Index Creation, Information, and External Finance*

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ABSTRACT

How do firms added to an equity index change their financing strategies? We use the formation of new equity indexes and changes to index methodology as a setting to examine how shocks to a firm's information environment affect the debt supply and financing of firms. Firms added to an index are covered by more equity analysts and have greater news coverage, resulting in higher information production. Consequently, bond liquidity improves and firms benefit from lower yield spreads on newly issued debt. Treatment firms increase their leverage by about two percentage points relative to control firms. The response is primarily in the more information-sensitive public debt market, with firms issuing more public debt.

JEL classification: G14, G15, G32
Keywords: index membership, leverage, debt supply, cost of debt, capital structure

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I. Introduction

Investors find evaluating and monitoring informationally opaque firms both costly and imperfect. Information frictions, therefore, determine whether firms finance internally or externally and whether they seek funds from private financiers or public investors in arm's length capital market transactions. In this paper, we examine shocks to a firm's information environment due to the addition of the firm to a newly created index (or due to changes in the underlying index methodology or criteria for inclusion). In our data, these index events reduce information asymmetries between firms and their investors. The question is then how financing responds to a firm's inclusion in a newly created index and the accompanying increase in information.

Adverse selection models predict that firms rely more on internal funds and less on risky debt and outside equity when information asymmetries are significant. The greater reliance on internal funds results in a decline in leverage as internal equity builds up through higher retained earnings. As information asymmetries lessen, firms switch from internal funds (and safe debt) to risky debt. They issue equity only when financing with debt would produce excessive leverage. When firms are operating below their debt capacities and information frictions diminish, debt issues will take precedence over equity issues. Furthermore, an improvement in information environments should matter less to bank lenders than to public debt investors. Thus, we expect a more significant response in public debt markets than in bank loans.[1]

We assemble a worldwide database of 198 index events leading to the addition of 5,290 firms to equity indexes. Most of these additions are to newly created equity indexes (91%

[1]Adverse selection models predict that firms should switch to more information sensitive debt as they raise more debt financing. Thus, as information asymmetries lessen, firms should issue public debt, which is more information sensitive. Bank lenders interact with firms over time and across many products, which gives them an information advantage. Among other things, banks have access to checking the accounts of their borrowers. It allows them to monitor borrowers without relying on public sources of information (Mester et al., 2001). Firms also reveal proprietary non-public information more readily to banks than to public debt investors, which gives banks an advantage in collecting and processing non-public information (Bhattacharya and Chiesa, 1995; Yosha, 1995).

of the sample). The rest are due to changes to the number of index constituents (8.5%), changes to the eligible index universe (2.3%), and changes to the index selection criteria or criterion weightings (0.6%).[2] These index events span 21 markets over the period from 1996 to 2014. In the process, we step away from much of the existing literature that relies on reconstitutions of existing U.S. indexes; the events in our sample are also unanticipated as they are announced on short notice.

We find that information asymmetries significantly lessen for firms added to indexes relative to matched control firms in a difference-in-differences (DID) setting. Analyst coverage increases by about 14% relative to control firms. News coverage is up by about 16%. Treatment firms also exhibit significant improvements in stock and bond liquidity in the post-event period. Their credit spreads decline by about 17 basis points relative to that of matched control firms in the post-event period. In short, the evidence suggests a remarkable reduction in information asymmetries between investors and firms surrounding index additions.

The key finding of the paper is a significant increase in leverage of firms added to an equity index relative to control firms in the three years after their inclusion in an index. The leverage of treatment firms is almost two to three percentage points higher (a 7% increase relative to the sample mean). We find a similar result for the subset of events for which we have information on the underlying, rule-based index methodology. For this sample we can compare firms that just made the threshold for index inclusion to those firms that just missed the threshold (based on the index construction methodology) in a regression discontinuity design (RDD). The RDD gets us closer to a quasi-random selection into treatment and non-treatment stocks and suggests that our findings cannot be attributed to unobserved heterogeneity at the country-industry-year or index level. Additional tests rule out concerns that our findings may reflect strategic behavior by the index providers.

[2]Firms can be treated multiple times.

Consistent with information frictions driving leverage response to index additions, we find that the effect of index membership on leverage is stronger in countries with low disclosure standards and poor accounting quality. Treated firms in countries with low disclosure standards or poor accounting quality exhibit leverage increase between 2.1% and 2.7% after inclusion in an index relative to control firms. By contrast, the leverage ratios of treatment firms are similar to those of control firms for firms in countries with high disclosure standards and strong accounting quality. Also consistent with the information channel, we find that almost all of the increase in leverage around index additions can be attributed to a rise in public debt. In contrast, private debt ratios show no statistical change following index additions.

Overall, the results show that index additions result in an increase in analyst and news coverage, improved stock and bond liquidity, and lower cost of debt. Firms respond by primarily issuing more public debt, which increases their leverage. The evidence suggests that shocks in equity markets to information production affects debt financing. The finding that additions of firms to indexes improve their information environment with significant consequences for their financing contributes to the literature on the effect of information and other supply-side frictions on the financing of firms. For example, the prior literature highlights the importance of debt ratings (Faulkender and Petersen, 2006), bank funding constraints (Leary, 2009), bank lines (Sufi, 2009), branching restrictions (Rice and Strahan, 2010), and the availability of credit default swaps (Saretto and Tookes, 2013). We show that equity index membership reduces supply frictions and increases debt financing. Thus, we offer evidence that shocks to the information environment of firms emanating from equity markets propagate to public debt markets.

The paper also contributes to the recent literature that examines index membership, its effect on institutional ownership, and how that leads to changes in governance (Appel et al., 2016; Schmidt and Fahlenbrach, 2017), changes in disclosure and trading costs (Boone and White, 2015; Bird and Karolyi, 2016) and payout ratios (Crane et al., 2016). These papers largely rely on index reconstitutions and focus mostly on two major U.S.

indexes – S&P 500 and Russell 1000/2000. So far, there is limited evidence on the effects of index membership on leverage, cost of financing and whether financing is from private investors or arm's length capital markets.[3] We contribute to these studies by constructing, for the first time, a worldwide sample of index events across 21 markets related to the formation of new indexes or changes to the methodology of existing indexes.

Finally, the paper provides a more granular understanding of how inclusion in an index affects not just the firm's leverage but also issuance decisions and costs of financing. Joint evidence on the effect of inclusion in indexes on bond and stock liquidity, analyst coverage, media coverage, and the costs of debt and equity financing is new. For this, we construct a worldwide sample of index events, which allows us to provide a broader view of index launches and changes to index methodologies, an advantage that we have over single-index or single-country studies. The global sample also allows us to exploit cross-country differences in the effect of index membership on financing. We provide consistent evidence in support of the information channel. In essence, index membership increases analyst coverage and the amount of news as well as it increases the liquidity of both stocks and publicly traded bonds. We also present additional analyses consistent with the information channel, which shows that the leverage effects arise only through greater public debt issuances and are significantly more pronounced in countries with poor disclosure regimes.

[3]Index reconstitution, as an empirical setting, is also used in a recent paper by Cao et al. (2019), which examines the effect of index membership on small firm financing. Cao et al. find that small firms added to the Russell 2000 reduce their reliance on bank loans and increase their equity financing. These results largely reflect their focus on small firms, which rarely include public debt as one of their financing choices. Also, these findings may be size-specific since prior work on S&P 500 reconstitutions suggests that index additions do not affect equity issuance for S&P 500 firms (Chen et al., 2004).

II. Data

A. Identifying Index Events

We searched through all available press releases (including archived press releases) from major index providers worldwide to identify the four types of index events described below. For this, we reviewed 54,000 press releases from 32 index providers from January 1996 to June 2014.[4]

(a) **New index launches**: These events include formations of new equity indexes. For example, FTSE launched the FTSE 350 Supersector Indexes in September 2004 to give investors a *"new way of looking at the U.K. market... suitable for a range of OTC investment products, including exchange-traded funds (ETF) and derivative products."* In July 2012, China Securities Index Co., Ltd. launched CSI Hong Kong 300 *"to measure the performance of securities in the Hong Kong market."*

(b) **Changes to the number of index constituents**: These events consist of increases in the number of index constituents. For example, the number of constituents of the Dow Jones US Select Dividend Index increased from 50 to 100 at the end of 2004. In making the change, Dow Jones stated that more companies are paying cash dividend and expanding their existing payout. Thus, doubling the number of stocks *"would do a better job of representing this type of equity investment."*

(c) **Changes to eligible index universe**: These events include changes to the countries or industries that are eligible for inclusion in an index. For example, in 1998, NASDAQ-100 included foreign stocks into the index. Similarly, in 2007, S&P and its partner Australian Securities Exchange (ASX) added foreign-domiciled shares to the core S&P/ASX suite of indexes. The index providers reasoned that this change *"will result in a more diversified investible benchmark... [T]he enhanced index suite... reflects the increased globalization and dynamic nature of the Australian equity market."*

(d) **Changes to index selection criteria or criterion weightings**: These events involve changes to index selection criteria or criterion weightings. For example, in 2002, Dow Jones revised its ranking methodologies for its Titans indexes, reducing the number of fundamental factors used to select index components from four to two (free float and liquidity). Dow Jones argued that *"eliminating two fundamental factors will make the index easier for investors and analysts to understand and if they wish, to replicate, all of which adds up to improved transparency."*

[4] Most of the 54,000 press releases, as it turned out, refer to events such as regular periodic updates to index membership or other general news (e.g., reports on index performance).

While we started with the 45 markets in Amihud et al. (2015), we ended up with 21 markets since many had few such events, or it was not possible to obtain the list of index constituents. We exclude indexes that only cover financial firms or those that are tailored to a specific client with no further information available. The FTSE Group created the highest number of indexes and accounted for roughly 25% of the indexes in the sample. Other prominent index providers include the China Securities Index Company (CSI), Dow Jones, and S&P. The newly created indexes are significant in many respects. They represent about 15% of the market capitalization of all publicly traded firms in their respective countries at the time of the index's launch. The number of ETFs benchmarked against the indexes in our sample is comparable to the average for all indexes in the Morningstar global ETF database. An average of 3.26 ETFs benchmark against the indexes in our sample while the average for all indexes in the Morningstar global ETF database is 4.51.

Index constituents are obtained from Datastream, Bloomberg, newswires, or from press releases depending on data availability. For event types (b) to (d), we require constituent lists to be available both before and after the respective events. The search yielded 198 index events resulting in 8,149 individual non-financial stocks added to an index.[5] Since several of these stocks appear in multiple index events, the number of unique firms is 5,290. Panel A of Table I shows that the majority of the affected firms reflect the formation of new indexes. New index launches represent 155 out of 198 events and more than 90% of the treatment sample. Panel B presents the distribution of firms across the major markets that survived our sample selection criteria. China, Hong Kong SAR, U.S., United Kingdom, Japan, France, and Germany dominate the list and have the highest numbers of affected firms.

— Table I about here —

[5] While we also identified index events that led to the deletion of stocks from indexes because of the closure of indexes, changes to eligible index universe, number of index constituents, and ranking methodology, we do not explore these in detail since the sample is small.

