How do sustainability ratings affect firm ESG performance and disclosure?

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ABSTRACT: This study investigates the impact of sustainability rating agencies on the environmental, social, and governance (ESG) performance and disclosure behavior of American firms, specifically focusing on their estimated greenhouse gas emissions and discussion of climate change during earnings conference calls. By leveraging a plausibly exogenous shock in the coverage expansion of Sustainalytics in mid-2016 related to its acquisition by Morningstar, this study uses a difference-in-difference approach to assess the effects of Sustainalytics' coverage expansion on firms that began to receive coverage at that time, referred to as the treatment group. The findings reveal a positive effect of the coverage expansion on reported emissions, but a negative effect on the discussion of climate change during earnings conference calls. Furthermore, the effects differ for firms with above and below-median first ESG ratings. This study contributes to the understanding of how firms' incentives to maintain good and transparent ESG conduct differ based on their ESG costs, and how rating agencies' decisions may result in unintended negative consequences.

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1 Introduction

This paper examines how sustainability ratings affect firms' environmental, social, and governance (ESG) performance and disclosure. In light of the increasingly pressing threat of climate change, the importance of sustainability has been recognized by investors. The United Nations Principle for Responsible Investments (UNPRI) has attracted over 5000 signatories since its launch, including 4103 institutional investors, indicating a growing interest in responsible investment practices; the Net Zero Asset Managers (NZAM), a group dedicated to maintaining net-zero greenhouse gas portfolios by 2050, has 301 signatories currently with $59 trillion AUM as of 2022. Empirical evidence suggests that individual and retail investors also value sustainability in their investment decisions. For instance, Haltzmark and Sussman (2019) document experimental evidence that mutual funds investors' demand for high sustainability funds is much higher than that of low sustainability funds. Li et al. (2023) find that retail investors' trading behavior is influenced by materially relevant ESG news, and that their trading demand can predict future abnormal returns, suggesting some ability to profit from trading on ESG news.

The growth in demand for sustainable investment options and ESG-related information has led to the emergence of ESG research and rating agencies. These agencies, which are frequently associated with larger financial entities, data vendors, or research firms, provide ESG scores for firms or funds they cover, purporting to reflect relevant ESG information and news accurately. ESG ratings have become a widely accepted proxy for corporate social responsibility, both in academia and amongst investors. This is because ESG ratings must consider factors that do not have a clear economic meaning and can be difficult for even professionals to understand. As a result, ESG ratings have made ESG-related information more accessible and comprehensible, even though the rating methodologies often remain opaque "black boxes" to users of the data.

A growing body of literature has begun to view the ESG ratings as separate from actual ESG performance and examine them as distinct agents. Serafeim and Yoon (2022) demonstrate that ESG ratings' ability to predict stock returns diminishes when rating agencies disagree. Similarly, Brandon et al. (2021) examine ESG rating disagreement and find that it is positively correlated with stock returns, suggesting that firms with opaque ESG information may carry a risk premium. Berg et al. (2021) find that when Refinitiv, a widely used ESG rating agency favored by academics, revises historical rating data after methodology adjustments, the revised data shows a correlation with stock returns that is absent in the original data; this finding raises questions not only about the accuracy of ESG ratings but also their motives and integrity. As such, it is crucial to differentiate ESG ratings from actual ESG performance and disclosures. This study seeks to elucidate the relationship between ESG ratings and these measures and contribute to multiple areas of literature.

Extant literature on ESG performance finds various firm and manager characteristics among its determinants. Chen et al. (2020) find that an exogenous increase in institutional holding lead to improvement in portfolio firms' ESG performance. Manner (2010) finds that CEO characteristics such as gender, college major, time at the company, and previous job experiences all have some explanatory power towards the ESG performance of the company. Yuan et al. (2018) find that corporate strategy is important to ESG performance, with results indicating that firms following an innovation-oriented strategy tend to have better ESG performance compared to firms following an efficiency-oriented strategy. What is interesting about this literature is that a plurality of them uses sustainability ratings as the measure of sustainability performance. This study thus aims to add to this literature by separating ESG ratings and sustainability performance, and instead considering it as one of its potential determinants.

Extant literature on ESG disclosure behavior identifies firm size (Hahn and Kühnen 2013, Li et al. 2021), ownership structure (Cormier et al. 2005), and corporate governance structures (Mallin et al. 2013) as determining factors. These factors are all more or less important to non-ESG-related disclosures as well. Research on factors that are uniquely relevant to ESG-related disclosures is comparatively much less extensive. This study thus aims to add to this literature by exploring rating agency coverage as a factor unique to ESG disclosures and the ESG environment at large. If firms wish to maintain a high ESG rating, they may be incentivized to release more ESG information and shape their disclosures in a certain way. Given the largely unregulated and voluntary nature of ESG disclosures in the U.S. currently, identifying the effect of ESG rating agencies on ESG disclosures can have important implications for future regulations.

The final stream of related literature studies the impact of financial intermediaries on firm behavior.One line of research has investigated the role of banks in encouraging firms to improve their ESG performance, with Houston and Shan (2021) finding that banks are more likely to grant loans to firms with similar ESG profiles. Another prominent area of study concerns the impact of analyst coverage on firms, with mixed results for the relationship between analyst coverage and ESG engagement. Jo and Harjoto (2014) report a positive relationship between analyst coverage and ESG engagement, whereas Qian et al. (2019) found the opposite. As ESG rating coverage can be seen as a form of specialized analyst coverage, it is reasonable to expect that such coverage has some effect on firm behavior. This study seeks to contribute to the ongoing discussion of the influence of analysts on ESG and expand the literature into the realm of ESG disclosures.

The relationship between financial intermediaries and firms, as well as the study of ESG disclosure, often face challenges due to endogeneity issues. In the former, external factors may influence both ESG ratings and ESG performance, while in the latter, it is difficult to distinguish ESG disclosure from underlying ESG activities, as noted in Christensen et al. (2021, A). Moreover, mandatory ESG disclosure requirements are currently lacking in the United States, and identifying causality in voluntary disclosure poses further challenges due to the possibility of anticipation, early adoption, or avoidance by firms, as pointed out by Leuz (2018).

To address these issues, this paper leverages an exogenous shock to ESG rating coverage resulting from the partnership and acquisition of Sustainalytics, a prominent ESG rating provider, by Morningstar. This strategy enables us to disentangle the effects of ESG rating coverage on firms' ESG disclosures from any potential confounding factors. Between late 2015 and mid-2016, the number of firms covered by Sustainalytics more than doubled. This early-2016 time frame coincided with discussions of a 40% stake acquisition of Sustainalytics by Morningstar and the establishment of an agreement that would allow its future parent company to build its mutual fund sustainability ratings based on Sustainalytics firm ratings. The acquisition was eventually completed in early 2017. After reaching out to Sustainalytics employees, I was able to confirm that while demand from other customers and a growth in its research capabilities contributed to the expansion of the rating coverage, demand from Morningstar was certainly the most important factor. This rating coverage expansion was a substantial shock to the affected firms because at the time firms were not subject to mandatory ESG reporting requirements and few reported voluntarily; Sustainalytics was also one of the largest ESG ratings providers identified by OECD (Boffo and Patalano, 2020).

Using this exogenous shock, this paper employs a difference-in-difference design to test the impact of the rating coverage expansion on firm ESG performance and ESG disclosure behavior, with a focus on greenhouse gas emissions and conference call climate exposure. The sample period spans the years from 2013 to 2019, with 2017, the first full year after the rating coverage expansion, as the first post-treatment year. The treatment sample comprises all firm-year observations for firms that received their first ESG ratings from Sustainalytics in 2016. The control sample is further split into two groups, one with firms that were always covered by Sustainalytics during the sample period (hereafter referred to as Always Covered), and firms that were never covered by Sustainalytics, but are listed in the U.S. and have the relevant outcome variable data available (hereafter referred to as Never Covered).

The greenhouse gas emissions data are total air pollutants externality costs and Scope 1 and 2 emissions from S&P Trucost. A considerable portion of this data set in the sample period is estimated rather than directly provided in voluntary ESG reports. The conference call data are from Sautner et al. (2022), with variables measuring the relative frequency in which climate change and related phrases appeared in earnings conference call transcripts, as well as the frequency with which climate change is discussed in relation to risks. Intuitively, there would be strategic differences when engaging stakeholders between firms facing different costs in doing ESG work. As such, I split the Treatment and Always Covered sample into high and low ESG ratings based on the firms' first ESG rating by Sustainalytics in mid-2016. To control for firm characteristics and remove as many confounding variables as possible, I include firm fixed effects controls and year fixed effect controls in the models.

