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Andrew Taber April 7, 2022

The Effects of Racial Equity Pledge Making on Firms' Stock Valuation

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Abstract

The Effects of Racial Equity Pledge Making on Firms' Stock Valuation

By Andrew Taber

In the wake of the Summer 2020 Black Lives Matter movement, firms made over $67 Billion dollars worth of pledges to racial equity. This paper explores the effect of these pledges as a representation of racially focused positive Corporate Social Responsibility (CSR) activity on the market valuation of the firms that made them using event study and difference-in-differences designs to study the relationship between CSR and perceived firm performance and profitability. The event study reveals a short term negative effect on logged firm stock value, followed by a generally positive trend over the ten weeks following making a pledge. The difference-in-difference results reveal a statistically insignificant -0.84% decrease in stock value for firms in the 10 weeks following a pledge. Heterogeneity tests show that a short-term positive effect may appear for firms that made smaller pledges, suggesting that investors might value these racial equity pledges when they are not too costly. Robustness and randomization checks validate these findings.

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Contents

List of Figures

List of Tables

1 Introduction

Reducing racial inequity in the United States would yield significant economic gains. American firms stand to benefit financially from the economic boost that a United States with less racial disparity would spark. Citi (2020) estimates that Black inequality in the United States has direct, negative impacts on the American economy, costing the American economy $16 trillion over just the last 20 years. Ani Turner's findings follow that of Citi's; Turner (2018) estimates that $8 trillion in GDP could be added to the American economy by 2050 from closing the racial equity gap. McKinsey & Company estimated in 2019 that the racial wealth gap has a cost of $1 to $1.5 trillion annually (Noel et al. 2019). Using a standard value per statistical life estimate of $7.8 million, reducing just inequities in maternal mortality would add $1 billion to the U.S. G.D.P. annually, while reducing Black infant mortality inequity could add an additional $25 billion (Creative Investment Research 2021a). An improved and richer economy is certain to benefit the firms that sell in it. Ignoring moral and social benefits, it's thus profitable, at least in the long run, for companies at large to help reduce racial inequality in a variety of sectors. The Corporate Racial Equity Alliance provides evidence of various firms in many industries gaining business value through racially focused Corporate Social Responsibility (CSR) (Blackwell et al. 2017). Hence, if firms are gaining financial value from advancing racial equity, it should similarly be reflected in their market valuation. Ultimately, economic gains to racial equity ought to motivate firm and shareholder action.

Papers concerning the subject of economic performance and racial equity similarly pre-

dict positive returns for companies upon pushing for equity and inclusivity, and not just due to potential economic growth. Conceptually, Nneka Logan‘s research further motivates this study and contends that corporate person-hood is a force of oppression in the United States (Logan 2019). She argues that firm anti-discriminatory practices and Corporate Social Responsibility actions are only actually socially valuable when conducted with genuine intent, and that multiple forms of corporate activism, including donations to racial equity organizations, are unfortunately tools of virtue signaling, not change (Logan 2019). Virtue signaling is defined as "moral grandstanding", and suggests in this instance that firms may perform CSR for profitable optics (Stollznow 2022). There is evidence for virtue signaling. Corporate Social Responsibility actions are empirically strategically chosen, as firms are found to be more likely to engage in CSR in more competitive industries; further, this effect increases with additional competitiveness (Fernández-Kranz and Santaló 2010). Theoretical gains from virtue signaling may provide another motivator for positive CSR. cxzs

The observed relationship between CSR and firm performance factors at large is a growing field of research. Literature on the topic generally reveals no clear direction of impact. Stock valuation provides an interesting tool to examine projected firm performance. At minimum, stock valuation is reflective of perceived profitability of a firm by investors (Caudle 2008). Hence, I assume changes in stock price reflect changes in the market's perception of a firm based on a variety of factors, including profitability and projected performance. These effects should appear fast, as traders react rapidly to public news Huang et al. (2020). Testing the relationship between CSR and stock valuation allows me to examine how the market values

racial equity focused Corporate Social Responsibility actions and believes they will impact firm performance. There are three theoretical causal theories here.

First, there is a socially pessimistic view. Corporate Social Responsibility can be financially expensive. Companies are giving millions and sometimes billions of dollars away with no clear direct return. Shareholders may plausibly shudder at the cost of this activity, regardless of their social effect. Even non-financial CSR, like for instance pushing for more inclusion in board composition, has no clear financial return and absorbs energy that theoretically could be focused on profit maximization. This would lead to the expectation that the market negatively values CSR and a loss in stock price should be observed.

Second, there is a socially and financially positive view. Not only is racial equity socially valuable, advocating for racial equity may bolster the public relations and marketing of a firm. Satisfying customers is profitable. Further, there are the economic gains to racial equity discussed prior. Either way, this optimistic lens would expect a positive return to firms‘ stock prices when they perform CSR. Investors may consider these actions to be both favorable and profitable, pushing up the firm‘s stock price.

The final theory is the hypothesis to which this paper subscribes. Ultimately, while the social responsibility of firms is ethically important, investors are likely to prioritize financial performance over CSR and Environmental, Social, and Governance (ESG) factors. As the relationship between financial performance and these social variables is debated across academic literature, there is likely not enough clarity for investors to clearly respond to CSR in

one direction versus another. However, the benefits of positive advertising is likely to make up for any upfront costs, and thus no significant effect on stock price is expected to result from Corporate Social Responsibility actions.

The murder of George Floyd in the summer of 2020 sparked a new wave of civil upheaval and nationwide protest in the United States. Americans and people around the world demanded an end to institutional racism, desperately seeking tangible action and change. Critically, this movement saw unprecedented economic involvement, as numerous American firms not only published messages of solidarity, but pledged financial commitments and contributions to the fight for racial equity in the United States. Over $67 billion has been pledged by companies in the wake of the 2020 Black Lives Matter (BLM) movement. This magnanimous figure will likely impact the lives of millions of Americans and could profoundly shape the American corporate relationship with racial equity. That said, very little has actually been studied in relation to these pledges. Are firms trying to make a difference, or are donations simply virtue signaling and strategic marketing? How do investors respond? Studying the financial consequences of making a pledge on the donating firm itself could deepen the understanding of how investors value CSR commitments, focusing specifically on racial equity. This leads to my research questions.

Do corporate racial equity pledges in response to the 2020 Black Lives Matter movement affect firms' market valuation? Further, does this depend on the size of pledge or firm?

My novelty is two-fold.

First, I examine the effects of the 2020 Black Lives Matter Movement. This movement sparked a change in the public perception of race in America. As White (2021) finds, the 2020 BLM Movement additionally changed the relationship between Corporate America and racial equity. Stakeholders, shareholders, and wide-ranging CEOs became vocal about racial issues and firm diversity, contrary to Corporate America's traditional stance of "anodyne centrism" (White 2021). Investors may respond to actions that qualify as CSR differently than before. Already, it appears that this new relationship may have manifested. Shareholders are placing high levels of pressure on firms to interact with racial equity (Clouse 2021). A particular emphasis has been placed on increasing workforce diversity, which saw not only more disclosure demand, but disclosure rates more than triple from 2020 to 2021 (The Conference Board 2021). If BLM changed the public and private dialogue on race and activism, the manner in which investors responded in 2020 and onward to these actions may be different from periods studied in prior literature.

Second, I fill a hole in the literature by focusing on CSR actions that specifically target Black inequality through racial equity pledges. Many papers examine general CSR on firm performance and valuation, but do not narrow their lens to American racial equity. Those that do usually focus on workforce diversity. Mechanically, the only published economic research I found that utilizes these recent corporate pledges is a dissertation that removes the financial tag and looks at immediate stock response to BLM support (Gomes 2021). The author finds a modest positive result. Further, the data set utilized in my research includes monetary prices and is confidential, contemporary, and at the forefront of discussion on

the topic. As minimal analysis surrounding this unprecedented disbursement of capital for racial equity has occurred, this paper presents a new setting for research on the general relationship between market response and CSR and fills a novel hole in the literature by analyzing the investor response to a recent and unique moment in the history of American corporate responsibility.

