# The Spillover Effects of Insurer Climate Supervision into Shadow Markets*

Weiling Liu Saptarshi Mukherjee Kandarp Srinivasan

This Version : Feb 2026

**Abstract**

We find that climate supervision causes unintended consequences in institutions' shadow asset allocations. Following staggered climate supervision, affected insurers lose 0.6% in average yields by shifting towards greener outright holdings. Offsetting this loss, they purchase higher-yielding, securitized holdings—explaining $58.2 billion or 13.7% of insurers' total increase in shadow holdings between 2010 and 2019. This spillover effect is strongest among insurers with tight statutory capital limits, consistent with a supervisory mechanism. Breaking down collateralized loan obligations (CLOs) into individual loans, we show that affected insurers lend to browner companies, which additionally pollute 12.87 pounds of emissions per dollar of total assets.

***Keywords***— insurance, climate risk, CLOs, fixed income, regulation, securitization.

*Weiling Liu is from Northeastern University, Saptarshi Mukherjee is from Northeastern University, and Kandarp Srinivasan is from Bentley University. The authors can be reached at we.liu@northeastern.edu, sa.mukherjee@northeastern.edu, and ksrinivasan@bentley.edu. We would like to thank Rajesh Aggarwal, John Campbell, Lauren Cohen, Kyeonghee Kim, Iwan Meier (discussant), Philip Mulder (discussant), Simon Oh, Joan Schmit (discussant), Jeremy Stein, Ana-Maria Tenekedjieva, and the participants in the Northeastern Brown Bag, the 2024 International Symposium on Climate, Finance, and Sustainability, Florida State University's 2024 Risk Management & Insurance Research Symposium, the Midwest Finance Association Annual Meeting, Bentley University Seminar, Isenberg School of Management Seminar, and the Canadian Sustainable Finance Network Conference for helpful feedback. Gregory Guo provided excellent research assistance. All errors are our own.

# 1 Introduction

Institutional investors (pension funds, endowments, insurers etc.) are increasingly delegating their investment decisions to private equity (Sensoy et al., 2014; Cavagnaro et al., 2019) and managers of asset-backed securities (ABS) (Kirti and Sarin, 2024) including collateralized loan obligations (CLOs) (Foley-Fisher et al., 2023). As Figure 1 shows, insurance companies increased their holdings of ABS nearly tenfold from $115 billion in 2001 to over $1.1 trillion in 2020. This shift towards externally managed portfolios has amplified climate risk concerns – non-profit organizations claim that the fossil fuel investments of just 21 private equity firms account for 1.17 gigatons of annual emissions – an estimate that exceeds the combined carbon footprint of the global aviation industry.[1] The opaqueness and lack of transparency of delegated portfolios makes it very challenging to assess their environmental risks. In this paper, we use unique data on the securitized holdings of insurance companies as a laboratory to understand how climate risk can manifest in externally managed portfolios. We exploit a natural experiment where the constraints imposed on direct portfolios motivate insurance companies to seek more securitized investments, specifically CLOs. By hand-matching data on the thousands of underlying firms held by CLOs to the underlying holdings data, we are among the first studies to analyze climate risk in insurers' shadow market investments.

In 2010, the National Association of Insurance Commissioners (NAIC) adopted a climate supervisory initiative (the annual Climate Risk Disclosure Survey) to increase insurance company accountability towards climate risk. The survey was rolled out in a staggered fashion across states (see Figure 2), and it required insurance companies to publicly disclose the impact of climate change in their investment portfolios. We hypothesize that this natural experiment incentivized insurers' outright portfolios to become greener, but concurrently, it led insurers to seek alternative investments to recoup lost yields. Prior work has established that green bonds are associated with lower yields (Zerbib 2016; Pástor et al. 2022), and that CLO debt tranches earn higher returns than similarly rated corporate bonds (Cordell et al., 2023). Thus, if insurers view climate supervision as a constraint on the choice of their direct holdings, they may reach-for-yield through securitized investments and push substantial capital flow into shadow markets.

We begin our analysis by establishing portfolio effects of climate supervision: insurers tilt their direct holdings towards greener firms, lose significant corporate bond yields, and invest more cap-

[1]This study was conducted in 2024 by the Private Equity Climate Risks consortium https://www.peclimaterisks.org/wp-content/uploads/2024/10/PECR_Report_Climate-Risks-Scorecard_2024.pdf. See also https://www.ai-cio.com/news/institutional-investors-expect-to-raise-risk-tolerance-for-private-markets/.

ital in asset-backed securities. Following the first year of disclosure: Figure 3 shows that climate sentiment of direct holdings, which are primarily corporate bonds, significantly increases as insurers shift toward greener investments.[2] Consistent with a green premium, we also find that yields in their direct portfolios drop by 0.6%. Next, Figure 5 shows that insurers increase ABS holdings by up to $5 million per year, and that this is likely driven by the fact that these ABS holdings yield up to 2.5% more. Aggregating over all years and insurers that were required to disclose between 2010 and 2019, we estimate that this adds up to roughly $58.2 billion of shadow investments that may be attributed to increased climate supervision. The impact is significant, representing about 13.7% of the total $400 billion increase in ABS holdings by insurers over the same period.

Our preliminary empirical analyses suggest that NAIC Climate Disclosures, despite being surveys, significantly increased climate oversight for affected insurers. To test whether these surveys carry real weight, we analyze the text of the individual survey responses. Empirically, we find that higher levels of commitment in survey responses (measured by variables such as response length and number of action-based verbs) are linked to a higher tilt towards green portfolios. This analysis rejects the notion that the NAIC climate surveys are merely cheap talk; instead, insurers appear to act in accordance with the actions promised in the surveys. Prior work, along with anecdotal evidence, further corroborates our evidence. We are not the only paper to show that disclosures aligned with the international Task Force on Climate Disclosure (TCFD) Framework – such as the NAIC Climate Survey – have a significant impact on firm behavior (Wei, 2025; Cohen et al., 2023). Within the insurance industry, Pacific Life states that the NAIC Climate Survey holds them "accountable" and helps ensure "proper governance" on climate risks. Similarly, MetLife considers the NAIC survey as an "important source of disclosure" for its climate change governance, strategy and risk management. [3] Regulators such as the New York Department of Financial Services use

[2]To measure the greenness of a firm, we use the general "Climate Sentiment" measure from Sautner et al. (2023) – this metric captures the positive/negative tone of the conversation devoted to climate-related topics in earnings call transcripts. We choose to use sentiment measures, because they have a directional component, allowing us to interpret negative (positive) investor sentiment on climate topics as indicative of greater (lower) climate risk for the firm. Our results are not sensitive to the choice of measurement – when we use the "Climate Exposure" measure of Sautner et al. (2023) (without the tone association), we find similar results. Sautner et al. (2023) also show that climate attention in earnings calls is priced in the cross section, which merits a risk-based interpretation.

[3]For Pacific Life, see https://www.pacificlife.com/home/corporate-social-responsibility.html. The survey's importance for Metlife is discussed in https://sustainabilityreport.metlife.com/content/dam/metlifecom/us/sustainability/Sustainability-2024/sustainability-evergreen-microsite/pdf/MetLife-2023-CDP-Climate-Change-Report.pdf. For the EU-U.S. Insurance Project, see https://home.treasury.gov/system/files/311/government-affairs-eu-us-insurance-dialogue-project-ws1-climate-risk-financial-oversight-summary-report-june-2023.pdf

NAIC Survey responses to publicly grade insurers (NYDFS, 2024). In addition, these responses form a key component of a joint effort by the U.S. Department of Treasury and the European Union (EU-U.S. Insurance Project) to understand climate risk and financial oversight. Taken together, compelling evidence exists on the regulatory/supervisory consequences of the NAIC Climate Survey. This paper is among the first to study spillover effects of such supervision into shadow markets.

One of the contributions of the paper is to quantify the climate consequences, at the firm level, of insurers' shift towards more securitized holdings. To this end, for the remainder of the paper, we focus on CLOs—a rapidly growing class of ABS that are made up of individual firm loans. This unique data allows us to precisely identify the effects of supervision by allowing us to perform firm-level comparisons of climate risk between the underlying CLO firms and insurers' corporate bond holdings. To link insurers to firms within CLO markets, however, we have to first disaggregate CLO investments from the Insurer×CLO tranche level to the Insurer×Firm level. To do this disaggregation, we hand-match insurer data to the thousands of underlying firms held by each CLO. Thus, a key contribution of our paper is to study climate risks within the CLO "black box". By hand-matching insurer data to the underlying firms held via shadow market investments, we can study the behavior of the *underlying* firms in the CLO portfolio that receive additional capital.

Using a stacked difference-in-difference (DID) regression, we show that insurers subject to climate supervision tilt their direct portfolios toward greener bonds, resulting in a 0.6% decrease in average yields. To offset this loss, disclosing insurers purchase $0.8 million worth of additional CLO debt tranches per year. This estimate aggregates to $9.32 billion additional investments between 2010-2019—equal to 8% of the total growth in CLO holdings by insurers within this period. It is important to note that insurers could also purchase riskier and higher-yielding corporate bonds that are not brown, in order to recoup lost yields from selling brown firms. But riskier corporate bonds carry a higher risk-based capital (RBC) charge. In contrast, securitized holdings such as CLOs have a low RBC charge because insurers generally purchase the highest rated tranches. Crucially, CLOs offer higher yields than similarly rated corporate bonds (Cordell et al., 2023). Consistent with this logic, the redistribution of capital from direct to indirect holdings appears to be driven by regulatory constraints—insurers that are more limited by regulatory capital constraints redirect funds two to three times more than insurers with stronger RBC levels.

Since climate supervision was implemented in a staggered fashion across states and time, our results are unlikely to be driven by confounding factors such as macro-trends in institutional investing or unobserved characteristics associated with large insurers. But, staggered research designs can produce biased estimates if they are not correctly estimated (Callaway and Sant'Anna, 2021; Baker

et al., 2022). To address this problem, we implement a stacked difference-in-differences regression in all our main tests – we focus on a window spanning 13 years around the disclosure year using a control group that is comprised of insurers that either never disclosed, or that didn't disclose until much later in the sample. We also employ an alternate estimator specifically designed in Callaway and Sant'Anna (2021) to address staggered treatment bias and find consistent results.

A key concern with our natural experiment is that treatment effects could be correlated with insurer size because large insurers are more active in CLO markets relative to smaller insurers. Further, the rollout of NAIC climate survey, especially in the initial years (2010-2012), was based on total premiums (i.e. insurer size). We perform a number of tests to allay this concern. First, in all our regressions, we employ (high dimensional) Size Decile $\times$ Year fixed effects. Thus, our comparisons of treated and control groups are restricted to insurers that are similar in size. Next, we explicitly exclude the early filing period (2010-2012) which removes large insurers based out of California and New York. Remarkably, our results become economically more salient after this exclusion. This evidence is hard to square with an explanation where large insurers active in CLO markets are primarily driving our results. Further, we repeat our main analysis by scaling the dependent variable with the portfolio size to ensure differential growth rates between large and small insurers are not driving our results. Finally we also repeat our DiD analyses using a tightly matched sample, with a control group that (1) has a similar line of business, (2) operates in the same states, and (3) is comparable in terms of total annual premiums. Within this closely comparable group, we continue to find similar estimates. Taken together, these results strongly suggest that unobservables correlated with insurer size are not the main driver of our findings.

Next, we address the concern that climate-related losses on insurer liabilities, such as natural disasters, may be correlated with climate supervision and drive the observed diversion of capital into shadow markets. If this is true, then we would expect our results to be stronger for Property & Casualty (P&C) insurers. Yet, we find the opposite — results are primarily driven by Life insurers as opposed to P&C insurers, a pattern that is hard to square with climate-related losses. We also construct a "Liability Exposure" measure to analyze insurers' liability exposure to key disaster-prone states (ie California, Texas, Florida, and Louisiana). We confirm that our findings are not statistically different for disclosing insurers with greater liability exposures. In sum, both analyses fail to find evidence supporting the idea that liability risks, rather than supervisory pressures, drive the observed capital flow into shadow markets.

Ultimately, we want to understand how the supervision shock propagates to real climate outcomes by funding firms in shadow markets. To do this, we construct two measures that capture shocks to the CLO manager ("CLO manager shock") and the underlying firm ("Firm Shock"). First,

we measure the relationship between an insurer and a CLO manager using the amount that the insurer previously invested with the same manager. We find that the probability of an insurer investing in a CLO manager (as well as the dollar investment amount) significantly increases with the strength of the relationship. Summing across all insurers that CLO managers have relationships with, we create a *CLO manager shock*, which captures the extent to which disclosure shocks affect each CLO manager. We find that this shock leads to a greater probability of subsequent CLO fund launches, an increase in the total number of funds launched by the manager, as well as an increase in total dollar amount assets managed. These results not only support the notion that climate supervision increases the flow of funds to shadow markets, but they further show that this capital flow is selective and relationship-based. In addition, we find that CLO managers with strong insurer relationships are more likely to include browner firms in their portfolio. In sum, climate supervision had a significant, unintended consequence of financing brown firms by funding CLO managers that included brown firms in their portfolio.

To assess climate consequences on the underlying firms of the CLOs, we calculate a *Firm Shock* measure based on the total size of past investments between CLO managers who receive the supervision shock and the underlying firms. Consistent with a story of loosening credit in CLO markets, we find that the *Firm Shock* measure is associated with increased issuance of future leveraged loans. We then study the environmental impact of the firms that receive extra capital flow from insurers via shadow markets. Using the EPA's Toxics Release Inventory data, we find that firms which receive a one s.d. greater shock through the CLO market have an average emission increase of 12.87 pounds per dollar of assets, or a 17% increase from the unconditional average. This striking evidence of the Green Paradox (Jensen et al., 2015) has a significant policy implication for insurance regulators – bringing shadow market investments into the climate disclosure framework may be important to achieve effective climate supervision.

To summarize, even though climate supervision may initially lower the net climate risk of insurer holding (by lowering risks in direct bond portfolios), a key finding of our paper is that regulatory climate scrutiny can have the unintended consequence of *shifting significant climate risk into rapidly growing shadow markets*. This finding has relevance beyond the insurance industry, given the ongoing trend of institutional investors funding non-transparent, outsourced assets.

Our paper makes three main contributions. First, we show that insurance companies, through their investments in CLO markets, play a significant role in the financing of brown firms. While our initial results on corporate bond holdings are consistent with emerging evidence showing that institutions mitigate climate risk (Cao et al., 2021), our main findings on CLOs stand in sharp contrast because CLO investment decisions appear to be dominated by reach-for-yield considerations

that increase investment into brown firms. Next, by drawing a link from the Climate Risk Disclosure Survey to its effect on insurers' underlying investments, we show that regulation intended to make climate risks more transparent can have the unintended consequence of *increasing* risks in hard-to-monitor markets. Since CLO market investments are rapidly growing, our evidence suggests that it may become harder for stakeholders to monitor project outcomes and incentivize investments that align with a sustainable future. This finding is especially important given the rising trend of institutional investors choosing externally managed portfolios. Finally, we contribute to the climate disclosure literature which studies the relationship between climate disclosures and future carbon emissions. While research has focused on the impact of underlying firms' climate disclosures on firm value (Matsumura et al., 2014), our paper studies the effect of climate disclosures by the institutional *investors* that fund those firms.

### 1.1 Related Literature

Our paper relates to three strands of literature: the regulation of insurance firms, the study of how institutional investors respond to climate risk, and understanding the environmental impact of climate supervision.

First, we contribute to a large and growing body of work that studies the financial regulation of insurance companies. On statutory requirements, Becker and Ivashina (2015) show that RBC weights lead insurers to reach for yield within a rating bucket; Koijen and Yogo (2015) show that life policies significantly deviated from fair value to capture regulatory arbitrage on reserve requirements; Koijen and Yogo (2016) show that insurers sold liabilities to shadow re-insurers to avoid regulatory oversight. Unlike banks that are largely regulated at the national level, insurers are regulated by the states they operate in, which allows for heterogeneous frictions arising from the state regulators. Tenekedjieva (2020) and Liu and Liu (2024) find evidence of political frictions at the state level. Focusing on climate risk, Oh et al. (2022) find that states with low price regulation subsidize the cost of climate risks from states with high price regulation, and Jung et al. (2024) find that insurers operating in states with high climate risks have higher sensitivity to physical risk, requiring different capital shortfalls under various stress testing scenarios. Our study adds to these findings by quantifying the unintended effects of mandatory NAIC climate disclosure laws enforced in a staggered fashion across states.

Second, we study the flow of capital when insurance companies– the largest institutional investors in corporate bond markets – respond to climate risk; we provide new evidence that insurers respond to this risk differentially in their direct versus shadow market investments. This contrast speaks to the distinction between *value-based* and *values-based* investing motivations described in

Starks (2023). Past literature has shown that institutional investors can influence the climate risk disclosure practices of the underlying firms they invest in (Ilhan et al., 2023); institutional investors pay attention to the financial implications of perceived climate risks (Krueger et al., 2020; Berry-Stölzle et al., 2024); and insurance companies are more likely to sell bonds that are issued by firms which are high carbon emitters (Cao et al., 2021). Perhaps most closely related to this paper, Ivantsova et al. (2021) find that insurers hold fewer carbon-intensive bonds after being required to disclose their views on climate change. Adding to this discussion, we find that incentives for investing in CLOs can differ from those of corporate bonds.

