symposium, and the Paris Financial Management Conference, for helpful comments and suggestions. We also thank Raaj Zutshi for excellent research assistance, and Naoko Jinjo and Nobuo Sato for coordinating interviews in Japan. Comments are welcome.

1 Introduction

How can persistent economic behavior be changed? The standard approach in economics is to focus on formal contracts and pecuniary rewards to incentivize the desired behavior. Another approach is through non-pecuniary strategies—such as changing norms or the status of behaviors—that have increasingly attracted the attention of scholars in economics and adjacent fields (e.g., Goode, 1978; Guiso, Sapienza, and Zingales, 2015b). This approach could be especially valuable in settings that impose constraints on formal incentives or their effectiveness. In corporations, for example, persistent culture (Guiso, Sapienza, and Zingales, 2015a) or imbalance in bargaining power between CEOs and boards (Bebchuk and Fried, 2003) may render formal incentives an inefficient channel by which changes in corporate behaviors can be induced.

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

But little is known about whether, how, or how effectively stock indexes can influence corporate behavior. One theory is that stock indexes intensify the 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 et al., 2015b), thereby leveraging corporate managers' status incentives to facilitate changes in behavioral norms.

Motivated by this phenomenon, and by the broader question of how to change persistent behavior, 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. Our setting is Japan, whose persistently low corporate capital efficiency—specifically, low return on equity—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 contracts have limited effectiveness in such a 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). Created in collaboration with the Japan Exchange Group, the parent of Japan's two largest stock exchanges (Tokyo Stock Exchange and Osaka Stock Exchange), the JPX-Nikkei Index 400 (JPX400) debuted in 2014. Considered "the shiniest toy in the Abenomics box" (Lewis, 2017), this index showcased the 400 large and liquid Japanese firms that performed best in terms of profitability, capital efficiency, and (by extension) corporate governance. Membership was considered highly prestigious, a status attained in part due both to formal endorsement of the index by the Government Pension Investment Fund (GPIF) and to intense media coverage of its membership churn. The JPX400's status as the gauge of Japan's best-run companies is evident in its colloquial nickname, "the shame index," in reference to the experience of excluded firms.

In this paper, we study whether the *ex ante* JPX400-inclusion incentives induced Japanese managers to improve capital efficiency and, if so, why. Because of the norms of low compensation and low pay-performance sensitivities, and because index membership *per se* offers no formal financial benefits (i.e., in the form of a direct monetary reward), agency theory would not predict the JPX400 to have an effect on corporate managers. However, non-pecuniary incentives may play a role in this institutional setting, where managers could be expected to care about the status conveyed by index membership, or the indirect benefits associated with such status. Thus, this setting could shed light on the motivating power of status incentives.

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

[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.

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

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

We document three main empirical findings. First, firms closest to the JPX400-inclusion threshold achieved differential and economically significant improvement in ROE. We estimate that these firms increased ROE by 2.4-percentage-points, a 41% increase relative to the pre-period mean for treated firms. A battery of tests supports the hypothesis that this effect is driven by index-inclusion incentives—that is, by the firms' efforts to improve ROE in order to be included in (or to avoid exclusion from) the index. We document that the ROE improvements: (1) do not appear to be driven by differential trends; and (2) are declining in a firm's distance from the threshold of inclusion. Moreover, leveraging a fuzzy regression discontinuity design, we show that our documented

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

effect is not a realized consequence of index inclusion.

Second, we show that the status associated with the index significantly contributed to firms' motivations to improve ROE. Implementing fuzzy regression discontinuity tests, we show that firms did not realize significant *ex post* capital-market, product-market, or executive-compensation benefits from inclusion in the JPX400 *per se*: this is true of the larger firms belonging to the Nikkei225 index as well as the smaller non-Nikkei225 firms. We further examine whether firms' *ex ante* expectations of capital- or product-market benefits drove the observed improvement in ROE, or whether status incentives played a role. To distinguish between these explanations, we compare the ROE response of non-Nikkei225 firms to that of firms belonging to the Nikkei225 (Japan's longstanding leading index, consisting of its most well-established and well-known firms). By virtue of belonging to a closely tracked and salient market index, Nikkei225 firms already enjoyed the highest liquidity in the Japanese market; thus the *incremental* capital-market benefits of inclusion in the JPX400 would presumably be more important to non-Nikkei225 firms. But Nikkei225 firms would probably also be more sensitive to status incentives, specifically fear of loss of status if they were not included in the new gauge of Japan's best-run corporations. Consistent with a significant role for status incentives, our DID and triple-differences tests show that Nikkei225 firms exhibit significantly larger ROE responses to index-inclusion incentives than non-Nikkei225 firms.

Third, we show that firms improved ROE by means of improved margins, asset efficiency, or shareholder payouts. Specifically, they exploited the channels that offered them the most slack to improve their ROE: those with below-median profit margins differentially improved their margins; those with below-median asset turnover improved efficiency; and those with above-median cash-to-equity ratios increased shareholder payouts. We find evidence that firms improved margins in part by cutting discretionary expenses like research & development; however, the economic magnitude of this effect is small and does not drive the main effect in ROE. We do not find evidence that treated firms improved ROE by means of accounting-based earnings management, cutting capital investments, or reducing employment or average employee pay. Overall, the firms' improvements in ROE, rather than inclusion in the index *per se*, led to higher market valuations.

Overall, the JPX400 performed well as a mechanism for improving persistently low profitability at some Japanese firms. In fact, our analyses suggest that, in the aggregate, instituting the JPX400 made a significant contribution to the overall market in terms of profitability and market capitalization. Our back-of-the-envelope estimates suggest that introduction of the JPX400 increased aggregate net income by JPY1.2 trillion per year, a figure that represents an 8.9% increase over the pre-period average aggregate income of Japanese firms, or 16% of the change in average annual aggregate net income between the pre-period and the post-period. Moreover, we estimate that the incremental total earnings attributable to the JPX400 represent a 6.9% increase in Japanese market capitalization, accounting for 20% of overall market-capitalization growth between June 2014 and June 2017.

Our work makes several significant contributions. First, we contribute empirical evidence to the literature on the role of status-based incentives in human motivation. Adam Smith famously wrote "... rank among our equals, is, perhaps, the strongest of all our desires" (Smith, 1759). Despite a recognition of its importance and a significant body of theoretical work on status incentives (e.g., Williamson, 1963; Hayek, 1967; Harbaugh, 1998a; Moldovanu, Sela, and Shi, 2007; Auriol and Renault, 2008; Besley and Ghatak, 2008; Dubey and Geanakoplos, 2010), there has been relatively little empirical evidence of their motivating power in economic contexts. Recent empirical work, largely experimental, has focused on the role of awards and tournaments in organizational contexts (e.g., Ariely, Bracha, and Meier, 2009; Kosfeld and Neckermann, 2011). Most relevant to our work is the small and growing literature on the role of status incentives in the behavior of corporate executives and directors (e.g., Avery, Chevalier, and Schaefer, 1998; Malmendier and Tate, 2009; Masulis and Mobbs, 2014; Raff and Siming, 2017; Focke, Maug, and Niessen-Ruenzi, 2017). For example, Avery et al. (1998) and Malmendier and Tate (2009) infer status incentives arising from acquisitions and business-press awards respectively, by examining ex-post payoffs to CEOs; Focke et al. (2017) examines executives' compensating differential for firm prestige. Perhaps closest in spirit to our work is Masulis and Mobbs (2014), which examines the incentive effects of firm prestige on independent directors' performance. We extend this literature by showing that status incentives can be actively employed to influence the behavior of a large number of firms in a setting with relatively strong identification.

Our findings also speak to a broad literature in economics and sociology on how persistent behaviors or norms can be changed (Goode, 1978; Guiso et al., 2015a,b), and in particular on how the allocation of status can affect norms and the resource-allocation process (Zingales, 2015). Our study highlights a novel mechanism—a stock index—for instigating change in corporate norms, which are persistent (Guiso et al., 2015a). Our evidence provides support for the theory that, in settings where formal incentives are limited in their ability to influence persistent behavior, changing the status hierarchy of desired behaviors can induce the desired changes (Guiso et al., 2015b). Moreover, our findings are consistent with the observation and theory of Goode (1978) that changes in social and cultural norms tend to be precipitated by a shift in behavior within a small group of social elites that can "lead by example" (Guiso et al., 2015b). By incentivizing some of the most established and respected firms in the Japanese market (e.g., in the Nikkei225) to change their behavior, the JPX400 index mechanism could promote a broader shift in corporate norms.

Finally, we contribute to the literature on the effect of indexes. There has been abundant research on how index constituents behave and perform *in response to inclusion* in an index, including the capital-market consequences thereof (e.g., Shleifer, 1986; Harris and Gurel, 1986; Chen, Noronha, and Singal, 2004; Greenwood, 2008; Doh, Howton, Howton, and Siegel, 2009; Boone and White, 2015; Appel, Gormley, and Keim, 2015). By contrast, our study provides large-sample evidence that the desire for inclusion (or to avoid exclusion) can powerfully motivate changes in firm behavior. Overall, insights from our analyses could be valuable to policy makers seeking to leverage non-pecuniary channels of influence on corporate managers. Such channels may be particularly useful in the context of increasing pressures, in the United States and abroad, to limit executive compensation, and in developing-market contexts where there may be greater constraints to contracting.

2 Background

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

2.1 Governance, Reform, and Corporate Culture in Japan

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

Over the years the Japanese legal system developed strong formal legal rights to shareholders—arguably even stronger than the statutory rights enjoyed by U.S. shareholders. For example, any 1% shareholder in Japan can can submit a proposal to appear on ballots at the company's annual general meeting; poison pills are highly restricted; and a director can be elected or dismissed by a simple majority shareholder vote at any time, making staggered boards meaningless (Hill, 2017). Moreover, various institutional-reform efforts aimed to create a corporate-governance environment that would be more favorable to shareholders (e.g., Milhaupt, 2006; Allen, Carletti, and Marquez, 2015; Kato, Li, and Skinner, 2017).

