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August 5, 2025

Dear Professors Flammer and Giglio,

With this letter, I wish to express my strong interest in participating in the NBER PhD Student Workshop on Climate Finance.

My research contributes to examining how climate-related risks shape financial decisions, with a focus on the resulting real economic outcomes.

In my first paper, co-authored with Emanuela Benincasa, Miguel Ferreira, and Emilia Garcia-Appendini, I study how uncoordinated climate policies across jurisdictions incentivize firms to rewire production networks away from carbon-pricing regimes. This reallocation generates a form of carbon leakage, as emissions and economic activity shift along supply chains toward less regulated regions.

In my second paper, co-authored with Felix von Meyerinck and Zacharias Sautner, I analyze how insurers with voluntary carbon underwriting policies—public commitments to restrict the underwriting of risks of the fossil fuel sector—significantly reduce the number of coal mines and the coal volumes they insure, in turn increasing the likelihood of mine abandonment and operational constraints.

Going forward, I plan to extend this agenda by studying how firms internalize and manage climate risk, focusing on the role of insurance amid the climate crisis.

These projects are under active revision and rewriting. Therefore, I would greatly benefit from feedback and discussion with other scholars and peers in the field. Should I be selected, I am confident that the insights gained will meaningfully help my future work.

Thank you for your time and consideration,
Olimpia Carradori

References:

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EDUCATION

Swiss Finance Institute, **University of Zurich**, Ph.D. in Finance, 2022 - 2028 (expected)

University of St. Gallen, M.A. in Banking and Finance, 2019 - 2022

LUISS University, B.A. in Economics and Management, 2016 - 2019

RESEARCH AREAS

Empirical Corporate Finance, Sustainable Finance

RESEARCH PAPERS

Insurers' Carbon Underwriting Policies

(with Felix von Meyerinck and Zacharias Sautner)
Swiss Finance Institute Research Paper No. 25-58

Abstract: We study the determinants, structure, implementation, and effects of carbon underwriting policies among the world's largest insurers. Adoption is more common among European insurers and less so among specialty and unlisted firms, with coal policies emerging earlier than those targeting oil and gas. Implementation of coal policies is often incomplete, and some insurers expand coal coverage despite commitments. Using merged mine-insurance policy data, we show that insurers with coal policies reduce the number of insured mines by about 16%, insured coal volumes by 56%, and make continued coverage 13pp less likely. Affected mines reduce output, cut employment, and are abandoned more often in subsequent periods.

Rewiring Supply Chains Through Uncoordinated Climate Policy

(with Emanuela Benincasa, Miguel Ferreira and Emilia Garcia-Appendini)
Swiss Finance Institute Research Paper No. 24-56

Abstract: We show that climate transition risks can significantly disrupt supply chain networks. Specifically, suppliers affected by the California cap-and-trade program are more likely to lose customer relationships and less likely to form new ones compared to their competitors unaffected by the program. The effects are more pronounced among suppliers facing high competitive pressure and producing standardized inputs. Additionally, affected suppliers experience declines in revenues, assets, and profitability. This supply chain rewiring induced by uncoordinated climate policies is consistent with carbon leakage, as customers exposed to the program through production networks show an increase in their supply chain emission intensity.

PRE DOCTORAL RESEARCH

Financing the Low-Carbon Transition in Europe

(with Margherita Giuzio, Sujit Kapadia, Dilyara Salakhova and Katia Vozian)
European Central Bank Working Paper Series No. 2813

CONFERENCE AND SEMINAR PRESENTATIONS

2025

Conferences: AFA PhD Poster Session; ESCP Sustainable Finance Conference; SFI

Research Days; GRASFI
Seminars: University of Zurich PhD Seminar; QFinLab Seminar at Politecnico di Milano

2024

Conferences: 5th University of Oklahoma and Review of Financial Studies Climate and Energy Finance Research Conference; ESADE, ECB, EBRD, EBA, CEPR, CompNet & IWH Conference on Finance and Productivity; Banca d'Italia-EABCN-EUI-CEPR Joint Conference on The Macroeconomic and Financial Dimensions of the Green Transition; Rising Scholars Conference on Sustainable Finance; SFI Research Days; CEPR-FGN Sustainability and Public Policy Workshop

Seminars: University of Zurich PhD Seminar

2023

Conferences: Energy Finance Italia Conference; Young Swiss Economist Meeting

2022

Conferences: GRASFI; RiskLab, Bank of Finland, European Systemic Risk Board Conference on Systemic Risk Analytics