We also add to the emerging literature on disclosure and climate risk, which has had mixed findings. On the one hand, Cohen et al. (2023) show that investors who are signatories of the CDP (Carbon Disclosure Project), engage more with and divest from polluting firms. On the other hand, Giannetti et al. (2023) show that banks that showcase concern for the environment in their disclosures to investors, lend more to brown industries. They argue that greenwashing by banks is because of zombie lending practices, i.e. banks continue to prop up low-performing firms that are close to financial distress. Bartram et al. (2022) show that firms faced with climate regulation in a particular state shift their emissions to other states where their plants are underutilized, undermining the effectiveness of climate regulation. Perhaps closest to our paper, Hackenberg et al. (2024) show that banks facing divestment pressure are more likely to invest in opaque CLOs in order to keep exposure to brown industries. In contrast, our paper studies the broader class of ABS and it unveils the reinvestment motives behind insurance companies—one of the biggest CLO investors who hold over a quarter of all assets outstanding (Johnson et al., 2023). Ultimately, we find that mandated disclosure can lead to more capital flow into shadow markets, leading to the unintended financing of brown firms. This result speaks to research in environmental economics on the Green Paradox – a situation where climate policies intended to mitigate climate risk have the opposite effect (Jensen et al., 2015).

## 2 Data

We combine data from multiple sources to conduct our empirical analysis, and these sources are described below.

**NAIC Insurance Data.** First, we obtain insurers' holdings information from annual NAIC Statutory filings, ranging between 2001 and 2020. More specifically, we use Schedule D Part 1 filed by P&C, Life and Fraternity, as well as Health Insurance companies. In addition to the insurer's name,

filing year, and line of business, we also obtain information on the CUSIP, total holding amount, name, effective interest rate and maturity date of each fixed income security held. Since our focus is primarily on the non-financial firms, we filter out government bonds, municipal bonds, as well as bonds issued by financial institutions from the insurer balance sheets. In Panel A of Table 1, we summarize these insurer holdings: insurers hold, on average, $141 million of ABS and $6.23 million of CLOs. In addition, securitized holdings are not pervasive— more than half of insurer x year observations do not have either type of securitized asset in our sample.

To match the NAIC holdings data with the climate database, we found that the most effective match came from linking to Compustat and then matching to the climate data using GVKEY. Matching the holdings data to the climate database requires careful detailed effort because many firms were either privately owned without a CUSIP, or had CUSIPs that changed over time, or simply had multiple CUSIPs despite being owned by the same parent company. As a result, we hand match a substantial number of companies based on their name and their parent group.

**Climate Disclosure Data.** We gather insurers' public climate disclosures using the NAIC Climate Risk Disclosure Surveys published online from 2010 until 2021. Prior to 2011, only companies with greater than $500 million in direct premiums had to disclose. In 2011, only companies that did business in the state of California with total direct premiums of $300 or more had to disclose. The biggest jump in coverage came in 2012, when California, New York, and Washington required all active insurers with premiums of $300 million or more to take the survey and make the results public. In 2013, the premium requirement for filing was lowered to $100 million, and three more states (Connecticut, Minnesota, and New Mexico) mandated participation from their active insurers. While the threshold remained at $100 million, nine more states/jurisdictions mandated survey participation in 2021: Delaware, District of Columbia, Maine, Maryland, Massachusetts, Oregon, Pennsylvania, Rhode Island, and Vermont. NAIC estimates that feedback from the mandated insurers capture nearly 80% of the entire U.S. insurance market in 2021.

The exact questions asked on the survey change over time and may reflect concurrent issues, such as natural disasters. However, they generally cover the following broad areas: emissions, risk assessment, and investments. Most relevant for our study, question 5a asks "Has the company considered the impact of climate change on its investment portfolio?" As Figure A1 shows, the majority of insurers respond "yes," and further, the fraction of "yes" responses increase over time. A key institutional detail is that more than half of the insurers responding to the survey are volunteers, not mandated to disclose by any particular state. This is because over 90% of the time, voluntary filings are submitted by companies belonging to a larger insurance group that is required to disclose. Usually, these volunteers submit the exact same response as fellow insurance companies mandated

to respond under the same parent group (Lin et al. 2023).

Figure 2 Panel A shows the number of insurance companies that first responded to the climate survey over time. While many companies started responding from the beginning of our sample, we see the biggest jump in participation in 2012, as the number of participating states increased from just California to include New York and Washington.

**CLO Data.** We use the data on Collateralized Loan Obligations (CLO) from Acuris Creditflux, who source detailed information on fund launches, portfolio holdings and transactions, tranche investments and payouts from more than 45,000 fund trustee reports of CLO funds globally. It contains monthly information on fund capital structure, detailed security holdings, transactions, and monthly covenant tests of 3134 funds launched by 223 managers over the period 2001 through 2020. We hand-match fund and tranche names to the portfolio holdings of insurance companies, obtained from NAIC Schedule D filings to identify investments of insurance companies in CLO funds over the sample period.[4] We identify investments in debt and equity tranches of 2418 funds by 3457 insurance companies.

Figure 1 shows the rapid, striking growth in CLO holdings by insurance companies, as reported in Schedule D of NAIC statutory filings, in blue. We see that it grew from 3.91 billion in 2001 to over $175 billion in 2020. As a percentage of total insurer assets, investments in CLO funds represented less than 0.5 percent in 2001, but grew to more than 5 percent of total assets by 2020. In the same figure, the red line represents the growth in general securitized asset holdings, or ABS, over the same period. We see that, mirroring the rapid growth in CLOs, it grew from roughly $115 billion in total holdings in 2001 to roughly $1.11 trillion in total holdings in 2020.

Panels B through C of Table 1 provide summary statistics for the CLO dataset. We see that the average CLO fund (Panel B) holds $724 million, includes 480 different firms, and is highly leveraged (93%). CLO fund managers are active and launch 0.56 new funds every year (Panel C).

**Climate Data.** Our climate measures come from Sautner et al. (2023). In this data, climate exposure is the share of conversation during an earnings call transcript that is related to climate-related topics. These measures count the frequency of specific climate change bigrams in a transcript, scaled by the total number of bigrams. They further categorize the sentiment around these bigrams as either positive or negative, based on the tone of the words. For instance, one example of a positive sentiment change was when TI Fluid Systems committed to a significant reduction in Scope 1 and 2 $CO_2$ emissions and more renewable energy projects. Generally, firms with more

[4]Our approach is similar to that in Bhardwaj et al., 2022. However, we cover all insurance companies while they focus only on Life and Property & Casualty insurers.

positive climate-related sentiment are engaging in more green activities. Sautner et al. (2023) document a statistically significant association between the positive climate sentiment and future green investments and green job creation.

**Debt Origination Data.** We obtain data on syndicated loan originations from LPC Dealscan database. It contains details of all syndicated loans issued during our sample period, including origination and amendment dates, size, credit spreads, maturities, and the names of arrangers and lenders, as well as their lending commitments. We match issuing firms in the Dealscan sample to those in the SNL and CLO databases based on their names, yielding a sample of $9,428$ unique borrowers.

We restrict attention to two types of loans : (i) leveraged loans, which are primarily originated to be distributed by financial institutions; and (ii) revolving credit facilities which are typically kept on the books of the underwriting institutions. We define a leveraged loan as senior-secured term-loan facility which has an all-in-drawn spread of at least 125 basis points over the base rate, typically LIBOR. This restriction yields $33,526$ leveraged loans issued by $10,486$ firms over our sample period. A typical syndicated leveraged loan in our sample has an issue amount of $350 million, a maturity of 6 years, and a all-in-drawn spread of 350 basis points over LIBOR. Our sample also includes $58,412$ revolving credit facilities issued by $22,177$ unique firms. A median revolver has a size of $474 million, a credit spread of 150 basis points, and a maturity of 3.67 years. Panel D of Table 1 summarizes information on firm issuances. We find that each firm in our sample has a 6% chance of being included in a CLO fund, and the average loan size is about $67 million.

**Emissions/Reprisk Data.** We use facility-level information on the polluting activities of firms from the U.S. Environmental Protection Agency (EPA). The EPA's Toxics Release Inventory (TRI) program tracks emissions of toxic chemicals by industrial facilities, and makes this data publicly available.[5] Panel E of Table 1 summarizes the emissions data at the firm level: on average, each firm emits 221,000 pounds of pollution per chemical, and they release 21 different chemicals on average. There is a lot of variation among firms, and the dataset is skewed by large firms who pollute more. For example, the median firm only emits 9,000 pounds of pollution per chemical and emits 11 different chemicals. For robustness, we also use data from RepRisk, an ESG data science company that specializes in collecting information on climate risk-related incidents. RepRisk screens public sources such as media outlets, regulatory documents etc. for news related to ESG risks. RepRisk classification of pollution incidents include: "climate GHG pollution" "local pollution",

[5]https://www.epa.gov/toxics-release-inventory-tri-program/tri-data-and-tools

"waste issues", "airborne pollutants", "forest burning", "greenhouse gas emissions", and "wastewater management". These incidents are further categorized based on their "severity" and "reach" (i.e. readership in media outlets). For our main analysis, we use the Sautner et al. (2023) climate measures instead of RepRisk for two reasons: a) RepRisk data focuses on negative reputational damage from climate-related incidents, whereas the Sautner et al. (2023) exposure measures allow for climate change to be an *opportunity* for some firms – thus the latter measures explore both positive and negative firm-level impact. b) Unlike the Sautner et al. (2023) measures, the distribution of news-based risk incidents in RepRisk tends to be highly skewed (Hoepner et al., 2024).

## 3 Direct Effects of Climate Supervision

We begin our analysis by establishing the effects of the NAIC climate supervision on insurer balance sheets: insurers tilt their corporate bond holdings towards greener firms, and these portfolios experience a loss of yields. To measure greenness, we analyze the average climate sentiment of the bonds in an insurer's portfolio. In subsequent sections, we will study how insurers recoup lost yields by investing more capital into asset-backed securities.

In Figure 3 Panel A, we plot dynamic coefficients from the following regression:

$$\mathrm{CC}_{it} = \alpha_{ie} + \alpha_{et} + \sum \beta_t D_{i,t} + \varepsilon_{it} \tag{1}$$

where $\mathrm{CC}_{it}$ denotes average climate sentiment (weighted by holding amount) of the bonds in the portfolio of insurer $i$ at time $t$. The climate sentiment $C_{jt}$ data for firm $j$ at time $t$ is taken from Sautner et al. (2023). We construct the aggregate climate sentiment $CC_{it}$ at the insurer-year level as

$$\mathrm{CC}_{it} = \sum_{j \in \mathcal{I}} \omega_{ijt} \mathrm{CC}_{jt} \tag{2}$$

where $\omega_{ijt}$ is the weight of firm $j$ in the portfolio of insurer $i$ at time $t$. The dummy variable $D_{i,t}$ equals one for each year $t$ relative to event year $e$ for treated insurers, and zero otherwise. We focus on the window $[e-3, e+6]$ around the first disclosure year. From Figure 3 Panel A, we see that immediately following supervision, the climate sentiment of insurers' direct holdings, when compared to a control group of non-disclosing insurers, significantly increases as they shift toward greener investments.

The regression setup in Figure 3 and all subsequent figures employ a stacked panel spanning 13 years around the event date, with $e \in (2010, 2019)$ representing the year in which an insurer first disclosed its climate strategy. For each event year $e$, treated insurers are those that first disclose

their investment strategy related to climate risk in year $e$, while control insurers are those that either begin disclosing in year $e+7$ or never disclose during the sample period. This stacked specification is crucial for mitigating the timing biases introduced by staggered treatment adoption. Additionally, every regression includes event-insurer fixed effects ($\alpha_{ie}$) and event-time fixed effects ($\alpha_{et}$), and all standard errors are clustered at the insurer level.

In Figure 3 Panel B, we study the effect of climate supervision in the yields of direct bond holdings. More specifically, we plot dynamic coefficients from the following regression:

$$Yields_{it}^{Direct} = \alpha_{ie} + \alpha_{et} + \sum \beta_t D_{i,t} + \varepsilon_{it} \tag{3}$$

where $Yields_{it}^{Direct}$ is the holdings-weighted effective yield on the direct bond holdings of the insurance company $i$ in year $t$. Again, the dummy variable $D_{i,t}$ equals one for each year $t$ relative to event year $e$ for treated insurers, and zero otherwise. From this figure, we see that at the same time that the direct portfolios of disclosing insurers are rebalanced towards greener assets, their return performance decreases relative to the direct portfolios of insurers who are not forced to disclose—on average, yields of affected insurers drop by up to 2 percentage points. This evidence suggests that climate supervision restricted disclosing insurers to a constrained set of firms, and thus, these insurers were less able to optimize their yield performance post-disclosure. Appendix Table A6 provides further evidence —a one standard-deviation increase in firms' climate sentiment is associated with a 5 to 8 basis points decrease in their bond coupons issued the following year.

### 3.1 Exogeneity of the Climate Supervision shock

While the staggered implementation of NAIC's climate supervision is an advantage for empirical identification, the initial administration of the survey was based on insurer size thresholds. This fact raises concerns that affected insurers (the "treatment") may be correlated with insurer size. Although the distribution of size across treated insurers remains stable for most of our sample (Figure A1(b)), we may be concerned that large insurers, affected by the initial survey wave, are systematically different from those that were treated at a later point in time. If so, these systematic size differences, rather than climate supervision, could be driving the tilt towards green bonds.

We address these concerns in multiple ways. In Table 2, we use a (static) stacked DID regression specification where the main dependent variable ($y_{it}$) is the holdings-weighted climate sentiment (Column (1)), climate exposure (Column (2)), and effective yield on the direct holdings (Column

(3)) of insurer $i$ in year $t$. :

$$y_{it} = \alpha_{ie} + \alpha_{et} + \beta \text{Treated Insurers}_{ie} \times \text{Post}_{et} + \varepsilon, \tag{4}$$

In both panels of Table 2, in addition to Event × Insurer FE and Event × Year FE, we use high-dimensional size fixed effects (Size Decile × Year FE). This means, the analysis in Table 2 compares treated and control insurance firms that are *within the same size decile in a given year*. After soaking up size-based variation among insurers, we continue to find results that are consistent with our findings in Panels A and B of Figure 3. Across all treatment years, treated insurers increase the climate sentiment of their direct portfolio by 0.076 or by 9% (0.076/0.83) of the unconditional average. Concurrently, they lose 0.128% in direct investment yields, representing 3.1% (0.128/4.12) of the unconditional average yield.

In Panel B of Table 2, we perform a more stringent test where we fully exclude all early filers (i.e., the large insurers of California and New York who participated in the initial wave). Remarkably, despite the exclusion of early survey adopters that comprised several large insurers, our coefficient magnitudes are virtually unchanged. Thus, Panel B rejects the claim that our results are driven by unobservables correlated with insurer size.

### 3.2 Are the climate disclosure surveys merely cheap talk?

Prior work has shown that climate disclosures aligned with the Task Force on Climate Disclosure (TCFD) framework have a significant impact on firm behavior (Cohen et al., 2023). Anecdotally, insurance companies hold this view as stated in their official corporate responsibility statements. For example, Pacific Life states that the NAIC Climate Survey holds them "accountable" and helps ensure "proper governance" on climate risks. Similarly, MetLife considers the NAIC survey as an "important source of disclosure" for its climate change governance, strategy and risk management. Even so, we might still be concerned that our survey-based natural experiment may not be effective, since the consequences of non-compliance are not explicitly stated by NAIC.

We address this concern by taking a closer look at the textual content of insurers' survey responses.[6] First, we begin by plotting a histogram of their length in Panel A of Figure 4. We find that while most insurers respond with a short paragraph of 200 words or less, there is a lot of variation and some responses span 1000 words or more. Next, if insurers are actively managing climate risk, then this should be reflected in the words of their responses. Panel B of Figure 4 shows

[6]Table IA6 alternatively addresses this concern by showing that insurers are more likely to stop doing business in states that adopt a disclosure rule—especially if they do not do much business in that state.

the 50 most commonly used verbs scaled by their frequency in the surveys. We see that "manage", "invest", "identify," and "assess" dominate these responses, which suggests that the surveys promise meaningful actions.

Importantly, not all verbs used in the survey involve taking action or making a change. Commonly used verbs such as "associate", "believe", "continue", and "maintain" merely imply mental reflection or keeping the status quo. Thus, to isolate verbs that are more likely to imply a change in behavior, in Panel (b), we plot a word-cloud using the subset of action-based verbs. This word cloud includes many of the top verbs from before, but it also highlights new verbs such as "mitigate", "reduce", "limit", and "divest".

To investigate further, we construct measures of "verbal commitment" by insurers and incorporate these textual measures in regression analysis to verify if they correlate with actual behavior. *Text Length* measures the length of the survey response (total number of words divided by 100), *Action Verb Count* measures the number of action-verbs in the survey disclosure, and *Action Verb Intensity* measures the number of action verbs normalized by the total text length. If climate disclosure surveys carry real weight, then higher values of these measures would indicate greater attention to climate risk and be correlated with the tilt towards green bonds. On the other hand, if climate disclosures are ineffective by virtue of being surveys, we should not see any statistically significant relationship with insurer portfolio behavior.

Table 3 presents the results. Across all three measures of textual commitment, we find that the triple interaction term Treatment $\times$ Post $\times$ *Textual Commitment* is significantly associated with: greater tilt towards green bonds (Columns 1 and 2) and lower yields in the direct bond portfolio (Column 3). Ultimately, we find evidence that the textual content of survey responses carry real weight. These results are not consistent with notion that climate disclosures are just cheap talk, and they suggest that insurers follow through with the claims made in these surveys by changing their direct investments consistently.