I propose a benchmarking causal channel similar to that in Darendeli et al. (2022), through which the ESG rating agency could influence firm ESG and disclosure behavior. Under the benchmarking channel, the incremental information provided by the ESG rating agency allows stakeholders to better compare and benchmark the firm that recently received coverage against firms that were previously covered. This new competitive pressure causes firms to re-evaluate their overall ESG strategies given their ESG standing amongst their competitors. A simple model is proposed in this paper to illustrate firms' changing incentives to do ESG work firms in response to the rating coverage expansion.

The tests for emissions find a positive average effect on the treatment group for all three outcome variables, log air pollution externality costs, and log Scopes 1 and 2 emissions as estimated by Trucost. These results are driven by firms with below-median ESG ratings immediately after the coverage expansion. Though different in direction compared to the findings of Darendeli et al. (2022), the results are still consistent with the benchmarking mechanism, as the firms themselves make decisions on emissions and would have to weigh the costs against possible gains. Under this benchmarking mechanism, a low ESG firm with naturally poor ESG performances and high costs in reducing emissions when compared to high ESG firms may lose incentives to reduce emissions and remain transparent about its ESG activities as a result of the rating coverage expansion. In a pre-expansion ESG rating coverage environment, firms with low ESG ratings may have had the incentive to present themselves as front-runners in ESG initiatives in order to appeal to ESG-minded investors and consumers and differentiate themselves from their similarly low ESG-rated peers. However, following an expansion of ESG rating coverage, these firms are now compared to a different set of firms, revealing their subpar ESG performance relative to the rest of the newly covered firms. Consequently, these firms may no longer be able to reap the same benefits from appealing to ESG-minded stakeholders unless they significantly increase their ESG efforts, particularly in reducing emissions. As a result, we anticipate that low ESG-rated firms will demonstrate different behavior compared to their high ESG-rated counterparts, with an increase in emissions and a decrease in transparency being more prevalent in low ESG-rated firms.

The tests for the conference call climate change exposure find a negative effect on the treatment group for both overall climate change exposure and climate change risk exposure in conference calls. The effect on the overall climate change exposure is driven mainly by low ESG firms, while the effect on climate change risk exposure is driven by high ESG firms. This result is also consistent with the benchmarking mechanism. Firms with poor ESG performance similarly lose incentives to disclose their ESG efforts, as they do not benefit as much from the increase in ratings associated with greater transparency, but could be further reputationally harmed if what is revealed is considered bad; firms with good ESG performance, on the other hand, would maintain the intensity of their ESG disclosures but shift their focus away from risks to achieve a high rating. Therefore, competitive pressure created by the initiation of rating coverage has made all firms less transparent with their ESG disclosures albeit in different ways. This is an interesting unintended consequence of ESG ratings and could have implications for future regulations. Combined, the results from my two sets of tests suggest that while ESG rating agencies add incremental information to the ESG information environment, they create competitive pressure that causes firms to become less active and transparent in their ESG efforts, especially those that are naturally poor on the ESG metrics and have difficulties in improving.

This paper checks the conditional parallel trends assumption in two ways. The first method uses only the pre-treatment sample and regresses the outcome variables on year-fixed effect variables interacted with the treatment group indicator and other control variables. The results for these terms are all not significant, indicating that the assumption is met. The second method replaces the post-treatment term in the model with year-fixed effects interacted with the treatment indicator while controlling for firm fixed effects. The treatment effects in this second method are not significant before the first post-treatment year, 2017, and become significant afterward. This would indicate that the conditional parallel trends assumption is satisfied.

This paper contributes to the existing literature in several ways. First, it adds to research on the determinants of firm ESG action and disclosure behavior by identifying a factor that is unique to the ESG scenario. Unlike various manager or firm characteristics that could naturally affect a wide range of firm activities, ESG rating agencies' coverage is unique to the context of ESG. This study differs from previous studies such as Chen et al. (2020) in that this paper separates ESG ratings from actual ESG performance and instead examines their relationship. Second, the paper adds to the literature on the effect of financial intermediaries and analyst coverage on firm behavior. My findings add to the debate on the effect of analyst coverage on ESG that involves Jo and Harjoto (2014) and Qian et al. (2019), while differing from these previous works by focusing on a type of analyst specialized in ESG. Finally, this paper contributes to the growing literature on the economic role of ESG rating agencies and the ratings they provide. Differing from previous research such as Christensen et al., (2021B), this paper focuses on rating coverage rather than the final rating. This paper adds to this literature by showing that the rating agencies' decisions may have unintended negative consequences on firms' ESG action and transparency. Given the rating agencies' continued growth and their role in providing useful incremental information on firm ESG, regulatory bodies may need to take a more active role in countering the ratings' unintended negative consequences.

The rest of the paper is organized as follows: part 2 develops the hypotheses, part 3 establishes the data, part 4 describes the methodology and checks necessary conditions, parts 5 and 6 present the results on emissions and conference call climate exposure respectively, and part 7 concludes.

2 Hypothesis Development

2.1 Conceptual Underpinning

I predict that the ESG rating expansion will affect firm ESG behavior. It is well established in the extant literature that maximizing firm value is not equivalent to maximizing shareholder welfare (Hart and Zingales, 2017). As investors may have preferences for ESG activities, firms' ESG performance naturally has economic value and asset pricing implications, and extant research identifies several pieces of empirical evidence such as a pollution premium (Hsu, Li, and Tsou, 2023) and lower downside risks for high ESG firms (Hopener et al. 2022). As such firms can benefit from achieving good ESG performance or maintaining a good ESG image in the eyes of investors. The ESG rating, as a widely used and easy-to-interpret measure of ESG, will affect investors' perception of the firm and the amount of benefit the firm can gain from ESG. As each system of ESG ratings follow a specific set of methodology and outputs a number that facilitates easy comparison, all firms rated by the same system face competitive pressure from one another. From the previous discussion, I establish two premises on which I will base my empirical prediction: i) firms can benefit from good ESG performance, and ii) firms affected by the rating coverage expansion are being compared to a different set of firms as a result.

Based on these two premises, I propose a benchmarking channel similar to Darendeli et al. (2022). As rating providers standardize ESG metrics on an industry level, the initiation of rating coverage provides relative ESG information and facilitates the comparison of otherwise noisy absolute measures, such as emissions, workplace fairness, etc.; i.e., it enables benchmarking of ESG performance across firms. Firms may then face competitive pressure from a new set of firms that it is recently compared to and adjust their ESG behavior to accommodate.

Unlike the scenario in Darendeli et al., where customers with a clear ESG preference make the contracting decisions, in this paper's setting the firms themselves, who will have to balance the costs and benefits of doing ESG work, make the decisions on ESG action and disclosure. As such it is not immediately obvious in which direction would the firms shift the level of their ESG action following the rating coverage expansion. To illustrate this problem, I propose a simple model is the subsequent section.

2.2 A Simple Model on ESG Action Decision

Consider a firm that can decide whether to do ESG work at cost c. For a fixed focal firm i, denote its realized ESG action as p_i, which can take on values of 0 or 1, (0 for no ESG work and 1 for good ESG work), and let its ESG cost be c_i. Suppose further that the firm benefits from ESG work based on its ESG performance relative to its competitors. Denote the average ESG performance of the firm's competitors as α, then write the benefit the firm gains from ESG as $U(p_i - \alpha)$, where U is monotonically increasing and non-linear. This benefit U can represent higher stock prices, more investments from investors, or greater ease in financing. It is defined with respect to the difference between firm performance and competitor average because investors make their decisions by comparing the focal firm with a set of benchmark firms as proposed in the benchmarking channel. Then, firm i's net gains from doing ESG work can be written as:

$$U(p_i - \alpha) - c$$

Then the firm's net benefit from doing ESG is:

$$U(1 - \alpha) - c$$

And the firm's benefit from not doing ESG is:

$$U(-\alpha)$$

The firm would only pursue ESG action when:

$$U(1-\alpha) - c \geq U(-\alpha)$$

For algebraic simplicity, let us assume $U(p_i - \alpha) = k(p_i - \alpha)^3$, where k is some positive constant. Then the firm pursues ESG action when:

$$k(1-\alpha)^3 - c \geq -k\alpha^3$$

Simplify and we would have

$$1 - \frac{c}{k} - 3\alpha + 3\alpha^2 \geq 0 \tag{1}$$

Thus the firm pursues ESG action when $\alpha > \frac{3+\sqrt{9-12(1-\frac{c}{k})}}{6}$ or $\alpha < \frac{3-\sqrt{9-12(1-\frac{c}{k})}}{6}$.

