

# The risk of being ranked: Investor response to marginal inclusion on the 100 Best Corporate Citizens list

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## Abstract

**Research Summary:** Despite the proliferation of lists and rankings that recognize firms for superior performance, empirical studies have been limited in their ability to causally evaluate how inclusion for the marginal firm influences shareholder value. We address this limitation by examining how investors responded to firms that were barely included or excluded from the 100 Best Corporate Citizens list. Contrary to prevailing theoretical expectations, our findings indicate that marginal firms that were included in the ranking experienced negative abnormal returns compared to marginal firms that were excluded. We discuss the theoretical implications of these findings and how they inspire future research questions for scholarship on rankings and status. We also discuss implications for managers that question whether and when being ranked results in financial benefits or liabilities.

**Managerial Summary:** Because being ranked is generally seen as an important strategic objective for companies, managers must carefully consider how much attention and resources to allocate toward this pursuit. Although existing research suggests that being ranked can be beneficial, we have a limited understanding about whether barely making a ranked list is worth the effort. We provide new insights for managers by showing that investors not only respond negatively to marginal inclusion on a ranking but also provide suggestions for how companies might counteract this effect. Specifically, our results suggest that making

consistent, focused investments that are relevant to external stakeholders may buffer organizations from the potential negative effects of marginal inclusion on a ranking. These insights may also inspire managers to reconsider their firms' commitments to being ranked.

#### KEY WORDS

corporate social responsibility, rankings, regression discontinuity, reputation, status

## 1 | INTRODUCTION

Corporate lists that rank and recognize firms for superior performance have proliferated in recent years (Espeland & Sauder, 2016; Fombrun, 2007; Sadowski, Whitaker, & Buckingham, 2010b). Consistent with this trend, an increasing number of CEOs and other top managers are prioritizing efforts to gain inclusion on such lists (Gunther, 2010; Lewis, Walls, & Dowell, 2014; Sadowski, Whitaker, & Buckingham, 2010a). Indeed, a quick glance at annual reports of large public corporations reveals an intense competition among companies for inclusion on prominent rankings such as the 100 Best Places to Work (Dineen & Allen, 2016), the Dow Jones Sustainability Index (DJSI) (Carlos & Lewis, 2018; Hawn, Chatterji, & Mitchell, 2018), or the 100 Best Corporate Citizens (BCC) list (Brammer, Brooks, & Pavelin, 2009). These trends suggest that managers see rankings as a valuable strategic asset and will invest significant resources and attention toward inclusion on these prestigious lists (Espeland & Sauder, 2007).

To date, the literature on rankings has generally assumed that the consequence of inclusion on a ranked list is bimodal in that it produces unequal benefits for ranked firms relative to the unranked (Azoulay, Stuart, & Wang, 2014; Merton, 1968; Podolny, 2010) and that this effect is most salient for firms at the margins. Referred to as “the Matthew effect” (Merton, 1968, p. 3), this notion that rankings produce disproportionate compounding benefits to award recipients compared with essentially equivalent unrecognized peers has largely become taken for granted by both scholars and practitioners (Piezunka, Lee, Haynes, & Bothner, 2018; Reschke, Azoulay, & Stuart, 2018). Despite these prevailing theoretical assumptions, few empirical analyses have been able to causally examine whether marginal inclusion on a ranked list is beneficial. Consequently, companies often seek recognition “without clearly knowing whether they will derive benefits” or “whether recognition has potential downsides” (Dineen & Allen, 2016, p. 92). This raises an important empirical question: Are companies that are on the margin better off being included or excluded from a ranked list?<sup>1</sup>

To address this question, we examine how investors respond to firms' marginal inclusion on a prestigious ranking. Although other studies have examined the average effect of being ranked (Brammer et al., 2009; Edmans, 2011; Klassen & McLaughlin, 1996; Lyon & Shimshack, 2015),

<sup>1</sup>We define a ranking as relative rank ordering of actors against one another that confers award-like status upon recipients (Graffin & Ward, 2010). We also distinguish rankings from ratings or accreditations where organizations are assessed relative to a general standard instead of being compared against other organizations under consideration. Although some rankings may include all firms under consideration, we focus exclusively on those that are finite in length (i.e., lists) and have a defined threshold for inclusion (e.g., Best 100, Top 50, etc.) (Rossman & Schilke, 2014). Consequently, we use the terms “rankings” and “lists” interchangeably.

most have been limited in their ability to draw causal inferences about firms on the margin, primarily because they have not been able to access the full list of firms that were evaluated, but not included in the ranking (Hawn et al., 2018). We address this methodological limitation by examining the effect of inclusion on shareholder value for marginal firms that barely make or barely miss the 100 BCC list, a prominent ranking that evaluates the corporate social performance of public corporations. Because we are able to identify similar firms around the inclusion threshold, we can treat inclusion on the list as akin to a random assignment (Flammer, 2015a), a necessary element of causal inference. This approach of comparing outcomes for marginal firms around a discontinuous threshold is known as regression discontinuity (RD).

Our primary result departs from prevailing theoretical expectations and suggests that inclusion on the 100 BCC list can in some cases decrease shareholder value for firms just above the threshold. On the day of announcement, we find that firms that barely make the list experience a negative 1.3% abnormal return relative to firms that barely miss the cut. To better understand the boundary conditions of this result, we also examine whether individual firm characteristics correlated with a firm's reputation for corporate social responsibility (CSR) may moderate how investors respond to the ranking. These effects, we maintain, help to explain why and when marginal firms might be buffered from negative evaluations by investors for just barely gaining inclusion on the ranking.

Theoretically, we believe that through a process of commensuration, ranking systems may distill and simplify complex information into a new category that groups together similar actors and allows for easier comparison of actors within that category (Bowers, 2015; Espeland & Stevens, 1998; Lee, Hiatt, & Lounsbury, 2017; Shocker, Ben-Akiva, Boccaro, & Nedungadi, 1991). This comparison, we suspect, magnifies the relative weaknesses of bottom-ranked firms by making the performance disparity between bottom- and top-ranked firms more salient. Taken together, we suggest that the categorization and reevaluation of marginally ranked firms may lead to a liability of comparison whereby investors devalue low-ranked members of a high-status category (Leclerc, Hsee, & Nunes, 2005).

In summary, we believe this surprising negative effect of marginal inclusion on a ranking has several important theoretical and practical implications for scholars and managers. First, these results motivate the reconsideration of common theoretical assumptions about the benefits of rankings and status and directly speak to recent work on unearned status gains and losses. Second, our findings help reconcile tension in the literature about the impact of CSR on firm performance by highlighting the need to distinguish between CSR implementation and CSR recognition. Third, our results suggest that marginally included firms may protect themselves from a negative investor response by taking a focused, consistent, and relevant approach to reputational investments. Although we recognize that our primary result is not generalizable to all public firms, the fact that we do find robust support within this context suggests that similar findings may occur with other rankings. Given the proliferation of rankings, we thus call for scholars to perform additional studies to determine whether and when similar liabilities for marginally ranked firms might occur.

