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Abstract

In my dissertation, I exhibit three studies in Finance. In Chapter 1, I provide a new perspective on the optimal call policy for corporate callable bonds by evaluating the proportion called in call exercises. My data suggests that firms on average call only 66% of the amount initially offered when they call their bonds with fixed-price call provisions, and 69% when they call their bonds with make-whole call provisions. I examine how the proportion called in a call exercise is correlated with bond and firm characteristics. In Chapter 2, I analyze the impact of the maturity of a firm's bonds upon the liquidity of the firm's remaining bonds. The results suggest that a reduction in the number of bonds outstanding leads a reduction in liquidity. In Chapter 3, I characterize the presence and consequences of sustainability compensation targets using novel hand-collected data of managerial annual incentive plans (AIP) goals in 1994-2020. The data indicates that sustainability targets are often quantified with corresponding overall weight in the incentive plan instead of rarely observed sustainability modifiers. Moreover, sustainability compensation goals are valuable if implemented in firms with high past pollution levels, exclusively, consistent with optimal contracting.

Chapter 1:

Partial Call: A New Perspective of Optimal Call Policy

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Abstract: I provide a new perspective on the optimal call policy for corporate callable bonds by evaluating the proportion called in call exercises. My data suggests that firms on average call only 66% of the amount initially offered when they call their bonds with fixed-price call provisions, and 69% when they call their bonds with make-whole call provisions. I examine how the proportion called in a call exercise is correlated with bond and firm characteristics. I find that the proportion called in a call exercise is positively correlated with the call premium for fixed-price call exercises and the cash holdings of the issuing firm for both fixed-price and make-whole call exercises. When a bond is called to be refinanced, it is called in a higher proportion, regardless of the type of the call provision. In addition, the proportion called increases with the number of bondholder protective covenants for make-whole call provisions, but not fixed-price call provisions. Last, I show that for make-whole call provisions, the impact of refinancing the bond on proportion called in a call exercise is higher for speculative-grade bonds as compared to investment-grade bonds.

1. Introduction

Research concerning callable bonds often focuses on two main factors: (1) whether a call provision is attached at the issuance of the bond and, if attached, (2) the optimal call policy which indicates how and when a bond should be called. In a traditional Modigliani and Miller perfect market, there is no advantage in attaching a call provision to a corporate debenture (Myers, 1971). In a more complex setting, there are numerous reasons to attach a fixed-price call provision, a make-whole call provision, or both. The most discussed factors relating to attaching a fixed-price call provision to a corporate bond are the tax-deductibility of the call premium (Boyce and Kalotay, 1979), agency costs (Bodie and Taggart, 1978), refinancing risk (Xu, 2018), information asymmetry and interest rate risk control (Kraus, 1973). However, the incentives for attaching a make-whole provision to a corporate bond are more limited. Most of the rationale behind attaching a fixed-price call provision does not apply to a make-whole call provision because they are by design rarely in-the-money. Existing literature finds that the motivation of managing refinancing risk is the main driver for the issuance of make-whole callable bond (Elsaify and Roussanov, 2016; Xu, 2018).

The studies on the optimal call policy of callable bonds also evolves with time. The early literature of optimal call policy using dynamic programming describes the optimal call policy in terms of the market interest rate relative to the coupon rate on the bond (Kraus 1973). Later studies apply the valuation methodologies of contingent-claims and derive the optimal call policy as a function of the stochastic process of the value of the bond or the firm. Surprisingly, most of the previous studies focus only on the timing of the call policy, ignoring the amount called. That is, the previous empirical literature implicitly assumes the entirety of the principal of the issue is called at once.

The purpose of this research is to explore and analyze the partial exercise of corporate bond call features. More specifically, I emphasize that call exercises frequently do not retire the entire bond issue but only a proportion of it, contrary to the literature's common assumption. Using mainly the Mergent FISD data set, I examine the proportion called for 1,273 non-convertible US corporate debentures during the period 2002-2017. In my final sample, the average amount retired in a call exercise is 71.58% of the issuance, and only 49.40% of the sample called the full principal.

My analysis consists of two main parts. First, I examine the common bond and firm-level characteristics that affect the proportion called in both fixed-price and make-whole call exercises. I find that the proportion called increases with the call premium for fixed-price call provisions. Moreover, the proportion called increases with the amount of cash held by the issuing firm for both fixed-price and make-whole call provisions. I also show that when a firm issues a new bond to finance the call exercise, the firm will subsequently retire a higher proportion of its callable bond, regardless of the call provision type. The economic magnitude of the impacts depends on the measure of the average effect. When considering the marginal average effect, a 1% increase in the call premium is correlated with a 2.7% increase in the proportion called as measured by the percentage of the amount offered. In addition, for bonds with a fixed-price (make whole) provision, a 1% increase in the cash held by the issuing firm corresponds to a 0.8% (1.2%) increase in the proportion called of the amount offered. Finally, when a firm finances its call exercise with a newly issued bond, it calls 14.2% more of the amount offered if the retired bond is fixed-price callable, and 7.3% more of the amount offered if the retired bond is make-whole callable. These results are consistent when I measure the proportion called by the percentage of the amount remaining outstanding prior to the call or use the conditional marginal effect at means instead of the average margin effect measure as the measure of the marginal effect.

Second, I study the factors that affect the proportion called of fixed-price and make-whole call exercises differently. For example, I find that the number of bondholder protective covenants does not have a statistically significant impact on the proportion called in a fixed-price call exercise. However, one additional bondholder protective covenant in a make-whole call exercise on average leads to a 1.6% increase in the proportion called measured by the percentage of amount offered. Similarly, the refinancing behavior in fixed-price call exercises and make-whole call exercises are also different. Speculative-grade firms retire similar proportions compared to investment-grade firms when they refinance the call exercises with newly issued bonds in fixed-price call exercises. However, for make-whole call exercises, speculative-grade firms retire 21.0% more when measured by the percentage of amount offered. Similar to the first part of results, the second part of the results are also robust to the changes in the measure of proportion called, as well as the measure of marginal effect.

I contribute to the existing literature on the call policy of corporate bonds in three primary ways. First, to the best of my knowledge, this research is the first to analyze and explain the proportion called in corporate bond call policies. Prior research concerning corporate bond call exercises focuses on other aspects of the call exercise. For example, Vu (1984) provides evidence that most firms do not call their callable bonds when they initially become in-the-money. Additionally, he shows that the market value of the called bonds can be below the call price at the announcement of call. King and Mauer (2000) confirmed these results and further show that the average length of the call delay is correlated with the size and liquidity constraints of the firm. The most relevant paper on the partial retirement of corporate bonds is Mann and Powers (2007), but they focus on bond tender offers, not call exercises. This paper extends the optimal corporate bond retirement policy literature to the study of the proportion called when exercises occur.

Second, I extend the study of optimal call policy to incorporate the difference between fixed-price callable bonds and make-whole callable bonds. Make-whole callable bonds have become the dominant form of callable bonds in recent years (Alderson et al., 2017). However, due to the design of the make-whole call provisions, they are almost always out-of-the-money during the entirety of their lifetime. Thus, the motivations for issuing make-whole callable bonds must be different from that of traditional fixed-price callable bonds. For example, Elsaify and Roussanov (2016) shows that make-whole callable bonds do not mitigate asymmetric information, underinvestment, or risk-shifting problems. Alderson et al. (2017) further provides evidence that make-whole callable bonds do not reduce shareholder-creditor conflict as fixed-price callable bonds do. This paper takes the differences between fixed-price and make-whole call provisions into consideration and studies how optimal corporate call policy differs from one type of call provision to the other.

Finally, I contribute to the literature on the procyclical refinancing decisions of investment-grade and speculative-grade firms. Previous studies show that the investment-grade firms and the speculative-grade firms make their procyclical refinancing decisions differently. However, the empirical analysis provides contradicting evidence on the direction of the difference. For example, Xu (2018) shows that speculative-grade firms procyclically refinance their bonds more often compared to investment-grade firms, while Mian and Santos (2018) suggests that investment-grade firms do more procyclical bank loan refinancing compared to speculative-grade firms. In this paper I study the procyclical bond refinancing via call exercises and provide consistent evidence that speculative-grade firms do more procyclical refinancing via make-whole call exercises. This is consistent with Xu (2018).

The rest of the paper is organized as follows. Section 2 introduces the hypotheses. Section 3 describes the data and summary statistics. Empirical results and robustness are in Section 4 and Section 5, respectively. Section 6 concludes the paper.

2. Hypotheses

Calling a bond when its market price is above the exercise price benefits the shareholders at the expense of the bondholders. This is the wealth transfer effect of the call execution. Based on this effect, existing literature predicts and proves that most call exercises happen when then market price of the bond is higher than the call price (King and Mauer, 2000). I notice that the wealth transferred per share of the bond issue called increases with the difference between the market price of the callable bond and its exercise price. Ergo, I predict that in the context of a fixed-price callable bond a traditional shareholder value maximizing manager will call a higher proportion of outstanding bonds when the difference between the market price and the exercise price of the bond rises.

However, the situation for make-whole callable bonds is very different. In particular, the exercise price of a make-whole call provision is designed to almost always be greater than the market price of the bond. Hence, the make-whole call provisions are almost always exercised at prices that are "out-of-the-money" (Elsaify and Roussanov, 2016). This means that the value transfer effect in the fixed-price call provision execution discussed above should be irrelevant in the execution of the make-whole callable provision. Consequently, the price difference between the market price and the call execution price is not likely to affect the proportion called of a make-whole call provision.

Hypothesis 1: The proportion called in a call exercise increases with the call premium of the bond for fixed-price call exercises.

The exercise of the call provision should also depend on the cash availability of the firm. A firm can call a higher proportion of the bond if they have more cash available. Cash can be derived from either issuing replacement bonds or the firm's current cash balance. In this paper, a bond is recognized as a replacement bond if the new bond is issued 45 days before or after the call exercise of the initial bond and the offering amount is between 75% and 125% of the amount called. Therefore, I make two predictions. First, the proportion called increases as the cash owned by the firm increases. Second, the proportion called increases when the firm refinances the call execution with a replacement bond.

Hypothesis 2: The proportion called in a call exercise increases with the cash of the issuing firm.

Hypothesis 3: When a firm refinances its call exercise with a replacement bond, the firm calls a higher proportion of the callable bond.

Since the make-whole call provisions are rarely in the money, there must be other motivations for including these call provisions. Mann and Powers (2003) suggests that a firm may retire a bond using the attached make-whole call provision to bypass a covenant. A natural extension to this argument would be that the firm retires a higher proportion of its make-whole callable bonds when the bond contains more bondholder protective covenants. However, given the importance of the call premium in the fixed-price call exercises, the impact of covenants might be less important in determining the proportion called for fixed-price call exercise.

Hypothesis 4a: The proportion called in a call exercise increases with the number of bondholder protection covenants for a make-whole call exercise.

Hypothesis4b: The proportional called is not affected by the number of bondholder protection covenants for a fixed-price call exercise.

Despite the prior hypothesis that refinancing the call exercises with a newly issued bond increases the proportion called in the call exercise, previous studies show that investment-grade firms and speculative-grade firms behave differently when they refinance. In their recent paper, Mian and Santos (2018) show that investment-grade firms have a significantly higher propensity to refinance their syndicate loans than speculative-grade firms. They attribute this effect to the investment-demand motivation which says that investment-grade firms procyclically refinance more in order to hedge the refinancing risk caused by their future capital expenditure needs.

In a similar paper, however, Xu (2018) provides contradicting results for overall bond refinancing. She shows the on average, speculative-grade firms refinance significantly more of their bonds procyclically compared to investment-grade firms. She argues that speculative firms do so to manage their refinancing risk, while the flight-to-safety phenomenon provides good credit conditions for the investment-grade firms, and therefore reduces their refinancing motivation.

While both papers provide strong hypotheses and evidence, they provide contradictory results. Consequently, it is difficult to predict whether investment-grade firms refinance their call exercises more or less than speculative-grade firms. Hence, I test the following contradiction hypotheses for the fixed-price and make-whole call exercises separately.

Hypothesis 5a: Investment-grade firms call a higher proportion in a call exercise than speculative-grade firms when they refinance the call exercise with a replacement bond.

Hypothesis 5b: Investment-grade firms call a lower proportion in a call exercise than speculative-grade firms when they refinance the call exercise with a replacement bond.

3. Sample and Summary Statistics

The initial sample contains all call exercises that occurred between January 1, 1982 and December 31, 2018. These call exercises are collected from tables Amount_Outstanding and Amt_Out_Hist in the FISD Mergent Database for events where either an entire issue is called (E), the remaining outstanding balance of an issue is called (B), or some other portion of an issue called (P). I limit the sample to only non-puttable, non-convertible, non-sinking U.S. corporate debentures with a fixed coupon rate. This sample contains 3,782 observations. I then merge the sample with the TRACE Enhanced database to find the last available average daily trade price for the call exercises. I exclude all bonds that are not traded for 30 days prior to the call exercise. This leaves 1,928 call exercises. Next, I merge the resulting data set with the Compustat quarterly fundamental database to observe firm-level characteristics. Finally, I separate the sample into two parts, one contains bonds that are called with a fixed-price call provision, and the other contains bonds that are called using a make-whole call provision. The final sample contains a total of 1,597 call exercises between January 1, 2002 and December 31, 2017, of which 911 are fixed-price call exercises and 686 are make-whole call exercises.

[Insert Table 1 Here]

Table 1 shows the time distribution of the sample in terms of the number of call exercises, the dollar amount outstanding prior to the call, the dollar amount called, and the proportion called by the type of call provision. As seen in the table, the number of call exercises declines significantly during 2008 which is likely a result of the financial crisis. At the beginning of the sample period there are substantially more fixed-price call options relative to make-whole call options. This ratio reverses later in the sample as make-whole bonds rose in popularity and ultimately became the most common type of callable bond (Alderson et al., 2017). This trend is true for both the number of make-whole bonds as well as the dollar amount of these bonds which strongly suggests the increasing importance of make-whole call provisions.

[Insert Table 2 Here]

In Table 2 I report the summary statistics of the hypothesized and key control variables. Panel A shows that the median fixed-price called bond has an initial offering amount of $225 million, $163 million remaining outstanding prior to the call, is called at 53% of its maturity, and is rated as speculative-grade[1]. Meanwhile, Panel B shows that the median make-whole called bond has an initial

[1] The median of the indicator variable $Investment\ Grade$ is 0 indicating that the median fixed-price callable bond is rated as speculative.

offering amount of $450 million, $300 million remaining outstanding prior to the call, is called at 80% of its maturity, and is rated as investment-grade.

Comparing these two groups shows that called make-whole bonds tend to be larger and more mature than the fixed-price bonds. Firms who call their make-whole bonds have a median cash balance of $866.5 million, while those who call their fixed-price callable bonds have a median cash balance of only $132.78 million. That is, firms who call their make-whole bonds are usually more liquid than their counterparts who call fixed-price bonds.

[Insert Figure 1 Here]

Figure 1 provides the density of proportion called by call provision types. Given the skewness of the sample at the extreme value of 1, I exclude bonds which were entirely called in Figure 1 to better emphasize the difference in proportion called when the bond issue is not entirely called. Comparing Panel A to Panel B, I conclude that the proportion called in fixed-price call exercises is more evenly distributed, while the proportion called in make-whole call exercises is concentrated near certain values, namely 10% and 50%.

4. Empirical Results

The objective of this paper is to analyze the factors that determine the proportion of bonds called when a call exercise occurs. Given the fact that many callable bonds are never called, selection bias might be a concern. In my model, firms simultaneously decide whether to call and, if called, what proportion to call. In this context, when a firm faces the opportunity to call one of its bonds, it first estimates the fixed cost of the call exercise. If the firm plans to call only a small portion of its bonds, it may find that the call exercise would be disadvantageous after taking the fixed cost of the call exercise into consideration. Thus, the firm will stop the plan to call. On the other hand, if the firm decides to call a large portion of the bond, the fixed cost of the call per bond is diluted, which increases the net benefit (or reduces the loss) of the call, leading to a higher chance of exercising the call option.

To address potential selection bias, I compute the Heckman correction by estimating the probability that a bond will be called in a given year with the following probit model:

$$Call_{it} = \Phi(\alpha + \beta X_{it} + \delta_t + \varepsilon_{it}),$$

$Call_{it}$ is a dummy variable equal to 1 if bond i is called in year t, X_{it} is a vector of predictor variables of the probability of a call exercise for bond i in year t, and δ_t indicates year dummies. The elements in the predictor vector X are mainly from previous literature (Sarkar, 2001; Mann and Powers, 2003; Becker et al., 2018). They are mostly the same for fixed-price call provisions and make-whole call provisions, but with minor differences. The fixed-price call strike price and the in-the-money dummy are only available for fixed-price call provisions, while the make-whole spread is only available for make-whole call provisions. The common predictors for both types of call provisions include the trade price, the rating, the rating change, the bond age, the number of covenants, the cash holding, the ten year treasury rate, the treasury slope, and the Baa-Aaa spread. Appendix 1 provides detailed definition of all variables.

I then compute the Mills ratio using the estimated model in order to compute the Heckman correction, which is the inverse of the Mills ratio. The Heckman correction fully addresses the selection bias if and only if the main regression model is linear. However, since my main dependent variable, the proportion called, is a variable that is defined only between 0 and 1, all my main empirical tests are based on a generalized linear model using the logit link function, which is not linear. Thus, my model does not satisfy all the assumptions of the Heckman selection model. Nonetheless, I still use the Heckman correction term for two reasons. First, controlling for the Heckman correction is the best way that I can address the selection bias concern. Second, the results are similar when I estimate the model with a linear regression. This provides robustness to address the nonlinearity concern.

[Insert Table 3 Here]

Table 3 presents the regression results of the probit model. Columns (1), (2), and (3) provide the regression coefficients, the average marginal effect (AME), and the conditional marginal effect at means (MEM) for fixed-price call provisions, respectively. Columns (4), (5), and (6) provide the results for make-whole call provisions in a similar form. For fixed-price calls, the most important factor explaining the probability of the call decision is the moneyness as seen in Column (2). On average, an in-the-money fixed-price call option embedded in a bond is 32.7% more likely to be exercised compared to an out-of-the-money fixed-price call option. In addition, the age of the bond, the rating of the bond, and the number of covenants contained in the bond are also statistically significant. Column (3) provides similar results when considering the conditional marginal effect at means instead of the average marginal effect.

Meanwhile, for make-whole call provisions, the most important factors affecting the probability of an exercise are the age of the bond, the number of bond-holder protective covenants contained in the bond, the cash holding of the issuing firm, the rating change, and the make-whole spread. However, besides the age of the bond, none of these factors strongly dominate the probability of a call exercise for make-whole callable bonds.

In conclusion, Table 3 shows that the factors impacting the probability of call for a fixed-price call provision are very different from the factors that affect the probability of call for a make-whole call provision. Moreover, even if the factors are overlapping, the economic magnitude of these overlapping factors varies between the two types of call provisions. This is consistent with Elsaify and Roussanov's (2016) finding that the motivation of exercising fixed-price call provisions and make-whole call provisions are very different and provides empirical support for the sample separation in this study.

With the Heckman correction, I can now test the hypotheses. I start with testing the first three hypothesis for fixed-price call provisions with the following specification:

$$Proportion\ Called_i = \beta_0 + \beta_1 Call\ Premium_i + \beta_2 \ln(Cash)_i + \beta_3 Refinance_i + Controls_i + \gamma_t + \varepsilon_i$$

[Insert Table 4 Here]

Table 4 shows the regression results for the specification. The dependent variable $Proportion\ Called$ is measured as the proportion of the amount originally offered that is called in Panel A and the proportion of the amount remaining outstanding prior to the call in Panel B. In both panels, Column (1) shows the results of the regression without the Heckman correction, while Column (2) includes the Heckman corrected results. The differences between the coefficients in Column (1) and Column (2) of both panels strongly suggest that controlling for the selection bias related to the probability of call is necessary for this study.

Column (3) and Column (4) of the two panels provide the average marginal effect and the conditional marginal effect at means of Column (2). According to Column (3) of both panels, a 1% increase in call premium leads to a 2.7% increase in the proportion of the amount offered called and a 2.5% increase in the proportion of the amount remaining outstanding called. Meanwhile, a 1% increase in the cash held by the issuing firm leads to a 0.8% increase in the proportion called as measured by the percentage of amount initially offered, and a 1.2% increase when measured by the percentage of amount remaining outstanding prior to the call. Lastly, compared to the call exercises financed by other sources, the coefficient of the variable $Refinance$ shows that call exercises that are financed by newly issued bonds on average retire 14.2% more of the amount offered, and 15.0% more of the amount remaining outstanding. All the results above are statistically significant at the 1% level, except for the impact of call premium on the proportion of the amount remaining outstanding and the impact of cash holding on the proportion of amount initially offered, which are significant at the 5% level and the 10% level, respectively. These results are consistent with Hypotheses 1, 2, and 3.

Column (4) of the two panels shows similar results, with the only difference being the economic magnitude. The magnitude of the difference is a natural result of the skewness in the data. In this study, more than half of the observations are called in full and the dependent variable, $Proportion\ Called$, is subsequently coded as 1. Such an unusual concentration on this extreme value has a stronger impact on the average marginal effect than on the conditional marginal effect at means.

In summary, for fixed-price call provision exercises, Table 4 provides strong support for Hypotheses 1, 2 and 3. The results are significant both statistically and economically. Moreover, the results are robust to the different measures of both the proportion called and the average effect.

Next, I test Hypotheses 2 and 3 for make-whole call exercises with the following specification:

$$Proportion\ Called_i = \beta_0 + \beta_1 \ln(Cash)_i + \beta_2 Refinance_i + Controls_i + \delta_t + \varepsilon_i$$

[Insert Table 5 Here]

Table 5 presents the results of the specification for make-whole call provisions using the same format as Table 4. In Panel A, the dependent variable, $Proportion\ Called$, is measured by the percentage of the amount initially offered. According to Panel A Column (3), on average, a 1% increase in the cash held by the issuing firm leads to a 1.2% increase in the proportion called as a percentage of the amount initially offered in a make whole call exercise. Also, a make-whole call exercise financed by a newly issued bond calls 7.3% more as measured by the percentage of the amount initially offered than make-whole call exercises financed by other sources. The results are statistically significant at the 10 % level and the 1% level, respectively, and are consistent with Hypotheses 2 and 3. Panel A, Column (4) provides similar results when considering the conditional marginal effect at means instead of the average marginal effect.

Table 5, Panel B examines the proportion called as a percentage of the amount remaining outstanding prior to the call. Here I exclude the year dummies included in all previous tests, because when including the year dummies into the specification, the test statistics and the significance level of Column (3) and (4) are unable to be calculated. This is again due to the unusual concentration of the main sample at the extreme value of 1 which leads to a highly singular variance matrix in this specification. According to Column (3) of Panel B in Table 5, make-whole call exercises which are financed by newly issued bonds call on average 7.3% more of their amount remaining outstanding prior to the call exercises compared to those that are financed by other sources. This result is statistically significant at the 5% level, and is consistent with the result in Panel A. The results are similar when considering the marginal effect at means. These results provide additional supporting evidence for Hypothesis 3. However, neither the average marginal effect nor the conditional marginal effect at means of $\ln(Cash)$ on $Proportion\ Called$ is statistically significant in Panel B. This raises some concerns to Hypothesis 3 for make-whole call provisions.

Next, in connection with Hypothesis 4, I explore the impact of bondholder protective covenants on proportion called for fixed-price call provisions and make-whole call provisions. One important note when testing this impact is that the fixed-price call provisions sample contains two types of observations. The first category of observations are bonds that contain only a fixed-price call provision. The second category of observations are bonds that contain a make-whole call provision followed by a fixed-price call provision and the bond is called with the fixed-price call provision. For the second type of observations, while the call exercises happen during the fixed-price callable period, the decision and the proportion to call could be determined during the make-whole callable period. Consequently, the motivation of exercising make-whole call provisions might play a role even if the bond is called using the fixed-price call provisions. To better capture the potential difference between the two types of fixed-price call provisions, I use the following model to test Hypothesis 4 for fixed-price call provisions:

$$Proportion\ Called_i = \beta_0 + \beta_1 \cdot Number\ of\ Covenants_i + \beta_2 \cdot Pure\ FP\ Call\ Dummy_i + \beta_3 \cdot Number\ of\ Covenants_i \times Pure\ FP\ Call\ Dummy_i + Controls_i + \delta_t + \varepsilon_i$$

The $Pure\ FP\ Call\ Dummy_i$ is a dummy variable equal to 1 if bond i contains only fixed-price call provisions and 0 otherwise.

[Insert Table 6 Here]

Table 6 shows the impact of covenants on proportion called for all bonds with a fixed-price call provision. Panel A shows the results of proportion called as a percentage of the amount initially offered, and Panel B shows the results of proportion called as a percentage of amount remaining outstanding prior to the call. According to the table, neither of the two measures of proportion called are sensitive to the number of bond-holder protection covenants.

Similar to bonds with fixed-price call provisions, the sample of bonds with make-whole call provisions also contains two types of observations. The first type of observations are bonds that contain only a make-whole call provision for the whole maturity. The second type of observations are bonds that contain a make-whole call provision followed by a fixed-price call provision. For the second type of observations, the make-whole callable period occurs before the fixed-price callable period. Therefore, if a bond is called in the make-whole call period then it was never treated as a fixed-price call and should be treated solely as a make-whole callable bond. Hence, I test Hypothesis 4 for make-whole call provisions using the following specification:

$$Proportion\ Called_i = \beta_0 + \beta_1 \cdot Number\ of\ Covenants_i + Controls_i + \delta_t + \varepsilon_i$$

[Insert Table 7 Here]

Table 7 presents the impact of covenants on the proportion called for make-whole call provisions. As before, the dependent variable, $Proportion\ Called$, is measured as the proportion of the amount initially offered in Panel A and the amount remaining outstanding prior to the call in Panel B. Column (3) shows the average marginal effect which indicates that for each additional bondholder protective covenant the issuing firm on average calls 1.6% more of the amount offered and 1.7% more of the amount remaining prior to the call. The results are statistically significant at the 5% level. Column (4) provides similar evidence when considering conditional marginal effects at means.