I plot the expression represented by the left-hand side of Equation 1 in Figure 1. The solid line corresponds to a larger c (higher ESG cost) compared to the dotted line.

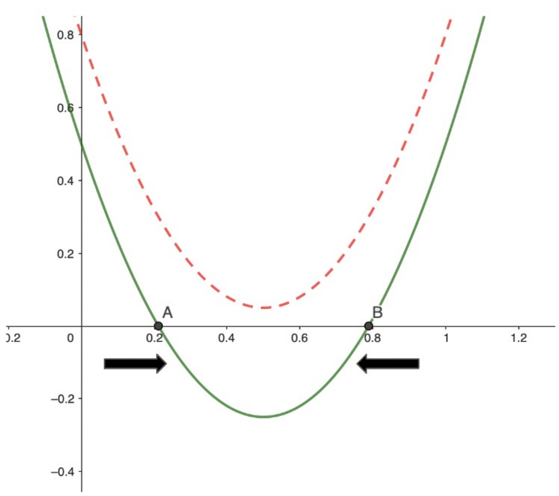

Figure 1: Model of Firm ESG Choice

From this relationship, we make several observations: when the set of firms a firm is being compared to changes, the focal firm may change its ESG action or disclosure decisions. Under the current functional form assumption, when a firm does ESG work under a small α, i.e. when it was compared to a set of firms that generally does not do much ESG work, it may stop doing ESG when α increases, i.e. when it is compared to a different set of firms that do more ESG work than itself on average, to beyond point A in the figure. On the other hand, when a firm does ESG work under a large α, it may stop doing so when α decreases to below point B in the figure. [1] The rating coverage expansion could be seen as a shock to α, as the focal firm is being compared to a different set of firms as a result. We can assume that before the focal firm receives coverage, it is only being compared to firms that are similar to itself, who likely pursue similar ESG strategies, and as such $p_i - \alpha$ would be small in magnitude. After the expansion, $p_i - \alpha$ would shift away from 0 based on the focal firm's ESG performance relative to its new competitors, which we can observe from the ESG rating. Further, the model suggests that when it is not costly for the firm to do ESG work, i.e. c is small, the determinant is more likely to be negative, and the bounds of the inequality are undefined, under which the firm would always do ESG work; a firm with high ESG cost will adjust its ESG action decisions before a firm with low ESG cost does.

Note the model proposed above is specific to events or comparison schemes that does not all firms similar to the ESG rating coverage expansion. Events that affect all firms, such as ESG disclosure mandates, cannot be captured by the model. This is because while the disclosure mandate results in more information on firm ESG that also made comparison easier, it affects all firms equally and does not place the focal firm in a pre-determined group like the rating agency expansion does. As such, investors are unlikely to begin comparing it with a different set of firms as a result.

[1] The specific direction of the change given the initial α is functional form dependent, but the general result is that firms may stop doing ESG work when α changes persist. See Appendix B for two more examples.

2.3 Statement of Hypotheses

Based on the model above, the benchmarking channel may either suggest a deterioration in firm ESG action and disclosure practice - higher emissions and less disclosure - or an improvement. The direction of this change remains an empirical question. I thus propose the following null hypothesis:

H0: *Treated firms will exhibit no difference in emissions following the rating coverage expansion.*

According to the benchmarking mechanism, firms with high ESG costs - or lower ESG performance at a fixed cost - will adjust their actions first. Given the data, I will use above/below the median in the first rating after the rating coverage expansion as the proxy for high/low ESG, because this rating reflects ESG action by the firms before they are able to adjust to Sustainalytics' coverage decision. I propose the following alternative hypotheses for emissions to test against my null hypothesis:

H1: *Treated firms will exhibit increased emissions following the rating coverage expansion.*

On how high and low ESG firms would differ, I hypothesize:

H2: *Emissions of firms with below median ESG rating will increase by a greater magnitude than firms with above median ESG.*

For ESG disclosure, I similarly hypothesize:

H3: *Treated firms will be less transparent in their ESG disclosures following the rating coverage expansion.*

And for high and low ESG firms,

H4: *ESG disclosure of firms with below median ESG rating will decrease by more than firms with above median ESG*

3 Sample Selection and Summary Statistics

3.1 Sample Selection

This paper uses three different data sets of NYSE and NASDAQ listed firms spanning from 2013 to 2019: Sustainalytics ESG rating data, Trucost Emissions data from S&P, and a conference call data set constructed by Sautner et al. (2022). Using the Sustainalytics ESG rating data, I construct indicators for whether a company has received ESG rating coverage at a particular time, and if that rating is below or above the median. Treatment firms in the sample are defined as firms that begin to receive Sustainalytics coverage in 2016; Always Covered firms are defined as firms that have consistently been covered by Sustainalytics between 2013 to 2019; Never Covered firms are firms that have not received any rating coverage from Sustainalytics during and before the sampled period. I remove from the sample firms that have at one point received rating coverage but were discontinued, and firms that began to receive Sustainalytics in the post-treatment time period.

From the Trucost dataset, I use Scope 1 and Scope 2 GHG Emissions, and Absolute Direct and Indirect Costs from air pollutants. Greenhouse gas emissions by a firm are typically broken down into three parts, referred to as Scopes 1, 2, and 3 emissions respectively. Scope 1 is defined as the greenhouse gas emissions resulting from the daily operation of the firm's equipment and facilities directly; Scope 2 is defined as the indirect greenhouse gas emissions related to the resources, such as electricity or cooling, consumed by the firm during its regular operations; Scope 3 is defined as the greenhouse gas emissions produced by the firm's value chain, such as suppliers of the necessary raw materials. Trucost provides data on all three, however as Scope 3 is generally out of the firm's control and a lot of guesswork goes into its estimation, it is generally considered a more unreliable measure for the environmental performances of the firm compared to the first two. Trucost also provides a series of estimated negative externality costs from the firm's air and water pollution. I use the variable representing total negative externality as caused by air pollutants in this study. As large parts of the Trucost data set are estimated, I also examine the GHG Scope 1 and 2 Disclosure variables from the dataset, which are indicator variables for whether the emissions information is directly provided by the firm via its annual or ESG reports, or estimated. To better understand the characteristics of the sample, I merge the Trucost data with Market Capitalization and Industry data from World Scope. Table 1 contains summary statistics of the Trucost dataset.

[Insert Table 1]

Sautner et al. (2022) construct a Conference Call dataset using a machine learning keyword discovery algorithm. From this dataset I use two variables: climate change exposure and climate change risk exposure. Climate change exposure is derived from the frequency in which climate change-related topics appear in the firm's earnings conference call transcripts; climate change exposure risk is the frequency in which climate change is mentioned in sections of the conference call pertaining to risk. Specifically, the exposure measure is the number of climate change-related bigrams in the conference call transcript divided by the total number of bigrams in the transcript, whereas the risk measure is the number of climate change bigrams mentioned in the same sentence as the words "risk", "uncertainty", and their synonyms divided by the total number of bigrams in the transcript. Indeed, I must recognize that these two variables are not perfect measures of the firm's ESG disclosure practices, as they also capture questions asked by investors who participate in the conference calls. The paper's authors' original use for these measures is to proxy the attention devoted to climate change by both financial analysts and firm management. However, intuitively there certainly should be a positive relationship between the firms' willingness to remain transparent about ESG, and the frequency in which climate-related issues are discussed in these conference calls. Since during the sample period specialized ESG reports are not yet widely published by U.S. firms, I will have to rely on this dataset's imperfect measure due to its availability and exhaustiveness.

In the regressions, I multiply the original climate change exposure and climate change risk measures by 1000 so the coefficients are easier compared for the reader. I also merge the data with Market Capitalization and Industry data from World Scope. Table 2 contains summary statistics of the Conference Call dataset.

[Insert Table 2]

3.2 Caveats of Trucost

As previously discussed, the Trucost Data has the issue of being in large parts estimated. To better understand the data set I examine the proportion of estimated data before further analysis. First I plot how the proportion of estimated data changed over time in the full sample.