## 2 | THIRD-PARTY RECOGNITION AND SHAREHOLDER VALUE

Research on the effects of third-party recognition paints a fairly consistent picture; receiving an award, making it on a prestigious list, or achieving a high rank are assumed to provide unequal benefits to winners, compared to similar others who are not recognized (Elsbach & Kramer, 1996; Rindova, Williamson, Petkova, & Sever, 2005; Rossman & Schilke, 2014; Waguespack & Sorenson,

2011). Empirically, research on the financial effects of receiving awards generally supports these arguments. For example, studies indicate that investors respond positively to news about awards for diversity (Wright, Ferris, Hiller, & Kroll, 1995), quality (Hendricks & Singhal, 1996), and environmental performance (Klassen & McLaughlin, 1996). Other studies report financial benefits for organizations included on prominent lists such as the 100 Best Companies for Working Mothers (Jones & Murrell, 2001), the 100 BCC (Brammer et al., 2009), and the 100 Best Companies to Work For (Edmans, 2011; Fulmer, Gerhart, & Scott, 2003). In general, these studies show that being included on a ranked list can generate significant short- and long-term financial benefits, even after controlling for other observable characteristics.

Given the purported benefits of third-party recognition, it should be of no surprise that companies and their executives increasingly strive to be recognized on such lists. Carlos and Lewis (2018), for example, noted how companies explicitly set goals to be included or remain on the DJSI. Dineen and Allen (2016), likewise, found that companies will spend significant amounts of time and resources and make substantial changes to their employee benefits in order to compete for a spot on a list such as Fortune's 100 Best Companies to Work For. Highlighting this motivation to be recognized, one critic commented that companies "put a lot of time into checking their data" and "do their best to hit the right data points" to increase their likelihood of inclusion (Gunther, 2010). Because of the importance that executives appear to place on such achievements, it is important to understand empirically whether inclusion on a ranked list actually impacts financial performance.

Although companies are attracted to rankings for their obvious marketing appeal, few empirical investigations have convincingly addressed whether marginal inclusion on a prestigious list is actually beneficial (Dineen & Allen, 2016). To date, the primary research design employed to analyze the financial benefits of list inclusion has been an event study. While such studies can provide valuable information about the average effect of list inclusion, they typically do not evaluate the abnormal returns of similar companies that do not make the cut (Hawn et al., 2018), often because the information is proprietary or not available to the public. By ignoring the unranked or unlisted firms, researchers thus limit their ability to empirically examine whether a firm that barely makes a list is actually better off than a firm that is barely excluded. Indeed, such knowledge could be extremely valuable for managers who wonder if their firm's inclusion on a list will derive any benefits and question whether their firm should enter, reenter, or exit these competitions (Dineen & Allen, 2016).

### 3 | DATA

To examine this empirical question, we study investor response to marginal inclusion on the 100 BCC list. Established by the magazine *Business Ethics* in the year 2000, the 100 BCC list was the first prominent ranking of corporate citizenship for large public corporations. Certainly awards, prizes, and other forms of recognition were given for superior corporate citizenship prior to 2000, but we found no evidence of a systematic evaluation and subsequent ranking of similar scale prior to the 100 BCC list.<sup>2</sup>

The stated objective of publicizing the 100 BCC list was to "move corporations toward ever-better practices" by "pushing the envelope on what represents good citizenship" (Waddock, Graves, & Kelly, 2000, p. 17). Implicit in this statement is an assumption that being included on the

<sup>2</sup>Other prominent lists in existence included the Domini Social 400 Index, a stock index constructed in 1990 by KLD to track the costs and benefits of applying social criteria to investment (Godfrey, 2011), and the DJSI, established in 1999. Unlike the 100 BCC, the DS 400 and the DJSI were stock indices, not rankings. Consequently, an investor would only know whether a firm was included on the index, but would not know the firm's relative ranking within the index (Graffin & Ward, 2010).

list can generate substantial recognition and notoriety which can consequently spur imitation and improvement among the excluded firms (Fombrun, 1996; Lewis, 2018).

Although the 100 BCC list has been published annually since the year 2000, we focus our analysis on the initial year the list was published for three reasons. First, studying the emergence of a new ranking increases the likelihood of observing investors' reactions to novel information (Brammer et al., 2009), an essential assumption of an event study (McWilliams & Siegel, 1997). Second, once the criteria for making the list is known, motivated firms may be able to game the system (Gunther, 2010) which could invalidate our assumption of quasi-randomized treatment (Lee & Lemieux, 2010), also an essential component of our research design. Focusing on the inaugural year thus reduces the chance that self-selection could bias our results. Third, using the initial year helps to isolate the effect of recognition bestowed by a ranking from other information about firm performance that may have been previously available.

The inaugural 100 BCC ranking was constructed using social ratings data from the Kinder, Lydenberg, and Domini (KLD) Socrates database. At that time, KLD's evaluation universe included members of the S&P 500 and Domini 400 indices. Eligible firms were evaluated based on relations with communities, customers, employees, and shareholders from 1996 to 1998. An overall score was computed by averaging each category over the 3-year period in order to smooth year-to-year fluctuations. We describe the rankings process and display a snapshot of the initial 100 BCC list and breakdown by industry in Tables S1 and S2 in Data S1.

KLD evaluated 714 unique firms from 1996 to 1998. Of these 714 firms, 166 were not considered eligible because they had been evaluated by KLD less than the required 3 years. An additional seven firms were dropped from consideration because of recent events that would contradict the signal of superior corporate citizenship (Waddock et al., 2000). These removals left 541 firms that were evaluated and ranked by *Business Ethics* magazine. Of these remaining ranked firms, 28 had their stocks delisted prior to the announcement date largely because they had been acquired. Our final sample consists of these 513 firms that were (a) ranked by *Business Ethics* and (b) had stocks that were actively being traded on the announcement day.

Unlike prior research that has examined investor response to CSR rankings and lists (Brammer et al., 2009; Edmans, 2011; Fulmer et al., 2003; Hawn et al., 2018; Lyon & Shimshack, 2015), our identification strategy relies on understanding the social performance of firms that were not only included on the 100 BCC list, but also those that fell just outside of the top 100. Although the total scores used to rank firms were not publicly available for firms that were excluded from the 100 BCC list, we were able to obtain these scores directly from Waddock et al. (2000), the team of researchers who had performed the ranking analysis for the magazine. Obtaining these data was necessary to understand which firms were similar to 100 BCC members but barely missed the cutoff.

## 4 | METHODS

To evaluate the investor response to CSR recognition, we adapt an empirically rigorous research design known as RD to the analysis of stock returns in an event study (Cuñat, Gine, & Guadalupe, 2012; Flammer, 2015a). RD is a research design that is noted for its ability to produce reliable estimates of a causal relationship (Calonico, Cattaneo, Farrell, & Titiunik, 2017) and is "often seen as the sharpest tool of causal inference since it approximates very closely the ideal setting of a randomized control experiment" (Flammer & Bansal, 2017, p. 1828).