Panel C shows that most indexes are objectively constructed and follow well-defined selection criteria for inclusion into an index. Thus, almost 86% of firms are included in our treatment sample following quantitative and transparent rules determined at the time of index construction or subsequent modifications to the index methodology. The fact that most treatment firms are included following well-specified rules mitigates concerns about omitted unobservable factors driving inclusion and possibly also outcome variables. Panel D shows the broad geographic distribution of firms in our indexes. Only about 16% of stocks are in global indexes and a majority are in regional or national indexes. Panel E shows that more than half of the firms are part of broad market indexes. The majority of the remainder are in indexes categorized by market capitalization, where almost 20% refer to large firms, 5% to medium-sized firms, and about 17% to small-sized firms. Panel F shows that 70% of stocks are in diversified indexes with the remaining 30% being in sector indexes with a focus on manufacturing, consumer goods, telecommunications, and services.

Comparison to the Existing Literature: Our approach to selecting index additions differs from much of the existing literature, which is mostly about index reconstitutions. Earlier papers focused on S&P 500 reconstitutions.[6] A potential concern with S&P 500 index reconstitution is that the index provider does not always enforce the criteria for inclusion in the index (Chen et al., 2004). A broader concern with reconstitution methodologies is that the criteria for index inclusion are predictable. This predictability enables firms to alter their characteristics to meet the threshold for index inclusion. For example, in Germany in September 2015, Vonovia SE acquired two firms and thereby increased its market capitalization shortly before its inclusion in the DAX, an index consisting of 30 German blue-chip stocks. Another firm, ProSiebenSat.1 Media S.E., would have been included in the DAX instead had Vonovia not increased its market value. In these cases, index revisions reflect endogenous changes.

[6] See, for example, Shleifer (1986), Harris and Gurel (1986), Pruitt and Wei (1989), Beneish and Whaley (1996), Erwin and Miller (1998), Wurgler and Zhuravskaya (2002), Hegde and McDermott (2003), Denis et al. (2003), Chen et al. (2004), Blume and Edelen (2014), Elliott and Warr (2003), and Becker-Blease and Paul (2006).

There is an equally large and more recent literature that exploits the Russell 1000/2000 index reconstitution. The advantage of this approach is that market capitalization rankings at the end of May of each year determine the Russell index assignments.[7] However, the experimental setting based on Russell index reconstitution presents several challenges. First, the forcing variable is not observable, and the assignment is imperfect as it relies on the CRSP end-of-May market capitalization.[8] Second, institutional investors that track these indexes could preemptively hold shares in companies that are closer to the cutoff and thus have a higher likelihood of moving to the other index. Finally, Russell makes endogenous float adjustments in determining index weights. Thus, the index around the threshold becomes sorted endogenously based on characteristics related to market float.

Because we examine new indexes or changes to the number of index constituents or ranking methodologies, we mitigate concerns that firms could influence their addition to an index or that investors could predict which firms would be added to the index. The events are unanticipated since index providers announce these events on short notice – on average 44 days (median: 23 days) before the index event. The exact firms affected by the index events are announced only 25 days later (median: 1 day).

We pursue two alternative estimation methods to mitigate concerns that index providers are forming these indexes strategically in response to macroeconomic factors or industry conditions. The two methods present distinct trade-offs – a larger sample versus a tight identification strategy. We construct the difference-in-differences (DID) sample by matching within country, year, and industry and further match along several firm dimensions that drive the financing decisions of firms. It yields a larger sample since we do not have to know the exact index construction methodology. In the regression discontinuity design (RDD), we examine firms near the threshold and compare firms that just made the threshold for inclusion in the index to firms that just missed it. We do this for the subset of events with known, rule-based index methodology. While the sam-

[7] See, for example, Appel et al. (2016), Bird and Karolyi (2016), Boone and White (2015), Chang et al. (2015), Crane et al. (2016), and Schmidt and Fahlenbrach (2017).

[8] See Ben-David et al. (2019) for more recent instructions on how to predict index assignment.

ple shrinks, we are closer to a quasi-random selection into treatment and control firms. Because we focus on the small bandwidth around the index inclusion threshold, causal inferences from the RDD are potentially more credible and complement the results from the DID sample. We describe the DID and RDD samples next.

B. DID sample

We identify treatment firms as those added to an index. For each treatment firm, we select a control firm from all other firms in the same country, industry, and year in the Worldscope database. The control firms are the nearest neighbor on size, profitability, tangibility, and market-to-book ratios based on a propensity score.[9] We could match 3,913 out of the 5,290 (unique) affected firms.

We obtain additional information on debt structure from Capital IQ, analyst forecasts from I/B/E/S, and stock market variables from Datastream. Appendix A defines the variables and provides the corresponding data sources. We provide descriptive statistics in Appendix B.

Table II presents descriptive statistics for the DID sample before and after the propensity score matching. Following Imbens and Wooldridge (2009), we examine normalized differences between treatment and control stocks. If these normalized differences are less than 0.25, one could conclude that treatment and control firms are similar in characteristics we matched them on. Matching does reduce normalized differences to a considerable extent, with the remaining differences being both economically small and statistically insignificant. For two of the four variables, the normalized differences are close to the threshold of 0.25. In Section III, we, therefore, examine the robustness by applying a caliper to improve matching quality (Cochran and Rubin, 1973). The findings imply that after matching, treatment and control firms are observably similar based on robust factors

[9] We define industries based on the two-digit Industry Classification Benchmark (ICB) supersector level. The findings are similar when we represent the industry at the three-digit ICB sector or four-digit ICB subsector levels.

that determine leverage. Through our matching process, we ensure that our treatment and control firms are from the same country and industry and are similar in terms of size, profitability, tangibility, and growth opportunities in the year before the treatment.

— Table II about here —

C. RDD sample

The regression discontinuity framework compares stocks that just made the threshold for inclusion in the index to those that just missed the threshold. Overall, we could retrieve index ranking methodologies for 1,660 stock additions. Typical ranking variables are free float, market capitalization, and liquidity. We use these published index methodologies to rank firms in the eligible index universe (i.e., all firms in the Datastream Worldscope lists for a given country) and identify those that just missed the index threshold (referred to as control firms). Firms that rank just below the cutoff (i.e., firms that just missed the cutoff for inclusion in the index) are good comparisons to those ranking just above the cutoff (i.e., firms that just made the cutoff). Let n be the number of treated stocks per index event ("bandwidth"). For each index event, we further include the next n stocks below the index inclusion threshold as the control group. For example, for a new index with 50 stocks, we refer to these 50 stocks as treated, and based on the index ranking methodology, add the next 50 stocks not included in the index as control firms to the sample.

Table III presents descriptive statistics for the regression discontinuity sample. Mean values for the covariates are close to those reported in Table II. When we indicate a bandwidth of "all", it means that the bandwidth equals the total number of treatment stocks for an event. We also perform robustness tests, where we set the bandwidth to "1/2", "1/3", or "1/4", which, in the case of "1/4", would mean that we consider 12 treated and 12 control stocks. In doing so, we ensure a higher degree of randomness around the threshold, although this approach reduces the statistical power of the tests. Also, note that all four bandwidths are consistent with the prior literature.[10] Other than profitability, the remaining covariates are well balanced. Once we reduce the bandwidth to "1/2", differences in profitability between treatment and control firms decrease to two percentage points. For a bandwidth of "1/4", the difference in profitability between treatment and control firms becomes economically small (one percentage point; normalized difference: 0.66).

— Table III about here —

While the criteria for the selection of controls differ between the DID and RDD samples, we find a significant degree of overlap among control firms across the two samples. For the set of events with both the DID sample and the RDD sample, about 43% of control firms in the DID sample also appear as control firms in the RDD sample. Thus, our matching approach works well, even though we sort on different (but correlated) criteria to obtain the samples.[11]

III. Index Additions and Leverage

A. DID Results

A.1. Financing Patterns

Before presenting results on how leverage ratios evolve for firms that become part of an index, it is instructive to examine how the two components of leverage – debt and equity – change. Figure I plots the median total debt and median market value of

[10]For example, Chang et al. (2015) look at the ±100 firms around the Russell 1000/2000 threshold. Boone and White (2015) look at the ±50 to ±200 firms, while the corresponding numbers for Crane et al. (2016) are ±100 to ±750 firms. Appel et al. (2016) look at ±250 firms. Schmidt and Fahlenbrach (2017) look at ±500 firms.

[11]For the DID sample, we match based on well-known determinants of leverage, while for the RDD sample we mostly rely on free float.

equity around the fiscal year of the corresponding index event (year 0) with both variables normalized based on their values in year -1 for each firm in the sample. The figure shows that both debt and equity develop in parallel until the event year for both treatment and control firms. However, after the treatment, debt levels of treatment firms experience a substantial increase relative to that of the control group. By contrast, the increase in normalized equity after the treatment is small relative to control firms.

— Figure I about here —

The question is whether these changes in debt and equity reflect active financing decisions of firms and whether they impact leverage ratios. To answer these questions, we estimate the following DID regression specification:

$$\begin{aligned} \text{Financial Policy}_{i,j,k,t} = \alpha \cdot \text{Treated}_i \cdot \text{Post}_t + \beta \cdot \text{Post}_t + \vec{\gamma} \cdot \vec{X}_{i,j,k,t-1} \\ + \delta_1 \cdot I_i + \delta_2 \cdot I_t \cdot I_j + \delta_3 \cdot I_t \cdot I_k + \epsilon_{i,j,k,t}, \end{aligned} \tag{1}$$

where FINANCIAL POLICY$_{i,j,k,t}$ is the financial policy decision of firm i in industry j in country k in year t.

In Equation (1) above, TREATED$_i$ equals one if the firm is treated, i.e., the firm is added to an index for reasons identified earlier, and it is zero for matched controls. POST$_t$ equals one if year $t > 0$, and zero otherwise.[12] $\vec{X}_{i,j,k,t-1}$ is a vector of control variables. We follow Frank and Goyal (2009) and include the most important determinants of financial leverage, i.e., LN(TOTAL ASSETS), PROFITABILITY, TANGIBILITY, and the MARKET-TO-BOOK RATIO. Variable definitions are in Appendix A. Control variables are lagged by one year. I_i, I_j, I_k and I_t are firm, industry, country, and year fixed effects. $\epsilon_{i,j,k,t}$ is the error term.[13]

[12] Index inclusion is at $t = 0$ so that financial statements at the end of that year may already reflect the short-term effects of index inclusion. We therefore perform robustness tests where we set POST$_t$ to one if year $t \geq 0$, and obtain similar results.

[13] Please note that a firm may be treated multiple times. For notational ease, however, we do not include a further subscript for the respective index event.

Table IV reports results from tests that examine debt and equity issuance decisions. The dependent variables, DEBT ISSUANCE and EQUITY ISSUANCE, measure the issuance of debt and equity as a fraction of the outstanding amounts of both debt and equity. We examine three different event windows surrounding the event year: [-1,+1], [-2,+2], and [-3,+3]. The event year (year 0) itself is excluded from the analysis. In parentheses, we report Huber/White robust standard errors clustered by firm.

— Table IV about here —

The results show that treated firms issue significantly more debt in the years following their addition to indexes relative to control firms. The coefficient estimate on the interaction between TREATED and POST in Model 1 shows that treated firms issue 1.8 percentage points more debt in the period after the index inclusion relative to controls. The coefficient estimate is significant at the 1% level. The higher share of debt issuance by treated firms persists as we expand the event windows. By contrast, equity issuances show almost no change. Overall, the results show that firms added to an index issue relatively more debt than equity in subsequent years. The equity results are consistent with those reported by Chen et al. (2004), who show that firms added to the S&P 500 index also do not exhibit an increase in equity issuance in subsequent years.