Combining Table 6 and Table 7, I conclude that while the number of bondholder protective covenants embedded in a bond does not affect the proportion called if the bond is called using a fixed-price call provision. However, the number of covenants is positively correlated with the proportion called of the bond if the bond is called under a make-whole call provision. This is consistent with Hypothesis 4.

In the discussion of Hypothesis 5, I show that Xu (2018) and Mian and Santos (2018) provide contradicting conclusions on whether investment-grade firms refinance more of their debt than speculative-grade firms. Therefore, I suggest that the impact of the refinancing decision on the proportion called would be different for investment-grade firms and speculative-grade firms, but the difference could be either positive or negative. Next, I will test the existence and direction of the difference with the following specification:

$$Proportion\ Called_i = \beta_0 + \beta_1 \cdot Refinance_i + \beta_2 \cdot Investment\ Grade_i + \beta_3 \cdot Refinance_i \times Investment\ Grade_i + Controls_i + \delta_t + \varepsilon_i$$

To test if the proportion called for investment-grade callable bonds and speculative-grade callable bonds react differently to the refinancing decision, I need to find the difference between the marginal effect of the refinancing decision on the proportion called in a call exercise for investment-grade bonds and speculative-grade bonds. In a linear model, this is simple and straightforward. The β_3 in the specification is the coefficient of interest. However, for a nonlinear model, the coefficient of the interaction term does not reflect the difference of the marginal effect. Therefore, I compute the marginal effect of the refinance decision on the proportion called for investment-grade bonds and speculative-grade bonds separately, and then calculate the difference.

[Insert Table 8 Here]

[Insert Table 9 Here]

Tables 8 and 9 provide the difference between the impact of the refinancing decision on proportion called for fixed-price call provisions and make-whole call provisions, respectively. Table 8 shows that the difference in the impact of refinancing decision on proportion called between investment-grade bonds and speculative-grade bonds is either insignificant or only marginally significant for bonds called with fixed-price call provisions, depending on the measure of the dependent variable $Proportion\ Called$ and the measure of the marginal effect.

However, the results are different for make-whole call provisions. According to Table 9, when investment-grade firms finance their call exercises by issuing new bonds they call 21.0% less of the amount initially offered and 25.9% less of the amount remaining outstanding compared to speculative-grade firms as measured by the average marginal effect. The difference is statistically significant at the

1% level and is economically significant. The results are consistent when I change the measure of marginal effect to conditional marginal effect at means.

In summary, the impact of the refinancing decision on the proportion called in a call exercise for investment-grade bonds and speculative-grade bonds is similar for fixed-price call provisions, but significantly different for make-whole call provisions. For the make-whole callable bonds the impact of refinancing on investment-grade bonds is much weaker than the effect on speculative-grade bonds. This contradicts Hypothesis 5a but is consistent with Hypothesis 5b and Xu's (2018) finding that the speculative-grade firms do more procyclical refinancing than investment-grade firms to manage the maturity of their debt in order to reduce the refinancing risk.

5. Robustness

The Heckman correction has many assumptions that are not fully valid for empirical inference. Therefore, I conduct several robustness tests to support the results in the previous section.

First, the Heckman correction assumes that the model in the second stage is linear. However, the primary analysis of this study is conducted using a nonlinear model. To address the potential misspecification concern, I cross-check my primary analysis by running a linear regression for every specification in Section 4 with the same set of variables. While the results are not presented in the paper, they are consistent with the analysis presented in Section 4.

Second, previous literature suggests that financial service and utility firms are very different from industrial firms in the fixed income market (Mann and Powers, 2017). Therefore, it is a valid concern that the results could be driven by bonds issued by financial service and utility firms. I do not exclude financial service and utility firms from the final sample because it further reduces our relatively small sample size. However, to ensure the validity of the results, I run the empirical tests in Section 4 again with a sub-sample that contains only callable corporate debentures issued by the industrial firms. The size of this sub-sample ranges from 70% to 75% of the final sample size, depending on the specification. Thus, the sub-sample is still relatively representative. The results, which are not presented in the paper, are consistent with the main results.

Last, there is not a commonly accepted definition of the variable $Refinance$ which is the main variable in Hypotheses 3 and 5. In the main empirical tests I follow the procedure in Xu's (2018) paper. A call exercise is considered to be a part of a refinancing process of the bond called and the variable $Refinance$ is set to 1 if a new bond is issued within 45 days before or after the call exercise and the amount issued of the new bond is between 75% to 125% of the amount called in the call exercise. There are two potential concerns here. First, the main results may depend on the 45-day range which is arbitrary to an extent. Second, a bond issued after a call exercise is less likely than a bond issued before the call exercise to be a bond explicitly used for refinancing. To address these two concerns, I run the specifications of Tables 4, 5, 8, and 9 with two alternative measures of the refinance dummy. First, I expand the refinance window to 90 days before or after a call exercise. Second, I exclude bonds issued after the call exercise thereby limiting the refinance window to only 45 days before the call exercise. The unablated results are consistent with prior tests.

6. Conclusion

In this paper I show that when a call option is exercised the proportion of a bond called is not always the entirety of the initial bond issuance, as extant literature assumes. I find that often only a proportion of the bonds are called. I then evaluate the characteristics driving the decision to exercise a call and the magnitude of the call. The relevant characteristics include the call premium of the bond, the cash holdings of the firm, and the refinance decision related to the call exercise. In addition, I illustrate that the impact of these factors could be different for fixed-price call exercises versus make-whole call exercises. In particular, the number of bond holder protective covenants is positively correlated with the proportion called in a make-whole call exercise, but not fixed-price call exercises. Finally, I show that the impact of the different factors on proportion called also depends on the rating of the issuing firm. The evidence suggests that the speculative-grade firms retire their callable bods more when they refinance the call with newly issued bonds compared to their investment-grade peers. This is consistent with Xu's (2018) finding that speculative-grade firms procyclically refinance their bonds more compared to investment-grade firms, but in contrast to Mian and Santos (2018) who maintain the opposite conclusion.

This paper provides a new evidence on the optimal call policy of corporate bonds. While previous studies focus on the timing portion of the optimal call policy, they ignore the proportion called. The findings in this paper fill in this gap in the existing literature by providing evidence of how bond and firm characteristics are correlated with the proportion called which is an important part of the optimal call policy.

In addition to providing a new understanding of optimal call policy, this paper also raises questions for future scholars to answer. For example, why do firms call their fixed-price callable bonds when they are out-of-the-money, especially when only a proportion of the bond issue is called. Since the call exercises have negative wealth transfer effects under this condition, and thus are value-destroying actions, the firm must have some other motivation for this type of call exercise. I look forward to studies on this and other related topics.

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Appendix A. Variable definitions

The variables used in this study are defined in the table below.

Variables	Description	Data Source
Offering Amount	The par value of bond as initially issued.	FISD
Call Price	The price, expressed as a percent of par, at which the issue may be called.	FISD
Make-Whole Spread	The spread above a comparable treasury issue that will have to be paid if the make whole call option is exercised.	FISD
Call Amount	The amount of the issue called in a call exercise.	FISD
Amount Outstanding	The amount of the issue remaining outstanding prior to the call exercise.	FISD
Last Trade Price	The price, expressed as a percentage of par, at which the bond is last traded for prior to the call exercise.	TRACE Enhanced
eff_trd_d_diff	The number of days between the date of the call exercise and the date the bond is lastly trade prior to the call	FISD, TRACE Enhanced
Call Premium	The difference between the *Call Price* and the *Last Trade Price*, expressed as a percent of par,	FISD, TRACE Enhanced
Proportion of Offering Amount Called	The proportion called as a percentage of amount initially offered.	FISD, TRACE Enhanced
Cash	Cash and short-term investments on the balance sheet of the issuing firm at the quarter prior to the call	Compustat
Refinance	An indicator variable which equals to 1 if a call exercise is a part of a refinancing process. If a new bond is issued within 45 days before or after a call exercises and the amount issued of the new bond is 75% to 125% of the amount called in the call exercise, the call exercises is considered to be a part of a refinancing process of the bond called.	FISD
Investment Grade	An indicator variable which equals 1 if a bond is rated as investment-grade by Standard & Pool prior to the call exercise.	FISD
Number of Covenants	The number of bondholder protective covenants contained by the bond called.	FISD
Bond Age	The number of days since the bond was issued, as a percentage of the whole maturity of the bond.	FISD
Combo Call	An indicator variable which equals to 1 if a bond contains both a fixed-price call provision and a make-whole call provision.	FISD
Pure Fixed-Price Call	An indicator variable which equals to 1 if a bond contains only fixed-price call provisions.	FISD

In the Money Dummy	An indicator variable which equals to 1 if a bond is fixed-price callable and the fixed-price call provision is in-the-money.	FISD, TRACE Enhanced
Ten Year Treasury	10-year treasury rate.	FRED
Treasury Slope	The difference between 10-year treasury rate and 1-year treasury rate	FRED
Baa-Aaa Spread	Moody's seasoned corporate bond Baa-Aaa spread.	FRED

Figure 1. Density of Proportion Called by Call Provision Type

The density of proportion called for those call exercises that are not entirely called. The proportion called is measured as the amount called as a percentage of the amount remaining outstanding prior to the call. The bin width is 0.05, and the density has been standardized.

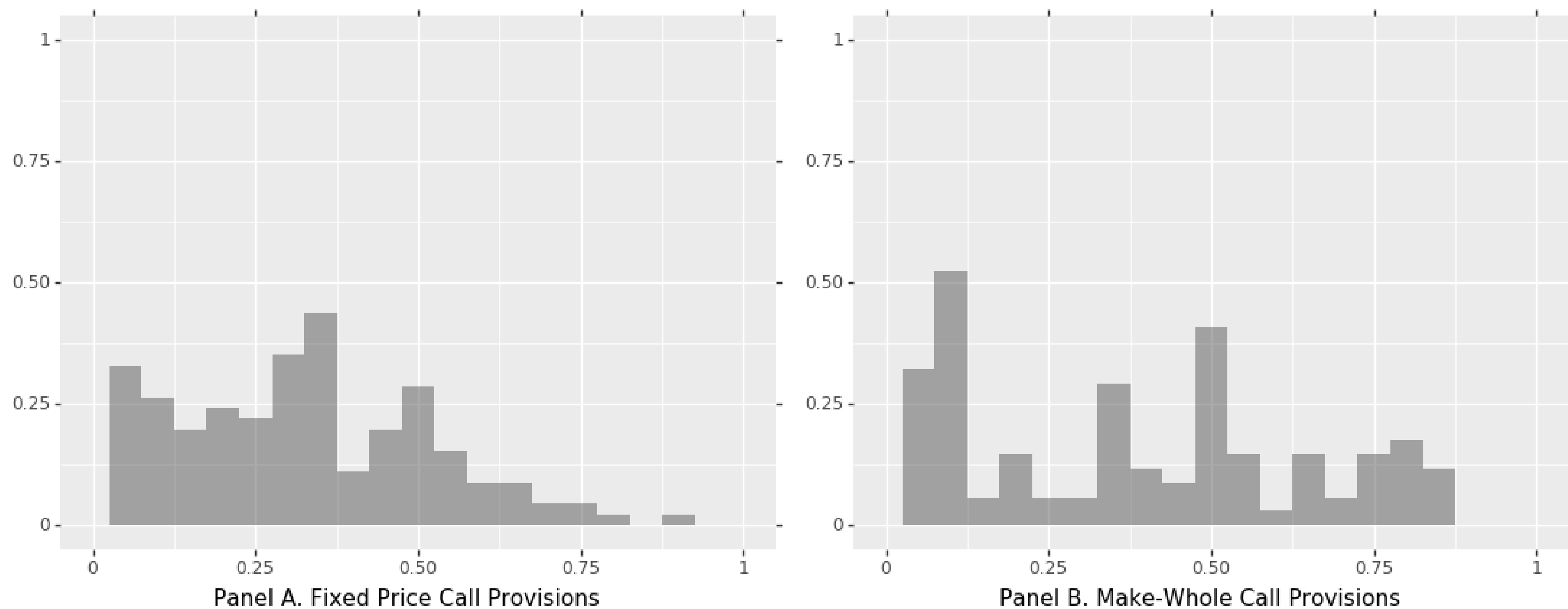

Table 1. Time distribution of the sample of call exercises

Columns (2), (3), (4), and (5) provide the number of call exercises, the aggregated dollar amount outstanding prior to the call exercises, the aggregated dollar amount called, and the aggregated proportion called of the fixed-price call exercises in our final sample. Columns (5), (6), (7), and (8) show the same details of make-whole call exercises in our final sample. The fixed-price call provision exercises might be from bonds with only a fixed-price call provision, as well as bonds that contain a make-whole call provision followed by a fixed-price call provision. Similarly, the make-whole call provision exercises might be from bonds with only a make-whole call provision, or bonds that contain a make-whole call provision followed by a fixed-price call provision. I note that no bond contains a fixed-price call provision followed by a make-whole call provision.

	Fixed-Price Call Provision				Make-Whole Call Provision			
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Year	Number of Call Exercises	Amount Outstanding ($ million)	Amount Called ($ million)	Proportion Called	Number of Call Exercises	Amount Outstanding ($ million)	Amount Called ($ million)	Proportion Called
2002	35	36,489.27	35,561.67	97.46%	2	26,000.00	12,000.75	46.16%
2003	123	177,575.39	157,897.00	88.92%	4	13,482.00	13,482.00	100.00%
2004	105	177,272.95	130,261.41	73.48%	21	93,589.48	89,101.19	95.20%
2005	69	139,059.38	106,040.04	76.26%	29	129,257.01	112,257.47	86.85%
2006	30	62,253.98	52,662.16	84.59%	18	55,138.58	43,593.58	79.06%
2007	45	95,550.90	84,467.17	88.40%	15	51,907.15	33,777.15	65.07%
2008	17	33,190.29	31,616.41	95.26%	5	8,651.45	7,368.45	85.17%
2009	29	57,100.67	54,578.59	95.58%	10	24,723.39	19,773.39	79.98%
2010	76	142,277.12	130,814.88	91.94%	38	141,715.81	115,453.94	81.47%
2011	68	213,117.56	139,020.00	65.23%	39	173,810.52	135,942.99	78.21%
2012	71	196,373.50	155,341.09	79.10%	76	371,991.58	303,610.46	81.62%
2013	46	86,622.46	82,365.78	95.09%	85	283,831.59	257,109.85	90.59%
2014	46	132,233.02	119,933.02	90.70%	105	529,105.77	456,492.33	86.28%
2015	54	161,395.84	152,049.84	94.21%	69	307,193.30	299,321.03	97.44%
2016	56	186,330.39	157,415.99	84.48%	109	566,503.81	542,103.16	95 .69%
2017	41	133,983.72	110,666.67	82.60%	61	436,667.55	429,517.55	98.36%
Total	911	2,030,826.44	1,700,691.73	83.74%	686	3,213,568.99	2,870,905.29	89.34%

Table 2. Summary Statistics

Panel A. Fixed-Price Call Provisions

This table present the summary statistics for fixed-price call exercises. Detailed variable definitions are in Appendix A.

Variable	N	Mean	Standard Deviation	1%	10%	25%	50%	75%	90 %	99%
Offering Amount	911	325,567.34	482,393	22,816	100,000	150,000	225,000	350,000	525,000	2,103,647
Call Price	909	102.61	4.03	100	100	101.06	102.97	104	105	108.7
Call Amount	911	186,684.05	207,837	2,065	20,000	60,000	148,647	250,000	390,000	999,413
Amount Outstanding	911	222,922.77	263,939	5,481	30,000	89,769	163,175	256,230	420,000	1,249,324
Last Trade Price	911	102.18	6.75	97.03	100	100.38	102.76	104	105.06	112.56
eff_trd_d_diff	911	9.31	7.56	1	2	4	7	13	21	30
Call Premium	909	-0.38	7.06	-5.98	-1	-0.17	0	0.1	0.36	4.1
Proportion Called	908	66%	40%	0%	10%	28%	85%	97%	100%	100%
Cash	911	1,196.59	10,001.24	0.05	6.73	33.22	132.78	455.5	1,431.03	9,060.60
Refinance	911	0.11	0.32	0	0	0	0	0	1	1
Investment Grade	911	0.2	0.4	0	0	0	0	0	1	1
Number of Covenants	866	4.81	1.77	0	2	4	6	6	6	7
Bond Age	911	0.56	0.19	0.15	0.33	0.42	0.53	0.7	0.82	0.98
Combo Call	911	0.38	0.49	0	0	0	0	1	1	1

Panel B. Make-Whole Call Provisions

This table present the summary statistics for make-whole call exercises. Detailed variable definitions are in Appendix A.

Variable	N	Mean	Standard Deviation	1%	10%	25%	50%	75%	90%	99%
Offering Amount	686	600,618	525,482	100,000	200,000	300,000	450,000	750,000	1,174,930	2,866,715
Make-Whole Spread	684	35.2	14.94	10	15	20	35	50	50	75
Call Price	670	105.15	8.75	100	100.18	101.36	103.96	108.42	112.74	126.82
Call Amount	686	418,499	438,377	1,987	52,500	150,000	300,000	500,000	900,000	2,060,000
Amount Outstanding	686	468,450	459,678	4,210	103,478	200,000	345,834	575,000	1,000,000	2,415,000
Last Trade Price	686	105.43	5.48	99	100.4	101.43	104.2	108.02	112.19	124.99
eff_trd_d_diff	686	7.07	6.87	1	1	2	5	9	18	28.15
Call Premium	670	0.28	7.42	-8.16	-1.99	-0.82	-0.17	0.04	1.04	11.32
Proportion Called	668	69%	36%	1%	12%	32%	89%	97%	99%	100%
Cash	686	2555.68	5105.6	0.25	52.27	222.13	866.5	2630	6351	29338.3
Refinance	686	0.18	0.39	0	0	0	0	0	1	1
Investment Grade	686	0.64	0.48	0	0	0	1	1	1	1
Number of Covenants	663	4.01	1.46	1	2	3	4	5	6	7
Bond Age	686	0.71	0.26	0.04	0.3	0.51	0.8	0.92	0.96	0.99
Combo Call	686	0.21	0.41	0	0	0	0	0	1	1

Table 3. Heckman Correction: Probability of Call

This table presents an estimation of the probability of call with the following specification :

$$Call_{it} = \Phi(\alpha + \beta X_{it} + \delta_t + \varepsilon_{it}).$$

$Call_{it}$ is a dummy variable equal to 1 if bond i is called in year t, X_{it} is a vector of predictor variables of the probability of a call exercise for bond i in year t, and δ_t indicates year dummies. The detailed definition of variables in predictor vector X_{it} is in Appendix A.

	Fixed-Price Call Provisions			Make -Whole Call Provisions		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Coefficient	AME	MEM	Coefficient	AME	MEM
Fixed-Price Call Price	0.022*	0.006*	0.008*			
	(1.887)	(1.889)	(1.918)			
In-the-money	1.165***	0.327***	0.414***			
	(11.390)	(12.618)	(11.297)			
Make-Whole Spread				0.018***	0.001***	0.000***
				(10.556)	(10.206)	(8.345)
Trade Price	-0.003	-0.001	-0.001	-0.017***	-0.001***	-0.000***
	(-0.245)	(-0.245)	(-0.246)	(-5.244)	(-5.204)	(-4.958)
Rating	-0.090***	-0.025***	-0.032***	0.004	0.000	0.000
	(-7.316)	(-7.639)	(-7.310)	(0.446)	(0.445)	(0.446)
Rating Change	-0.032	-0.009	-0.011	-0.070**	-0.003**	-0.001**
	(-0.837)	(-0.838)	(-0.838)	(-2.325)	(-2.322)	(-2.301)
Bond Age	1.316***	0.369***	0.468***	2.615***	0.100***	0.035***
	(5.352)	(5.470)	(5.330)	(26.632)	(21.575)	(13.349)
Number of Covenants	0.091***	0.026***	0.032***	0.069***	0.003***	0.001***
	(3.680)	(3.720)	(3.689)	(3.839)	(3.818)	(3.833)
Cash	0.021	0.006	0.007	0.044***	0.002***	0.001***
	(1.331)	(1.332)	(1.331)	(4.300)	(4.278)	(4.158)
Ten Year Treasury	-0.107	-0.030	-0.038	-0.134	-0.005	-0.002
	(-0.977)	(-0.977)	(-0.977)	(-1.072)	(-1.071)	(-1.082)
Treasury Slope	0.213	0.060	0.076	0.014	0.001	0.000
	(0.964)	(0.965)	(0.964)	(0.093)	(0.093)	(0.093)
Baa-Aaa Spread	-0.122	-0.034	-0.044	-0.210	-0.008	-0.003
	(-0.239)	(-0.239)	(-0.239)	(-0.529)	(-0.529)	(-0.531)
Constant	-0.873			-2.138***		
	(-0.795)			(-3.682)		
Year Dummies	YES			YES		
Pseudo R-squared	0.2596			0.2524		
Observations	2,058			27,915		

Table 4. Determinants of Proportion Called: Fixed-Price Call Provision

This table presents the empirical results of Hypotheses 1, 2, and 3 for fixed-price call provisions. The dependent variable is the proportion called in a call exercise. It is measured as a percentage of the amount initially offered in Panel A and a percentage of the amount remaining outstanding prior to the call exercise in Panel B. All non-dummy variables are winsorized at the 1% and 99% levels. Column (1) and Column (2) show the coefficient of regression without (Plain) and with the Heckman correction. Column (3) and Column (4) present the average marginal effect (AME) and conditional marginal effect at means (MEM) of Column (2). *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Proportion of Amount Initially Offered

	(1)	(2)	(3)	(4)
VARIABLES	Plain	Heckman	AME	MEM
Call Premium	0.070	0.191***	0.027***	0.043***
	(0.902)	(3.354)	(3.343)	(3.338)
Ln(Cash)	0.089***	0.059*	0.008*	0.013*
	(3.358)	(1.871)	(1.856)	(1.864)
Refinance	1.018***	1.020***	0.142***	0.230***
	(6.982)	(6.035)	(5.869)	(6.038)
Ln(Offering Amount)	-2.280***	-2.309***	-0.322***	-0.521***
	(-22.256)	(-21.771)	(-26.898)	(-22.216)
Ln(Amount Outstanding)	1.840***	1.768***	0.247***	0.399***
	(23.501)	(22.054)	(32.633)	(22.132)
Bond Age	-0.437	-2.156***	-0.301***	-0.486***
	(-1.203)	(-4.553)	(-4.496)	(-4.540)
Call Price	-0.101***	-0.115***	-0.016***	-0.026***
	(-3.171)	(-2.840)	(-2.831)	(-2.833)
eff_trd_d_diff	0.020***	0.021***	0.003***	0.005***
	(2.852)	(2.735)	(2.729)	(2.726)
Investment Grade	1.026***	0.560***	0.078***	0.126***
	(7.045)	(3.374)	(3.328)	(3.387)
10 Year Treasury Rate	0.321	-0.109	-0.015	-0.024
	(1.560)	(-0.429)	(-0.429)	(-0.430)
Yield Curve Slope	-0.391**	0.084	0.012	0.019
	(-1.973)	(0.340)	(0.340)	(0.340)
Baa-Aaa Spread	-0.333	-0.082	-0.011	-0.019
	(-1.001)	(-0.263)	(-0.263)	(-0.264)
Mills Ratio		0.683***	0.095***	0.154***
		(3.462)	(3.444)	(3.450)
Constant	6.340*	9.258**		
	(1.769)	(2.040)		
Year Dummies	YES	YES	YES	YES
Observations	902	702	702	702

Panel B. Proportion of Amount Remaining Outstanding Prior to the Call Exercise

VARIABLES	(1) Plain	(2) Heckman	(3) AME	(4) MEM
Call Premium	0.094	0.333**	0.025**	0.012**
	(0.702)	(2.138)	(2.226)	(2.290)
Ln(Cash)	0.186***	0.157***	0.012***	0.006**
	(3.852)	(2.602)	(2.608)	(2.407)
Refinance	2.026***	2.022***	0.150***	0.075***
	(5.030)	(4.413)	(4.346)	(3.648)
Ln(Offering Amount)	0.260	0.229	0.017	0.008
	(1.483)	(1.067)	(1.066)	(1.055)
Ln(Amount Outstanding)	-0.727***	-0.960***	-0.071***	-0.035***
	(-5.237)	(-5.076)	(-5.184)	(-4.281)
Bond Age	-0.664	-3.745***	-0.278***	-0.138***
	(-0.998)	(-3.809)	(-3.900)	(-3.708)
Call Price	-0.072	-0.106	-0.008	-0.004
	(-1.204)	(-1.175)	(-1.179)	(-1.175)
eff_trd_d_diff	0.061***	0.063***	0.005***	0.002***
	(3.114)	(2.616)	(2.698)	(2.620)
Investment Grade	2.994***	2.559***	0.190***	0.094***
	(4.363)	(3.181)	(3.076)	(3.877)
10 Year Treasury Rate	0.456	-0.209	-0.015	-0.008
	(1.224)	(-0.409)	(-0.410)	(-0.414)
Yield Curve Slope	-0.447	0.228	0.017	0.008
	(-1.222)	(0.482)	(0.482)	(0.482)
Baa-Aaa Spread	-0.558	-0.291	-0.022	-0.011
	(-0.824)	(-0.423)	(-0.425)	(-0.431)
Mills Ratio		0.802*	0.060*	0.030*
		(1.868)	(1.866)	(1.751)
Constant	14.540**	22.646**		
	(2.111)	(2.110)		
Year Dummies	YES	YES	YES	YES
Observations	932	724	724	724