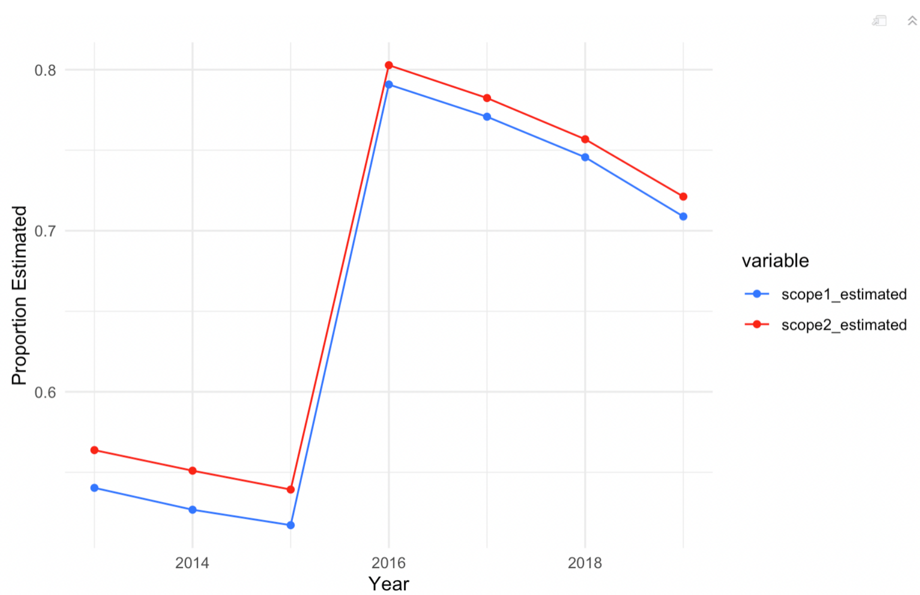

Figure 2: Time Trends of Proportion of Estimated Data, all firms

I also check the trends in the proportion of estimated data in the treatment group. It follows a similar trend as the overall sample, though the proportion of data estimated is generally much higher. This suggests that emissions information is generally much more opaque in the treatment group.

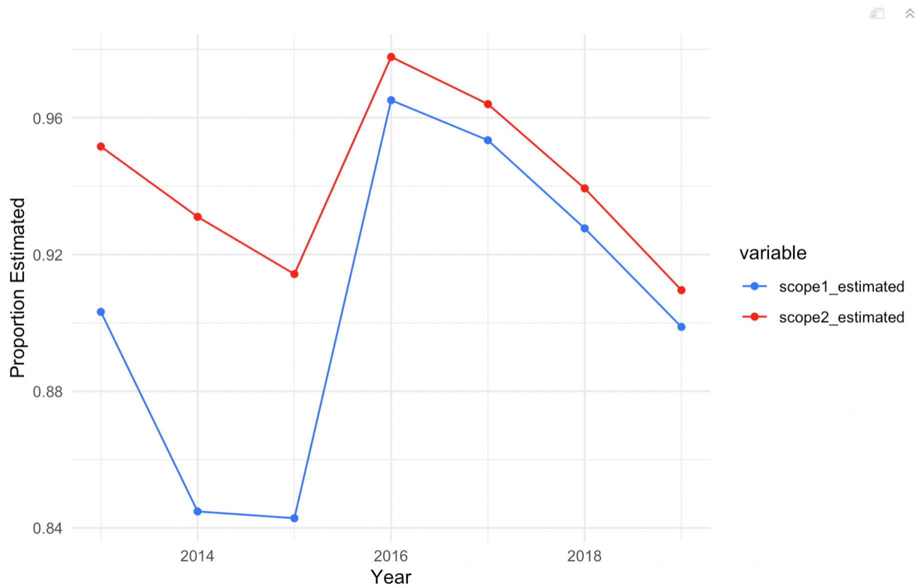

Figure 3: Time Trends of Proportion of Estimated Data, treated firms

We see that the proportion of estimated data generally decreases over time but experienced a major spike in 2016. This is explained by Trucost's own coverage expansion, which took place in 2016 as well. I expected this, as Trucost stated that the first year they begin to have significant coverage for small-cap firms, is 2016. Since Sustainalytics coverage expansion also affected small-cap firms more, there is a large overlap between the firms that begin to receive coverage from Trucost and the treatment group. This could lead to endogeneity issues, and I recognize this fact. However, it is unlikely that Trucost caused Sustainalytics' expansion, as Trucost's data reflects a whole financial year and became available only in early 2017, a year after the Sustainalytics expansion. It is possible that Sustainalytic's initiation of coverage in some ways lead to Trucost's expansion though, and this story, if true, would mean the results could be noisy. Due to the lack of good data I must proceed with this analysis for the purpose of the thesis and will strive to redo this study with higher-quality emissions data should they become available in the future.

4 Empirical Setup

4.1 Sustainalytics' Rating Expansion

My identification strategy relies on the expansion of Sustainalytics ESG rating coverage expansion. Founded in the Netherlands in the 1990s, Sustainalytics has grown to become a leading ESG rating and research firm with a global presence. With more than 800 research analysts covering more than 20000 firms across 172 countries, it is one of the most prominent ESG ratings used by investors, think tanks, and academics. OECD lists it as one of the most widely used ratings (Boffo and Patalano, 2020), and SustainAbility ranks it as the third highest quality and useful rating, beating out MSCI and Refinitiv (SustainAbility, 2019). A plethora of academic papers also use it (Liang et al, 2021; Li et al. 2023). The construction of Sustainalytics' ESG ratings consists of several steps. First, research analysts gather publicly available information on the firm's corporate governance, material ESG risks, and idiosyncratic ESG risks. The materiality of an ESG issue is determined on the subindustry level based on how likely it is to substantially impact the economic value and risk-return profile from an investment perspective; they are further adjusted based on companies' individual business models. These are then aggregated before Sustainalytics scores the firm on how well it manages these risks, producing a final ESG rating.

In March 2016, Morningstar and Sustainalytics launched a partnership where Morningstar would begin to build a fund ESG index based on Sustainalytics firm ESG ratings, along with negotiations of an acquisition that would take place in 2017. Coinciding with the time of this announcement, Sustainalytics' coverage rapidly increased over the first two quarters of 2016. Between December 2015, and June 2016, the number of firms with available Sustainalytics ESG ratings roughly doubled. After reaching out to a senior employee at Sustainalytics, I was able to confirm the causes of this expansion: while demands from other customers and a shift in the company's client platform contributed to the expansion, the main driving factors were the partnership with Morningstar and a change in methodology that allows for the rating to be done with a reduced number of indicators. As both the partnership and acquisition negotiation with Morningstar and the shift in rating methodology are events internal to Sustainalytics, I could make the reasonable assumption that there are no unobserved factors that could affect both firm ESG performance and disclosure behavior and Sustainalytics' rating initiation decision on the firm level. With this plausible assumption, the event becomes an exogenous shock valid for causal inference.

4.2 Difference-in-Difference Setup

To identify the treatment effect of rating coverage expansion of firms' ESG behavior and disclosure, this paper employs a difference-in-difference methodology. My basic model is as follows

$$Y = \alpha + \beta YearFE + \gamma FE + \delta Treatment * Post$$

This identification strategy allows me to compare the outcome variables across treatment and control firms around the time of the treatment. I select 2013-2019 as the sample period because it is a sufficiently wide time window so I can assess the sharpness of the treatment effect and examine how the treatment effect changes over time, while it is not too wide to include confounding effects (such as the Sustainalytics methodology change in 2021).

4.3 Conditional Parallel Trends

As this paper a DID design, I will need to check that its necessary condition of conditional parallel trends holds. To check this assumption, I regress the outcome variables on a set of control variables, and year dummy variables interacted with the treatment group dummy variable on only the pre-treatment sample. To satisfy the assumption, I anticipate these coefficients to all be insignificant. In Table 3 I present results on the Trucost variables regressed on year fixed effects interacted with the treatment group dummy, controlling industry fixed effects, market cap, and book-to-market ratios using the full sample. As we can see, none of the coefficients are significant, which indicates that the parallel trends assumption holds.

[Insert Table 3]

I also do the same for the Conference Call data set, regressing the outcome variable year fixed effects interacted with the treatment group dummy, controlling for industry fixed effects, market cap, and book-to-market ratio. The results are presented in Table 4. As we can see, none of the coefficients are significant, and the conditional parallel trends assumption holds.

[Insert Table 4]

The parallel trends assumption is shown to hold also in the error bar plots of the treatment effects (see the figures in sections 5.2 and 6.2), as the treatment-year fixed effect interaction terms and generally not significant until after the chosen event.