However, shareholders' *de facto* power to influence management remained weak: many domestic investors appeared unwilling to exercise their rights in a manner that could create conflict with corporate management. This norm limited the role of the market for corporate control—takeovers, private equity, and shareholder activism—in resolving agency problems at Japanese firms. For example, in cases where activists or prospective acquirers turned hostile, they could rarely secure the votes of domestic retail, strategic, and institutional investors, who were typically unwilling to break the norm of siding with management (Puchniak and Nakahigashi, 2017), even when doing so was likely to convey economic benefits (Buchanan et al., 2013).[3] Thus, despite shareholders' strong formal statutory rights, the corporate culture of de-prioritizing shareholders' interests persists in Japan (Hill, 2017). In 2014 Japanese managers rated shareholders as their companies' fourth most

[3] As Buchanan et al. (2013) document, activist shareholders in the early 2000s were frequently able to compel open shareholder votes on proposals that cash-rich Japanese firms increase their payouts. They faced no formal structural barriers (such as from corporate charters, classified shareholders, or Japanese courts), but their proposals were simply voted down by investors loyal to management.

important stakeholders, behind customers, employees, and suppliers.[4]

The persistence of Japan's corporate culture of de-prioritizing shareholders, despite years of corporate-governance reform efforts, is consistent with the claim in Guiso et al. (2015a) that norms are generally sticky, slow to change, and often more powerful than formal laws or institutions in driving behavior. Many observers, including Japanese policy makers, have pointed to the country's stakeholder-oriented culture as an explanation for its lagging corporate capital efficiency—its low return on equity and low return on assets—and for decades of economic stagnation since the bubble economy of the 1980s (Ito, 2014).

By 2013, many policy makers had come to believe that improving Japanese corporations' capital efficiency was vital to reviving the economy—an urgent concern in light of Japan's looming problems. That year, the Ministry of Economy, Trade, and Industry commissioned a systematic review of "competitiveness and incentives for sustainable growth"; the result was the influential Ito Report (Ito, 2014), which notes:

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

These goals and concerns were embodied in the Abe's administration's "third arrow" of structural reforms—considered the most important component of Abenomics.[5] As Ito (2014) summarized, "ROE improvement can be regarded as the core of the third arrow of Abenomics." Among the various policy tools, Abe's administration hoped to change increasing corporate managers' dialogue with and focus on shareholders.[6] In other words, in its efforts to boost capital markets and revi-

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

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

[6] Two 2014 reforms by the Financial Services Agency were designed to encourage manager-shareholder dialogue: the Stewardship Code and the Corporate Governance Code. The Stewardship Code encourages institutional investors to pursue long-term returns and to engage companies in constructive dialogue. The Corporate Governance Code, a "bill of rights" for shareholders (Gow, Wang, Jinjo, and Sato, 2017), urges companies to respect shareholder rights, improve capital efficiency, engage investors in dialogue on a regular basis, and appoint at least two external directors

talize the economy, Japan sought to change the longstanding corporate norms that de-prioritized shareholders' economic interests.

2.2 The JPX-Nikkei 400 Index

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

Contributing to the elevation of the JPX400's status was subsequent media coverage. Each summer, the JPX reconstitutes its membership, dismissing firms that no longer make the cut and adding firms that have improved their performance. Although the index *per se* offers no direct financial benefits to its constituent firms or their managers in the form of a direct monetary award, the index's creation and its churn each August have generated substantial excitement and attention in the media. Its colloquial nickname, "the shame index," refers to the experience of firms that are excluded.

A unique feature of JPX400's selection criterion is that its algorithm is explicit, transparent, and, with the exception of a small number of "qualitative adjustments," based on publicly avail-

to their boards. Neither code is legally binding: neither institutional investors nor companies subject to the codes are required to abide by all of their principles; instead they are required to either comply or explain.

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

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

Why did policy makers expect the creation of the index to influence corporate behavior? One possibility, suggested by standard corporate financial theory, is that Japanese firms would compete for index inclusion hoping to realize capital-market benefits, such as increased demand for their shares or improved liquidity. Another possibility—one consistent with our interviews at the JPX and with the clever marketing of the JPX400 as the shame index—is that firms are motivated by the status conferred by inclusion in this elite new club. Cast in the terms of the Zingales (2015) framework, the JPX400 represents policy makers' attempt to leverage status incentives to direct

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

corporate behavior toward socially beneficial ends.

This use of a stock index to influence corporate behavior is a novel and emerging worldwide phenomenon. In the past decade, demand has been increasing for so-called ESG indexes, which some investors hope will influence firms to improve their environmental, social, and governance practices. In mid-2017, S&P Dow Jones and FTSE Russell announced that they would begin excluding firms with classified shares, that deviate from the "one share, one vote" principle, from some of their indexes in an apparent attempt to counter IPO trends.[8] The JPX400 is notable as the first instance of a central government deploying a stock index as a primary policy tool to change a persistent behavioral outcome (i.e., low corporate capital efficiency), an effort that it views as fundamental to the transformation and revitalization of the overall economy. This setting thus provides a unique laboratory to study the incentive effects of stock indexes, as well as mechanisms for influencing persistent behavior sustained by strong corporate norms. The remainder of the paper empirically examines whether, how effectively, and via what incentive channels the JPX400 influenced Japanese corporate behavior.

3 Empirical Design

We study how the JPX400's index-inclusion incentives affect firm performance. Firms nearer to the inclusion threshold are most likely to see their inclusion status change as a function of their performance. Thus we hypothesize that, all else equal, firms closer to the threshold of inclusion will differentially improve ROE, the pertinent performance metric that is most readily controllable by managers.[9]

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

[8] These controversial decisions stimulated robust debate over whether such exclusion was grounded in sound policy, whether it could effectively counter the trend toward dual-class IPOs, and whether it was an appropriate role for an index provider. In an open letter dated April 19, 2018, Blackrock argued that, though it was itself opposed to dual-class share structures, "policymakers, not index providers, should set corporate governance standards."

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

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

3.1 Synthetic JPX400 Ranks and Sample Construction

We obtain Worldscope data on annual fundamentals (including but not limited to all the underlying variables listed in Appendix A) and Datastream data on monthly prices, volume, outstanding shares, and returns, for a comprehensive list of Japanese securities in the period 1990–2017.[10] We omit observations that are missing returns, have an empty "data date" field for fundamentals data, or are duplicated on their Datastream identifier and relevant time indicator. We merge in executive compensation data from Toyo Kezai, an indicator for Nikkei225 membership constructed using historical updates on constituent firms archived on the Nikkei website, and an indicator for JPX400 membership constructed using historical updates on constituent firms archived on the Japan Exchange Group website.[11]

We then replicate the algorithm used by the Japan Exchange Group to construct the JPX400 index, and employ the resulting synthetic ranks to design our empirical analyses. As Appendix B shows, the JPX selects a new crop of JPX400 members each year, on the last day of June, using available price and volume data and financial-statement data released prior to April.[12] We use the same information used by the JPX to construct our synthetic ranking.

The JPX400 selection algorithm begins by filtering TSE-listed companies on several criteria. First, it excludes all companies that (1) have been listed on the TSE fewer than three consecutive

[10]Selection of firms for inclusion in the JPX400 was initiated in 2013; we collect data as far back as 1990 in order to perform placebo tests described in detail below.

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

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

years, (2) have had negative book value in *any* of the past three years, (3) have had negative operating income in *all* of the past three years, or (4) are in the process of being de-listed.[13] From this pool of eligible firms, the JPX then selects the top 1,200 stocks by "trading value" (price times volume, or the total value of transactions in the stock over the preceding year). Finally, the JPX winnows down these 1,200 stocks to the top 1,000 stocks by market capitalization.

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

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

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

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

3.2 Research Design

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

[13] We designate as TSE-listed all companies listed on the First, Second, and Mothers Sections of the Tokyo Stock Exchange and those listed on the JASDAQ.

base and, *ceteris paribus*, decreasing ROE. Thus we expect to observe the incentive effects of the JPX400 most cleanly in ROE.

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

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

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

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

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

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

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

3.3 Validation of Synthetic Ranks

Our research design relies on a synthetic replication of the JPX400 ranks, which is necessary because the JPX publishes the index's constituents but not the underlying rankings. Thus, before proceeding to our DID analysis, we begin by empirically validating our synthetic rankings and testing whether their variations are meaningfully associated with the probability of JPX400 inclusion (and thus with the true rankings). We estimate the following OLS regression:

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

where $JPX\ Member_{i,t}$ is an indicator for actual JPX400 membership, $Above\ Cutoff_{i,t}$ is an indicator that takes a value of 1 for all firm-year observations for which our synthetic JPX400 rank is is 400 or less and zero otherwise, and $X_{i,t}$ is a vector of contemporaneous firm-level controls. To ensure that our analysis of prediction accuracy includes all false negatives, the regression sample includes all firms with synthetic ranks of 1–2000 (based on the top 2,000 firms in terms of trading value) for the selection years 2013–2015.

Table 1, columns 1 and 2, report the results of this analysis. Column 1 shows that, unconditionally, a synthetic rank of 400 or less is associated with a 92% probability of index inclusion. Column 2 adds firm-level controls to assess the extent to which 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—*Log Book to Market*, *Sales Growth*, *LT Debt to Equity*, or *Cash to Equity*—does so. Because the JPX400 is weighted toward large firms, the positive association between firm size and inclusion likelihood is probably due to the fact that the small number of false-negative firms is on average much larger in size than the full sample of 2,000 firms.

Table 1, columns 3 and 4, report the results of similar analyses, but instead of *Above Cutoff* they use indicators of synthetic rank ranges—1–200, 201–400, 401–600, and over 800—as the main predictors of interest. These tests represent a more granular assessment of whether variation in the synthetic rank is meaningfully associated with the likelihood of JPX400 inclusion. Because the quintile indicators are exhaustive, these specifications are estimated without a separate intercept term.