Table 5. Determinants of Proportion Called: Make-Whole Call Provision

This table presents the empirical results of Hypotheses 2 and 3 for make-whole call provisions. The dependent variable is the proportion called in a call exercise. It is measured as a percentage of amount initially offered in Panel A and a percentage of amount remaining outstanding prior to the call exercise in Panel B. All non-dummy variables are winsorized at the 1% and 99% level. Column (1) and Column (2) show the coefficient of regression without (Plain) and with the Heckman correction. Column (3) and Column (4) present the average marginal effect (AME) and conditional marginal effect at means (MEM) of Column (2). *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Proportion of Amount Initially Offered

VARIABLES	(1) Plain	(2) Heckman	(3) AME	(4) MEM
Ln(Cash)	0.138***	0.085*	0.012*	0.018*
	(4.163)	(1.827)	(1.821)	(1.821)
Refinance	0.478***	0.511***	0.073***	0.109***
	(3.495)	(3.299)	(3.287)	(3.310)
Ln(Offering Amount)	-2.490***	-2.436***	-0.347***	-0.519***
	(-20.149)	(-17.803)	(-19.747)	(-16.836)
Ln(Amount Outstanding)	2.326***	2.235***	0.318***	0.476***
	(23.505)	(21.265)	(28.705)	(20.195)
Bond Age	1.306***	0.836**	0.119**	0.178**
	(5.708)	(1.967)	(1.974)	(1.963)
Make-whole Spread	-0.010**	-0.010**	-0.001**	-0.002**
	(-2.382)	(-2.074)	(-2.071)	(-2.059)
Call Price	-0.031***	-0.035***	-0.005***	-0.008***
	(-2.825)	(-2.807)	(-2.858)	(-2.862)
eff_trd_d_diff	0.024***	0.017*	0.002*	0.004*
	(2.975)	(1.884)	(1.879)	(1.878)
Investment Grade	0.369***	0.625***	0.089***	0.133***
	(2.945)	(4.553)	(4.565)	(4.534)
10 Year Treasury Rate	0.187	0.312	0.044	0.066
	(0.598)	(0.953)	(0.953)	(0.953)
Yield Curve Slope	0.028	-0.262	-0.037	-0.056
	(0.083)	(-0.731)	(-0.730)	(-0.730)
Baa-Aaa Spread	0.266	-0.188	-0.027	-0.040
	(0.804)	(-0.426)	(-0.426)	(-0.426)
Mills Ratio		1.321	0.188	0.281
		(1.365)	(1.359)	(1.358)
Constant	-6.140***	-5.242*		
	(-2.820)	(-1.864)		
Year Dummies	YES	YES	YES	YES
Observations	674	505	505	505

Panel B. Proportion of Amount Remaining Outstanding Prior to the Call Exercise

VARIABLES	(1) Plain	(2) Heckman	(3) AME	(4) MEM
Ln(Cash)	0.209***	0.096	0.006	0.004
	(3.563)	(1.069)	(1.054)	(1.034)
Refinance	1.178***	1.187**	0.073**	0.047***
	(2.618)	(2.422)	(2.448)	(2.618)
Ln(Offering Amount)	0.681**	0.504	0.031	0.020
	(2.273)	(1.548)	(1.583)	(1.639)
Ln(Amount Outstanding)	-0.972***	-0.876***	-0.054***	-0.034***
	(-4.943)	(-4.230)	(-4.172)	(-4.066)
Bond Age	2.214***	0.805	0.050	0.032
	(5.256)	(0.822)	(0.824)	(0.815)
Make-whole Spread	-0.031***	-0.032***	-0.002***	-0.001**
	(-2.960)	(-2.603)	(-2.634)	(-2.540)
Call Price	0.026	0.024	0.002	0.001
	(0.890)	(0.709)	(0.714)	(0.709)
eff_trd_d_diff	0.059***	0.055**	0.003**	0.002**
	(2.891)	(2.079)	(2.040)	(1.991)
Investment Grade	0.795***	1.330***	0.082***	0.052***
	(2.677)	(4.090)	(4.044)	(3.874)
10 Year Treasury Rate	-0.032	0.046	0.003	0.002
	(-0.209)	(0.268)	(0.268)	(0.267)
Yield Curve Slope	-0.503***	-0.633***	-0.039***	-0.025***
	(-2.666)	(-3.017)	(-3.045)	(-3.057)
Baa-Aaa Spread	-1.111**	-1.197**	-0.074**	-0.047**
	(-2.454)	(-2.015)	(-2.050)	(-2.020)
Mills Ratio		4.390		
		(1.277)		
Constant	6.028	1.936		
	(1.406)	(0.280)		
Year Dummies	NO	NO	NO	NO
Observations	684	513	513	513

Table 6. The Impact of Covenants on Proportion Called: Fixed-Price Call Provision

This table presents the impact of covenants on proportion called for fixed-price call provisions. The dependent variable is the proportion called in a call exercise. It is measured as a percentage of the amount initially offered in Panel A and a percentage of the amount remaining outstanding prior to the call exercise in Panel B. All non-dummy variables are winsorized at the 1% and 99% levels. Column (1) and Column (2) show the coefficient of regression without (Plain) and with the Heckman correction. Columns (3) and Column (4) present the average marginal effect (AME) and conditional marginal effect at the means (MEM) of Column (2). *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Proportion of Amount Initially Offered

VARIABLES	(1) Plain	(2) Heckman	(3) AME	(4) MEM
Number of Covenants	-0.100	-0.126	-0.016	-0.026
	(-0.972)	(-1.257)	(-1.301)	(-1.327)
Pure Fixed-Price Call	-0.562	-0.479	-0.056**	-0.092**
	(-0.838)	(-0.723)	(-2.527)	(-2.320)
Number of Covenants x	0.002	0.015	0.000	-0.000
Pure Fixed-Price Call	(0.021)	(0.128)	(0.024)	(-0.018)
Controls	YES	YES	YES	YES
Year Dummies	YES	YES	YES	YES
Observations	840	702	702	702

Panel B. Proportion of Amount Remaining Outstanding Prior to the Call Exercise

VARIABLES	(1) Plain	(2) Heckman	(3) AME	(4) MEM
Number of Covenants	-0.374	-0.337	-0.018	-0.007
	(-0.774)	(-0.664)	(-0.732)	(-1.121)
Pure Fixed-Price Call	-2.170	-0.830	-0.060**	-0.028*
	(-0.721)	(-0.256)	(-2.253)	(-1.774)
Number of Covenants x	0.192	0.001	-0.011	-0.008
Pure Fixed-Price Call	(0.381)	(0.003)	(-0.378)	(-0.805)
Controls	YES	YES	YES	YES
Year Dummies	YES	YES	YES	YES
Observations	840	702	702	702

Table 7. The Impact of Covenants on Proportion Called: Make-Whole Call Provision

This table presents the impact of covenants on proportion called for make-whole call provisions. The dependent variable is the proportion called in a call exercise. It is measured as a percentage of amount initially offered in Panel A and a percentage of amount remaining outstanding prior to the call exercise in Panel B. All non-dummy variables are winsorized at the 1% and 99% levels. Column (1) and Column (2) show the coefficient of regression without (Plain) and with Heckman correction. Column (3) and Column (4) present the average marginal effect (AME) and conditional marginal effect at means (MEM) of Column (2). *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Proportion of Amount Initially Offered

VARIABLES	(1) Plain	(2) Heckman	(3) AME	(4) MEM
Number of Covenants	0.105***	0.113**	0.016**	0.024**
	(2.597)	(2.295)	(2.321)	(2.298)
Controls	YES	YES	YES	YES
Year Dummies	YES	YES	YES	YES
Observations	654	505	505	505

Panel B. Proportion of Amount Remaining Outstanding Prior to the Call Exercise

VARIABLES	(1) Plain	(2) Heckman	(3) AME	(4) MEM
Number of Covenants	0.283***	0.285**	0.017**	0.011***
	(2.780)	(2.439)	(2.562)	(2.663)
Controls	YES	YES	YES	YES
Year Dummies	NO	NO	NO	NO
Observations	840	702	702	702

Table 8. The Impact of Refinance: Investment-Grade vs. Speculative-Grade, Fixed-Price Call Provisions

This table presents the impact of refinance on proportion called for fixed-price call provisions based on the rating status. The dependent variable is the proportion called in a call exercise. It is measured as a percentage of the amount initially offered in Panel A and as a percentage of the amount remaining outstanding prior to the call exercise in Panel B. All non-dummy variables are winsorized at the 1% and 99% levels. Column (1) shows the coefficient of regression with Heckman correction. Column (2) and Column (3) present the average marginal effect (AME) and conditional marginal effect at means (MEM) of Column (1). *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Proportion of Amount Initially Offered

	(1)	(2)	(3)
VARIABLES	Heckman	AME	MEM
Refinance	1.119***	0.147***	0.235***
	(5.823)	(5.981)	(5.659)
Investment Grade	0.420**	0.052**	0.090**
	(2.402)	(2.271)	(2.414)
Refinance x Investment Grade	-0.542*	0.013	0.015
	(-1.739)	(-0.084)	(-0.133)
Controls	YES	YES	YES
Year Dummies	YES	YES	YES
Observations	702	702	702

Panel B. Proportion of Amount Remaining Outstanding Prior to the Call Exercise

	(1)	(2)	(3)
VARIABLES	Heckman	AME	MEM
Refinance	2.086***	0.160***	0.066
	(4.511)	(4.570)	(1.533)
Investment Grade	2.064***	0.180***	0.056*
	(2.752)	(3.247)	(1.723)
Refinance x Investment Grade	9.195***	0.016	-0.050*
	(9.374)	(0.117)	(1.660)
Controls	YES	YES	YES
Year Dummies	YES	YES	YES
Observations	724	724	724

Table 9. The Impact of Refinancing: Investment-Grade vs. Speculative-Grade, Make-Whole Call Provisions

This table presents the impact of refinancing on proportion called for make-whole call provisions based on the rating status. The dependent variable is the proportion called in a call exercise. It is measured as a percentage of the amount initially offered in Panel A and a percentage of the amount remaining outstanding prior to the call exercise in Panel B. All non-dummy variables are winsorized at the 1% and 99% levels. Column (1) shows the coefficient of regression with Heckman correction. Column (2) and Column (3) present the average marginal effect (AME) and conditional marginal effect at means (MEM) of Column (1). *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Proportion of Amount Initially Offered

	(1)	(2)	(3)
VARIABLES	Heckman	AME	MEM
Refinance	1.477***	0.087***	0.352***
	(5.067)	(4.120)	(5.051)
Investment Grade	0.867***	0.091***	0.190***
	(5.781)	(4.768)	(5.711)
Refinance x Investment Grade	-1.347***	-0.210***	-0.327***
	(-4.134)	(-4.493)	(-4.372)
Controls	YES	YES	YES
Year Dummies	YES	YES	YES
Observations	505	505	505

Panel B. Proportion of Amount Remaining Outstanding Prior to the Call Exercise

	(1)	(2)	(3)
VARIABLES	Heckman	AME	MEM
Refinance	3.144***	0.110***	0.149***
	(3.029)	(3.301)	(9.993)
Investment Grade	1.654***	0.078***	0.049
	(4.657)	(4.502)	(.)
Refinance x Investment Grade	-2.666**	-0.259***	-0.035***
	(-2.294)	(-2.919)	(-3.853)
Controls	YES	YES	YES
Year Dummies	YES	YES	YES
Observations	513	513	513

Chapter 2:

What Happens to Bond Liquidity When Some Bonds of the Issuer Mature?

What Happens to Bond Liquidity When Some Bonds of the Issuer Mature?

Abstract: Bond portfolio managers constantly worry about the liquidity of their portfolio. Consider a bond portfolio manager that holds bonds of a particular firm that has numerous maturities outstanding. Assume that some bonds of the issuing firm mature. Do the firm's remaining bonds become more liquid or less liquid? We analyze the impact of the maturity of a firm's bonds upon the liquidity of the firm's remaining bonds where a reduction in the number of bonds outstanding suggests a potential reduction in liquidity. Alternatively, the leverage reduction due to the reduction in number of bonds outstanding may improve the credit quality of the firm and result in greater liquidity. Our results strongly suggest the former where the strength of reduction depends on the ratio of the dollar amount matured to total debt. The results have important implications for how to hedge the portfolio against interest changes.

Introduction

Corporate bond portfolio managers need to manage the liquidity risk of their portfolios. Taking into account the other complex factors that affect bond liquidity, we choose to focus upon how a very frequent important event affects liquidity.[2] That is, we analyze the impact of the realized scheduled maturation of a corporate bond upon the liquidity of other bonds of the firm. Bond portfolio managers should be aware of how the *realized scheduled maturity* of a corporate bond may affect the liquidity of other bonds previously issued by the same firm that remain outstanding. If these bonds are expected to become less liquid, for reasons we describe below, the portfolio manager may decide to sell holdings before the scheduled maturation. Or, a portfolio manager attempting to hedge his bond positions may wish to adjust their hedge strategy to respond to the greater volatility that often accompanies lesser liquidity. Of course, portfolio managers may hope the liquidity of their holdings does not decline or, even better, increases.

We note that a bond retirement can occur in multiple ways besides realized scheduled maturation. More specifically, a firm can retire its outstanding bonds via embedded call option executions, tender offers, or open market repurchases.[3] We focus on realized scheduled maturations, hereafter RSM, in this paper because of natural exogeneity. That is, a maturity is naturally exogenous because the RSM of a bond is pre-determined at the issuance of the bond and is not affected by any other events during the lifespan of the bond. Of course, our analysis of the exogeneity with regard to bond investors acting in the secondary market is in contrast to the endogenous maturity decision of the firm at the time of bond issuance. The RSM of a bond is a very clean event compared to other

[2] Bao, Pan, and Wang (2011) analyze the liquidity of corporate bonds at the individual bond level and, also, at the aggregate level while Helwege, Huang, and Wang (2014) attempt to isolate the part of corporate bond spreads due to differential liquidity. Schestag, Schuster, and Uhrig-Homburg (2016) compare the numerous measures of bond liquidity that have been developed. Chen, Huang, Sun, Yao, and Yu (2020) analyze the liquidity of insurance company corporate bond portfolios. Wang and Zhong (2019) attempt to explain puzzling post-crisis liquidity behavior where bid-ask spreads narrow along with reductions in dealer inventory and increased difficulty in executing large trades. Jiang, LI, and Wang (2020) analyze liquidity risk and find that corporate bond mutual funds reduce liquid asset holdings during tranquil market conditions

[3] Additionally, one can suggest that default can crudely represent retirement. However, the date of default can be difficult to define.

ways a corporate bond can be retired where such a clean sample allows us to better focus upon factors which affect liquidity. [4]

The purpose of this research is to help bond portfolio managers analyze the impact of a maturing bond upon the liquidity of other preexisting bonds of the same firm that are still outstanding. The answer is important because a large volume of bonds is always maturing. One logical view is that the RSM will decrease the liquidity of other bonds issued by the same firm that remain outstanding simply because the quantity of similar bonds outstanding has declined. That is, the market may be thinner due to a reduction in trading opportunities and trading. Thus, a bond portfolio manager may be discouraged from holding bonds of firms that have numerous bond issues scheduled to mature in the near future. On the other hand, it may be that the RSM will increase the liquidity of other bonds because the firm has shown that it can service debt and, also, is less levered and thus less risky after the scheduled maturity value is paid off. Therefore, a portfolio manager may be encouraged to trade in bonds of such firms.

Our results tend to support reduced liquidity using both a transactions cost measure and a depth measure. More specifically, both bid-ask spreads and the Amihud illiquidity measures of a bond tend to increase significantly after the RSM of another bond issued by the same firm. This result is consistent across different event windows including windows of one day, one week, one month, and three months. Also, we find that the impact of an RSM on the liquidity of other bonds issued by the same firm is stronger when the ratio of retired bonds to total debt of the firm is higher, the remaining bond(s) were more recently issued, and the maturity of the remaining bond(s) is longer.

The next section develops our hypotheses for the impact of a maturing bond on the liquidity of other bonds of the same firm in detail. We draw upon theoretical arguments by Chen, Cui, He, and Milbradt (2018) and Dass and Massa (2014) to illustrate opposing hypotheses. The following sections describe our data and the computational processes. We note that our sample carefully matches observed maturities with similar corporate bonds. Then our results section describes how, dependent upon several factors, the liquidity of a firm's bonds is affected by the RSM of an issue. Finally, we summarize our research in the conclusion.

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[3] Xu (2018) maintains that bond retirement before maturity is commonly done to reduce interest costs and that lower credit quality firms are more inclined to refinance before maturity.

Hypotheses

Many researchers have analyzed the liquidity of different financial instruments in recent years. It is quite popular to analyze the cross sectional and time series variation of liquidity measures where researchers may focus on how these variations are explained by different characteristics such as the amount of the financial instrument outstanding and the credit quality of the issuer.

Kothare (1997) analyzes the bid-ask spread of common stock after a publicly underwritten offering of seasoned equity and shows that the bid-ask spread *decreases* where this likely occurs because the increase in shares outstanding leads to a wider and more dispersed market. In contrast to the above, Kothare (1997) also shows that the bid-ask of common stock *increases* after a rights offering which is attributed to the more concentrated ownership structure of the rights issue. [5]

Chen and Stock (2021) present alternative hypotheses for the potential effects of a new corporate bond issuance upon the liquidity of the firm's preexisting bonds. That is, the liquidity of preexisting bonds may *increase* because there is an increase in the quantity of bonds outstanding. Furthermore, Chen and Stock (2021) note that, upon issuance, firms must release public information required by regulators and rating agencies. Thus, liquidity may also increase due to greater information about the firm where such information is used by underwriters, investment bankers and others potentially involved in a new issuance. On the other hand, they suggest that the liquidity of preexisting bonds could *decrease* upon a new bond issuance due to investment bankers aggressively attempting to sell the new issuance. That is, investment banker performance is based upon how well the new issuances sell. Thus, there may be relatively less attention paid to marketing preexisting bonds where this would lead to a decrease in their liquidity. Furthermore, the firm becomes more levered and riskier if new debt is issued in which case Goldstein and Hotchkiss (2020) and Ho and Stoll (1981) show that dealers are typically less willing to act as a market maker thus leading to reduced liquidity and increases in bid-ask spreads for preexisting bonds.

We note a considerable literature that relates the risk and credit quality of the firm to bond liquidity. For example, Edwards, Harris, and Piowar (2007) find that bonds with a credit rating above BBB have considerably greater liquidity than bonds rated BBB and below. Similarly, Bao, Pan, and

[5] With respect to corporate bonds, Helwege, Huang, and Wang (2014) attempt to separate the credit spread component from the liquidity component by using bonds of the same firm trading on the same day. They note that bond liquidity is also often estimated by the size (face value) of the issue.

Wang (2011) find that their measure of illiquidity, γ, indicates that bonds of higher ratings are more liquid than those with lower ratings. Dick-Nielsen, Feldhutter, and Lando (2012) find a similar relationship and note that bonds with a higher rating are particularly more liquid than lower grade bonds during a crisis such as that of 2007-2009.

Chen, Cui, He, and Milbradt (2018) analyze the total credit spread, CS, and decompose it. Most relevant to our work, the liquidity component has two parts as given by the last two terms in the below equation: a pure liquidity component, $CS_{pureLIQ}$, and a default driven liquidity component, $CS_{DEF\rightarrow LIQ}$.

$$CS = CS_{pureDEF} + CS_{LIQ\rightarrow DEF} + CS_{pureLIQ} + CS_{DEF\rightarrow LIQ} \qquad (1)$$

In this equation, the pure liquidity component, $CS_{pureLIQ}$, represents the spread solely due to liquidity, and the default-driven liquidity component, $CS_{DEF\rightarrow LIQ}$, represents how the credit spread is affected by default driven liquidity. They show that both $CS_{pureLIQ}$ and $CS_{DEF\rightarrow LIQ}$ contribute a significant piece of the total credit spread and related spread changes. Thus, it is important to consider both components when studying the events that may lead to bond liquidity changes.

The impact upon the liquidity of remaining bonds outstanding when a particular bond matures could be different on the pure liquidity component versus the default-driven liquidity component. On one hand, upon maturity, there are fewer bonds outstanding, tending to reduce the pure liquidity component. On the other hand, Chen, Cui, He, and Milbradt (2018) and others maintain that bonds of less risky firms are typically more liquid. That is, upon maturity, the leverage of a firm decreases, potentially leading to a lower default probability and thus a lesser default-driven liquidity component.

In this study, we analyze the impact of a realized scheduled maturity (RSM) upon the liquidity of other bonds of the same firm which remain outstanding. For example, Abbott Laboratories issued bonds, FISD issue 80792, at par value $1,000 on September 10, 1998 where the bonds matured on September 15, 2008. At the maturity date, what was the effect on liquidity of other Abbott bonds that remained outstanding? Clearly, the RSM decreases the quantity of similar instruments of the firm outstanding while reducing the leverage and credit risk of the firm. Thus, we present these competing hypotheses.

Hypothesis 1a. The scheduled maturity of an individual corporate bond will decrease the liquidity of other bonds of the firm that remain outstanding because the volume of outstanding bonds available to trade in the market, decreases.

Hypothesis 1b. The scheduled maturity of an individual corporate bond will increase the liquidity of other bonds of the firm that remain outstanding because the firm is less levered and less risky.

Note that no matter which of the potential impacts above is true, one would expect that the relative size of the RSM bond plays an important role. That is, if the liquidity of a corporate bond decreases because the outstanding volume of the bonds issued by the firm decreases, then the liquidity should decrease more when the relative size of the RSM bond is larger. Similarly, if the liquidity of a corporate bond improves because of the firm is less levered, then the liquidity should improve more when the relative size of the RSM bond is larger. Based on the above, we propose hypothesis 2 concerning the relative size of the RSM bond as a proportion of the total long-term debt of the firm.

Hypothesis 2a. In the context of hypothesis 1a, given the firm's total debt level, the greater (lesser) the amount of bonds that has just matured, the lesser (greater) the quantity of similar bonds remain outstanding and the greater (weaker) the reduction in liquidity.

Hypothesis 2b. In the context of hypothesis 1b, given the firm's total debt level, the greater (lesser) the amount of bonds that has just matured, the greater (lesser) the reduction of the riskiness of the firm and the greater (weaker) the increase in liquidity.

Several studies have addressed the impact of the age a of a bond upon liquidity. Sarig and Warga (1989) find that a government bond tends to be less liquid as its age increases because a higher fraction of the bonds has been locked into investors' inactive portfolios. They show that this effect tends to be more permanent than temporary. Later studies, such as Helwege, Huang, and Wang (2014) and Richardson and Palhares (2019) find similar results with respect to age for corporate bonds. Thus, we hypothesize that the liquidity of a bond of greater age is likely less affected by events such as the maturation of other bonds.

Hypothesis 3. Bonds of a greater age are less affected by the maturation of other bonds of the firm. The age of the bond is the time elapsed since issuance. With regard to hypothesis 1a, the decline in *liquidity is weaker the greater the age of the existing bond(s) of the firm. With regard to hypothesis 1b, the increase in liquidity is weaker the greater the age of the existing bond(s) of the firm.*

Dass and Massa (2014) maintain that bond investors are very aware of the search costs involved in bond investing and strive to minimize such search costs. Thus, investors frequently prefer to concentrate their bond investments in relatively few firms because the search cost will be smaller compared to investing in a large number of different firms. Furthermore, the bond investors prefer to diversify their holdings with respect to maturity and thus hold a variety of different maturities offered by the same issuer. Relatedly, our argument is that when a bond matures, the investor may be inclined to invest in other bonds of the same firm in order to minimize search costs. The other bonds of the same firm will tend to be short maturity in order to roughly replace the short maturity bonds that have just matured and maintain a diversity of maturities, both short term and longer term. In summary, when a bond matures, there is demand for short term bonds of the same firm so that the liquidity of existing short-term bonds may not decline as much as the liquidity of outstanding longer-term bonds of the same firm.

Hypothesis 4. Many investors wish to maintain approximate portfolio positions in the bonds of the issuing firm and thus tend to purchase short-term bonds of the firm to replace bonds that have matured. Thus, the shorter maturity bonds of the firm will enjoy relatively stronger demand and greater liquidity than longer maturity bonds of the firm.

Data and Methodology

We collect corporate bond maturations that occur from December 1984 to October 2020 from the Mergent Fixed Income Securities Database (FISD). Then we find other bonds issued by the same firm that remain outstanding after the above maturation. To be included in the remaining outstanding post-maturation sample, we require the bond to be non-callable, non-puttable, non-convertible, and non-asset backed.

To compute the liquidity of the non-maturing corporate bonds around the date of the maturation, we collect intra-day trade data from the Trade Reporting and Compliance Engine (TRACE). This data base began providing comprehensive intra-day bond transaction data for AAA, AA, A, and BBB rated bonds whose size at issuance was greater than $1 billion in July 2002. Since then, TRACE has expanded its data collection to include other grades of bonds. In this research, we use the TRACE Enhanced database because it provides not only the price but also the volume in each transaction.

We clean the TRACE Enhanced database following the processes of Dick-Nielsen (2009, 2014) for duplicate trades, withdrawn trades, and corrected trades. Also, we follow Schestag, Schuster, and Uhrig-Homburg (2016) and exclude trades under special conditions, trades taking place on a holiday, trades before bond origination, and trades after the bond is fully repaid. Next, we access accounting information of the issuing firm from the Compustat Fundamentals Annual database. This information is used to control for firm-specific characteristics.