5 Effects on Emissions

5.1 Baseline Results

In the first set of empirical tests, I examine the first part of the main research question of how ESG rating coverage affects firm ESG performance, using the Trucost dataset and Scope 1, Scope 2 Greenhouse Gas Emissions and air pollution externality cost as proxies for ESG performance. Table 5 presents the results of estimating the different-in-different model for each of the three outcome variables. As the control group consists of both firms that were always covered by Sustainalytics over the sample period and firms that were never covered, I perform the three for the combined control group (presented in Panel A), and for each of the sub-control groups (presented in Panels B and C respectively).

[Insert Table 5]

We see that the coefficient on the Treatment*Post term is positive and significant for both scope 1 and scope 2 emissions and the total externality cost from air pollutants. Further, the result remains qualitatively the same when examining both of the control groups separately. This suggests that on average, firms that were newly included in the ESG rating expansion increased their emissions and polluted more. These findings are economically significant, as treated firms increased their scopes 1 and 2 emissions by 14.4% and 21.3% rehttps://www.overleaf.com/project/642da5b8eba9aaed1645665aspectively.

5.2 High vs. Low ESG

I previously hypothesized, based on the benchmarking mechanism, that low ESG firms would lose the incentive to expend resources on ESG after being labeled as such by the rating agency. To test this hypothesis, I divide the sample into two non-overlapping parts based on the firms' first ESG rating by Sustainalytics. High ESG firms are defined as firms that scored above the median in the first few months following the rating coverage expansion event, and low ESG firms are defined as firms that scored below the median. Note this analysis does not include the never-covered control group, as they do not have available ESG rating data. I then split the Treatment*Post term into two based on this high/low ESG indicator. The adjusted regression is as follows:

$$Y = \alpha + \beta YearFE + \gamma FE + \delta_H Treatment * Post * HighESG + \delta_L Treatment * Post * LowESG$$

I present the results of this adjusted regression in Table 6.

[Insert Table 6]

We see that the increase in emissions is indeed mostly driven by firms with lower than median ESG scores, with high ESG groups Treatment*Post term coefficient being insignificant for both Scope 1 emissions and air pollutants costs and weaker than the low ESG group's coefficient for Scope 2 emissions. This is thus consistent with my hypothesis and is suggestive evidence that the competitive pressure created by the ESG rating expansion may cause firms with a naturally poor ESG profile and facing high costs in doing ESG action to lose the incentive. This increase in emissions is therefore a negative unintended consequence of the rating agency's decision and could warrant further research.

As changes to firm spending on ESG may take time before it is reflected in emissions, it is important to check how the treatment effect changes over time. To this end, I instead regress the outcome variables on an interaction between the treatment group indicator and year indicators split by high and low ESG groups. The results of regressing Log Scope 1 Emission and Log Scope 2 Emissions are plotted in errorbar plots presented below, in Figures 4 and 5 respectively.

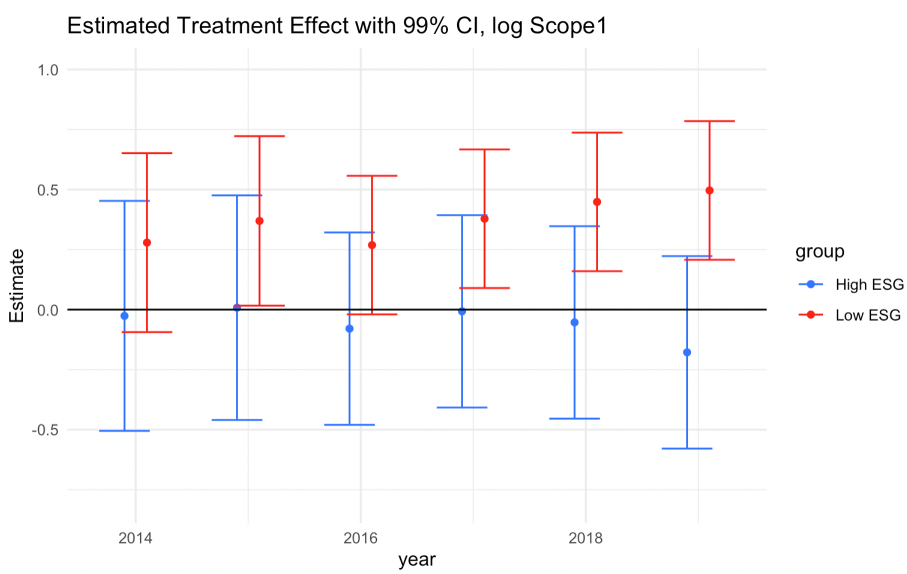

Figure 4: Treatment Effects over Time: Log Scope 1 Emissions

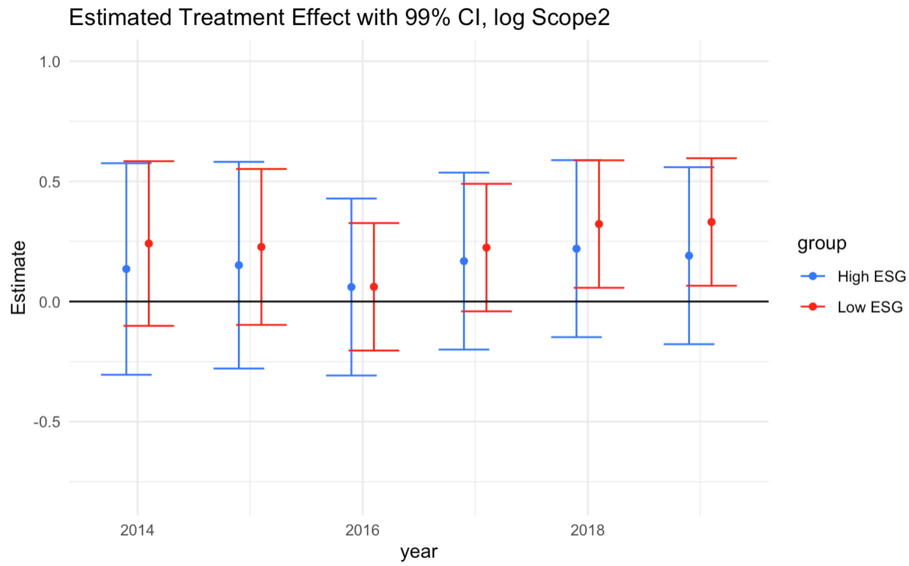

Figure 5: Treatment Effects over Time: Log Scope 2 Emissions

Note the treatment effects are insignificant prior to the event time and become increasingly positive and significant for the low ESG group. This is consistent with my hypothesis and earlier results and suggests the parallel trends assumption holds.

5.3 Additional Tests

As a sanity check to ensure the variation across high/low ESG ratings is not purely driven by firm size, I regress the outcome variables on Treatment*Post term interacted with a firm size indicator. Large-cap firms are defined as the largest firms whose combined market capitalization makes up 70% of the total volume of the U.S. capital market. The results of this regression are presented in Table 7.

[Insert Table 7]

We see that while the firm size indicator and the Treatment*Post term are both significant, their interaction term is not. As such large-cap and small-cap firms are not impacted significantly differently by the rating coverage expansion and are not the source of the variability observed earlier.

6 Effects on Conference Call

6.1 Baseline Results

For the remainder of the empirical tests, I examine the second part of the main research question of how ESG rating coverage affects firm ESG disclosure practice, with a focus on the discussion of environmental issues in its earnings conference calls. These analyses use the Sautner et al. (2022) earnings conference call dataset, and climate change exposure and climate change risk exposure as the outcome variables (see section 3.1 for variable definitions). Table 8 presents the results of estimating the different-in-different model for the two outcome variables. As the control group consists of both firms that were always covered by Sustainalytics over the sample period and firms that were never covered, I perform the three for the combined control group (presented in Panel A), and for each of the sub-control groups (presented in Panel B).

[Insert Table 8]

The results are negative and significant for both variables, but only when using always-covered firms as the control group. This means treated firms reduced, relative only to always covered firms, both the frequency they discuss climate change overall and the frequency they discuss climate change in relation to risks during earnings conference calls.

6.2 High vs. Low ESG

Similar to the analysis for emissions, I adjust the specification and divide the sample into two non-overlapping parts based on the firms' first ESG rating by Sustainalytics. High and low ESG firms may have different disclosure strategies when they try to maintain a high ESG score. Furthermore, the hypothesis about the incentives of ESG action based on the benchmarking mechanism may also apply for ESG disclosures. Thus, as before, I split the Treatment*Post term into two based on this high/low ESG indicator, reaching the following specification:

$$Y = \alpha + \beta YearFE + \gamma FE + \delta_H Treatment * Post * HighESG + \delta_L Treatment * Post * LowESG$$

The results for these regressions when using always covered firms as the control group is presented in Table 9.