The results in columns 3 and 4 show that the probability of JPX400 inclusion decreases with our synthetic ranks. By interpreting the coefficients in column 3, we find that, unconditionally, firms we ranked 1–200 have a 99.1% likelihood of inclusion, and firms we ranked 201–400 have an 87.9% likelihood of inclusion. Lower-ranked firms have low likelihoods of inclusion: firms we ranked 601–800 have a 0% likelihood of inclusion. Again, because our synthetic ranks produce a small number of false negatives, we find a statistically significant but economically negligible likelihood (0.5%) of inclusion for firms we ranked above 800. The results reported in column 4, which include firm fundamentals as additional controls, are virtually identical to those in column 3. As in column 2, the only firm characteristic significantly associated with the likelihood of inclusion is firm size, because false-negative firms are on average larger.

Collectively, these results show that our reconstructed synthetic JPX400 rankings possess a high degree of validity. That these ranks cannot be improved on by additional controls other than size suggests 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 of their differences. Definitions of covariates appear in Appendix A.

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

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

4 Empirical Analysis

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

4.1 The JPX400 Inclusion Threshold and Future ROE

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

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

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

Columns 2-5 present estimates from increasingly robust DID specifications relative to column 1. Column 2 replaces the *Post* indicator with time-fixed effects; column 3 adds industry-fixed effects; column 4 adds linear controls for contemporaneous firm attributes that can also potentially explain future ROE, specifically *Log Market Cap*, *Log Book to Market*, *Sales Growth*, *LT Debt to Equity*, and *Cash to Equity*; and column 5 includes contemporaneous ROE as an additional firm-level control. Most notably, the coefficients on *Treat*×*Post* remain similar in magnitude and statistical significance across columns 2-5; by contrast, the adjusted R^2 of the regression increases from 2.19% in column 2 to 30.31% in column 5, mitigating concerns about omitted variable biases (Oster, 2017). Interpreting the coefficient in column 5, the most robust and the main specification of interest, we report a DID coefficient of 2.4 basis points, or a 41% increase relative to the pre-period treatment-group mean.

4.2 Testing the Identifying Assumptions

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

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

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

Although we find evidence that the differences in ROE between those firms ranked 301-500 and 501-800 were stable in the years prior to the introduction of the JPX400, it is possible in theory that the counter-factual trends between the treated and control firms diverged *after* introduction of the index. For example, after 2013 some omitted variables (e.g., managerial incentives) could have changed in ways that correlated with the JPX400 ranks and could thus explain the relative increase in ROE we document in treated firms.

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

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

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

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

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

4.3 Consequences of Index Inclusion

We now turn to the consequences of inclusion in the JPX400. This analysis allows us to examine more precisely whether the main effects we have documented in ROE are indeed driven by index-inclusion incentives or are an outcome of index-inclusion itself. Moreover, understanding the consequences of index inclusion allows us to better understand *why* managers may have been motivated to pursue or maintain membership in the JPX400.

4.3.1 A Fuzzy Regression-Discontinuity Approach

To estimate the effects of JPX400 index inclusion *per se*, we rely on a fuzzy regression-discontinuity design (FRD), following Mullins (2014) and Chang, Hong, and Liskovich (2015). Regression discontinuity is natural in our setting, since index inclusion is a function of a running score—the JPX400 ranking. In this setting, however, the regression-discontinuity design is "fuzzy" due to our inability to observe firms' true JPX400 rankings and thus to our reliance on a synthetic replication of the selection algorithm. That is, firms' inclusion status is not a deterministic function of our synthetic JPX400 rankings, which has measurement errors (e.g., due to unobserved "qualitative adjustments"). Unlike sharp regression-discontinuity designs, FRD designs imply (and require) a jump in the likelihood of inclusion at the predicted threshold of inclusion (rank of 400) that is less than 1. Figure 2 clearly illustrates the presence of such a discontinuity at the predicted threshold, lending support to the use of FRD in our context.

Moreover, FRD is appropriate in our setting due to firms' inability to *precisely* control their JPX400 rank—not only because they cannot precisely control their size and, to a lesser extent, their profitability, but also because they cannot control the size and profitability of *other* firms. Thus, firms cannot precisely choose their index-inclusion status, leading to some degree of randomness in treatment assignment, and lending credence to the assumption that potential outcomes are continuous in the rank measure, at the threshold of inclusion (e.g., Lee, 2008).

To further validate the appropriateness of FRD, we examine pre-determined variables for discontinuity at the predicted inclusion threshold. The presence of a discontinuity in any variable observed prior to JPX400 re-constitution would suggest precise self-selection, and would challenge the assumption of continuity in the potential outcomes at the threshold of inclusion. In Figure 3, we examine and fail to find any graphical evidence of discontinuity at the predicted threshold of inclusion in the following pre-determined covariates: *ROE*, *Sales Growth*, *Log Illiquidity*, *Log Avg Exec Pay*, *Log BM*, *Log Market Cap*, *Leverage*, and *Cash to Equity.* Formal statistical tests, using linear fit for and a bandwidth of 200 firms on each side of the threshold, fail to reject the null of no differences in average values at the threshold of inclusion for each of the variables. Varying the bandwidth or the polynomial order does not affect these inferences. Collectively, our analyses suggest that FRD is the appropriate design for evaluating the effects of inclusion in the JPX400.

We implement the FRD following the standard procedure using two-stage least squares (2SLS). We follow previous methodological work on RDD (e.g., Imbens and Lemieux, 2007; Skovron and Titiunik, 2015) in the following estimation choices: (1) polynomial of order one on either side of the predicted-inclusion threshold, (2) an uniform kernel, and (3) the same bandwidth for both outcome and treatment regressions. As Hahn, Todd, and Van der Klaauw (2001) show, these choices result in the estimation of the following system of equations through 2SLS:

$$\begin{aligned} \textit{JPX Member}_{i,t} &= \delta_{0r} + \delta_{1r}\,\textit{Centered Rank}_{i,t} + \\ &\quad \textit{Above Cutoff}_{i,t}\left[\delta_{0l} + \delta_{1l}\,\textit{Centered Rank}_{i,t}\right] + u_{i,t} \qquad (4) \\ Y_{i,t+1} &= \beta_{0r} + \beta_{1r}\textit{Centered Rank}_{i,t} + \\ &\quad \textit{JPX Member}_{i,t}\left[\beta_{0l} + \beta_{1l}\textit{Centered Rank}_{i,t}\right] + \epsilon_{i,t+1} \qquad (5) \end{aligned}$$

where $\textit{JPX Member}_{i,t}$, the treatment variable, is an indicator denoting actual index inclusion; $\textit{Above Cutoff}_{i,t}$ is an indicator for having a synthetic rank of 1–400; $\textit{Centered Rank}_{i,t}$ is the synthetic rank minus 400; and $Y_{i,t+1}$ represents the outcome variable of interest. The subscripts l and r for the regression coefficients indicate that we allow the intercepts and the slope coefficients to differ for treatment and control groups, or in other words for observations that lie to the *left* and *right* of the cutoff respectively. To improve the precision of our estimates, we augment our regressions with a set of pre-determined firm-level controls likely to be associated with the outcome, the lagged dependent variable, and time-fixed effects (Lee and Lemieux, 2010). Following Calonico, Cattaneo, Farrell, and Titiunik (2016), we do not interact the additional covariates with the treatment indicator.

4.4 Fuzzy Regression-Discontinuity Results

Table 5 documents the results of our FRD regressions for a variety of forward (one-year-ahead) outcome variables: *ROE*, *Sales Growth*, *Log Executive Compensation*, *Log Illiquidity*, and *Log BM*. For these baseline specifications, we choose a bandwidth of 200 ranks on either side of the cutoff.[14]

Consistent with the visual evidence in Figure 2, we find a strong first-stage result in column 1:

[14] In untabulated tests, we document that our results are robust to narrower bandwidths, although the instruments weaken in strength.

the Kleibergen-Paap rk Wald F statistic (Kleibergen and Paap, 2006) of 36.1 comfortably clears the critical value (7.03 at 5% significance level for a maximal size distortion in finite samples of 10%) (Stock and Yogo, 2002).[15] In column 2, our second-stage regression finds no significant effect on *ROE* of index inclusion at the threshold. The regression results are corroborated by the first row of graphs in Figure 3, which shows no discernible discontinuity in ROE at the threshold of inclusion. Thus our main results are most likely due to managers' responses to *ex-ante* index-inclusion incentives, rather than an *ex-post* result of index inclusion (e.g., due to changes in monitoring or due to changes in compensation), consistent with the results in column 1 of Table 4.

Furthermore, in columns 3-6, we do not find any significant effects of inclusion in the JPX400 on firms' future sales growth, top-level executives' future average compensation, firms' future liquidity, or firms' future book-to-market ratio. The lack of significant capital-market, compensation, or product-market benefits of inclusion in the JPX400 are also evident in Figure 3: for all firms and for the subsamples of Nikkei225 firms (relatively larger and more liquid) and non-Nikkei225 firms (relatively smaller and less liquid), we do not observe discernible discontinuity in any of the outcome variables around the boundary of inclusion.[16]

That firms and managers do not appear to derive any measurable benefits on these dimensions from JPX400 index-inclusion *per se* may at first appear surprising—but these results can all be explained by institutional features and by recent work on the capital-market effects of indexes. Though the JPX400 was seen as highly prestigious by the Japanese business, finance, and political communities, it was less salient for ordinary Japanese consumers. Thus, it is not clear that mere inclusion in the index would generate significant product-market benefits (e.g., pricing-power and brand-value benefits) that could drive improvements in *Sales Growth* and *ROE*. That we do not find significant effects on *Executive Compensation* can be explained by the fact that CEO pay-to-performance sensitivity is very low in Japan, as had been documented in prior literature (Nakazato, Ramseyer, and Rasmusen, 2011; Kato and Kubo, 2006) and as we confirmed in untabulated tests.[17]

[15]The Stock-Yogo critical values are computed assuming homoskedastic standard errors; however, our standard errors are clustered. In the absence of a benchmark of critical values for the case of non-homoskedastic standard errors, we find reassurance from the large margin by which the Kleibergen-Paap rk Wald F statistic clears the Stock-Yogo critical values.

[16]These graphs are based on "residualized" outcome variables. In untabulated graphs, we also observe no discernible discontinuities when we plot the raw outcome variables as a function of *Centered Rank*.

[17]We found that top managers at our treatment firms did not earn significantly more than their counterparts at control firms. Moreover, 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

Thus, even if shareholders and directors were pushing managers to achieve index inclusion, we would not expect success to yield significantly higher managerial compensation.