Bid-ask spread changes and Amihud measures are calculated with the high-frequency data from the enhanced TRACE database following the procedure of Schestag, Schuster, and Uhrig-Homburg (2016). The traded bid-ask spread is the difference between the average daily selling price and average daily buying price divided by their sum. Assigning i to each individual bond and t to time periods, we have

$$Bid - Ask\ Spread_{it} = \frac{\overline{Sell}_{it} - \overline{Buy}_{it}}{\overline{Sell}_{it} + \overline{Buy}_{it}}, \quad (2)$$

where $\overline{Sell}_{it}$ and $\overline{Buy}_{it}$ are the average daily selling price and buying price, respectively. We calculate the spread each day on which there is at least one buy and one sell trade and use the weekly, monthly, and quarterly mean as weekly, monthly, and quarterly transaction cost measures. Dick-Nielsen, Feldhütter, and Lando (2012) and Schestag, Schuster and Uhrig-Homburg (2016) use the same procedure.

We use the Amihud (2002) illiquidity measure as a proxy for the depth dimension of liquidity and estimate it as follows

$$Amihud_{id} = \frac{100}{T} \times \sum_{t=2}^{n} \frac{abs\left(ln(p_{i,t,d}) - ln(p_{i,t-1,d})\right)}{q_{i,t,d}/1{,}000{,}000} \quad (3)$$

The numerator in the summation is the absolute (abs) change in the natural logarithm of the price from trade t-1 to trade t. Here *q* indicates the dollar volume of trade t for bond *i* on day *d* and T is the number of trades. This measure captures the change in price for a given quantity traded which is what the theoretical model of Kyle (1985) suggests. Weekly, monthly, and quarterly measures of the price impact are obtained as the means of daily measures.

We note that the Amihud illiquidity measure is a relatively unstable liquidity measure compared to other liquidity measure because it represents the price impact per unit traded. Schestag, Schuster, and Uhrig-Homburg (2016) confirm such by showing that the average magnitude of the standard deviation of the Amihud's illiquidity measure is about 2.5 times the average magnitude of the mean. This multiple is significantly larger compared to those of the bid-ask spread and other liquidity measures. Such behavior may make finding significant patterns in the Amihud measure more challenging than for bid-ask and other liquidity measures.

Traditional bond market studies typically focus on the abnormal bond return. A widely accepted procedure to compute the abnormal bond return is

$$ABR_i = R_i - BMR_i, \quad (4)$$

where ABR_i is the abnormal return of bond i, R_i is the return of bond i, and BMR_i is the mean return on a benchmark rating and maturity matched portfolio that corresponds to bond i. See Ederington, Guan, and Yang (2015) for details about the process for computing ABR_i. We define abnormal liquidity in a similar way given below.

$$ABL_i = L_i - BML_i, \quad (5)$$

Here ABL_i is the abnormal liquidity of bond i, L_i is the liquidity of bond i, and BML_i is the mean liquidity on a benchmark industry, rating, and maturity matched portfolio that corresponds to bond i. For the matched portfolio, we require the matched bond to have the same 3-digit SIC code, the same S&P rating, and be within the same maturity grouping: 1 to 3 years, 3 to 5 years, 5 to 10 years, and over 10 years.

In Exhibit 1, we provide the key characteristics of the bonds used in the study. The definitions of variables are provided in Appendix A. The median abnormal spread changes are all positive except for the 30-day window. The median 1-day abnormal bid-ask spread change is 0.000102 and the median 1-day abnormal Amihud change is 0.0133. The median age (time since issuance) of the bonds is 3.463 years, the median remaining maturity is 8.286 years, the median retired ratio (amount bonds retired at maturation as a percentage of total bonds outstanding of the issuing firm) is 8.02%.

Results

In Panel A of Exhibit 2, we show the abnormal liquidity changes of the full sample. We first observe that the abnormal bid-ask spread is on average 0.000521 larger one day after the RSM of bonds issued by the same firm compared to one day before the RSM. This result is statistically significant at the 1% level. Such a result suggests that the liquidity of the firm's bonds declines after the RSM of some of the firm's bonds which supports Hypothesis 1a while not supporting Hypothesis 1b. The bid-ask results are similar when we use 7-day, 30-day, and 90-day windows instead of a 1-day window. All results are statistically significant at 1% level except the 30-day window, which is significant at the 5% level. Note that the magnitude of the changes in the bid-ask abnormal liquidity tends to be smaller with greater horizons. For example, the bid-ask change is only 0.000161 for the 7-day window compared to 0.000521 for the 1-day window. This pattern is consistent with the idea that other factors occurring in wider windows may diminish the relative impact of a bond maturity. The Amihud results for Panel A are quite similar where, again, the magnitude of change tends to diminish in the longer windows and the 30-day window is positive but not significant.

We provide further results using only bonds with less than 10 years to maturity in Panel B of Exhibit 2. The bid-ask results are not consistent with Panel A in that the change in abnormal liquidity is negative for 1-, 7-, and 30-day windows. For Amihud measures, the abnormal change is positive for 1- and 7-day windows but negative for 30- and 90-day windows. In contrast to Panel B, both Panel C, containing bonds with remaining maturities between 10 and 20 years, and Panel D, containing bonds with remaining maturities greater than 20 years, illustrate strongly positive and significant results for both bid-ask and Amihud which are consistent with Panel A. Thus, we conclude that the decline in liquidity for the full sample given in Panel A is driven by bonds with the longer maturities given in Panels C and D. The abnormal liquidity change for shorter remaining maturity bonds is unclear. In summary, the results in Exhibit 2 tend to generally support hypothesis 1a, suggesting a decrease in liquidity, as opposed to Hypothesis 1b.

In Exhibit 3, we examine the determinants of abnormal liquidity change and address hypotheses 2, 3, and 4. That is, abnormal liquidity change is regressed on retired amount, age of the bond, remaining maturity of the bond, total assets of the firm, total long- term debt and the VIX index. Exhibit 3a is for bid-ask spread and Exhibit 3b is for the Amihud measure. Regarding Hypothesis 2 and abnormal bid-ask change, we find that the abnormal bid-ask spread change in the 1-day and 90-

day windows is significantly increased by the retired amount as a percentage of the total bonds outstanding of the issuing firm. According to Column 1 in Panel A, a 1% increase in the retired amount of the RSM bond leads to a 0.00233 increase in the 1-day abnormal bid-ask spread. For 90-day abnormal bid-ask spreads, the magnitude of the increase is smaller at 0.000109. For the 1-day window Amihud measure in Exhibit 3b, there is a strongly significant 0.023327 increase while the increase is also significant, but much smaller, at 0.002453 for the 90-day window. Again, we suggest that the longer the event window, the more factors could occur that diminish the impact of a single bond RSM.

Hypothesis 3 proposes that the greater the age of the bond remaining outstanding, the less the liquidity impact caused by the RSM of bonds issued by the same firm. Consistent with this hypothesis, Columns 1 through 4 of Exhibit 3, Panel A, shows that the abnormal bid-ask spread change is strongly reduced by the age of the bond remaining for 1-day, 7-day, 30-day, and 90-day event windows. For example, according to Column 1, a 1% increase in the age of the bond leads to a statistically significant 0.000249 smaller abnormal bid-ask spread change. The results are similar for the Amihud measure in Panel B for the 30-day and 90-day event windows where the coefficients are negative and statistically significant at the 1% level. These results are consistent with hypothesis 3. The change of the abnormal Amihud measure is not statistically sensitive to the age of the bond for 1-day and 7-day event windows. This may be due to the instability of the Amihud measure, as discussed above.

Lastly, hypothesis 4 suggests that the greater the remaining maturity of the outstanding bonds, the greater the reduction in liquidity. Columns 1 to 4 of Exhibit 3, Panel A strongly support this hypothesis. According to Column 1, for the 1-day event window, a 1% increase in the remaining maturity leads to a 0.000742 increase in the bid-ask. This result is statistically significant at the 5% level. The results of 7-day, 30-day, and 90-day event windows are also significant. Amihud results in Exhibit 3, Panel B are quite similar to bid-ask results.

Our results are consistent when we choose to use other groups of fixed effects. For example, in Exhibit 4, we provide the test results when using firm and year fixed effects instead of industry-year fixed effect. The results are similar to those in Exhibit 3.

Conclusion

Recent research has considered the impact of a new corporate bond issuance upon the liquidity of other outstanding bonds of the firm. In contrast, we analyze the impact of a bond maturity upon the liquidity of other outstanding bonds of the firm. We find that the scheduled maturity of a firm's bond tends to decrease the liquidity of other bonds of the firm that are still outstanding where the strength of the decrease is related to the ratio of the dollar amount matured to the firm's total debt. Bonds of a greater age are less affected by the maturation of a bond. Finally, we find that, upon maturity of a firm's bond, the remaining shorter maturity bonds of the firm enjoy relatively greater liquidity than longer maturity bonds of the firm. Our results are useful to those devising hedging strategies for their corporate bond holdings.

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Exhibit 1. Descriptive statistics for Bonds Remaining Outstanding

Bid-Ask spread changes and Amihud changes are calculated with the high-frequency data in the enhanced TRACE database following the procedure of Schestag, Schuster, and Uhrig-Homburg (2016). The abnormal changes are the differences between the abnormal liquidity measures after the bond maturation and the liquidity measures before the bond maturation. The abnormal liquidity measures are defined as the liquidity measure of the target bond less the average liquidity of the benchmark bonds. The periods are 1-day, 7-day, 30-day and 90-day windows around bond maturation. Age is the number of years since the issuance of a bond. Remaining maturity is the number of years between the event date (the date that a bond issued by the same firm was retired) and the maturity date. The retired ratio is the amount bonds retired at maturity as a percentage of total bonds outstanding of the issuing firm where total bonds outstanding is approximated by the *dlttq* term in Compustat. The liquidity changes are winsorized at 1% and 99% level. The definition of all variables is listed in the Appendix.

VARIABLES	N	Mean	SD	$Q_{0.25}$	Median	$Q_{0.75}$
1-Day Abnormal Spread Change	2,378	0.000521	0.00563	-0.00239	0.000102	0.00244
7-Day Abnormal Spread Change	18,153	0.000161	0.00423	-0.00172	0.000673	0.00199
30-Day Abnormal Spread Change	30,191	0.000057	0.00394	-0.00152	-0.000007	0.00154
90-Day Abnormal Spread Change	37,079	0.000269	0.00347	-0.00123	0.000081	0.00153
1-Day Abnormal Amihud Change	6,458	0.0408	0.622	-0.178	0.0133	0.225
7-Day Abnormal Amihud Change	29,390	0.0201	0.384	-0.0869	0.00156	0.103
30-Day Abnormal Amihud Change	38,806	0.000111	0.186	-0.0479	-0.000205	0.0488
90-Day Abnormal Amihud Change	41,856	0.00299	0.134	-0.0345	0.000595	0.0393
Age	41,856	4.380	3.794	1.623	3.463	6.140
Remaining Maturity	41,856	12.97	10.67	4.666	8.286	21.95
RSM Retired Ratio	14,203	26.18%	49.84	1.541%	8.020%	27.82%

Exhibit 2. Univariate Analysis: Changes in Abnormal Liquidity Around the RSM of a Bond

The changes of abnormal liquidity around the RSM of bonds issued by the same firm. The abnormal liquidity changes are defined as the differences between the abnormal liquidity measures after the RSM less the abnormal liquidity measures before the RSM. The first column of results represents the abnormal liquidity for 1 day after less the liquidity of 1 day before. Later columns are for longer windows of 7, 30, and 90 days.

*, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Full Sample					
		1 Day	7 Days	30 Days	90 Days
Spread	Mean	0.000521***	0.000161***	0.000057**	0.000269***
	T-Statistic	(4.51)	(5.12)	(2.52)	(14.95)
	Observations	2,378	18,153	30,191	37,079
Amihud	Mean	0.040755***	0.020130***	0.000111	0.002989***
	T-Statistic	(5.27)	(8.98)	(0.12)	(4.55)
	Observations	6,458	29,390	38,806	41,856

Panel B. Remaining Maturity Less Than 10 Years					
		1 Day	7 Days	30 Days	90 Days
Spread	Mean	-0.000295**	-0.000074**	-0.000064**	0.000221***
	T-Statistic	(-2.55)	(-2.14)	(-2.45)	(10.65)
	Observations	1,480	11,392	18,560	22,237
Amihud	Mean	0.028045***	0.016599***	-0.00376***	-0.000019
	T-Statistic	(2.99)	(6.29)	(-3.55)	(-0.03)
	Observations	4,070	18,040	22,902	24,404

Panel C. Remaining Maturity Between 10 Years And 20 Years					
		1 Day	7 Days	30 Days	90 Days
Spread	Mean	0.002734***	0.000929***	0.000432***	0.000405***
	T-Statistic	(6.16)	(7.28)	(4.56)	(6.02)
	Observations	208	1,575	2,796	3,909
Amihud	Mean	0.043798*	0.035960***	0.010501***	0.011129***
	T-Statistic	(1.68)	(4.52)	(3.20)	(5.06)
	Observations	592	2,968	4,336	4,942

Panel D. Remaining Maturity More Than 20 Years					
		1 Day	7 Days	30 Days	90 Days
Spread	Mean	0.001602***	0.000442***	0.000192***	0.000319***
	T-Statistic	(5.91)	(6.38)	(4.17)	(8.62)
	Observations	690	5,186	8,835	10,933
Amihud	Mean	0.068553***	0.022123***	0.003886*	0.005642***
	T-Statistic	(4.35)	(4.76)	(1.90)	(3.93)
	Observations	1,796	8,382	11,568	12,510

Exhibit 3. Determinants of Abnormal Liquidity Change

This exhibit shows the relationship between the change in the abnormal liquidity of the remaining existing bonds and the three factors in our hypotheses. The abnormal liquidity changes are the difference between the abnormal liquidity measures after the retired bond maturation and the abnormal liquidity measures before the retired bond maturation. The total long-term debt is approximated by the *dlttq* term in Compustat. The 1-day spread change refers to the spread one day after the maturation compared to one day after maturation. We also report results for 7-, 30-, and 90-day relative to maturation. Panel A shows the results of the bid-ask spread, and Panel B shows the results of the Amihud measure.

*, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Bid-ask Spread				
	(1)	(2)	(3)	(4)
VARIABLES	1-Day Abnormal Spread Change	7-Day Abnormal Spread Change	30-Day Abnormal Spread Change	90-Day Abnormal Spread Change
Retired Amount	0.000233*	-0.000069**	0.000006	0.000109***
	(1.71)	(-2.06)	(0.30)	(3.25)
Age	-0.000249**	-0.000072*	-0.000111***	-0.000200***
	(-2.01)	(-1.72)	(-3.84)	(-7.54)
Remaining Maturity	0.000742**	0.000190**	0.000320***	0.000232***
	(2.47)	(2.57)	(5.70)	(5.08)
Total Assets	0.000660	0.000230	-0.000474	-0.000372*
	(0.36)	(0.65)	(-1.26)	(-1.71)
Total Long-Term Debt	-0.000647	-0.000550*	0.000478	0.000331*
	(-0.28)	(-1.76)	(1.27)	(1.67)
VIX	0.000243	0.000014	0.000016	-0.000052***
	(1.64)	(0.71)	(1.22)	(-3.97)
Industry-Year FE	YES	YES	YES	YES
Rating FE	YES	YES	YES	YES
Observations	1,134	7,718	10,910	12,680
R-squared	0.198	0.073	0.069	0.085

Panel B. Amihud				
VARIABLES	(1) 1-Day Abnormal Amihud Change	(2) 7-Day Abnormal Amihud Change	(3) 30-Day Abnormal Amihud Change	(4) 90-Day Abnormal Amihud Change
Retired Amount	0.023327***	0.001627	-0.000092	0.002453***
	(3.59)	(0.75)	(-0.11)	(2.68)
Age	-0.006797	0.004225	-0.004944***	-0.006627***
	(-0.64)	(1.50)	(-4.17)	(-6.92)
Remaining Maturity	0.051098**	0.012175**	0.000333	0.006008***
	(2.16)	(2.06)	(0.15)	(3.84)
Total Assets	-0.373329***	0.007723	0.004421	0.003009
	(-3.08)	(0.22)	(0.49)	(0.45)
Total Long-Term Debt	0.245734**	-0.001682	-0.004410	-0.005167
	(2.21)	(-0.05)	(-0.56)	(-0.91)
VIX	0.000526	0.002027	-0.000596	-0.000390
	(0.09)	(1.50)	(-1.20)	(-1.14)
Industry-Year FE	YES	YES	YES	YES
Rating FE	YES	YES	YES	YES
Observations	2,893	10,858	13,414	14,185
R-squared	0.073	0.063	0.068	0.069

Exhibit 4. Determinants of Abnormal Liquidity Change with Firm and Year Fixed Effects

This exhibit shows the relationship between the change in the abnormal liquidity of the remaining existing bonds and the three factors in our hypotheses with firm and year fixed effect. The abnormal liquidity changes are the difference between the abnormal liquidity measures after the retired bond maturation and the abnormal liquidity measures before the retired bond maturation. The total long-term debt is approximated by the *dlttq* term in Compustat. The 1-day spread change refers to the spread one day after the maturation compared to one day after maturation. We also report results for 7-, 30-, and 90-day relative to maturation. Panel A shows the results of the bid-ask spread, and Panel B shows the results of the Amihud measure.

*, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Bid-ask Spread				
	(1)	(2)	(3)	(4)
VARIABLES	1-Day Abnormal Spread Change	7-Day Abnormal Spread Change	30-Day Abnormal Spread Change	90-Day Abnormal Spread Change
Retired Amount	0.000151	-0.000072**	-0.000007	0.000105***
	(0.99)	(-2.15)	(-0.33)	(3.04)
Age	-0.000135	-0.000016	-0.000088***	-0.000163***
	(-1.20)	(-0.38)	(-3.05)	(-6.29)
Remaining Maturity	0.000963***	0.000227***	0.000328***	0.000250***
	(3.09)	(3.09)	(5.80)	(5.38)
Total Assets	-0.006373	0.001887***	0.000249	0.000057
	(-0.80)	(2.64)	(0.47)	(0.14)
Total Long-Term Debt	-0.012684**	-0.000228	0.000508	0.000054
	(-2.01)	(-0.63)	(1.36)	(0.21)
VIX	0.000382**	0.000012	0.000043***	-0.000046***
	(2.31)	(0.47)	(3.19)	(-2.79)
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Rating FE	YES	YES	YES	YES
Observations	1,124	7,683	10,878	12,649
R-squared	0.254	0.093	0.094	0.105

Panel B. Amihud				
VARIABLES	(1) 1-Day Abnormal Amihud Change	(2) 7-Day Abnormal Amihud Change	(3) 30-Day Abnormal Amihud Change	(4) 90-Day Abnormal Amihud Change
Retired Amount	0.022375***	0.002010	-0.000073	0.002696***
	(3.58)	(0.96)	(-0.09)	(3.08)
Age	-0.004417	0.003361	-0.007313***	-0.007256***
	(-0.40)	(1.28)	(-5.96)	(-7.91)
Remaining Maturity	0.055050**	0.011240*	0.000661	0.005207***
	(2.17)	(1.86)	(0.30)	(3.19)
Total Assets	0.346636*	-0.075176*	0.017442	0.020431
	(1.83)	(-1.66)	(0.98)	(1.50)
Total Long-Term Debt	-0.220975	0.014951	-0.004573	0.002832
	(-1.53)	(0.55)	(-0.43)	(0.41)
VIX	-0.003047	0.004871***	0.000914	0.000371
	(-0.54)	(3.52)	(1.38)	(0.82)
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Rating FE	YES	YES	YES	YES
Observations	2,882	10,823	13,381	14,151
R-squared	0.107	0.094	0.072	0.096

Appendix A. Variable definitions

The variables used in this study are defined in the exhibit below.

Variables	Description	Data Source
Age	The number of years since the issuance of a bond.	FISD
Remaining Maturity	The number of years between the event date (the date that a bond issued by the same firm was retired) and the maturation date.	FISD
Retired Ratio	The retired amount as a percentage of the total bond outstanding of the issuing firm. The total bond outstanding is approximated by the dltt term in Compustat.	FISD; Compustat
ln(age)	The natural log of variable *Age*.	-
Age (Scaled by Total Maturity)	The number of years since the issuance of a bond as a percentage of the total maturity of the bond.	-

Chapter 3:

The CEO Compensation Sustainability Goals' Disconnect:

Evidence from the Oil & Gas Industry

The CEO Compensation Sustainability Goals' Disconnect:

Evidence from the Oil & Gas Industry

Abstract: Using novel hand-collected data of managerial annual incentive plans (AIP) goals in 1994-2020, we characterize the presence of sustainability compensation targets. We show that sustainability targets are often quantified with corresponding overall weight in the incentive plan instead of rarely observed sustainability modifiers. Examining Fortune 250 firms, we find that such sustainability goals are uncommon and available in about 8% of the sample as recent as 2020. Moreover, most such firms are in the oil & gas industry. Turning exclusively to the latter, we show that sustainability goals are only effective in reducing CO_2 emissions, environmental penalties, and toxicity emissions for past polluters. No such effect is present in green innovation. Our results suggest that sustainability compensation goals are valuable if implemented in firms with high past pollution levels, exclusively, consistent with optimal contracting. Since sustainability goals are associated with slower sales growth and lower profitability, therefore boards need judiciously apply such contracts. Using firm inclusion in the Board Accountability Project and state adoption of director duties statutes as well as industry sustainability compensation practices as instruments for adopting sustainability targets, we confirm that our results are causal.

I. Introduction

A large literature in finance examines the effect of executive compensation on corporate performance (see for example Coles, Daniel, and Naveen, 2006 and Gopalan, Milbourn, and Thakor, 2014). Yet, only a limited set of papers have asked the question – how do *corporate social responsibility* (CSR) goals in the managerial annual incentive plan (AIP) influence CSR performance and financial outcomes for the firm. Several papers have broadly and descriptively examined CSR goals in executive compensation. For example, Hong, Li and Minor, 2016; Flammer, Hong, Minor, 2019; Francoeur, Lies, Gaia, and Aresu, 2017; Cohen, Kadach, Ormazabal and Reichelstein, 2022; and Bebchuk and Tallarita, 2022 all study sub-samples of data and provide a basic nomenclature of such goals. However, these papers use very broad (and basic) proxies for CSR targets (goals) in the executive compensation contracts, and – unsurprisingly – generate a discord regarding the effect of such broad sustainability-related compensation goals on corporate CSR performance. For example, Flammer et al. 2019 find that green patenting increased and that pollution levels decline while Tobin's Q increased in response to broad CSR targets in executive compensation contracts in 1994-2013, while work of Bebchuk and Tallarita, 2022 shows that the effects of such broadly defined CSR compensation goals are mixed and can lead to tangible harm to the total shareholders returns. Moreover, representative of this discourse, Cohen et al. 2022 shows similar mixed effects in an international sample, but once again, using a broad CSR goals proxy in the CEO compensation contracts.

Motivated by the lack of (i) consistent characterization of the CSR goals in the executive compensation agreements and (ii) consistent evidence of their impact on the CSR performance of the firm, we set forth to resolve both issues in the literature by hand collecting a novel data on the environmental (i.e., sustainability) goals in executive annual incentive plans for Fortune 250 firms. Since only 217 firms in that list are public, our available data consists of 217 such firms cumulatively over 1994-2020. We also choose to focus exclusively on the environmental goals in the AIP agreements as opposed to other (e.g., social responsibility) goals due to the rising importance of the climate risk. Equipped with such data, our first goal is to characterize the sustainability targets in the AIPs for the largest firms in the U.S., accounting for more than 85% of the total market cap of publicly listed firms. Importantly, we also describe the structure of such environmental goals in the CEO compensation contract.

Our first observation is that large U.S. firms broadly structure environmental objectives in the annual incentive plan to have three elements: (i) a distinct environmental objective to be met; (ii) an attached weight to the importance of such objective in the annual CEO compensation evaluation; and (iii) an environmental modifier which acts to modify the entire CEO compensation – after achieving any of the performance (environmental or financial) targets in the AIP – if certain environmental targets are met. It is important to stress that companies in the Fortune 250 list rarely use all three elements, in fact, the companies exclusively use (i) or (iii) but never both. Moreover, we note that a substantial portion of the list of firms with sustainability targets also have a quantitative weight attached to those, i.e., (i) and (ii) are highly correlated.

Our second observation is that there exists a substantial disconnect between the perceived importance of environmental goals – anecdotally and in social media - and their actual presence in AIPs. More precisely, as of 1994 less than one percent (!!) of the Fortune 250 firms had environmental goals in their AIPs; and as recent as 2020, only about 8% had such environmental targets in the executive compensation agreement. Second, and notably, most of those firms with environmental goals in the CEO's annual incentive plan within the sample Fortune 250 firms are in the oil and gas industry. For example, in as recent as 2018, 10 firms in the Fortune 250 list had environmental targets in the AIP; yet 8 of those firms were in the oil and gas sector. Interestingly, it is only the most toxic industry members – i.e., the oil and gas industry firms – that are most often setting sustainability goals in their CEO's annual incentive plans.

Given the importance of the oil and gas industry in the Fortune 250 list of firms *with* sustainability goals in executive contracts, we next turn our attention to the oil and gas sector and hand collect the sustainability goals in AIPs data for these firms, exclusively. Rather than using only the 11 oil and gas firms (as of 2020) in the list of Fortune 250 firms, we collect data for all oil and gas firms with available financials in Compustat 1994-2020. More precisely, we collect data for all public firms in Comustat's SIC codes 1311 (crude petroleum and natural gas), 2911 (petroleum refining), 4610 (pipelines, excluding natural gas) and 4922 (natural gas transmission). We also impose the requirement that data is available for such firm in databases of environmental pollution and environmental penalties (i.e., S&P's TrueCost, EPA's Toxic Release Inventory and Corporate Research Project's Violation Tracker). With these filters, our oil and gas sector's firm count is 228 and for those we collect data on sustainability goals in the executive's annual incentive plans, 1994-2020. Therefore, our data represents the most comprehensive nomenclature of such goals in the oil and gas industry, as well as the Fortune 250 firms.