Seminars: Sustainable Finance Initiative Seminar of the Stanford Precourt Institute for Energy; University of St. Gallen Finance Seminar; European Central Bank Climate Change Center Seminar Series

AWARDS AND GRANTS

2025

SFI Best Discussant Doctoral Award

2024

SNSF Grant Nr. 100018 204561 / 1 (Project Corporate Short-Termism)
SFI Best Discussant Doctoral Award

DISCUSSIONS

Rising Biodiversity Risk and Firm Exposure along the Supply Chain. V. A. Dang, N. Gao, and H. Lin. ESCP Sustainable Finance Workshop, 2025

Retail Trading Fire Sales: The Impact of Wildfires on Investment Decisions. F. Martins, V. Gianinazzi, V. Mendes, and M. Abreu. SFI Research Days, 2025

From Funding to Footprint? The Influence of Debt Structure on Corporate Sustainability. M. Arnold, N. Kollmann, and V. Zeh. SFI Research Days, 2024

The Tragedy of the Common Heating Bill. H. Mayr and M. Souza CEPR-FGN Sustainability and Public Policy Workshop, 2024

Sovereign Debt Sustainability Analysis Under Climate Transition Risk. V. Mammetti and G. Morelli. Energy Finance Italia, 2023

WORKSHOPS ORGANIZATION

2025

Tri-City Bridge Workshop on Empirical Research in Finance, University of Zurich (with Jason Blunier, Thea Kolasa, Zhixi Wei)

PRIOR EXPERIENCE

University of Zurich, Zurich, Switzerland

Teaching Assistant, Advanced Banking, Master in Banking and Finance, 2024 - 2025

European Central Bank, Frankfurt, Germany

External Researcher, DG-Macroprudential Policy/Financial Stability, 2022 - 2024
Trainee, DG-Macroprudential Policy/Financial Stability, 2021 - 2022

University of St. Gallen, St. Gallen, Switzerland
Research Assistant, Chair of Banking, 2020 - 2021

UniCredit Bank, Munich, Germany and Belgrade, Serbia
Intern, Financials' Credit Research, 2020
Intern, International Internship Program, 2019

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Rewiring Supply Chains Through Uncoordinated Climate Policy*

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Emilia Garcia-Appendini¶

August 5, 2025

Abstract

We show that climate transition risks can significantly disrupt supply chain networks. Specifically, suppliers affected by the California cap-and-trade program are more likely to lose customer relationships compared to their peers unaffected by the program. The effects are more pronounced among suppliers facing high competitive pressure and producing standardized inputs. Additionally, affected suppliers experience declines in revenues, assets, and investments. This supply chain rewiring induced by uncoordinated climate policies is consistent with carbon leakage, as customers who rewire their sourcing network away from the cap-and-trade show a worsening in their supply chain carbon footprint.

JEL classification: G32, Q54, Q55
Keywords: Climate finance, Cap-and-trade policy, Carbon emissions, Supply chain, Product market competition, Input specificity

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1 Introduction

In recent decades, many governments have adopted climate policies to curb emissions, resulting in a patchwork of regulations across different legal jurisdictions. These uncoordinated climate policies can lead to different forms of environmental regulatory arbitrage (known as "carbon leakage"). Previous research finds that companies react to uncoordinated climate policies by moving production and emissions from facilities in jurisdictions with climate policies to facilities with unregulated emissions (Ben-David, Jang, Kleimeier, and Viehs, 2021; Bartram, Hou, and Kim, 2022). However, an equally significant form of carbon leakage could result from changes in the supply chain. Specifically, customers can shift their input source from suppliers subject to climate policies to unregulated suppliers in a strategy aimed at mitigating indirect exposure to climate transition risks and reducing supply chain uncertainty (Cosbey, Droege, Fischer, and Munnings, 2019). In turn, this can affect business relationships, trade patterns, and emissions production along supply chains.