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

In theory, inclusion in a stock index would improve liquidity and valuation multiples to the extent that it significantly changes the relative demand for a stock. However, inclusion in the JPX400 *per se* is unlikely to generate such a significant increase in relative demand. First, since 2013 there has been a large secular increase in total demand for Japanese equities, driven by two factors: (1) the Bank of Japan's (BOJ) quantitative-easing program, in which it significantly increased domestic equity holdings by purchasing index-linked ETFs (in quantities amounting by the end of 2016 to $16 trillion Yen); and (2) the GPIF's decision to increase the proportion of assets under management (more than $130 trillion Yen) allocated to equity investments, from a target of 12% to 25% (Barbon and Gianinazzi, 2017). In both cases, the increase in total demand benefits all firms belonging to the TOPIX index, which remains the most frequently tracked by institutional investors among indexes in Japan, and especially firms belonging to the Nikkei225, the index that the BOJ's program heavily targeted. Although the BOJ and the GPIF both allocated some amount of their capital to the JPX400, they are relatively insignificant. For example, only 4% of the BOJ's purchases flowed to ETFs tracking the JPX400; 53% flowed to ETFs tracking the Nikkei225 and 43% flowed to ETFs tracking the TOPIX (Barbon and Gianinazzi, 2017). Similarly, the GPIF only benchmarked approximately 6% of its domestic equity portfolio to the JPX400 (vs. 74% benchmarked to the TOPIX).

These institutional factors imply insignificant changes in relative demand for JPX400 stocks, in light of the significant rise in total demand for Japanese stocks in the TOPIX (which includes all of the JPX400 firms) and the Nikkei225. In fact, our computations suggest that the BOJ's

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 estimated a pay-to-stock-return-performance sensitivity of 20%, about 4.5 times as large as in Japan. Finally, we did not find evidence that this sensitivity changed after introduction of the JPX400, for either the treatment or control group of firms.

quantitative easing and the GPIF's increase in allocation to equity resulted in more than a doubling of demand for stocks in the TOPIX, whereas inclusion in the JPX400 *per se* results in an incremental increase of 10% in demand. Also, recent work on ETFs challenges the assertion that inclusion in an index leads to capital-market benefits. For example, this research has found that increased ownership due to ETFs reduces liquidity (Hamm, 2014), increases non-fundamental volatility (Ben-David, Franzoni, and Moussawi, 2018), reduces information-production incentives (Israeli, Lee, and Sridharan, 2015), and creates excess co-movement in stock returns (Da and Shive, 2018). Jointly, these factors could explain why we do not observe empirical evidence of significant capital-market benefits from inclusion in the JPX400 *per se*.

Overall, our findings do not support the premise that managers and firms realized significant financial or capital-market benefits from index inclusion *ex post*. It remains possible, however, that firms coveted membership in the index because they *expected* to realize benefits, such as greater liquidity or lower cost of capital *ex ante* (both realistic expectations, given that the GPIF promised to track the index and that other institutional investors could be expected to follow suit).

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

Table 5 fails to document any significant capital- or product-market benefits to the firm, or compensation benefits to managers, of inclusion in the JPX400 *per se*. This finding naturally raises the question: why did managers and firms exert effort to be included? One possibility is that the firms *expected* these financial benefits, *ex ante*, and worked to attain index membership on that basis. Another possibility—one consistent with our interviews with investors and with the JPX400's nickname—is that managers were motivated by status concerns: the prestige of inclusion and fear of the shame of exclusion.

We address these alternative explanations by examining cross-sectional variation in the treatment response between Nikkei225 and non-Nikkei225 firms. Nikkei225 firms would have been relatively more attuned to status concerns, and relatively less driven by expected capital-market benefits; thus a difference in the treatment response between the two groups could provide evidence on whether status incentives played a role.

The Nikkei225 has long been Japan's leading stock index, closely tracked by institutional investors; it consists of Japan's largest, best-established, best-known, and most liquid firms in 36 industries. Since Nikkei225 firms already enjoyed the highest liquidity in the Japanese market, the *incremental* capital-market benefits of inclusion in the JPX400 would presumably be more salient for, and appear greater to, non-Nikkei225 firms. But Nikkei225 firms would probably be more sensitive to status incentives, and particularly to fear of loss of status. Because endorsement by the GPIF (and subsequent media attention to "the shame index") elevated the status of the JPX400 as "the" prestige index and the gauge of best-run firms in Japan, thereby disrupting the status hierarchy among Japanese stock indexes, Nikkei225 firms would presumably fear loss of status if they were excluded. 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.[18]

Table 6 reports the results of our empirical tests of this hypothesis, using two different specifications and our two treatment-intensity measures (the binary indicator and the *Quintile(Closeness)* measure. The first pair of columns (1 and 2) employs our main DID specification, but subdivides our treatment-group indicator into *Nikkei225* and *non-Nikkei225* subgroups. More formally, we estimate the equation:

$$\begin{aligned} ROE_{i,t+1} &= \alpha + \beta_1 Treat_{i,t} \times Nikkei225_{i,t} \times Post_t \\ &\quad + \beta_2 Treat_{i,t} \times non\text{-}Nikkei225_{i,t} \times Post_t + \beta_3 Treat_{i,t} \times Nikkei225_{i,t} \\ &\quad + \beta_4 Treat_{i,t} \times non\text{-}Nikkei225_{i,t} + \gamma X_{i,t} + f_t + \epsilon_{i,t}. \end{aligned} \quad (6)$$

This specification allows for treatment-effect differences but assumes no Nikkei225- and non-Nikkei225-specific effects; that is, it does not allow for the control firms to differ in ROE based on Nikkei225 status. In this regression, we are interested in the sign and significance of the two triple-interaction coefficients and the test for their equality. Under the status-incentive hypothesis, β_1 should be larger in magnitude than, and statistically different from, β_2.

The next pair of columns (3 and 4) employs a difference-in-difference-in-differences (DDD) de-

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

sign, once again implemented using both our binary treatment indicator and the *Quintile(Closeness)* measure. Specifically, we estimate the following equation:

$$\begin{aligned} ROE_{i,t+1} &= \alpha + \beta_1 Treat_{i,t} \times Nikkei225_{i,t} \times Post_t + \beta_2 Nikkei225_{i,t} \times Post_t \\ &+ \beta_3 Treat_{i,t} \times Post_t + \beta_4 Nikkei225_{i,t} + \beta_5 Treat_{i,t} + \gamma X_{i,t} + f_t + \epsilon_{i,t}. \end{aligned} \tag{7}$$

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

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

Columns 1 and 2 of Table 6 report the results of estimating equation 6 using our binary treatment measure and the *Quintile(Closeness)* treatment measure respectively. In column 1, the estimated coefficient of 0.066 on *Treat*×*Nikkei225*×*Post* is six times the estimated coefficient of 0.011 on *Treat*×*non-Nikkei225*×*Post* (a difference that is statistically significant at the 5% level), consistent with prestige incentives being the primary driver of the overall ROE effects. Similarly, in column 2, the estimated treatment response of Nikkei225 firms is larger in magnitude—over three times as large—and statistically different from that of non-Nikkei225 firms at the 5% level.

Columns 3 and 4 report coefficient estimates from equation 7. In column 3, using our binary treatment measure, we report a coefficient of 0.050 on the triple interaction, *Treat*×*Nikkei225*×*Post*; the DDD estimate is both economically and statistically significant. The point estimate indicates that the treatment response for the Nikkei225 firms is more than 5 percentage points higher than that of the non-Nikkei225 firms; moreover, these results suggest that the DID estimate for Nikkei225 firms is statistically different from the DID estimate for non-Nikkei225 firms at the 5% level. Column 4 repeats this exercise using the *Quintile(Closeness)* treatment measure. We find a positive and significant (at the 10% level) coefficient on the triple interaction, *Quin-*

tile(Closeness)×*Nikkei225*×*Post*, further evidence of a stronger treatment response for Nikkei225 firms. Collectively, these results provide support for status incentives as an important factor in explaining the overall ROE effect.

4.6 Drivers of the ROE Response

Next we study which levers managers pull to generate the improvement in ROE that we document. Table 7 reports the results of regressions examining the behavior of the major drivers of ROE: return on assets—which is driven by sales growth, profit margin, and asset utilization—and financial leverage. In Panel A, columns 1-4, we replicate our main baseline specification (Table 3), column 5, but use forward *Sales Growth*, *ROA*, *Profit Margin*, *Asset Turnover*, and *Leverage* respectively as dependent variables. In Panel B, to examine whether firms' responses vary with the areas in which they have the most "slack," we run each specification of Panel A for the subset of companies with above-median and below-median contemporaneous values in the ROE driver of interest.

The results in Table 7 suggest that improvement in ROE is predominantly driven by improvements in operational efficiency (as measured by ROA) rather than changes in capital structure. Improvements in ROA, in turn, appear to be on average driven by increased margins rather than asset turnover or sales growth. Panel A, column 1, shows no significant treatment effect (at the 10% level) on *Sales Growth*. Panel B, columns 1 and 2, show no statistically significant treatment effect on *Sales Growth* within the subset of companies with below-median sales growth or the subset with above-median sales growth, suggesting that top-line growth was not the driver of the documented ROE effect.

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%. Moreover, 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%. Panel B, columns 3 and 4, document that the increase in net margins is driven by the subsample of treatment firms with below-median margins: the coefficient on $Treat \times Post$ is significant only for the below-median profit-margin sample, and the point estimate of the treatment effect is 0.0158, more than twice the treatment effect for the full sample.

In contrast, our analysis of *Asset Turnover* suggests that improvement in asset efficiency is not the dominant channel for improvement in ROA. The treatment effect for the full sample, reported in Panel A, column 4, is not statistically significant at the 10% level. However, the subsample analysis in Panel B, columns 5 and 6, reveals a statistically significant improvement in asset utilization among those firms with below-median asset turnover. Nevertheless, this effect, at around 1.78% of the treatment group's average pre-period turnover, is economically much smaller than the treatment effect in profit margin.