If firms issue more debt and little equity following index additions, then we expect their leverage ratios to increase. Figure II plots the median market leverage ratios for treatment and control firms around the index addition events with leverage normalized to its value in year -1. Treated firms increase market leverage after index inclusion relative to control firms. The trends in leverage ratios for treatment and control firms are parallel up until the year of the treatment and only start diverging after the treatment. We infer from these results that index additions lead to long-term improvements in a firm's access to external financing.

— Figure II about here —

Table V reports results from estimates of Equation (1) where the dependent variables are market and book leverage ratios. Models 1-3 which examine market leverage show that firms included in an equity index have 1.1 to 1.7 percentage points higher market leverage in the period following the event relative to matched controls.[14] The increase is statistically significant and represents an almost 7% increase in leverage, given that a typical firm in our sample has a leverage ratio of about 25%. We further reestimate all our tests with book leverage as the dependent variable and find similar results. For example, for the [-3,3] year window, we observe an almost 2% higher book leverage for treated firms in the post-event period relative to control firms. The results for other time windows are similar.

— Table V about here —

Several additional tests confirm that these results are robust. First, we reestimate the leverage ratio regressions after excluding stocks that are already part of another significant (and existing) index in a given country in the year before the index event.[15] The results in Model 1 of Appendix C show that the effects of index membership on leverage are about 0.3 percentage points stronger when we exclude these stocks.

Second, we apply different calipers to the propensity score matching procedure to reduce potential bias (Cochran and Rubin, 1973). While the matching described earlier removes substantial bias, a tighter matching could potentially remove more cross-sectional differences between treatment and control firms. These restrictions also result in a smaller

[14] The findings remain unchanged when we exclude the most frequent event type (i.e., launches). Subsample tests excluding countries with the most observations (such as the U.S. or China) provide robust results as well. We also vary the number of control stocks per treatment stock (up to five control stocks per treated stock) and obtain very similar regression results. Our results are also robust to different industry classification methodologies (up to 4-digit ICB codes).

[15] We consider ASX 200 for Australia, TSK 60 for Canada, Shanghai Stock Exchange 50 Index for China, CAC 40 for France, DAX for Germany, ATHEX 20 for Greece, Hang Seng Index for Hong Kong, Nifty 50 for India, Tel Aviv 35 Index for Israel, Nikkei 225 for Japan, AEX 25 for the Netherlands, WIG 30 for Poland, PSI 20 for Portugal, Straits Times Index for Singapore, KOSPI 2000 for South Korea, IBEX 35 for Spain, OMXS 30 for Sweden, SMI for Switzerland, TAIEX for Taiwan, FTSE 100 for the U.K., and the S&P 500 index for the U.S.

sample since it implies that we only consider those observations where the difference between propensity scores of treated and control firms are smaller than the caliper. Appendix D shows the tighter caliper results. Absolute values for the normalized differences almost vanish. Model 2 of Appendix C reports the regression results with the sample restricted to the tighter caliper match. It confirms our earlier findings regarding changes in market leverage. In unreported tests, we experiment with other calipers and find that all of our results remain unchanged.

Third, we conduct placebo tests on the timing of the events. Models 3 to 5 of Appendix C present the leverage regression results for the treatment and control firms from a different time window. Specifically, we look at various windows around year -7. Thus, there is no overlap with the time windows from the primary analysis. For most time windows, no significant differences exist between treatment and control firms. The only statistically significant coefficient for TREATED $\times$ POST in Model 3 even exhibits a negative sign, which is opposite to our main findings.

Fourth, the results are robust to the inclusion of country-industry-year or index event fixed effects (Appendix E). The addition of country-industry-year is of particular importance in a cross-country setting. Thus, changes in leverage are unlikely to be driven by unobserved time-varying heterogeneity in an industry in a given country and year. The inclusion of index fixed effects further mitigates concerns that the results may stem from specific, time-invariant index characteristics.

A.2. International Heterogeneity

The findings that both book and market leverage increase for firms added to an index contrast those in Appel et al. (2016) where leverage does not show an appreciable change around Russell index reconstitutions. While there are differences in our index events – we focus on primarily on index launches while previous papers focus on reconstitutions of existing indexes – there are also country differences. To reconcile our evidence to existing work, we re-run our tests on just the U.S. firms. Similar to Appel et al. (2016), we do not find a significant effect of index additions on the leverage of U.S. firms.

These difference between our key findings and those for the U.S. highlight an important point that information frictions would largely matter in countries with weak disclosure standards and poor accounting quality. Are the index events that we document mostly present in countries that score poorly on disclosure standards and accounting quality? These are countries where we expect information asymmetries to be particularly severe. If we find that the effect of index additions on leverage is significantly higher in these countries compared to those in countries with better accounting quality, that will provide additional support for the information channel that we highlight.

We test this prediction by re-estimating Equation (1) for subsamples conditioned on country-level differences in disclosure standards and accounting quality. According to La Porta et al. (2006), security laws that describe the disclosure obligations of companies make it easier for investors to value companies and increase their willingness to finance them. Laws mandating greater disclosures, therefore, benefit stock markets. Countries that score high on disclosure standards have a better information environment, and firms in these countries should enjoy relatively smaller benefits from additions to equity indexes.

— Table VI about here —

Panel A of Table VI report results from DID market leverage regressions for countries that score below and above median disclosure standards in our sample. The results show that during the [-3,+3] year window centered around the index event, treated firms in countries with low disclosure standards increase market leverage by an average of 2.7% relative to controls. By contrast, index addition does not affect the leverage of firms in countries with high disclosure standards.

Panel B reports results for sample splits based on a measure of accounting quality. La Porta et al. (1998) attribute the cross-country differences in accounting quality to differences in corporate accounting practices and external reporting systems. The literature since then has argued that more transparent disclosure environments reduce the cost and increase the availability of external financing (see, for example, La Porta et al. (1998) and Doidge et al. (2004)). A useful measure of accounting quality is the index developed by the Center for International Financial Analyses (CIFAR). It represents the average number of ninety items included in the financial statements of a sample of domestic companies in that country. We find that in countries that score low on this index, firms that are added to an index increase their market leverage by 2.1% over the [-3,+3] years relative to their controls. By contrast, we find no effect of index addition on the leverage of firms in countries with a strong financial disclosure environment.

In unreported tests, we also examine sample splits based on country-level measures of weak governance such as measures of self-dealing or tunneling and lack of shareholder protection. We find no support for the view that passive institutions affect corporate governance with implications for leverage. The evidence is primarily consistent with the belief that what matters for credit extension is information.

A.3. Public and private debt

As discussed earlier, private debt markets offer an advantage in investigating informationally opaque firms. Banks or private lenders often have close and continued interactions with the borrowers.[16] Private lenders interact with firms over time and across many products; these intermediaries are thus able to partially alleviate the information asymmetries that typically exist between firms and investors. On the other hand, arm's length debt investors with coarser and more costly screening technology face more significant information frictions. The addition of a firm to an index will, therefore, have a more substantial effect on screening and monitoring costs of arm's length public debt investors than it would on costs incurred by relational lenders.

[16]See Petersen and Rajan (1994) for a discussion.

If public debt investors become less informationally disadvantaged, then we would expect a more significant increase in the supply of public debt than bank debt.[17] So, the question is how much of the increase in debt issuances that we witnessed in Section A.1 is due to the issuance of public debt relative to the issuance of bank loans? Information on debt composition is from Capital IQ. We match the sample firms to those in the Capital IQ database using a variety of matching algorithms, including various firm identifiers, which we supplement with a manual matching of firm names. We define the public debt ratio by dividing the sum of senior and subordinate bonds and notes by total market capitalization (total debt plus market value of equity). We then categorize all the remaining debt as private debt and estimate the DID specification similar to those for the leverage ratios. The results are in Table VII.

— Table VII about here —

Our primary interest is in the coefficients on the treated indicators interacted with the post-event indicator. In Models 1 to 3, we report results for the public debt ratio. In all event windows, the difference-in-differences term is significantly positive. The magnitude of the effect is similar to the overall leverage change indicating that much of the increase in leverage reported earlier is due to the public debt component of total leverage.

By contrast, the coefficients on the treated indicator interacted with the post-event indicator for the private debt ratio in Models 4 to 6 are not significantly different from zero. The results confirm that the addition of firms to indexes increase the public debt as a proportion of total market capitalization by 1.1% to 1.7% relative to controls, with no apparent change in private debt ratios. This supports our prediction that shocks to the information environment should matter more to arm's length investors than to relational lenders.

[17] Previous studies that examine debt structures, such as those by Rauh and Sufi (2010) and Colla et al. (2013) show that, as information asymmetries lessen, firms switch to public debt sources.

B. Regression discontinuity results

The regression discontinuity model is specified as follows:

$$\Delta \text{Lev}_{i,j,k,t_1,t_2} = \beta \cdot \text{Treated}_i + \vec{\gamma} \cdot \Delta \vec{X}_{i,j,k,t_1,t_2} + \sum_{p=1}^{4} \theta_p \cdot D_i^p + \sum_{p=1}^{4} \vartheta_p \cdot D_i^p \cdot \text{Treated}_i + \delta_1 \cdot I_t \cdot I_j + \delta_2 \cdot I_t \cdot I_k + \epsilon_{i,j,k,t}, \tag{2}$$

where $\Delta\text{LEV}_{i,j,k,t_1,t_2}$ is the change in market leverage of firm i in industry j in country k from year t_1 to year t_2.[18] Treated_i equals one if firm i made the inclusion threshold, and zero otherwise. $\Delta \vec{X}_{i,j,k,t_1,t_2}$ is a vector of changes in control variables from year t_1 to year t_2. D_i is the assignment variable, defined as the threshold of index inclusion minus the index ranking based on the index methodology. The cutoff point is defined as the ranking of the lowest ranked firm from the treatment group, i.e., $D_i \geq 0$ if firm i belongs to the treatment group, and $D_i < 0$ if not. We employ polynomials of order 1, 2, and 4 on both sides of the threshold and denote the order of the polynomial by p. I_j, I_k and I_t are industry, country, and year fixed effects. ϵ_{i,j,k,t_1,t_2} is the error term.

— Figures III and IV about here —

Figure III shows graphical results. We present a regression discontinuity plot with a linear fit and the corresponding 90% confidence intervals. The y-axis represents the change in market leverage from the fiscal year before the index event to three fiscal years after the event. The x-axis is the distance from the respective index threshold. The greater the absolute value of the x-axis, the farther away the stock is from the cutoff. Dots to the right of the cutoff represent stocks added to indexes, while those to the left represent stocks not included in the index. We can interpret the dots as the average change in leverage for all observations in the same bin, where the bin size is ten.

[18] For brevity, we only report results based on market leverage ratios. We find similar results with book leverage ratios.

The plot shows that firms that have just been included in an index increase their leverage by about 3-4 percentage points, while control firms close to the threshold do not appear to change leverage. As one goes farther away from the threshold, the number of observations per bin decreases because few index events affect a large number of stocks. Thus the confidence intervals widen up as we move farther away from the threshold. The kernel density plot around the index inclusion threshold in Figure IV also confirms that most of the observations in the RDD sample are relatively close to the index inclusion threshold.

Panel A of Table VIII reports regression results of Equation (2). The dependent variable is the change in MARKET LEVERAGE over the time windows indicated in the column titles.

— Table VIII about here —

Overall, the regression results confirm the findings from the DID regressions, and they are robust to different polynomials and bandwidths. They are also statistically significant, except for Models 7 and 8, where power is low due to the limited number of observations. Nevertheless, the point estimates remain relatively high (2.1-2.7%), even in those cases. Also note that, in Model 1, the distance to the threshold of the median treatment firm per event is 22 firms. In models with one-quarter of the bandwidth, the distance decreases to 7, consistent with the density plot in Figure IV, which suggests that most observations are close to the index threshold.