# 4 Unintended Effects: Capital Spillovers into Shadow Markets

So far, we have established the baseline finding that climate supervision affected insurers' portfolio allocation strategies. Treated insurers rebalance their direct portfolios towards greener firms, which results in an average decrease in yields. These findings raise a natural question: do insurance companies compensate for the reduced yield by directing capital to higher-yielding investments? If yes, do they increase their allocation to high-yield bonds within their direct portfolio or do insurers invest indirectly through funds that potentially offer higher yields? The first strategy is costly: (i)

it may heighten investor scrutiny, and (ii) it could lead to an increase in risk-based capital (RBC) requirements for insurers. In contrast, the safest tranches of securitized bonds such as ABS and CLOs offer a solution – Cordell et al. (2023) show that CLOs offer higher yields than similarly rated corporate bonds. We confirm this fact in Appendix table Table A4 which shows that CLO tranches in our sample provide, on an average, 57 basis points more (96 basis points when we remove the highest $AAA$ rated securities) relative to corporate bonds with similar rating and maturities. So we expect climate supervision to increase indirect investment into securitized bonds by insurance companies.

We begin by studying trends in ABS investments (which include CLOs) of treated and control insurers. Figure 5 Panel A plots the dynamic coefficients of the regression:

$$Investment_{it}^{ABS} = \alpha_{ie} + \alpha_{et} + \sum \beta_t D_{i,t} + \varepsilon_{it} \tag{5}$$

where $Investment_{it}^{ABS}$ denotes the total investment (in \$ millions) of insurance company $i$ in ABS tranches in year $t$, and $D_{i,t}$ is the year dummy from before. From this, we see that that treated insurers immediately increase indirect ABS holdings by up to \$5 million in the year following supervision—and this cumulates up to almost a \$15 million investment increase over our event window. [7]

Next we look at the average yields of these indirect investments. Figure 5 Panel B plots the dynamic coefficients of the regression:

$$Yields_{it}^{ABS} = \alpha_{ie} + \alpha_{et} + \sum \beta_t D_{i,t} + \varepsilon_{it} \tag{6}$$

where $Yields_{it}^{ABS}$ is the holdings-weighted effective yield on the indirect investments of the insurance company $i$ in year $t$, and $D_{i,t}$ is the year dummy from before. Now, we see that the increased capital flow into securitized, opaque holdings is likely driven by the fact that these ABS holdings yield up to 2.5% more. This magnitude is also strikingly similar to the one found in Figure 3 Panel B, suggesting that these new securitized investments can offset the lost yields from re-balancing the direct portfolio.

Aggregating over all treated insurer-year observations, the consequences of climate disclosure on ABS capital flow is economically significant. We estimate that the additional shadow capital flow of approx. \$5 million per year translates to ≈\$58.2 billion of shadow investments, attributable

[7] To save space, a formal table with all estimates are shown in the Appendix Table A2.

to tightening regulations around climate change.[8] In economic terms, this estimate represents an increase of 13.7% of the total increase of $400 billion in ABS holdings by insurers over this period.

### 4.1 Capital flow into CLO markets

In our subsequent analyses of capital flow into shadow markets, we will focus on CLOs. CLOs are not only a large and rapidly growing type of ABS, but also have the added advantage of being naturally comparable with the majority of insurers' direct investments—corporate bonds. To perform this comparison, however, we need firm loans within a CLO tranche to be deconstructed and individually hand-matched to firm-level measures of climate risk. This disaggregation procedure provides a unique view of climate risks within the CLO "black box" – a new contribution of our paper. In the following sections, we will provide evidence of capital flow into shadow markets that mainly comprise of CLO purchases. We will also provide new evidence of the supervisory mechanism behind this substitution effect and conduct several robustness tests. Then, in Section 5, we will estimate the implications of this shadow capital flow by tracing out the impact of supervision on the behaviors of the underlying firms.

To begin, Table 4 uses a stacked difference-in-difference (DID) regression to verify that the baseline findings on all ABS investments also hold for the CLO subset. The regressions in Table 4 estimate:

$$y_{ijt} = \alpha_{ie} + \alpha_{et} + \beta \, \text{Treated Insurers}_{ie} \times \text{Post}_{et} + \varepsilon_{ijt}. \tag{7}$$

where $y_{ijt}$ is the total holdings of CLOs to date (Columns (1) and (3)), active new CLO purchases (Columns (2) and (4)), or CLO spreads (Column (5)). The insurance company is denoted by $i$, CLO tranche by $j$, year is $t$.

The main takeaway from Table 4 is that, relative to the non-disclosing control group, treated insurers (i) hold a higher stock of CLOs on average, (ii) actively purchase more CLOs per year, and (iii) CLO investments following climate supervision have higher spreads. Column (1) of Panel (A) shows that, on average, the stock of CLOs held by treated insurers is $1.45 million higher than control insurers per year. In terms of flow, treated insurers purchase an additional $760,000 worth of CLO tranches per year, relative to the control group. This flow of CLO capital aggregates to

[8]To arrive at this estimate, we use Figure 2 to first calculate the number of insurers that disclosed per year. For example, the 31 insurers who started disclosing in 2010 were treated for 10 years between 2010-2019; the 436 insurers who started disclosing in 2011 were affected for 9 years; the 585 insurers who started disclosing in 2012 were affected for 8 years, etc. Then, we calculate: $5 million $\times$ (31$\times$10+436$\times$9+585$\times$8+199$\times$7+92$\times$6+80$\times$5+32$\times$4+21$\times$3+62$\times$2+61$\times$1)=$58.2 billion.

$8.85 billion of additional CLO investments between 2010-2019.[9] This growth accounts for 8% of the total aggregate growth in CLO holdings by all insurers within our sample period. Panel A Column (5) of Table 4 shows that the CLO spreads are 13.6 basis points higher for treated insurers following climate supervision. This evidence is consistent with insurers seeking higher yielding CLO investments to offset the loss of yields on their direct portfolios.

Table 4 performs a tight comparison of treated and control insurers that mitigates a number of concerns with the interpretation so far. Our sample period included an episode of low interest rates, so we may be concerned that time trends in reach-for-yield behavior could explain our results. But note that our empirical analysis studies the *differential* trend between treated and control insurers, and is conducted in *event time*, not calendar time. Thus, our staggered DiD research design mitigates these concerns. Nevertheless, we use high dimensional fixed effects to control for time trends in the insurance industry as well as insurer preferences for CLOs using event time ($\alpha_{et}$) and Insurer *times* Event ($\alpha_{ie}$) fixed effects.

### 4.1.1 Insurer size as a potential omitted variable

We acknowledge that capital flow into CLOs by large insurers can be substantially different from that of small insurers.[10] It is important to note that the analysis in Table 4 compares treated and control insurers *within the same size decile*. All our regressions employ Size Decile × Year fixed effects [11]. In addition to restricting the comparison to insurers that are comparable in size, there are two key aspects of the comparison in Table 4 that reject the notion that size differences are primarily driving our results. First, to distinguish between a general increase in portfolio size versus an active pivot towards CLOs, Columns (3) and (4) of Table 4 scale the dependent variable by the total insurer portfolio. After this scaling, we continue to find consistent results. Thus, if large insurers grew at a faster pace than small insurers in the CLO market, and if this growth somehow coincided with climate supervision, such differential trends would still not explain the findings in Columns (3) and (4). Second, in Panel B of Table 4, we perform a stringent test by excluding all early filers – which specifically excludes all large insurers from important markets such as California and New York. Remarkably, excluding these large insurers *increases* the economic significance of

[9] Similar to the ABS analysis in Section 3, we can use the number of treated insurers in Figure 2 to calculate: $0.76 million × (31×10+436×9+585×8+199×7+92×6+80×5+32×4+21×3+62×2+61×1)=$8.85 billion.

[10] https://content.naic.org/sites/default/files/capital-markets-special-reports-clo-ye2023-final.pdf

[11] Additional specifications that control for insurer size using a cubic spline are shown in Table IA1.

our results across all columns. If our results could simply be explained away by size as an omitted variable, it is difficult to explain why our results become more economically salient after excluding large insurers. These results, taken together, strongly reject size-based differences as a key alternate explanation for our findings.

### 4.2 The importance of risk-based capital constraints

Why do insurance companies redirect capital into CLO markets, when they can potentially rebalance their direct portfolio towards higher yielding securities? While climate supervision may keep insurers from directly investing in brown firms, they could potentially substitute into other direct investments with high yields, such as corporate bonds with lower credit quality. We conjecture that insurers have to manage their RBC (risk-based capital) ratios, and low-rated corporate bonds carry a higher capital charge while highly rated tranches of CLOs carry a lower capital charge.

Consequently, RBC costs *combined with* climate supervision make securitized assets an attractive substitute. If our hypothesis is true, then we would expect our findings to be stronger for insurers who have low RBC ratios, and thus, have more binding capital constraints. To test this empirically, in columns (2) and (4) of Table 5, we interact the treatment variable with a dummy equal to one if the insurer had more binding regulatory capital constraints, defined as being in the bottom half of all insurers when ranked by their RBC ratio.[12] Indeed, looking at the coefficient on the triple interaction term "Treated Insurers x Post x Low RBC", we see that insurers with disclosure requirements *and* tighter RBC constraints hold $2.5 million more CLOs (Column (1)) and buy $1.16 million more CLOs (Column (2)) on average. Compared to the coefficients on "Treated Insurers x Post" alone ($2.9 million and $1.4 million in columns (1) and (2), respectively), we find that the effect of capital reallocation into CLOs is essentially doubled for insurers with tighter RBC constraints. [13]

[12]While regulatory consequences for insurers technically bind at RBC ratios of 2 or less, ratios this low are rare in practice. Interestingly, Ge and Weisbach (2021) finds that insurers' portfolio strategies appear to be pre-cautionary and are affected by RBC concerns even at much higher ratios such as 5. Following this, we break insurers into high versus low RBC instead of drawing a strict cutoff at 2.

[13]Furthermore, consistent with a story of regulatory arbitrage by reaching for yield in CLO's, Table A5 shows that insurers with low RBC's are precisely the ones who purchase CLO's with higher yields following disclosure.

### 4.3 Liability-driven capital flow

We now address a key concern related to our findings: rather than asset-side drivers, the adoption of the climate survey by various states may be driven by insurers' exposure to climate risk on their liabilities side, such as their states of operation. For example, California, one of the first states to adopt the disclosure mandate, is prone to natural hazards like floods and wildfires, which often in large insurer outflows. Is it possible that insurers' reach-for-yield behavior instead stems from their incentives to manage losses due to natural disasters?

We first note that if liability exposure to natural disaster is a major driver of our results, we expect the increase in CLO investments to be driven by the behavior of Property & Casualty insurers rather than Health or Life insurers. To test this hypothesis, Table 6 studies heterogeneity of CLO investments across insurer types. In Columns (1) and (3) of Table 6, insurers are classified into Life and Non-Life groups. The coefficient of interest is the triple interaction term Treatment $\times$ Life $\times$ Post. We see that the coefficient is both economically and statistically significant, suggesting that the diversion of capital after climate supervision is concentrated among Life insurers. Next, in Columns (2) and (4), we study the differences between P&C and other types. Here, we see that the triple-interaction coefficient is *negative* and significant, a pattern that is hard to square with incentives driven by climate losses. If treatment were correlated with climate disasters, then we would expect a stronger effect on Property & Casualty insurers, but the evidence rejects this possibility.

In addition, we construct a measure that captures the sensitivity of insurer liabilities to natural disasters by calculating the fraction of total premiums that come from four disaster-prone states: California, Florida, Texas, and Louisiana. We test whether insurers with higher-than-median levels of this "Liability Exposure" are more sensitive to the supervision shock. If true, such a result would heighten the concern that climate risks on the liabilities-side are a key explanation for our results. To verify this hypothesis, Table 7 interacts the *Liability Exposure* measure with Treatment $\times$ Post. To test whether these insurers are more likely to hedge against liability risks in their direct portfolios, the dependent variables in Columns (1) and (2) are the climate sentiment and climate exposure of direct holdings. Next, to test if these insurers are more likely to recoup lost profits through shadow markets, the dependent variables in Columns (3) and (4) are CLO purchases and CLO holdings, respectively. In all these cases, we find that the triple interaction term Treatment $\times$ Liability Exposure $\times$ Post is insignificant. The effects of climate supervision are not greater for insurers with greater exposure to disaster-prone states. This result confirms that climate risks of insurer liabilities cannot be the main explanation for our findings.

### 4.4 Additional Robustness Checks: Matched-Pair Design, Alternative Estimator, and Group-level Analysis

In this section, we conduct three additional robustness tests that address potential omitted factors, the stacked treatment design, and decision-making at the insurer-group level.

First, to address the concern that the control group is not selected "at-random" but are instead correlated with omitted characteristics such as type of insurer (eg life or P&C), total size, or states of operation, we repeat the stacked DID analyses using a matched control sample. The matched sample is based on (1) line of business (2) states of operation and (3) a proxy for insurer size— total annual premiums collected. Table A1 reports estimates from the matched specification:

$$y_{it} = \alpha_{i,e} + \alpha_{e,t} + \pi \text{Treatment}_{i,e} \times \text{Post}_{e,t} + X_{i,t} + \varepsilon_{i,t}, \tag{8}$$

We see that, from this matched pair analysis, reallocation effects from climate disclosure are consistent and actually slightly larger than the standard stacked regression. Compared to a matched control sample of non-disclosing insurers, column (1) shows that disclosing insurers buy $4.7 million more CLOs, and column (2) shows that disclosing insurers hold $8.4 million CLOs on average. Since we match insurers based on the states of operation, we account for the state-level variation such as political attitudes towards green investments. Overall, we fail to find evidence that our results are driven by potentially confounding factors such as insurer size, type of insurance company, and political leaning at the state level.

Next, since treatment dates are dispersed over time, in addition to a stacked regression, we can alternatively account for heterogeneous treatment effects by using the Callaway and Sant'Anna (2021) estimators. This method is shown in Table IA2.

Table IA2 estimates the following Callaway and Sant'Anna (2021) regression:

$$y_{it} = \alpha_i + \alpha_t + \beta \mathbf{1}(\text{Treated})_{it} + X_{it} + \varepsilon_{it} \tag{9}$$

In the first two columns, we use the insurers who never disclosed as the control group, while in columns (3) and (4), we use the insurance companies who disclosed later as the control group. We find results that are qualitatively consistent with the baseline stacked regression shown in Table 4: insurers significantly increase their CLO holdings in both the intensive and extensive margin following climate supervision. This result supports our finding that supervision appears to push insurers' capital into shadow markets.

Last but not least, one may be worried that investment decisions are made at the insurer-group

level rather than at the insurer-company level. If so, then we may be capturing an incomplete picture or overinflating standard errors if we instead conduct our analyses at the individual company level. To address this concern, we rerun all of our main outcomes but collapse the data to the group level. The results are shown in Table A3, and they are consistent with our analyses at the individual company level. Following a mandate to disclose by one of their companies, insurer groups as a whole are more likely to tilt greener in their direct portfolios (columns 1-2), spend more capital on CLO investments (columns 3-4), and perhaps as a result, receive a higher spread on their shadow investments (column 5).

# 5 Effect on the Underlying Firms Held by CLOs

After establishing that climate supervision causes insurers to reallocate capital to shadow markets like CLOs, we ask: What are the implications for firms that borrow from these capital markets? What are the climate consequences of increasingly funding firms through shadow markets? In this section, we address these questions by tracing how the climate disclosure shock propagates to real climate outcomes by funding firms in shadow markets.

We start by understanding how insurers choose CLOs to fund. We measure the relationship between an insurer and a manager using the amount that the insurer previously invested with the same manager and find that the probability of an insurer investing in a CLO manager (as well as the dollar investment amount) significantly increases the relationship. Summing across all insurers that each CLO manager has a relationship with, we create a new measure, called *CLO manager shock*, which captures the extent to which disclosure shocks affect each CLO manager. We find that *CLO manager shock* is associated with a greater probability of subsequent CLO fund launches, an increase in the total number of funds launched by the manager, as well as an increase in total dollar amount assets managed. Interestingly, we also find that CLO managers with past relationships are more likely to include browner firms in their portfolio. In turn, the firms that receive more funding from *CLO manager shock* both increase future leveraged loan issuance as well as engage in greater polluting activities.

## 5.1 The Importance of Insurer and CLO Fund Manager Relationships

In this section, we begin by establishing the empirical fact that funds supplied by disclosing insurers depend significantly on pre-existing relationships with CLO fund managers. We follow the established literature on bank-firm relationships (see, for instance, Bharath et al. (2011)) to construct a measure of manager-insurer relationship. Specifically, we define a $K$-period relationship

between insurance company $i$ and fund manager $m$ as

$$\mathcal{R}^{K}_{i,m,t} = \frac{1}{K} \sum_{\tau=t-(K-1)}^{t} \frac{L_{i,m,\tau}}{\text{AUM}_{m,\tau}} \tag{10}$$

where $L_{i,m,\tau}$ is the total investment of insurer $i$ in the debt and equity tranches of funds managed by $m$ at year $\tau$, while $\text{AUM}_{m,\tau}$ is the total dollar value of the manager $m$'s corpus at time $\tau$. The ratio thus measures the relative importance of insurer $i$ as a source of funds. The insurer-manager relationship $\mathcal{R}^{K}_{i,m,t}$ is then defined as the average of this measure over the previous $K$ periods inclusive of the current year. Following the standard practice in the literature, we take $K = 5$. Panel A of Figure IA3 plots the evolution of insurer-manager relationships over the sample period. Note that the plot starts at 2005 since we use the initial period $[2000, 2004]$ to define the relationships. As documented in the plot, relationships become progressively stronger over the sample period, with a median insurer accounting for about 5% of the funds under a typical manager's control. However, there is strong dispersion in the data, as evident from the scatter plots of the relationship measure. For instance, the standard deviation of $\mathcal{R}_{i,m,t}$ over the sample period is 0.12, and the $90^{\text{th}}$ percentile value is 0.20. Panel B of Figure IA3 plots the number of relationship insurers per \$100 million of assets managed. On an average, a manager with \$100 million in AUM has relationships with around 3 insurers ($99^{\text{th}}$ percentile value is 28). The largest (top 1 percentile) asset managers can have relationships with around 400 insurers in our sample.