Across the board, there is no clear academic consensus about whether firm valuation shifts in a positive or negative direction when CSR or ESG news comes out about a company. For instance, Krüger (2015) looks directly at the stock effect of CSR news. Interestingly, he finds a negative investor reaction to both positive and negative news. However, the effect is smaller for positive news, and there is the potential for positive investor reaction when the positive news emerges about a firm that has poor agency relations (Krüger 2015). This research cumulatively suggests that a small negative effect on stock prices might result from CSR. Marsat and Williams (2013) find a negative relationship between CSR and firm valuation as well. Serafeim and Yoon (2022), on the other hand, do find positive investor reactions and believe that stock prices already reflect positive news and high ratings among ESG-type metrics. In that case, a small positive reaction to CSR might be seen, and it would be greater for firms with less of a public history advocating for racial equity. Lu et al. (2021) break down the multifaceted relationship somewhat similarly, concluding that investors react positively along firm-specific factors. Specifically, CSR can improve a firm's market value, but only when it is done strategically; investors like CSR that reduces risk or improves profits. When CSR is performed at inopportune times, they find that investors consider this activity as

"value-destroying" (Lu et al. 2021). My research looks at racial equity focused CSR in a very notable time, so a potentially more pronounced effect may emerge. Glossner (2021) looks at ESG incident recidivism and ultimately concludes that investors continually underreact to negative ESG news. If investors are failing to internalize negative ESG and CSR behavior, an underreaction might be predicted for positive CSR news like announcements of racial equity pledges as well. That said, the costs of poor ESG behavior may lead to gains from positive ESG, and thus motivate a positive reaction instead. Contemporary neutral findings exist as well. Chava et al. (2022) conclude that while positive social activities of firms decrease downside risk, the actual gains to stock returns are economically small. Further, Berk and van Binsbergen (2022) finds that ESG-related divestment by investors ultimately has little effect on the cost of capital of firms. This implies that if investors divest from poor CSR firms, it is unlikely to impact the firm's corporate strategy and decision-making; thus, firm valuation should not be expected to shift significantly following ESG news. Overall, the lack of consensus on the stock effect of CSR activity guides my hypothesis towards finding a null effect. Altogether, shareholders do not clearly react in one direction to CSR and ESG news, potentially because CSR's relationship to financial performance is similarly mixed.

The general relationship between positive CSR and actual measured firm performance itself is debated across various literature too. Board diversity is a particularly salient and popular CSR feature commonly studied. Testing and regressing on several different indicators of performance, some financial, including stock return, as well as non-financial, including CSR scores, Gupta et al. (2015) do not find statistically significant returns from board diversity to

financial performance, other than a negative effect of more racially diverse boards on stock returns. Matsa and Miller (2013) find that a 2006 Norwegian board diversity quota actually decreased short-term firm profitability as the policy affected firm strategy, increased relative employment, and finally, increased labor costs. Further, Bertrand et al. (2019) contend that a 2003 Norwegian board diversity quota, while effective in improving leadership quality among some dimensions and reducing gender pay gaps among corporate boards, did little for women in Norwegian business at large. That said, positive financial returns have been found in the board diversity setting as well. Erhardt et al. (2003) conclusively find that board diversity likely improves firm performance due to its positive correlation with several financial indicators. Further, Rodriguez-Fernandez (2016) predicts positive financial returns from CSR activity. Looking specifically at Spanish companies, Rodriguez-Fernandez examines the dual relationship between the financial performance of firms and CSR activity. As a result, she finds statistically significant positive relationships in both directions (Rodriguez-Fernandez 2016). That is, that more CSR activity and higher ratings lead to positive financial performance, and high financial performance predicts positive CSR activity. Increasing board diversity and pledging racial equity commitments both represent positive firm CSR activity and features; my paper examines the effect of these pledges as a new form of positive CSR focused specifically on racial equity, using firm valuation as a measurement. Altogether, the unclear financial effect of social factors and actions by firms leads to my hypothesis of finding solely a null effect. That said, the racial context within which I study could involve new factors and relationships.

By utilizing racial equity pledges in response to the 2020 Black Lives Matter movement, I isolate a racial equity-based CSR action and look at changes in stock valuation in a contemporary timespan. Due to debating literature on the firm effects of CSR, my primary hypothesis is that the market does not significantly react to CSR and social ESG factors. This is studied following the base assumption that racial equity commitments are effective signals of social responsibility.

Using a quasi-experimental event study and difference-in-differences designs, I examine changes in stock valuation in the weeks following a publicly traded firm‘s pledge. A short-term decrease in stock price is observed after pledging, followed by a long-term positive trend. No overall significant effect is estimated. Further, I test on heterogeneous factors such as free cash flow and pledge size and discover that investors respond more favorably to less expensive pledges. I additionally condition pledge size on free cash flow and find a subtler but still positive effect.

Research on this topic could be particularly significant as revealing whether these corporations‘ values increased, decreased, or remained the same after making a pledge sheds light on the effect of social corporate activism, provides insight on corporate motivation and behavior, and helps understand how the market weighs actions such as these.

2 Data

At the onset of the new Black Lives Matter movement in June 2020, while working at Creative Investment Research (CIR) I was tasked with compiling comprehensive information for the total figure of new capital pledged towards Black racial equity by firms. The Creative Investment Research Corporate Pledge Tracker Database is now the most thorough collection of corporate pledges to racial equity in the wake of the 2020 Black Lives Matter Movement and has been cited by several major American news outlets as well as the United States Congress (Goldber-Zelizer 2021).

The data was hand-collected by searching through smaller lists of pledges, often sorted by industry, in the news as well as looking for public statements by large, well-known firms. Importantly, these announcements had to include a financial tag. This querying was certain to exclude and miss several pledges, but over time the database grew to include well over 250 unique pledges. As the total grew with the inclusion and announcement of new pledges, Creative Investment Research published updates. For instance, on June 10, 2020, Creative Investment Research published in Black Enterprise that the total was at least $1.678 billion dollars (Cunningham 2020). The following day, five foundations including the Ford Foundation announced a $1.7 billion dollar pledge (Ford Foundation 2020).

As the database grew, so did the statistics measured. Originally, pledge size, date, and firm name were the only factors recorded. The summer of 2021 saw Creative Investment

Research and I try to measure the actual amount of this capital that had reliably been released, leading to an estimate of under $3 billion. CEO of Creative Investment Research, William Michael Cunningham, additionally petitioned the SEC for rule making in 2021 on the disclosure of information by firms on their progress related to these pledges (Creative Investment Research 2021b).

Supplementing the existing database, for this research I first went through each listed pledge and marked whether they were publicly traded companies at the beginning of April, 2020 or not in a variety of categories. Some companies are not public, or have a parent company that is. These were marked separately as well and subsequently excluded from this research's sample. Private companies were dropped as their market valuation is unknown. There are many potential confounding factors, like other subsidiary firms, that could impact a parent company's market valuation. Hence, as the effect of pledges would be more likely to directly influence the stock price of specific firms that made them compared to a parent company, firms with only publicly traded parent companies were dropped from the sample. Public companies traded on other global stock markets were additionally excluded as adding additional markets could confound investor behavior; foreign markets may react differently to American racial equity pledges. Next, each publicly traded firm‘s stock ticker was recorded. For firms that made pledges on multiple occasions. I include only their first pledge. This led to a final sample size of 102 firms. Then, for each of these firms in the sample I have marked the closing stock price according to MarketWatch for every day of public trading between April 6th, 2020 and April 1st, 2021. Only two pledges were made outside of this span and

hence were separately labeled and subsequently dropped.

In order to additionally test across heterogeneous factors like pledge size and firm wealth, I collected the 2019 free cash flow for each of the 102 firms in the sample according to Macrotrends.[1] As this is a measure of wealth pre-dating any pledges, it serves as an estimate of capital gained in 2019 available to firms heading into 2020 after accounting for costs. To condition pledge size based on available capital, a relative pledge size measure was transformed by simply dividing pledge size by 2019 free cash flow. Two subsections of the sample by pledge size were created by separating the sample in half across the median pledge size. The separation was made at the median to allow for a simple comparison of results based on whether a firm's pledge size was relatively large or small compared to the pledge size of other pledgers. This process was repeated across the median of 2019 free cash flow and relative pledge size for the entire sample as well. Firms are split by free cash flow to study whether investors react differently based on firm wealth and the original data is additionally subsetted by relative pledge size to condition the investor response to pledge size on firm wealth.

Summary statistics are taken across the entire 102 firm sample in terms of pledge date, size, and 2019 free cash flow and visualized in Table 1. As their means significantly exceed their median measures, skew likely exists in the distributions of pledge date and pledge size.

[1] e.l.f. Cosmetics was the only firm without a readily available 2019 free cash flow. The average of their 2018 and 2020 free cash flow was calculated and listed as their 2019 measure in my sample. This estimation is not problematic to the results of this research because free cash flow is only utilized to divide the sample by median. The median sample value of $2.7 billion dollars is far greater than any recorded free cash flow in the firm's listed history, so it is highly unlikely that e.l.f. Cosmetics' 2019 free cash flow exceeded that value.