[Insert Table 9]

We see that the decrease in climate change exposure is mainly driven by firms with below-median ESG ratings, whereas the decrease in climate change risk exposure is mainly driven by firms with above-median ESG ratings. An explanation for the former could be that low-ESG companies began to avoid discussing ESG as a result of more scrutiny because they do not have positive information to disclose. They could also have lost the incentive to disclose ESG information, as consistent with the benchmarking channel. An explanation for the decrease in climate risk discussion would be that high-ESG companies wish to maintain their good ratings and therefore avoid discussing their ESG performance since Sustainalytics ratings are determined based on risks. These results are consistent with my hypothesis and suggest that high and low ESG firms face different incentives and therefore strategies when disclosing ESG information. For both types of firms, the competitive pressure created by the rating agency had the unintended consequence of making the firms less transparent in their conference calls.

Finally, I examine how these treatment effects change over time by once again interacting the Treatment*Post*high/low ESG indicator with year indicators. These estimates and their corresponding confidence intervals are plotted in error bar plots in Figure 6 and Figure 7 respectively.

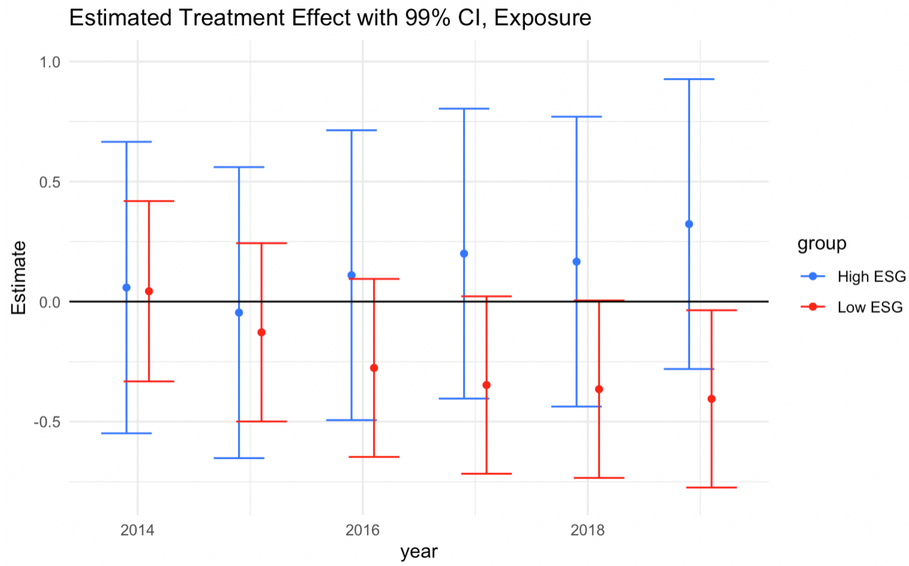

Figure 6: Treatment Effects over Time: Conference Call Climate Change Exposure

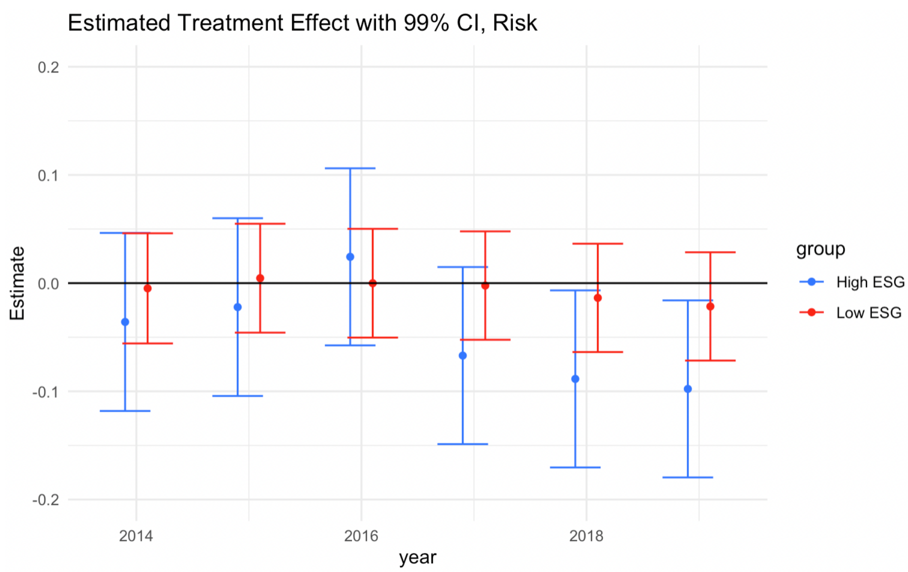

Figure 7: Treatment Effects over Time: Conference Call Climate Change Risk Exposure

Again, we see decreases in ESG exposure are driven by low ESG firms and this treatment effect began to become increasingly negative and significant after the coverage expansion event, while decreases in ESG risks are driven by high ESG firms and follow a similar trend. This is consistent with my hypothesis and shows the parallel trends assumption holds.

6.3 Additional Tests

I also regress the conference call outcome variables on Treatment*Post term interacted with a firm size indicator to check if the results are driven by firm size. The results of this regression are presented in Table 10.

[Insert Table 10]

We see that the treatment effect on conference call climate exposure varies across small and large-cap firms, while the effect on conference call climate risk does not. This confirms that the results from earlier cannot be completely attributed to firm size.

7 Conclusions

In this paper, I explore how ESG rating agencies' coverage decisions affect firm ESG action disclosure decisions. Exploiting a plausibly exogenous ESG rating coverage expansion by Sustainalytics, a major ESG rating agency, and employing a difference-in-difference methodology, I demonstrate that firms, especially those with poor ESG performances, polluted more and became less transparent about their ESG practices as a result of receiving coverage. While counter-intuitive, this result is consistent with a benchmarking channel I propose wherein the focal firm loses the incentive to do good ESG work because it is being compared to firms that it could not realistically outcompete in terms of ESG. Overall the results show that firms face different incentives and employ different strategies when they aim to maintain a good ESG rating, and the competitive pressure created by rating agencies' coverage decisions can have unintended negative consequences.

My findings contribute to the existing literature on firm ESG action and disclosure by identifying a factor unique to the ESG context. I also contribute to the literature on the relationship between analyst coverage and ESG performance by considering ESG rating agencies as a type of specialized analyst. My results provide new evidence to the debate on whether analyst attention positively or negatively affects firm ESG. What further distinguishes the paper from these two streams of literature is that I separate the rating agency from the measure of ESG performance and instead examine the connection between the two. Finally, I add to the new stream of literature studying the economic role of ESG rating agencies and highlight the potential unintended negative consequence their decisions may have.

My research has implications for ESG disclosure regulations. At the current stage, ESG disclosure regulations in the U.S. remain relaxed compared to that of Europe and Asia, and stakeholders must rely on voluntary disclosure by the firm to learn exact ESG information. Given the current prominence of ESG rating agencies and the unintended negative consequence their coverage decisions may have on ESG disclosure transparency, this is further evidence in support of a well-defined set of regulations regarding how and what firms should disclose about their ESG practices. Further research examining the relationship between voluntarily disclosed ESG reports (which were not widely available during the sample period) and ESG rating agencies is necessary to confirm the unintended negative consequences of ESG rating agencies' decisions as a justification for further regulation.

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Appendix A: Tables

Table 1: Summary Statistics, Trucost Data

Panel A: Sample Selection	
Initial Trucost Sample (2013-2019 US firms)	12339
Removing Firms with no data before 2017	(735)
Removing Treated Later firms [1]	(857)
Final Sample	10747
Treated Group	4152
Always Covered Group	4791
Never Covered Group	1804

Panel B: Sample Distribution by Year

Year	Total	Treatment	Never Covered	Always Covered
2013	818	65	105	648
2014	827	63	111	653
2015	847	75	110	662
2016	2100	994	396	710
2017	2077	992	377	708
2018	2061	987	366	708
2019	2017	976	339	702

Panel C: Sample Distribution by Industry

GICS	Total		Treated		Never Covered		Always Covered	
	Count	%	Count	%	Count	%	Count	%
Energy	579	5.73%	82	2.06%	213	14.30%	284	6.12%
Materials	549	5.43%	152	3.82%	77	5.17%	320	6.90%
Industrials	1640	16.23%	731	18.39%	206	13.83%	703	15.15%
Consumer Discretionary	1366	13.52%	543	13.66%	125	8.39%	698	15.04%
Consumer Staples	472	4.67%	153	3.85%	70	4.70%	249	5.37%
Health Care	1286	12.73%	639	16.08%	169	11.34%	478	10.30%
Financials	1646	16.29%	669	16.83%	344	23.09%	633	13.64%
Information Technology	1224	12.11%	523	13.16%	117	7.85%	584	12.58%
Communication Services	446	4.41%	151	3.80%	92	6.17%	203	4.37%
Utilities	366	3.62%	104	2.62%	26	1.74%	236	5.09%
Real Estate	531	5.25%	227	5.71%	51	3.42%	253	5.45%
Total	10105	100%	3974	100%	1490	100%	4641	100%

Note: Panel C uses Trucost data merged with Industry data from World Scope based on ticker. 642 firm-year observations were unmatched and removed from this table.