Although firms cannot manipulate their inclusion into the index, it is still possible that index providers behave strategically and distinguish between prospective "winners" and "losers" in determining which stocks to include in their indexes. One may also argue that decisions by index providers reflect the asset management industry's identification of factors that consistently outperform traditional benchmark models (for example, see the discussion in Cremers et al. (2012); Berk and van Binsbergen (2015); Crane and Crotty (2018). This description, however, does not match the stated objective of almost all the index providers, which is to create indexes to be a proxy for a market or sector and not to create a list of future "winners". Importantly, the results show no pre-trends and we find no differences in leverage-relevant characteristics for firms that are in the index relative to those that are not.

Nevertheless, to further deal with this concern, we restrict the dataset to index events where the index provider has less control over the size of the index. For this, we can exploit two aspects of the dataset. First, we restrict the dataset to index families where the index providers create several related industry indexes at the same time. When these indexes all have the same size, it is unlikely that expectations from a single industry drive the size of these indexes. For example, the D.J. Titans family of indexes includes 30 stocks each. Furthermore, index constituents are sometimes selected from the same universe using the same ranking methodology. For example, the CSI 300 Consumer Staples, the CSI 300 Energy index, and the CSI 300 Health Care index are all selected from the CSI 300 index. Second, index providers often choose index sizes that are multiples of 10 (e.g., 20, 30, 50, 100, etc.). We reestimate Equation (2) on this smaller sample and find results that are consistent with the above analysis. The results can be found in Panel B of Table VIII. Even though we have fewer observations, the signs and statistical significance of our findings are similar to, or sometimes even stronger (Models 7 and 8) than our main results. Our central finding, therefore, does not stem from strategic index creation by index providers.

Finally, to ensure that changes in the covariates do not drive these findings, we estimate a specification similar to Equation (2) for the control variables (i.e., replace the dependent variable by each of the leverage controls and reestimate the equation). Table IX reports the results for changes from year -1 to year 3, second-degree polynomials, and a bandwidth that corresponds to all affected treatment stocks (cf. Model 4 of Table VIII). The results confirm that the control variables do not drive leverage changes around the threshold since the changes in the covariates around the index events are mostly not statistically significant. Unreported regressions using other specifications where we vary time windows, polynomials, and bandwidths yield the same conclusion. The only exception is the market-to-book ratio, which exhibits an increase (significant at the 10% level). However, given the widely observed negative relation between the market-to-book ratios and leverage, the increase in market-to-book ratios for treatment firms after index additions only biases against observing a treatment effect in leverage.

— Table IX about here —

These results collectively indicate that firms that became part of an index have 2 to 3% higher leverage in the three years following the formation of the index event compared to otherwise similar firms not included in the index. The higher leverage of affected firms in the post period is a result of higher debt issuances by these firms. The results also suggest no significant response in the equity markets in the three years following the addition of a firm to an index.

IV. Index Additions and Information Environment

A. Analyst Following and News Coverage

Do index events change the information environment for firms? We expect institutional investors bench-marking to equity indexes to optimally tilt their portfolios towards indexed firms. Thus, we expect a greater production of information about firms that join the index and it should show up in greater analyst following of stocks in an index. Stocks in an index may receive more attention in general, creating more demand for information, and thus more extensive news coverage.[19]

[19] Appel et al. (2016) show that even passive index funds are active monitors of companies since they own significant voting blocs. Furthermore, many active funds are "closet-indexers" (Cremers et al., 2012) and therefore hold sizable proportions of stocks in their benchmark index. Large and concentrated ownership of passive investors reduces coordination costs of activism and increases campaigns by active investors (Appel et al., 2019). Boone and White (2015) show that analyst coverage increases with institutional ownership using the annual reconstitution of the Russell 1000 and 2000 indexes as an empirical setting. In contrast, we use a worldwide sample of index events that mostly consist of new index launches.

Analysts enhance the informational efficiency of security prices by engaging in a search for private information (Frankel et al., 2006). They determine the speed with which prices reflect public information, especially bad news (Hong et al., 2000). Analysts also serve as whistle-blowers and are often the first to detect managerial misbehavior (Dyck et al., 2010). Finally, they influence the nature of corporate disclosure leading to improved financial reporting quality (Irani and Oesch, 2013).

The recent literature on the effect of analyst coverage on corporate policies offers mixed conclusions. This literature uses two natural experiments: broker closures as in Kelly and Ljungqvist (2012) and broker mergers as in Hong and Kacperczyk (2010). Derrien and Kecskés (2013) show that firms losing an analyst decrease their investment and financing. Derrien et al. (2016) show that losing analysts results in a higher cost of debt and more frequent credit events. In both papers, the authors attribute their findings to the hypothesis that a fall in analyst coverage increases information asymmetries. By contrast, Li and You (2015) show that changes in analyst coverage do not lead to changes in information asymmetries; instead, they affect investor recognition. Li and You further argue that the effect of coverage terminations are more likely to be short-term since other brokers may start to cover the firm. Thus, we expect the effect of positive and negative shocks to analyst coverage on information frictions to be asymmetric.

We employ two variables to measure INFORMATION. First, we use ANALYSTS, measured as the natural logarithm of the number of unique analysts providing earnings forecasts for the sample firms. Information on the number of analysts comes from the Thomson Reuters Institutional Brokers Estimate System (I/B/E/S). Second, we use NEWS COVERAGE, estimated from information retrieved from the Ravenpack database, as the natural logarithm of the number of news about the firm in a year. We exclude firm-issued press releases and include only news with new information according to the classification algorithm used by the Ravenpack database.

We then estimate the changes in analysts and news coverage for treatment firms relative to control firms over the [-3,+3] year window around the index event. These tests include firm fixed effects to control for time-invariant differences, Year $\times$ industry fixed effects capture time-varying industry factors, and Year $\times$ country fixed effects capture time-varying country factors.

— Table X about here —

Consistent with the view that index inclusion reduces adverse selection costs through higher information production, we find in Model 1 of Table X that 14% more analysts follow firms after their addition to an index relative to otherwise similar control firms. In Model 2, we find that, after index inclusion, treatment firms have 16% more news coverage. If this lower adverse selection increases debt supply, then we expect indexed firms to issue more public debt and to increase their leverage.[20]

B. Stock and Bond Liquidity

We further test whether the liquidity of a firm's publicly traded securities responds to changes in information availability. Search and trading frictions in stock and bond markets should decline as a firm's information environment improves because of higher information production by analysts and investors. The higher information production for indexed firms should lower the asymmetric information costs of trading.[21] Other factors could also contribute to an increase in liquidity. If securities of a firm in an index trade more widely and if indexing brings in more liquidity-motivated traders, it should result in lower trading costs and greater liquidity. Overall, the consensus in the literature is that a richer information environment leads to greater liquidity.[22] If so, we then expect the

[20]In unreported tests, we find that the effect of index inclusion on leverage is stronger for firms with a more significant increase in analyst coverage around the index inclusion event. We also observe that more news coverage is associated with larger increases in leverage, although the results are weaker for news coverage.

[21]See, for example, discussion in Shleifer (1986), Wooldrige and Ghosh (1986), and Edmister et al. (1996). Analyst coverage is also positively related to market liquidity. See evidence in Roulstone (2003).

[22]Harris and Gurel (1986) and Dhillon and Johnson (1991) show increased volume and enhanced liquidity following the addition of stocks to the S&P 500. Lynch and Mendenhall (1997) find a permanent

addition of a firm to an index to increase the liquidity of its publicly traded stocks and bonds. We examine this prediction using the following DID specification on measures of stock and bond illiquidity.

$$\begin{aligned} \text{Liquidity Costs}_{i\ (or\ b),j,k,t} &= \alpha \cdot \text{Treated}_i \cdot \text{Post}_t + \beta \cdot \text{Post}_t + \vec{\gamma} \cdot \vec{Z}_{i,t-1} \\ &\quad + \delta_1 \cdot I_{i\ (or\ b)} + \delta_2 \cdot I_t \cdot I_j + \delta_3 \cdot I_t \cdot I_k + \epsilon_{i,j,k,t}, \end{aligned} \tag{3}$$

When we examine stock liquidity, the dependent variable, LIQUIDITY COSTS$_{i,j,k,t}$ is the average daily relative bid-ask spread (bid-ask-spread divided by mid-price) for stock i in industry j in country k in a given fiscal year t. We include MARKET CAPITALIZATION, TRADING VOLUME, RETURN, and RETURN VOLATILITY as control variables.[23] The data for constructing these variables is from Datastream. Variable definitions are in Appendix A. As before, the specification includes firm fixed effects, year × industry fixed effects, and year × country fixed effects.

When we examine bond liquidity, the dependent variable, LIQUIDITY COSTS$_{b,j,k,t}$, is one of the two measures of bond illiquidity described below for bond b in industry j in country k in a given fiscal year t. We first construct a bond level database by first identifying outstanding bonds issued by the sample firms from CapitalIQ. We then match bonds issued by treatment firms with those issued by control firms. To improve the matching quality, in addition to the determinants of leverage, we match the bonds based on the coupon rate and issue amounts. We then match these bonds to Bloomberg and obtain daily closing prices for publicly traded bonds.

Our first measure of bond illiquidity, ROLL_ZERO, is estimated as $2\sqrt{-\text{Cov}(r_t, r_{t-1})}$ when $\text{Cov}(r_t, r_{t-1}) < 0$, and zero otherwise.[24] Here, r_t is the return on day t. We calculate this measure on a daily basis for rolling 21 trading day windows and take the median value.

increase in trading volume following addition to the S&P 500 index. Hegde and McDermott (2003) and Chen et al. (2004) find that liquidity costs fall after inclusion in the S&P 500 index.

[23]For more discussion on control variables in liquidity costs regressions for stocks, see Copeland and Galai (1983), Stoll (2000), Pastor and Stambaugh (2003), Chordia et al. (2009).

[24]See Roll (1984) and Schestag et al. (2016).

Our second measure of illiquidity, ZERO_RET, based on Lesmond et al. (1999), uses the proportion of zero return days in a year. Less liquid bonds have less trading volume and hence more zero return days.

The time-invariant differences in illiquidity that are specific to bonds are captured by bond fixed effects δ_1. Time variations in bond illiquidity that are driven by industry and country factors are picked up by δ_2 and δ_3. $\vec{X}_{i,t-1}$ includes variables that control for time-varying issuer characteristics related to bond illiquidity. These variables include book leverage, firm size, tangibility, profitability, and the market-to-book ratio. The coefficient of interest is α, which measures the change in bond illiquidity for treated bonds of issuers relative to matched controls.

The result in Model 3 of Table X is consistent with the argument that index membership reduces information costs. The negative and significant coefficient on the interaction of TREATED and POST implies that bid-ask spreads decline significantly more for firms added to an index relative to otherwise similar firms that are not in the index.

Model 4 presents the results for ROLL_ZERO, while Model 5 presents the results for ZERO_RET. Both measures show that bond illiquidity declines significantly for treatment bonds after the issuer is in an index relative to matched bonds issued by control firms.[25]

In short, the finding that stock and bond liquidity improves for firms added to an index supports the information channel view. We can conclude that index additions result in an improvement in the information environment in public securities markets.