Finally, we find no evidence that financial leverage was significantly impacted by firms' JPX400-inclusion incentives, either for the overall sample (Panel A, column 5) or for the subsamples of above-median and below-median-leverage firms (Panel B, columns 7 and 8). These findings are not entirely surprising, since JPX400's ranking algorithm does not necessarily incentivize firms to increase leverage *per se*. Under Modigliani and Miller (1958) assumptions, a profitable firm making a simple *ceteris paribus* increase in financial leverage—such as by issuing debt and repurchasing shares—would increase the company's ROE but might decrease its total market capitalization, a component of the selection algorithm.

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

In Table 8, column 1, we find a statistically significant effect (at the 5% level) on shareholder payouts overall. Columns 2 and 3 show that that this effect is driven by the subsample of firms with more excess cash—that is, those with above-median cash-to-equity ratios. This overall effect size—

0.59 percentage points—is economically large, representing approximately a 21% increase in the payout ratio relative to the pre-period treatment-group mean. Relative to total cash and investment on treatment firms' balance sheets, however, the magnitude of the payouts is quite small: our estimates suggest that shareholder payouts represent 1.5% of the treatment-group pre-period cash-to-equity ratio. This finding might explain why an effect driven through the shareholder-payouts channel is statistically difficult to detect by examining financial leverage. Another possibility is that, all else equal, the JPX400's positive effects on earnings may have a countervailing and positive effect on total cash, lowering financial leverage.

Jointly, these findings provide further support for the thesis that firms improved ROE in response to index-inclusion incentives, and shed light on the channels through which managers sought ROE improvements. The analyses in Tables 7 and 8 show that the overall effects on ROE are driven in part by operating changes—that is, by improving ROA—and in part by increasing shareholder payouts. Furthermore, they suggest that firms improved ROE where there was most slack.

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

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

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

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

An alternative may be for managers to change their real investments, such as by shedding assets or cutting R&D. In order to examine the index-inclusion effects on real investments in net operating assets, column 2 reports the results of the specification with *Log NOA* as the dependent variable. Once again, we find no statistically significant treatment effect on *Log NOA*, suggesting that the JPX400 did not drive a statistically measurable change in investments in net operating assets. Column 3 examines how firms altered R&D intensity (R&D-expense-to-sales ratio) in response to JPX400-inclusion incentives. R&D is often seen as having positive externalities and long-term benefits that are not captured by the accounting system, but managers might cut R&D—as a discretionary expense item—in order to boost reported earnings (e.g., Roychowdhury, 2006). R&D expenditures could thus potentially entail a tradeoff between ROE and the macroeconomic and social-welfare goals of Japanese policy makers. The point estimate on $Treat \times Post$ in column 3 of -.0011 is significant at the 10% level and indicates a small 7.05% reduction in R&D intensity relative to the treatment group's pre-period mean of 1.56%. This effect also explains about 15% of the average treatment effect on profit margin improvement. To the extent that this effect is suboptimal, rather than efficiency-improving, cutting discretionary expenses like R&D does not appear to be the main driver of companies' efforts to improve margins and ROE.

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

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

4.8 Market Valuation

Our final analysis focuses on market-valuation outcomes associated with JPX400-inclusion incentives. Table 10 reports the results of our main DID specification using *Log Book to Market* as the outcome variable of interest. Column 1 reports the results of a reduced-form OLS DID with firm-level controls and time-fixed. The negative, statistically significant coefficient on $Treat \times Post$ in this column indicates that treatment firms experienced a relative improvement in their valuation multiples. The point estimate of -.038 represents a 3.7% decrease in book-to-market relative to the pre-period treatment-group mean of 1.018. In conjunction with our FRD analysis in Table 5, our findings suggest 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, and that this revision was not compensated for by a commensurate increase in expectations of firm risk.

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

5 The JPX400's Overall Effect

The JPX400 has generated significant interest. In Japan and globally, there is a lively discussion among regulators, policy makers, and investors about its efficacy. We conclude our analysis by assessing the overall impact of the index on the Japanese economy and equity market.

In our view, comparing the realized returns of the JPX400 to those of other Japanese indices, as some policy makers and commentators do, does not capture the success of the index or its incentive effects on corporate performance and value. That metric has two conceptual flaws. First, as our analyses show, part of the effect of the index resides in its performance effects on excluded firms that improve their performance to earn membership. Second, to the extent that valuation benefits accrue to such firms prior to their inclusion in the JPX400—when market participants either respond to or anticipate performance improvements in response to JPX400-inclusion incentives—they would not be captured in the returns of the JPX400 index. Instead, a relative-return comparison would attribute the beneficial effect of the JPX400 to the benchmark index. These two factors could explain why the JPX400 *under*performed the Nikkei225 over the three-year period from June 2014 to June 2017, and why some stocks outperform after being excluded from the JPX400.

Our research design avoids both conceptual issues by leveraging differences in the intensity of JPX400-inclusion incentives and by focusing on a fundamental measure of performance (ROE). Using that approach, we find that the JPX400 index has been substantially more successful than its portfolio returns would suggest.

To provide a conservative estimate of the JPX400's overall impact on the aggregate Japanese economy, we focus on the incremental income generated by the firms that we classify as treated due to their index-inclusion incentives. In untabulated results, we estimate a simple DID in forward net income using our baseline treatment and control groups, and find an on-average annual firm-level improvement in net income of JPY6.0 billion. Aggregating across the 200 firms in the treatment group yields a JPX400 effect on aggregate net income of JPY1.2 trillion per year. This represents an 8.9% increase relative to pre-period average aggregate income across all public Japanese firms (JPY13.6 trillion per year). Moreover, relative to the change in average aggregate net income from the pre-period to the post-period (from JPY13.6 trillion to JPY21 trillion per year), the effects attributable to the JPX400 account for 16%.[19]

To estimate the total wealth or valuation effects, we multiply the JPX400's net-income effect by a range of plausible incremental price-to-earnings (P/E) multiples. A very conservative and lower-bound incremental P/E ratio on the new profits generated by the JPX400 would be 1: this would be the appropriate incremental P/E on the new profits if the markets perceived the earnings boost as completely unsustainable and not likely to generate any internal alpha if reinvested. Under this assumption, the wealth effect would be JPY1.2 trillion per year, for a total of JPY3.6 trillion, or a 0.77% increase relative to the 2014 total Japanese market capitalization of JPY469 trillion. A less conservative incremental P/E ratio would be the treatment firms' cash-adjusted P/E ratio: $(MCap - Cash)/NetIncome$.[20] The average cash-adjusted P/E multiple for treated firms in the post period is 17.07, implying a valuation impact of JPY61.45 trillion, or approximately 13.10% of total market capitalization in June 2014. Taking the midpoint of these P/E multiple estimates, our back-of-the-envelope estimation suggests that JPX400 improved the overall Japanese market's market capitalization by roughly 6.9%. Compared to the growth in market capitalization (measured as of end-June) between 2014 and 2017 of JPY163 trillion, these estimates suggest that the JPX400 explains approximately 20% of overall market-capitalization growth since introduction of the index. These estimates may also be conservative to the extent that firms further from the threshold are still affected by JPX400-inclusion incentives, though to a lesser degree than firms nearer the threshold of inclusion. We emphasize that these back-of-the-envelope estimates are meant to characterize the approximate magnitude of the JPX400's effect on Japanese corporate profitability and market capitalization. At face value, these estimates suggest that the creation of the JPX400 and the incentives it presented to Japanese corporate managers had a meaningful impact on aggregate corporate profitability and valuation.

[19] We use the Datastream universe of public Japanese firms in computing aggregate earnings and market capitalization.

[20] This multiple is justified under the assumptions that (a) the market values Japanese corporations' cash holdings 1-for-1, such that the cash-adjusted P/E ratio captures the market's valuation of the companies' earnings; and (b) because the market perceives the income effect as sustainable, expected growth of and discount rates on the incremental profitability remain fixed.

6 Conclusion

Economists, sociologists, and psychologists have long recognized the role of non-pecuniary incentives in human motivation. Goode (1978) suggests that status incentives have been among the central mechanisms motivating human action throughout human history.[21] The analyses presented in this paper suggest that status-based mechanisms can also be powerful in the realm of corporate governance, the study of which has primarily focused on formal or pecuniary incentives and regulation. In particular, our study highlights a novel mechanism for changing corporate behavior sustained by persistent corporate norms.

Our evidence is consistent with the theory that, in a setting where formal incentives are limited in their ability to influence persistent behavior, changing the status hierarchy of desired behaviors could help induce desired changes. By leveraging the certification provided by the GPIF and reinforced by the media, the JPX400 represented a virtuous club of Japan's best-run companies that could have motivated managers to change their behavior to attain membership. Moreover, our findings could be consistent with Goode (1978), which observes that changes in social norms tend to be precipitated by a shift in the behavior of a small group of respected elites. By incentivizing some of the most well-established and respected firms in the Japanese market (e.g., in the Nikkei225) to change their behavior, the JPX400 index mechanism could serve to coordinate a broader shift in corporate norms.

Although the long-run effects of the JPX400 on Japanese corporate norms remains to be seen, at a minimum our findings can inform corporate-governance reform efforts in capital markets with similar patterns of low capital efficiency and weak de-facto shareholder rights, such as those of other East Asian economies like China, Korea, Singapore, and Taiwan. We also believe that these findings highlight the potential of non-pecuniary incentives in corporate governance in developed markets. Given growing calls in the United States and Europe to limit executive compensation, understanding alternative mechanisms to elicit meaningful changes in corporate behavior is useful in the evolving corporate-governance arena.

[21] Examples include the Homeric Greeks and the Vikings, who assigned honor to those with exceptional courage and skill on the battlefield; Britain's Royal Society of Arts, which used honorary awards to encourage innovation (Khan, 2017); developing economies that encouraged entrepreneurship by assigning prestige to entrepreneurial activities (Steinhoff, 1980); and philanthropic organizations that create salient donor categories to encourage more donations (Harbaugh, 1998a,b).

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

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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.