## 4.1 | Regression discontinuity in the 100 BCC ranking

The primary objective of this study is to estimate the relative effect of 100 BCC inclusion on shareholder value on the announcement day for firm  $i$ . Each firm in the sample is rated using the KLD data and then ranked according to their overall score  $S_i$ . The overall score was computed by standardizing and adjusting the raw scores obtained from *Business Ethics* so that scores  $\geq 0$  indicate 100 BCC inclusion. By definition, firms that are ranked 1 through 100 are included on the 100 BCC list denoted by the indicator variable  $BCC_i = 1(S_i \geq S^*)$ , where  $S^*$  is threshold for inclusion.

In an ideal world, the causal effect of  $BCC_i$  would be estimated by randomly assigning firms to be included (i.e. the treatment condition) or excluded (i.e. the control condition) from the 100 BCC list. We use a RD design to approximate this idealized experiment. Central to any RD design is an assumption that firms just above or below a threshold are essentially the same. In this context, that threshold for treatment is inclusion on the 100 BCC list. Assuming that firms that barely make the list (e.g., a ranking of 100) are similar to firms that were barely excluded (e.g., a ranking of 101), marginal inclusion on the 100 BCC is thus akin to random assignment and can be used to test the causal impact of being recognized for CSR on subsequent shareholder value.

We first employ a nonparametric method that analyzes the difference in means between included and excluded firms in small bandwidths around the inclusion threshold. Following Flammer (2015a), we then use parametric estimation by employing the following RD specification to evaluate the causal effect of 100 BCC inclusion:

$$y_i = \beta \times BCC_i + P_l(S_i, \gamma_l) + P_r(S_i, \gamma_r) + \varepsilon_i,$$

where  $y_i$  is outcome variable of firm  $i$ ,  $\beta$  is the treatment effect of 100 BCC inclusion,  $P_l(S_i, \gamma_l)$  and  $P_r(S_i, \gamma_r)$  are first-order polynomials on the left and right sides of the inclusion threshold used to control for differences among nonclose firms.<sup>3</sup>

## 4.2 | Event study of the 100 BCC ranking

We quantify the impact of marginal inclusion on firm value using an event study, a method developed to measure the effect of an unanticipated event on stock prices (McWilliams & Siegel, 1997). Like all event studies, we assume that financial markets are efficient which implies that stock prices incorporate all relevant information that is available to the market. If the market is efficient, novel information that was previously unanticipated should generate an abnormal return. We also assume that investors had access to the ranking once published and incorporated the information accordingly.

Knowledge of the initial 100 BCC ranking was disseminated through a press release issued by *Business Ethics* magazine on PR Newswire on March 27, 2000 (Klusmann, 2000). Only the companies that made the list were published. The remaining firms that were evaluated but not included were therefore unknown to the public. We searched the Factiva and Lexis-Nexis databases and found no evidence that the ranking was introduced prior to the announcement date. This absence of evidence suggests that it was very unlikely that investors anticipated the initial publication and that the ranking therefore contained novel information.

<sup>3</sup>In Data S1, we provide a brief description about the differences between nonparametric and parametric estimation. We also demonstrate that our results in parametric estimation are robust to alternative specifications using second- and third-order polynomials (Data S1, Table S3).

Because the 100 BCC ranking was determined using data from KLD that was previously available to investors, one could still question whether the ranking actually provided new information. While we acknowledge this concern, we also note that (a) prior research has shown that investors will respond to rankings as novel information even if the underlying data used to construct the ranking were already available (Lyon & Shimshack, 2015), (b) some investors were likely uninformed given that access to the underlying KLD data was not free (Brammer et al., 2009; Godfrey, 2011), and (c) even previously informed investors can revise their expectations about the future prospects of the company if they think that the information will be novel to other stakeholders (consumers, employees, etc.) (Brammer et al., 2009; Lyon & Shimshack, 2015). Given these factors, we believe that there is a strong likelihood that many investors would perceive the initial 100 BCC list as novel and respond accordingly.

## 5 | VARIABLE DEFINITIONS AND SAMPLE CHARACTERISTICS

### 5.1 | Abnormal return

We evaluate investor response to inclusion on the 100 BCC list by computing the abnormal return on the announcement day (Flammer, 2015a). Using an abnormal return as the primary dependent variable of interest is appealing because it provides a direct estimate of the effect that 100 BCC inclusion might have on firm value. Following prior research (Godfrey, Merrill, & Hansen, 2009; Madsen & Rodgers, 2015; McWilliams & Siegel, 1997), we collect stock return data from the Center for Research in Security Prices (CRSP) database. We compute abnormal returns using the market model which calculates expected returns by regressing a firm's daily stock return against a market portfolio over a period of time prior to the announcement day. As a robustness test, we also compute abnormal returns using the market-adjusted model and obtain substantively similar results. Consistent with prior work, we used a 255-day estimation window ending 46 days before the event and the CRSP value-weighted market portfolio.

### 5.2 | Summary statistics

Table 1 provides summary statistics for the abnormal return on the announcement day as well as other relevant firm characteristics commonly used to assess similarity between firms (Flammer, 2015a, 2015b). We construct an overall measure of corporate citizenship (*KLD index*) by summing a firm's strengths and concerns along the following dimensions: customers, communities, the natural environment, minorities, and employees (Waddock et al., 2000). *Total assets* are the book value of assets. *Market value* is the number of shares outstanding multiplied by the stock price at the end of the fiscal year prior to announcement day. *Return on Assets* (ROA) is measured as the ratio of net income to book value of assets. *Cash* is the ratio of cash and short-term investments to total assets. *Leverage* is the ratio of debt in current liabilities and long-term debt to total assets. *Institutional Ownership* is the proportion of shares owned by institutional investors. *Analyst Coverage* is the number of unique analyst estimates in the Institutional Brokers' Estimate System (I/B/E/S) database in the prior year. Domini Social 400 (DS 400) Index and DJSI are indicator variables equal to 1 if the firm was a member of the DS 400 Index or the DJSI, respectively, and 0 otherwise. Finally, we use four-digit Standard Industrial Classification codes to identify firms whose customers are primarily other

**TABLE 1** Summary statistics

Variable	N	Mean	Median	SD	10th percentile	90th percentile
Abnormal return on announcement day	513	-0.003	-0.005	0.029	-0.034	0.026
KLD index	513	0.893	1	2.616	-2	4
Total assets (\$ million)	513	18,698	5,008	50,044	590	37,156
Market value (\$ million)	513	17,471	4,689	43,588	507	39,024
Return on Assets	513	0.059	0.050	0.049	0.006	0.131
Cash	513	0.072	0.033	0.087	0.006	0.209
Leverage	513	0.264	0.274	0.145	0.044	0.462
Institutional ownership	513	0.609	0.629	0.141	0.405	0.784
Analyst coverage	513	20.856	20	12.169	6	36
DS 400	513	0.626	1	0.484	0	1
DJSI	513	0.072	0	0.259	0	0
B2B industry	513	0.708	1	0.455	0	1