It is ex-ante unclear whether uncoordinated climate policies lead to the rewiring of supply chains away from regulated suppliers. On the one hand, climate policies expose affected firms to climate transition risks, likely to materialize as reductions in jobs, capital, and output (Greenstone, 2002; Ryan, 2012), thereby placing them at a disadvantage relative to their competitors.[1] In turn, increased exposure to climate transition risks can increase risks of future supply chain disruptions and trigger termination of business relationships with affected suppliers.[2] On the other hand, regulators often mitigate the impact of climate policies on firm competitiveness by granting exemptions to highly energy-intensive and trade-exposed

[1]For instance, in its Securities and Exchange Commission (SEC) 10K filings, Air Products, and Chemicals Inc. notes that "Any legislation that limits or taxes greenhouse gas (GHG) emissions could impact the Company's growth, increase its operating costs, or reduce demand for certain of its products" (available at https://www.sec.gov/Archives/edgar/data/2969/000119312512476878/d409668d10k.htm).

[2]For instance, in its SEC 10K filings, Campbell Soup Company notes that "Increased compliance costs and expenses due to the impacts of climate change and additional legal or regulatory requirements regarding climate change that are designed to reduce or mitigate the effects of carbon dioxide and other greenhouse gas emissions on the environment may cause disruptions in, or an increase in the costs associated with, the running of our manufacturing facilities and our business, as well as increase distribution and supply chain costs." (available at https://www.sec.gov/Archives/edgar/data/16732/000001673223000109/cpb-20230730.htm).

sectors (Fowlie, Reguant, and Ryan, 2016). Other factors may also impact the stability of supply chain relationships. High switching costs, such as those related to search efforts or input specificity (Barrot and Sauvagnat, 2016; Bernard, Moxnes, and Saito, 2019) generally increase the stickiness of supply chains. Furthermore, the anticipation of future supply chain emissions regulations (Ramadorai and Zeni, 2024), as well as pressure from environmentally conscious stakeholders and consumers (Krueger, Sautner, and Starks, 2020; Aghion, Bénabou, Martin, and Roulet, 2023) may discourage cutting down relationships with suppliers subject to climate policies.

In this paper, we empirically examine how climate policies affect supply chains. Specifically, we investigate whether suppliers suffer the termination of their pre-existing customer relationships as they compete with other suppliers in jurisdictions without climate policies in place. In addition, we assess how these policies affect firms' financial and environmental performance, focusing on supply chain (scope 3) emissions.

To analyze the effect of carbon pricing policies on supply chains, we focus on suppliers subject to the California cap-and-trade program, the second largest cap-and-trade program globally, and the only cap-and-trade policy that regulates industrial emissions in the United States. Implemented in 2013, this program sets a price on emissions for firms that produce more than 25,000 tons of carbon dioxide equivalents in their California facilities. California provides an ideal setting to examine the effects of carbon pricing programs on supply chains, as no other U.S. state regulates industrial emissions, thus offering a unique context of within-country policy heterogeneity. This setting allows us to study whether customers terminate or maintain relationships with suppliers affected by the cap-and-trade program while engaging in new relationships with similar suppliers not subject to the program.

We implement our identification strategy using a difference-in-differences (DiD) methodology to analyze changes in the likelihood of terminating an existing relationship with a supplier affected by the cap-and-trade program. To identify suppliers subject to the program (here-

after *treated suppliers*), we combine facility-level emissions data from the U.S. Environmental Protection Agency (EPA) with supplier-customer relationship data from FactSet Revere. Treated suppliers are defined as firms with at least one facility in California that reported emissions exceeding 25,000 tons of CO2 equivalents in 2010. To control for potential (unobserved) factors unrelated to the cap-and-trade program that may affect supplier–customer relationships —such as state-level trends in inflation, labor costs, or production prices—, we compare treated suppliers with suppliers having at least one plant in California producing below 25,000 tons of CO2 equivalents in the same year (hereafter *control suppliers*).[3] In addition, our methodology incorporates customer-by-supplier sector fixed effects to ensure comparability of supplier-customer relationships and control for alternative demand-side determinants. Given our identification strategy, we assess the likelihood that the same customer terminates its relationship with a treated supplier compared to a control supplier within the same sector.

Our baseline results indicate that treated suppliers experience an 11.4 percentage point increase in the likelihood of terminating their existing customer relationships compared to control suppliers following the introduction of the cap-and-trade program. This result is economically significant, representing an increase in the probability of ending a relationship with suppliers subject to the program of approximately 28% of its standard deviation.[4]

The higher probability of terminating relationships with suppliers subject to the cap-and-trade program might be a direct consequence of suppliers' reallocation of production outside California. For example, Bartram et al. (2022) show that financially constrained suppliers often shift their operations to plants in other states, potentially leading to the formation of new

[3]Note that, for firms with multiple plants in California to be included in the control group, all of their plants must have emissions below the 25,000 tons of CO2 threshold in 2010.