[25]In unreported tests, we also examine the FHT measure described by Fong et al. (2017), which combines the proportion of zero returns with the volatility of the return distribution. Also, we re-run our tests on different event windows to check if the results are sensitive to the period surrounding index additions. The results are robust, and these additional tests yield consistently negative effects of index additions on bond illiquidity.

C. Costs of Debt and Equity

The effect of information frictions will also show up in the cost of debt and equity. If index additions reduce information asymmetries for firms, then do treatment firms experience a more significant reduction in costs of debt and equity after being added to an index relative to control firms?

We estimate the following DID specification:

$$\begin{aligned}\text{Cost of Debt (or Equity) Capital}_{i,j,k,t} &= \alpha \cdot \text{Treated}_i \cdot \text{Post}_t + \beta \cdot \text{Post}_t + \vec{\gamma} \cdot \vec{X}_{i,j,k,t-1} \\ &+ \delta_1 \cdot I_i + \delta_2 \cdot I_t \cdot I_j + \delta_3 \cdot I_t \cdot I_k + \epsilon_{i,j,k,t}.\end{aligned} \tag{4}$$

$\vec{X}_{i,j,k,t-1}$ is a vector of lagged firm controls expected to be associated with the cost of capital of firms. These include book leverage, firm size, tangibility, profitability, and market-to-book ratio. α measures the change in cost of capital in response to additions of firms to indexes for treatment firms, relative to matched controls. We examine changes in both the cost of debt capital and the cost of equity capital.

To obtain the cost of debt capital, we collect information on the amounts and offered yields of about 24,000 bonds issued by treatment firms (and their respective controls) during the period from three years before to three years after the relevant index addition. Offered yield spreads are the difference between offered bond yields and the corresponding yield on a government bond with a similar maturity (obtained from Bloomberg) as of the offering date. When government bonds of the same maturity are not available, we estimate the government bond yields through interpolation. Finally, we aggregate a firm's cost of debt by either equally-weighting spreads of its outstanding debt or value-weighting them for each year during the [-3,+3] year window surrounding the index addition.

— Table XI about here —

The estimation results are presented in Models 1 and 2 of Table XI. We present results for equally-weighted spreads in Model 1 and value-weighted spreads in Model 2. We find that offered yield spreads decline significantly for treatment firms following their addition to an index relative to matched controls. Treatment firms offered spreads that are 17 basis points lower in the post-event period relative to control firms. This corresponds to about 8% of the average offered yield spread in the pre-event period (which is approximately 2.2%). Thus the reduction in offered yield spreads is both statistically significant and economically meaningful.

We next measure a firm's cost of equity capital by estimating its implied cost of capital (ICC), commonly used in both the accounting and finance literature to measure the cost of equity (see, for example, Chen et al. (2011)). ICC is the discount rate (or the internal rate of return) that makes the stock price equal to the present value of expected future cash flows. We obtain analyst forecasts, which proxy for a firm's cash flow expectations, from the IBES database.

The literature presents several different methods for estimating ICC with little consensus on which one performs best. The methods differ in their use of forecasted earnings over a finite forecast horizon and in the assumptions regarding short-term and long-term growth rates. We follow the common practice of estimating ICC using three different methods and then take the median estimate. The ICC models we estimate include: (1) Claus and Thomas (2001)'s residual income valuation model (ICC_CT), (2) Easton (2004)'s modified PEG ratio model (ICC_MPEG), and (3) Ohlson and Juettner-Nauroth (2005)'s abnormal earnings growth valuation model (ICC_JG).

In Model 3 of Table XI, we present estimates of Equation (4) where the dependent variable is ICC_MED, which is the median of the three ICC estimates discussed above. The results show no significant reduction in the cost of equity following a firm's inclusion in an index, relative to control firms. While the coefficient estimate of the interaction term, α, is negative, it is not reliably different from zero. In unreported tests, we examine each of the three ICC estimates separately and find no qualitative differences. Perhaps, the lack of significance is related to the ICC estimates themselves, which are inherently imprecise despite our estimating three different models.

V. Conclusions

A central theme of much of the corporate finance literature is the role that information asymmetries play in determining access to financing. However, it is challenging to identify exogenous shocks to market frictions to evaluate firms' responses to changes in their information environments. We construct a worldwide sample of events related to the formation of new equity indexes, changes to the number of index constituents, and changes to index selection criteria. Firms added to an index have substantially more analyst following, greater news coverage, a marked improvement in stock and bond liquidity, and a significant reduction in the cost of debt financing. Firms respond to the improvements in the information environment by significantly increasing debt issuances, which raises their leverage. The debt response occurs mostly in public debt markets.

By examining a worldwide sample of index events, we also consider less well-developed markets with poor accounting quality and weak disclosure standards. We show that debt responds much more strongly to index membership in these countries than in countries with relatively good information environments, where index membership has a more muted effect on financing. Furthermore, the results of this paper show that information production due to a firm's addition to an equity index has spillover effects on debt markets. We conjecture that information provided by analysts and improved liquidity as a response to index addition significantly reduces credit evaluation and monitoring costs for debtholders. This improvement in the information environment, in turn, lowers the costs of debt financing and increases the willingness of debtholders to buy the debt of those firms.

Overall, the findings highlight the role of indexing in mitigating information frictions and in improving financing outcomes for firms. While the index providers design new indexes or change existing ones to provide more accurate proxies for the market or the industry, they also create information externalities for firms, which improve their access to financing. The growing importance of ETFs worldwide and the rise of indexing will make these information externalities even more critical for firms that become a constituent of a major index.

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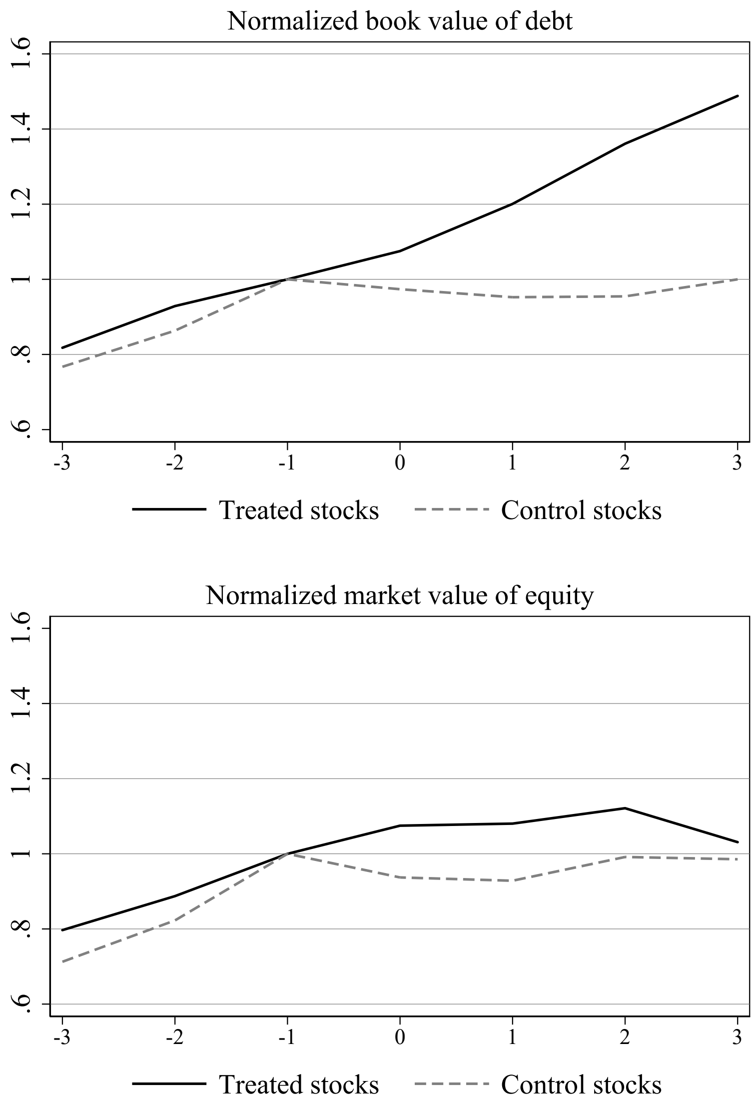

Figure I. The figure shows the observed real values of book debt and market equity of treatment and control firms around index events consisting of new index launches, changes in the number of index constituents, and changes in the ranking methodology of existing indexes. We designate firms that are added as a result of these index events as treatment firms and otherwise similar firms not in the index as control firms. The sample consists of 198 index events during the 1996-2014 period in 21 markets around the world. $t = 0$ is the year of the index inclusion. The plots report median values normalized to the value before the event for the three years after and the three years before the event.

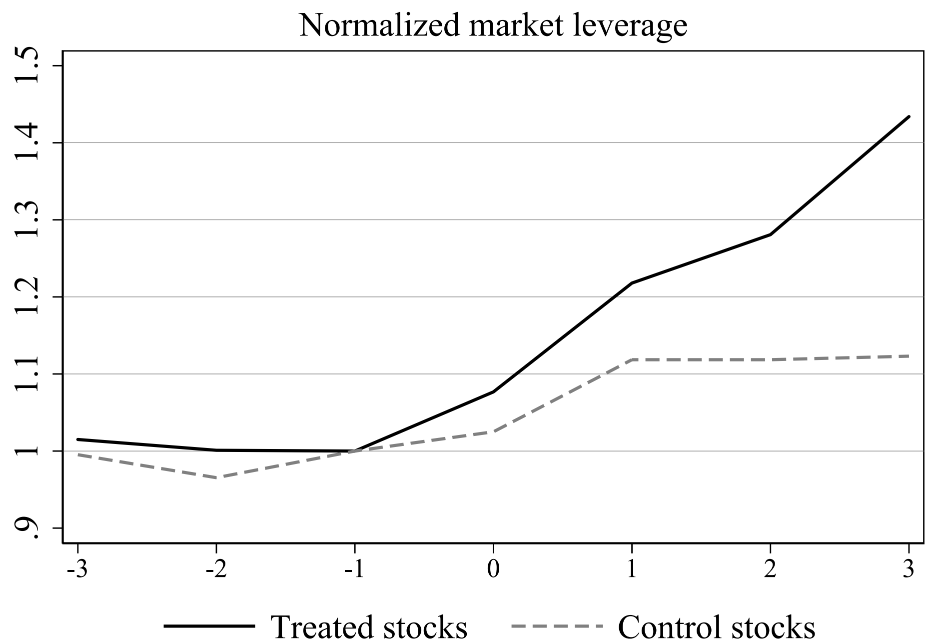

Figure II. The figure shows the evolution of market leverage around index events consisting of new index launches, changes in the number of index constituents, and changes in the ranking methodology of existing indexes. We designate firms that are added as a result of these index events as treatment firms and otherwise similar firms not in the index as control firms. The sample consists of 198 index events during the 1996-2014 period in 21 markets around the world. $t = 0$ is the year of the index inclusion. The plots report median values normalized to the value before the event for the three years after and the three years before the event.