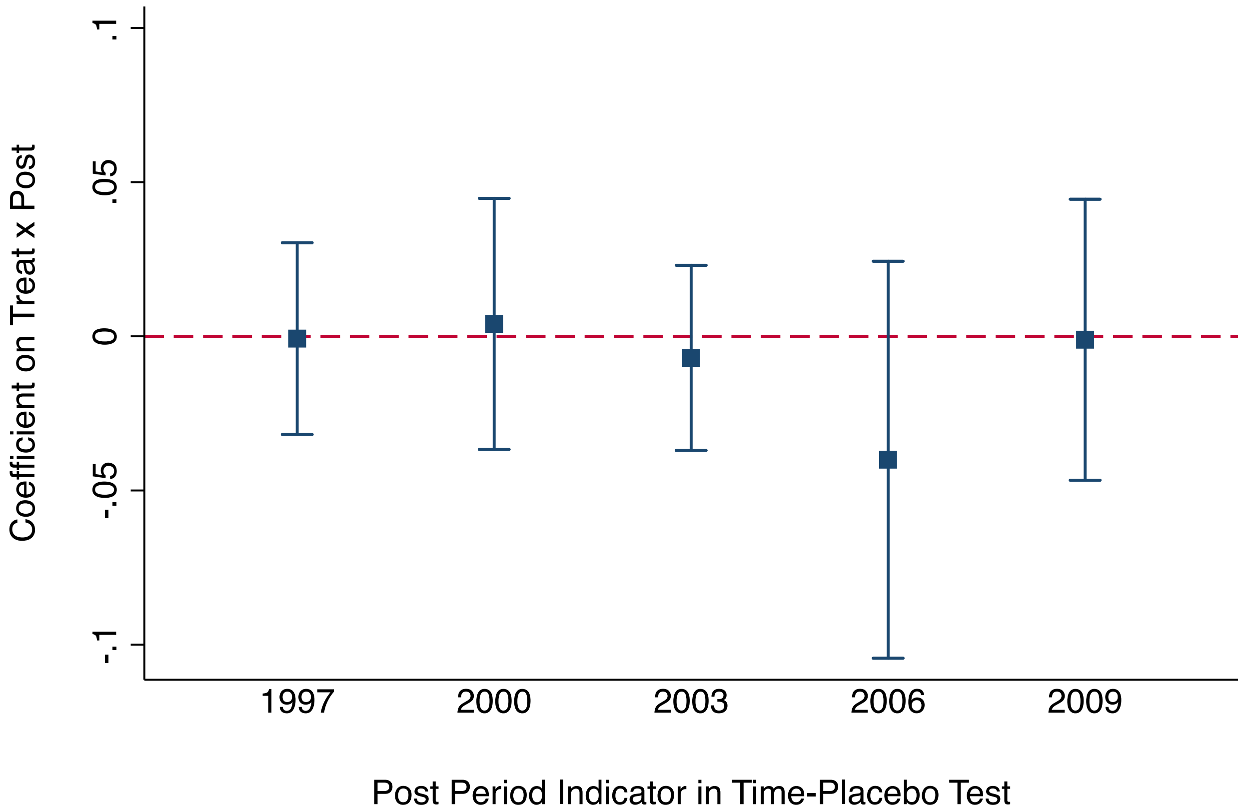


Fig. 2. Examining Discontinuity in Inclusion Likelihood around the Predicted Index-Inclusion Cutoff

This figure is a graphical representation of the first stage of the Fuzzy RD two-stage least squares (2SLS) estimation in Table 5. The x-axis represents the synthetically constructed JPX400 ranks, centered at the index-inclusion threshold of 400, and spanning a bandwidth of 400 ranks on either side of this threshold. Firms to the left of the threshold are predicted to be included in the JPX400; firms to the right are predicted to be excluded. The y-axis represents the likelihood of inclusion. The data points in each scatter plot represent average inclusion status for the firm-year observations in each of 160 non-overlapping bins (80 on each side of the threshold) created based on the JPX400 rank variable. The lines in each plot represent third-order polynomial fits of inclusion status as a function of Centered Rank. The upper graph is generated using the raw data. The lower graph represents a closer correspondence to the regression specification used in Table 5, and is generated using the residual from a regression of inclusion status on firm-level covariates and year-fixed effects used in the first-stage regression reported in Table 5.

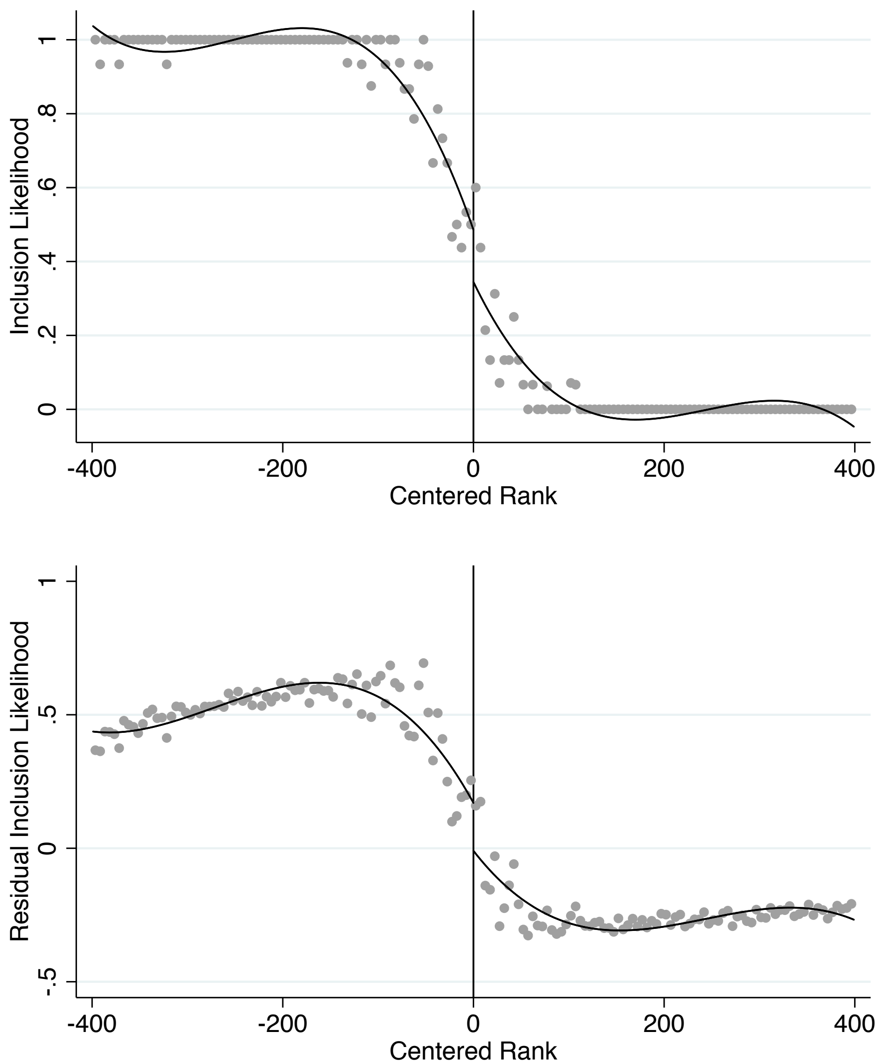


Fig. 3. Examining Discontinuity in Pre-Determined Variables around the Predicted Index-Inclusion Cutoff

This figure examines discontinuities in various pre-determined variables at the predicted index-inclusion cutoff. The y-axis of each scatter plot varies based on the variable of interest. These include the contemporaneous versions of the outcome variables examined in Table 5 and in Figure 3—*ROE*, *Sales Growth*, *Log Illiquidity*, *Log Avg Exec Pay*, and *Log BM*. We also examine discontinuities in other covariates used in our FRD estimation in Table 5—*Log Market Cap*, *Leverage*, and *Cash to Equity*. The x-axis represents the synthetically constructed JPX400 ranks, centered at the index-inclusion threshold of 400 and spanning a bandwidth of 400 ranks on either side of this threshold. Firms to the left of the threshold are predicted to be included in the JPX400; firms to the right are predicted to be excluded. The data points in each scatter plot represent average values of the variable of interest for the firm-year observations in each of 160 non-overlapping bins (80 on each side of the threshold) created based on the JPX400 rank variable. The lines in each plot represent third-order polynomial fits of the variables as a function of Centered Rank.