The median date is June 5th, 2020 but the average is June 24th, despite the first pledge occurring on March 29th. The final pledge occurs on February 8th, 2021. With an observed timespan ending in April 2021, almost four months of observations after making a pledge still exist for this pledge's corresponding firm. For pledge size, the median value is only $2.75 million, while the average jumps all of the way up to $356 million. This is interesting, as 50% of pledges in the sample were made between $1 and $10 million dollars. The minimum pledge size was $25,000, while the maximum amount, pledged by JPMorgan Chase, was $30 billion. The minimum and maximum 2019 free cash flow are almost $80 billion apart, as the wealthiest firm earned $61 billion, compared to the minimum statistic of almost -$18 billion. The middle 50% of recorded 2019 free cash flows fall between $347,000 and $6.5 billion, however. The right skew of pledge date and amount pledged is important to consider, while the bulk of observations of amount pledged and 2019 free cash flow compare interestingly to their extremes. Importantly, these distributions motivate the use of median measures to subset the data by pledge size and free cash flow, as splitting by mean would likely lead to very uneven sample sizes.

Data is averaged to weekly data before conducting a log transformation. Mitchell (2022) explains that moving averages of stock prices reduce noise and smooth stock price distributions by reducing short-term volatility. Although there are many manners of taking these averages, trends of stock price over time are easier to visually interpret when averaged (Devcic 2021). Weekly stock price data was calculated by averaging daily stock data for each firm by business week. I did not utilize weekly moving averages for each weekday individually

as I wanted to be able to look at each business week separately in the event study. Hence, there is no overlap between each recorded average stock price and the measure is thus not a pure moving average. That said, volatility should still be reduced by averaging across time. To better compare changes in stock price across different firms, a log transformation was performed on stock price.

Understanding how the American stock market was changing over the observed timespan is important as well. Largely, in my timespan of April 2020 to April 2021 the American economy was experiencing recovery in the wake of COVID-19's global recession. On average, Murphy (2021) contextualizes that the stock market plunged to new lows in March 2020 at the onset of the pandemic, but improved dramatically over the course of late March to April 2021. For instance, the S&P 500 improved by 90% over that timespan (Murphy 2021). This was true for NASDAQ firms on average as well, as Macrotrends (2022) easily visualizes a considerable average increase throughout the timespan, with a few short-term drops. This constant recovery is observed across various indices and can be extrapolated to the American stock market as a whole, including firms on the NASDAQ as well as generally those traded on the New York Stock Exchange. Ultimately, it is possible that my research involves a timespan marked by abnormally behaving markets and investors due to a unique context. This would limit the external validity of my findings.

The positive growth of stock price across firms and industries over the time interval studied necessitates a thoughtful test design. An interrupted time-series of pledgers could potentially mistakenly attribute market valuation growth from COVID-19 recovery to pledges and

weaken the internal validity of my research. Further, comparing pledgers to non-pledgers would require a complicated matching design as the act of pledging is almost certain to be non-random. Hence, an event study design was crafted, sacrificing some external generalizability of my findings but allowing me to isolate the impact of pledging using only my sample of pledgers. Grouped by week, Figure 1 shows the general distribution of pledges. Many, but not all, of the pledges are bunched around the beginning wave of the 2020 Black Lives Matter Movement; that is, the pledges are highly concentrated around June 2020. The existence of late pledgers allows me to estimate changes in stock price from making a pledge for early pledgers by comparing them with the weekly stock price changes of later pledgers over the same time interval in an event study. If the event study is designed and performed correctly, the causal effects of making a racial equity pledge on stock market valuation, if they exist, will be isolated from confounding factors.

3 Methodology

I utilize an event study design to examine the effect of making a pledge on a given firm‘s market value. Utilizing early and late pledgers to form treatment and control groups and following these over time, I am able to examine the dynamics of stock price and isolate the impact of pledge making.[2] The identifying assumption relies on early and late pledgers exhibiting parallel trends before making their pledges. The preferred binned weekly regression

[2] “Children and Gender Inequality: Evidence from Denmark” by Kleven et al. (2019) uses a similar idea in building their model by utilizing an event study design to examine differences in earnings between men and women across time before and after having a child.

model is as follows:

$$(1) \quad Log(V)_{Ct} = \beta_0 + \sum^{-5\leq t-J\leq -2} (\beta_{1,t-j} P_{Ct-j}) + \sum^{0\leq t-J\leq 10} (\beta_{2,t-j} P_{Ct-j}) + \delta_t + \lambda_C + \epsilon_{Ct}$$

C is each individual company, t is each week, j is the indexed event time. Event time refers to the week in which each firm made their pledge. $Log(V)_{Ct}$ is the dependent variable, the log of closing stock price. Taking a log allows changes in weekly stock price to be measured relatively in percentage form. $\sum^{0\leq t-J\leq 10}(\beta_{2,t-j} P_{Ct-j})$ creates the estimates for each relative event time, where $\beta_{2,t-j}$ is the coefficient of interest and P_{Ct-j} is a dummy for each relative event time. Each relative event week P_{Ct-j} was calculated by taking the difference in time between the stock price of a given firm in a given week t and the week that that firm made their pledge j. For example, separate estimates are generated for $\beta_{2,0}$, $\beta_{2,1}$, and $\beta_{2,4}$, and $\hat{\beta}_{2,0}$ is the estimate of the effect on a week's average closing stock price during the same week as the pledge was announced compared to the estimated stock price absent pledge making; $\hat{\beta}_{2,1}$ is the estimated stock price the week following a pledge. By including more than I just the difference at $t - j = 0$, I can hopefully uncover delayed effects if they exist.[3] By including some $t - j < 0$ through $\sum^{-5\leq t-J\leq -2}(\beta_{1,t-j} P_{Ct-j})$, I test whether a pre-trend exists; making a pledge should not affect stock prices before making the pledge. The event time referenced

[3]These estimates are generated by effectively comparing early pledgers to late pledgers. Because event time $t - j = 4$ occurs at different times for firms across the timespan, I hope to isolate this effect from time-level fixed effects.

by $t-j=-1$, the week before making a pledge is utilized as the reference point for all other estimates. The coefficients of vector $\beta_{1,t-j}$ should equal zero if the parallel trend assumptions hold, as they are being compared to another observation in the pre-pledge making period.

I bin the week range to $t-j=(-5,10)$ in order to focus on this time range. Particularly, further estimates for weeks before $t-j=-5$ and after $t-j=10$ are likely less independently indicative of market valuation or accurately measured and now can be generalized into longer theoretical periods of before $t-j=-5$ and after $t-j=10$; as event times increase further from 0 in either direction, fewer comparisons exist and precision declines. Figure 2 demonstrates the distribution of observations across each weekly event time. The binned period is in red, and aims to contain a highly dense sample with observations. The comparative value for each estimate remains. $t-j=10$, for instance, after binning compares the logged stock price ten weeks after making a pledge to each of the fifteen weeks (excluding $t-j=-1$) before it independently, the general period before $t-j=-6$, and the period after $t-j=10$. Mechanically, all observations before event week $t-j=-5$ and after $t-j=10$ were binned into $t-j=-6$ and $t-j=11$, respectively. The estimates for $\beta_{1,-6}$ and $\beta_{2,11}$ are then excluded from the model's visualizations.

I include two sets of fixed effects in my models. δ_t is a fixed effect dummy variable for each week. Using a timespan of April 6th, 2020 to April 1st, 2021, this ultimately includes 50+ variables which isolate the relative stock price changes on each individual week independent of pledges. λ_C is a fixed effect dummy variable for each firm that captures time invariant firm characteristics. General confounding attributes to firms and trends in the stock market

should be attributed to these fixed effect dummy variables instead of event times relative to pledge making. ϵ_{Ct} is the error term. Standard errors are clustered by firm due to repeated observations throughout all of the event studies performed.

The event study was performed using the *feols* (Fixed Effect OLS) function from the package *fixest* in *R*. The only additional requirement is that I choose an observation to exclude due to only utilizing dummy variables. Since I want everything relative to $t-j=-1$, this is the observation excluded.

In order to estimate average treatment effects, I additionally run difference-in-difference (DiD) analyses. Unlike the original event study where each date after the pledge is treated independently, this approach combines them. Two periods are created; the time before each firm made their pledge, and the time after. In other words, the treatment group includes firm observations after making their pledges, while the control group involves the firms that have yet to make a pledge. Hence, the test estimates the differences in stock price for firms before and after making a pledge between firms that have pledged and have yet to pledge. I estimate the following equation:

$$Log(V)_{Ct} = \beta_0 + \alpha P_{C1\{t-j\geq 0\}} + \delta_t + \lambda_C + \epsilon_{Ct} \tag{2}$$

I include the same set of fixed effects as in Equation 1 while $P_{C1\{t-j\geq 0\}}$ is now a dummy

variable indicating post-pledge periods of firm C. An identity function is used for $t - j$ to separate the periods. α is the parameter of interest and estimates the average treatment effect on logged stock price of having made a pledge. The same *fixest* package and *feols* function are used again.