### 5.2 Tracing Out the Effect of Supervision on CLO Managers

We next explore whether managers with past relationships are more likely to receive capital from insurers once they start reporting their climate strategies in state filings. Bhardwaj et al. (2022) document the presence of significant stickiness in insurer-manager relationships. That is, an insurer who has invested with a manager before, is significantly more likely to invest in future funds launched by the same manager. We conjecture that capital flow into CLO markets after climate disclosure is sensitive to the stickiness of insurer-manager relationships. To test our hypothesis we consider the following difference-in-difference specification:

$$y_{i,f(m),t} = \alpha_i + \alpha_f + \alpha_t + \gamma_1 \text{Post}_{i,t} + \gamma_2 \mathcal{R}^{5}_{i,m,t-1} + \gamma_3 \mathcal{R}^{5}_{i,m,t-1} \times \text{Post}_{i,t} + \epsilon_{ift} \tag{11}$$

where $y_{i,f(m),t}$ is insurer $i$'s investment in fund $f$ launched by manager $m$ in year $t$. Here each observation is at an insurer-fund level. Suppose a total set of $\mathcal{I}$ insurers are active in the CLO

industry, and denote by $\mathcal{F}$ the total set of funds.[14] For every fund $f \in \mathcal{F}$ launched, we consider investments by any insurer $i \in \mathcal{I}$ in the debt or equity tranches of the fund at its inception. Thus, $y_{i,f(m),t}$ is either the total dollar value of insurer $i$'s investment in the fund $f$, or 0 otherwise.

Our key independent variable is $\text{Post}_{i,t}$ which assumes a value of one if the first reporting year for insurer $i$ is less than year $t$, and zero otherwise. We allow for a one year lag to mitigate any possibility of reverse bias.[15] To gauge the impact of past relationships in attracting new capital from insurers, especially in the post-disclosure period, we include the relationship variable $\mathcal{R}_{i,m,t-1}$, and its interaction with $\text{Post}_{i,t}$. We further include insurer ($\alpha_i$), fund ($\alpha_f$) and year ($\alpha_t$) fixed effects to absorb any unobservable characteristics from biasing our results. For instance, Fringuellotti and Santos (2021) document that some funds especially cater to insurance companies, and hence attract a larger insurer investment compared to an average fund. The inclusion of fund-fixed effects mitigate the impact of such unobservable factors.

Table 8 presents the results of specification (11), and provides evidence supporting our conjecture. In columns (1) and (2), we use a dummy version of the dependent variable $y_{i,f(m),t}$ which assumes a value one of the insurer $i$ invest any positive amount with the fund $f$, and zero otherwise. In column (1), we find that an insurer $i$ is 0.2 percentage point more likely to invest in a new fund $f$ during the reporting years. To put this into perspective, note that the unconditional investment probability is only 1 percent. Thus, an average insurer is 20 percent more likely invest with a CLO fund when reporting, than when they are not.

More importantly, as shown in column (2) of Table 8, the funds which are most likely to attract new investments from insurers are launched by managers with past relationships. To understand the magnitude and economic significance, consider a one standard deviation increase (.016) in the relationship measure $\mathcal{R}_{i,m,t-1}$. Our estimates indicate that the probability of insurer $i$ investing in a new fund $f$ launched by manager $m$ increases by 0.93 percentage points, or 93 percent relative to the unconditional mean.[16] The statistically significant and economically strong magnitude of $\gamma_2$ also

[14]The data considered here is essentially $\mathcal{I} \otimes \mathcal{F}$. We label an insurer $i \in \mathcal{I}$ as active if it has made at least one investment in any debt or equity tranche launched by any manager in the previous 5 years. Our estimates remain remarkably stable if we expand the set $\mathcal{I}$ to include all insurers instead.

[15]Suppose we observe a fund launch in the third month of year $t$, and the first disclosure date of the insurer also falls on year $t$. Note that disclosure dates and Schedule-D reporting do not provide exact dates, we may falsely classify this as a post-disclosure investment. By classifying the variable *Post* from the next year onward mitigates any possibility of erroneous assignment.

[16]In an unreported alternate specification, we replace the relationship variable $\mathcal{R}_{i,m,t}$ with the indicator variable $1_{\mathcal{R}_{i,m,t}}$. We find, similarly, that an insurer $i$ is 42 percent more likely to invest in the new fund $f$ launched by manager $m$ if it had a prior relationship.

captures the stickiness in insurer-manager relationships, and corroborates the finding in Bhardwaj et al. (2022).

In columns (4) through (6) of Table 8, we use the dollar value of tranches bought, conditional on investment, as the dependent variable. Looking at column (6), our estimates indicate that once insurers start reporting, they increase their investments in CLO tranches by $1.8 million, or about 25 percent relative to the unconditional average of $7.09 million. However, for relationship managers, this jump is significantly larger. Post disclosure, a similar one standard deviation increase in the relationship measure increases the insurer investment in a new fund by $8.4 million.[17]

The results presented in Section 5.1 document a pronounced increase in the probability of, and conditional capital flow into CLO fund managers from insurers who report their climate strategy. Furthermore, both these effects increase manifold if the said manager has an existing investing relationship with the insurer. Thus, a manager who is connected to a larger number of reporting insurers receives a considerably bigger cash inflow shock relative to one who have lesser number of such relationships. This provides us with important cross-sectional and time-series variation in manager exposures to insurer disclosure shocks. Formally, we define a manager shock as:

$$\lambda_{m,t} = \sum_{i \in \mathcal{I}} \frac{\mathcal{R}^5_{i,m,t}}{\sum_{i \in \mathcal{I}} \mathcal{R}^5_{i,m,t}} \times D_{i,t}, \tag{12}$$

where $\mathcal{R}_{i,m,t}$ is the manager-insurer relationship, as defined in equation 10, and $D_{i,t}$ is a dummy which equals one if the first disclosure year of insurer $i$ is $\tau \leq t$. We normalize the manager shock variable by the total size of insurer-manager relationships $\sum_{i \in \mathcal{I}} \mathcal{R}_{i,m,t}$ so that the manager shock $\lambda_{m,t}$ lies in the interval $[0, 1]$. The extremes correspond to the cases where either all or none of the insurers report in year $t$. Figure IA4 depicts the cross-sectional and time evolution of manager shocks in our sample.

We next explore whether an increased exposure to reporting insurers materially impact a manager's fund launches. In particular, we ask whether managers exposed to larger shocks launch more and/or bigger funds in the following year. Also, whether these funds have any differential characteristics compared to other funds launched by managers with smaller shocks. To do so, we run the following regression:

$$Y_{m,t} = \alpha_m + \delta_t + \pi \lambda_{m,t-1} + \nu_{m,t}, \tag{13}$$

where $\alpha_m$ and $\delta_t$ respectively denote the manager- and year-fixed effects, to absorb unobserved

[17] The t-test for the linear combination $\gamma_2 + \gamma_3 = 0$ is rejected at 1% confidence level with a t-statistic of 5.01.

managerial and aggregate factors which may influence new fund creations.

The results, reported in Table 9, document a positive and significant effect of manager shocks $\lambda_{m,t-1}$ on new fund creation. A one-standard-deviation increases fund launch probability next year by 11 percentage points, or by 47 percent relative to its unconditional mean of 0.23. This translates to one extra fund launch roughly every two years, as shown in column (2). The funds launched by managers receiving a larger shock are also considerably bigger. For instance, the estimate expressed in column (3) suggests that a one-standard deviation increase in manager shock increases the total size of funds launched the next year by $186 million - a 70 percent increase over the total corpus size of managers with no shock.

### 5.3 The Effect of Supervision on CLO's Firm Investments

The results reported in subsections 5.1 and 5 show that managers who are connected to insurers experience excess capital inflows when these insurance companies start reporting their climate strategy. In turn, these managers launch more, and larger funds. In this section, we focus on the firms that are selected when new funds are launched. We show that brown firms have a higher likelihood of being chosen, especially when they have been a part of the managers' portfolio before. Formally, we define a manager-firm relationship as

$$\mathcal{R}^5_{m,j,t} = \frac{1}{5}\sum_{s=t-4}^{t} \frac{L_{m,j,s}}{\sum_{m\in\mathcal{M}} L_{m,j,s}}, \tag{14}$$

where $L_{m,j,s}$ is the outstanding dollar amount of debt of firm $j$ held by CLO manager $m$ at time $s$, and $L_{j,s} := \sum_{m\in\mathcal{M}} L_{m,j,s}$ is the total outstanding debt of firm $j$ at time $s$. Thus, firm $j$ has a stronger (weaker) relationship with a manager $m$, if the manager held a larger (smaller) share of the total outstanding leveraged debt issued by the firm.

To understand which firms are chosen when managers launch new funds, we adopt an approach similar to the one used in specification (11). In particular, we focus on the fund portfolio in the first year of inception. Denote by $\mathcal{J}$ the set of firms which are being held actively by different managers at this time. Which of these firms are included in the portfolio of the new fund $f$ launched by the manager? Let $y_{f(m),j,t}$ be an indicator variable which assumes the value one if the firm $j \in \mathcal{J}$ is included in the formation-period portfolio of fund $f$ launched by manager $m$ at time $t$, and zero otherwise. We use the following specification:

$$y_{f(m),j,t} = \alpha_f + \alpha_j + \alpha_t + \gamma_1 \mathrm{CC}_{j,t-1} + \gamma_2 \mathcal{R}^5_{m,j,t-1} + \gamma_3 \mathrm{CC}_{j,t-1} \times \mathcal{R}^5_{m,j,t-1} + \epsilon_{f(m),j,t}. \tag{15}$$

In equation (15), the coefficient $\gamma_2$ captures the stickiness of managerial portfolio choices. We also include the last observed climate sentiment measure ($\gamma_1$) of the firms to explore whether managers show any particular proclivity towards brown or green firms. Finally, we also include fund-fixed effects $\alpha_f$, firm-fixed effects $\alpha_j$, and fund-formation year fixed effects $\alpha_t$ to account fund or firm specific unobservables and aggregate factors.

The results from the estimation of equation (15) are reported in Table 10. In column (1) we focus only on the recent climate sentiment of the firm $j$. According to our estimates, a move from the $99^{\text{th}}$ percentile to $1^{\text{th}}$ percentile value (3.47 to $-0.76$) increases the probability of inclusion by 1.2 percentage points, or 18 percent relative to the unconditional mean. In column (2), we focus on the impact of previous investment relationships on inclusion probability in the new fund launched by the same manager. A one-standard-deviation increase in $\mathcal{R}^5_{m,j,t-1}$ increases the probability of selection by 41 percent. Note that in column (2) we include a stronger firm $\times$ year fixed effects, which controls for firm-level time varying factors orthogonal to firm-manager relationships which impacts firm inclusion probability.[18]

Finally, in column (3) we estimate the joint effect of firm's climate sentiment and past relationships on its probability of being included in the new fund. The baseline coefficient $\gamma_1$ is not estimated independently since it is subsumed by the use of firm $\times$ year fixed effects. The coefficient of the interaction term $\gamma_3$ is strongly significant and negative, implying managers load up on brown firms they have held before when choosing a portfolio for the new fund. Let us consider two simple examples. First, holding the past relationship estimate $\mathcal{R}^5_{m,j,t-1}$ constant, a move from the $99^{\text{th}}$ percentile to $1^{\text{th}}$ percentile value (3.47 to $-0.76$) increases the probability of inclusion by 86 percent relative to the unconditional mean. Second, if we add a one-standard-deviation increase in the manager-firm relationship measure, the total jump in firm inclusion probability is 121 percent.

Our results suggest a strong predisposition among fund managers towards brown firms which they have held before.[19] When combined with the results presented in subsection 5.3, this establishes a clear link of capital flow from insurers to firms. Insurance companies who report their climate

[18]Managerial preference for firms that were previously part of their portfolios has also been shown in Bhardwaj et al. (2022). They show that firms which have been held before by the manager are 7 times more likely to be selected in the new fund portfolio. The authors argue that this preference can be related to two primary sources : i) information acquisition costs about firm's debt and default probabilities, and ii) traditional banking relationship between the firms and the funds' underwriting bank. We also find a substantially higher probability, albeit a smaller magnitude compared to the other paper, of being included. The key difference in the results stems from the sets of firms considered. We focus only on public firms since climate data is only available for these firms. While we are agnostic about the exact reason of preferences for existing firms, we note that both these channels are likely to considerably weaker for public firms.

[19]Table IA3 provides further evidence consistent with this notion.

strategy direct capital towards managers who have an existing relationship with them. These managers in turn purchase a larger quantity of loans from that which they have a relationship with. Combining these two links, we define a firm level shock, similar to the manager-level shocks defined in subsection 5, as

$$\theta_{jt} = \sum_{m \in \mathcal{M}} \frac{\mathcal{R}^5_{m,j,t}}{\sum_{m \in \mathcal{M}} \mathcal{R}^5_{m,j,t}} \times \lambda_{m,t} \tag{16}$$

where $\mathcal{R}^5_{m,j,t}$ is the firm-manager relationship as defined in equation (14), and $\lambda_{m,t}$ is the manager shock defined in equation (12). The ratio on the right-hand side of the above equation reflects the strength of the manager-firm relationship. A larger value implies that the manager holds a higher share of the total outstanding debt issued by the firm. Figure IA5 depicts the distribution of firm shocks over the period 2012 through 2020. Also note that there is significant cross-sectional variation in the distribution of firm shocks, which we exploit in the next section.

### 5.4 The Effect of Supervision on Underlying Firms' Decisions

We have seen that climate supervision results in capital flow from insurance companies to CLO-fund managers, who respond by launching new funds. New fund launches increase demand for leveraged loans. In this section, we explore whether firms with prior manager relationships respond to this excess demand by issuing new loans. To conduct this test, we hand-match the company names in the LPC Dealscan database to the issuer names in the CLO database and Insurance Schedule-D files, and create an annual panel of loan issuances over the sample period. Following standard definitions (see Yago and McCarthy, 2004), we define a leveraged loan as a syndicated, secured term loan with a spread of 125 basis points over the base rate, typically LIBOR.

To identify the impact of firm shocks on future loan issuances, we use the following specification:

$$y_{j,t} = \alpha_j + \alpha_t + \pi \theta_{j,t-1} + \varepsilon_{jt}, \tag{17}$$

where $y_{j,t}$ is an indicator variable which assumes a value one if the firm $j$ issues a leveraged loan in year $t$, and zero otherwise. The results from this estimation is reported in Table 11. The coefficient $\pi$ is positive, strongly significant, and economically large, delineating the importance of insurance shocks for future loan market access for firms. A one-standard-deviation (0.325) increase in the firm-level shock $\theta_{j,t-1}$ increases the probability of loan issuance by 1.95 percentage points, or by 28 percent relative to the unconditional mean. In column (2) we replace the year fixed effect $\alpha_t$ by a stronger state-industry-year fixed effects. In this case the coefficient $\pi$ is estimated off the cross-sectional differences in insurer exposures of firms headquartered in the same state and operating

in the same SIC 4 digit industry. In column (3) we add an indicator variable denoting the firm's previous bond market access.[20] Intuitively, the probability of loan issuances are higher for firms which did not historically have access to the bond market. However, the interaction with the firm shock variable is not significantly different from zero. This implies that the impact of insurer exposure shocks works in general, regardless of access to other credit markets.

The firm-shock $\theta_{jt}$ captures the exposures of firms to insurance companies through the CLO markets. If our channel is valid, then we should expect no differential impact for other forms of loans not held by the CLO funds. Column (4) addresses this issue by exploring the impact of firm shocks on revolving credit-line issuances. These loans remain in the books of banks and never sold off in the secondary market. As expected, we find no impact on the probability of credit line issuance.

Next, we examine how the firm-shock $\theta_{jt}$ affects future climate-related activity of the firm in Table 12. We estimate the following:

$$y_{j,t} = \alpha_j + \alpha_t + \pi\theta_{j,t-1} + \epsilon_{jt}, \tag{18}$$

where the dependent variable $y_{j,t}$ is the total emissions of firm $j$ during the period $t$ scaled by total assets of the firm. The key explanatory variable is the firm shock $\theta_{j,t-1}$ as defined in equation (16). To control for natural variation in the pollution-activity of different firms and facilities, we include firm-facility; to account for time-varying trends in pollution activity across sectors, we also include industry-year fixed effects. For data aggregated at facility-chemical-year level, we use a stronger facility-chemical and chemical-industry-year fixed effects. Firm controls include Lagged Assets, Tobin's Q, Profitability, and market leverage. Standard errors, clustered at the facility level, are reported in parentheses.

Starting with columns (1) to (3) of Table 12, which show results at the facility-chemical-year level, we consistently find that firms that receive a positive shock are subsequently more likely to engage in future polluting activity. Using column three as our benchmark, as it has the most stringent controls, a one-standard-deviation (0.32) increase in the firm-level shock $\theta_{j,t-1}$ increases total emissions by 12.87 pounds per dollar of total assets. Compared to the unconditional average of 76.81 pounds, this represents a significant 17% increase. Next, moving to columns (4) and (5) of Table 12, which are aggregated at the facility-year level, we find very similar results. Looking at column (5), which has the most stringent controls, we find that a one-standard-deviation (0.32)

[20]To perform this exercise, we using the Dealscan CompanyID-CUSIP linking file provided by Refinitiv, enabling us to cross-match firms in the Dealscan and in the Mergent FISD issuance files. A firm is said to have a bond market access if it has ever issued a bond in the past.

increase in the firm-level shock $\theta_{j,t-1}$ increases total emissions by 45.17 pounds per dollar of total assets, or 13% increase from the unconditional mean.