Figure 3. Continued

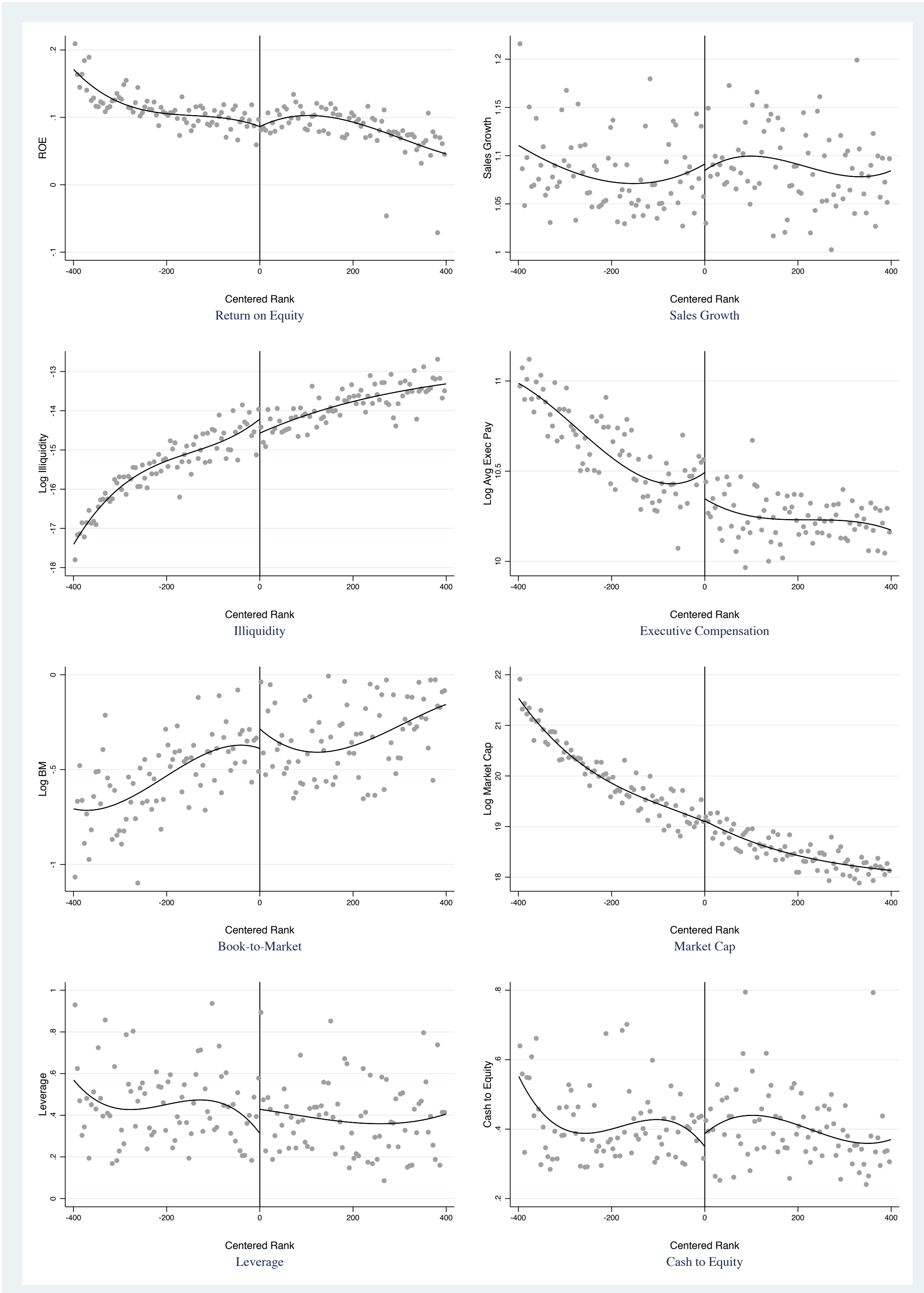
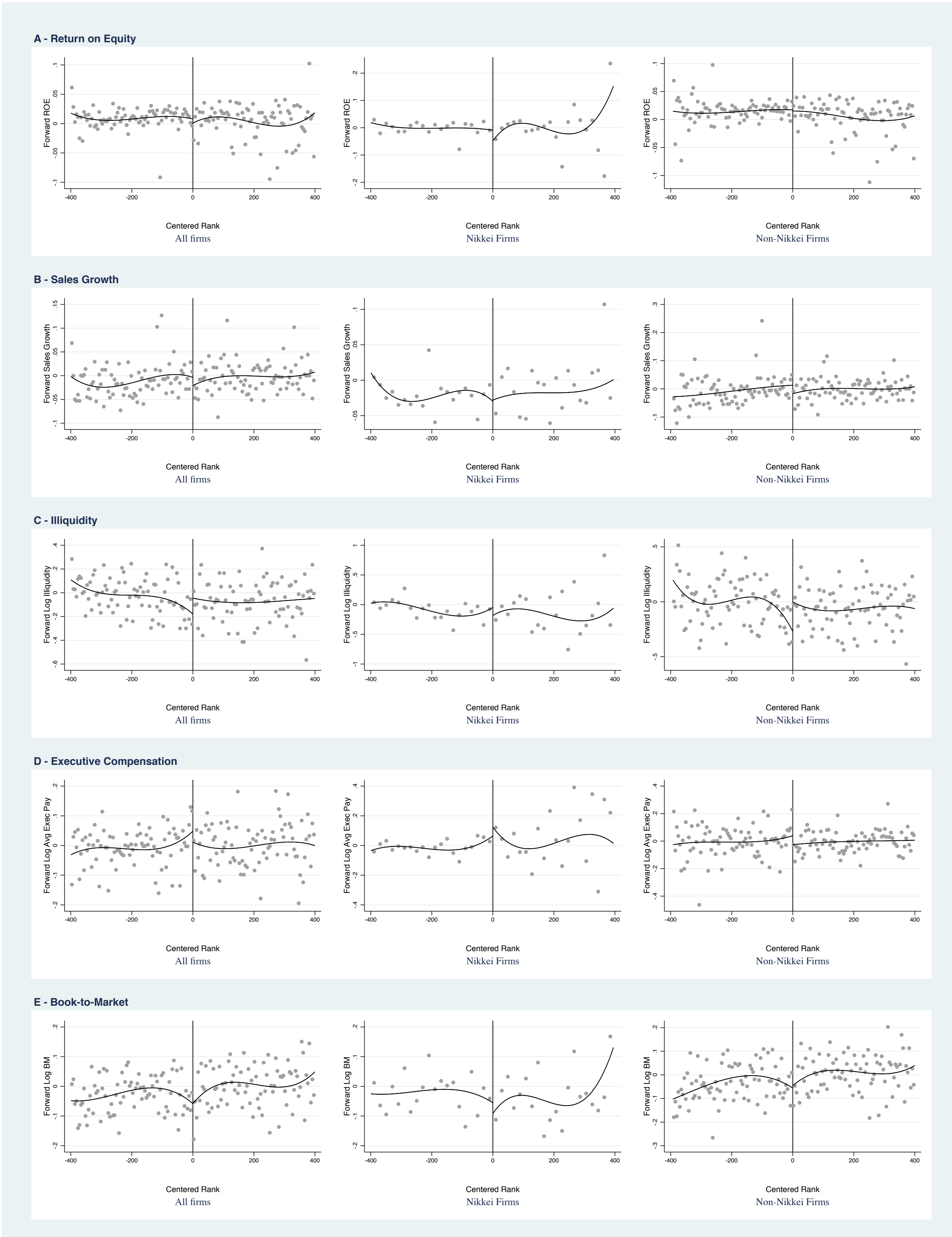


Fig. 4. Examining Discontinuity in Future Outcomes around the Predicted Index-Inclusion Cutoff

This figure is a graphical version of our main RD analysis in Table 5. It examines discontinuities in various outcome variables at the predicted index-inclusion cutoff. The x-axis represents the synthetically constructed JPX400 ranks, centered at the index-inclusion threshold of 400 and spanning a bandwidth of 400 ranks on either side of this threshold. Firms to the left of the threshold are predicted to be included in the JPX400; firms to the right are predicted to be excluded. The y-axis varies by row, and represents the one-year-ahead outcome variables examined in Table 5: *Forward ROE*, *Forward Sales Growth*, *Forward Log Avg Exec Pay*, *Forward Log Illiquidity*, and *Forward Log BM*. Specifically, we graph the residuals from a regression of these outcome variables on the set of firm controls used in Table 5, including time-fixed effects. The graphs in the first column are plotted using data from all firms; in the second and third columns, we partition the data into firms included in the Nikkei225 index and those that are not included respectively. The data points in each scatter plot represent average values of the variable of interest for the firm-year observations in non-overlapping bins created using the JPX400 rank variable. With the exception of Nikkei225 plots, we use 80 bins on each side of the threshold. Given the relative sparsity of Nikkei225 firms, the Nikkei225 plots use 20 bins on each side of the threshold. The lines in each plot represent third-order polynomial fits of the outcome variables as a function of Centered Rank.

Figure 4. Continued



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 Illiquidity	Natural logarithm of the Amihud (2002) illiquidity measure	ln(Daily Average of Absolute Return [RETURN] / Volume [VO])
Log Market Cap	Natural logarithm of market capitalization	ln(Market Value [MV])

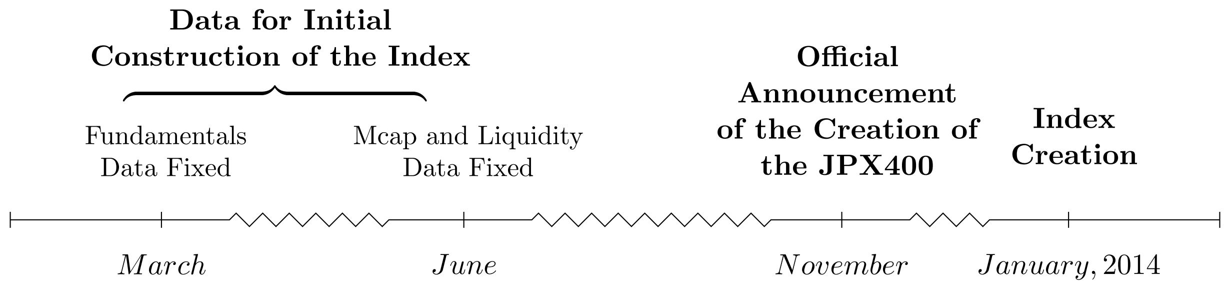
Appendix A Continued

Variable	Description	Computation
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
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]

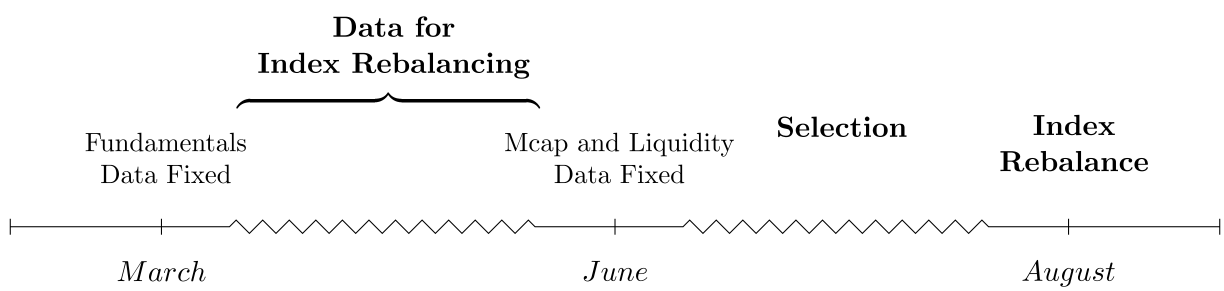
Appendix B Timeline, Selection of JPX400 Index Constituents

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

Panel A: 2013



Panel B: 2014+



Appendix C Sample Construction

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

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

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

Step 3: Construct baseline treatment and control sample.