Heterogeneity tests are performed separately using the below median and above median sub-samples of free cash flow and pledge sizes of these publicly traded firms. The same binned weekly event study and DiD test is performed. The methods and variables are completely identical, however they are conducted on even splits of the data to reveal whether the effect on stock price is dependent on characteristics of firms in the sample.

4 Results

The graphical results from the binned weekly event study are displayed in Figure 4. No clear pre-trend is observed in the weeks leading up to pledge making. If anything, the pre-trend is going in the opposite direction of the immediate post-effect. This is essential to gaining the ability to causally interpret estimates in the post-period that makes up the treatment group. The first few weeks after making a pledge see a negative effect on stock price. This suggests that at least in the short-term, in each of these first two weeks after a pledge is made investors react negatively to pledge news, pushing stock price down. After the second week beyond making a pledge, this pattern reverses, and an increasingly positive effect is estimated. Particularly, from event week three and onward, positive estimates for logged

stock price are estimated. This suggests that over a longer period of time, market valuation is higher than it would be absent pledge making. Altogether, however, when standard errors are taken into account, none of these estimates appear to be significant at the 95% confidence level. Standard error appears to increase over time as well. The observed reversing pattern of negative and positive estimates confirms the hypothesis of no clear statistically significant finding in either direction. Pledge making does not appear to clearly push market valuation in one direction over another in the ten weeks following a pledge.

Table 2 displays the results from the preferred difference-in-difference tests. The left column reflects the estimate using the preferred weekly averaged data, while the right uses daily data. As some pledges were made in the middle of weeks, daily data allows the prior days to be included in the control group. A statistically insignificant average treatment effect is estimated. stock prices are -0.84% lower following a pledge compared to the weeks prior. A 95% confidence interval provides evidence that the effect of making a pledge has between a -5.08% drop and 3.40% increase in stock price. This largely confirms the finding of no clear effect as the effect may go in either direction. That said, it may have a decently large effect on stock price; at it's greatest impact, stock price may decrease for a firm by 5% following a racial equity pledge. I am essentially only able to rule out large changes of over 5% of stock price in both directions. The daily version estimates a slightly smaller and still insignificant effect. When looking at the entire time range, insignificant estimates are found using both daily and weekly data.

Although the binned weekly event study shows some positive estimates in the latter period,

when looking at the entire timespan a slightly positive average estimate is observed in the pre-pledge period. Hence, although the post-period is visually centered more around zero than a negative estimate, a slightly positive effect was likely estimated for the control group and thus reflected in the comparative average treatment effect estimate for the treatment group.[4]

Figure 5 displays four series of event time coefficients plotted next to each other. Looking at pledge size, above median pledgers (labeled with green rectangles) clearly exhibit a worse investor reaction, with lower stock prices than those that pledged a below median amount of money (black circles). Simultaneously, those with lower than median pledge size receive positive returns, at an eventually statistically significant level. This suggests that investors positively value racial equity pledges when they are not overly large. When the event study is performed for above median (blue hollow circles) and below median firms (red triangles) in terms of 2019 free cash flow, a similar effect is seen. This is displayed together with pledge size in Figure 5. Investors react more positively to pledges for firms that had lower 2019 free cash flow.

[4]Stock return, as opposed to closing stock price, is another measure of market valuation. Abnormal stock return is a potentially preferred dependent variable. For simplicity, I performed the same tests on a basic measure of stock return. I calculated daily stock return by dividing a day's closing stock price by the previous day's. I then excluded the first date, April 6th, 2020. In daily form, I then took the log. In weekly form, I averaged the stock return for every week and then logged my averages. I also performed the weekly transformation separately again by logging the return between averaged weekly stock prices. I conducted unbinned and binned event studies using weekly and daily data using this measure instead of logged stock price as my dependent variable.

While the trends are somewhat different using stock return, a positive pre-trend is observed in both the daily and especially weekly event study designs. In the post-period, the first 50 days generally seem to have positive, but slightly insignificant estimates. The overall trend is null. With either weekly return calculation, the period after making a pledge also appears to reveal a short-term positive and statistically significant effect on stock return from pledge making, but concerningly the positive trend in the pre-period is of a greater magnitude. Hence, these tests were excluded as the pre-trend is too great due to an issue arising in my coding and testing.

The relative pledge size event study was performed to test for confounding and see whether pledge size robustly affects stock price even when accounting for free cash flow. This is presented in Figure 6. Companies that pledged less money relative to their free cash flow (labeled with black circles) also saw the positive investor response in terms of stock price, while those above median (red triangles) were found with lower, albeit surprisingly slightly positive, insignificant estimates in the weeks following pledge making. That said, standard errors for below median relative pledgers appear notably large, and something of a pre-trend is suggested in the weeks before a pledge was made.[5]

Altogether, these event study findings suggest that pledge size is a determining factor in investor response. In fact, investors appear to prefer relatively lower donations as logged stock price is estimated to increase over time for firms that pledged less than a median amount of capital compared to the other firms in the sample. These positive effects seem to withstand free cash flow conditioning, although estimation accuracy appears to decline.

In order to estimate average treatment effects again looking at the entire timespan, DiD tests were performed across all six data subsections. Table 3 presents the results. Logically following the findings of the heterogeneous event studies, a 4.51% increase in stock is estimated after making a pledge for firms that made pledges of below-median magnitude. However, this effect is still statistically insignificant at the 95% level, with an effect size interval of -1.91% to 10.93%. Anything more than a modestly negative effect on stock price for

[5]The standard deviation of logged stock price within each subsection are very similar and do not exhibit any clear pattern explaining this result. Differences slightly increased when broken down by event time, but remained quite similar.

cheap pledges is ruled out, but very large positive increases may exist. A slightly insignificant negative effect of -2.96% is estimated for their above-median counterpart, with an interval of -7.62% to 1.70%. While overlap in their intervals exists and neither effect is statistically significant, a point estimate difference of 7.47 percentage points is estimated between the point estimates for below- and above-median pledgers. This suggests that pledge size may directly impact changes in market valuation. Positive and negative estimates are calculated for below- and above-median firms in terms of free cash flow, respectively, but neither are significant. The magnitude of the estimates is lower, which makes sense following the patterns observed in the event studies. For relative pledgers, clearly insignificant estimates very close to 0 are calculated. This suggests that when pledge size is conditioned by free cash flow, the effects are lost. That said, the standard errors for the relative pledge samples are not small, with 95% confidence intervals of -8.39% to 8.53% and -5.82% to 4.18% for below and above median relative pledgers, respectively. Potentially large effects are not fully ruled out, particularly for below median relative pledgers. Altogether, these results suggest that pledge size, a heterogeneous factor in the data set, may have an undetermined independent effect on market valuation for pledge making firms.

5 Robustness

Although my preferred event study design involves weekly binning, I perform many other variations on the event study design in order to test the robustness of my findings and look

for other effects.

An initial variation involved leaving the data in daily form for both reported stock price and relative event date. This was conducted in order to check whether putting not stock prices in weekly averaged forms would result in a different pattern. Visually trimmed to the one hundred days prior to and two hundred days following a pledge in order to stick within ranges with several observations, Figure 7 displays the daily event study across the entire time frame. As seen in the Binned Weekly event study, no clear pre-trend is observed, and a very short negative trend followed by the positive trend with large standard error is again observed.

Then, I bin the daily event study design to the twenty days preceding pledges and forty days following. This was performed to effectively mimic the binned weekly design but for daily data in a more confined time range. This is visualized in Figure 8. Once again, similar effects are seen as in the binned weekly design, but with more volatility likely introduced by daily stock price noise.

Figure 9 displays a weekly event study without binning in order to compare estimates absent end-point grouping. Interestingly, a slight negative trend is observed at the very end of the time frame, but I conclude this is largely due to a lack of observations forty weeks after making a pledge. That is, there are less observations in this range. A pre-trend appears to perhaps emerge, but with large standard errors this figure largely confirms the findings of the preferred method but with less clear intuitive findings.

As mentioned in the Data portion of this paper, most of the pledges are made in early June. As grouping by week inherently groups even more of these pledges together, I perform a binned daily event study however only including pledges made in the first month of pledging. This is seen in Figure 10. Even when the data is narrowed to this range, we see similar trends as exhibited elsewhere. Crucially, this is a weaker design as excluding later pledges reduces the comparative value of pledge dates against event dates. For instance, if I only included pledges made on a given date and then looked at changes in stock price, I would lose the ability to estimate time-based fixed effects and create a comparative control group.