*Note:* Abnormal returns on the announcement day are computed using the market model. Kinder, Lydenberg, and Domini (KLD) index is measured in the calendar year prior to the announcement date. All financial variables are obtained from Compustat and computed in the fiscal year that ends prior to the announcement day. Total assets are the book value of total assets (AT). Market value is the number of shares outstanding (CSHO) multiplied by the stock price at end of the fiscal year prior to announcement day (PRCC\_F). ROA is measured as the ratio of net income (NI) to book value of assets (AT). Cash is the ratio of cash and short-term investments (CHE) to total assets (AT). Leverage is the ratio of debt in current liabilities (DLC) and long-term debt (DLTT) to total assets (AT). Institutional ownership is the proportion of shares owned by institutional investors from the Thomson Reuters Institutional Managers Holdings database in the prior year. Analyst coverage is the number of unique analyst estimates in the I/B/E/S database in the prior year. DS 400 and DJSI are indicator variables equal to 1 if the firm was a member of the Domini Social 400 Index or the Dow Jones Sustainability Index respectively, and 0 otherwise. Business-to-business (B2B) industry is an indicator variable equal to 1 if the company operates in a B2B industry and 0 otherwise. All ratios are winsorized at the 5th and 95th percentiles of their empirical distribution.

businesses (Srinivasan, Lilien, & Sridhar, 2011). Specifically, we create *B2B industry*, an indicator variable equal to 1 if the company operates in a B2B industry and 0 otherwise.

## 6 | TESTS FOR QUASI-RANDOMIZED ASSIGNMENT

To test for quasi-random assignment, we evaluate (a) the continuity of BCC scores around the inclusion threshold and (b) the similarity of firms immediately in the treatment and control groups based on ex-ante characteristics.

### 6.1 | Continuity in the distribution of BCC scores

We first examine whether the distribution of the overall score  $S_i$  is continuous around the inclusion threshold. Discontinuities in the overall score around the threshold likely provide evidence of ratings manipulation which call into question the assumption of quasi-random assignment.

As shown in Figure S1 in Data S1, the distribution of BCC scores appears normal and continuous around the inclusion threshold. We evaluate this condition more formally using the *rddensity* command, a STATA command introduced by Cattaneo, Jansson, and Ma (2018) that tests for evidence of manipulation (i.e., discontinuity in the BCC score at the inclusion threshold). As can be seen in

Figure S2 in Data S1, we find no evidence to reject the null of a continuous distribution ( $p$  value = .743), thus supporting our assumption of a quasi-randomized treatment.

## 6.2 | Preexisting differences

We also examine whether firms just above and below the inclusion threshold are similar on relevant ex-ante characteristics. We expect to find no significant differences among these firms if inclusion near the cutoff is indeed as good as random.

We test for significant differences among marginal firms using all of the aforementioned variables. We display the results of our tests in Table 2. To determine the optimal bin width for analysis, we used the STATA command *rdrobust* and its companion command *rdbwselect* (Calonico et al., 2017; Calonico, Cattaneo, & Titiunik, 2014). We found the optimal bin width to be a window of  $\pm 20$  ranks. Firms that fall within this range are what we consider “marginal” firms. We report an additional window of  $\pm 30$  ranks to demonstrate how bias can increase with increasing distance from the threshold. As shown in Column 1, we find that firms that are included on the 100 BCC list differ significantly from companies that are excluded as one would expect. In Column 2, we see that most of these differences become insignificant at a distance of 30 ranks, with the exception of membership on the DS 400 index. In Column 3, we narrow the threshold to a distance of 20 ranks and discover no significant differences among marginally included or excluded firms. These results are consistent with the full-sample RD specification in Column 4 that controls for distance from the threshold. As supporting evidence, we also plot each covariate against the overall BCC score and find no indication of significant discontinuities at threshold as shown in Figure S3 of Data S1. Jointly, these results are consistent with our assumption that inclusion is approximately random and thus provide further support for our identification strategy.

## 7 | RESULTS

### 7.1 | Main results

#### 7.1.1 | Graphical analysis

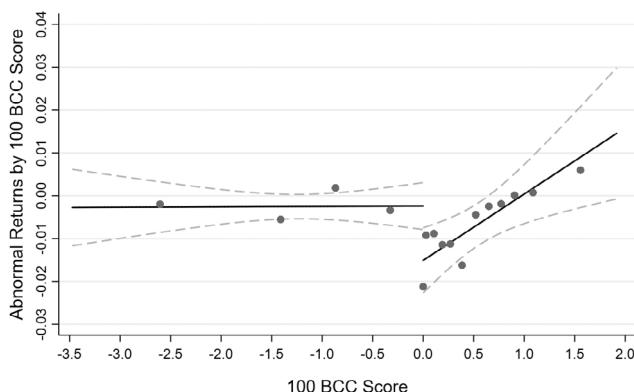
To measure the impact of 100 BCC inclusion on shareholder value, we examine the stock market reaction on the day that the rankings were announced. Figure 1 provides a visualization of this impact. Specifically, the figure plots the abnormal returns against the overall BCC score. Firms with positive scores were included on the list while with negative scores were excluded. Each dot represents the average abnormal return within an optimized bin width on each side of the threshold. The solid lines plot predicted values of abnormal returns from first-order polynomials in the 100 BCC score to the left and right of the inclusion threshold. The light gray dashed lines represented 95% confidence intervals.

Looking at the predicted values to the left of the threshold, we see that excluded firms do not appear to garner any significant investor response as one would expect given that on average, no news is being publicized about these firms that would lead to a significant abnormal return. To the right of the threshold, however, we see a positively sloping line suggesting that returns, relatively speaking, increase with higher 100 BCC scores and thus higher rank. This result is entirely consistent with recent research that examines the relationship between rank and shareholder value (Lyon & Shimshack, 2015) and suggests that many investors found the ranking to contain novel information, despite being based on data that were previously available to the public. The most interesting aspect of the graph, however, is the discontinuous drop at the inclusion threshold. Indeed, this drop suggests that firms that just barely make the list experience a decrease in firm value compared to firms that just miss the cutoff.

**TABLE 2** Preexisting differences as a function of the 100 BCC score

	<b>Before announcement (<math>t - 1</math>)</b>			
	<b>Difference in means (full sample)</b>	<b>Difference in means [-30, +30]</b>	<b>Difference in means [-20, +20]</b>	<b>RD estimate (full model)</b>
	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>	<b>(4)</b>
Abnormal return	-0.002 0.589	-0.012 0.463	-0.005 0.669	-0.005 0.393
KLD index	3.835 0.000	0.167 0.714	-0.250 0.651	0.357 0.292
Total assets (log)	0.478 0.013	-0.128 0.771	0.485 0.354	-0.048 0.905
Market value (log)	0.984 0.000	-0.416 0.341	-0.400 0.456	-0.593 0.123
Return on assets	0.021 0.000	-0.007 0.601	-0.019 0.225	-0.010 0.260
Cash	0.027 0.012	0.019 0.442	0.027 0.403	-0.005 0.757
Leverage	-0.034 0.056	-0.004 0.924	0.038 0.402	0.023 0.412
Institutional ownership (%)	-0.028 0.074	-0.008 0.830	-0.059 0.194	-0.003 0.907
Analyst coverage	5.806 0.000	-4.167 0.193	-5.650 0.131	-2.729 0.336
DS 400	0.316 0.000	0.200 0.069	0.100 0.442	0.130 0.179
DJSI	0.084 0.022	-0.033 0.694	-0.100 0.304	-0.010 0.855
B2B industry	-0.084 0.113	-0.100 0.399	-0.100 0.503	0.031 0.786

Notes: This table tests whether 100 BCC inclusion is systematically related to firm characteristics prior to the announcement day. All variables are defined in Table 1. Column 1 reports the difference in means among all firms in the sample; Column 2 reports the difference-in-means among all firms that lie within 30 ranks of the inclusion threshold; Column 3 reports the difference-in-means among all firms that lie within 20 ranks of the inclusion threshold; Column 4 reports the difference at the inclusion threshold by estimating the global parametric RD specification including first- or second-order polynomials on both sides of the threshold. *p* Values are shown below each estimate.