Equipped with the AIP sustainability targets data for the oil and gas sector we proceed to address two questions: (i) what factors determine the adoption of such goals (i.e., we separately study the adoption likelihood of sustainability targets, sustainability targets with weight in the overall executive performance evaluation, and sustainability modifiers), and (ii) whether such sustainability goals influence CO_2 emissions, environmental penalties, green innovation (patenting) and firm profitability and growth, post-goals-adoption. Our findings are easily summarized. We find that neither size nor prior pollution levels influence the adoption of any sustainability goals, controlling for time invariant firm fixed effects. Interestingly, we show that firms with greater cash reserves are more likely to adopt sustainability targets, over the next two years. Importantly, firms with greater cash reserves are also more likely to adopt sustainability targets with quantitative weights in the overall compensation evaluation. Moreover, oil and gas firms with greater carbon footprint adopt sustainability modifiers less frequently.

Next, we characterize the effect of AIP sustainability goals onto future CO_2 emissions, environmental penalties, a composite score of overall toxicity of air releases and green innovation among oil and gas firms. The unconditional effect on those is largely absent, i.e., adopting such goals has on average no effect on future CO_2 emissions, environmental penalties imposition, air releases' composite chemical toxicity nor green innovation. Yet, however, we show that apart from a short-term increase in sales if a sustainability modifier is adopted, the long-term effect - three-years post-adoption – is that firms that have such sustainability targets with quantified weight in the annual incentive plan evaluation tend to lower their sales growth. It appears, therefore, that such adoption while costly, yet it does not lead to the specified goals of improved sustainability of the treated firms. We next ask *when* such sustainability provisions affect sustainability corporate performance and find that the adopted sustainability goals are only effective in reducing carbon emissions, lowering environmental penalties and improving a composite score of chemical toxicity of air releases when the firm has been a past polluter. Importantly, this effect is persistent (up to three years after the adoption of AIP sustainability goals) and especially strong across metrics of pollution if a sustainability target with quantified weight in the annual executive evaluation is adopted (i.e., when the intensity of the focus on sustainability goals among polluting firms is stronger). This result is consistent with optimal contracting, implying that – given the operating costs (lower growth and lower profitability) of adopting sustainability goals in the AIP – that firms should carefully select including such goals in the AIP only when they must remedy elevated levels of past pollution. Importantly, we find no effect –

conditional on past pollution, or free of it – on green innovation, a finding that stands in contrast with the findings of Cohen, Gurun and Nguyen (2020).

After establishing the association between sustainability goals and sustainability outcomes, we investigate whether such correlations are causal. To do so, we employ in particular three sets of instruments, and show that the above results are causal. More precisely, the three instruments that we include are: (i) the adoption of director duties state statutes that exempt directors from liability lawsuits by shareholders should they decline to accept an offer to acquire the company at a premium price, so they cater to other, non-shareholder stakeholders' interests; (ii) the inclusion of a firm in the Board Accountability Project (BAP) by the Comptroller of New York State for the purposes of investing in such companies by the New York City Pension Plans, and (iii) industry practices of adopting sustainability goals in the AIP plans of executives over time, measured as the average (or median) annual frequency of such within each industry (excluding the firm in point). Our instrumental variables analysis results appear largely consistent with the OLS result, suggesting an interpretation of causality. Importantly, the causal link is stronger – both in the OLS and instrumental variable analyses – for the sustainability targets with quantified weight in the annual executive evaluation. With the exception of facility-chemical and year-chemical level data analysis of air releases toxicity, nearly all IV models are well identified with strong instruments (i.e., Cragg-Donald F-statistics greater than the Stock-Yogo (2005) 10% weak identification test; Hansen J-test unable to reject the null of no overidentification).

We conclude that sustainability goals among Fortune 250 firms are uncommon and concentrated mainly among firms in the oil and gas sector. Studying a comprehensive (i.e., beyond oil and gas firms in the Fortune 250) list of 228 oil and gas firms in the U.S., we show that sustainability goals are effective in reducing CO_2 emissions, environmental penalties, and a composite measure of toxicity of air releases only for firms that are high polluters prior to the adoption of such AIP goals, consistent with optimal contracting. Moreover, we show that the adoption of such sustainability objectives may lead to slower sales growth and lower internal profitability up to three years following their adoption. Given the latter cost of adopting sustainability goals, and overall, we therefore conclude that companies in the oil and gas industry as well as firms in Fortune 250 need judiciously approach the adoption of such sustainability goals and adopt such only when to remedy high levels of past pollution. Moreover, we note that not all elements of the sustainability targets in the AIPs are equally influential. We note that sustainability targets with weights in the annual executive evaluation appear to be more influential on future CO_2 emissions, emissions' toxicity and environmental penalties while sustainability modifiers are least effective.

We contribute both to the literature on executive compensation and to the literature on corporate social responsibility. In the former literature, we introduce a novel, detailed data to describe the CEO compensation plan sustainability goals that are characterizing the contract. More precisely we show that there are three elements of such goals – a sustainability target, a sustainability target with quantified weight in the annual CEO performance evaluation and a sustainability modifier. We also study the effect of these goals on corporate environmental performance showing that such contracts are rarely observed outside of the oil and gas industry, and within the latter, they are only effective for firms with elevated past pollution levels. Meanwhile, these contractual mechanisms appear to be unimportant for green innovation and are also costly as they may lead to long-term operational underperformance. Moreover, we contribute to the literature on the corporate social responsibility by outlining an important channel through which a firm can achieve effectively an improved environmental performance – however, such channel is only effective for firms with elevated past pollution. Lastly, our work highlights the importance of attaching weight to sustainability goals in the annual incentive plan as those are more effective than sustainability modifiers or sustainability goals without target weights.

The rest of our paper is organized as follows. Section II discusses the empirical design. Section III presents the data. Section IV describes our results and Section V concludes.

II. Empirical Design

Our primary format of investigation is to relate sustainability goals in AIPs to metrics of performance of the firm – e.g., CO_2 emissions, environmental penalties, and chemical toxicity based on the Toxic Release Inventory (TRI) data for all firms in the oil and gas sector. We define the sector to include the four segments by 4-digit SIC codes as follows: 1311 (crude petroleum and natural gas), 2911 (petroleum refining), 4610 (pipelines, excluding natural gas) and 4922 (natural gas transmission). Additionally, we are interested in relating the sustainability goals to corporate green innovation as well as operating performance including sales growth and internal profitability (as measured by ROE).

We conduct our analysis at two distinct levels: (i) firm-year panel and (ii) facility-chemical and year panel level. To examine whether the adoption of incentive measures affects firm-level outcomes, we estimate the following fixed effects regression:

$$\Upsilon_{i,t+1} = \beta_1 I\left(Sust.Target_{i,t}\right) + \beta_2 I\left(Sust.Target\ with\ Weight_{i,t}\right) + \beta_3 I\left(Sust.Modifier_{i,t}\right) + \Upsilon_{i,t-1} + \boldsymbol{\gamma}'\mathbf{X}_{\boldsymbol{i,t}} + \delta_i + \delta_t + \varepsilon_{i,t}, \tag{1}$$

where *i* indexes firms in most cases but indexes facilities-chemical pairs when the dependent variable is Toxic Release Inventory's (TRI) toxicity; *t* indexes years, but indexes year-chemical pairs; Υ is the dependent variable of interest, *I(Sust. Target)*, *I(Sust. Target with Weight)*, and *I(Sust. Modifier)* are dummy variables for the three key explanatory variables: sustainability target, sustainability target with weight in annual evaluation, and sustainability modifier; **X** is the vector of control variables (assets, leverage, and cash asset ratio) which helps mitigate the possibility that our findings are driven by omitted variables; δ_i and δ_t are firm and year fixed effects (correspondingly, facility-chemical and year-chemical pairs in the case of the dependent variable being TRI composite toxicity of air releases); and ε is the error term. The coefficients of interest are β_1, β_2, and β_3, which capture the change in Υ following the adoption of the three incentive measures respectively. Since pollution is a function of productivity, we are careful in specifying our dependent variable, Υ. While we use firm-level output related to environmental performance (CO_2 emissions from S&P's TrueCost, Environmental Violation Penalties from the Violation Tracker data, and green patenting from U.S. PTO) under most circumstances, we use facility-level output for TRI Toxicity due to data limitations. We also report results at time t+2 and t+3 to better recognize how the outcomes will be affected.

To examine the conditional effect of each incentive measure on firm-level outcomes, we add interaction terms to the previous fixed effects regression:

$$\Upsilon_{i,t+1} = \theta_1 I(Sust.Target_{i,t}) + \theta_2 I(Sust.Target_{i,t})\Upsilon_{i,t-1} +$$
$$\theta_3 I(Sust.Target\ with\ Weight_{i,t}) + \theta_4 I(Sust.Target\ with\ Weight_{i,t})\Upsilon_{i,t-1} +$$
$$\theta_5 I(Sust.Modifier_{i,t}) + \theta_6 I(Sust.Modifier_{i,t})\Upsilon_{i,t-1} + \Upsilon_{i,t-1} + \boldsymbol{\gamma}'\mathbf{X}_{\boldsymbol{i,t}} + \delta_i + \delta_t + \varepsilon_{i,t}, \qquad (2)$$

The specifications are exactly the same as those of the previous regression model. Different from what we expect from the previous model, we are concerned about not only θ_1, θ_3, and θ_5, but also, more importantly, θ_2, θ_4, and θ_6, which assess the conditional effect of the three incentive measures separately. Again, the dependent variable is firm-level in most cases except in the case of TRI Toxicity.

III. Data

Our data come from various sources. Our main sample consists of 228 oil and gas companies whose standard industrial classification ("SIC") codes are 1311 (Crude Petroleum and Natural Gas), 2911 (Petroleum Refining), 4610 (Pipelines for no Natural Gas), or 4922 (Natural Gas Transmission) from 1994 to 2020. We hand collect the annual incentive plan ("AIP") sustainability goals of these companies from the corresponding U.S. Securities and Exchange Commissions ("SEC") proxy statements. For each of the 2564 firm-year observations, we look at whether they have environmental targets, whether they set the weightings for the environmental targets, and whether they regard the environmental targets as modifiers. More specifically, we carefully go over the compensation structure of the CEO of those companies to figure out whether there exists AIP or some similar short-term incentive bonus. If the CEO compensation structure includes such an incentive plan, we go further to look at what measures are these incentive plans based on. We create three novel dummy variables under this circumstance: *esg_target*, *Iweight*, and *esg_modifier*. The variable *esg_target* equals 1 if companies include environmental targets in the AIP of their CEO, and 0 otherwise. The variable *Iweight* equals 1 if companies not only include environmental targets but also specify the weightings of those targets in the AIP of their CEO, and 0 otherwise. The variable *esg_modifier* equals 1 if companies have environmental targets but those targets work as a modifier to the short-term incentive bonus, and 0 otherwise. Besides the oil and gas companies, we also collect the above information for the first 250 public companies in Fortune 500 between 1994 and 2020, resulting in another novel hand-collected AIP data on these 250 companies.

We create different measures of environmental outcomes. We collect carbon emission data from TrueCost and environmental penalties data from Violation Tracker. These are two widely accepted environmental outcomes in sustainable corporate finance area. Besides the two existing databases, we also obtain the Toxic Release Inventory ("TRI") data from the U.S. Environmental Protection Agency ("EPA"), enabling us to get a knowledge of toxic chemical releases as well as pollution prevention activities reported by individual facilities. Following King and Lenox (2000), we weigh each chemical by its toxicity in the way of multiplying the quantity of each chemical emitted by the inverse of its reportable quantity. We then aggregate toxicity-weighted emissions of each chemical. This provides us a total emission toxicity level of all notable chemicals. To further understand the toxicity level, we employ the data on the biological influence of chemicals from the Integrated Risk Information System ("IRIS") which allow us to address the health impacts of the TRI listed chemicals. The IRIS database identifies whether a certain chemical listed in the TRI database may bring potential harms to human organs, causing respiratory, developmental, nervous, hematologic, urinary, or hepatic issues.

Following the instructions of the Organization for Economic Co-operation and Development ("OECD"), we filter patents and define patents related to air pollution abatement, water pollution abatement, waste management, soil remediation, environmental monitoring, and water-related adaption technologies as green patents. The filtering criteria are based on both the International Patent Classification ("IPC") and the Cooperative Patent Classification ("CPC"). The IPC is a hierarchical classification system used primarily to classify and search patent documents (patent applications, specifications of granted patents, utility models, etc.) according to the technical fields they pertain. CPC is an extension of the IPC and is jointly managed by the European Patent Office and the U.S. Patent and Trademark Office ("USPTO"). In order to identify the efforts of companies to alleviate the harm they do to the environment in a more advanced way, we collect data regarding green patents and then name match the database to our main sample.

When try to establish the causal relationship, we instrumented our independent variables with three variables: the inclusion in the Board Accountability Project, the state adoption of director duties statutes, and industry-year average excluding own value of the dependent variables. Preceded by

Naaraayanan, Sachdeva, and Sharma (2021), we employ BAP as an additional instrument. BAP was initiated by the New York City Pension System ("NYCPS") and intended to enable pensioners to speak out in oversight related to climate change, board diversity, and excess CEO pay. Since one of its most important mandates was climate change risk, this campaign targeted firms based on their underlying carbon reserves. Due to its target of carbon-intensive companies unable to fully tackle their climate change concerns, BAP is an almost ideal instrument for our study considering that it can absolutely make a difference to companies with heavy pollution while these companies did not anticipate the campaign before. Furthermore, enlightened by Karpoff and Wittry (2018), we use constituency statutes as our first instrument variable. Constituency statutes, also known as directors' duties ("DD") law, force board members to take into consideration the interests of all corporate stakeholders including non-investor stakeholders. This law is evidently not influenced by any specific company but indeed cause some effect on companies in states that have it. Moreover, we use industry-year average excluding own value of our independent variables as instruments. Industry-year average excluding own value of our AIP variables indicate the overall situation of AIP of companies within the same industry as a given company we are looking at, but other than that company. This ensures that the company we study makes no difference to the industry average but could be influenced by the industry average in the meantime.

We use log total assets, leverage, and cash and cash equivalents as our control variables. We pull the relevant data from Compustat.

IV. Results

In this section we present our main findings. To illustrate, we begin with a discussion of four figures.

[Insert Figures 1A and 1B about here]

We start with Figure 1A and 1B that show the number and percentage of Fortune 250 firms that include sustainability targets, sustainability targets with weights or sustainability modifiers in the AIP of their CEO. The figure provides evidence that the number and percentage relative to the total number of Fortune 100 firms with ESG targets increase steadily in the whole sample period, despite some small drops in 2002, 2006, and 2013. Figure 1C shows how the number of Fortune 250 firms with sustainability targets, the number of petroleum firms with sustainability targets (within the set of Fortune 250 firms), the number of non-petroleum firms with sustainability targets (within the set of Fortune 250 firms), and the percent of petroleum firms with sustainability targets from the set of all firms with sustainability targets change over time. The red columns represent the number of Fortune 250 oil and gas firms with sustainability targets while the blue columns represent the number of Fortune 250 non-oil-and-gas firms with sustainability targets. The line indicates the ratio of the number of petroleum firms with sustainability targets in Fortune 250 over the number of total Fortune 250 firms with sustainability targets. From 2000, non-petroleum firms with sustainability targets debuted in Fortune 250, but petroleum firms with sustainability targets always dominated in number until 2020. However, despite its dominance, the number of petroleum firms with sustainability targets has seen a flexural drop in its fraction relative to the total number of Fortune 100 companies with sustainability targets.

[Insert Figures 1C and 1D about here]

Figure 1D depicts the number of Fortune 250 firms with sustainability targets, the number of petroleum firms with sustainability targets, and the number of non-petroleum firms with sustainability targets in a more straightforward way and draws the same conclusion as Figure 1C.

[Insert Table 2 about here]

Next, we turn to the tabular form of the above-discussed figures. Table 2 depicts the summary statistics of our key concerned variables, sustainability targets, sustainability targets with weight, and sustainability modifier, for the Fortune 250 firms, over 1994 to 2020. The table reveals a steady increase in both the absolute amount and the percentage of Fortune 250 firms with sustainability targets. The situation is similar for Fortune 250 oil and gas companies. Some firms just appear on the list of firms with sustainability targets and exit right away while some other firms didn't have proxy statement in some specific years so we cannot infer whether they have sustainability targets in those years. Surprisingly, no firms have sustainability modifiers.

[Insert Table 4 about here]

Next, in Table 4 we present the results of a linear probability model on the propensity to observe an annual incentive plan that includes a sustainability target, a sustainability target with weight, and a sustainability modifier. We first note that the inclusion of the BAP significantly increases the probability of observing a sustainability target with weight. Compared to the firms that are not included in the BAP, firms in the BAP have a roughly 20% higher chance to introduce a sustainability target with weight in their AIP. This result is statistically significant at 5% level confidence. Another important observation is that the past carbon emissions seem to have no impact on the propensity to set up a sustainability target, no matter with or without weight. Moreover, the past carbon emission leads to a decrease in the propensity that a firm introduces a sustainable modifier in their AIP as an incentive to improve their environmental performance. Lastly, we observe that the director duties law has no impact on all three sustainability related terms in the AIP. We believe this is due to the lack of variation in the director duties law in our sample.

[Insert Tables 5A and 5B about here]

Next, we illustrate how the existence of sustainability related incentives in the AIP would change the environmental outcomes of a firm. We first present the impact of sustainability related incentives in the AIP on the carbon emissions in Table 5. In Table 5A, we show the overall effect of the sustainability related incentives. According to Panel I, among all three sustainability related incentives, only sustainability target with weight has an impact on the carbon emission. On average, the inclusion of sustainability target with weight in the AIP will lead to a 52.5% decrease on carbon emissions in the following year, an 56.9% decrease one year after that, and a 60.3% decrease in the third year. All results are statistically significant at 1% level. Since the three sustainability related incentive measures are correlated, to address the concern of multi-collinearity, we present the coefficient where we test the three sustainability related measures separately in Panel II. The results are consistent.

We then explore the cross-sectional difference of the previous results. More specifically, we test how would the impact of sustainability related incentives on the carbon emissions change with the historical pollution level of a firm. The results are presented in Table 5B. Our estimates in Panel I of Table 5B suggest that the inclusion of sustainability target with weights and sustainbale modifier in the AIP would improve the environmental performance of a firm if and only if the firm is a heavy polluter in the past. For example, for firms with sustainability target with weight in their AIP, the carbon emission reduces by 0.21% when the past carbon emission increases by 1% in the following year. Similarly, for firms including sustainable modifier in their AIP, the carbon emission reduces by 0.20% when the past carbon emission increases by 1%. These results are statistically significant at 1% level. However, we do not observe a similar effect for the sustainability target. We attribute this non-significant result to the multi-collinearity issues of our three sustainability related incentive measures. Therefore, we present the separated estimates for all three measures in Penal II. The results of Panel II indicate that all three sustainability related incentive measures are effective if and only if the firm is a historically heavy polluter.

[Insert Tables 6A and 6B about here]

Table 6A shows us the results of an instrumental variables regression of CO2 emissions as a function of Sustainability Target, Sustainability Target with Weight, and Sustainability Modifier at times t+1, t+2, t+3. As mentioned in the data section, we use BAP indicator, DD State indicator, and industry-year sustainability practices as our instruments. The first three columns show the effect of sustainability goals on future CO2 emissions. The next three columns indicate the effect of sustainability goals with specified weight on future CO2 emissions. The last three columns reveal the effect of sustainability modifiers on future CO2 emissions. Among the three, only sustainability targets with weights shows significantly negative and consistently increasing effect on future carbon dioxide emissions at 5% level. Sustainability targets without specified weights only reveal negative effect at t=2 and t=3. Clearly, the effect of sustainability targets without weights is stronger at t=3 than at t=2. As to sustainability modifiers, it has a quite different situation. While the results indicate that companies with sustainability targets, with weight or not, are more likely to incur lower CO2 emissions, companies with sustainability modifiers will expect significantly higher CO2 emissions in the third year. One possible explanation is that such companies which employ sustainability modifiers are actually not concerned about their environmental performance and they are using sustainability modifiers just to do "window dressing". Besides, the instruments seem not very strong, but this can be due to the dual firm and year fixed effects. Removing the firm fixed effects, the results should be well identified.

Table 6B presents cross-sectional results of a similar instrumental variables regression of CO_2 emissions on Sustainability Target, Sustainability Target with Weight, and Sustainability Modifier, with their interactions with lagged CO_2 emissions included. The results are noteworthier, but much less fitted as can be seen by the bunch of negative R-squares. The first three columns show the effect of sustainability goals on future CO_2 emissions. The next three columns indicate the effect of sustainability goals with specified weight on future CO_2 emissions. The last three columns reveal the effect of sustainability modifiers on future CO_2 emissions. It is evident that sustainability targets with and without weights all somewhat have a positive effect on future carbon dioxide emissions while sustainability modifiers show no effect. This means that establishments of those sustainability targets actually encourage the emissions of carbon dioxide. Moreover, sustainability targets with weights have a greater impact on future CO_2 emissions relative to sustainability targets without specified weights. Adding the interaction terms allow us to get a sense of how sustainability goals can indeed influence the future carbon dioxide emissions of those carbon-intensive firms. The results provide the evidence that if a company has increased CO_2 emissions at present and set sustainability goals, they are more likely to expect lower CO_2 emissions in the future. The effect is stronger for companies that have sustainability goals with weights. Lastly, the instruments should be strong without the dual fixed effects of firm and year.

[Insert Tables 7A and 7B about here]

The next environmental outcome variable we test on is the environmental related penalties. Like that of carbon emissions, we first report the overall effects of sustainability related incentives on the environmental related penalties in Table 7A. The estimates of Panel I suggest that of all three environmental related incentives, only sustainability target with weights reduces the environmental penalties in the following three years. More specifically, the inclusion of sustainability target with weight in the AIP will lead to a 67.6% decrease in the environmental related penalties in the following year, an 58.0% decrease one year after that, and a 74.7% decrease in the third year on average. A counter intuitive finding is that the environmental related penalties increases when a firm decide to use sustainbale modifier as the managerial incentive. Again, to address potential multi-collinearity issues, we report the separated estimates of the three sustainability related incentive measures in Panel II, and the results are consistent.

We then examine the how this impact would change cross-sectionally. The results are reported in Table 7B. According to Panel I, only the inclusion of sustainbale target with weights in the AIP might reduce the environmental related penalties of a firm, and that happens if and only if the firm is a heavy polluter in the past. Within firms including sustainability target with weight in their AIP, the environmental related penalties reduces by 0.191% in the following year for every 1% increase in the past environmental penalties. The result is significant at 5% level. However, we do not observe any similar results for sustainability target and sustainbale modifiers. Panel II report the estimates where we separate the three environment related incentive measures, and the results are consistent.

[Insert Table 8A and 8B about here]

Table 8A shows the results of an instrumental variables regression of environmental violation penalties on Sustainability Target, Sustainability Target with Weight, and Sustainability Modifier. The results indicate that neither sustainability targets without weight nor sustainability targets with weight have any significant effect on future environmental violation penalties. Sustainability modifiers only have a negative effect on future environmental violation penalties at t+3, which is significant at 5% level. The instruments should be strong without the dual fixed effects of firm and year.

Table 8B presents the results of a similar instrumental variables regression of environmental violation penalties on the same three key explanatory variables as Table 8A but including their interactions with environmental violation penalties. Surprisingly, the results tell us that companies with sustainability targets are more likely to receive environmental violation penalties in two and three years. The result is significant at 5% level. There's no significant effect of sustainability targets with weights on environmental penalties. On the other hand, the effect of sustainability modifiers on future environmental violation penalties at t+1 and t+2, probably because companies with sustainability modifiers are less concerned about their environmental performance considering the difference in the role of targets and modifiers. After adding interaction terms, there's evidence that companies currently suffering from environmental penalties and having sustainability targets, whether with weights or not, are less likely to receive penalties due to their environmental performance in three years. The results are significant at 1% level. The effect of sustainability goals without weights and that of sustainability goals with weights are very close to each other. In fact, for companies that have sustainable goals with weights, the effect appears since t+2 and the result is significant at 10% level. When it comes to sustainability modifiers, the situation is a little bit different. The negative effect of sustainability modifiers shows up in the exact following year and then no longer exists. Again, the instruments should be strong without the dual fixed effects of firm and year.

[Insert Tables 9A and 9B about here]

The last environmental outcome variable we test on is the TRI toxicity. Unlike the carbon emissions data and the environmental related penalties data which are firm-year level, the toxicity emissions data from TRI comes at a more detailed facility-chemical-year level. Therefore, we are able to test the impact of our sustainability related incentives with more detail. In Table 9A, we present the unconditional effect of sustainability related incentives on the toxic chemical emissions at facility-year level. In Panel I, we provide the results where we estimate the impact of sustainability target, sustainability target with weight, and sustainable modifier together. According to our estimates, only sustainable modifier reduces the toxic chemical releases in the following year. Compared to firms without sustainable modifiers, firms with sustainable modifiers in their AIP would reduce their toxic chemical emissions by 18.3% in the following year. This result is significant at 5% level. However, the effect last for only one year. We also report the results where we estimate the impact of our three sustainability related incentives separately in Panel II, and the results are consistent.

The cross-sectional variation of the impact on the toxic chemical releases is reported in Table 9B. We report the estimation with all three sustainability related incentives in Panel I. Our results suggest that overall, all three sustainability related incentives reduce the release of toxic chemicals in the next three years if and only if the firm adopting these incentives has heavy toxic chemical releases in the past. For instance, compared to the firm without sustainable modifiers, firms with sustainable modifiers in their AIP reduces their toxic chemical releases by 0.143% when the historical toxic chemical releases increase by 1%. However, only the sustainability targets at the second and third year, and the sustainable modifiers at the first year is statistically significant. We again argue that this is because of multi-collinearity and report the results where we estimate the three sustainability related incentives separately in Panel II. The consistent and statistically significant estimates suggest our argument is worth considering.