A randomization test is performed in order to enhance the findings of this research by judging whether the random treatment of making a pledge causally affects stock price over time. This test involved randomly assigning pledge dates among the firms in the sample and then performing a difference-in-difference test with the same method preferred before. Despite a clustering of firms in June, with 1500 repetitions sufficient randomization is accounted for. Clearly, if the DiD test design is accurate, randomly assigning pledge values should result in a normal distribution centered around 0. Effectively random and thus inaccurate pledge dates should not produce abnormal stock prices in the post-period if the difference-in-differences test design is valid.

Figure 3 displays the result of this repeated test. A normal distribution is observed, and the center of the distribution is close to the main estimate from our preferred DiD technique of -0.84%. This estimate and its confidence intervals are graphed on the distribution in red and blue, respectively, to show this general difference and the overlap of the estimate's

confidence interval with a null finding of 0%.

Altogether, this supports the claim that the event study and difference-in-difference designs are robust to randomization as no effect was found when randomizing pledge date.

6 Conclusion

Overall, Corporate Social Responsibility at large does not appear to impact firm valuation. The event study design demonstrates that as time goes on, making a pledge less accurately predicts changes in market valuation. Further, the effects move from a short-term negative drop to a prolonged positive increase in logged stock price. Ultimately, the difference-in-difference test effectively confirms the finding of no significance. Various robustness tests validate these findings, as additional event studies display the generally unclear effect of pledge making and CSR as a whole.

Importantly, it appears that investors may respond based on firm and pledge specific factors. In particular, the size of the pledge made visually appears to affect logged stock price in the ten weeks following a pledge. That said, the DiD estimate is insignificant at the 95% level and the exact mechanism remains unknown, but it is possible that this effect may be suggested because of pledges' positive marketing effects. Classified under virtue signaling, some pledges could have been made in order to make the market perceive a given firm as socially good, as this results in positive advertising and satisfying customers. The pledge size

trend relatively robust to firm free cash flow in the event study design, but the effect becomes very small and statistically insignificant after collapsing to the difference-in-difference test.

These results are not surprising given the general academic debate on the topic. Estimates of the cost of racism point towards huge economic gains for American firms from the fight against racial inequality. Yet, actual papers examining CSR and firm performance or market valuation find conflicting results. Potentially, these long-term benefits have not been internalized by firms or the market at large. Regardless, as literature develops the concept of virtue signaling, the positive reaction to cheaper pledges is not altogether surprising.

Future research on this subject should compare pledgers to non-pledgers in order to get a better estimate of how the market would predict stock price change for these firms absent making a pledge. Pledging is likely not random, and thus external validity and causal evidence is lost in my design in order to gain internal validity through the event study design. Future studies should utilize abnormal stock return instead of closing stock price as a dependent variable to study stock market trends more standardly. Additionally, future research should further explore virtue signaling and the connection between the size of CSR expense and market response. The relationship between Corporate America and racial equity should continue to be questioned, studied, and explored.

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8 Appendix

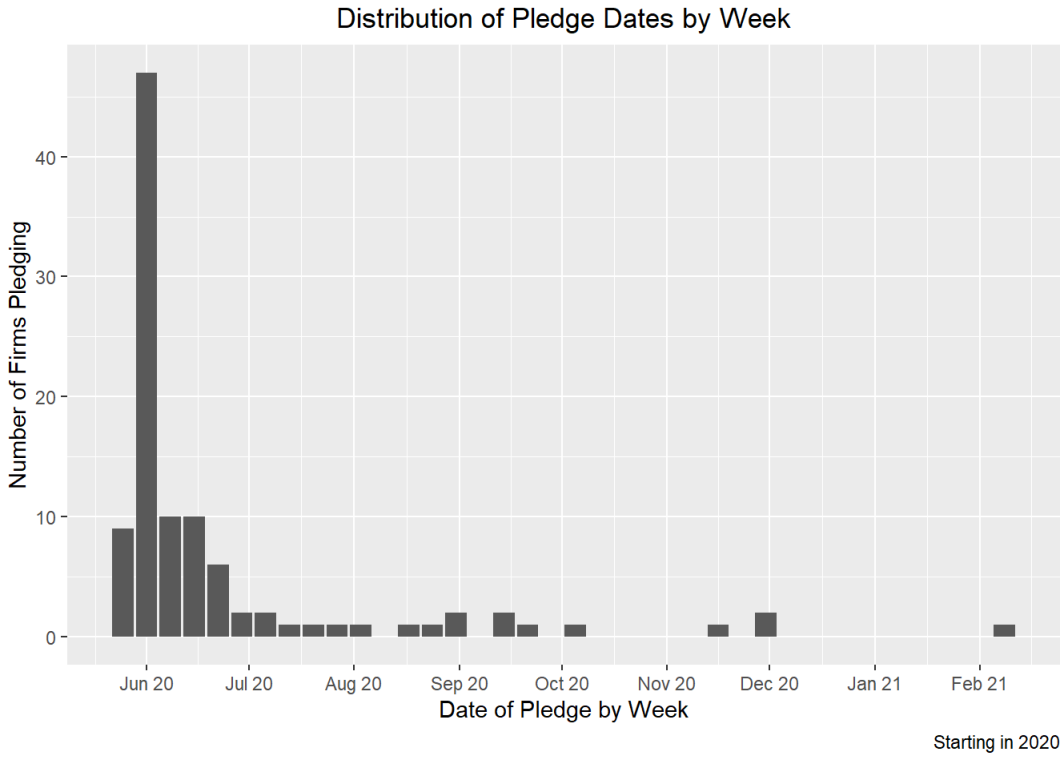

Figure 1: Pledge Distribution by Week

Notes: Bar graph distribution of Pledge Dates grouped by week, with the count of firms on the y axis. There are pledges made throughout the timespan, but it is clearly seen that the graph is skewed as the bulk of pledges came in early June.

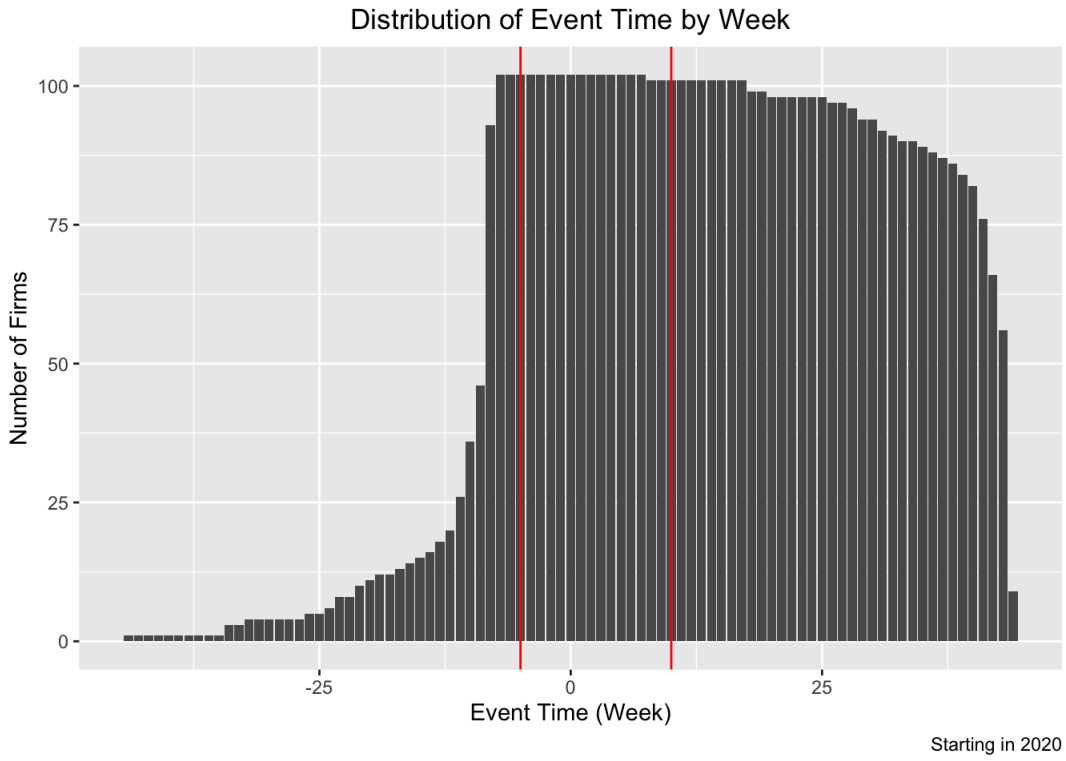

Figure 2: Event Time Distribution by Week

Notes: Bar graph distribution of Event Times grouped by week, with the count of firms on the y axis. The binned week selection of -5 to 10 is highlighted with red vertical lines at each level. As is immediately evident in conjunction with the pledge date distribution, due to the early cluster of pledging firms, the amount of observations within each event time is highly skewed. Many observations are made for later event times, while the pre-trend event time period is less populated due to few late pledgers.