As a robustness check, we finally use a different, news-based measure of pollution and find consistent results in Table IA4. To confirm the positive effect of the firm-level shock on future polluting activity, we report the estimates from the following specification:

$$y_{j,t\to t+4} = \alpha_j + \alpha_t + \pi\theta_{j,t-1} + \epsilon_{jt}, \tag{19}$$

where the dependent variable $y_{j,t\to t+4}$ is the relevant environmental impact of firm $j$ over the period $[t, t+4]$. In column (1) the key dependent variable is the number of negative news incidents involving the firm over the interval as reported by Reprisk. If an incident covers multiple firms, then they are labeled as "unsharp" incidents. In column (2) we remove all such cases, and focus only on the news generated by the firm as a standalone entity. In columns (3) - (5), we focus on the pollution incidents. The key corresponding incidents reported by Reprisk are : "climate GHG pollution" "local pollution", "waste issues", "airborne pollutants", "forest burning", "greenhouse gas emissions", and "wastewater management". Each incident is further categorized into 3 "severity" and 3 "reach" levels. In column (3) we focus on the raw pollution variable, while in columns (4) and (5), we weight them according to their severity and reach respectively. Finally in the last 3 columns, we focus on the environmental impact of these polluting incidents. The corresponding Reprisk variables we focus on here are : "impacts on communities", "impacts on landscapes", "health environment issues", "violation of international standards", "violation of national legislation", "waste issues", "economic impact", "health impact", and "water scarcity". We again report the raw numbers, as well as numbers weighted by their severity and reach. The key explanatory variable is the firm shock $\theta_{j,t-1}$ as defined in equation (16). Same as before, we include firm, and year (or state-industry-year) fixed effects and cluster standard errors at the firm level.

Starting with columns (1) and (2) of Table IA4 , we see that firms which receive more capital from the CLO market following increased insurance disclosure, are associated with more negative news in the following year. Next, in columns (3) through (5), we see that the firm receives more negative press on pollution-related topics. Finally, in columns (6) through (8), we see that these firms are involved more with news that have negative impacts, including items such as "impacts on communities", "impacts on landscapes", and "health environment issues".

Overall, our evidence is consistent with CLO firms having a negative impact on the environment following an increased ability to receive financing via the CLO market. CLO lenders may care less about polluting activity than bond lenders, especially if securitization makes it harder for end

investors to monitor each of the underlying firms. Confirming this hypothesis, Table A6 finds that new bond issuances have a much higher coupon if their firms' climate sentiment is low, consistent with heavier polluting activity–in contrast, Table A7 shows that there is no such penalty when a firm borrows new capital via CLOs. These results are striking, because climate supervision appears to unintentionally enable some firms to pollute more, achieving the opposite outcome of its original intent.

# 6 Conclusion

In this paper, we study a climate supervision shock (the NAIC's Climate Risk Disclosure Survey) and its effects on insurer capital flows into shadow markets such as CLOs. Crucial to our analysis, we undertake a hand-matching exercise in order to disaggregate CLO investments from the Insurer×CLO tranche level to the Insurer×Firm level, enabling us to understand the ultimate impact of disclosure on firms' climate-related decisions. We find evidence that insurers view climate supervision as a constraint on the choice of their direct holdings, leading them to seek shadow market investments to recoup lost yields. Once required to disclose, insurers purchase $2 million more CLOs and they hold $4.5 million more in CLOs on average. Aggregating up across all years and insurers in our sample, this sums to 23.3 billion of addition flow into CLOs—accounting for a striking 20.4% of all growth in CLO investments between 2010 and 2019. Furthermore, at the underlying firm level, we find that disclosure is ultimately associated with higher volumes of future leveraged loans as well as increased future polluting activities.

Our paper has three main contributions. First, we show that insurance companies, through their increasing investments in CLO markets, play a significant and perhaps even increasing role in the financing of brown firms. Second, we also show that insurance supervision, intended to mitigate climate risk, can unintentionally support climate risk taking in the subset of firms that borrow from shadow markets. Finally, we add to a growing climate disclosure literature by finding evidence of spillovers into opaque funding markets. Overall, our findings have specific policy recommendations–tailoring portfolio disclosure to include details of securitized assets may be beneficial. Given the growing investment of insurance companies in CLOs, insurance regulators may need to pay attention to the Green Paradox (Jensen et al., 2015) while designing climate supervision policies.

While we have made one of the first efforts to understand how increased scrutiny affect insurers' behavior in their direct versus indirect portfolios, many more interesting research questions remain. For example, does climate risk naturally present in insurers' liabilities interact with supervision when it comes to their risk-taking activities in their assets? How does a squeeze in financial constraints,

such as a large payout by a P&C company after an unexpected natural disaster, affect this spillover? In addition, how do these supervisory pressures affect other, less traditional parts of the insurers' portfolio such as real estate, which carries its own unique set of climate risks?

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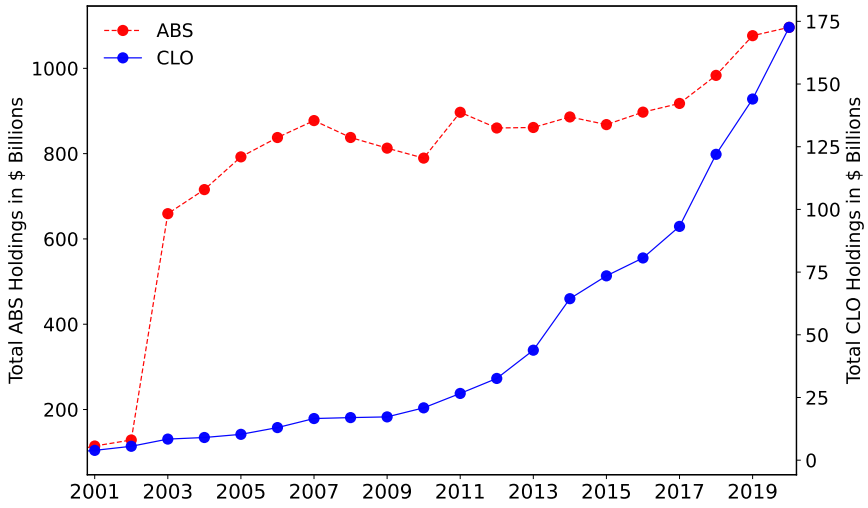
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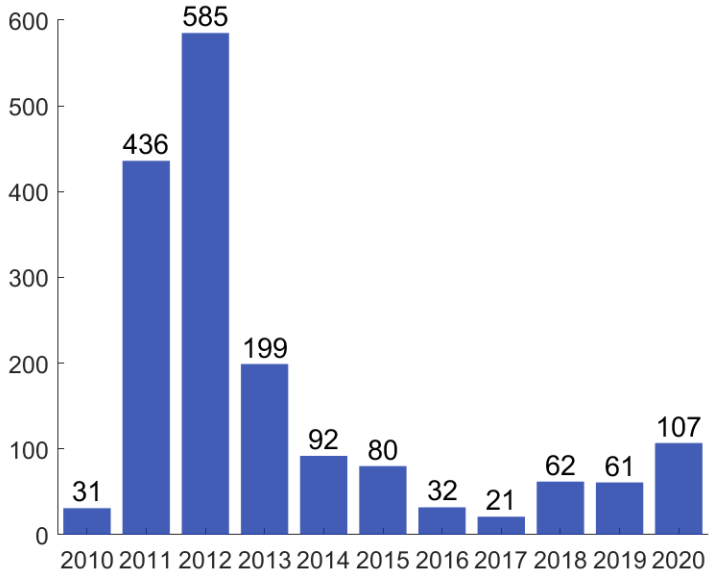
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Figure 1: Insurers' Growing Investments into Shadow Markets



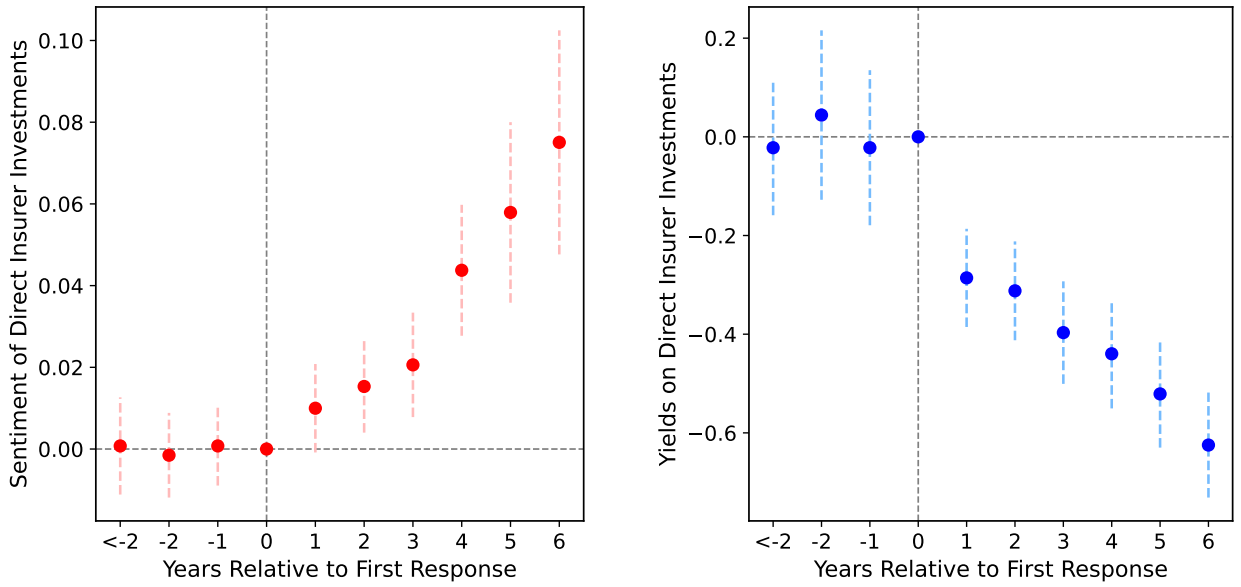

*Note:* This figure shows the rapid rise in insurers' aggregate holdings of Asset backed securities (in red, left hand axis), and as a subset, tranches of Collateralized Loan Obligations (in blue, right hand axis) between 2001 and 2019. Data on CLO tranches comes from Acuris Creditflux, and data on insurance portfolios come from NAIC Schedule D files.

Figure 2: First-Time Insurer Responses to Climate Supervision




*Notes:* The figure above plots a histogram of the first year in which an insurance company submitted a response to the NAIC's climate surveys. The data comes from NAIC climate disclosure results published online and shared with us.

Figure 3: Sentiment and Yields of Direct Holdings Around Disclosure




(a) Weighted Climate Sentiment

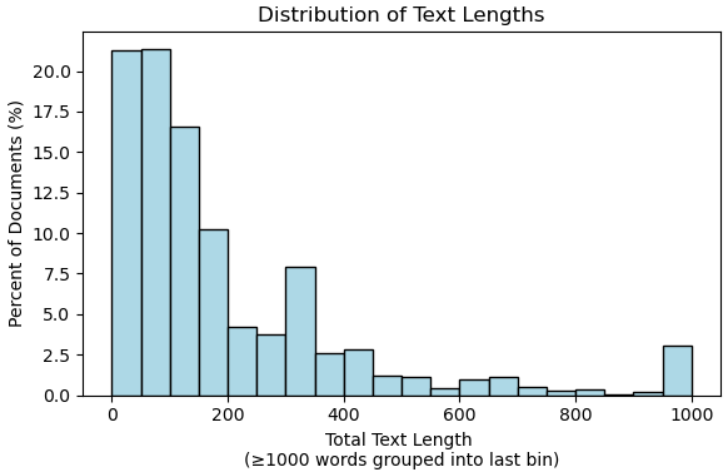
(b) Yields on Direct Investments

*Note:* This figure plots the weighted average climate sentiment of the insurance companies around the disclosure date. Specifically, it displays the $\beta_t$ coefficients from the following regression:

$$y_{it} = \alpha_{ie} + \alpha_{et} + \sum \beta_t D_{i,t} + \varepsilon_{it}$$

where $y_{it}$ is the holdings-weighted climate sentiment (in Panel A) and holdings-weighted effective yield on the direct bond holdings (in Panel B) of the insurance company $i$ in year $t$. We construct a stacked panel spanning 13 years around the event date, with $e \in (2010, 2019)$ representing the year in which an insurer first disclosed its climate strategy. For each event year $e$, treated insurers are those that first disclose their investment strategy related to climate risk in year $e$, while control insurers are those that either begin disclosing in year $e + 7$ or never disclose during the sample period. This stacked specification mitigates timing biases introduced by staggered treatment adoption. The dummy variable $D_{i,t}$ equals one for each year $t$ relative to event year $e$ for treated insurers, and zero otherwise. We focus on the window $[e-3, e+6]$ around the first disclosure year. The regression includes event-insurer fixed effects ($\alpha_{ie}$) and event-time fixed effects ($\alpha_{et}$). Standard errors are clustered at the insurer level.

Figure 4: Word Count and Most Common Verbs in Insurer Disclosures




(a) Histogram of word count




(b) Top 50 Verbs

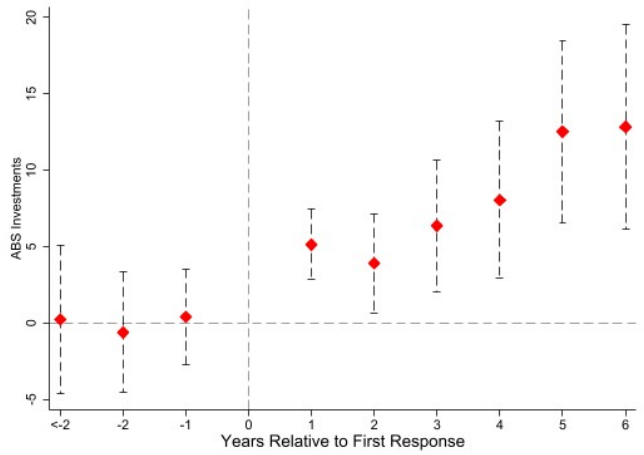



(c) Top Action Verbs

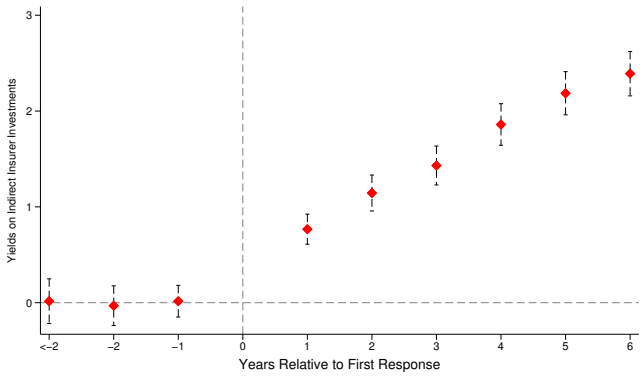


*Notes:* The figures above demonstrate various aspects of insurers' verbal response to question 5 of NAIC's climate disclosure survey from 2023-2022. Panel A plots a histogram of the total word count per response, Panel B plots the top 50 verbs, and Panel C plots the the subset of action-based verbs only.

Figure 5: Insurer Investments in ABS following Climate Supervision




(a) Total ABS Investments



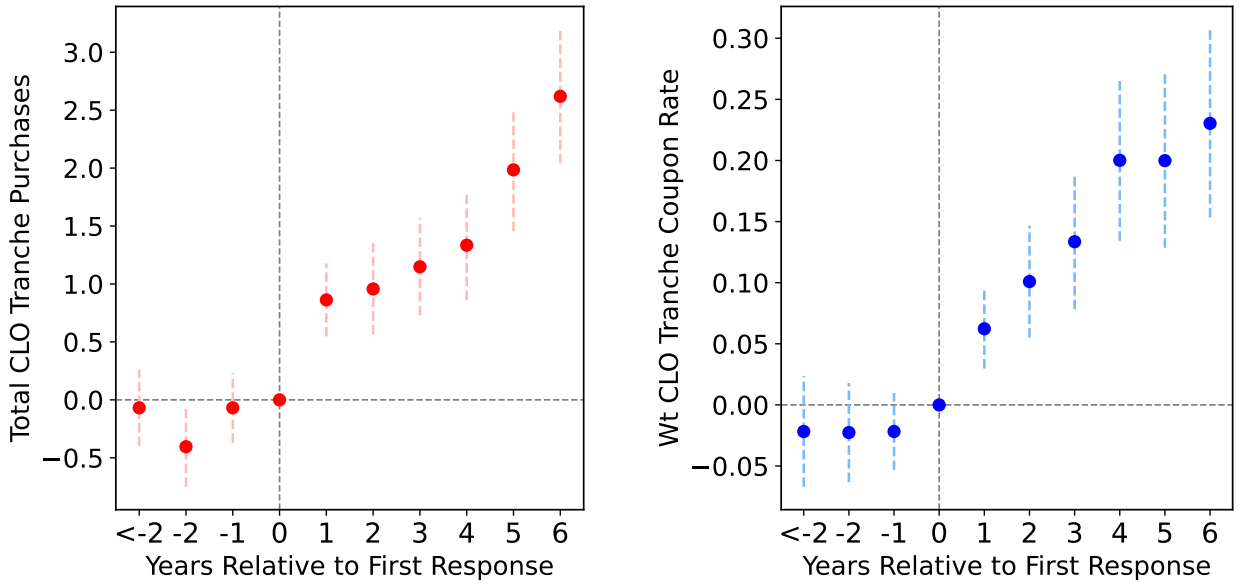

(b) Weighted ABS Yields

*Notes:* This figure plots ABS fund investments of insurance companies around the disclosure date. Specifically, it displays the $\beta_t$ coefficients from the following regression:

$$y_{it} = \alpha_{ie} + \alpha_{et} + \sum \beta_t D_{i,t} + \varepsilon_{it}$$

where $y_{it}$ denotes the total investment (in \$ millions) of insurance company $i$ in ABS tranches in year $t$ (Panel A) or the average value-weighted yield on all ABS investments (Panel B). We construct a stacked panel spanning 13 years around the event date, with $e \in (2010, 2019)$ representing the year in which an insurer first disclosed its climate strategy. For each event year $e$, treated insurers are those that first disclose their investment strategy related to climate risk in year $e$, while control insurers are those that either begin disclosing in year $e + 7$ or never disclose during the sample period. This stacked specification mitigates timing biases introduced by staggered treatment adoption. The dummy variable $D_{i,t}$ equals one for each year $t$ relative to event year $e$ for treated insurers, and zero otherwise. We focus on the window $[e-3, e+6]$ around the first disclosure year. The regression includes event-insurer fixed effects ($\alpha_{ie}$) and event-time fixed effects ($\alpha_{et}$). Standard errors are clustered at the insurer level.