[4]The magnitude of the effect is consistent with previously documented strong responses to the California cap-and-trade program. For instance, Bartram et al. (2022) report a 42% increase in output shift from plants located in other states. Similarly, Ivanov et al. (2024) find that following the program's passage, banks shortened loan maturities to carbon-intensive firms by five months and reduced loan commitments to power generators in California by 20%.

customer relationships closer to these non-Californian locations. However, we show that our baseline results remain both economically and statistically significant within the sub-sample of suppliers that operate exclusively in California, as well as among financially unconstrained suppliers. This finding indicates that heterogeneous carbon policies can create a channel for carbon leakage by requiring supply chain reconfigurations amid climate transition risks.

Next, we examine the economic mechanisms underlying the reconstruction of supply chains in response to climate transition risks. First, we test whether the loss of competitiveness faced by firms subject to carbon pricing drives our results. This mechanism predicts stronger effects for firms operating in more competitive industries, where customers can more easily mitigate climate transition risk by switching to alternative suppliers if in competitive industries. Consistent with this notion, we find that our baseline effects are more pronounced among suppliers operating in industries with a low Herfindahl-Hirschman Index (HHI), a low Lerner Index, and larger groups of direct competitors.

Second, we hypothesize that, in response to climate transition risks, customers with low switching costs to source their inputs more readily and cheaply from alternative suppliers should be the ones more prone to reconstruct their business relationships. Consistent with this prediction, we find that our baseline effects are concentrated among suppliers offering standardized inputs and those with lower investment in innovation (i.e., research and development, R&D, expenditures). The results are also stronger among suppliers with shorter relationship durations.

To further investigate the economic mechanism underlying our results, we document that the introduction of the cap-and-trade program worsens firms' performance. This suggests that the uncoordinated climate action in the U.S. impairs the competitiveness of suppliers subject to the cap-and-trade program relative to their peers, due to their differential exposure to

climate transition risks.[5] Specifically, we document that treated suppliers experience greater declines in revenues, assets, and investments as the climate risk induced by the policy materializes. Importantly, we find no evidence of changes in average costs of goods sold (COGS), suggesting that the higher likelihood of terminating business relationships with firms subject to the program—relative to similar firms located in California but unaffected—is driven solely by policy-related uncertainty, rather than by changes in production or transportation costs. Furthermore, we find that the deterioration of financial performance is more pronounced for suppliers that produce standardized inputs. These findings align with those of Greenstone (2002) and Ryan (2012), indicating that firms producing standardized inputs face a higher probability of supply chain disruption.

We then explore whether customers anticipate future environmental regulations outside of California might mitigate the observed effects. Even with climate transition risks induced by the cap-and-trade program, customers under pressure to decarbonize their supply chain may opt to maintain relationships with suppliers subject to the cap-and-trade program to enhance their prospects of lower scope 3 emissions. Supporting this idea, we find that customers headquartered in environmentally conscious jurisdictions–such as Democrat-led states–and customers reporting relatively high attention to climate transition risks in the pre-program period (as proxied by the index of climate change attention in Sautner, Van Lent, Vilkov, and Zhang, 2023) are less likely to terminate relationships with suppliers subject to the program.

In the last part of the paper, we examine the environmental implications of uncoordinated climate policies. We find that the introduction of the cap-and-trade program is associated with a worse carbon footprint along the supply chains of customers who rewire away from California, relative to comparable customers who keep their ties with treated suppliers. These

[5]For instance, in its SEC 10K filings, the United States Steel Corporation explicitly addresses the heterogeneity in climate policies as a source of disadvantage: "International environmental requirements vary. While standards in the EU, Canada, and Japan are generally comparable to U.S. standards, other nations, particularly China, have substantially lower requirements that may give competitors in such nations a competitive advantage [...] GHG policies could negatively affect our results of operations and cash flows" (available at https://www.sec.gov/Archives/edgar/data/1163302/000119312513061613/d448577d10k.htm).

findings provide suggestive evidence of the unintended consequences of uncoordinated climate policies, consistent with the primary implication of a carbon leakage described by Cosbey, Droege, Fischer, and Munnings (2019).