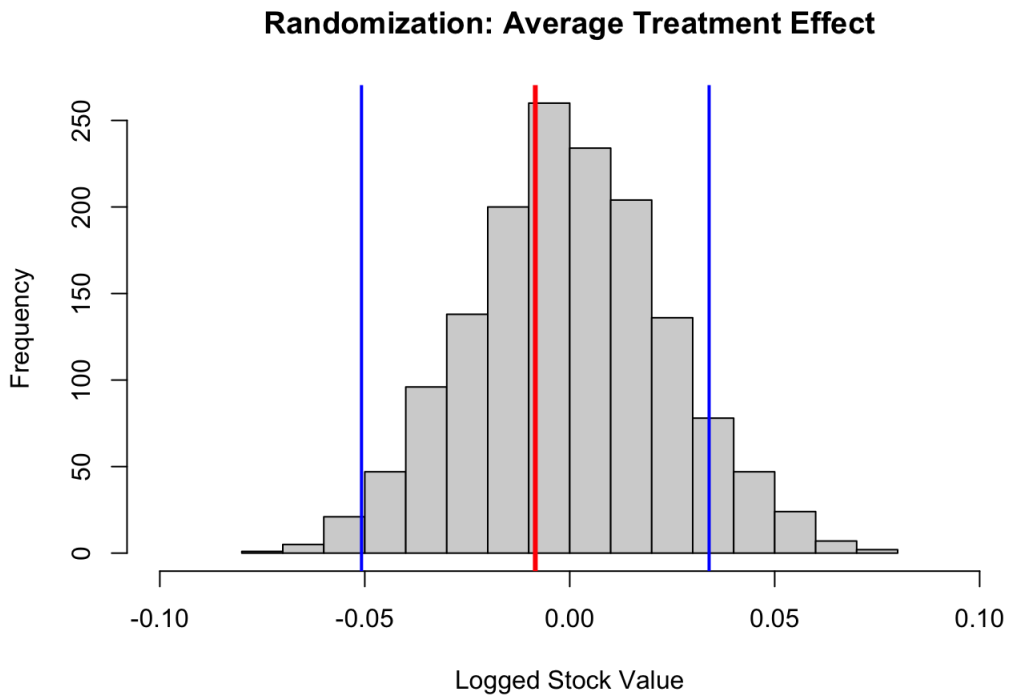

Figure 3: Randomization ATE Histogram

Notes: Randomization test with 1500 Repetitions. The weekly difference-in-difference test method was iterated across 1500 variations of random pledge date sampling. By repeatedly randomizing the pledge dates and performing tests, the coefficient estimate distribution is graphed. The red line reflects the estimate of 0.0084 calculated from the main DiD test. The blue lines signify this estimate's 95% confidence interval.

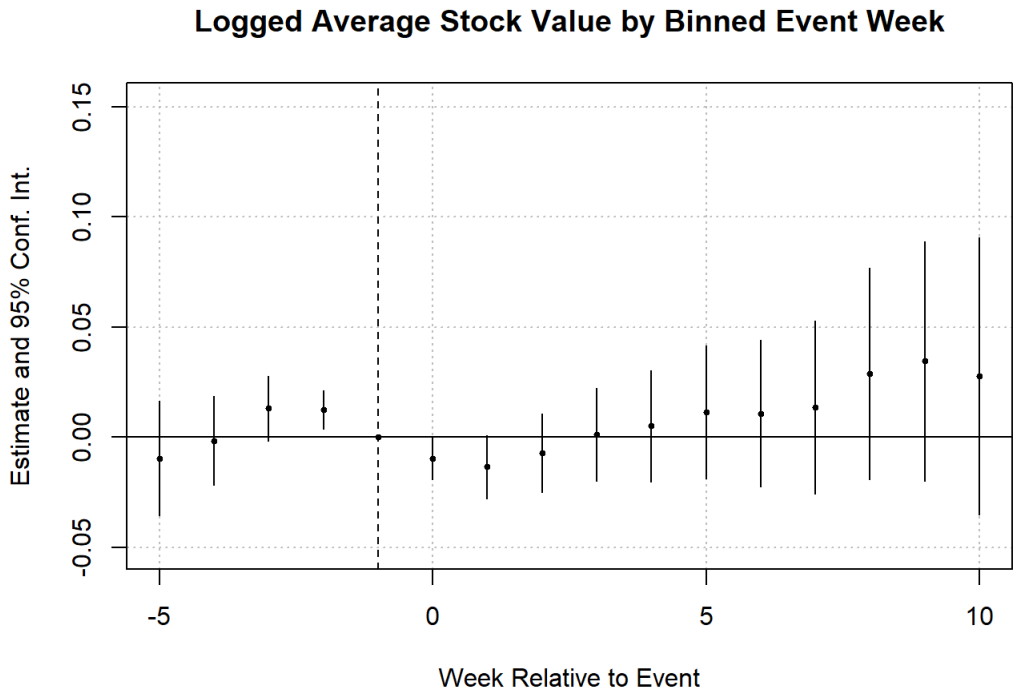

Figure 4: Binned Weekly Event Study

Notes: Event study regressing logged average stock price on event time relative to pledge making, with time and firm-level fixed effects. The event times and time fixed effects are grouped and averaged to weeks, and the range of event times studied is binned to the five weeks prior to making a pledge and the ten weeks after. The dots reflect point estimates, while the standard errors are the surrounding lines. The reference point is at event time -1, the week before making a pledge. Standard errors are clustered by firm.

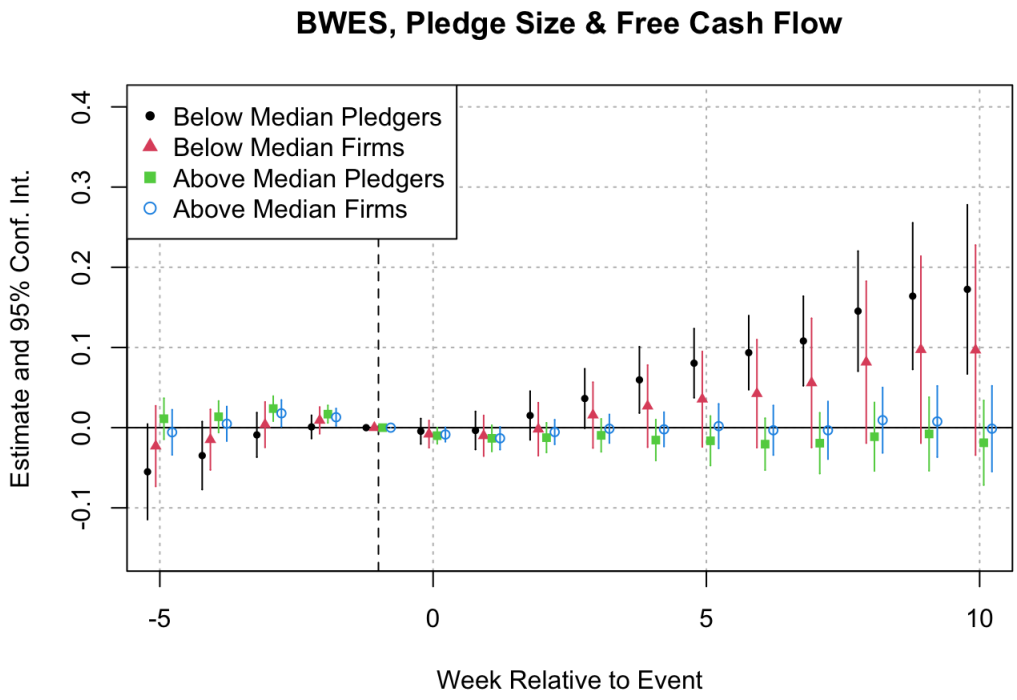

Figure 5: Binned Weekly Event Study, by Pledge Size and Firm Free Cash Flow

Notes: Four event studies regressing logged average stock price on Event Time relative to pledge making, with time and firm-level fixed effects. The event times and time fixed effects are grouped and averaged to weeks, and the range of event times studied is binned to the five weeks prior to making a pledge and the ten weeks after. The dots reflect point estimates, while the standard errors are the surrounding lines. The reference point is at event time -1, the week before making a pledge. First, the data was subsetted into below median pledgers and above median pledgers in terms of quantity pledged. The preferred binned weekly event study design was performed and the series of estimates graphed in red and blue for below median pledgers and above median pledgers, respectively. Next, the original sample was subsetted into below median and above median firms in reference to 2019 free cash flow, and again the binned weekly event study test was conducted. Referred to as "Below Median Firms" in the graph, firms with below median 2019 free cash flow are graphed in black while firms with above median free cash flow are graphed in green. They are all presented in this figure. Standard errors are clustered by firm.