Figure 6: Insurance Companies Investments in CLO funds following Disclosure




(a) Total Tranches Purchased

(b) Coupons of Tranches Purchased

This figure plots CLO fund investments of insurance companies around the disclosure date, and the total coupons offered by those tranches. Specifically, it displays the $\beta_t$ coefficients from the following regression:

$$y_{it} = \alpha_{ie} + \alpha_{et} + \sum \beta_t D_{i,t} + \varepsilon_{it}$$

where $y_{it}$ denotes either the total investment (in \$ millions) of insurance company $i$ in CLO tranches in year $t$ as in Panel A, or the weighted average coupon rate offered by the CLO tranches held by insurer $i$ in year $t$ in Panel B. We construct a stacked panel spanning 13 years around the event date, with $e \in (2010, 2019)$ representing the year in which an insurer first disclosed its climate strategy. For each event year $e$, treated insurers are those that first disclose their investment strategy related to climate risk in year $e$, while control insurers are those that either begin disclosing in year $e + 7$ or never disclose during the sample period. This stacked specification mitigates timing biases introduced by staggered treatment adoption. The dummy variable $D_{i,t}$ equals one for each year $t$ relative to event year $e$ for treated insurers, and zero otherwise. We focus on the window $[e-3, e+6]$ around the first disclosure year. The regression includes event-insurer fixed effects ($\alpha_{ie}$) and event-time fixed effects ($\alpha_{et}$). Standard errors are clustered at the insurer level.

Table 1: Summary Statistics

| | Mean | Std. Dev. | P5 | Median | P95 |
|---|---|---|---|---|---|
| **Panel A : Insurers** | | | | | |
| Insurer Holdings Per Firm ($, '0,000s) | 58.54 | 247.17 | 0.00 | 0.00 | 300.00 |
| Climate Sentiment (Direct holdings) | 0.22 | 0.80 | -0.24 | 0.02 | 1.16 |
| Insurer Wt. Avg Climate Sentiment | 0.22 | 0.35 | -0.05 | 0.14 | 0.77 |
| Insurer Wt. Avg Climate Exposure | 1.07 | 1.07 | 0.16 | 0.80 | 2.81 |
| Insurance ABS Holdings ($, Millions) | 140.64 | 1356.70 | 0.00 | 0.34 | 294.41 |
| Insurance CLO Holdings ($, Millions) | 6.23 | 31.79 | 0.00 | 0.00 | 17.43 |
| ABS Tranche Purchase ($, Millions) | 44.26 | 451.75 | 0.00 | 0.00 | 95.73 |
| CLO Tranche Purchase ($, Millions) | 1.94 | 10.76 | 0.00 | 0.00 | 5.00 |
| **Panel B : CLO Funds** | | | | | |
| CLO Fund Balance ($, Millions) | 724.32 | 380.72 | 355.50 | 607.00 | 1431.05 |
| Fund Leverage (%) | 92.58 | 6.14 | 87.11 | 92.00 | 100.00 |
| No. of Portfolio Firms | 481.64 | 219.37 | 186.00 | 449.00 | 883.00 |
| Fund Holdings ($, '0,000s) | 193.99 | 156.64 | 28.63 | 151.82 | 498.12 |
| Avg. Loan Maturity | 5.20 | 1.51 | 2.50 | 5.50 | 7.00 |
| Avg. Loan. Rating Score | 11.22 | 1.75 | 9.00 | 11.00 | 14.00 |
| **Panel C : Fund Managers** | | | | | |
| Prob. CLO Tranche Investment | 0.56 | 7.48 | 0.00 | 0.00 | 0.00 |
| Prob. (Insurer-Manager Relationship $\mathcal{R}^5_{i,m,t} > 0$) | 0.05 | 0.21 | 0.00 | 0.00 | 0.00 |
| $\mathcal{R}^5_{i,m,t}$ (Conditional on Relationship) | 8.78 | 21.52 | 0.08 | 1.37 | 45.30 |
| Manager Shock $\lambda_{m,t}$ | 0.19 | 0.37 | 0.00 | 0.00 | 0.97 |
| Prob. Fund Launch | 0.23 | 0.42 | 0.00 | 0.00 | 1.00 |
| No. of Annual Fund Launches | 0.56 | 1.40 | 0.00 | 0.00 | 3.00 |
| Launch Size ($ Millions) | 250.93 | 635.49 | 0.00 | 0.00 | 1625.50 |
| **Panel D : Firm Issuance** | | | | | |
| Prob. Firm Included in Fund Portfolio | 0.06 | 0.24 | 0.00 | 0.00 | 1.00 |
| Prob. of Manager-Firm Relationship ($\mathcal{R}^5_{m,j,t} > 0$) | 0.12 | 0.32 | 0.00 | 0.00 | 1.00 |
| $\mathcal{R}^5_{m,j,t}$ (Conditional on Relationship) | 8.11 | 16.08 | 0.34 | 3.15 | 33.09 |
| Firm Shock $\theta_{j,t}$ | 0.14 | 0.32 | 0.00 | 0.00 | 0.93 |
| Firm Issued Leveraged Loan Dummy | 0.07 | 0.26 | 0.00 | 0.00 | 1.00 |
| Loan Size ($ Millions) | 66.56 | 515.71 | 0.00 | 0.00 | 250.00 |
| All-in-Drawn Spread (bps) | 383.88 | 161.60 | 161.84 | 374.07 | 675.00 |
| Firm Issued Revolver Loan Dummy | 0.07 | 0.25 | 0.00 | 0.00 | 1.00 |
| **Panel E : Firm Emissions** | | | | | |
| Total Emissions in 000 lbs. (Per Chemical) | 221.12 | 894.57 | 0.00 | 9.23 | 833.11 |
| Number of Chemicals Released | 21.42 | 32.77 | 1.00 | 11.00 | 80.00 |
| Scaled Emissions (Per Chemical over Lagged Asset) | 70.82 | 784.26 | 0.00 | 0.37 | 133.09 |
| Total Emissions in 000 lbs. (Per Facility) | 1273.70 | 4125.79 | 0.00 | 28.64 | 8442.97 |
| Scaled Emissions (Per Facility over Lagged Asset) | 354.67 | 2237.53 | 0.00 | 1.64 | 943.44 |

*Notes:* This table summarizes the summary statistics for the five main datasets in our sample. Insurance holdings data comes from NAIC schedule D, disclosure dates come from the published NAIC climate surveys, CLO data comes from Acuris Creditflux, climate sentiment measures come from Sautner et al. (2023), and emissions data comes from the U.S. Environmental Protection Agency (EPA). Sample period runs from 2001 to 2020.

Table 2: Effect of Supervision on Insurer Climate Exposures and Yields

| **Panel A: All Insurers** | | | |
|---|---|---|---|
| | (1)<br>Climate Exposure | (2)<br>Climate Sentiment | (3)<br>Direct Yield |
| Treatment × Post | 0.076*** | 0.016*** | -0.128*** |
| | (0.019) | (0.005) | (0.038) |
| Obs | 367,645 | 367,645 | 373,548 |
| $R^2$ | 0.52 | 0.46 | 0.69 |
| $\bar{y}$ | .83 | .17 | 4.12 |
| Event-Insurer FE | Yes | Yes | Yes |
| Event-Year FE | Yes | Yes | Yes |
| Size Decile-Year FE | Yes | Yes | Yes |
| **Panel B: Excluding Early Filers** | | | |
| | (1)<br>Climate Exposure | (2)<br>Climate Sentiment | (3)<br>Direct Yield |
| Treatment × Post | 0.082*** | 0.016** | -0.112*** |
| | (0.029) | (0.008) | (0.043) |
| Obs | 251,131 | 251,131 | 251,105 |
| $R^2$ | 0.51 | 0.46 | 0.70 |
| $\bar{y}$ | .82 | .16 | 4.09 |
| Event-Insurer FE | Yes | Yes | Yes |
| Event-Year FE | Yes | Yes | Yes |
| Size Decile-Year FE | Yes | Yes | Yes |

*Notes:* This table reports the results from the following specification:

$$y_{it} = \alpha_{ie} + \alpha_{et} + \beta \text{Treated Insurer}_{ie} \times \text{Post}_{et} + \varepsilon,$$

where $y_{it}$ is the holdings-weighted climate exposure (Column 1), or Climate Sentiment (Column 2), or effective yield on the direct holdings (Column 3) of insurer $i$ in year $t$. Direct holdings include all bonds reported by insurers in NAIC Schedule D filings, excluding government and munis. We construct a stacked difference-in-differences (DID) framework by considering all disclosure events $e \in [2010, 2019]$. For each event year $e$, treated insurers are those that first disclose their investment strategy related to climate risk in year $e$, while control insurers are those that either begin disclosing in year $e + 7$ or never disclose during the sample period. The stacked specification addresses timing biases inherent in staggered treatment adoption. The indicator variable *Post* equals one for years $t$ after the event year $e$, and zero otherwise. The regression includes event-insurer fixed effects ($\alpha_{ie}$) and event-time fixed effects ($\alpha_{et}$). Furthermore, to account for differences in portfolio allocations and climate exposures stemming from size differences of insurers, we include Size Decile × Year FE in all our specifications. To calculate the size deciles, we use the total assets of insurance companies from 2009 filings, before the first disclosure shock. Standard errors are clustered at the insurer level.

Table 3: Link between Textual Commitment and Insurers' Yields and Climate Exposures

| | (1) Climate Sentiment | (2) Climate Exposure | (3) Direct Yield |
|---|---|---|---|
| Treatment × Post × *Text Length* | 0.012*** | 0.003*** | -0.007* |
| | (0.002) | (0.001) | (0.004) |
| Obs | 367,645 | 367,645 | 373,548 |
| $R^2$ | 0.52 | 0.46 | 0.69 |
| Treatment × Post × *Action Verb Count* | 0.015*** | 0.004*** | -0.009*** |
| | (0.003) | (0.001) | (0.003) |
| Obs | 367,645 | 367,645 | 373,548 |
| $R^2$ | 0.52 | 0.46 | 0.69 |
| Treatment × Post × *Action Verb Intensity* | 0.002*** | 0.001*** | -0.003*** |
| | (0.001) | (0.000) | (0.001) |
| Obs | 367,645 | 367,645 | 373,548 |
| $R^2$ | 0.52 | 0.46 | 0.69 |
| $\overline{y}$ | .83 | .17 | 4.12 |
| Event-Insurer FE | Yes | Yes | Yes |
| Event-Year FE | Yes | Yes | Yes |
| Size Decile-Year FE | Yes | Yes | Yes |

*Notes:* This table reports the results from the following specifications:

$$y_{it} = \alpha_{ie} + \alpha_{et} + \beta \text{Treated Insurer}_{ie} \times \text{Post}_{et} + \beta \text{Treated Insurer}_{ie} \times \text{Post}_{et} \times TextualCommitment + \varepsilon,$$

where $y_{it}$ is the holdings-weighted climate sentiment (Column 1), or Climate Exposure (Column 2), or effective yield on the direct holdings (Column 3) of insurer $i$ in year $t$. We report the triple interaction term, where we interact treatment with a textual measure of commitment from the insurers: textual length (panel A), number of action-based verbs (panel B), or "action verb intensity," defined as the number of action verbs divided by text length (panel C). We construct a stacked difference-in-differences (DID) framework by considering all disclosure events $e \in [2010, 2019]$. For each event year $e$, treated insurers are those that first disclose their investment strategy related to climate risk in year $e$, while control insurers are those that either begin disclosing in year $e + 7$ or never disclose during the sample period. The stacked specification addresses timing biases inherent in staggered treatment adoption. The indicator variable *Post* equals one for years $t$ after the event year $e$, and zero otherwise. We focus on a symmetric window around the event year, $[e-6, e+6]$. The regression includes event-insurer fixed effects ($\alpha_{ie}$) and event-time fixed effects ($\alpha_{et}$). Standard errors are clustered at the insurer level.

Table 4: Effect of Climate Supervision on Insurers' CLO Investments

**Panel A: All Insurers**

| | Total Balance in $ Millions | | Scaled Percentage of Portfolio | | Spread |
|---|---|---|---|---|---|
| | Holdings | Purchase | Holdings | Purchase | Holdings |
| | (1) | (2) | (3) | (4) | (5) |
| Treatment × Post | 1.456** | 0.758*** | 0.123*** | 0.044*** | 0.136*** |
| | (0.567) | (0.226) | (0.040) | (0.015) | (0.037) |
| Obs | 158,121 | 158,121 | 158,121 | 158,121 | 158,121 |
| $R^2$ | 0.71 | 0.57 | 0.51 | 0.36 | 0.52 |
| $\bar{y}$ | 2.24 | .77 | .03 | .01 | .16 |
| Event-Insurer FE | Yes | Yes | Yes | Yes | Yes |
| Event-Year FE | Yes | Yes | Yes | Yes | Yes |
| Size Decile-Year FE | Yes | Yes | Yes | Yes | Yes |

**Panel B: Excluding Early Filers**

| | Total Balance in $ Millions | | Scaled Percentage of Portfolio | | Spread |
|---|---|---|---|---|---|
| | Holdings | Purchase | Holdings | Purchase | Holdings |
| | (1) | (2) | (3) | (4) | (5) |
| Treatment × Post | 2.543*** | 0.969*** | 0.301*** | 0.105*** | 0.152*** |
| | (0.652) | (0.247) | (0.106) | (0.041) | (0.041) |
| Obs | 100,017 | 100,017 | 100,017 | 100,017 | 100,017 |
| $R^2$ | 0.67 | 0.57 | 0.55 | 0.38 | 0.54 |
| $\bar{y}$ | 1.99 | .71 | .04 | .02 | .19 |
| Event-Insurer FE | Yes | Yes | Yes | Yes | Yes |
| Event-Year FE | Yes | Yes | Yes | Yes | Yes |
| Size Decile-Year FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* This table reports the results from the following specification:

$$y_{it} = \alpha_{ie} + \alpha_{et} + \beta \text{Treated Insurer}_{ie} \times \text{Post}_{et} + \varepsilon$$

Column (1) uses the total dollar value of total outstanding CLO tranches as the dependent variable. Column (2) uses the dollar value of newly acquired CLO tranches. Finally, Column (3) uses the value weighted average coupon offered by the outstanding CLO tranches. We construct a stacked difference-in-differences (DID) framework by considering all disclosure events $e \in [2010, 2019]$. For each event year $e$, treated insurers are those that first disclose their investment strategy related to climate risk in year $e$, while control insurers are those that either begin disclosing in year $e + 7$ or never disclose during the sample period. The stacked specification addresses timing biases inherent in staggered treatment adoption. The indicator variable *Post* equals one for years $t$ after the event year $e$, and zero otherwise. The regression includes event-insurer fixed effects ($\alpha_{ie}$) and event-time fixed effects ($\alpha_{et}$). Furthermore, to account for differences in portfolio allocations and climate exposures stemming from size differences of insurers, we include Size Decile × Year FE in all our specifications. To calculate the size deciles, we use the total assets of insurance companies from 2009 filings, before the first disclosure shock. Standard errors are clustered at the insurer level.

Table 5: Effect of RBC Constraints on Insurers' CLO Investments

| | All Insurers | | Except Initial Filers | |
|---|---|---|---|---|
| | Holdings (1) | Investments (2) | Holdings (3) | Investments (4) |
| Low RBC | -0.281 | -0.139* | -0.287 | -0.127 |
| | (0.223) | (0.083) | (0.275) | (0.098) |
| Treated Insurers × Post | 2.945*** | 1.443*** | 2.026*** | 0.828*** |
| | (0.430) | (0.181) | (0.561) | (0.250) |
| Low RBC × Post | 0.993*** | 0.402*** | 1.031*** | 0.437*** |
| | (0.279) | (0.104) | (0.333) | (0.118) |
| Treated Insurers × Low RBC | -0.749 | -0.473** | 0.424 | 0.055 |
| | (0.557) | (0.237) | (0.991) | (0.381) |
| Treated Insurers × Post × Low RBC | 2.495*** | 1.162*** | 2.514** | 0.865** |
| | (0.718) | (0.290) | (1.145) | (0.435) |
| Obs | 171,366 | 171,366 | 109,493 | 109,493 |
| $R^2$ | 0.69 | 0.54 | 0.66 | 0.56 |
| $\bar{y}$ | 2.24 | .77 | 2.03 | .73 |
| Event-Insurer FE | Yes | Yes | Yes | Yes |
| Event-Year FE | Yes | Yes | Yes | Yes |
| Size Decile-Year FE | Yes | Yes | Yes | Yes |

*Notes:* This table reports the results from the following specification:

$$\begin{aligned} y_{ijt} = \alpha_{ie} + \alpha_{et} + \beta\,\text{Treated}_{ie} \times \text{Post}_{et} + \gamma\,\text{Low RBC}_{ie} + \delta\,\text{Low RBC}_{ie} \times \text{Post}_{et} \\ + \pi\,\text{Treated}_{ie} \times \text{Low RBC}_{ie} + \lambda\,\text{Treated}_{ie} \times \text{Post}_{et} \\ + \theta\,\text{Treated}_{ie} \times \text{Post}_{et} \times \text{Low RBC}_{ie} + \varepsilon_{ijt}. \end{aligned}$$

Columns (1) and (2) use the total dollar value of newly acquired CLO tranches as the dependent variable. Columns (3) and (4) use the dollar value of outstanding CLO investments held by insurance companies. We construct a stacked difference-in-differences (DID) framework by considering all disclosure events $e \in [2010, 2019]$. For each event year $e$, treated insurers are those that first disclose their investment strategy related to climate risk in year $e$, while control insurers are those that either begin disclosing in year $e+7$ or never disclose during the sample period. The stacked specification addresses timing biases inherent in staggered treatment adoption. The indicator variable *Post* equals one for years $t$ after the event year $e$, and zero otherwise. We focus on a symmetric window around the event year, $[e-6, e+6]$. The indicator variable Low $\text{RBC}_{it}$ equals one if insurer $i$'s risk-based capital (RBC), expressed as a percentage of total assets, is below the median in year $t$, and zero otherwise. The regression includes event-insurer fixed effects ($\alpha_{ie}$) and event-time fixed effects ($\alpha_{et}$). Standard errors are clustered at the insurer level.