Our paper contributes to several strands of the literature. First, we add to the literature on carbon leakage. Empirical studies suggest that differences in climate policies across regions can incentivize firms to increase pollution outside the regions subject to those policies (Benincasa, Kabas, and Ongena, 2022; Laeven and Popov, 2023; Duchin, Gao, and Xu, 2024; Ivanov, Kruttli, and Watugala, 2024). Related to our paper, Ben-David et al. (2021) and Bartram et al. (2022) provide evidence of within-firm reallocation of production to facilities outside carbon-regulated jurisdictions.[6] Our paper builds on this literature by studying the export of goods from cap-and-trade regimes through the relationships between suppliers exposed to climate transition risks and their customers. Importantly, we document a novel carbon leakage mechanism that occurs beyond the firm's boundaries through supply chain realignment.

We also contribute to the literature on the effects of shocks on production networks (Acemoglu, Carvalho, Ozdaglar, and Tahbaz-Salehi, 2012; Jiang, Rigobon, and Rigobon, 2021; Baldwin and Freeman, 2022; Acemoglu and Tahbaz-Salehi, 2024), focusing on the network effects of the introduction of cap-and-trade programs. Demir, Javorcik, Michalski, and Ors (2024) examine the effects of import taxes on production networks using customer-supplier links, and Devulder and Lisack (2020) explore the impact of carbon taxes using input-output data. We contribute to this literature using a micro-level approach focusing on supplier-customer relationships to study the effects of heterogeneous cap-and-trade programs across regions. Our findings also complement research on the propagation of corporate social

[6]In contemporary work, Coster, di Giovanni, and Mejean (2024) show that as the EU ETS tightened, French firms increased their imports of dirty products from countries outside of the EU-ETS. Martin, Muûls, and Stoerk (2024) find no evidence of significant real effects on Belgian firms with higher indirect exposure to the EU ETS through their supply chains. However, these firms appear to respond by diversifying their supplier portfolios.

responsibility (CSR) and environmental, social, and governance (ESG) practices along the supply chain (Schiller, 2018; Dai, Duan, Liang, and Ng, 2021a; Dai, Liang, and Ng, 2021b; Asgharian, Dzieliński, Hashemzadeh, and Liu, 2023; Bisetti, She, and Zaldokas, 2023; Hege, Li, and Zhang, 2023; Lu, Peng, Shin, and Yu, 2023; Homroy and Rauf, 2024).

Our paper also relates to research on the impact of climate risks on supply chains. Barrot and Sauvagnat (2016) find that suppliers affected by major natural disasters lead to output losses for their customers, especially when they produce a specific input. Carvalho, Nirei, Saito, and Tahbaz-Salehi (2021) show that downstream propagation explains a sizable fraction of the decline in output following the Great East Japan Earthquake of 2011. Pankratz and Schiller (2023) show that suppliers experiencing heat days above expectations are more likely to suffer the termination of their customer relationships. In contrast, our paper focuses on climate transition risks, particularly those arising from state-level climate policies. This focus allows us to analyze carbon leakage and uncover the unintended environmental consequences of these policies. Unlike physical climate risks or ESG shocks, the climate policy risks we examine produce a more permanent impact on firms' production networks.

Last, we contribute to the literature exploring how climate regulations impact firms' behavior, performance, and production processes (Greenstone, 2002; Ryan, 2012; Bushnell, Chong, and Mansur, 2013; Chan, Li, and Zhang, 2013; Dechezleprêtre and Sato, 2017; De Jonghe, Mulier, and Schepens, 2020; Bolton, Lam, and Muûls, 2023; Martinsson, Sajtos, Strömberg, and Thomann, 2024). Our study shows that carbon pricing programs impact firms' performance and drive a reconfiguration of production networks to circumvent the uncertainty stemming from the regulations.

2 Institutional background

2.1 The California cap-and-trade program

There is a broad consensus among economists that the most effective way to reduce global carbon emissions is through carbon pricing programs. As a result, many jurisdictions have implemented carbon taxes or cap-and-trade systems to promote a transition to a low-carbon economy.

In line with this approach, California enacted Assembly Bill 32 in 2006, setting a target to reduce emissions to 1990 levels by 2020. The California cap-and-trade program was introduced in January 2013 as part of this commitment. During its first phase, from 2013 to 2014, the program focused on electricity generation (including imports) and industrial facilities that emit more than 25,000 tons of equivalent carbon dioxide (CO2) annually. In 2015, the program was expanded to include petroleum and natural gas distributors.