**FIGURE 1** Abnormal returns on announcement day ( $t = 0$ ). The vertical axis indicates the abnormal returns on the day of the announcement. Abnormal returns are computed using the market model. The horizontal axis indicates the 100 BCC score with 0 indicating the inclusion threshold. Firms with positive scores were included on the list while with negative scores were excluded. Each dot in the figure represents the average abnormal return within an optimized bin width calculated for each side of the threshold using the STATA command, *rdplot* with quantile spacing (Calonico et al., 2017; Calonico, Cattaneo, & Titiunik, 2015). The solid lines plot predicted values of abnormal returns form first-order polynomials in 100 BCC score estimated separately to the left and right of the inclusion threshold. The light gray dashed lines represented 95% confidence intervals. BCC, Best Corporate Citizens

### 7.1.2 | Regression analysis

The graphical analysis in Figure 1 suggests that inclusion on the 100 BCC list leads to a decrease in shareholder value for marginal firms. We provide a formal test of this claim in Table 3 using both nonparametric and parametric estimation.

We first report our results using nonparametric estimation which computes estimates of the difference in abnormal returns between firms that are included and excluded for increasingly small intervals around the inclusion threshold. In Column 1 of Table 3, we examine the full sample of 513 firms. Consistent with prior work, we find a negative but insignificant effect for 100 BCC inclusion (Brammer et al., 2009). Column 2 restricts the sample to nonclose firms, that is, firms that were more than 30 ranks from the inclusion threshold. Again, the results show no discernable difference in abnormal returns between the included and excluded firms, likely because this subsample is largely composed of excluded firms for whom one would not expect a market response.

In Columns 3 and 4, we restrict the sample to increasingly smaller bin widths around the inclusion threshold. We use the STATA command *rdrobust* to compute nonparametric estimates with triangular kernels that put greater weight on firms closer to the threshold (Calonico et al., 2014; Calonico et al., 2017; Tian, Smith, & King, 2019). For firms within 30 ranks of the inclusion threshold, we find that the difference in abnormal returns is  $-2.2\%$  ( $p = .007$ ). For firms within 20 ranks, the optimal bin width, we see the difference between included and excluded firm increases in magnitude to  $-2.7\%$  ( $p = .007$ ). Overall, the results displayed in Columns 3 and 4 indicate that firms who barely make the 100 BCC list experience a significant decrease in shareholder value compared to firms that barely miss the cut.

We also report our results using parametric estimates in Table 3. In Column 5, we run our primary RD regression that controls for the distance from the inclusion threshold using first-order polynomials. Unlike the nonparametric estimates listed in Columns 3 and 4, this model incorporates the full sample of firms ( $N = 513$ ) and thus provides a more efficient and precise estimate of the causal effect

**TABLE 3** Results

<b>Panel A: Abnormal returns around the inclusion threshold</b>						
	Non-parametric estimation			Parametric estimation		
	All firms	Nonmarginal	[−30, +30]	[−20, +20]	Full model	
	(1)	(2)	(3)	(4)	(5)	
100 BCC member	−0.001 (0.001) 0.169	−0.001 (0.004) 0.742	−0.022 (0.008) 0.007	−0.027 (0.010) 0.007	−0.013 (0.005) 0.005	
<i>R</i> <sup>2</sup>	0.003	0.000	0.028	0.139		0.016
Observations	513	453	60	40		513

<b>Panel B: moderating effects of the specificity, consistency, and materiality of CSR investments</b>						
	Specificity of CSR		Consistency of CSR		Relevancy of CSR	
	No	Yes	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)
100 BCC member	−0.017 (0.005) 0.002	−0.007 (0.007) 0.340	−0.019 (0.007) 0.005	−0.006 (0.007) 0.363	−0.017 (0.006) 0.003	−0.002 (0.008) 0.831
<i>F</i> tests: Differences between columns		4.180 0.041		4.580 0.033		5.060 0.025
<i>R</i> <sup>2</sup>	0.038	0.004	0.032	0.005	0.021	0.034
Observations	265	248	219	294	363	150

*Note:* In all regressions, we report coefficients followed by the *SE* and respective *p* values. Panel A presents regressions of the abnormal returns on the day of the announcement of the 100 BCC list on the 100 BCC inclusion dummy, that is, a dummy variable that equals 1 if the firm is recognized as a member of the list and 0 otherwise. In Panel B, we explore moderating effects of a prior reputation by splitting the full sample into various subsamples. We test for significant differences between subsamples using an *F* test in a fully interacted model (i.e., the moderating variable interacted with each covariate from the baseline parametric model) as shown in Data S1, Tables S7–S9.

of marginal inclusion on abnormal returns. Similar to the nonparametric results in Columns 3 and 4, we find a statistically significant −1.3% difference between included and excluded firms, even after controlling for distance from the inclusion threshold. Although this magnitude may seem small, we found it to be consistent with other event studies that examine investor responses to CSR-related information (Flammer, 2015a; Flammer & Bansal, 2017). Taken together, these results provide strong evidence that inclusion on the 100 BCC list leads to a decrease in firm value for marginal firms. In Data S1, we demonstrate that this finding is robust to a number of additional sensitivity tests.

## 7.2 | Moderating effects: CSR specificity, consistency, and relevancy

Our initial results suggest that marginal inclusion on the 100 BCC list can be detrimental to firm value. We suspect, however, that investor reactions to a ranked list may also be moderated by other

salient firm characteristics that are not captured by the ranking, namely, a firm's prior reputation (Kim & King, 2014). Prior research, for example, suggests that firms that engage in predictable patterns of organizational outcomes and behaviors that are relevant to specific audience interests become "known for something" (Jensen, Kim, & Kim, 2012; Lange, Lee, & Dai, 2011) which helps to establish a domain-specific reputation among key stakeholders (Carlos & Lewis, 2018; McDonnell & King, 2018). Such a reputation could enhance firm value by leading to increased revenues (Lev, Petrovits, & Radhakrishnan, 2010) or reduced capital costs (Cheng, Ioannou, & Serafeim, 2014) or a preservation of firm's value when experiencing a negative event (Flammer, 2013; Godfrey et al., 2009; Luo, Kaul, & Seo, 2018), all outcomes valued by investors (Fombrun, 1996; Pfarrer, Pollock, & Rindova, 2010; Rhee & Haunschild, 2006). Because investors may be more willing to accept a firm's CSR investments if the connection between the investment and some valued outcome such as reputation is more salient, we suspect that marginally included firms with strong reputations for CSR may be less likely to be devalued when barely making the 100 BCC list.