Table 1 Continued

Panel D: Sample Description of Trucost Variables				
	Min.	Median	Mean	Max.
Full Sample (N = 10747)				
log GHG Scope 1	0	9.925	9.922	19.506
log GHG Scope 2	0	10.379	10.140	16.921
log Absolute Direct and Indirect Costs	-6.818	3.230	3.105	10.555
GHG Scope 1 Disclosure	0	0	0.299	1
GHG Scope 2 Disclosure	0	0	0.2865	1
log Market Cap	15.38	21.84	21.88	27.15
Treatment Group (N = 4152)				
log GHG Scope 1	0	9.110	8.944	16.313
log GHG Scope 2	0.3173	9.291	9.089	15.320
log Absolute Direct and Indirect Costs	-6.768	2.158	2.068	7.204
GHG Scope 1 Disclosure	0	0	0.067	1
GHG Scope 2 Disclosure	0	0	0.053	1
log Market Cap	17.07	20.90	20.88	27.01
Never Covered (N = 1804)				
log GHG Scope 1	0.103	8.653	8.569	18.534
log GHG Scope 2	0.151	9.073	8.784	16.533
log Absolute Direct and Indirect Costs	-6.818	1.844	1.709	10.484
GHG Scope 1 Disclosure	0	0	0.192	1
GHG Scope 2 Disclosure	0	0	0.189	1
log Market Cap	15.38	20.21	20.70	26.14
Always Covered (N = 4791)				
log GHG Scope 1	0	11.102	11.278	19.506
log GHG Scope 2	0	11.48	11.56	16.92
log Absolute Direct and Indirect Costs	-2.998	4.454	4.529	7.204
GHG Scope 1 Disclosure	0	0	0.496	1
GHG Scope 2 Disclosure	0	0	0.479	1
log Market Cap	17.46	23.04	23.12	27.15

Table 2: Summary Statistics, Conference Call Data

Panel A: Sample Selection

Initial Conference Call Sample (2013-2019 US firms)	9352
Removing Firms with no data before 2017	(581)
Removing Treated Later firms	(437)
Observations Lost from Merging Data	(918)
Final Sample	7416
Treated Group	2593
Always Covered Group	2102
Never Covered Group	2721

Panel B: Sample Distribution by Year

Year	Total	Treatment	Never Covered	Always Covered
2013	933	312	392	292
2014	1050	345	408	297
2015	1124	378	446	300
2016	1133	382	448	303
2017	1129	392	435	302
2018	1052	392	356	304
2019	995	392	299	304

Panel C: Sample Distribution by Industry

GICS	Total		Treated		Never Covered		Always Covered	
	Count	%	Count	%	Count	%	Count	%
Energy	500	6.74%	28	1.08%	384	14.16%	88	4.19%
Materials	343	4.63%	66	2.55%	109	4.02%	168	7.99%
Industrials	1072	14.46%	520	20.05%	282	10.40%	270	12.84%
Consumer Discretionary	1007	13.58%	380	14.65%	327	12.06%	300	14.27%
Consumer Staples	248	3.34%	80	3.09%	53	1.95%	112	5.33%
Health Care	1069	14.41%	410	15.81%	444	16.38%	215	10.23%
Financials	1158	15.61%	406	15.66%	402	14.83%	343	16.32%
Information Technology	1035	13.96%	367	14.15%	433	15.97%	235	11.18%
Communication Services	377	5.08%	112	4.32%	153	5.64%	112	5.33%
Utilities	183	2.47%	39	1.50%	25	0.92%	119	5.66%
Real Estate	424	5.72%	185	7.13%	99	3.65%	140	6.66%
Total	7406	100%	2593	100%	2721	100%	2102	100%

Table 2 Continued

Panel D: Sample Description of Conference Call Variables				
	Min.	Median	Mean	Max.
Full Sample (N = 7416)				
Climate Change Exposure (scaled by 1000)	0	0.284	1.017	43.327
Climate Change Risk (scaled by 1000)	0	0	0.0341	7.366
log Market Cap	11.11	21.23	21.22	27.15
Treatment Group (N = 4152)				
Climate Change Exposure (scaled by 1000)	0	0.247	1.030	43.327
Climate Change Risk (scaled by 1000)	0	0	0.0317	7.366
log Market Cap	17.62	20.94	20.87	23.12
Never Covered (N = 1804)				
Climate Change Exposure (scaled by 1000)	0	0.274	0.767	22.441
Climate Change Risk (scaled by 1000)	0	0	0.030	3.972
log Market Cap	11.11	20.14	19.99	25.22
Always Covered (N = 4791)				
Climate Change Exposure (scaled by 1000)	0	0.347	1.324	36.213
Climate Change Risk (scaled by 1000)	0	0	0.0417	1.417
log Market Cap	18.74	23.14	23.24	27.15

Table 3

	Dependent variable:		
	log(Absolute_Direct_Indirect)	log(GHG_Scope1)	log(GHG_Scope2)
	(1)	(2)	(3)
treatment*factor(year)2014	0.270	0.392	0.420
	(0.234)	(0.306)	(0.282)
treatment*factor(year)2015	0.206	0.369	0.373
	(0.224)	(0.293)	(0.270)
treatment*factor(year)2016	−0.040	0.014	0.198
	(0.173)	(0.227)	(0.209)
Control Variables	Yes	Yes	Yes
Observations	4,063	4,063	4,063
R^2	0.760	0.726	0.660
Adjusted R^2	0.759	0.725	0.659
Residual Std. Error (df = 4043)	1.201	1.572	1.451
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 4

	Dependent variable:		
	1000 ∗cc_risk_ew		
	(1)	(2)	(3)
treatment_groupTRUE:factor(year)2014	0.270	0.391	0.420
	(0.230)	(0.297)	(0.265)
treatment_groupTRUE:factor(year)2015	0.204	0.351	0.391
	(0.220)	(0.285)	(0.254)
treatment_groupTRUE:factor(year)2016	−0.042	0.011	0.223
	(0.170)	(0.220)	(0.197)
Control Variables	Yes	Yes	Yes
Control Group	Always Covered	Never Covered	Both Groups
Observations	2,640	2,675	3,989
R^2	0.764	0.736	0.687
Adjusted R^2	0.763	0.735	0.686
Residual Std. Error (df = 4043)	1.183	1.529	1.365

Note: *p<0.1; **p<0.05; ***p<0.01

Table 5: Panel A

	Dependent variable:		
	log(Absolute_Direct_Indirect)	log(GHG_Scope1)	log(GHG_Scope2)
	(1)	(2)	(3)
Treatment*Post	0.106***	0.131***	0.173***
	(0.017)	(0.025)	(0.024)
Year and Firm Fixed Effects	Yes	Yes	Yes
Control Group	Both Groups	Both Groups	Both Groups
Observations	10,747	10,747	10,747
R^2	0.983	0.974	0.967
Adjusted R^2	0.979	0.968	0.960
Residual Std. Error (df = 8690)	0.367	0.537	0.503
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 5: Panel B

	Dependent variable:		
	log(Absolute_Direct_Indirect)	log(GHG_Scope1)	log(GHG_Scope2)
	(1)	(2)	(3)
Treatment*Post	0.090***	0.135***	0.193***
	(0.018)	(0.028)	(0.025)
Year and Firm Fixed Effects	Yes	Yes	Yes
Control Group	Always Covered	Always Covered	Always Covered
Observations	8,943	8,943	8,943
R^2	0.982	0.972	0.963
Adjusted R^2	0.978	0.965	0.955
Residual Std. Error (df = 7287)	0.348	0.533	0.489