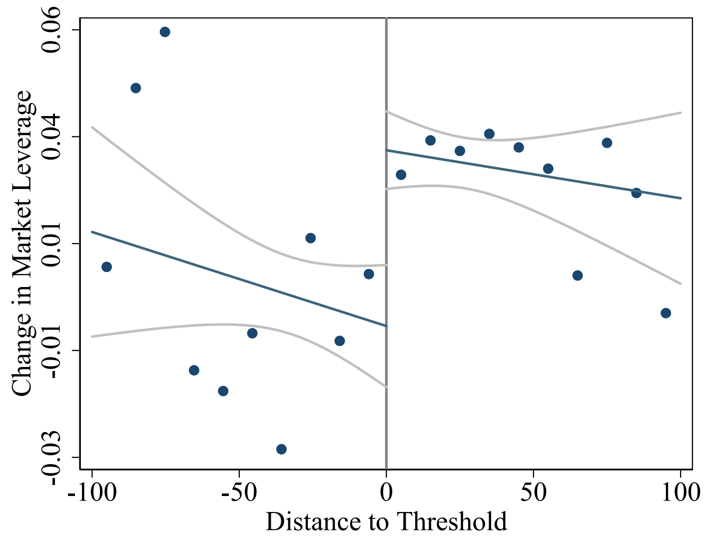

Figure III. The figure shows a regression discontinuity plot with linear fit and the corresponding 90% confidence intervals. The y-axis shows the change in mean MARKET LEVERAGE from one year before the event to three years after the event. The x-axis displays the distance from the respective index thresholds. Positive (negative) values refer to firms that are (not) included in an index. The bin width is 10 (firms).

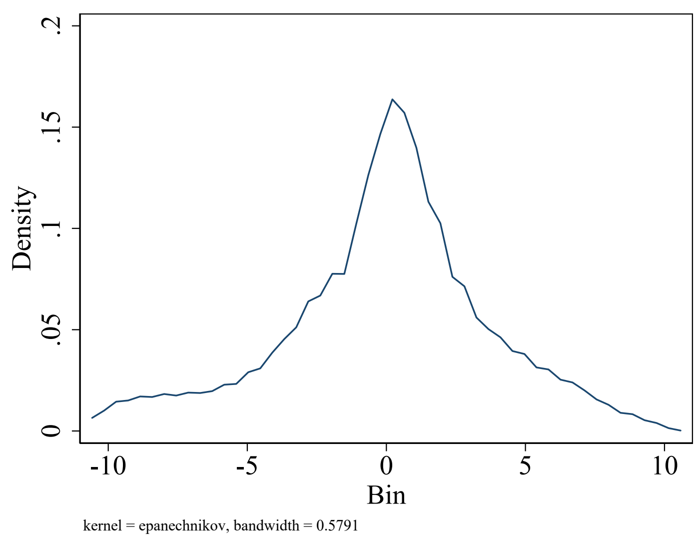

Figure IV. The figure shows a kernel density plot with an Epanechnikov kernel for the assignment of treatment and control firms into the 20 bins around the index inclusion threshold from Figure III.

Table I
Index Events

Panel A shows the distribution of equity index events and affected stocks across event types. "New launch" refers to the introduction of a new equity index. "Index universe" refers to a change to the countries or industries that are eligible for inclusion in an index. "Number" captures events based on an increase in the number of index constituents. "Methodology" change considers changes to index selection criteria and changes to criterion weightings. The sample period is from January 1996 to June 2014. Panel B shows the distribution of event firms across the main markets. Panel C provides a distribution of treatment stocks by index selection method, i.e., whether indexes are constructed based on a rule or by a committee. Panel D provides the distribution by geographic focus of the index, i.e., whether the index is global or regional (or national). Panel E provides a distribution of index styles, i.e., whether the index is broadly diversified or follows a style based on market capitalization or other characteristics. Panel F provides a distribution by industry sector, i.e., whether the index is diversified across industries or focuses on a particular sector.

Panel A: Event Type

Type (# of events)	Number	%
New Launch (155)	7,534	92.5%
Index universe (19)	132	1.6%
Number (10)	452	5.6%
Methodology (14)	31	0.4%
Total (198)	**8,149**	**100%**

Panel B: Markets

Market	Number	%
China	1,633	20.0%
Hong Kong	1,060	13.0%
USA	1,041	12.8%
UK	633	7.8%
Japan	590	7.2%
France	543	6.7%
Greece	448	6.5%
Germany	439	5.4%
Singapore	393	4.8%
Others (13)	1,369	16.8%
Total	**8,149**	**100%**

Panel C: Selection of index constituents

Selection Method	Number	%
Objective rule-based	6,996	85.9%
Subjective criteria	1,153	14.1%
Total	**8,149**	**100.0%**

Panel D: Index Geography

Focus	Number	%
Global	1,269	15.6%
Regional:	6,880	84.4%
Asia	3,976	48.8%
Europe	2,273	27.9%
Middle East	206	2.5%
Americas	425	5.2%
Total	**8,149**	**100.0%**

Table I: Continued

Panel E: Investment style			*Panel F: Industry*		
Style category	Number	%	Industry	Number	%
Broad market	4,568	56.1%	Diversified	5,702	70%
Style Indexes:			Sector Indexes:		
Large-cap	1,638	20.1%	Biotechnology	84	1.0%
Mid-cap	425	5.2%	Consumer goods	400	4.9%
Small-cap	1,368	16.8%	Energy & utilities	220	2.7%
Other	150	1.8%	Healthcare	82	1.0%
			Manufacturing	629	7.7%
			Real estate	92	1.1%
			Services	250	3.1%
			Telecommunication	384	4.7%
			Other	306	3.8%
Total	**8,149**	**100.0%**	**Total**	**8,149**	**100.0%**

Table II
DID: Propensity score matching

The table reports descriptive statistics for 3,913 non-financial stocks added to an index and their nearest-neighbor control stocks before and after propensity score matching. The control stocks are from the same country, year, and industry as the treated stocks, and are matched based on a propensity score using the natural logarithm of the dollar value of total assets, profitability, tangibility, and the market-to-book ratio. All stocks in the Worldscope country lists excluding the stocks in the treatment group are potential control stocks. The mean of treated and (unmatched and matched) control stocks, the mean difference between treated and control stocks, and the normalized difference in coefficients according to Imbens and Wooldridge (2009) in the year before the treatment are presented in the table. Normalized differences not exceeding 0.25 are considered to be not significantly different from zero.

	Before matching			
Variable	Mean (treated)	Mean (unmatched control)	Difference	Normalized difference
Ln(Total assets)	14.14	11.71	2.43	0.31
Profitability	0.12	0.02	0.10	1.29
Tangibility	0.31	0.29	0.03	0.24
Market-to-book ratio	3.31	3.20	0.12	0.00
Firms	3,913	30,996		
	After matching			
Variable	Mean (treated)	Mean (unmatched control)	Difference	Normalized difference
Ln(Total assets)	14.14	13.32	0.82	0.12
Profitability	0.12	0.12	0.00	0.23
Tangibility	0.31	0.29	0.02	0.20
Market-to-book ratio	3.31	3.61	-0.29	-0.01
Firms	3,913	1,691		

Table III
RDD: Univariate comparisons

This table reports descriptive statistics for firms in the regression discontinuity sample. The sample consists of stocks that just made the threshold for inclusion in an index (treatment stocks) and those that just missed the threshold (control stocks). Panel A includes firms ranked within the full bandwidth around the threshold, while Panel B only reports characteristics for half the bandwidth. The full bandwidth is defined as the number of affected stocks per index event, i.e., if an index with 30 constituents is created, 60 stocks will be considered (30 treatment and 30 control stocks). A "1/2" bandwidth refers to half the number of affected treatment stocks. The mean of treated and control stocks, the mean difference between treated and control stocks, and the normalized difference in coefficients according to Imbens and Wooldridge (2009) in the year before the treatment are presented in the table. Normalized differences not exceeding 0.25 are considered to be not significantly different from zero.

Panel A: Full bandwidth

Variable	Mean (treated)	Mean (control)	Difference	Normalized difference
Ln(Total assets)	14.12	12.75	1.37	0.24
Profitability	0.11	0.07	0.04	1.61
Tangibility	0.30	0.28	0.02	0.20
Market-to-book ratio	2.96	2.61	0.34	0.02
Firms	1,660	1,241		

Panel B: 1/2 bandwidth

Variable	Mean (treated)	Mean (control)	Difference	Normalized difference
Ln(Total assets)	13.70	12.73	0.97	0.16
Profitability	0.10	0.08	0.02	1.01
Tangibility	0.29	0.27	0.02	0.22
Market-to-book ratio	2.67	2.58	0.09	0.01
Firms	1,248	747		

Table IV
Debt and equity issuances: Difference-in-differences (DID) regressions for changes in index membership

The table reports coefficients from DID regressions. The dependent variables, DEBT ISSUANCE and EQUITY ISSUANCE, measure the issuance of debt and equity relative to the outstanding amount of both debt and equity. The sample is restricted to firm-year observations in the time window indicated in the column titles. The event year (0) is not included in the analysis. TREATED is a dummy variable set to one for stocks added to an index, and zero otherwise. Treatment stocks are those included in an index, while control stocks are not included in an index, but have similar firm characteristics. Control stocks are from the same country, year, and industry, and are matched based on a propensity score using the natural logarithm of the dollar value of total assets, profitability, tangibility, and the market-to-book ratio. POST is a dummy variable set to one in firm-years after a change in index membership. Control variables are lagged by one year. Huber/White robust standard errors clustered by firm are shown in parentheses. ***, **, and * indicate significance at the 1%-, 5%-, and 10%-levels, respectively.

Model	1	2	3	4	5	6
Window (years)	[-1,1]	[-2,2]	[-3,3]	[-1,1]	[-2,2]	[-3,3]
Dep. variable	DEBT ISSUANCE			EQUITY ISSUANCE		
Treated x Post	**0.018*** **	**0.015*** **	**0.012** **	**-0.004**	**-0.006* **	**0.001**
	(0.006)	**(0.006)**	**(0.005)**	**(0.003)**	**(0.003)**	**(0.005)**
Post	-0.010	-0.006	-0.011**	0.001	-0.000	-0.004
	(0.007)	(0.005)	(0.005)	(0.003)	(0.002)	(0.003)
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18,734	31,527	42,013	18,478	31,178	41,646
Treated	9,974	17,046	24,171	9,864	16,854	23,891
Adjusted R^2	0.510	0.424	0.361	0.621	0.435	0.193

Table V
Leverage changes around new index additions

The table reports coefficients from DID regressions. The dependent variables are MARKET LEVERAGE in Models 1 to 3 and BOOK LEVERAGE in Models 4 to 6. The sample is restricted to firm-year observations in the time window indicated in the column titles. The event year (0) is not included in the analysis. TREATED is a dummy variable set to one for stocks added to an index, and zero otherwise. Control stocks are from the same country, year, and industry, and are matched based on a propensity score using the natural logarithm of the dollar value of total assets, profitability, tangibility, and the market-to-book ratio. POST is a dummy variable set to one in firm-years after a change in index membership. Control variables are lagged by one year. Huber/White robust standard errors clustered by firm are shown in parentheses. *** and ** indicate significance at the 1%- and 5%-levels, respectively.

Model	1	2	3	4	5	6
Window (years)	[-1,1]	[-2,2]	[-3,3]	[-1,1]	[-2,2]	[-3,3]
Dep. variable	MARKET LEVERAGE			BOOK LEVERAGE		
Treated x Post	**0.011****	**0.015*****	**0.017*****	**0.014*****	**0.014*****	**0.019*****
	(0.004)	**(0.005)**	**(0.006)**	**(0.004)**	**(0.004)**	**(0.005)**
Post	-0.003	-0.005	-0.010**	-0.008**	-0.004	-0.010**
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22,460	38,433	51,439	22,460	38,433	51,439
Treated	11,816	20,587	29,277	11,816	20,587	29,277
Adjusted R^2	0.910	0.881	0.843	0.904	0.873	0.837

Table VI
Leverage: International variation in the index effect

The table reports coefficients from DID regressions based on median sample splits for the variables provided in the column titles. MARKET LEVERAGE is the dependent variable. The sample is restricted to firm-year observations in the [-3,3] time window. The event year (0) is not included in the analysis. TREATED is a dummy variable set to one for treatment stocks, and zero otherwise. Treatment firms are those added to an index, while control stocks are not included in an index but have similar firm characteristics. Control firms are from the same country, year, and industry, and are matched based on a propensity score using the natural logarithm of the dollar value of total assets, profitability, tangibility, and the market-to-book ratio. POST is a dummy variable set to one in firm-years after a change in index membership. Control variables are lagged by one year. Huber/White robust standard errors clustered by firm are shown in parentheses. ***, **, and * indicate significance at the 1%-, 5%-, and 10%-levels, respectively.