Accordingly, we examine three antecedents to a socially responsible reputation that may explain investors' varying responses, namely, the specificity, consistency, and relevance of a firm's CSR investments (Lange et al., 2011). First, we expect that firms that make concentrated or focused investments in specific dimensions of CSR are more likely to generate a domain-specific reputation for CSR relative to other firms that engage in more general or diverse behaviors (Carlos & Lewis, 2018; Jensen et al., 2012). Second, we anticipate that an established history of consistent socially responsible behavior is more likely to be perceived as genuine by stakeholders which may lead to higher levels of reputational capital among investors (Barnett, 2007; Godfrey, 2005). Third, we predict that engaging in activities that are relevant and viewed as material by key stakeholders can help to strengthen a company's reputation among that audience (Khan, Serafeim, & Yoon, 2016; Rindova & Martins, 2012). Collectively, we suspect that these three factors represent antecedents of a socially responsible reputation that are likely to attenuate the negative investor response to marginally included firms.

We examine each of these moderating effects in Panel B of Table 3. We first evaluate the moderating impact of investing in specific or focused dimensions of CSR. To operationalize this variable, we compute the standard deviation (*SD*) of ratings among the four stakeholder groups evaluated by the BCC list: community, employees, customers, and shareholders. Firms that have concentrated their investments in one particular domain are likely to have a higher *SD* than firms whose investments were spread more equally (see Data S1 for an example). We then split the full sample into two based on a median split of the *SD* score for 100 BCC firms. As displayed in Columns 1 and 2, the negative effect of inclusion appears to affect only firms whose CSR investments are less specific (i.e., more general). Marginal firms that have focused CSR investments appear to be less likely to experience decreases in firm value, perhaps because they develop domain-specific reputations (Carlos & Lewis, 2018; McDonnell & King, 2018), which may be more salient to investors.

We then examine the impact of a prior history of investments in CSR. To evaluate this history, we compute the change in the KLD index between 1999 ( $t - 1$ ) and 1996 ( $t - 4$ ). We then classify firms into two groups: (a) those whose change in the KLD index was  $>0$  (No History of CSR) and (b) those whose change in the KLD index was  $\leq 0$  (history of CSR). Firms with significantly positive differences are not likely to have a well-established history of CSR investments (see Data S1 for an example). Based on the results from Columns 3 and 4, it appears that the negative effect of inclusion for marginal firms does indeed only apply to those firms who lack a strong historical record of CSR investments. Conversely, marginal firms that do have a relatively strong history of CSR investments appear to be somewhat protected from the decrease in firm value, likely because consistent

investments over time are more likely to be perceived as credible (Barnett, 2007; Barnett & Salomon, 2012) and thus generate more reputational capital (Godfrey, 2005).

Finally, we investigate the effect of making relevant or material investments in CSR for marginal firms. Following recent work by Khan et al. (2016), we utilize guidance from the Sustainability Accounting Standards Board to identify material sustainability issues for companies within the same industry and then map these material issues to the KLD ratings (see Data S1 for more details). We then compute the ratio of material strengths enacted by the firm to the number of material strengths assigned to the firm's industry. Firms with positive proportions (i.e., those that are making investments in material issues) are deemed as having a higher level of material CSR investment while firms with a proportion equal to zero are deemed as making no material CSR investment. As shown in Columns 5 and 6, a negative investor response to marginal inclusion seems to apply to firms that do not invest in material CSR initiatives. On the other hand, marginal firms that make material CSR seem to be buffered from a decrease in firm value, arguably because targeting initiatives that match investors' interests helps to build their reputation among shareholders (Khan et al., 2016; Lange et al., 2011).

### 7.3 | External validity

Although the RD design employed in this study is advantageous for identifying the causal effect of being marginally included on the 100 BCC list, our analysis may be limited in generalizability given that the effect is identified by a small subset of firms around the inclusion threshold that, while being similar to each other, may be very different from firms farther away from the threshold. Likewise, the companies in the 100 BCC evaluation universe may not be representative of the broader population of public firms from which they are drawn. To address these concerns, we evaluate (a) whether companies around the inclusion threshold differ systematically from companies that are farther away and (b) whether the sample of BCC evaluated firms differs significantly from the broader population of public corporations. We display the results of these comparisons in Table S10 of Data S1.

We find that firms near the inclusion threshold have significantly higher levels of corporate citizenship as measured by the KLD index, are more profitable, and are also more likely to be members of the DS 400 index. This result, while interesting, is not surprising given that (a) the 100 BCC ranking, by construction, selects approximately the top 20% of firms in terms of corporate citizenship and shareholder return and (b) the DS 400 is a compilation of socially responsible companies as determined by KLD (Godfrey, 2011; Waddock et al., 2000). Taken together, these comparisons suggest that marginal firms near the inclusion threshold may not necessarily be representative of the broader population from which they were selected (i.e., S&P 500 firms). They do, however, suggest that barely making a list may be detrimental to shareholder value, but only for companies that appear to be actively investing in the dimension of performance that is being ranked. We also find firms evaluated for inclusion on the 100 BCC to be significantly different from the broader population of public firms. Indeed, firms in the 100 BCC universe are much larger, more profitable, and more likely to be followed by analysts and institutional investors. Although these differences do not bias our estimate of the treatment effect of marginal inclusion, they do suggest that our findings may not apply to the typical U.S. public firm.

## 8 | DISCUSSION

Firms face important strategic decisions about how to manage their resources and attention in pursuing rankings. Although prior theory generally assumes that rankings bestow disproportionate benefits

to ranked actors (Merton, 1968), empirical support for this causal relationship is limited. Through our access to a unique data set and implementation of a novel empirical design, we found surprising empirical evidence that departs from prevailing theoretical expectations.

## 8.1 | Empirical findings

Our primary results indicate that firms that were marginally included on the 100 BCC's list experienced a 1.3% decrease in firm value compared to firms that were marginally excluded. We also sought to understand the materiality of these results by interpreting the estimated decrease in absolute terms. Given that the median marginal firm (those within 20 ranks of the inclusion threshold) had a market value that ranged from \$805 million (10th percentile) to \$32 billion (90th percentile), the estimated effect of marginal inclusion of -1.3% would translate to an expected decrease ranging from \$10 to \$416 million in market value, with a median estimate of \$83 million.

From an empirical perspective, our study highlights a potential opportunity for scholars who study investor responses to rankings and lists. Although many academics have sought to understand the financial consequences of being included on prestigious lists (Brammer et al., 2009; Edmans, 2011; Fulmer et al., 2003; Jones & Murrell, 2001; Lyon & Shimshack, 2015), most of these studies "do not compare abnormal returns of similar firms that are not on the index" (Hawn et al., 2018, p. 950) and are thus limited in their ability to estimate the causal consequences of inclusion (Flammer, 2015a). Recent studies have sought to address this limitation by using matching methods to construct a comparable group of control firms (Durand, Paugam, & Stolowy, 2019; Hawn et al., 2018) but even these studies cannot estimate a true causal relationship because they lack knowledge about which firms apply but are rejected.