[Insert Tables 10A and 10B about here]

To create the causal relationship, we instrumented our sustainability related incentives, and the results are reported in Tables 10A and 10B. We first show the instrumented unconditional results in Table 10A. Our estimate in Table 10A suggest that the unconditional effect of sustainable modifier we find in Table 9A are not stable. None of the three sustainability related incentives have any statistically significant unconditional effect after instrumented. This is consistent with our argument that there is a disconnect between the sustainability related incentives and the unconditional environmental outcomes of firms. Due to the extreme large sample size of the facility-chemical-year level data, our instruments are strong in this test, with the lowest Cragg-Donald F statistics being 549.4. Meanwhile, as the Hansen J test suggests, we do not have any over identification issue.

In Table 10B, we reported the instrumented cross-sectional variation. The instrumented estimates suggest that firms with sustainability target or sustainability target with weigh in their AIP reduce their toxic chemical releases if they are historically heavy toxic chemical emitters. According to our instrumented results, for firms with sustainability target in their AIP, a 1% increase in the historical toxic chemical releases translate into a 0.063% decrease in the toxic chemical releases in the following year, a 0.101% decrease one year later, and 0.127% decrease in the third year. Similarly, for firms with sustainability target with weight in their AIP, a 1% increase in the historical toxic chemical releases translate into a 0.164% decrease in the toxic chemical releases in the following year, a 0.615% decrease in the following year, and a 0.316% decrease in the third year. Meanwhile, we do not find similar trends or any other impact for the sustainable modifier. All these results are consistent with our findings in Table 9B Panel II. Our instruments are strong in this test as well, with the lowest Cragg-Donald F statistics being 18. However, with the interaction term, our endogenous variables might be over identified, as suggested by the low Hansen J test p-value.

[Insert Tables 11A and 11B about here]

Table 11A presents the results of an OLS regression of green patenting on sustainability target, sustainability target with weight, and sustainability modifier at t+1 through t+3. The results show no effect of any one of these three incentive measures at any time on the green patenting. This means that even though some companies seem to have sustainability incentives, they are actually not incentivized to do anything innovative in the environmental aspect, which to some degree again prove that they are "window dressing". Meanwhile, past carbon dioxide emissions also have nothing to do with future green patenting.

Table 11B adds interaction terms of the three incentive measures with past CO2 emissions on the basis of what we have done in Table 11A. The results indicate that sustainability target itself only has a positive effect on green patenting at t=1, meaning that companies that have sustainability targets tend to more actively develop environmental innovation right after the targets are established. However, the situation is quite the opposite for sustainability target with weight. The results reveal that while companies that have sustainability targets without weight tend to make more efforts on green patenting in the future, companies that have sustainability targets with weight are more likely to pay less attention to environmental innovation.

[Insert Tables 12A and 12B about here]

Table 12A provides the results of an instrumental variables analysis of green patenting on the three key incentive measures. Looking at the results, it is easy to tell that even with assistance of instrumental variables, we can only detect a positive effect of sustainability target without weight on green patenting significant at 10% level. This suggests that firms that have sustainability targets without weight are more likely to have green patents right after they set those targets. Unfortunately we are unable to see any effect of either sustainability target with weight or sustainability modifier on future green patenting. We find no significant conditional effect of green patenting on the incentive measures so we will not present the table here.

After investigating in the benefit of the sustainability related incentives, we next examine the potential cost of them. Given the fact that set up a sustainability incentive in the AIP is not the optimal decision from pure financial perspective, we examine how these sustainability related incentives change the sales and profitability of a firm. We first present the unconditional results in Table 13A. According to our estimates, the unconditional effect of sustainability related incentives on sales and ROE are generally statistically identical to 0 with a few exceptions. The existence of sustainability target with weight in AIP lead to a decrease in sales and increase in ROE in the third year after the target is set, and the inclusion of sustainable modifier increases the sales in the following year.

[Insert Table 13A and 13B about here]

In Table 13B, we present the cross-sectional difference of the cost when adopting sustainability related incentives. The main take-away from Table 13B is that after introducing the sustainability target into AIP, the historical heavy polluters experience a statistically significant reduction in their sales growth and ROE compared to their non-heavy-polluting peers. More specifically, among all firms implementing a sustainability target in their AIP, a 1% increase in the historical carbon emission leads to a 0.011% decrease in the sales growth and 0.015% decrease in the ROE in the following year, 0.019% decrease in the sales growth and 0.001% decrease in the ROE one year after that, and 0.02% decrease in the sales growth and 0.021% decrease in the ROE in the third year. Although not statistically significant, we find similar patterns in the other two sustainability related incentives, the sustainability targets with weight, and the sustainable modifier, as well, except that the sustainability target with weight will increases the sales growth in the third year. Overall, we believe our evidence is consistent with the story that the sustainability related incentives increase the environmental outcomes of a firm at the cost of financial performance.

[Insert Table 14A and 14B about here]

We further test whether the impact of the sustainability related incentives on the sales growth and ROE are causal using instrumented variables. We present the instrumented unconditional results in Table 14A. The overall conclusion is consistent with Table 13A, that the unconditional effects of sustainability related incentives on sales growth and ROE are statistically indifferent from 0. The only exception is the impact of sustainability target with weight on sales growth. Introducing the sustainability target with weigh into AIP boosts the growth of sales in the following year, but then reverted in the next year. Our instruments turn out to be pretty strong, the lowest Cragg-Donald F statistics is 11.7 among all 18 specifications for both sales growth and ROE, while the Stock-Yogo 10% maximal relative bias critical value is only 9.1. Meanwhile, we also recognize the potential over identification issue, especially for ROE, as suggested by the Hanse J test.

The estimates of instrumented cross-sectional variation are presented in Table 14B. Overall, we observe that although not all statistically significant, historical heavy polluters experience decrease in the sales growth and ROE after introducing sustainability related incentives compared to their non-heavy-polluting peers. This is consistent with our findings in Table 13B. Again, the instrument here does not seem to be as strong as in Table 14A, but that likely due to the dual firm and year fixed effect. Removing the firm fixed effect would provide us well-identified results.

V. Conclusion

In this paper we address the question – are sustainability targets in the CEO annual incentive plan leading to improved sustainability performance of the firm? We examine this research question, equipped with a novel hand collected data. Using this novel hand-collected data of managerial annual incentive plans (AIP) goals, we characterize the presence of sustainability compensation targets. We show that sustainability targets are often quantified with corresponding overall weight in the incentive plan instead of rarely observed sustainability modifiers. Examining Fortune 250 firms, we find that such sustainability goals are uncommon and available in less than 8% of the sample. Moreover, most such firms are in the oil & gas industry. Turning exclusively to the latter, we show that sustainability goals are only effective in reducing CO2 emissions, environmental penalties, and toxicity emissions for past polluters. No such effect is present in green innovation. Our results suggest that sustainability compensation goals are valuable if implemented in firms with high past pollution levels, exclusively, consistent with optimal contracting. Since sustainability goals are associated with slower sales growth and lower profitability, therefore boards need judiciously apply such contracts. Using firm inclusion in the Board Accountability Project and state adoption of director duties statutes as well as industry sustainability compensation practices as instruments for adopting sustainability targets, we confirm that our results are causal.

As venues for future work, we plan to examine – using our novel hand collected data – the effect of such sustainability goals on the corporate performance of the firm, in particular their stock returns. A possible further extension will be to hand collect the data in the AIPs regarding other – social – goals and examine their impact on the social and corporate performances of the firms, and their relationship to the environmental goals set in the AIPs (e.g., examine any tradeoffs or complementaries between the environmental and social goals in the AIPs). Lastly, it is important to examine the role of institutional investors, in particular socially responsible investmeng firms (SRI) in the adoption of sustainability and social goals within the executive annual incentive plans. We leave these research questions using our novel data to future research.

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Figure 1A. In this figure we show the number of firms in our sample of Fortune 250 firms with sustainability targets in the Annual Incentive Plans (AIP) during 1994-2022. We also show the number of firms with sustainability targets that have weights of the overall compensation.

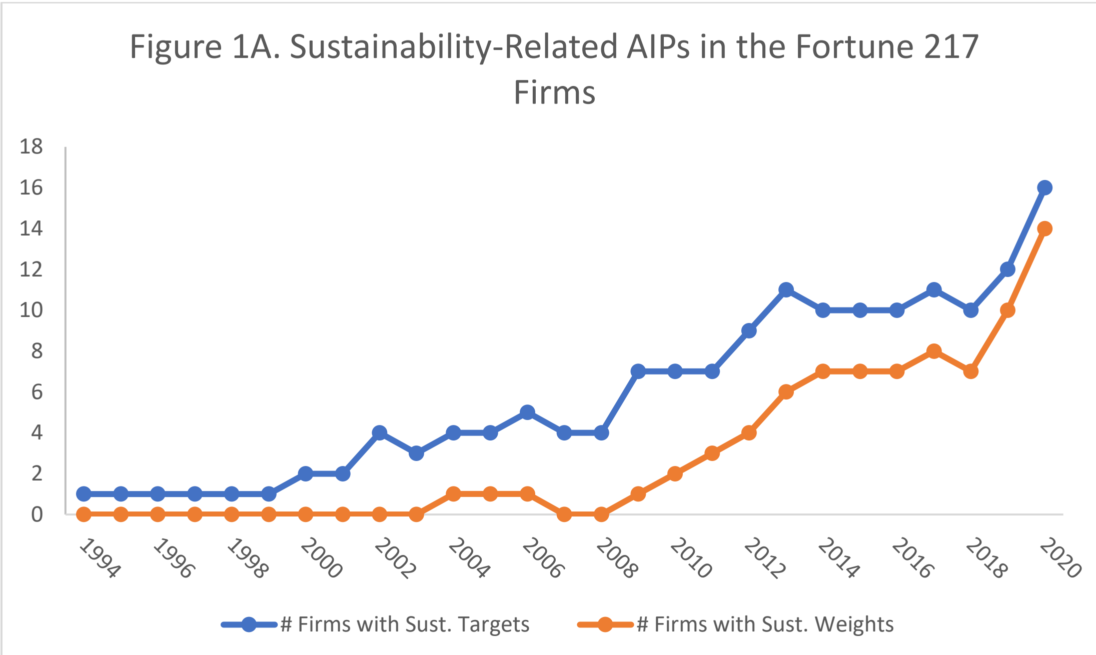

Figure 1B. This figure mimics Figure 1A with the presentation of the percentages.

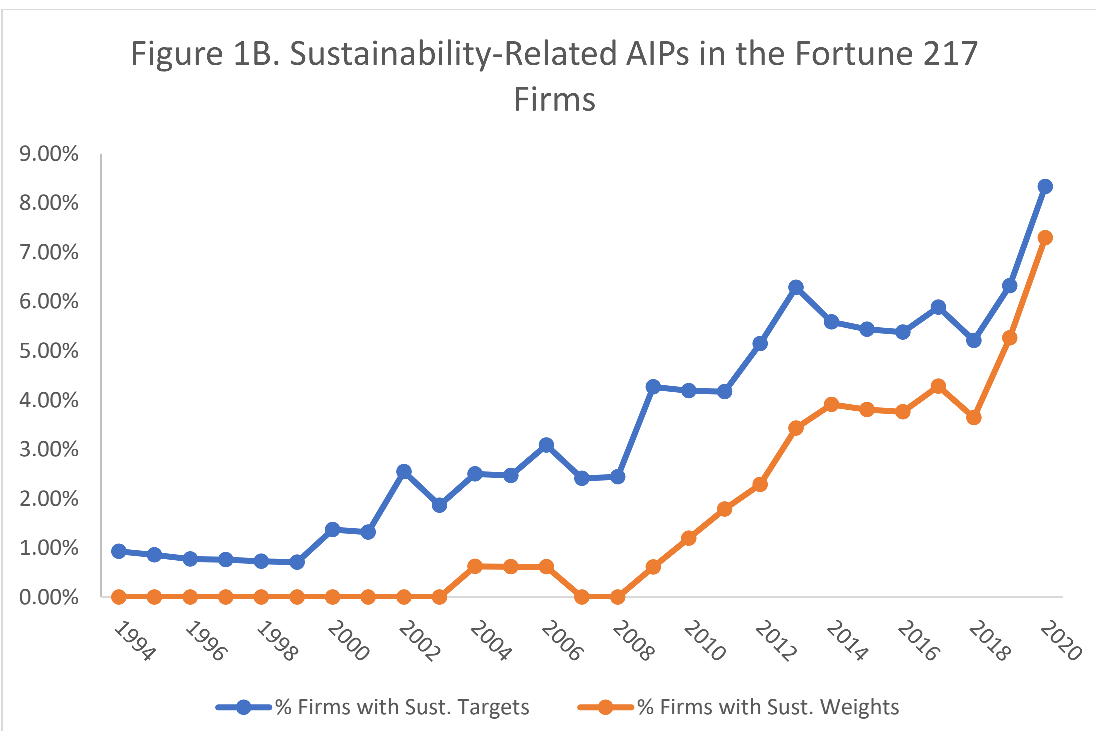

Figure 1C. This figure shows the # of Fortune 217 firms with sustainability targets, the number of petroleum firms with sustainability targets (within the set of Fortune 217 firms), the number of non-petroleum firms with sustainability targets (within the set of Fortune 217 firms), and the percent of petroleum firms with sustainability targets from the set of all firms with sustainability targets.

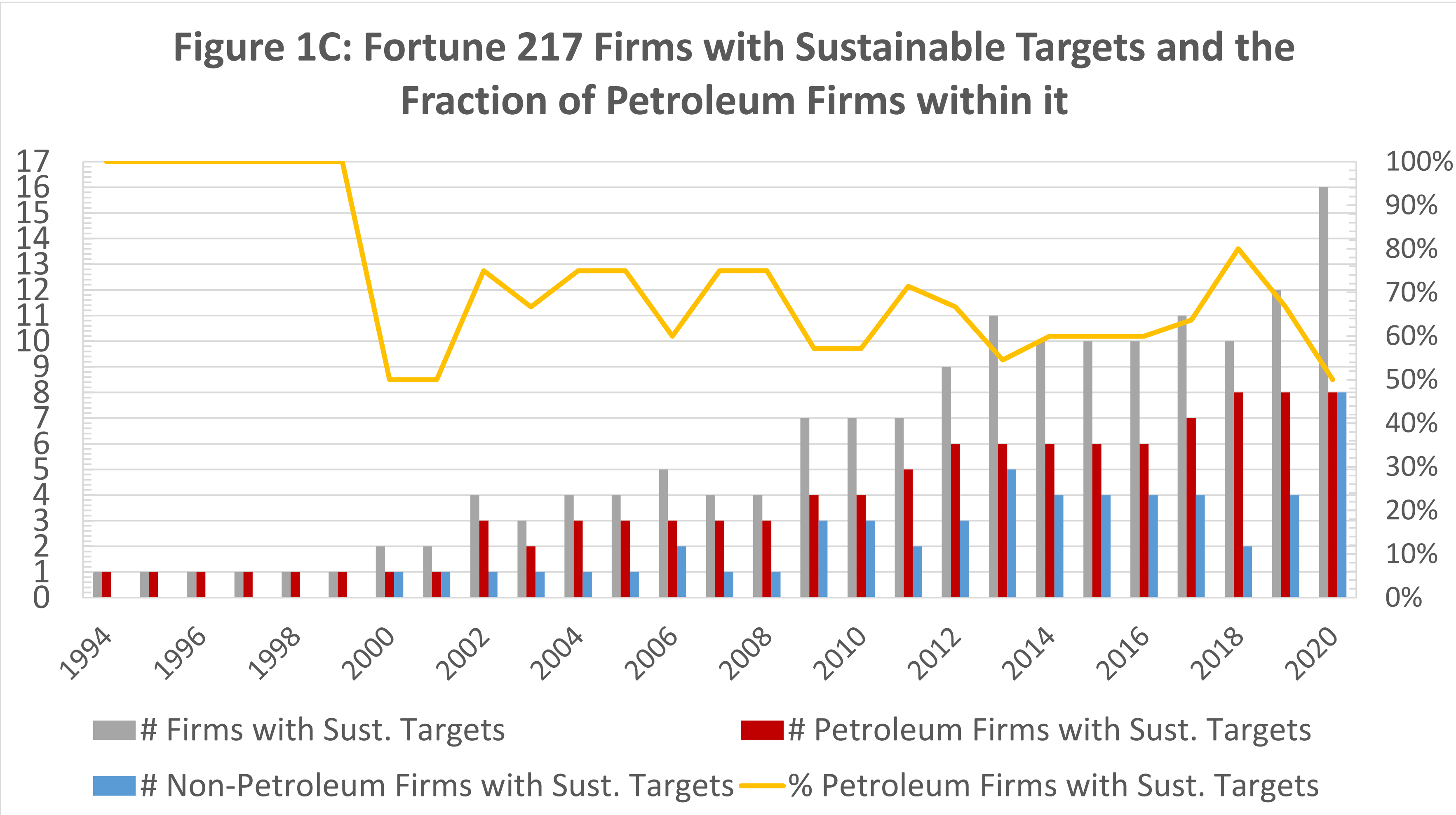

Figure 1D. Figure 1D shows the number of firms within the set of Fortune 217 firms with sustainability targets, split in (1) non-petroleum firms and (2) petroleum firms.

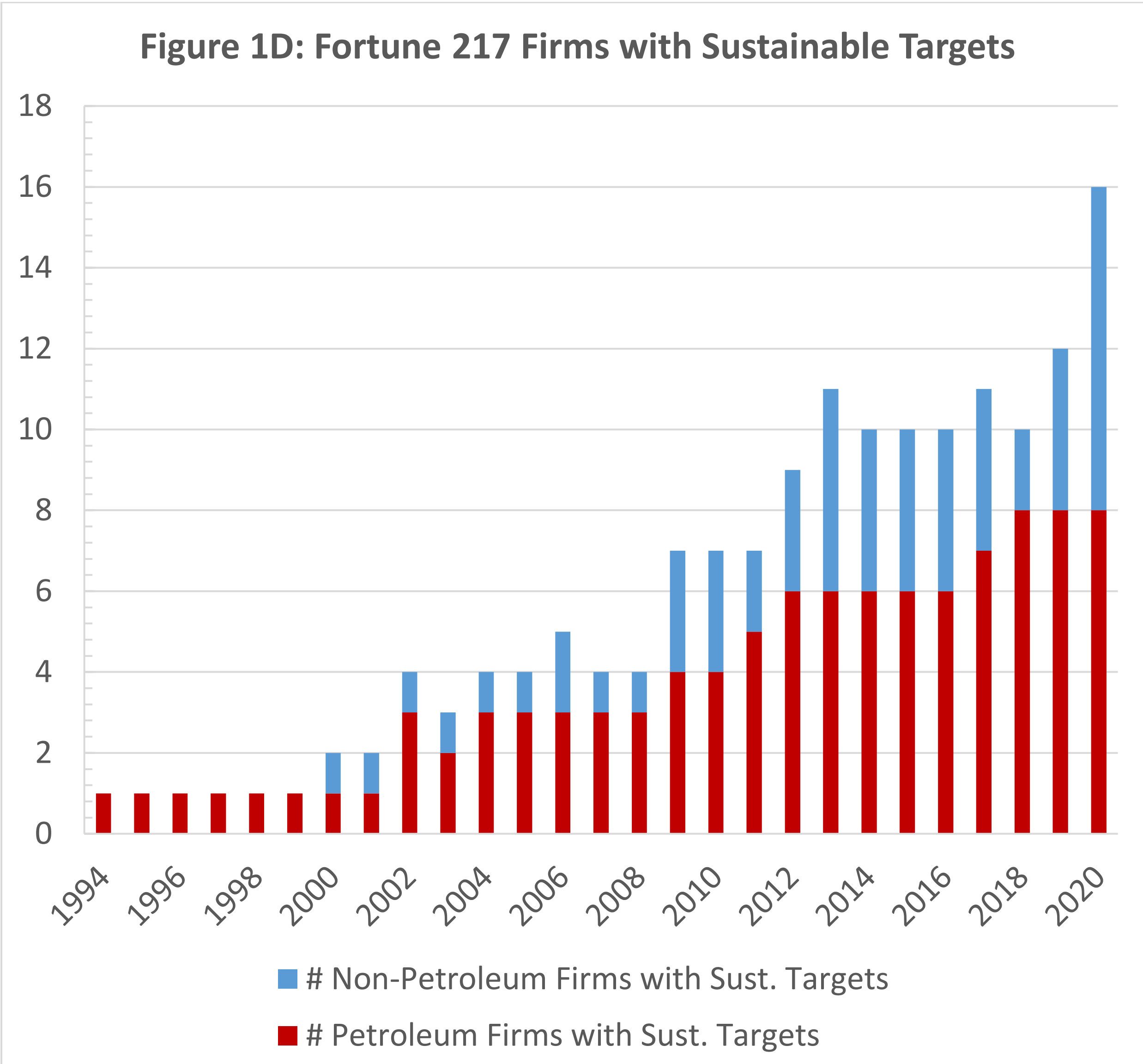

Table 1. Variables Definitions

Variable Name	Definition
Dependent Variables	
CO_2 emission	The nature log of Scope 1 carbon emissions intensity scaled by sales. The source of the data is TrueCost dataset.
Violation Tracker Penalty	Total enforcement amount with respect to environmental issues. The data source is the Violation Tracker dataset provided by the Corporate Research Project by Philip Mattera.
TRI Toxicity	The aggregated toxicity level of the TRI recorded chemical emissions. The chemical emissions data comes from TRI, available at https://www.epa.gov/toxics-release-inventory-tri-program/tri-basic-data-files-calendar-years-1987-present, and the toxicity level data comes from EPA, available at https://www.epa.gov/epcra/consolidated-list-lists-under-epcracerclacaa-ss112r-april-2022-version.
Key Independent Variables	
Sust. Target	A dummy variable equals to 1 if a firm has a sustainability target in its Annual Incentive Plan (AIP) and zero otherwise. Source: hand-collected data from www.SEC.gov.
Sust. Target with Weight	A dummy variable equals to 1 if a firm has a sustainability target with an assigned weight in its AIP, and zero otherwise. Source: hand-collected data from www.SEC.gov.
Sust. Modifier	A dummy variable equals to 1 if a firm has a sustainable modifier in its AIP, and zero otherwise. Source: hand-collected data from www.SEC.gov.
Controls and Instruments	
DD Adoption	The adoption status of director duty law in the headquarter state. Source: Karpoff and Wittry (2018).
BAP Focus	The inclusion of the firm in the Board Accountability Project (BAP) list. Source: Ivanov et al. (2022).
Total Assets	Total Assets. Source: WRDS Compustat-CapIQ.
Leverage	Leverage ratio defined as LT debt to total assets. Source: WRDS Compustat-CapIQ.
Cash	Log of cash and cash equivalents to total assets. Source: WRDS Compustat-CapIQ.

Table 2. Firms in the (i) Fortune 250 Sample and (ii) Oil & Gas Sample

Panel A. Fortune 250 Sample

Tabulations of the number of firms within the set of Fortune 250 firms into (i) percent firms with sustainability targets, (ii) percent firms with sustainability weights, (iii) percent firms with sustainability modifiers. We further segment the tabulation to show (in columns 6 and 7) the share of all petroleum firs within the Fortune 250 with either sustainability targets or sustainable weights. Note that of the Fortune 250 firms only 217 are public with available data.

Fiscal Year	# Firms	% Firms with Sust. Targets	% Firms with Sust. Weights	% Firms with Sust. Modifier	# Petroleum Firms	% Petroleum Firms with Sust. Targets	% Petroleum Firms with Sust. Weights
1994	108	0.93%	0.00%	0.00%	7	100%	0%
1995	117	0.85%	0.00%	0.00%	7	100%	0%
1996	129	0.78%	0.00%	0.00%	7	100%	0%
1997	132	0.76%	0.00%	0.00%	7	100%	0%
1998	137	0.73%	0.00%	0.00%	7	100%	0%
1999	141	0.71%	0.00%	0.00%	8	100%	0%
2000	146	1.37%	0.00%	0.00%	7	50%	0%
2001	152	1.32%	0.00%	0.00%	7	50%	0%
2002	157	2.55%	0.00%	0.00%	7	75%	0%
2003	161	1.86%	0.00%	0.00%	9	67%	0%
2004	160	2.50%	0.63%	0.00%	7	75%	0%
2005	162	2.47%	0.62%	0.00%	7	75%	0%
2006	162	3.09%	0.62%	0.00%	8	60%	0%
2007	166	2.41%	0.00%	0.00%	8	75%	0%
2008	164	2.44%	0.00%	0.00%	7	75%	0%
2009	164	4.27%	0.61%	0.00%	7	57%	0%
2010	167	4.19%	1.20%	0.00%	7	57%	50%
2011	168	4.17%	1.79%	0.00%	8	71%	100%
2012	175	5.14%	2.29%	0.00%	12	67%	75%
2013	175	6.29%	3.43%	0.00%	10	55%	67%
2014	179	5.59%	3.91%	0.00%	10	60%	71%
2015	184	5.43%	3.80%	0.00%	10	60%	71%
2016	186	5.38%	3.76%	0.00%	10	60%	71%
2017	187	5.88%	4.28%	0.00%	11	64%	75%
2018	192	5.21%	3.65%	0.00%	11	80%	86%
2019	190	6.32%	5.26%	0.00%	11	67%	70%
2020	192	8.33%	7.29%	0.00%	11	50%	57%

Panel B. Oil & Gas Sample

Tabulations of the number of petroleum firms (SIC codes 1311, 2911, 4610, 4922) and the share of such firms with (i) sustainability targets, (ii) sustainability weights, and (iii) sustainability modifiers.