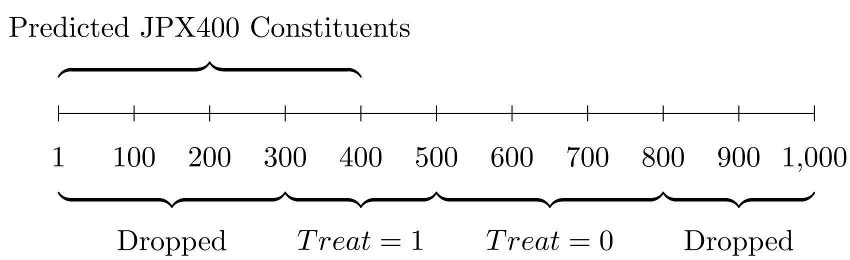


Table 1.
Predicting JPX400 Membership

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

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

Table 2.

Summary Statistics

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

	Full Pre-Period Sample							
	p25	*Mean*	*p50*	*p75*	*SD*	*Mean* $Treat = 1$	*Mean* $Treat = 0$	*T-Stat of Difference*
ROE	0.0300	0.0541	0.0500	0.0821	0.1007	0.0585	0.0512	1.3145
ROA	0.0086	0.0284	0.0238	0.0427	0.0382	0.0311	0.0267	2.1998
Profit Margin	0.0136	0.0469	0.0301	0.0605	0.0855	0.0485	0.0459	0.5784
Asset Turnover	0.6064	0.9565	0.8990	1.2423	0.6316	1.0121	0.9197	2.7198
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. 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*	0.028***	0.028***	0.025***	0.025***	0.024***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Treat	-0.006	-0.006	-0.005	0.007	-0.005
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Post	0.018***				
	(0.01)				
ROE					0.384**
					(0.15)
Log Market Cap				-0.026***	-0.013**
				(0.00)	(0.01)
Log Book to Market				-0.069***	-0.045***
				(0.01)	(0.01)
Sales Growth				0.038**	-0.011
				(0.02)	(0.02)
LT Debt to Equity				-0.013*	-0.007
				(0.01)	(0.00)
Cash to Equity				-0.016	-0.001
				(0.01)	(0.01)
Time FE	No	Yes	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes	No
Observations	2,783	2,783	2,783	2,783	2,783
R^2	0.0221	0.0219	0.0514	0.2472	0.3031

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: Fuzzy Regression Discontinuity

Table 5 reports the results of fuzzy regression-discontinuity designs estimated using two-stage least squares (2SLS) and a bandwidth of 200 firms around the predicted rank of 400. The 2SLS estimation uses *Above Cutoff*, an indicator variable that evaluates to 1 for firms ranked 400 and above and to 0 for firms ranked below, as an instrument for actual JPX400 inclusion. Column 1 reports the results of the first-stage regression for explaining actual JPX400 membership; the Kleibergen-Paap rk Wald F-Statistic (testing the null of weak instruments) is reported in the last row. Column 2 reports the result of the second-stage regression, with *Forward ROE* as the dependent variable of interest. Columns 3, 4, 5, and 6 also report the second-stage results, using *Forward Sales Growth*, *Forward Log Executive Compensation*, *Forward Log Illiquidity*, and *Forward Log BM*, respectively, as the dependent variables of interest. Time-fixed effects and linear firm-level controls are included throughout. Since our dependent variables of interest are forward variables measured at an annual horizon, we use selection years 2014, 2015, and 2016. We choose a bandwidth of 200 firms, on each side of the cutoff, and lack required data for 52 firms per year, resulting in 1044 observations for each specification. 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) *JPX Member*	(2) *Forward ROE*	(3) *Forward Sales Growth*	(4) *Forward Log Exec Comp*	(5) *Forward Log Illiquidity*	(6) *Forward Log BM*
Above Cutoff	0.432***					
	(0.05)					
Centered Rank x *Above Cutoff*	-0.001**					
	(0.00)					
JPX Member		0.016	0.019	0.028	-0.124	-0.023
		(0.02)	(0.03)	(0.07)	(0.17)	(0.06)
Centered Rank x *JPX Member*		0.000	0.000	0.000	-0.000	-0.000
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Centered Rank	-0.002***	0.000	-0.000	-0.000	-0.001	0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,044	1,044	1,044	1,044	1,044	1,044
R^2	0.7248	0.3871	0.2263	0.7682	0.7714	0.8832
Kleibergen-Paap rk Wald F-stat	36.105					

Table 6.

Treatment Effects in ROE for Nikkei vs. Non-Nikkei

Table 6 examines the differential ROE responses to JPX400-inclusion incentives of Nikkei225 firms and non-Nikkei225 firms. Columns 1 and 2 report the OLS results from an estimation of equation 6; columns 3 and 4 report the results of estimating equation 7. All estimations use the full sample of firms ranked 1–1,000. *Nikkei225* indicates contemporaneous membership in the Nikkei 225 index; *Treat* is an indicator variable assuming a value of one for firms ranked 301–500 under our replication of the JPX400 selection algorithm and zero for all other firms ranked 1–1,000; *Quintile(Closeness)* is a quintile ranking, ranging from the values of 0 to 4, of a firm's distance from the rank of 400, where the highest quintile reflects those firms whose ranks are closest to 400 and the lowest quintile reflects those furthest from 400; and *Post* is an indicator variable assuming a value of one for the years 2014–2016 and zero for the pre-treatment period, 2010–2012. In columns 1 and 2, the last two rows report the F-statistic and the corresponding p-value from a Wald test of equality between the two triple-interaction coefficients. In columns 1–4, we include time-fixed and firm-level controls as in Table 3, column 5, but their coefficient estimates are unreported. Definitions for all firm-level control variables appear in Appendix A. Standard errors, clustered at the firm level, are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

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

Table 7.
Drivers of ROE

Table 7 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*, and *Leverage*. 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. *Panel A* reports results estimated on the full sample; *Panel B* reports results estimated on subsamples. 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.

Panel A: Full Sample Analysis

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

Table 7. Continued

Panel B: Subsample Analysis

	Sales Growth		Forward Profit Margin		Forward Asset Turnover		Forward Net Debt Leverage	
	(1) Below-Median Sales Growth	(2) Above-Median Sales Growth	(3) Below-Median Profit Margin	(4) Above-Median Profit Margin	(5) Below-Median Asset Turn	(6) Above-Median Asset Turn	(7) Below-Median Leverage	(8) Above-Median Leverage
Treat x *Post*	0.0114	0.0027	0.0158***	0.0010	0.0180***	-0.0104	0.0254	-0.0213
	(0.013)	(0.014)	(0.006)	(0.005)	(0.007)	(0.015)	(0.020)	(0.026)
Treat	-0.0363***	-0.0116	-0.0090*	-0.0104**	-0.0249***	-0.0078	-0.0142	0.0242
	(0.013)	(0.012)	(0.005)	(0.004)	(0.005)	(0.010)	(0.014)	(0.021)
Sales Growth	-0.1887**	0.0610	-0.0067	-0.0344**	-0.0239	0.0083	0.0733**	0.0667
	(0.080)	(0.049)	(0.013)	(0.015)	(0.024)	(0.049)	(0.037)	(0.061)
ROA								
Profit Margin			0.0910	0.9318***				
			(0.112)	(0.025)				
Asset Turnover					0.9919***	0.9442***		
					(0.006)	(0.011)		
LT Debt to Equity	-0.0016	0.0128**	-0.0051**	-0.0052	0.0012	0.0213	0.6923***	0.9515***
	(0.007)	(0.006)	(0.002)	(0.006)	(0.002)	(0.014)	(0.105)	(0.029)
Cash to Equity	-0.0268*	-0.0253*	0.0060	-0.0082*	-0.0003	-0.0291	-0.7505***	-0.8910***
	(0.016)	(0.013)	(0.006)	(0.005)	(0.004)	(0.022)	(0.049)	(0.061)
Log Market Cap	-0.0006	0.0001	-0.0038**	0.0096***	0.0069***	0.0044	0.0063	0.0234**
	(0.005)	(0.004)	(0.002)	(0.002)	(0.002)	(0.005)	(0.007)	(0.011)
Log Book to Market	-0.0863***	-0.0814***	-0.0148***	-0.0113***	-0.0192***	0.0062	0.0232**	0.0518**
	(0.014)	(0.008)	(0.004)	(0.004)	(0.004)	(0.011)	(0.011)	(0.024)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,300	1,485	1,255	1,530	1,355	1,429	1,281	1,502
R^2	0.1762	0.1421	0.0700	0.7175	0.9320	0.9036	0.7293	0.8546

Table 8.
Shareholder Payouts

Table 8 reports the results of DID regressions following the specification in Table 3, column 5, but using one-year-ahead *Shareholder Payouts* as the dependent variable. 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. Column 1 reports results for the regression estimated on the full sample; columns 2 and 3 report results estimated on the subsample of firms with above-median and below-median contemporaneous cash-to-equity ratios. 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. 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) All	(2) Below-Median Cash-to-Equity	(3) Above-Median Cash-to-Equity
Treat x *Post*	0.0059**	0.0023	0.0095*
	(0.003)	(0.002)	(0.005)
Treat	-0.0039***	-0.0011	-0.0083***
	(0.001)	(0.001)	(0.002)
Repurchases to Equity	0.0147	0.1370*	-0.0315
	(0.033)	(0.083)	(0.028)
Dividends to Equity	0.9491***	0.8850***	0.9872***
	(0.070)	(0.057)	(0.105)
Log Market Cap	-0.0001	0.0004	0.0009
	(0.001)	(0.001)	(0.001)
Log Book to Market	-0.0077***	-0.0055***	-0.0081***
	(0.002)	(0.001)	(0.003)
Sales Growth	0.0071	0.0003	0.0127
	(0.007)	(0.004)	(0.011)
LT Debt to Equity	-0.0010	-0.0027***	0.0001
	(0.001)	(0.001)	(0.002)
Cash to Equity	0.0051	-0.0037	0.0025
	(0.003)	(0.006)	(0.005)
Time FE	Yes	Yes	Yes
Observations	2,781	1,548	1,233
R^2	0.2611	0.3026	0.2449

Table 9.
Accruals and Investments

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

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

Table 10.

Market Valuation

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

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