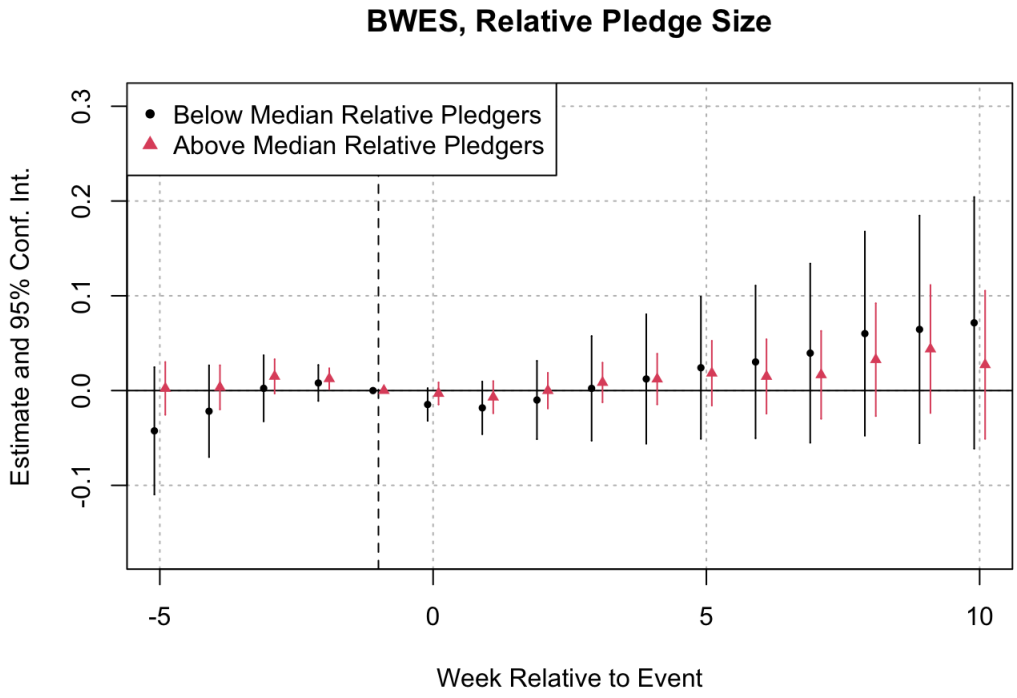

Figure 6: Binned Weekly Event Study, by Relative Pledge Size

Notes: Two event studies regressing logged average stock price on event time relative to pledge making, with time and firm-level fixed effects. The event times and time fixed effects are grouped and averaged to weeks, and the range of event times studied is binned to the five weeks prior to making a pledge and the ten weeks after. The dots reflect point estimates, while the standard errors are the surrounding lines. The reference point is at event time -1, the week before making a pledge. The data was subsetted into below median relative pledgers and above median pledgers. Relative pledge size was calculated as quantity pledged divided by logged 2019 free cash flow. The binned weekly event study design was performed for both groups and the series of estimates graphed in black and red for below median relative pledgers and above median relative pledgers, respectively. Standard errors are clustered by firm.

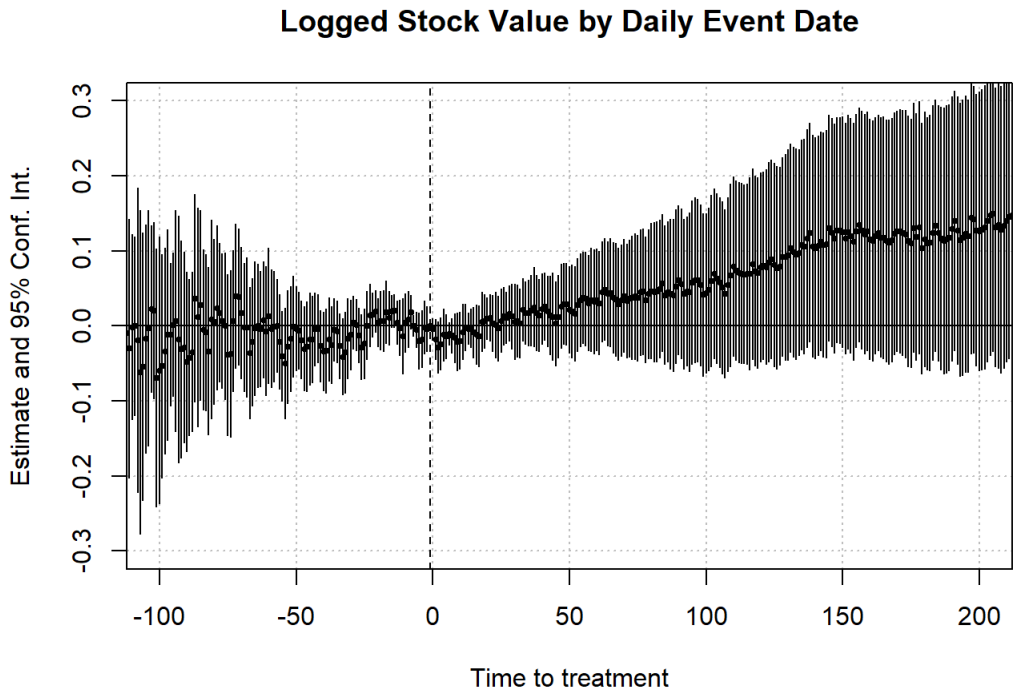

Figure 7: Trimmed Daily Event Study

Notes: Event study regressing logged average stock price on event time relative to pledge making, with time and firm-level fixed effects. The event times and time fixed effects are in daily form, and the range of event times studied includes the entire time range. The displayed estimates are trimmed to the 100 days preceding and 200 days following making a pledge. The dots reflect point estimates, while the standard errors are the surrounding lines. The reference point is at event time -1, the day before making a pledge. Standard errors are clustered by firm.

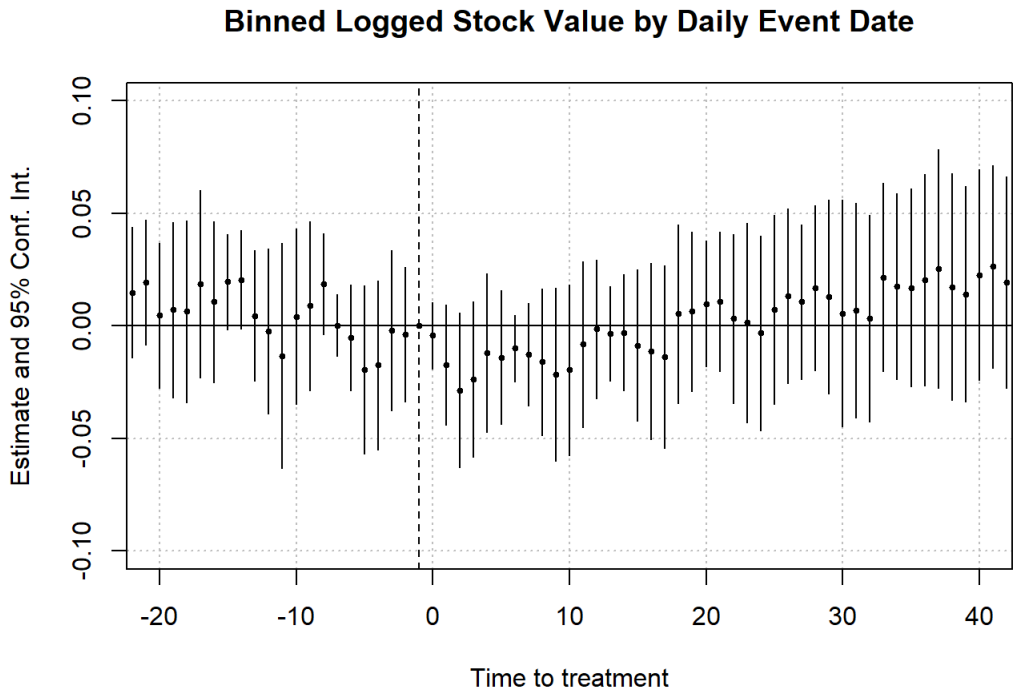

Figure 8: Binned Daily Event Study

Notes: Event study regressing logged average stock price on event time relative to pledge making, with time and firm-level fixed effects. The event times and time fixed effects are in daily form, and the range of event times studied is binned to the 20 days preceding and 40 days following a pledge. The dots reflect point estimates, while the standard errors are the surrounding lines. The reference point is at event time -1, the day before making a pledge. Standard errors are clustered by firm.