We address this limitation by collecting the underlying performance data for all firms that were evaluated for the 100 BCC, even those that fell outside of the top 100 ranked firms. Indeed, this unique access to data is what enabled us to estimate the causal relationship of 100 BCC inclusion. While access to proprietary data will always be a challenge, many ranking agencies (e.g., *Business Ethics*) base their evaluations on public information that is already available (e.g., KLD). Often all that is needed to reconstruct the ranking for the full population of evaluated firms is a more in-depth explanation of the third-party's ranking methodology. We thus encourage scholars who study rankings and lists, whenever possible, to seek out information on the ranked but unpublished organizations. Doing so will allow future research to employ rigorous research designs to estimate causal relationships (Flammer, 2015a; Hawn et al., 2018; Lev et al., 2010), thus expanding our empirical knowledge about the consequences of being ranked.

## 8.2 | Theoretical implications

### 8.2.1 | Rankings and status

Our empirical findings represent a surprising departure from prior theoretical expectations dating back to Merton's conceptualization of the 41st chair. Using the example of the French Academy and its limit of only recognizing the top 40 scientists in the field, Merton identified the 41st chair as "an artifact of having a fixed number of places available at the summit of recognition" (1968, p. 2). As he explains, despite generally indiscernible differences in talent and quality of research, scientists that occupy the 40th chair receive incommensurate career-propelling benefits compared with peers who were not recognized by the Academy and thus relegated to the 41st chair.

Although our focus was on the empirical phenomenon, our findings point to an important theoretical dimension not considered by Merton's 41st chair analogy, namely, the potential for a liability of comparison that may arise from gaining inclusion in a prominent category. By grouping together ranked firms into a comparison set, rankings may restructure how firms are evaluated by focusing audience attention to the comparison of a given firm with the other ranked firms on the list (Bowers, 2015). As our results suggest, these categorical comparisons may be detrimental for lower ranked firms by enhancing the salience of the performance disparity with higher ranked firms. We see this counterintuitive finding as providing further evidence of the limits and potential downsides of status (Azoulay et al., 2014; Graffin, Bundy, Porac, Wade, & Quinn, 2013; Reschke et al., 2018) and as further motivation to study the implications of competition near status boundaries (Otner, 2018; Piezunka et al., 2018).

Our results also build directly on recent research that examines how status gains and losses can occur as a result of structural changes in categories. When status shifts are independent of substantive changes in firm performance, they are referred to as "unearned" (Bowers & Prato, 2018; Neeley & Dumas, 2016). Although prior research has investigated the impacts of these changes on firm outcomes (Bowers & Prato, 2018) and has considered how individuals respond to experiencing unearned status gains and losses (Neeley & Dumas, 2016), our study may help to delineate new boundary conditions regarding how audiences interpret and respond to unearned status gains (George, Dahlander, Graffin, & Sim, 2016). Consistent with prior research, our results indicate that highly ranked firms seem to benefit from structural changes that bestow status. However, unlike prior work, we note that, at the margin, unearned status may represent a liability. Our moderators help unpack this relationship by indicating that firms that had strong CSR reputations were buffered from these liabilities. In other words, firms with a prior reputation for CSR were likely viewed as having "earned" their status gains, compared with those at the margin lacking a reputation for CSR. From these findings, we surmise that at the margins unearned status gains or losses may be more likely to be detected by audiences.

Finally, our results contribute to recent conversations regarding the potential liabilities associated with positive recognition. For example, Kovács and Sharkey (2014) show that the ratings of award-winning books tend to decline after those books receive an esteemed award, primarily because awards broaden the audience for books and in so doing attract more diverse readers with different tastes. Similar to this research, we find potential negative implications of awards in the form of rankings. However, our results highlight a potential alternative mechanism. While Kovács and Sharkey (2014) link negative outcomes to audience expansion, our findings suggest that rankings may create a liability of comparison by emphasizing the relative performance disparity between low- and high-ranked firms.

### 8.2.2 | Strategic CSR

Our study also speaks to the ongoing conversation regarding the strategic value of CSR investments. Using a similar research design to our own, recent work has found a positive causal relationship between CSR and firm value (Flammer, 2015a; Flammer & Bansal, 2017). Although our results may seem to contradict these previous findings, we believe they differ in important ways and are thus complementary, not contradictory.

First, we believe these studies differ in their treatments. For example, Flammer and colleagues examine investor response to the *approval* and *implementation* of CSR-related shareholder proposals (Flammer, 2015a; Flammer & Bansal, 2017). Our study, however, focuses on investor response to

recognition of CSR by a third party. Because we use similar research designs, we suspect that these different findings are likely a result of the treatment itself: implementation versus recognition. Second, we also note that the ratings used to construct the 100 BCC list in the inaugural year (2000) were based on KLD data that were already publicly available (1996–1998) (Brammer et al., 2009). Given this observation, if all investors cared about was the *implementation* of CSR initiatives, it would be unlikely that we would observe any substantive change in firms' stock prices on the day of the 100 BCC announcement given that the performance information underlying the ranking (i.e., the KLD ratings) was already available. The fact that we see any response at all is thus further evidence that the 100 BCC *recognition* changed investors' perceptions about the present value of the firm independent of what had already been approved and *implemented* by the firm (Lyon & Shimshack, 2015).

Taken together, we believe these studies jointly highlight an important conceptual distinction between CSR implementation and CSR recognition, a difference that has often been overlooked in the literature, perhaps due to assumptions that firms that engage in socially responsible initiatives will naturally want to publicize and therefore be recognized for their efforts (Carlos & Lewis, 2018; Delmas & Grant, 2014). While prior work has found shareholders do value the *approval* and *implementation* of CSR initiatives (Flammer, 2015a; Flammer & Bansal, 2017), based on our results, it appears that shareholders may also punish firms that fail to gain a certain level of *recognition* or notoriety for their CSR investments. Given these findings, we thus encourage scholars who study the strategic value of CSR to consider whether their empirical observation of CSR is an implementation event or a recognition event and how and why this distinction might lead differing performance results.

### 8.3 | Strategic implications for managers

Our primary empirical result suggests that firms that were barely included on the 100 BCC list experienced a decrease in firm value when the list was announced. Theoretically, we suspect that these negative effects may stem from a liability of comparison, whereby performance discrepancies between marginally ranked and top-ranked firms are amplified. In the case of a new nonvoluntary ranking, firms may have little control over the set of organizations to which they are compared. However, our results suggest that one viable strategy to buffer firms from the risk of a marginal ranking is to establish a strong reputation by engaging in focused and consistent activities that are relevant to the interests of their primary stakeholders (Lange et al., 2011).