Under California's cap-and-trade program, regulated facilities must surrender permits corresponding to their emissions. A portion of these permits is allocated for free, while the remainder is auctioned quarterly and traded in a permits market. The specific allocation of free permits at the plant level remains undisclosed. However, according to the California Air Resources Board, from 2013 to 2017, the free allocation of permits was designed to cover, on average, approximately 85% of the emissions produced by a theoretical industrial facility under the program.[7] The requirement to surrender emission permits is based on emissions produced in previous years. Therefore, the program's effects are expected to be significantly observable only from the first year after its implementation or subsequent amendments. In 2013, the average supplier subject to the policy produced 2.6 million tons of equivalent CO2 at its California facilities. The auction price for permits was $10 per ton of CO2 equivalents in 2013, averaging approximately $12.50 between 2013 and 2017.

[7]For these calculations, the California Air Resources Board assumes constant output and emission efficiency (refer to https://ww2.arb.ca.gov/our-work/programs/cap-and-trade-program/allowance-allocation).

2.2 Carbon leakage

The adoption of carbon policies such as the California cap-and-trade program is influenced by each jurisdiction's unique economic, political, social, and environmental circumstances. This results in considerable variation in the implementation of such policies in regions and countries. The uneven landscape of carbon policies, characterized by partial geographical coverage, creates opportunities for shifting emissions from regions with high carbon pricing to those with low or no carbon pricing, a phenomenon known as "carbon leakage".

The literature has identified several channels through which carbon leakage can occur (Cosbey et al., 2019). At the supply chain level, carbon leakage can occur via a *competitiveness channel*, which has been extensively discussed in prior research (see, for example, the comprehensive review by Fowlie, Reguant, and Ryan, 2016). According to this channel, uncoordinated climate action places suppliers subject to climate policies at a disadvantage relative to competitors, as these firms are exposed to a higher degree of climate transition risks. Downstream customers reduce this exposure to supply chain uncertainty by shifting their sourcing away from suppliers exposed to climate transition risks.

We build on this literature to identify the *competitiveness channel* of carbon leakage within the context of uncoordinated climate action in the United States. In this context, the literature suggests that customers who source from firms subject to the program could face increased supply chain uncertainty, which translates into an incentive to pre-emptively rewire their supply chains outside of California in anticipation of future disruptions (Custodio, Ferreira, and Garcia-Appendini, 2023; Ersahin, Giannetti, and Huang, 2024). This uncertainty may be driven by suppliers subject to the program experiencing a higher loss of profitability, a decrease in economic activity, an incentive to shift production away from California to less productive plants (Bartram et al., 2022), or growing financial constraints (Ivanov et al., 2024). Alternatively, it may be driven by suppliers that pass through the costs of complying with the climate policy to their customers (Cludius, de Bruyn, Schumacher,

and Vergeer, 2020).

To contextualize these drivers within the framework of the California cap-and-trade program, it is useful to analyze the 10-K forms of suppliers subject to the policy. Figure 1 illustrates the most frequent trigrams appearing in the sections of these records dedicated to discussions of the program. The figure reveals that, in these sections, treated suppliers primarily emphasize increased costs (in trigrams such as "increased compliance costs") and the pressure to decarbonize economic activity (in trigrams such as "GHG reduction initiatives"). They also express uncertainty (in trigrams such as "could negatively impact") and refer to the possibility of passing costs on to final customers (in trigrams such as "cost contractual terms" or "recover increased costs").

Suppliers affected by the California cap-and-trade program face multiple challenges, including the cost of emission allowances and a policy-induced increase in electricity and transportation costs (as of 2015). In addition, they face pressure to decarbonize production, which can lead to stranded assets, along with uncertainties surrounding policy adoption. For example, the filing of the climate change lawsuit *California Chamber of Commerce v. State Air Resources Board* at the time of the policy aimed to invalidate the program along with the auction of emission allowances.[8] In addition, firms face uncertainty about future compliance costs, although volatility in the price of emission allowances has been relatively low relative to other cap-and-trade programs, such as the EU ETS. Thus, uncertainty about the price of emission allowances probably plays a minor role, since the price of emission allowances remained close to its $10 floor during the initial phase of carbon trading, as shown by Fuchs, Stroebel, and Terstegge (2024). Overall, faced with the risk of future supply chain disruptions or higher input costs, downstream customers may have the incentive to switch to suppliers with lower exposure to climate transition risks.