Panel A: Sample splits based on disclosure standards	Low	High
Treated x Post	**0.027*****	**0.004**
	(0.009)	**(0.009)**
Post	-0.015**	0.018**
	(0.007)	(0.009)
Control variables	Yes	Yes
Firm FE	Yes	Yes
Year x Industry FE	Yes	Yes
Observations	27,739	8,679
Panel B: Sample splits based on accounting quality	Low	High
Treated x Post	**0.021****	**0.003**
	(0.009)	**(0.007)**
Post	-0.015*	0.011*
	(0.008)	(0.007)
Control variables	Yes	Yes
Firm FE	Yes	Yes
Year x Industry FE	Yes	Yes
Observations	16,344	13,278

Table VII
Effect of index membership on public and private debt ratios

The table presents estimates of Equation (1) for the Worldscope-Capital IQ matched difference-in-differences sample of treatment and control firms. The dependent variable is either the ratio of public debt to market capitalization (total debt plus market value of equity) (in Models 1 to 3) or the ratio of private debt to market capitalization (in Models 4 to 6). The sample is restricted to firm-year observations in the time window indicated in the column titles. The event year (0) is not included in the analysis. TREATED is a dummy variable set to one for firms added to an index, and zero otherwise. Treatment stocks are those included in an index, while control stocks did not experience an index change but have similar firm characteristics. Control firms are from the same country, year, and industry, and are matched based on a propensity score using the natural logarithm of the dollar value of total assets, profitability, tangibility, and the market-to-book ratio. POST is a dummy variable set to one in firm-years after a change in index membership. Control variables are lagged by one year. Huber/White robust standard errors clustered by firm are shown in parentheses. *** and ** indicate significance at the 1% and 5% levels, respectively.

Model	1	2	3	4	5	6
Window (years)	[-1,1]	[-2,2]	[-3,3]	[-1,1]	[-2,2]	[-3,3]
Dep. variable	PUBLIC DEBT RATIO			PRIVATE DEBT RATIO		
Treated x Post	**0.017***** **(0.005)**	**0.014***** **(0.005)**	**0.011**** **(0.005)**	**-0.003** **(0.006)**	**0.003** **(0.006)**	**0.001** **(0.007)**
Post	-0.009*** (0.003)	-0.011*** (0.003)	-0.008** (0.004)	-0.000 (0.005)	0.002 (0.005)	-0.001 (0.005)
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	21,319	39,208	56,412	21,319	39,208	56,412
Treated	13,698	25,444	37,659	13,698	25,444	37,659
Adjusted R^2	0.809	0.760	0.740	0.861	0.825	0.790

Table VIII: Leverage: Regression discontinuity design (RDD) for changes in index membership

The table reports regression coefficients based on a RDD for stock inclusions. The dependent variable is the change in MARKET LEVERAGE over the time windows indicated in the column titles. Panel A refers to the full dataset, while Panel B is restricted to index events where the index provider has less control over the exact index size (e.g., because the index provider creates a whole family of indexes each with the same number of constituents). A bandwidth of "all" ("1/2") refers to the full (half the) number of affected treatment stocks, while "1/3" ("1/4") refers to one-third (one-fourth) of the number of affected treatment stocks. TREATED is a dummy variable set to one for stocks added to an index, and zero otherwise. Distance to threshold indicates how far the median treatment firm is away from the index inclusion threshold of the respective event. Control variables are based on first differences for the time windows indicated in the column titles. Robust standard errors are shown in parentheses. ***, **, and * indicate significance at the 1%-, 5%-, and 10%-levels, respectively.

Model	1	2	3	4	5	6	7	8
Panel A: Full sample								
Window (years)	1 vs. -1	2 vs. -1	3 vs. -1	3 vs. -1	3 vs. -1	3 vs. -1	3 vs. -1	3 vs. -1
Polynomial	One	One	One	Two	Four	One	One	One
Bandwidth	All	All	All	All	All	1/2	1/3	1/4
Dep. variable	ΔMARKET LEVERAGE							
Treated	**0.012****	**0.022*****	**0.028*****	**0.030*****	**0.025***	**0.035*****	**0.021**	**0.027**
	(0.006)	**(0.007)**	**(0.008)**	**(0.010)**	**(0.015)**	**(0.012)**	**(0.014)**	**(0.017)**
ΔLn(Total assets)	0.096***	0.104***	0.101***	0.101***	0.101***	0.122***	0.129***	0.138***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.011)	(0.013)	(0.016)
ΔProfitability	-0.291***	-0.364***	-0.357***	-0.358***	-0.357***	-0.331***	-0.321***	-0.374***
	(0.029)	(0.033)	(0.035)	(0.035)	(0.035)	(0.044)	(0.054)	(0.069)
ΔTangibility	0.190***	0.188***	0.245***	0.245***	0.246***	0.210***	0.218***	0.215***
	(0.032)	(0.029)	(0.030)	(0.030)	(0.030)	(0.039)	(0.040)	(0.047)
ΔMarket-	-0.004***	-0.002**	-0.004***	-0.004***	-0.004***	-0.001	-0.002	-0.000
to-book ratio	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)
Year x Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,340	3,806	3,615	3,615	3,615	2,147	1,439	1,009
Treated	2,511	2,210	2,109	2,109	2,109	1,364	922	635
Distance to threshold	22	21	21	21	21	14	10	7
Adjusted R^2	0.346	0.401	0.361	0.361	0.360	0.381	0.420	0.445

Table VIII: Continued

Model	1	2	3	4	5	6	7	8
Panel B: Manipulation by index providers								
Window (years)	1 vs. -1	2 vs. -1	3 vs. -1	3 vs. -1	3 vs. -1	3 vs. -1	3 vs. -1	3 vs. -1
Polynomial	One	One	One	Two	Four	One	One	One
Bandwidth	All	All	All	All	All	1/2	1/3	1/4
Dep. variable	ΔMARKET LEVERAGE							
Treated	**0.011**	**0.022*****	**0.027*****	**0.030****	**0.023**	**0.040*****	**0.036****	**0.037***
	(0.007)	**(0.008)**	**(0.010)**	**(0.012)**	**(0.018)**	**(0.014)**	**(0.017)**	**(0.021)**
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year x Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,161	2,682	2,597	2,597	2,597	1,480	949	664
Treated	1,770	1,502	1,450	1,450	1,450	909	587	403
Dist. to threshold	26	25	25	25	25	15	10	7
Adjusted R^2	0.348	0.399	0.354	0.354	0.353	0.406	0.459	0.498

Table IX
RDD robustness: Covariates around the threshold

The table reports regression coefficients based on a RDD for stock inclusions. The dependent variables are the control variables from Table VIII. The bandwidth of "all" refers to the number of affected treatment stocks. TREATED is a dummy variable set to one for stocks added to an index, and zero otherwise. Distance to threshold indicates how far the median treatment firm is away from the index inclusion threshold of the respective event. Robust standard errors are shown in parentheses. ***, **, and * indicate significance at the 1%-, 5%- and 10%-levels, respectively.

Model	1	2	3	4
Window (years)	3 vs. -1	3 vs. -1	3 vs. -1	3 vs. -1
Polynomial	Two	Two	Two	Two
Bandwidth	All	All	All	All
Dep. variable	ΔLN(TOTAL ASSETS)	ΔPROFITABILITY	ΔTANGIBILITY	ΔMARKET-TO-BOOK
Treated	**0.005**	**-0.003**	**0.010**	**0.385****
	(0.038)	**(0.010)**	**(0.008)**	**(0.212)**
Year × Country FE	Yes	Yes	Yes	Yes
Year × Industry FE	Yes	Yes	Yes	Yes
Observations	3,724	3,616	3,718	3,724
Treated	2,167	2,109	2,165	2,167
Distance to threshold	21	21	21	21
Adjusted R^2	0.171	0.208	0.126	0.286

Table X
Channel: Information production

The table reports coefficients from DID regressions. The dependent variables are the (log) number of analysts following (ANALYST), the (log) number of non-press release news (NEWS COVERAGE), stock liquidity (LIQUIDITY COSTS), and two measures of bond liquidity (ROLL_ZERO and ZERO_RET). The sample is restricted to observations in the time window indicated in the column titles. Models 1 to 3 are at the firm-level, while Models 4 and 5 are at the bond-level. The event year (0) is not included in the analysis. TREATED is a dummy variable set to one for treatment stocks, and zero otherwise. Treatment firms are those included in an index, while control stocks are not included in an index but have similar firm characteristics. Control firms are from the same country, year, and industry, and are matched based on a propensity score using the natural logarithm of the dollar value of total assets, profitability, tangibility, and the market-to-book ratio. In addition, for the bond-level sample, we also match bonds based on coupon rates and the notional amounts. POST is a dummy variable set to one in firm-years after a change in index membership. Control variables are lagged by one year. In Models 1 to 3 (4 and 5), Huber/White robust standard errors clustered by firm (bond) are shown in parentheses. ***, **, and * indicate significance at the 1%-, 5%-, and 10%-levels, respectively.

Model	1	2	3	4	4
Window (years)			[-3,3]		
Unit of observation		Firm		Bond	
Dep. variable	ANALYST	NEWS COVERAGE	LIQUIDITY COSTS	ROLL_ZERO	ZERO_RET
Treated x Post	**0.141*****	**0.160*****	**-0.051****	**-0.046****	**-0.042****
	(0.019)	**(0.050)**	**(0.022)**	**(0.019)**	**(0.019)**
Post	-0.004	-0.185***	0.050***	0.033	<0.000
	(-0.011)	(0.040)	(-0.015)	(0.042)	(0.044)
Market capitalization			-0.318***		
			(-0.018)		
Trading volume			-0.218***		
			(-0.013)		
Return			0.160***		
			(-0.014)		
Return volatility			0.923***		
			(-0.132)		
Book leverage				-0.027	-0.101**
				(0.026)	(0.051)
Leverage controls	Yes	Yes	No	Yes	Yes
Firm FE	Yes	Yes	Yes	No	No
Bond FE	No	No	No	Yes	Yes
Year x Country FE	Yes	Yes	Yes	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes
Observations	29,000	22,836	42,338	31,590	52,861
Adjusted R^2	0.849	0.890	0.95	0.927	0.954

Table XI
Effect of index membership on the costs of debt and equity

The table reports coefficients from DID regressions. The dependent variables, indicated in the column titles, are different measures for the cost of debt and cost of equity. For the cost of debt, we use the (value-weighted) average spread of the offered yields of a firm's bonds over the yields of a maturity-matched government bond in Model 1 (Model 2). The cost of equity is approximated by the implied cost of capital as estimated by three different methodologies and by then taking their median. See Section IV for more information. The sample is restricted to observations in the time window indicated in the column titles. The event year (0) is not included in the analysis. TREATED is a dummy variable set to one for treatment stocks, and zero otherwise. Treatment stocks are those added to an index, while control stocks are not included in an index but have similar firm characteristics. Control stocks are from the same country, year, and industry, and are matched based on a propensity score using the natural logarithm of the dollar value of total assets, profitability, tangibility, and the market-to-book ratio. POST is a dummy variable set to one in firm-years after a change in index membership. Control variables are lagged by one year. Huber/White robust standard errors clustered by firm are shown in parentheses. ***, **, and * indicate significance at the 1%-, 5%-, and 10%-levels, respectively.