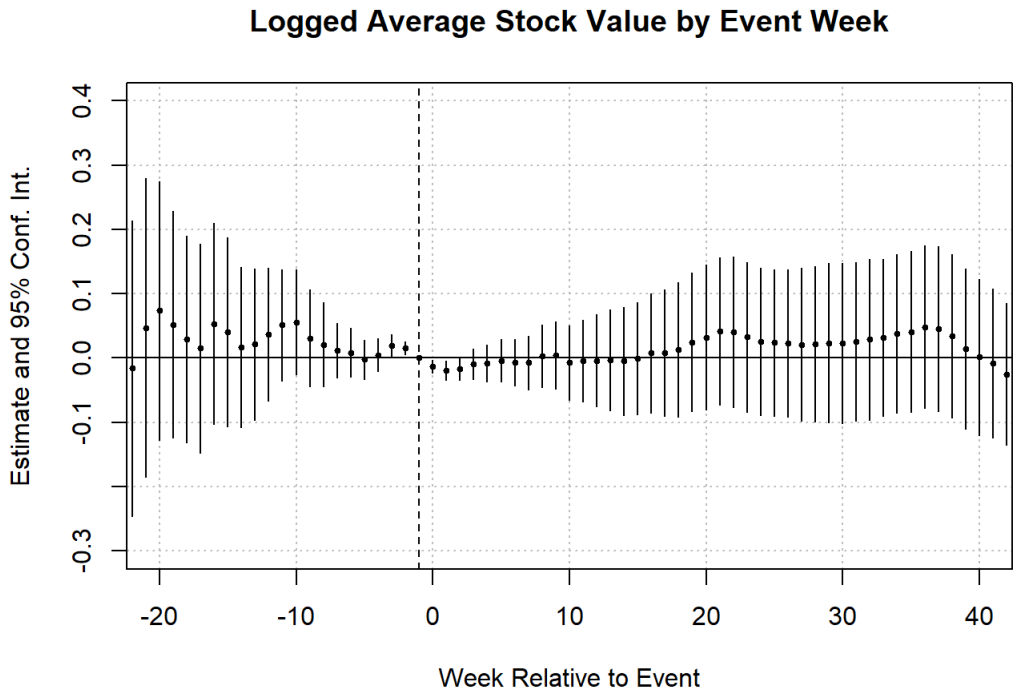

Figure 9: Trimmed Weekly Event Study

Notes: Event study regressing logged average stock price on event time relative to pledge making, with time and firm-level fixed effects. The event times and time fixed effects are in weekly averaged form, and the range of event times studied includes the entire time range. The displayed estimates are trimmed to the 20 weeks preceding and 40 weeks following making a pledge. The dots reflect point estimates, while the standard errors are the surrounding lines. The reference point is at event time -1, the week before making a pledge. Standard errors are clustered by firm.

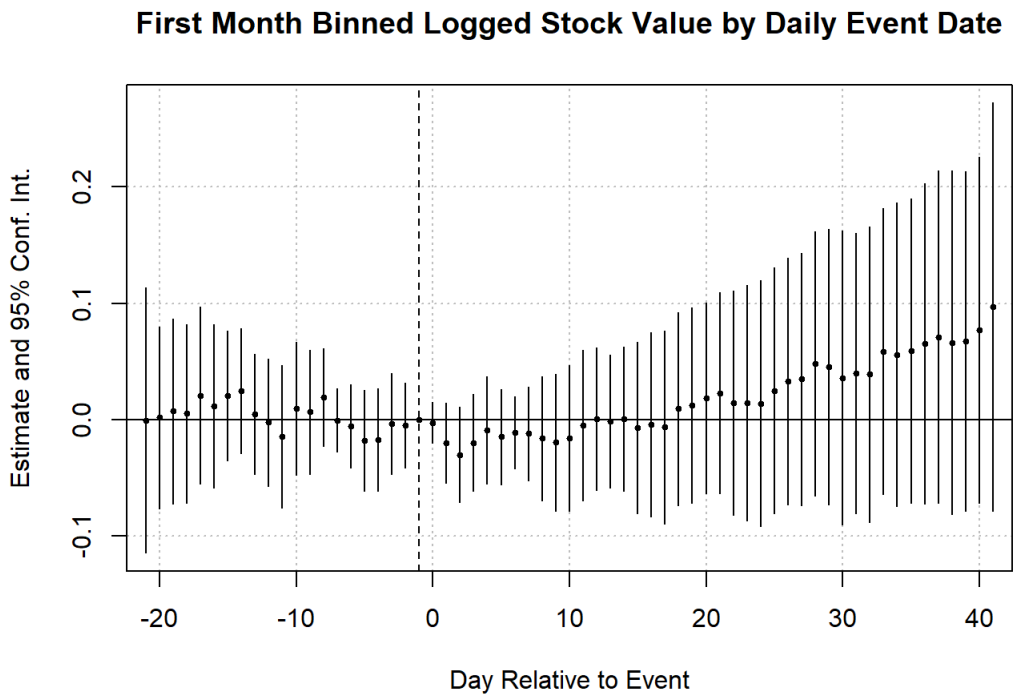

Figure 10: June Pledges, Binned Daily Event Study

Notes: Event study regressing logged average stock price on event time relative to pledge making, with time and firm-level fixed effects. The event times and time fixed effects are in daily form, and the range of event times studied is binned to the 20 days preceding and 40 days following a pledge. Only pledges made in the first month were included in the data set. The dots reflect point estimates, while the standard errors are the surrounding lines. The reference point is at event time -1, the day before making a pledge. Standard errors are clustered by firm.

	Pledge Date	Amount Pledged	2019 Free Cash Flow
Minimum	05/29/20	$25,000	-$17,910,000,000
1st Quartile:	06/03/20	$1,000,000	$346,900,000
Median:	06/05/20	$2,750,000	$2,734,000,000
Mean	06/24/20	$356,000,000	$6,462,000,000
3rd Quartile	06/23/20	$10,000,000	$7,432,000,000
Maximum	02/08/21	$30,000,000,000	$61,780,000,000

Table 1: Sample Summary Statistics

Notes: After the sample was limited to the first pledge by firms publicly traded on the New York Stock Exchange or NASDAQ, summary statistics are calculated for three variables: date of pledge, amount pledged, and 2019 free cash flow. The statistics include minimum, 1st quartile, median, mean, 3rd quartile, and maximum measures. These statistics help gather information about the distribution of pledges in regard to date, pledge size, and 2019 firm free cash flow in the final sample.

Dependent Variable:	Logged stock price	
Model:	Weekly	Daily
Variables		
stock price After Pledge	-0.0084	-0.0058
	(0.0212)	(0.0203)
Fixed-effects		
Firm	Yes	Yes
Date	Yes	Yes
Fit statistics		
Observations	5,304	25,500

Clustered (firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 2: Difference-in-Differences Results

Notes: Table displaying difference-in-difference results across two different methods. Column 1 involves weekly averaged data, while column 2 includes day-level stock data. Tests included the entire timespan. The stock price After Pledge variable presents the difference-in-difference estimate for change in logged stock price. None of the results are statistically significant. Standard errors are clustered by firm.

Dependent Variable:	Logged stock price					
Model:	*Pledge Size*		*2019 Free Cash Flow*		*Relative Pledge Size*	
Above or Below Median	Below	Above	Below	Above	Below	Above
Variables						
stock price After Pledge = 1	0.0451	-0.0296	0.0133	-0.0121	0.0007	-0.0082
	(0.0321)	(0.0233)	(0.0358)	(0.0207)	(0.0423)	(0.0250)
Fixed-effects						
Firm	Yes	Yes	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	2,652	2,652	2,652	2,652	2,652	2,652

Clustered (firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 3: Heterogeneity Difference-in-Differences Results

Notes: Table displaying difference-in-difference results across subsets of the data set by three variables. All include weekly averaged data. Columns 1, 3, and 5 look at below median data subsets, while columns 2, 4, and 6 look at the above median data across each variable. Columns 1 and 2 subset the data by pledge size. Columns 3 and 4 subset the data by 2019 free cash flow. Columns 5 and 6 subset the data by relative pledge size, calculated by dividing pledge size by 2019 free cash flow. Tests included the entire timespan. The stock price After Pledge variable presents the difference-in-difference estimate for change in logged stock price. None of the results are statistically significant. Standard errors are clustered by firm.