The regulatory bodies overseeing the California cap-and-trade program recognize risks

[8]In 2017, the California Supreme Court denied the petitions. For more details, see https://climatecasechart.com/case/california-chamber-of-commerce-v-california-air-resources-board/).

arising from uncoordinated climate actions in the United States. To mitigate risks of carbon leakage, the California cap-and-trade program provides exemptions for certain highly exposed entities. Fowlie et al. (2016) estimate the risk of carbon leakage in international trade flows due to incomplete climate policies affecting energy prices in California. This risk is particularly significant in the energy-intensive and trade-exposed industrial sectors. Moreover, a substantial body of literature has examined the potential impact of border carbon adjustment mechanisms designed to level the playing field between firms operating under different climate policies (Fowlie, Petersen, and Reguant, 2021).[9]

Building on this conceptual framework, we conduct an empirical analysis using supply chain data. We investigate how the introduction of California's cap-and-trade program impacts the likelihood of firms regulated by the program losing customers to competitors outside the regulated area and establishing new business relationships.

3 Methodology and data

This section outlines our empirical strategy, describes the sample, and defines the variables.

3.1 Empirical strategy

Our primary empirical strategy examines whether the introduction of the California cap-and-trade program has led to a rewiring of supply chains outside the state's boundaries. Specifically, we test whether suppliers subject to the program are more likely to have their customer relationships terminated vis-à-vis otherwise similar suppliers located in California not subject to the program.

To determine whether changes in customer relationships are attributable to the cap-and-

[9]Other channels include the market channel, where reduced demand for carbon in regulated jurisdictions lowers global fossil fuel prices, thereby increasing demand in unregulated areas (e.g., Harstad, 2012); the income channel, where carbon taxes alter relative prices and wages across regions, impacting the terms of trade and potentially leading to mixed effects on consumption and emissions outside the regulated area; and the technology spillover channel, where higher carbon prices incentivize investment and innovation in low-carbon technologies, ultimately reducing emissions.

trade program, our methodology exploits both the program's implementation timing and the suppliers it affects. We employ a difference-in-differences approach comparing changes in the likelihood of relationship termination between treatment and control groups around the program's enactment in 2013 (the treatment year). Specifically, we estimate the following linear probability model at the pair-year level:

$$Ending_{i,j,t+1} = \beta\, Treated_i \times Post_t + \gamma_1' X_{i,t} + \sigma_t + \mu_i + \eta_{j,s(i)} + \epsilon_{i,j,t}, \tag{1}$$

where $Ending_{i,j,t+1}$ is a dummy variable that takes the value one if the relationship between supplier i and customer j ends in a given year (i.e., it is not observed in year $t+1$), and zero if the relationship is observed in year t and continues into $t+1$. We account for mergers and acquisitions, delistings, and bankruptcies by excluding supplier-customer pairs that end their relationship in the same year as one of the firms exits the Compustat sample. $Treated_i$ is a dummy variable that takes the value of one for suppliers i that have at least one facility in California that reports emissions above 25,000 tons of CO2 equivalents at the beginning of the EPA emission disclosure policy in 2010, and zero for suppliers i that have at least one facility in California that reports emissions below 25,000 tons of CO2 equivalents in the same year. We exclude suppliers with multi-plants in California where at least one of them reported emissions above the threshold. $Post_t$ is a dummy variable that takes the value of one in 2013 and subsequent years, and zero otherwise.

$X_{i,t}$ includes controls for suppliers' size (logarithm of total assets), profitability (return on assets, ROA, defined as the ratio of EBITDA to assets), leverage (total debt-to-assets ratio), Tobin's Q, and R&D stock (ratio of the sum of past R&D expenditures to assets). These controls address potential correlations between treatment status and firm characteristics (e.g., firms with a larger business activity in California might exhibit unique features like higher profitability) and with the outcome variable (e.g., it may be less desirable to terminate relationships with highly profitable firms). In our preferred specification, the baseline model

is saturated with year fixed effects σ_t, supplier fixed effects μ_i, and customer-by-supplier sector fixed effects $\eta_{j,s(i)}$. Robust standard errors are clustered at the supplier level.