Fiscal Year	# Petroleum Firms	% Petroleum Firms with Sust. Targets	% Petroleum Firms with Sust. Weights	% Petroleum Firms with Sust. Modifier
1994	78	3.8%	1.3%	0.0%
1995	82	5.1%	0.0%	0.0%
1996	86	7.7%	0.0%	0.0%
1997	81	6.4%	1.3%	0.0%
1998	76	7.7%	1.3%	0.0%
1999	76	6.4%	0.0%	1.3%
2000	76	7.7%	1.3%	0.0%
2001	75	6.4%	1.3%	2.6%
2002	81	6.4%	0.0%	1.3%
2003	81	7.7%	0.0%	1.3%
2004	85	11.5%	0.0%	1.3%
2005	82	12.8%	0.0%	1.3%
2006	97	21.8%	7.7%	2.6%
2007	105	21.8%	7.7%	2.6%
2008	108	24.4%	9.0%	3.8%
2009	109	28.2%	10.3%	3.8%
2010	108	30.8%	10.3%	3.8%
2011	112	34.6%	16.7%	2.6%
2012	119	37.2%	21.8%	1.3%
2013	121	44.9%	26.9%	1.3%
2014	123	52.6%	32.1%	5.1%
2015	116	56.4%	38.5%	3.8%
2016	110	52.6%	42.3%	5.1%
2017	112	57.7%	42.3%	6.4%
2018	99	60.3%	44.9%	3.8%
2019	82	55.1%	44.9%	5.1%
2020	75	50.0%	39.7%	3.8%

Table 3. Descriptive statistics for main variables

Table 3 presents the descriptive statistics for main dependent, independent and control variables. All continuous variables are winsorized at 2.5% in each tail. We show definitions of each variable in the Table 1.

Variable	Mean	St. Dev.	Median
Sustainability Target	0.35	0.48	0.00
Sustainability Target *with Weight*	0.20	0.40	0.00
Sustainability Modifier	0.03	0.18	0.00
Carbon Emissions, t+1 (S&P's TrueCost)	1.70	2.75	0.00
Environmental violations penalty, t+1 (Violations Tracker)	2.94	5.38	0.00
Toxicity, t+1 (TRI data)	0.98	2.86	0.00
Green Patent Applications, t+1 (U.S. PTO)	0.30	0.30	0.00
Carbon Emissions, t-1	1.67	2.70	0.00
Environmental violations penalty, t-1	2.83	5.31	0.00
Toxicity, t-1 (TRI data)	1.06	2.96	0.00
Boardroom Accountability Project Company Indicator	0.04	0.20	0.00
Director Duties State Statutes	0.56	0.50	1.00
Log (Total Assets)	7.97	1.64	7.85
Leverage	0.33	0.19	0.33
Log(Cash/Assets)	3.73	2.31	3.64

Table 4. Adoption of Sust. Targets in Annual Incentive Plans (AIP)

Table 4 presents the results of a linear probability model of the propensity to observe an annual incentive plan with Sust. Target (columns 1 through 3), Sust. Target with Weight (columns 4 through 6), and an Sust. Modifier (columns 7, 8, and 9). We present tabulations at times t, t+1, and t+2. The independent variables are measured at t-1. We include firm and year fixed effects and double cluster independently the standard errors at the firm and year level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Sust. Target	Sust. Target	Sust. Target	Sust. Target w/ Weight	Sust. Target w/ Weight	Sust. Target w/ Weight	Sust. Modifier	Sust. Modifier	Sust. Modifier
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	*t*	*t+1*	*t+2*	*t*	*t+1*	*t+2*	*t*	*t+1*	*t+2*
DD Statutes Adoption	0.053	0.029	0.027	-0.028	-0.046	-0.051	-0.025	-0.008	0.008
	(0.720)	(0.364)	(0.341)	(-0.517)	(-0.775)	(-0.762)	(-0.523)	(-0.194)	(0.231)
BAP Focus Firm	0.079	0.048	0.080	0.208**	0.191*	0.219**	-0.055	-0.047	-0.050*
	(0.832)	(0.486)	(0.834)	(2.060)	(1.853)	(2.658)	(-1.486)	(-1.333)	(-1.800)
Lag CO_2 Emissions	0.004	0.001	-0.003	0.008	0.006	0.011	-0.004*	-0.006*	-0.004
	(0.321)	(0.049)	(-0.234)	(0.700)	(0.600)	(1.106)	(-1.879)	(-1.771)	(-0.957)
Log(Assets)	0.045	0.046	0.063*	0.012	0.014	0.028	-0.001	-0.003	-0.005
	(1.437)	(1.296)	(1.739)	(0.463)	(0.498)	(1.006)	(-0.059)	(-0.197)	(-0.335)
Book Leverage	-0.143	-0.229**	-0.227***	-0.024	-0.145	-0.182*	0.025	0.031	0.049
	(-1.300)	(-2.135)	(-2.870)	(-0.251)	(-1.358)	(-1.992)	(0.421)	(0.568)	(1.192)
Log(Cash/Assets)	0.014*	0.020**	0.021**	0.008	0.012	0.019**	-0.003	-0.006	-0.008
	(1.769)	(2.709)	(2.469)	(0.938)	(1.407)	(2.681)	(-0.369)	(-1.027)	(-1.636)
Observations	1,548	1,451	1,332	1,548	1,451	1,332	1,547	1,450	1,332
R-squared	0.682	0.696	0.702	0.632	0.638	0.642	0.492	0.520	0.558

Table 5A. CO_2 Emissions and Sustainability targets in Annual Incentive Plans

Table 5A presents the results of an OLS regression of the CO_2 emissions as a function of three key explanatory variables: Sust. Target, Sust. Target with Weight, and Sust. Modifier. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. We include firm and year fixed effects and double cluster independently the standard errors at the firm and year level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel I: Including Sust. Target, Sust. Target with Weight, and Sust. Modifier together

	(1)	(2)	(3)
VARIABLES	*t+1*	*t+2*	*t+3*
Sust. Target	0.230	0.243	0.167
	(1.315)	(1.085)	(0.645)
Sust. Target *with Weight*	-0.745***	-0.842***	-0.923***
	(-3.621)	(-3.599)	(-3.429)
Sust. Modifier	-0.002	0.255	0.413
	(-0.003)	(0.340)	(0.516)
CO_2 Emissions at (t-1)	0.423***	0.267***	0.147*
	(4.555)	(2.850)	(1.731)
Log(Assets)	-0.042	-0.020	0.063
	(-0.345)	(-0.143)	(0.413)
Book Leverage	-0.731	-0.942	-0.866
	(-1.332)	(-1.377)	(-1.254)
Log(Cash/Assets)	0.052	0.021	0.041
	(1.613)	(0.485)	(0.863)
Observations	1,543	1,543	1,543
R-squared	0.797	0.751	0.738

Panel II: Including Sust. Target, Sust. Target with Weight, and Sust. Modifier individually

Sust. Target	-0.152	-0.208	-0.337
	(-0.9)	(-1.03)	(-1.58)
Sust. Target with Weight	-0.613***	-0.711***	-0.844***
	(-3.6)	(-3.64)	(-4.55)
Sust. Modifier	0.107	0.384	0.593
	(0.15)	(0.51)	(0.74)

Table 5B. CO_2 Emissions and Sustainability targets in Annual Incentive Plans – Cross-Sectional Differences

This table presents the results of an OLS regression of the CO_2 emissions as a function of three key explanatory variables: Sust. Target, Sust. Target with Weight, and Sust. Modifier, and their interactions with past CO_2 emissions. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. We include firm and year fixed effects and double cluster independently the standard errors at the firm and year level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel I: Including Sust. Target, Sust. Target with Weight, and Sust. Modifier together

VARIABLES	(1) *t+1*	(2) *t+2*	(3) *t+3*
Sust. Target	0.168	0.382	0.453
	(0.685)	(1.390)	(1.332)
CO_2 Emissions at (t-1)	0.467***	0.000	0.000
	(5.507)	(3.496)	(0.000)
Sust. Target * CO_2 Emissions at (t-1)	0.006	-0.062	-0.115
	(0.123)	(-1.062)	(-1.638)
Sust. Target *with Weight*	0.267	-0.083	-0.212
	(0.803)	(-0.281)	(-0.500)
Sust. Target *with Weight* * CO_2 Emissions at (t-1)	-0.211***	-0.142**	-0.119
	(-3.022)	(-2.081)	(-1.367)
Sust. Modifier	0.556	0.655	0.767
	(0.816)	(0.861)	(0.845)
Sust. Modifier* CO_2 Emissions at (t-1)	-0.197***	-0.149***	-0.139**
	(-5.885)	(-3.229)	(-2.208)
Log(Assets)	-0.076	-0.061	0.013
	(-0.632)	(-0.451)	(0.088)
Book Leverage	-0.662	-0.867	-0.777
	(-1.213)	(-1.261)	(-1.122)
Log(Cash/Assets)	0.063*	0.030	0.049
	(1.889)	(0.658)	(1.015)
Observations	1,543	1,543	1,543
R-squared	0.800	0.754	0.742

Panel II: Including Sust. Target, Sust. Target with Weight, and Sust. Modifier individually

VARIABLES	(1) *t+1*	(2) *t+2*	(3) *t+3*
Sust. Target	0.168	0.269	0.29
	(0.71)	(1.03)	(1.08)
Sust. Target * CO_2 Emissions at (t-1)	-0.095*	-0.142***	-0.187***
	(-1.72)	(-2.52)	(-3.25)
Sust. Target *with Weight*	0.345	0.156	0.079
	(1.09)	(0.52)	(0.25)
Sust. Target with Weight * CO_2 Emissions at (t-1)	-0.20***	-0.181***	-0.193***
	(-3.08)	(-2.87)	(-2.83)
Sust. Modifier	0.554	0.65	0.781
	(0.78)	(0.83)	(0.85)
Sust. Modifier* CO_2 Emissions at (t-1)	-0.153***	-0.091***	-0.064
	(-5.07)	(-2.88)	(-0.93)

Table 6A. CO_2 Emissions and Sustainability targets in Annual Incentive Plans – Instrumental Variables Analysis

This table presents the results of an instrumental variables (IV) regression of CO_2 emissions as a function of three key explanatory variables: Sust. Target, Sust. Target with Weight, and Sust. Modifier, included separately. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. Instrumental variables include: BAP Indicator, DD State Indicator and Industry-Year Sustainability Practices. We include firm and year fixed effects and double cluster independently the standard errors at the firm and year level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Sust. Target			Sust. Weight			Sust. Modifier		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*
Sust. Target	-0.853	-1.547	-2.469**						
	(-1.157)	(-1.480)	(-2.287)						
Sust. Target *with Weight*				-1.590	-2.526**	-2.891**			
				(-1.693)	(-2.070)	(-2.158)			
Sust. Modifier							0.548	0.713	2.475
							(0.541)	(0.471)	(1.448)
CO_2 Emissions at (t-1)	0.417***	0.265***	0.149*	0.426***	0.278***	0.161*	0.423***	0.267***	0.152*
	(4.554)	(2.933)	(1.811)	(4.767)	(3.077)	(1.918)	(4.521)	(2.801)	(1.758)
Log(Assets)	0.005	0.056	0.173	-0.031	-0.009	0.070	-0.030	-0.004	0.072
	(0.036)	(0.340)	(0.951)	(-0.258)	(-0.067)	(0.478)	(-0.235)	(-0.024)	(0.439)
Book Leverage	-0.894	-1.181*	-1.212	-0.822	-1.038	-0.943	-0.726	-0.912	-0.876
	(-1.579)	(-1.720)	(-1.550)	(-1.539)	(-1.566)	(-1.364)	(-1.238)	(-1.280)	(-1.194)
Log(Cash/Assets)	0.061	0.040	0.071	0.066*	0.043	0.064	0.049	0.016	0.040
	(1.639)	(0.764)	(1.233)	(1.854)	(0.876)	(1.269)	(1.396)	(0.336)	(0.703)
Observations	1,546	1,546	1,546	1,548	1,548	1,548	1,547	1,547	1,547
R-squared	0.136	0.005	-0.110	0.136	-0.010	-0.062	0.152	0.058	0.001
Cragg-Donald	16.6	16.6	16.6	30.4	30.4	30.4	36	36	36
Stock-Yogo 10%	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1
Hansen J-Stat p-value	0.18	0.18	0.49	0.28	0.38	0.82	0.18	0.18	0.49

Table 6B. CO_2 Emissions and Sustainability targets in Annual Incentive Plans – Cross-Sectional Instrumental Variables Analysis

Instrumental variables (IV) regression of CO_2 emissions on three key explanatory variables: Sust. Target, Sust. Target with Weight, and Sust. Modifier, included separately, and their interactions with lagged CO_2 emissions. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. Instrumental variables include: BAP Indicator, DD State Indicator and Industry-Year Sustainability Practices; and their interactions with lagged CO_2 emissions. We include but not shown are firm and year fixed effects and controls from Table 6A; double cluster independently the standard errors at the firm and year level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Sust. Target			Sust. Weight			Sust. Modifier		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*
Sust. Target	3.793**	4.416**	3.376*						
	(2.319)	(2.333)	(1.912)						
Sust. Target * CO_2 Emissions	-0.901***	-1.042***	-0.963***						
	(-3.250)	(-4.191)	(-4.254)						
Sust. Target *with Weight*				6.603**	5.052**	3.500*			
				(2.472)	(2.156)	(1.717)			
Sust. Target *with Weight* * CO_2 Emissions				-1.526***	-1.319***	-1.078***			
				(-3.816)	(-4.141)	(-4.537)			
Sust. Modifier							2.398	2.627	2.969
							(0.785)	(0.778)	(0.883)
Sust. Modifier * CO_2 Emissions							-0.596	-0.641	-0.252
							(-0.817)	(-0.862)	(-0.394)
CO_2 Emissions at (t-1)	0.764***	0.663***	0.516***	0.667***	0.482***	0.327***	0.461***	0.307***	0.167*
	(6.315)	(5.232)	(4.404)	(6.591)	(4.627)	(3.382)	(5.249)	(3.171)	(1.862)
Observations	1,546	1,546	1,546	1,546	1,546	1,546	1,545	1,545	1,545
R-squared	-0.186	-0.323	-0.225	-0.265	-0.220	-0.142	0.137	0.035	0.004
Cragg-Donald	9.1	9.1	9.1	15.8	15.8	15.8	13.3	13.3	13.3
Stock-Yogo 10%	9.5	9.5	9.5	9.5	9.5	9.5	9.5	9.5	9.5

Hansen J-Stat p-value	0.53	0.78	0.57	0.33	0.16	0.11	0.16	0.17	0.23

Table 7A. Environmental Violation Penalties and Sustainability targets in Annual Incentive Plans

Table 7A presents the results of an OLS regression of the environmental violation penalties as a function of three key explanatory variables: Sust. Target, Sust. Target with Weight, and Sust. Modifier. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. We include firm and year fixed effects and double cluster independently the standard errors at the firm and year level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel I: Including Sust. Target, Sust. Target with Weight, and Sust. Modifier together

	(1)	(2)	(3)
VARIABLES	*t+1*	*t+2*	*t+3*
Sust. Target	0.262	0.385	0.402
	(0.476)	(0.652)	(0.838)
Sust. Target with Weight	-1.126**	-0.867	-1.374***
	(-2.176)	(-1.425)	(-3.505)
Sust. Modifier	1.612**	1.543*	1.780
	(2.137)	(1.957)	(1.339)
Environmental Penalty at (t-1)	0.163***	0.104*	0.026
	(3.162)	(1.977)	(0.527)
Log(Assets)	0.569*	0.646*	0.732**
	(1.828)	(1.734)	(2.056)
Book Leverage	-1.281	-1.648	-1.886*
	(-1.544)	(-1.409)	(-1.828)
Log(Cash/Assets)	0.108	-0.039	-0.015
	(1.251)	(-0.460)	(-0.167)
Observations	1,544	1,544	1,544
R-squared	0.624	0.603	0.581

Panel II: Including Sust. Target, Sust. Target with Weight, and Sust. Modifier individually

Sust. Target	-0.442	-0.181	-0.442
	(-1.14)	(-0.40)	(-0.88)
Sust. Target with Weight	-1.068***	-0.725*	-1.231***
	(-3.09)	(-1.67)	(-2.87)
Sust. Modifier	1.813***	1.628**	1.989
	(2.42)	(2.02)	(1.48)

Table 7B. Environmental Violation Penalties and Sustainability targets in Annual Incentive Plans – Cross-Sectional Differences

This table presents the results of an OLS regression of the environmental violation penalties as a function of three key explanatory variables: Sust. Target, Sust. Target with Weight, and Sust. Modifier, and their interactions with past environmental penalty. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. We include firm and year fixed effects and double cluster independently the standard errors at the firm and year level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel I: Including Sust. Target, Sust. Target with Weight, and Sust. Modifier together

VARIABLES	(1) *t+1*	(2) *t+2*	(3) *t+3*
Sust. Target	-0.222	0.183	0.577
	(-0.356)	(0.289)	(1.451)
Env. Penalty at (t-1)	0.185***	0.151**	0.104
	(3.651)	(2.404)	(1.564)
Sust. Target * Env. Penalty at (t-1)	0.086	0.018	-0.066
	(1.312)	(0.193)	(-0.709)
Sust. Target with Weight	0.008	0.264	-0.414
	(0.012)	(0.408)	(-0.890)
Sust. Target with Weight * Env. Penalty at (t-1)	-0.191**	-0.181*	-0.140
	(-2.088)	(-1.847)	(-1.659)
Sust. Modifier	1.976	1.458	1.909
	(1.705)	(1.654)	(1.461)
Sust. Modifier* Env. Penalty at (t-1)	-0.104	0.038	-0.019
	(-0.514)	(0.285)	(-0.166)
Log(Assets)	0.561*	0.616*	0.682*
	(1.884)	(1.708)	(1.993)
Book Leverage	-1.392*	-1.789	-2.017*
	(-1.736)	(-1.553)	(-2.011)
Log(Cash/Assets)	0.106	-0.055	-0.038
	(1.193)	(-0.635)	(-0.443)
Observations	1,544	1,544	1,544
R-squared	0.627	0.607	0.586

Panel II: Including Sust. Target, Sust. Target with Weight, and Sust. Modifier individually

VARIABLES	(1) *t+1*	(2) *t+2*	(3) *t+3*
Sust. Target	-0.309	0.274	0.279
	(-0.71)	(0.57)	(0.65)
Sust. Target * Env. Penalty at (t-1)	-0.029	-0.099	-0.158**
	(-0.47)	(-1.50)	(-2.11)
Sust. Target with Weight	-0.342	0.28	-0.135
	(-0.74)	(0.59)	(-0.28)
Sust. Target with Weight * Env. Penalty at (t-1)	-0.123*	-0.17***	-0.186***
	(-1.73)	(-2.62)	(-2.80)
Sust. Modifier	2.122*	1.358	1.815
	(1.94)	(1.52)	(1.32)
Sust. Modifier* Env. Penalty at (t-1)	-0.094	0.082	0.053
	(-0.46)	(0.68)	(0.48)

Table 8A. Environmental Violation Penalties and Sustainability targets in Annual Incentive Plans – Instrumental Variables

This table presents the results of an instrumental variables (IV) regression of environmental violation penalties as a function of three key explanatory variables: SUST. Target, SUST. Target with Weight, and SUST. Modifier, included separately. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. Instrumental variables include: BAP Indicator, DD State Indicator and Industry-Year Sustainability Practices. We include firm and year fixed effects and double cluster independently the standard errors at the firm and year level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	ESG-Target			SUST. Weight			SUST. Modifier		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*
SUST. Target	3.977	3.037	3.433						
	(1.620)	(1.596)	(1.207)						
Sust. Target *with Weight*				-0.577	-1.750	-1.457			
				(-0.362)	(-0.988)	(-0.577)			
Sust. Modifier							-0.853	-1.547	-2.469**
							(-1.157)	(-1.480)	(-2.287)
Env. Penalty at (t-1)	0.159***	0.102*	0.023	0.163***	0.103*	0.025	0.417***	0.265***	0.149*
	(3.074)	(1.965)	(0.443)	(3.132)	(1.958)	(0.508)	(4.554)	(2.933)	(1.811)
Log(Assets)	0.417	0.538	0.612	0.579*	0.661*	0.752**	0.005	0.056	0.173
	(1.095)	(1.309)	(1.462)	(1.851)	(1.865)	(2.145)	(0.036)	(0.340)	(0.951)
Book Leverage	-0.614	-1.154	-1.286	-1.245	-1.691	-1.871*	-0.894	-1.181*	-1.212
	(-0.577)	(-0.834)	(-0.985)	(-1.612)	(-1.557)	(-1.911)	(-1.579)	(-1.720)	(-1.550)
Log(Cash/Assets)	0.030	-0.097	-0.085	0.100	-0.030	-0.015	0.061	0.040	0.071
	(0.328)	(-1.059)	(-0.926)	(1.288)	(-0.350)	(-0.165)	(1.639)	(0.764)	(1.233)
Observations	1,546	1,546	1,546	1,546	1,546	1,546	1,546	1,546	1,546
R-squared	-0.072	-0.036	-0.067	0.041	0.019	0.020	0.136	0.005	-0.110
Cragg-Donald	16.6	16.6	16.6	30.5	30.5	30.5	36.7	36.7	36.7
Stock-Yogo 10%	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1
Hansen J-Stat p-value	0.18	0.15	0.24	0.22	0.15	0.25	0.49	0.58	0.7

Table 8B. Environmental Violation Penalties and Sustainability targets in Annual Incentive Plans – Cross-Sectional IV Analysis

IV regression of environmental violation penalties on three key explanatory variables: Sust. Target, Sust. Target with Weight, and Sust. Modifier, included separately, and their interactions with lagged CO_2 emissions. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. Instrumental variables include: BAP Indicator, DD State Indicator and Industry-Year Sustainability Practices; and their interactions with lagged CO_2 emissions. We include but not shown are firm and year fixed effects and controls from Table 6A; double cluster independently the standard errors at the firm and year level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Sust. Target			Sust. Weight			Sust. Modifier		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*
Sust. Target	2.292	4.494**	5.605**						
	(1.132)	(2.669)	(2.365)						
Sust. Target * Env. Pen. at t-1	-0.060	-0.238	-0.521***						
	(-0.377)	(-1.366)	(-2.864)						
Sust. Target *with Weight*				-0.568	1.369	3.012			
				(-0.381)	(0.733)	(1.243)			
Sust. Target *with Weight* * Env. Penalty				-0.152	-0.307*	-0.523***			
				(-1.008)	(-1.970)	(-3.159)			
Sust. Modifier							14.215**	10.646*	6.261
							(2.665)	(1.777)	(1.203)
Sust. Modifier * Env. Pen. at t-1							-1.244*	-0.066	0.878
							(-1.775)	(-0.086)	(0.832)
Env. Penalty at (t-1)	0.191**	0.220**	0.282**	0.209***	0.200**	0.190**	0.225***	0.114	-0.010
	(2.300)	(2.085)	(2.549)	(3.071)	(2.509)	(2.449)	(3.721)	(1.685)	(-0.133)
Observations	1,546	1,546	1,546	1,546	1,546	1,546	1,545	1,545	1,545
R-squared	0.004	-0.053	-0.097	0.046	0.026	-0.010	-0.115	-0.066	-0.078
Cragg-Donald	10.6	10.6	10.6	18.8	18.8	18.8	9.6	9.6	9.6
Stock-Yogo 10%	9.5	9.5	9.5	9.5	9.5	9.5	9.5	9.5	9.5
Hansen J-Stat p-value	0.36	0.42	0.41	0.27	0.48	0.36	0.9	0.6	0.46

Table 9A. TRI Toxicity and Sustainability targets in Annual Incentive Plans

Table 9A presents the results of an OLS regression of the TRI Toxicity as a function of three key explanatory variables: Sust. Target, Sust. Target with Weight, and Sust. Modifier. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. We include facility-chemical and year-chemical fixed effects and controls from Table 6A; double cluster independently the standard errors at the facility-chemical and year-chemical level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel I: Including Sust. Target, Sust. Target with Weight, and Sust. Modifier together

	(1)	(2)	(3)
VARIABLES	*t+1*	*t+2*	*t+3*
Sust. Target	0.020	0.031	0.010
	(0.730)	(0.988)	(0.309)
Sust. Target *with Weight*	0.038	-0.013	0.036
	(1.468)	(-0.358)	(1.053)
Sust. Modifier	-0.202**	-0.143	-0.217
	(-2.205)	(-1.048)	(-1.263)
TRI Toxicity at (t-1)	0.410***	0.265***	0.198***
	(18.840)	(10.989)	(8.473)
Log(Assets)	-0.013	-0.000	0.004
	(-0.569)	(-0.004)	(0.131)
Book Leverage	0.216	-0.004	-0.302
	(1.338)	(-0.019)	(-1.643)
Log(Cash/Assets)	0.008	0.010	-0.000
	(0.833)	(0.980)	(-0.044)
Observations	23,667	19,069	18,361
R-squared	0.940	0.936	0.934

Panel II: Including Sust. Target, Sust. Target with Weight, and Sust. Modifier individually

Sust. Target	0.018	0.024	0.009
	(0.77)	(0.87)	(0.32)
Sust. Target with Weight	0.036	-0.005	0.031
	(1.50)	(-0.14)	(0.97)
Sust. Modifier	-0.173*	-0.134	-0.17
	(-1.85)	(-1.01)	(-1.29)

Table 9B. TRI Toxicity and Sustainability targets in Annual Incentive Plans – Cross-Sectional Differences

This table presents the results of an OLS regression of the TRI Toxicity as a function of three key explanatory variables: Sust. Target, Sust. Target with Weight, and SUST. Modifier, and their interactions with past CO_2 emissions. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. We include facility-chemical and year-chemical fixed effects and controls from Table 6A; double cluster independently the standard errors at the facility-chemical and year-chemical level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel I: Including Sust. Target, Sust. Target with Weight, and Sust. Modifier together