Table 6: Heterogeneity: Results Unlikely to be Driven by Natural Disaster Liability Risks

| | CLO Investments ($ Millions) | | CLO Holdings ($ Millions) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treatment × Post | 0.444** | 1.563*** | 1.055** | 2.463** |
| | (0.192) | (0.482) | (0.488) | (1.223) |
| Post × Life | -0.209 | | -0.394 | |
| | (0.150) | | (0.400) | |
| Treatment × Post × Life | 1.172** | | 1.460 | |
| | (0.459) | | (1.186) | |
| Post × P&C | | 0.200 | | 0.416 |
| | | (0.138) | | (0.367) |
| Treatment × Post × P&C | | -1.120** | | -1.416 |
| | | (0.444) | | (1.145) |
| Obs | 156,076 | 156,076 | 156,076 | 156,076 |
| $R^2$ | 0.57 | 0.57 | 0.71 | 0.71 |
| $\bar{y}$ | .78 | .78 | 2.27 | 2.27 |
| Event-Insurer FE | Yes | Yes | Yes | Yes |
| Event-Year FE | Yes | Yes | Yes | Yes |
| Size Decile-Year FE | Yes | Yes | Yes | Yes |

*Notes:* This table reports the results from the following specification:

$$\begin{aligned} y_{ijt} = \alpha_{ie} + \alpha_{et} + \beta\,\text{Treated}_{ie} \times \text{Post}_{et} + \delta\,\text{Insurer Type}_i \times \text{Post}_{et} \\ + \pi\,\text{Treated}_{ie} \times \text{Insurer Type}_i + \lambda\,\text{Treated}_{ie} \times \text{Post}_{et} \\ + \theta\,\text{Treated}_{ie} \times \text{Post}_{et} \times \text{Insurer Type}_i + \varepsilon_{ijt}. \end{aligned}$$

Columns (1)–(2) use the total dollar value of newly acquired CLO tranches as the dependent variable, while Columns (3)–(4) use the dollar value of outstanding CLO investments held by insurance companies. The analysis employs a stacked difference-in-differences (DID) framework, considering all disclosure events $e \in [2010, 2019]$. For each event year $e$, treated insurers are those that first disclose their investment strategy related to climate risk in year $e$, while control insurers are those that either begin disclosing in year $e + 7$ or never disclose during the sample period. This stacked specification mitigates timing biases inherent in staggered treatment adoption. The indicator variable *Post* equals one for years $t$ after the event year $e$, and zero otherwise. We focus on a symmetric event window, $[e - 6, e + 6]$. In Columns (1) and (3), insurers are classified into Life and Non-Life groups. In Columns (2) and (4), the classification is P&C (which are most likely to have natural disaster risk in their liabilities) versus others. All regressions include event–insurer fixed effects ($\alpha_{ie}$) and event–time fixed effects ($\alpha_{et}$). Standard errors are clustered at the insurer level.

Table 7: No Differential Effects for Treated Insurers with Greater Liability Exposure

| | Climate Sentiment (Bond Holdings) (1) | Climate Exposure (Bond Holdings) (2) | CLO Holdings (3) | New CLO Investments (4) |
|---|---|---|---|---|
| Treatment × Post | 0.105*** | 0.024*** | 1.496** | 0.668** |
| | (0.024) | (0.006) | (0.676) | (0.268) |
| Post × Liability Exposure | 0.019 | 0.008 | 0.474* | 0.155 |
| | (0.024) | (0.007) | (0.284) | (0.106) |
| Treatment × Liability Exposure × Post | -0.063* | -0.019* | -0.250 | 0.096 |
| | (0.036) | (0.010) | (0.804) | (0.309) |
| Obs | 367,645 | 367,645 | 158,121 | 158,121 |
| $R^2$ | 0.52 | 0.46 | 0.71 | 0.57 |
| $\overline{y}$ | .83 | .17 | 2.24 | .77 |
| Cohort-Insurer FE | Yes | Yes | Yes | Yes |
| Cohort-Year FE | Yes | Yes | Yes | Yes |
| Size Decile-Year FE | Yes | Yes | Yes | Yes |

*Notes:* This table reports the results from the following specification:

$$\begin{aligned} y_{ijt} = \alpha_{ie} + \alpha_{et} + \beta\,\text{Treated}_{ie} \times \text{Post}_{et} + \delta\,\text{Liability Exposure}_{ie} \times \text{Post}_{et} \\ + \pi\,\text{Treated}_{ie} \times \text{Liability Exposure}_{ie} + \lambda\,\text{Treated}_{ie} \times \text{Post}_{et} \\ + \theta\,\text{Treated}_{ie} \times \text{Post}_{et} \times \text{Liability Exposure}_{ie} + \varepsilon_{ijt}. \end{aligned}$$

Columns (1) and (2) use the aggregate climate sentiment and exposure (of direct corporate bond holdings) at the insurer portfolio level as the dependent variable. Column (3) uses the dollar value of outstanding CLO investments held by insurance companies as the dependent variable. Finally Column (4) uses the total dollar value of newly acquired CLO tranches as the dependent variable. We construct a stacked difference-in-differences (DID) framework by considering all disclosure events $e \in [2010, 2019]$. For each event year $e$, treated insurers are those that first disclose their investment strategy related to climate risk in year $e$, while control insurers are those that either begin disclosing in year $e+7$ or never disclose during the sample period. The stacked specification addresses timing biases inherent in staggered treatment adoption. The indicator variable *Post* equals one for years $t$ after the event year $e$, and zero otherwise. We focus on a symmetric window around the event year, $[e-6, e+6]$. Finally we create an exposure measure to capture the sensitivity of insurer liabilities to natural disasters. To do this, we calculate the fraction of total premiums that come from four key disaster-prone states : California, Florida, Texas, and Louisiana. The indicator variable $\text{Liability Exposure}_{it}$ equals one if insurer $i$'s exposure to these disaster states is higher than the median in year $e-1$, and zero otherwise. The regression includes event-insurer fixed effects ($\alpha_{ie}$) and event-time fixed effects ($\alpha_{et}$). Standard errors are clustered at the insurer level.

Table 8: Insurer Investments into CLO Managers after Climate Supervision

| | Probability of Investing | | | Dollar Investment Volume | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post | -0.000 | | -0.001 | 0.005 | | -0.007 |
| | (0.000) | | (0.001) | (0.009) | | (0.008) |
| Insurer-Mgr Rel $\mathcal{R}^5_{i,m,t-1}$ | | 0.563*** | 0.228*** | | 6.973*** | 2.582*** |
| | | (0.050) | (0.069) | | (0.982) | (0.892) |
| Insurer-Mgr Rel $\mathcal{R}^5_{i,m,t-1}\times$ Post | | | 0.371*** | | | 4.853*** |
| | | | (0.081) | | | (1.330) |
| Obs | 2,856,546 | 2,856,546 | 2,856,546 | 2,856,547 | 2,856,546 | 2,856,546 |
| $R^2$ | 0.06 | 0.07 | 0.07 | 0.05 | 0.06 | 0.06 |
| $\overline{y}$ | .01 | .01 | .01 | .06 | .06 | .06 |
| Insurer FE | Yes | Yes | Yes | Yes | Yes | Yes |
| CLO FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Size Decile-Year FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* This table shows that the probability of insurer investments in CLO funds increase substantially after they start disclosing their climate change strategy. We employ the specification

$$y_{if(m)t} = \alpha_i + \alpha_f + \alpha_t + \gamma_1 \text{Post}_{it} + \gamma_2 \text{Relationship}_{i,m,t-5\to t-1} + \gamma_3 \text{Post}_{it} \times \text{Relationship}_{i,m,t-5\to t-1} + \varepsilon_{ifmt}$$

where $y_{if(m)t}$ is the insurer $i$'s investment strategy for fund $f$ launched by the fund-manager $m$ in year $t$. We consider two variations of the dependent variable : i) an investment dummy which equals one if the insurer $i$ invests in the fund $j$, and zero otherwise (columns 1, and 2); and ii) the dollar value of the insurer's investment in the fund $f$ (columns 3, and 4). $\text{Post}_{i,t}$ is an indicator variable which assumes one if the insurer $i$'s first disclosure filing occurs before year $t$, and zero otherwise. Insurers who have invested with a fund manager $m$ before may have more information about the manager $m$'s success and investing style, and may thus be more inclined to invest with her again. We capture this notion with the variable $\text{Relationship}_{i,m,t-5\to t}$, as defined in equation (10). The key explanatory variable is the interaction between past relationship and post disclosure, captured by the coefficient $\gamma_3$. We also include insurer ($\alpha_i$), fund ($\alpha_f$) and year ($\alpha_t$) fixed effects. Standard errors are clustered at the fund and insurer level. Insurance holdings data comes from NAIC schedule D, disclosure dates come from the published NAIC climate surveys, CLO data comes from Acuris Creditflux, and climate sentiment measures come from Sautner et al. (2023). Sample period runs from 2001 to 2020.

Table 9: Manager Shocks and New CLO Fund Launches

| | (1) Fund Launch Dummy | (2) No. of Funds Launched | (3) Total Manager Balance |
|---|---|---|---|
| Manager Shock $\lambda_{m,t-1}$ | 0.285*** | 1.012*** | 490.077*** |
| | (0.024) | (0.100) | (52.279) |
| Obs | 5,650 | 5,650 | 5,650 |
| $R^2$ | 0.45 | 0.52 | 0.49 |
| $\overline{y}$ | .23 | .56 | 264.71 |
| SD($\lambda_{m,t-1}$) | .38 | .38 | .38 |
| Manager FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |

*Notes:* This table reports the effects of insurer supply shocks in year $(t-1)$ on manager fund creation in the following year $t$, estimated from equation (13). The dependent variable is the probability of a fund launch in column (1), the total number of funds launched in year $t$ in column (2), and the natural logarithm of the total corpus of new funds launched in year $t$ in column (3). The key explanatory variable is the manager level exposure to insurer disclosure shocks, as defined in equation (12). All regressions include manager- and year-fixed effects. The standard errors are clustered at the manager level, and reported in the parentheses. Insurance holdings data comes from NAIC schedule D, disclosure dates come from the published NAIC climate surveys, CLO data comes from Acuris Creditflux, and climate sentiment measures come from Sautner et al. (2023). Sample period runs from 2001 to 2020.

Table 10: Climate Sentiment and Stickiness in CLO Fund Manager Portfolio

| | Dependent Variable : Is Firm Included | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Climate Sentiment$_{j,t-1}$ | -0.003*** | | |
| | (0.000) | | |
| Firm-Manager-Relationship $\mathcal{R}^5_{m,j,t-1}$ | | 0.464*** | 0.467*** |
| | | (0.014) | (0.014) |
| Climate Sentiment$_{j,t-1} \times \mathcal{R}^5_{m,j,t-1}$ | | | -0.014*** |
| | | | (0.005) |
| Obs | 4,489,216 | 4,489,209 | 4,489,209 |
| $R^2$ | 0.24 | 0.31 | 0.31 |
| $\overline{y}$ | .07 | .07 | .07 |
| Fund FE | Yes | Yes | Yes |
| Year FE | Yes | No | No |
| Firm FE | Yes | No | No |
| Firm-Year FE | No | Yes | Yes |

*Notes:* This table reports the estimates from the following specification:

$$y_{f(m),j,t} = \alpha_f + \alpha_j + \alpha_t + \gamma_1 \text{Climate Sentiment}_{j,t-1} + \gamma_2 \mathcal{R}^5_{m,j,t-1} + \gamma_3 \text{Climate Sentiment}_{j,t-1} \times \mathcal{R}^5_{m,j,t-1} + \epsilon_{f(m),j,t},$$

where the dependent variable $y_{f(m),j,t}$ is a dummy which assumes a value one if the firm $j \in \mathcal{J}$ is included in the initial portfolio of fund $f$ launched by manager $m$ at time $t$. The key explanatory variables include the firm's climate sentiment from the previous year, Climate Sentiment$_{j,t-1}$, previous investment relationship between firm $j$ and manager $m$, $\mathcal{R}_{m,j,t-1}$, and their interaction. The relationship variable is formally defined in equation (14). We also include find, firm, and year fixed effects. Additionally, we control for the rating and maturity of the outstanding loans of firms being traded in the secondary market. Standard errors, clustered at the fund level, are reported in the parentheses. Insurance holdings data comes from NAIC schedule D, disclosure dates come from the published NAIC climate surveys, CLO data comes from Acuris Creditflux, and climate sentiment measures come from Sautner et al. (2023). Sample period runs from 2001 to 2020.

Table 11: Firm Shock and Future Loan Issuances

| | Leveraged Loan Issuance | | | Revolver Issuance | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Sample | All | | Public Firms | All | Public Firms |
| Firm Shock $\theta_{j,t-1}$ | 0.060*** | 0.066*** | 0.072*** | -0.005 | 0.014 |
| | (0.004) | (0.007) | (0.011) | (0.007) | (0.011) |
| Obs | 126,320 | 48,040 | 22,060 | 48,040 | 22,060 |
| $R^2$ | 0.23 | 0.41 | 0.43 | 0.44 | 0.44 |
| $\overline{y}$ | .07 | .08 | .08 | .08 | .12 |
| Firm FE | Yes | Yes | Yes | Yes | Yes |
| Industry-Year FE | Yes | No | No | No | No |
| State-Industry-Year FE | No | Yes | Yes | Yes | Yes |

*Notes:* This table reports the estimates from the following specification:

$$y_{j,t} = \alpha_j + \alpha_{skt} + \pi\theta_{j,t-1} + \epsilon_{jt},$$

where the dependent variable $y_{j,t}$ is a dummy which assumes a value one if the firm $j$ issues a leveraged loan(columns 1 and 2) or a revolving line of credit (column 3) in year $t$. The key explanatory variable is the firm shock $\theta_{j,t-1}$ as defined in equation (16). We also include firm, and year (or state-industry-year) fixed effects. Standard errors, clustered at the firm level, are reported in the parentheses. Insurance holdings data comes from NAIC schedule D, disclosure dates come from the published NAIC climate surveys, CLO data comes from Acuris Creditflux, and climate sentiment measures come from Sautner et al. (2023). Sample period runs from 2001 to 2020.

Table 12: Firm Shock and Future Emission Activities

| Dependent Variable : Total Emissions in Pounds Scaled by Total Assets | | | | | |
|---|---|---|---|---|---|
| | Facility-Chemical-Year Level | | | Facility-Year Level | |
| | (1) | (2) | (3) | (4) | (5) |
| Firm Shock $\theta_{j,t-1}$ | 45.440*** | 44.132*** | 40.234*** | 145.203*** | 141.143*** |
| | (12.404) | (13.772) | (12.910) | (50.521) | (49.968) |
| Obs | 182,386 | 155,464 | 150,994 | 37,120 | 36,000 |
| $R^2$ | 0.12 | 0.83 | 0.84 | 0.82 | 0.82 |
| $\overline{y}$ | 72.90 | 77.8 | 76.81 | 352.22 | 345.48 |
| Facility FE | Yes | No | No | Yes | Yes |
| Industry-Year FE | Yes | No | No | Yes | Yes |
| Facility-Chemical FE | No | Yes | Yes | No | No |
| Industry-Chemical-Year FE | No | Yes | Yes | No | No |
| Firm Controls | No | No | Yes | No | Yes |

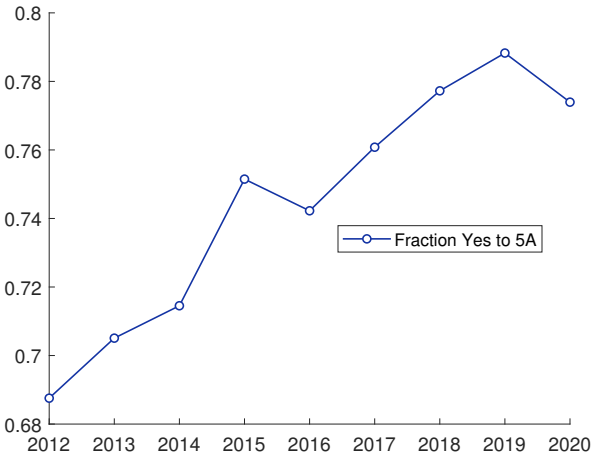
*Notes:* This table reports the estimates from the following specification:

$$y_{j,t} = \alpha_j + \alpha_t + \pi\theta_{j,t-1} + \epsilon_{jt},$$

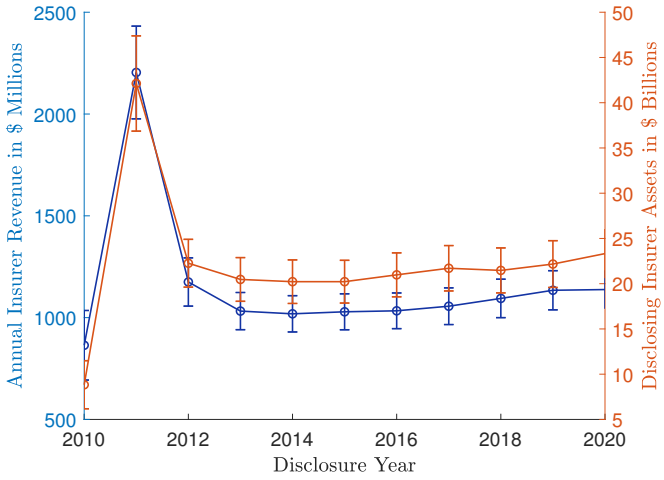
where the dependent variable $y_{j,t}$ is the total emissions of firm $j$ during the period $t$ scaled by total assets of the firm. The key explanatory variable is the firm shock $\theta_{j,t-1}$ as defined in equation (16). We also include firm-facility, and industry-year fixed effects. For data aggregated at facility-chemical-year level, we use a stronger facility-chemical and chemical-industry-year fixed effects. Firm controls include Lagged Assets, Tobin's Q, Profitability, and market leverage. Standard errors, clustered at the facility level, are reported in the parentheses. Insurance holdings data comes from NAIC schedule D, disclosure dates come from the published NAIC climate surveys, CLO data comes from Acuris Creditflux, and climate sentiment measures come from Sautner et al. (2023). Sample period runs from 2001 to 2020.