Indeed, our moderating analysis revealed that marginal firms were immune to negative investor perceptions when they were known for something specific (Lange et al., 2011), had pattern or history of consistent investments (Barnett, 2007; Godfrey, 2005), and when they focused on investments that were relevant and impactful (i.e., material) in the eyes of shareholders (Khan et al., 2016; Rindova & Martins, 2012). Taken together, these results provide three key takeaways for managers who desire to compete for a spot on a ranked list but wish to avoid possible liabilities of marginal inclusion. First, building a domain-specific reputation (Carlos & Lewis, 2018; McDonnell & King, 2018) may be a good initial strategy to buffer firms from the potential liability of a low ranking, at least until they acquire sufficient resources and capabilities to broaden the dimensions upon which their reputation is based. Second, given the value of consistency (Barnett & Salomon, 2012; Hawn et al., 2018), managers should ensure that their firm provides sufficient resources to sustain investments in the evaluated activities over time. Third, managers should become attuned to the needs and interests of

different stakeholders (Hiatt & Carlos, 2019; Hiatt, Carlos, & Sine, 2018) and consider whether their investments actually make a material difference.

Managers may also wish to reexamine their firms' commitment to being ranked. While our primary analysis focused on the negative effects of marginal inclusion, our findings do seem to suggest that being ranked near the top of a list could be beneficial, or at least not harmful (see Figure 1). Given these results, firms that desire to compete for a spot on a particular ranking may wish to continue making investments that result in a rank that facilitates favorable comparisons among listed entities. Conversely, firms that have little ability or desire to compete may wish to redirect their investments to avoid being ranked or perhaps elect not to participate in the ranking altogether. After all, firms that are lukewarm in their efforts to manage their ranking may find themselves "stuck in the middle" and subsequently discounted by investors who perceive them to be bearing the costs of pursuing a ranking without enjoying the benefits (Barnett & Salomon, 2006, p. 1119).

## 8.4 | A research agenda: Rankings and the impact of marginal inclusion

Although we are confident in the robustness of our results within the confines of this particular context, we cannot conclusively ascertain whether or when similar liabilities for marginally ranked firms would generalize to other rankings. For example, our study focused on a particular type of ranking (nonvoluntary) with one dimension of performance (corporate citizenship) and a finite number of ranks (100). We also evaluated reactions to marginal inclusion by one audience (investors) at one particular point in time in history (the year 2000 and the first year of the ranking). Given such specificity, one could indeed question whether marginal inclusion on a ranking with differing elements would generate a similar effect.

Fortunately, given the proliferation of rankings in recent years (Fombrun, 2007), we suspect there are a number of promising research contexts in which differing structural elements of a ranking could be examined in order to determine whether this effect generalizes beyond the current context. For example, future studies could examine how marginally ranked universities compare to the unranked on outcomes such as alumni donations, applications, placement, etc. (Elsbach & Kramer, 1996; Espeland & Sauder, 2007). Other promising contexts to examine may include other general firm reputation rankings (Bermiss, Zajac, & King, 2014), the ranking of hospitals (Pope, 2009), books (Kovács & Sharkey, 2014), hotels (Orlikowski & Scott, 2014), restaurants (Luca & Zervas, 2016), and automobiles (Rao, 1994). Consequently, despite limitations in generalizability, we believe this study provides a strong foundation upon which future research can build in order to understand the reach and limits of our primary empirical finding. We explore several of these areas of inquiry below.

### 8.4.1 | Ranking characteristics

First, we suspect that variation in ranking characteristics could impact the generalizability of our results. For example, one particular aspect of the 100 BCC list was that firms did not voluntarily apply for inclusion but were rather evaluated and ranked based on publicly available data. Although other rankings likewise evaluate actors without their approval, it is also common for firms to apply for inclusion on a ranking by filling out extensive questionnaires (Carlos & Lewis, 2018; Dineen & Allen, 2016), particularly if they already excel on the dimension of performance being ranked. Because we find that a prior reputation can attenuate the negative effect of marginal inclusion, these differences in selection raise questions about whether similar findings would extend to rankings that

are based on voluntary applications. One could also consider whether similar findings would apply to other ranked dimensions of performance. CSR, for example, has historically been viewed as subordinate to the core functions of the business. Rankings associated with activities more directly connected to core business operations, such as quality and innovation, could conceivably produce different results. Future research could therefore examine whether a liability of comparison generalizes to other ranked dimensions of performance. Last but not least, the number of available spaces on a ranked-list could impact the prevalence of this negative effect. In this study, that number was 100. Smaller lists (e.g., top 10 list) however, could attenuate the effect as the relative distance between a top- and bottom-ranked firm declines. Given the varying means of solicitation, ranked performance dimensions, and sizes of ranked lists, future research could examine whether each of these factors amplify or attenuate the negative effect of marginal inclusion.

#### 8.4.2 | Audience characteristics

Second, our study focused on investor response to marginal inclusion, but there are indeed many other audiences influenced by rankings. Investors, for instance, primarily consider how information conveyed by rankings is likely to impact future financial returns. Given that all companies would be evaluated on the same dimension, investors may treat individual firms like commodities that only differ in terms of their potential future return. In contrast, individual customers may care more about specific products and brands which could reduce the number of firms within their consideration set and thus decrease the relative distance between top-ranked and marginally ranked actors. Future research could thus examine whether the motives and interests of other key stakeholders (i.e., customers, employees, media, etc.) moderate the negative effect of marginal inclusion.

#### 8.4.3 | Historical attributes

Third, our study evaluated a ranking within a particular time in history, both the history of the ranking itself and the history of the performance dimension being ranked. For example, our analysis of the 100 BCC focused on investor responses during the inaugural year. These findings thus represent responses to a new ranking that represent structural changes in categories that give new meaning to existing performance metrics. Accordingly, we suspect that similar results may be found in situations where the ranking provides novel information to external audiences, either through the creation of a new ranking system or through significant changes to existing ranking systems that recategorize criteria in unanticipated ways. In addition, our analysis was also performed during a time period in which CSR, the dimension of performance being ranked, was still becoming institutionalized (Carlos & Lewis, 2018; Flammer, 2013; Ioannou & Serafeim, 2015; Marquis, Glynn, & Davis, 2007). Consequently, our findings regarding the negative effects of marginal inclusion could be partially explained by lack of knowledge and understanding among investors regarding the economic benefits of CSR investments. For example, one might expect that decreases in firm value could very well be attenuated by increased acceptance of CSR as a legitimate strategic function. While we are unable to test this notion using the same context given that it is historically fixed in time, future research could examine how external audience members respond to rankings over time when the dimension of performance being ranked becomes perceived as more legitimate (Hawn et al., 2018; Ioannou & Serafeim, 2015).

## 9 | CONCLUSIONS

In summary, we believe our study presents important implications for how ratings may impact firm performance. Our core finding is that firms at the margin may actually be penalized rather than rewarded for inclusion on a prestigious ranking. However, we also find that these effects may be attenuated by individual firm characteristics reflected in a firm's reputation within the same domain as the ranking. These results provide provocative new insights into taken for granted assumptions about the value of receiving positive social evaluations such as rankings, certifications, or awards. Given the increasing prevalence of these forms of recognition, we believe additional research in this area is imperative and hope that our findings will inspire future research that helps to bring forth new insights into this important topic.

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## SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of this article.

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