Note: *p<0.1; **p<0.05; ***p<0.01

Table 5: Panel C

	Dependent variable:		
	log(Absolute_Direct_Indirect)	log(GHG_Scope1)	log(GHG_Scope2)
	(1)	(2)	(3)
Treatment*Post	0.154***	0.139***	0.152***
	(0.026)	(0.033)	(0.031)
Year and Firm Fixed Effects	Yes	Yes	Yes
Control Group	Never Covered	Never Covered	Never Covered
Observations	5,956	5,956	5,956
R^2	0.977	0.975	0.967
Adjusted R^2	0.970	0.967	0.958
Residual Std. Error (df = 4586)	0.410	0.516	0.488

Note: *p<0.1; **p<0.05; ***p<0.01

Table 6

	Dependent variable:		
	log(Absolute_Direct_Indirect)	log(GHG_Scope1)	log(GHG_Scope2)
	(1)	(2)	(3)
Treatment*Post*HighRating	0.001	−0.013	0.118***
	(0.030)	(0.045)	(0.042)
Treatment*Post*LowRating	0.115***	0.178***	0.214***
	(0.019)	(0.029)	(0.027)
Year and Firm Fixed Effects	Yes	Yes	Yes
Control Group	Always Covered	Always Covered	Always Covered
Observations	8,939	8,939	8,939
R^2	0.982	0.972	0.963
Adjusted R^2	0.978	0.965	0.955
Residual Std. Error (df = 7283)	0.348	0.533	0.489

Note: *p<0.1; **p<0.05; ***p<0.01

Table 7

	Dependent variable:		
	log(GHG_Scope1)	log(GHG_Scope2)	log(Absolute_Direct_Indirect)
	(1)	(2)	(3)
Treatment*Post	0.135***	0.193***	0.089***
	(0.028)	(0.025)	(0.018)
LargeCap	2.256***	0.744**	2.027***
	(0.335)	(0.307)	(0.219)
Treat*Post*LargeCap	0.483	0.424	0.416
	(0.616)	(0.565)	(0.403)
Year and Firm Fixed Effects	Yes	Yes	Yes
Control Group	Always Covered	Always Covered	Always Covered
Observations	8,943	8,943	8,943
R^2	0.972	0.963	0.982
Adjusted R^2	0.965	0.955	0.978
Residual Std. Error (df = 7286)	0.533	0.489	0.348

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table 8 : Panel A

	Dependent variable:	
	1000 ∗cc_expo_ew	1000 ∗cc_risk_ew
	(1)	(2)
Treatment*Post	−0.059	−0.018***
	(0.049)	(0.007)
Constant	0.359	−0.006
	(0.494)	(0.067)
Firm and Year Fixed effects	Yes	Yes
Observations	7,416	7,416
R^2	0.890	0.501
Adjusted R^2	0.871	0.413
Residual Std. Error (df = 6310)	0.984	0.134

Note: *p<0.1; **p<0.05; ***p<0.01

Table 8 : Panel B

	Dependent variable:			
	1000 ∗cc_expo_ew	1000 ∗cc_risk_ew	1000 ∗cc_expo_ew	1000 ∗cc_risk_ew
	(1)	(2)	(3)	(4)
Treatment*Post	-0.190^{***}	-0.024^{***}	0.062	-0.012
	(0.062)	(0.008)	(0.053)	(0.009)
Firm and Year Fixed effects	Yes	Yes	Yes	Yes
Control Group	Always Covered	Always Covered	Never Covered	Never Covered
Observations	4,695	4,695	5,314	5,314
R^2	0.906	0.513	0.890	0.501
Adjusted R^2	0.892	0.436	0.869	0.409
Residual Std. Error	1.041 (df = 4058)	0.141 (df = 4058)	0.916 (df = 4486)	0.147 (df = 4486)

Note: *p<0.1; **p<0.05; ***p<0.01

Table 9

	Dependent variable:	
	1000 ∗cc_expo_ew	1000 ∗cc_risk_ew
	(1)	(2)
Treatment*Post*HighRating	0.198^{*}	-0.076^{***}
	(0.107)	(0.015)
Treatment*Post*LowRating	-0.274^{***}	-0.012
	(0.065)	(0.009)
Year and Firm Fixed Effects	Yes	Yes
Control Group	Always Covered	Always Covered
Observations	4,690	4,690
R^2	0.907	0.515
Adjusted R^2	0.892	0.439
Residual Std. Error (df = 4053)	1.038	0.141

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table 10

	Dependent variable:	
	1000 $*$cc_expo_ew	1000 $*$cc_risk_ew
	(1)	(2)
Treatment*Post	-0.179^{***}	-0.024^{***}
	(0.062)	(0.008)
LargeCap	-0.160	0.0004
	(0.555)	(0.075)
Treatment*Post*LargeCap	-4.394^{***}	0.097
	(0.848)	(0.115)
Year and Firm Fixed Effects	Yes	Yes
Control Group	Always Covered	Always Covered
Observations	4,695	4,695
R^2	0.907	0.513
Adjusted R^2	0.892	0.436
Residual Std. Error (df = 4057)	1.037	0.141
Note:		*p<0.1; **p<0.05; ***p<0.01

Appendix B: Additional Examples

Following the simple model devised in the hypothesis development section, I present figures for two examples of alternative function forms of U. Figure 8 presents the case when assuming

$$U(p_i - \alpha) = k(p_i - \alpha)^{\frac{1}{3}}$$

The solid line still corresponds to a higher c than the dotted line. As we can see, under this function form assumption, increases starting from a large α or decreases starting from a small α cause the firm to stop doing ESG work. The curve representing the lower ESG cost firm intersects the horizontal axis at points outside of [0,1], which means the firm would never stop ESG action as α is a percentage. The overall implications remain the same as firms may still stop doing ESG work when α increases and firms with low ESG costs adjust later or do not adjust at all.

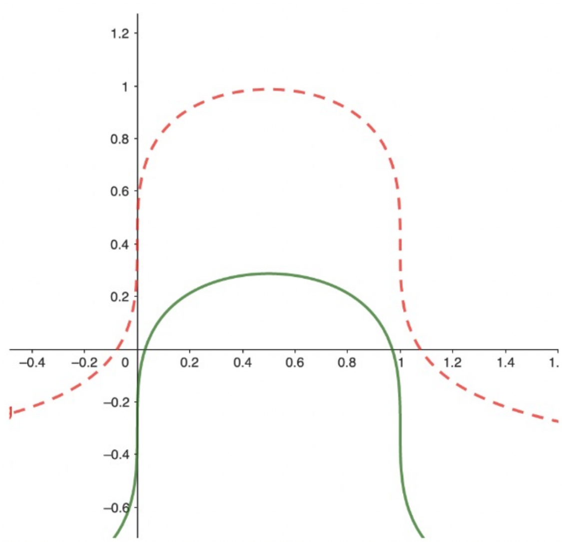

Figure 8: Model of Firm ESG Choice: Cube Root Function

Figure 9 presents the case when assuming

$$U(p_i - \alpha) = ke^{(p_i - \alpha)}$$

The solid line also corresponds to a higher c than the dotted line. Under this function form assumption firms face a single cutoff α for doing ESG work. When α is smaller than that value firms do ESG, and when α increases they will stop doing so. The curve representing the lower ESG cost firm intersects the horizontal axis at a larger value, meaning that low-cost firms change ESG decisions after the high-cost firms do. The overall implications remain the same as firms may still stop doing ESG work when α increases and firms with low ESG costs adjust later or do not adjust at all.

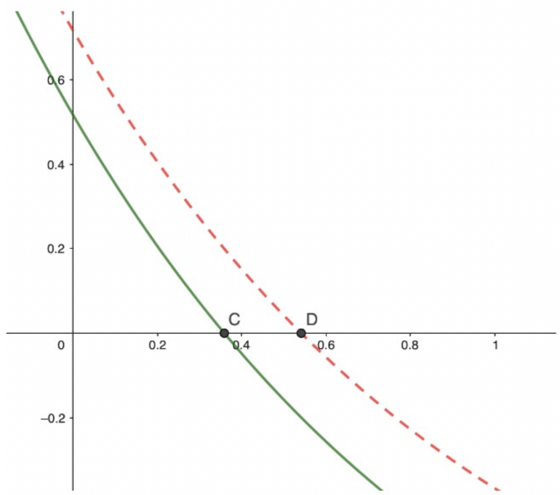

Figure 9: Model of Firm ESG Choice: Cube Root Function