# A Appendix: Additional Tables and Figures

Figure A1: Insurance Company Climate Disclosures




(a) Response to Question 5A in the Disclosure Survey




(b) Size Distributions of Disclosing Insurers

*Notes:* Panel A above shows the fraction of insurance companies that responded yes to Question 5A in the climate disclosure survey. Question 5a asks "Has the company considered the impact of climate change on its investment portfolio?". Panel B shows the average annual revenue in $ millions of the insurers who disclose their climate strategy in response to the state imposed mandates. The distribution is relatively stable across the years, especially after 2012, suggesting that size effects are unlikely to drive our results (especially those excluding 2010 to 2012). In 2011, only California insurers were surveyed, which added several large insurers to our sample. In subsequent years, the survey was expanded to other states, which added smaller insurers and lowered the overall average.

Table A1: Robustness Test: Matched Pair Design

| | (1) New CLO Investments ($ Millions) | (2) CLO Holdings ($ Millions) |
|---|---|---|
| Treatment × Post | 4.697*** | 8.359*** |
| | (0.686) | (1.140) |
| Obs | 56,080 | 56,080 |
| $R^2$ | 0.73 | 0.75 |
| $\overline{y}$ | 1.84 | 3.41 |
| Event-Insurer FE | Yes | Yes |
| Event-Year FE | Yes | Yes |

*Notes:* This table reports estimates from the matched specification:

$$y_{it} = \alpha_{e,i} + \alpha_{e,t} + \pi \text{Treated}_{i,e} \times \text{Post}_{e,t} + X_{it} + \varepsilon_{it},$$

Treated and control insurers in this regression are matched based on: line of business, states of operation, and size of total premiums collected. Standard errors are clustered at insurer level, and reported in the parentheses. Insurance holdings data comes from NAIC schedule D, disclosure dates come from the published NAIC climate surveys, CLO data comes from Acuris Creditflux, and climate sentiment measures come from Sautner et al. (2023). Sample period runs from 2001 to 2020.

Table A2: Effect of Disclosures on Insurers' ABS Investments

| **Panel A: All Insurers** | | | | | |
|---|---|---|---|---|---|
| | ABS Holdings in $ Millions | | Scaled Pct of Portfolio | | Wt. Coupon |
| | Holdings | Purchase | Holdings | Purchase | Holdings |
| | (1) | (2) | (3) | (4) | (5) |
| Treatment × Post | 5.945*** | 2.360*** | 0.293*** | 0.143*** | 0.194** |
| | (1.829) | (0.682) | (0.090) | (0.043) | (0.076) |
| Obs | 205,603 | 205,603 | 205,603 | 205,603 | 205,603 |
| $R^2$ | 0.80 | 0.68 | 0.72 | 0.59 | 0.63 |
| $\bar{y}$ | 10.63 | 3.98 | .2 | .09 | 1.95 |
| Event-Insurer FE | Yes | Yes | Yes | Yes | Yes |
| Event-Year FE | Yes | Yes | Yes | Yes | Yes |
| Size Decile-Year FE | Yes | Yes | Yes | Yes | Yes |
| **Panel B: Excluding Early Filers** | | | | | |
| | ABS Holdings in $ Millions | | Scaled Pct of Portfolio | | Wt. Coupon |
| | Holdings | Purchase | Holdings | Purchase | Holdings |
| | (1) | (2) | (3) | (4) | (5) |
| Treatment × Post | 5.970*** | 2.889*** | 0.762*** | 0.371*** | 0.182* |
| | (1.461) | (0.623) | (0.241) | (0.115) | (0.093) |
| Obs | 130,248 | 130,248 | 130,248 | 130,248 | 130,248 |
| $R^2$ | 0.78 | 0.64 | 0.79 | 0.65 | 0.63 |
| $\bar{y}$ | 7.44 | 2.9 | .21 | .1 | 1.65 |
| Event-Insurer FE | Yes | Yes | Yes | Yes | Yes |
| Event-Year FE | Yes | Yes | Yes | Yes | Yes |
| Size Decile-Year FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* This table reports the results from the following specification:

$$y_{it} = \alpha_{ie} + \alpha_{et} + \beta \text{Treated Insurer}_{ie} \times \text{Post}_{et} + \varepsilon$$

Column (1) uses the total dollar value of total outstanding ABS tranches as the dependent variable. Column (2) uses the dollar value of newly acquired ABS tranches. Finally, Column (3) uses the value weighted average interest-rates offered by the outstanding ABS tranches. We construct a stacked difference-in-differences (DID) framework by considering all disclosure events $e \in [2010, 2019]$. For each event year $e$, treated insurers are those that first disclose their investment strategy related to climate risk in year $e$, while control insurers are those that either begin disclosing in year $e + 7$ or never disclose during the sample period. The stacked specification addresses timing biases inherent in staggered treatment adoption. The indicator variable *Post* equals one for years $t$ after the event year $e$, and zero otherwise. The regression includes event-insurer fixed effects ($\alpha_{ie}$) and event-time fixed effects ($\alpha_{et}$). Furthermore, to account for differences in portfolio allocations and climate exposures stemming from size differences of insurers, we include Size Decile × Year FE in all our specifications. To calculate the size deciles, we use the total assets of insurance companies from 2009 filings, before the first disclosure shock. Standard errors are clustered at the insurer level.

Table A3: Robustness: Group Level Analysis

| | Climate | | CLO Investments | | Wt. Spread |
|---|---|---|---|---|---|
| | Exposure | Sentiment | $ Outstanding | $ Purchase | |
| | (1) | (2) | (3) | (4) | (5) |
| Treatment × Post | 0.077*** | 0.020*** | 7.933*** | 4.041*** | 0.388*** |
| | (0.023) | (0.006) | (0.853) | (0.348) | (0.041) |
| Obs | 168,720 | 168,720 | 117,220 | 117,220 | 117,220 |
| $R^2$ | 0.52 | 0.45 | 0.71 | 0.58 | 0.54 |
| $\overline{y}$ | .77 | .15 | 2.02 | .69 | .1 |
| Cohort-Group FE | Yes | Yes | Yes | Yes | Yes |
| Cohort-Year FE | Yes | Yes | Yes | Yes | Yes |

Columns (1)–(2) use the total weighted climate exposure and sentiment at the group level as the dependent variable, while Columns (3)–(4) use the dollar value of outstanding CLO investments and CLO tranche purchases respectively at the group level. Finally, Column (5) uses the weighted spread on the tranches as the dependent variable. The analysis employs a stacked difference-in-differences (DID) framework, considering all disclosure events $e \in [2010, 2019]$. For each event year $e$, treated insurance groups are those that first disclose their investment strategy related to climate risk in year $e$, while control insurance groups are those that either begin disclosing in year $e + 7$ or never disclose during the sample period. We use the earliest disclosure date within an insurance group as the disclosure date for the whole entity. This stacked specification mitigates timing biases inherent in staggered treatment adoption. The indicator variable *Post* equals one for years $t$ after the event year $e$, and zero otherwise. We focus on a symmetric event window, $[e - 6, e + 6]$. All regressions include event–group fixed effects and event–time fixed effects. Standard errors are clustered at the group level.

Table A4: Yields Offered by Corporate Bonds and CLO Securities

| | All Securities | | | Except AAA |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| CLO Tranche Dummy | 0.569*** | 0.744*** | 0.888*** | 0.959*** |
| | (0.060) | (0.037) | (0.041) | (0.038) |
| Obs | 4686542 | 4686257 | 4686257 | 4594410 |
| $R^2$ | 0.56 | 0.67 | 0.68 | 0.68 |
| $\overline{y}$ | 4.73 | 4.73 | 4.73 | 4.76 |
| Insurer FE | Yes | Yes | Yes | Yes |
| Rating-Year FE | Yes | | | |
| Maturity-Rating-Year FE | | Yes | Yes | Yes |
| Bond Controls | | | Yes | Yes |

*Notes:* This table reports the results of the specification

$$y_{ist} = \delta_{rt} + \beta \text{CLO Tranche Dummy}_s + \Gamma \mathbf{X}_{st} + \varepsilon_{ist}$$

where the dependent variable $y_{ist}$ is the effective yield offered by security $s$ held by insurer $i$ at time $t$. The securities $s$ held by insurers are of primarily two types : corporate bonds, and CLO tranches. The key independent variable CLO Tranche Dummy$_s$ assumes a value one if the security is a CLO tranche, and zero if it is a corporate bond. We collect yield and maturity data for all securities directly from the NAIC Schedule-D Part-1 files. We collect rating time series on corporate bonds from the Mergent Fixed Income database, and rating information on CLO tranches from the Creditflux CLO database. Control variables included in the vector $\mathbf{X}$ include the outstanding dollar amount of the security $s$ at time $t$. We also include security rating-year fixed effects $\delta_{rt}$ in column (1), and a stronger maturity-rating-year fixed effect $\delta_{rmt}$ in columns (2) - (4). In columns (1) - (3), we focus on all securities held by insurers over the period 2001-2020, while in column (4) we exclude the $AAA$-rated securities.

Table A5: Difference in yields across capital-constrained insurers after climate supervision

| | All Securities | | | Except AAA |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treated × Low RBC × CLO | 0.287*** | 0.178** | 0.172** | 0.156* |
| | (0.076) | (0.072) | (0.072) | (0.091) |
| Treated × CLO | 0.298*** | 0.485*** | 0.495*** | 0.650*** |
| | (0.081) | (0.072) | (0.073) | (0.089) |
| CLO × Low RBC | -0.110 | -0.082 | -0.077 | -0.110 |
| | (0.076) | (0.073) | (0.073) | (0.091) |
| Treated × Low RBC | -0.032*** | -0.010** | -0.008* | -0.008** |
| | (0.005) | (0.004) | (0.004) | (0.004) |
| Treated | 0.080*** | 0.049*** | 0.046*** | 0.048*** |
| | (0.008) | (0.007) | (0.007) | (0.007) |
| Low RBC | -0.007** | -0.015*** | -0.015*** | -0.014*** |
| | (0.004) | (0.003) | (0.003) | (0.003) |
| CLO Tranche Dummy | 0.171* | 0.285*** | 0.377*** | 0.335*** |
| | (0.088) | (0.077) | (0.078) | (0.093) |
| Obs | 2992801 | 2992474 | 2992474 | 2937008 |
| $R^2$ | 0.55 | 0.67 | 0.68 | 0.68 |
| $\overline{y}$ | 4.89 | 4.89 | 4.89 | 4.92 |
| Insurer FE | Yes | Yes | Yes | Yes |
| Rating-Year FE | Yes | | | |
| Maturity-Rating-Year FE | | Yes | Yes | Yes |
| Bond Controls | | | Yes | Yes |

*Notes:* This table reports the results of the specification

$$\begin{aligned} y_{ist} = \delta_{rt} + \beta_1 \text{Treated}_i \times \text{Low RBC}_{it} \times \text{CLO}_s + \beta_2 \text{Treated}_i \times \text{CLO}_s + \\ \beta_3 \text{Low RBC}_{it} \times \text{CLO}_s + \beta_4 \text{Treated}_i \times \text{Low RBC}_{it} + \beta_5 \text{Treated}_i + \\ \beta_6 \text{Low RBC}_{it} + \beta_7 \text{CLO}_s + \Gamma \mathbf{X}_{st} + \varepsilon_{ist} \end{aligned}$$

where the dependent variable $y_{ist}$ is the effective yield offered by security $s$ held by insurer $i$ at time $t$. The securities $s$ held by insurers are of primarily two types : corporate bonds, and CLO tranches. The key variable of interest is the triple interaction term $\text{Treated}_i \times \text{Low RBC}_{it} \times \text{CLO}_s$. We collect yield and maturity data for all securities directly from the NAIC Schedule-D Part-1 files. We collect rating time series on corporate bonds from the Mergent Fixed Income database, and rating information on CLO tranches from the Creditflux CLO database. Control variables included in the vector $\mathbf{X}$ include the outstanding dollar amount of the security $s$ at time $t$.

Table A6: Effect of Climate Sentiment on New Bond Issuances

| | Prob. Bond Issuance | Bond Coupon | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $CC_{j,t-1}$ | 0.001 | -0.078*** | -0.065*** | -0.066*** |
| | (0.004) | (0.024) | (0.022) | (0.020) |
| Obs | 38,553 | 10,790 | 8,921 | 7,956 |
| $R^2$ | 0.32 | 0.70 | 0.72 | 0.72 |
| $\overline{y}$ | .3 | 4.96 | 5.04 | 4.97 |
| Firm FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Bond Controls | | | Yes | Yes |
| Firm Controls | | | | Yes |

*Notes:* This table reports the results of the specification

$$y_{jt} = \alpha_j + \alpha_{rmt} + \pi \text{CC}_{j,t-1} + \Gamma \mathbf{X}_j + \varepsilon_{jt}$$

where the dependent variable $y_{jt}$ is an outcome variable related to the bond issuance of firm $j$ at time $t$. In column (1), we use an indicator variable which assumes a value one if the firm $j$ issued a bond at time $t$, and zero otherwise. In columns (2) - (4), we use the coupon on the new bond issued, conditional on issuance, as the dependent variable. The key independent variable is the climate sentiment $\text{CC}_{j,t-1}$ of firm $j$ in year $(t-1)$. We control for firm fixed effects $\alpha_j$, and year fixed effects in columns (1) and (2). In columns (3) and (4), we employ a more stringent maturity rating year fixed effect $\alpha_{rmt}$. We collect rating time series on corporate bonds from the Mergent Fixed Income database, and the first available rating of the bond as our rating measure. Control variables included in the vector $\mathbf{X}$ include control dummies for whether the bond was issued through a private 144A placement, and whether the bond is backed by an asset. Firm level controls also include the natural logarithm of total assets, Tobin's Q, return on assets, market leverage, and cash over assets. Standard errors are clustered at firm level, and reported in parentheses. Insurance holdings data comes from NAIC schedule D, disclosure dates come from the published NAIC climate surveys, CLO data comes from Acuris Creditflux, and climate sentiment measures come from Sautner et al. (2023). Sample period runs from 2001 to 2020.

Table A7: Effect of Climate Sentiment on New Leveraged Loan Issuances

| | Prob. Loan Issuance | Loan Spread | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $CC_{j,t-1}$ | 0.018 | -2.108 | -2.919 | 0.306 |
| | (0.039) | (3.242) | (3.263) | (3.155) |
| Obs | 20,948 | 5,442 | 5,374 | 4,372 |
| $R^2$ | | 0.60 | 0.60 | 0.60 |
| $\overline{y}$ | .19 | 302.81 | 302.39 | 293.11 |
| Firm FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Loan Controls | | | Yes | Yes |
| Firm Controls | | | | Yes |

*Notes:* This table reports the results of the specification

$$y_{jt} = \alpha_j + \alpha_{mt} + \pi \text{CC}_{j,t-1} + \Gamma \mathbf{X}_j + \varepsilon_{jt}$$

where the dependent variable $y_{jt}$ is an outcome variable related to the leveraged loan issuance of firm $j$ at time $t$. In column (1), we use an indicator variable which assumes a value one if the firm $j$ issued a leveraged loan at time $t$, and zero otherwise. In columns (2) - (4), we use the all-in-drawn spread (AISD) on the new loan issued, conditional on issuance, as the dependent variable. The key independent variable is the climate sentiment $\text{CC}_{j,t-1}$ of firm $j$ in year $(t-1)$. We control for firm fixed effects $\alpha_j$, and year fixed effects in columns (1) and (2). In columns (3) and (4), we employ a more stringent maturity-year fixed effect $\alpha_{mt}$. Control variables included in the vector $\mathbf{X}$ include loan size, the natural logarithm of total assets, Tobin's Q, return on assets, market leverage, and cash over assets. Standard errors are clustered at firm level, and reported in parentheses. Insurance holdings data comes from NAIC schedule D, disclosure dates come from the published NAIC climate surveys, CLO data comes from Acuris Creditflux, and climate sentiment measures come from Sautner et al. (2023). Sample period runs from 2001 to 2020.