VARIABLES	(1) *t+1*	(2) *t+2*	(3) *t+3*
Sust. Target	0.053	0.085**	0.074*
	(1.547)	(1.989)	(1.761)
TRI Toxicity at (t-1)	0.421***	0.278***	0.212***
	(17.969)	(10.710)	(8.265)
Sust. Target * TRI Toxicity at (t-1)	-0.014	-0.023*	-0.027**
	(-1.174)	(-1.716)	(-2.039)
Sust. Target *with Weight*	0.081**	0.037	0.064*
	(2.517)	(0.941)	(1.732)
Sust. Target *with Weight* * TRI Toxicity at (t-1)	-0.023	-0.026	-0.016
	(-1.611)	(-1.445)	(-1.057)
Sust. Modifier	-0.132	-0.047	-0.035
	(-1.478)	(-0.515)	(-0.359)
Sust. Modifier* TRI Toxicity at (t-1)	-0.143*	-0.201	-0.351
	(-1.780)	(-1.455)	(-1.458)
Log(Assets)	-0.015	-0.002	0.001
	(-0.628)	(-0.074)	(0.036)
Book Leverage	0.205	-0.010	-0.309*
	(1.261)	(-0.048)	(-1.677)
Log(Cash/Assets)	0.009	0.011	0.001
	(0.923)	(1.057)	(0.061)
Observations	23,667	19,069	18,361
R-squared	0.940	0.937	0.934

Panel II: Including Sust. Target, Sust. Target with Weight, and Sust. Modifier individually

VARIABLES	(1) *t+1*	(2) *t+2*	(3) *t+3*
Sust. Target	0.063**	0.08**	0.073*
	(2.05)	(2.13)	(1.88)
Sust. Target * TRI Toxicity at (t-1)	-0.02*	-0.025**	-0.028***
	(-1.86)	(-1.99)	(-2.26)
Sust. Target *with Weight*	0.094***	0.075*	0.091***
	(2.96)	(1.94)	(2.49)
Sust. Target with Weight * TRI Toxicity at (t-1)	-0.031**	-0.041**	-0.032**
	(-2.15)	(-2.21)	(-2.08)
Sust. Modifier	-0.057	0.029	-0.071
	(-0.67)	(0.37)	(-0.70)
Sust. Modifier* TRI Toxicity at (t-1)	-0.171**	-0.242*	-0.088
	(-2.09)	(-1.71)	(-1.61)

Table 10A. TRI Toxicity and Sustainability targets in Annual Incentive Plans – Instrumental Variables Analysis

This table presents the results of an instrumental variables (IV) regression of TRI Toxicity as a function of three key explanatory variables: Sust. Target, Sust. Target with Weight, and Sust. Modifier, included separately. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. Instrumental variables include: BAP Indicator, DD State Indicator and Industry-Year Sustainability Practices. We include facility-chemical and year-chemical fixed effects and controls from Table 6A; double cluster independently the standard errors at the facility-chemical and year-chemical level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Sust. Target			Sust. Weight			Sust. Modifier		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*
Sust. Target	-0.019	-0.048	-0.012						
	(-0.558)	(-1.128)	(-0.292)						
Sust. Target *with Weight*				-0.020	-0.084	-0.036			
				(-0.495)	(-1.501)	(-0.661)			
Sust. Modifier							0.049	0.558	-0.195
							(0.071)	(0.746)	(-0.265)
TRI Toxicity at (t-1)	0.408***	0.261***	0.197***	0.410***	0.266***	0.198***	0.408***	0.261***	0.197***
	(19.653)	(11.232)	(8.699)	(18.833)	(10.992)	(8.476)	(19.646)	(11.234)	(8.693)
Log(Assets)	-0.038	-0.025	-0.020	-0.027	-0.022	-0.010	-0.034	-0.017	-0.019
	(-1.620)	(-0.794)	(-0.671)	(-1.144)	(-0.654)	(-0.306)	(-1.443)	(-0.522)	(-0.586)
Book Leverage	0.390***	0.243	-0.055	0.173	-0.052	-0.348*	0.379**	0.180	-0.051
	(2.762)	(1.433)	(-0.332)	(1.078)	(-0.253)	(-1.865)	(2.439)	(0.937)	(-0.296)
Log(Cash/Assets)	0.021**	0.021**	0.011	0.019**	0.024**	0.010	0.019**	0.019*	0.009
	(2.428)	(2.158)	(1.132)	(1.985)	(2.208)	(0.922)	(2.265)	(1.879)	(0.984)
Observations	25,029	20,325	19,556	23,667	19,069	18,361	25,029	20,325	19,556
R-squared	0.183	0.075	0.044	0.186	0.079	0.045	0.183	0.074	0.044
Cragg-Donald	7455.6	10,000	6234.6	13,000	12,000	8222.4	549.4	1125	735.3
Stock-Yogo 10%	9.1	20	9.1	9.1	20	9.1	9.1	20	9.1
Hansen J-Stat p-value	0.52	0.69	0.81	0.59	0.94	0.93	0.47	0.81	0.82

Table 10B. TRI Toxicity and Sustainability targets in Annual Incentive Plans – Cross-Sectional IV Analysis

IV regression of TRI Toxicity on three key explanatory variables: Sust. Target, Sust. Target with Weight, and Sust. Modifier, included separately, and their interactions with lagged CO_2 emissions. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. Instrumental variables include: BAP Indicator, DD State Indicator and Industry-Year Sustainability Practices; and their interactions with lagged CO_2 emissions. We include facility-chemical and year-chemical fixed effects and controls from Table 6A; double cluster independently the standard errors at the facility-chemical and year-chemical level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

	Sust Target			Sust. Weight			Sust. Modifier		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*
Sust. Target	0.125*	0.181*	0.279***						
	(1.729)	(1.949)	(2.840)						
Sust. Target * TRI Toxicity	-0.063**	-0.101***	-0.127***						
	(-2.312)	(-2.831)	(-3.248)						
Sust. Target *with Weight*				0.296**	1.142***	0.584***			
				(2.248)	(3.207)	(2.589)			
Sust. Target *with Weight* * TRI Toxicity				-0.164**	-0.615***	-0.316***			
				(-2.507)	(-3.403)	(-2.834)			
Sust. Modifier							-2.293	-1.246	-1.366
							(-1.347)	(-0.704)	(-0.793)
Sust. Modifier * TRI Toxicity							3.408	2.740	1.123
							(1.321)	(1.045)	(0.690)
TRI Toxicity at (t-1)	0.439***	0.303***	0.251***	0.439***	0.319***	0.231***	0.406***	0.260***	0.195***
	(16.745)	(10.104)	(8.063)	(17.021)	(10.525)	(8.515)	(19.406)	(11.175)	(8.607)
Observations	25,029	20,325	19,556	23,667	19,069	18,361	25,029	20,325	19,556
R-squared	0.181	0.069	0.032	0.174	-0.100	-0.004	0.134	0.034	0.024
Cragg-Donald	1265.7	1316.4	861	287.5	126.1	154.5	20.1	34	18
Stock-Yogo 10%	9.5	7.6	9.5	9.5	7.6	9.5	9.5	7.6	9.5
Hansen J-Stat p-value	0.43	0.09	0.19	0.25	0.9	0.01	0.14	0.02	0.01

Table 11A. Green Patenting and Sustainability targets in Annual Incentive Plans

Table 11A presents the results of an OLS regression of the TRI Toxicity as a function of three key explanatory variables: Sust. Target, Sust. Target with Weight, and Sust. Modifier. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. We include firm and year fixed effects and double cluster independently the standard errors at the firm and year level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)
VARIABLES	*t+1*	*t+2*	*t+3*
Sust. Target	0.096	0.091	0.086
	(1.462)	(1.281)	(0.828)
Sust. Target *with Weight*	-0.076	-0.070	-0.096
	(-0.938)	(-0.790)	(-0.776)
Sust. Modifier	0.037	0.026	0.025
	(1.302)	(0.955)	(1.079)
CO_2 Emissions at (t-1)	0.008	0.006	-0.002
	(1.402)	(1.096)	(-0.648)
Log(Assets)	0.007	0.017	0.028
	(0.772)	(1.673)	(1.440)
Book Leverage	0.059*	0.075*	0.052*
	(1.832)	(1.723)	(1.857)
Log(Cash/Assets)	-0.002	-0.002	-0.002
	(-0.719)	(-0.497)	(-0.522)
Observations	1,431	1,286	1,139
R-squared	0.518	0.500	0.525

Table 11B. Green Patenting and Sustainability targets in Annual Incentive Plans – Cross-Sectional Differences

This table presents the results of an OLS regression of the TRI Toxicity as a function of three key explanatory variables: Sust. Target, Sust. Target with Weight, and Sust. Modifier, and their interactions with past CO_2 emissions. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. We include firm and year fixed effects and double cluster independently the standard errors at the firm and year level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

VARIABLES	(1) *t+1*	(2) *t+2*	(3) *t+3*
Sust. Target	0.078*	0.048	0.090
	(1.893)	(1.405)	(0.773)
CO_2 Emissions at (t-1)	0.006	0.000	0.000
	(1.456)	(0.000)	(0.000)
Sust. Target * CO_2 Emissions at (t-1)	0.006	0.015	-0.003
	(0.424)	(0.855)	(-0.434)
Sust. Target *with Weight*	-0.050*	-0.025	-0.046
	(-1.989)	(-0.776)	(-0.698)
Sust. Target *with Weight* * CO_2 Emissions at (t-1)	-0.007	-0.013	-0.009
	(-0.342)	(-0.511)	(-0.752)
Sust. Modifier	0.035	0.024	0.012
	(1.058)	(0.701)	(0.376)
Sust. Modifier* CO_2 Emissions at (t-1)	0.002	0.002	0.004
	(0.453)	(0.625)	(0.878)
Log(Assets)	0.008	0.020	0.024
	(0.808)	(1.535)	(1.631)
Book Leverage	0.058*	0.067	0.053*
	(1.886)	(1.595)	(1.769)
Log(Cash/Assets)	-0.002	-0.002	-0.002
	(-0.719)	(-0.488)	(-0.450)
Observations	1,434	1,289	1,143
R-squared	0.518	0.501	0.526

Table 12A. Green Patenting in Annual Incentive Plans – Instrumental Variables Analysis

This table presents the results of an instrumental variables (IV) regression of Green Patenting as a function of three key explanatory variables: Sust. Target, Sust. Target with Weight, and Sust. Modifier, included separately. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. Instrumental variables include: BAP Indicator, DD State Indicator and Industry-Year Sustainability Practices. We include firm and year fixed effects and double cluster independently the standard errors at the firm and year level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Dep. Var.: Green Pat. App.									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*
Sust. Target	0.592*	0.569	0.379						
	(1.625)	(1.353)	(1.016)						
Sust. Target *with Weight*				0.141	0.117	-0.002			
				(0.879)	(0.672)	(-0.015)			
Sust. Modifier							-0.103	0.074	0.523
							(-0.855)	(0.994)	(1.016)
CO_2 Emissions at (t-1)	0.007	0.002	-0.003	0.007	0.005	-0.003	0.007	0.006	0.000
	(0.822)	(0.275)	(-0.587)	(1.366)	(1.170)	(-0.911)	(1.349)	(1.215)	(0.474)
Log(Assets)	-0.004	0.008	0.024	0.013	0.023	0.030	0.010	0.020	0.029
	(-0.226)	(0.391)	(1.141)	(1.042)	(1.467)	(1.320)	(0.854)	(1.400)	(1.195)
Book Leverage	0.165	0.192	0.146	0.064	0.068	0.035	0.051*	0.055	0.021
	(1.565)	(1.541)	(1.363)	(1.654)	(1.665)	(1.072)	(1.867)	(1.584)	(0.521)
Log(Cash/Assets)	-0.010	-0.009	-0.007	-0.004	-0.003	-0.001	-0.002	-0.002	-0.001
	(-1.213)	(-0.896)	(-0.780)	(-0.955)	(-0.637)	(-0.395)	(-0.662)	(-0.381)	(-0.151)
Observations	1,435	1,290	1,145	1,435	1,290	1,145	1,435	1,290	1,144
R-squared	-0.286	-0.237	-0.109	-0.017	-0.010	0.004	-0.001	0.002	-0.054
Cragg-Donald	14.6	12.5	11.5	26	21.5	16.2	36.1	34.6	32.2
Stock-Yogo 10%	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1
Hansen J-Stat p-value	0.46	0.6	0.56	0.44	0.65	.	0.15	.	.

Table 13A. Profitability and Sales Growth and Sustainability targets in Annual Incentive Plans

Table 13A presents the results of an OLS regression of sales growth and profitability as a function of three key explanatory variables: Sust. Target, Sust. Target with Weight, and Sust. Modifier. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. We include firm and year fixed effects and double cluster independently the standard errors at the firm and year level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	*Sales Growth t+1*	*Sales Growth t+2*	*Sales Growth t+3*	*ROE t+1*	*ROE t+2*	*ROE t+3*
Sust. Target	0.051	0.028	0.038	0.033	0.007	-0.022
	(1.018)	(0.685)	(0.928)	(0.665)	(0.175)	(-0.608)
Sust. Target *with Weight*	0.006	-0.033	-0.126**	-0.041	0.053	0.111*
	(0.110)	(-0.603)	(-2.245)	(-0.582)	(0.597)	(1.829)
Sust. Modifier	0.144**	0.034	0.078	0.026	0.048	-0.087
	(2.445)	(0.851)	(1.705)	(0.514)	(0.958)	(-1.027)
CO_2 Emissions at (t-1)	-0.015	0.002	0.008	0.017	-0.000	0.010
	(-1.490)	(0.163)	(0.634)	(1.562)	(-0.029)	(1.011)
Log(Assets)	-0.065**	-0.123***	-0.109***	-0.063***	-0.049	-0.007
	(-2.187)	(-4.224)	(-4.216)	(-3.875)	(-1.529)	(-0.345)
Book Leverage	-0.222	0.006	-0.029	0.183	0.327*	0.436***
	(-1.142)	(0.028)	(-0.137)	(1.252)	(1.838)	(3.174)
Log(Cash/Assets)	0.005	0.004	0.001	0.017*	0.018**	0.001
	(0.731)	(0.433)	(0.112)	(1.709)	(2.559)	(0.081)
Observations	1,426	1,282	1,137	1,430	1,283	1,139
R-squared	0.516	0.527	0.536	0.282	0.271	0.268

Table 13B. Profitability and Sales Growth and Sustainability targets in Annual Incentive Plans – Cross-Sectional Differences

This table presents the results of an OLS regression of sales growth and profitability as a function of three key explanatory variables: Sust. Target, Sust. Target with Weight, and Sust. Modifier, and their interactions with past CO_2 emissions. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. We include firm and year fixed effects and double cluster independently the standard errors at the firm and year level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	*Sales Growth t+1*	*Sales Growth t+2*	*Sales Growth t+3*	*ROE t+1*	*ROE t+2*	*ROE t+3*
Sust. Target	0.079	0.082	0.115*	0.071	0.003	0.033
	(1.353)	(1.683)	(2.050)	(1.224)	(0.055)	(0.583)
CO_2 Emissions at (t-1)	-0.010	0.008	0.012	0.026**	0.005	0.019*
	(-0.835)	(0.498)	(0.842)	(2.317)	(0.436)	(1.907)
Sust. Target * CO_2 Emissions at (t-1)	-0.011	-0.019*	-0.02***	-0.015*	-0.001	-0.021*
	(-0.874)	(-1.841)	(-2.948)	(-1.938)	(-0.164)	(-1.853)
Sust. Target *with Weight*	0.057	-0.071	-0.33**	0.043	0.188	0.143
	(0.481)	(-0.730)	(-2.591)	(0.415)	(1.582)	(1.112)
Sust. Target *with Weight* * CO_2 Emissions at (t-1)	-0.007	0.012	0.047**	-0.014	-0.028	-0.002
	(-0.302)	(0.754)	(2.309)	(-0.704)	(-1.086)	(-0.075)
Sust. Modifier	0.128*	0.029	0.141**	0.096	0.122	-0.041
	(1.783)	(0.442)	(2.580)	(1.422)	(1.644)	(-0.352)
Sust. Modifier* CO_2 Emissions at (t-1)	0.001	-0.001	-0.021	-0.027	-0.028	-0.021
	(0.114)	(-0.077)	(-0.995)	(-1.291)	(-1.046)	(-0.546)
Log(Assets)	-0.070**	-0.13***	-0.11***	-0.07***	-0.054	-0.017
	(-2.365)	(-4.530)	(-4.311)	(-3.844)	(-1.620)	(-0.762)
Book Leverage	-0.210	0.016	-0.029	0.197	0.337*	0.448***
	(-1.067)	(0.069)	(-0.135)	(1.332)	(1.871)	(3.326)
Log(Cash/Assets)	0.005	0.004	0.001	0.018*	0.018**	0.001
	(0.743)	(0.433)	(0.069)	(1.776)	(2.572)	(0.120)
Observations	1,426	1,282	1,137	1,430	1,283	1,139
R-squared	0.517	0.529	0.540	0.284	0.274	0.271

Table 14A. Profitability and Sales Growth and Sustainability targets in Annual Incentive Plans – Instrumental Variables Analysis

Panel I. This table presents the results of an instrumental variables (IV) regression of sales growth as a function of three key explanatory variables: Sust. Target, Sust. Target with Weight, and Sust. Modifier, included separately. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. Instrumental variables include: BAP Indicator, DD State Indicator and Industry-Year Sustainability Practices. We include firm and year fixed effects and double cluster independently the standard errors at the firm and year level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: Sales Growth									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*
Sust. Target	0.032	-0.045	-0.077						
	(0.169)	(-0.212)	(-0.317)						
Sust. Target *with Weight*				0.278*	-0.322*	-0.406			
				(1.651)	(-2.022)	(-1.607)			
Sust. Modifier							0.246	0.057	0.295
							(0.699)	(0.246)	(0.912)
CO_2 Emissions at (t-1)	-0.016	0.002	0.007	-0.016	0.004	0.010	-0.015	0.003	0.008
	(-1.531)	(0.172)	(0.589)	(-1.448)	(0.329)	(0.788)	(-1.419)	(0.196)	(0.654)
Log(Assets)	-0.065**	-0.121***	-0.105***	-0.058*	-0.129***	-0.113***	-0.063**	-0.119***	-0.108***
	(-2.192)	(-4.085)	(-4.130)	(-1.739)	(-4.382)	(-3.817)	(-2.108)	(-4.017)	(-4.271)
Book Leverage	-0.219	-0.007	-0.047	-0.194	-0.027	-0.068	-0.235	0.011	-0.040
	(-1.062)	(-0.029)	(-0.199)	(-0.989)	(-0.114)	(-0.295)	(-1.186)	(0.046)	(-0.181)
Log(Cash/Assets)	0.005	0.004	0.001	0.002	0.007	0.004	0.006	0.003	0.001
	(0.787)	(0.455)	(0.136)	(0.317)	(0.808)	(0.386)	(0.833)	(0.323)	(0.121)

(Continued)

Observations	1,427	1,283	1,139	1,427	1,283	1,139	1,427	1,283	1,138
R-squared	0.019	0.031	0.025	-0.011	-0.014	-0.012	0.017	0.032	0.022
Cragg-Donald	14.3	12.3	11.7	26.1	21.6	16.2	37.7	36.6	35.1
Stock-Yogo 10%	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1
Hansen J-Stat p-value	0.64	0.63	0.24	0.63	0.47	0.45	0.44	0.68	0.35

Panel II. This table presents the results of an instrumental variables (IV) regression of ROE as a function of three key explanatory variables: Sust. Target, Sust. Target with Weight, and Sust. Modifier, included separately. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. Instrumental variables include: BAP Indicator, DD State Indicator and Industry-Year Sustainability Practices. We include firm and year fixed effects and double cluster independently the standard errors at the firm and year level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: ROE									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*
Sust. Target	-0.213	-0.065	0.085						
	(-1.114)	(-0.361)	(0.346)						
Sust. Target *with Weight*				-0.203	0.090	0.132			
				(-0.807)	(0.435)	(0.824)			
Sust. Modifier							0.157	0.028	-0.028
							(0.613)	(0.135)	(-0.156)
CO_2 Emissions at (t-1)	0.016	0.000	0.011	0.017	-0.001	0.010	0.017	-0.000	0.011
	(1.489)	(0.013)	(1.080)	(1.583)	(-0.078)	(0.991)	(1.604)	(-0.019)	(1.040)
Log(Assets)	-0.056***	-0.049	-0.011	-0.065***	-0.048	-0.008	-0.061***	-0.051	-0.011
	(-3.724)	(-1.603)	(-0.509)	(-3.350)	(-1.389)	(-0.355)	(-3.807)	(-1.669)	(-0.527)
Book Leverage	0.140	0.308	0.454**	0.161	0.331*	0.442***	0.179	0.318*	0.428***
	(0.941)	(1.699)	(2.667)	(1.076)	(1.835)	(2.954)	(1.231)	(1.853)	(3.035)
Log(Cash/Assets)	0.020*	0.019**	0.000	0.019*	0.018**	0.001	0.018*	0.019**	0.002
	(1.845)	(2.432)	(0.046)	(1.887)	(2.265)	(0.060)	(1.728)	(2.564)	(0.217)

(Continued)

Observations	1,431	1,284	1,141	1,431	1,284	1,141	1,431	1,284	1,140
R-squared	-0.014	0.009	0.016	-0.000	0.013	0.020	0.011	0.013	0.017
Cragg-Donald	14.5	12.4	11.9	25.4	21	16.1	36	34.5	32.8
Stock-Yogo 10%	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1	9.1
Hansen J-Stat p-value	0.1	.	.	0.11	.	.	0.17	.	.

Table 14B. Profitability and Sales Growth and Sustainability targets in Annual Incentive Plans – Cross-Sectional IV Analysis

Panel I. IV regression of sales growth on three key explanatory variables: Sust. Target, Sust. Target with Weight, and Sust. Modifier, included separately, and their interactions with lagged CO_2 emissions. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. Instrumental variables include: BAP Indicator, DD State Indicator and Industry-Year Sustainability Practices; and their interactions with lagged CO_2 emissions. We include but not shown are firm and year fixed effects and controls from Table 6A; double cluster independently the standard errors at the firm and year level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: Sales Growth									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*
Sust. Target	-0.356	0.089	0.043						
	(-1.240)	(0.415)	(0.152)						
Sust. Target * CO_2 Emissions	0.023	-0.083**	-0.054*						
	(0.532)	(-2.460)	(-1.921)						
Sust. Target *with Weight*				0.372	-0.124	-0.471			
				(1.152)	(-0.418)	(-1.143)			
Sust. Target *with Weight* * CO_2 Emissions				-0.061	-0.057	0.003			
				(-1.096)	(-1.310)	(0.051)			
Sust. Modifier							0.228	0.061	1.029*
							(0.322)	(0.139)	(1.717)
Sust. Modifier * CO_2 Emissions							0.024	0.007	-0.178*
							(0.222)	(0.092)	(-2.064)
CO_2 Emissions at (t-1)	-0.024	0.034*	0.028	-0.007	0.011	0.010	-0.016	0.002	0.019
	(-1.248)	(1.847)	(1.476)	(-0.624)	(0.760)	(0.649)	(-1.323)	(0.138)	(1.395)

(Continued)

Observations	1,427	1,283	1,139	1,427	1,283	1,139	1,427	1,283	1,138
R-squared	-0.057	-0.033	0.006	0.011	-0.053	-0.028	0.015	0.032	-0.040
Cragg-Donald	7.6	6.2	6.3	14.3	11.7	8.9	13.6	12.6	11.2
Stock-Yogo 10%	9.5	9.5	9.5	9.5	9.5	9.5	9.5	9.5	9.5
Hansen J-Stat p-value	0.5	0.42	0.46	0.16	0.49	0.53	0.22	0.68	.

Panel II. IV regression of ROE on three key explanatory variables: Sust. Target, Sust. Target with Weight, and Sust. Modifier, included separately, and their interactions with lagged CO_2 emissions. We present tabulations at times t+1, t+2, and t+3. The independent variables are measured at t-1. Instrumental variables include: BAP Indicator, DD State Indicator and Industry-Year Sustainability Practices; and their interactions with lagged CO_2 emissions. We include but not shown are firm and year fixed effects and controls from Table 6A; double cluster independently the standard errors at the firm and year level. T-statistics are shown in parentheses. All continuous variables are winsorized at 2.5% in each tail. The *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: ROE									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*
Sust. Target	0.447	0.304	0.410						
	(1.412)	(1.118)	(1.315)						
Sust. Target * CO_2 Emissions	-0.072**	-0.045	-0.027						
	(-2.073)	(-1.527)	(-0.855)						
Sust. Target *with Weight*				0.126	0.455	0.424			
				(0.386)	(1.157)	(1.223)			
Sust. Target *with Weight* * CO_2 Emissions				-0.042	-0.080*	-0.037			
				(-0.957)	(-1.804)	(-0.802)			
Sust. Modifier							-0.012	0.239	-0.338
							(-0.024)	(0.548)	(-0.795)
Sust. Modifier * CO_2 Emissions							0.028	-0.075	0.043
							(0.314)	(-0.952)	(0.536)
CO_2 Emissions at (t-1)	0.044**	0.015	0.021**	0.022*	0.009	0.013**	0.015	0.004	0.007
	(2.509)	(1.021)	(2.334)	(1.905)	(0.780)	(2.295)	(1.221)	(0.339)	(0.820)

(Continued)

Observations	1,431	1,284	1,141	1,431	1,284	1,141	1,431	1,284	1,140
R-squared	-0.027	-0.006	-0.022	0.013	0.007	0.011	0.010	0.011	0.012
Cragg-Donald	7.9	6.4	6.4	13.9	11.3	8.7	13	12.1	10.8
Stock-Yogo 10%	9.5	9.5	9.5	9.5	9.5	9.5	9.5	9.5	9.5
Hansen J-Stat p-value	0.28	.	.	0.14	.	.	.	.	.