Model	1	2	3
Window (years)		[-3,3]	
Dep. variable	OFFERING YIELD	OFFERING YIELD (weighted)	MED_ICC
Treated x Post	**-0.171*****	**-0.173*****	**-0.120**
	(0.066)	**(0.065)**	**(0.252)**
Post	0.111*	0.109*	0.023
	(0.057)	(-0.062)	(0.190)
Book leverage	0.691***	0.784**	2.283***
	(0.267)	(0.309)	(0.690)
Control variables	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Year x Country FE	Yes	Yes	Yes
Year x Industry FE	Yes	Yes	Yes
Observations	4,237	4,237	17,297
Adjusted R^2	0.414	0.399	0.589

Appendix A
Definition of variables

Variable	Description
Main variables	
Debt issuance	(Long term borrowings [WC04401] - reduction in long term debt [WC04701]) / (total debt [WC03255] + book value of common equity [WC03501]). Source: Worldscope.
Equity issuance	(Proceeds from the sale or issuance of stock [WC04302] - redemption of stock [WC04751]) / (total debt + book value of common equity). Source: Worldscope.
Market leverage	Total debt / (total debt + market value of common equity [WC08001]). Source: Worldscope.
Book leverage	Total debt / (total debt + book value of common equity). Source: Worldscope.
Public debt ratio	(Senior bonds and notes + subordinated bonds and notes) / (total debt + market value of common equity). Source: Capital IQ, Worldscope.
Private debt ratio	(Term loans + capital lease + revolving credit + commercial paper + other borrowings) / (total debt + market value of common equity). Source: Capital IQ, Worldscope.
Analyst	Natural logarithm of the total number of estimates for earnings per share ending in next fiscal year end. Source: I/B/E/S.
News coverage	Natural logarithm of the number of news in a year. Press releases by the firm itself and news with a global event novelty score below 100 are not considered. Source: Ravenpack.
Liquidity costs	Average relative bid-ask spread (bid-ask spread / mid-price) in a fiscal year. Source: Datastream.
Roll_Zero, Zero_Ret	Measures of bond liquidity. See Section IV for more information. Source: Bloomberg.
Offering yield	Average spread of bonds' offering yields over maturity-matched government bond yields (in percent). Source: Capital IQ, Bloomberg.
Med_ICC	Measure for a firm's implied cost of capital (in percent). See Section IV for more information. Source: I/B/E/S.

Appendix A: Continued

Control variables	
Ln(Total assets)	Natural logarithm of total assets [WC02999] in USD. Source: Worldscope.
Profitability	Earnings before interest, taxes, depreciation, and amortization (EBITDA) [WC18198] / total assets. Source: Worldscope.
Tangibility	Property, plant and equipment [WC02501] / total assets. Source: Worldscope.
Market-to-book ratio	Market value of common equity / book value of common equity. Source: Worldscope.
Market capitalization	Natural logarithm of market capitalization in USD. Source: Datastream.
Trading volume	Natural logarithm of the total number of shares traded in a fiscal year. Source: Datastream.
Return	Cumulative stock return in a fiscal year. Source: Datastream.
Return volatility	Standard deviation of monthly returns in a fiscal year. Source: Datastream.
Other variables	
Industry classification	Industry Classification Benchmark (ICB) supersector (2-digits). Source: Datastream.
Disclosure	Disclosure requirements index by La Porta et al. (2008) that measures the quality of company disclosure.
Accounting	Quality of accounting standards by La Porta et al. (1998) that measures the quality of accounting standards.

Appendix B
Descriptive statistics

This table shows descriptive statistics for the DID sample (Panel A) and the RDD sample (Panel B). All variables are measured at the firm-year level, except for ROLL_ZERO and ZERO_RET, which are at the bond-year level. Appendix A describes the variables.

Variable	N	Mean	Std. deviation	1^{st} quartile	Median	3^{rd} quartile
Panel A: DID sample						
Debt issuance [deflated by debt+equity]	42,013	0.02	0.15	-0.02	0.00	0.06
Equity issuance [deflated by debt+equity]	41,646	0.01	0.13	0.00	0.00	0.00
Market leverage	51,439	0.27	0.23	0.07	0.22	0.43
Book leverage	51,439	0.36	0.24	0.16	0.36	0.53
Public debt ratio	56,412	0.06	0.11	0.00	0.00	0.08
Private debt ratio	56,412	0.21	0.21	0.03	0.14	0.33
Analyst [#]	29,000	10.20	8.43	3.00	8.00	15.00
News coverage [#]	22,836	44.78	70.14	3.00	19.00	56.00
Liquidity costs [bps]	42,338	118.02	179.82	20.40	49.48	131.71
Roll_Zero	31,590	0.16	0.20	0.00	0.08	0.25
Zero_Ret	52,861	0.69	0.37	0.38	0.90	1.00
Offering yield [%]	4,237	2.12	1.61	1.03	1.95	2.90
Offering yield (weighted) [%]	4,237	2.08	1.62	1.01	1.83	2.82
Med_ICC [%]	17,297	11.82	5.27	8.46	10.64	14.13
Total assets [m$]	51,439	6,381	20,900	317	1,148	4,111
Profitability	51,439	0.12	0.10	0.07	0.11	0.16
Tangibility	51,439	0.31	0.23	0.12	0.26	0.46
Market-to-book ratio	51,439	3.03	3.59	1.14	2.01	3.49
Panel B: RDD sample						
Market leverage	4,340	0.25	0.23	0.05	0.20	0.41
Book leverage	4,340	0.33	0.23	0.12	0.32	0.50
ΔMarket leverage	4,340	0.03	0.14	-0.04	0.01	0.10
ΔBook leverage	4,340	0.02	0.14	-0.04	0.01	0.08
ΔTotal assets	4,340	0.00	0.00	0.00	0.00	0.00
ΔProfitability	4,340	-0.01	0.11	-0.04	-0.01	0.02
ΔTangibility	4,340	0.00	0.11	-0.03	0.00	0.03
ΔMarket-to-book ratio	4,340	-0.29	2.89	-0.93	-0.13	0.41

Appendix C
DID: Further robustness tests

The table reports coefficients from DID regressions. The dependent variable is MARKET LEVERAGE in all models. The sample is restricted to firm-year observations in the time window indicated in the column titles. The event year (0) is not included in the analysis. In Model 1, we do not consider treatment firms that are already part of a major stock index. In Model 2, we apply an additional caliper specification. See Appendix D for more details. In Models 3 to 5, we perform placebo DID regressions. The pseudo-event year (-7) is not included in the analysis. TREATED is a dummy variable set to one for treatment stocks, and zero otherwise. Treatment stocks are those added to an index, while control stocks are not included in an index but have similar firm characteristics. Control stocks are from the same country, year, and industry, and are matched based on a propensity score using the natural logarithm of the dollar value of total assets, profitability, tangibility, and the market-to-book ratio. POST is a dummy variable set to one in firm-years after a change in index membership. Control variables are lagged by one year. Huber/White robust standard errors clustered by firm are shown in parentheses. ***, **, and * indicate significance at the 1%-, 5%-, and 10%-levels, respectively.

Model	1	2	3	4	5
Window (years)	[-3,3]	[-3,3]	[-8,-6]	[-9,-5]	[-10,-4]
Test	Not part of important index	Strict caliper	Placebo	Placebo	Placebo
Dep. variable	MARKET LEVERAGE				
Treated x Post	**0.020****	**0.014****	**-0.021***	**-0.014**	**-0.014**
	(0.008)	**(0.006)**	**(0.011)**	**(0.011)**	**(0.013)**
Post	-0.006	-0.003	0.020***	-0.012	0.001
	(0.004)	(0.005)	(0.008)	(0.008)	(0.010)
Control variables	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year x Country FE	Yes	Yes	Yes	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes
Observations	38,621	29,538	6,162	8,699	9,911
Adjusted R^2	0.872	0.836	0.91	0.888	0.864

Appendix D
Propensity score matching with caliper specification

The table reports descriptive statistics for 2,698 non-financial stocks added to an index and their nearest neighbor control stocks before and after propensity score matching using an additional caliper specification that eliminates 99% of a potential matching bias. The control stocks are from the same country, year, and industry as the treated stocks, and are matched based on a propensity score using the natural logarithm of the dollar value of total assets, profitability, tangibility, and the market-to-book ratio. The matching basis for control stocks are all stocks in the Worldscope country lists without those from the treatment group. The mean of treated and (unmatched and matched) control stocks, the mean difference between treated and control stocks, and the normalized difference in coefficients according to Imbens and Wooldridge (2009) are presented in the table. Normalized differences not exceeding 0.25 are considered to be not significantly different from zero.

	Before matching				After matching			
Variable	Mean (treated)	Mean (unmatched)	Dif.	Norm. dif.	Mean (treated)	Mean (matched)	Dif.	Norm. dif.
Ln(Total assets)	13.24	11.71	1.54	0.23	13.24	13.28	-0.04	-0.01
Profitability	0.11	0.02	0.09	1.12	0.11	0.11	0.00	-0.01
Tangibility	0.30	0.29	0.01	0.10	0.30	0.29	0.01	0.11
Market-to-book ratio	3.30	3.20	0.11	0.00	3.30	3.50	-0.20	0.00
Firms	2,698	30,996			2,698	1,444		

Appendix E
DID: Country-industry-year and index event fixed effects

The table reports coefficients from DID regressions. The dependent variables are MARKET LEVERAGE or BOOK LEVERAGE in all models. The sample is restricted to firm-year observations in the time window indicated in the column titles. The event year (0) is not included in the analysis. Treatment stocks are those added to an index, while control stocks are not included in an index but have similar firm characteristics. Control stocks are from the same country, year, and industry, and are matched based on a propensity score using the natural logarithm of the dollar value of total assets, profitability, tangibility, and the market-to-book ratio. POST is a dummy variable set to one in firm-years after a change in index membership. Control variables are lagged by one year. Huber/White robust standard errors clustered by firm are shown in parentheses. ***, **, and * indicate significance at the 1%-, 5%-, and 10%-levels, respectively.

Model	1	2	3	4	5
Window (years)	[-1,1]	[-2,2]	[-3,-3]	[-3,-3]	[-3,-3]
Dep. variable	MARKET LEVERAGE			BOOK LEVERAGE	MARKET LEVERAGE
Treated x Post	**0.012*** **	**0.013*** **	**0.009**	**0.012** **	**0.017*** **
	(0.004)	**(0.005)**	**(0.006)**	**(0.005)**	**(0.006)**
Post	-0.004	-0.005	-0.002	-0.001	-0.005
	(0.004)	(0.003)	(0.004)	(0.004)	(0.005)
Control variables	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year x Country x Industry FE	Yes	Yes	Yes	Yes	No
Index Event FE	No	No	No	No	Yes
Observations	22,132	37,682	52,450	52,450	54,072
Adjusted R^2	0.922	0.908	0.874	0.876	0.842