We address concerns about reverse causality by defining our *Treated* dummy variable based on data from 2010, three years before the implementation of the California cap-and-trade program. Assuming that the pre-program supply chain configuration of the firms was optimal, there would be little incentive for the firms to alter their supply chains before the program was implemented. By setting the treatment date well in advance of the program's start, we adopt a conservative approach: if some firms in the treatment group reduce their emissions below the regulatory threshold between 2010 and 2012, thereby becoming exempt from the program, any observed negative effect would be diminished, resulting in a weaker estimated effect. Similarly, if firms in the control group increased their emissions above the threshold before the program was implemented, our estimates would likely be attenuated.

Our main coefficient of interest is β. Including customer-by-supplier sector fixed effects, this coefficient captures the variation in the change in the probability of termination between suppliers affected by the program (treated) and those not subject to the program (control) who operate in the same sector and have a business relationship with the same customer. This design reduces the likelihood that changes in customer-specific demand or supplier-specific characteristics drive our results.[10] Therefore, in our preferred specification, the estimated difference in the probability of termination can be reasonably attributed to the heterogeneous exposure to the cap-and-trade observed in the supply-side of the business relationship. If the program places affected suppliers at a disadvantage in relation to their competitors, we expect β to be positive. Our identification strategy relies on the assumption of parallel trends, meaning that, in the absence of the policy, the probability of terminating a relationship would

[10]The within-firm strategy mirrors approaches in the banking literature that compare banks for the same borrower to assess the impact of bank liquidity shocks (Khwaja and Mian, 2008). Ivashina et al. (2022) and Paravisini et al. (2023) highlight that this method assumes that credit demand is not specific to individual banks. In our context, this means that customer purchasing behavior is not tied to specific suppliers, which is more likely when suppliers belong to the same industry.

have been the same for both treated and control suppliers. This assumption is discussed in detail in Section 4.

3.2 Data sources and sample

To assess the effects of the California cap-and-trade program on the supply chain, we combine data from three sources: supply chain relationships and firm competitors from FactSet Revere, facility-level carbon emissions from the U.S. Environmental Protection Agency (EPA), and firm-level financial data from Compustat.

We obtain information on supply chain relationships and competitors from FactSet Revere, which primarily covers publicly listed companies' business relationships worldwide since 2003. This data set gathers information on supply chain connections and direct business competitors from various sources, including SEC 10-K annual filings, investor presentations, and press releases.

We collect facility-level emissions data from the U.S. Environmental Protection Agency (EPA). Since October 2009, the EPA has maintained the Greenhouse Gas Reporting Program (GHGRP), which provides data on scope 1 emissions from all U.S. establishments emitting 25,000 tons or more of CO2 equivalents annually. These data are publicly accessible through the Program's Facility Level Information on Greenhouse Gases Tool (FLIGHT). We manually merge facility-level emissions data with firm-level information from FactSet Revere using the parent company names provided by the EPA. When parent names do not match, we use facility names to merge.

We obtain financial data, along with information on the industries of both customers and suppliers, from the Compustat North America and Global Fundamentals database. We link Compustat data with FactSet Revere using FactSet IDs and PERMCO identifiers obtained through the Center for Research in Security Prices (CRSP).

Finally, we verify if suppliers have establishments in California that emit less than 25,000

tons of CO2 equivalents annually by relying on the National Establishment Time-Series (NETS) database. The NETS database provides detailed information on the location of the universe of U.S. establishments. Specifically, we hand-match headquarters companies from NETS to Compustat North America and Global Fundamentals database to obtain a mapping of each establishment's parent company, which allows us to identify our control group of suppliers.

Our initial sample consists of a panel of 166,525 supplier-customer pair-year observations from FactSet Revere spanning the period from 2010 to 2017. The sample includes suppliers whose financial information is available in Compustat and excludes supplier-customer pairs that end their relationship after the year in which one of the firms leaves the Compustat sample. This sample period enables us to analyze the evolution of supply chains starting from the beginning of the EPA emissions reporting (i.e., three years before the California cap-and-trade program's implementation in 2013), and continuing through four years post-implementation. The sample includes 1,490 unique suppliers and 17,329 unique business customers, of which 59 suppliers have at least one facility in California subject to the cap-and-trade program (the treated suppliers). The treated suppliers group includes only U.S. firms. These treated suppliers have a total of 2,304 unique customers. The remaining 1,431 suppliers are not subject to the program. Using data from FactSet Revere, we also identify the direct competitors of the treated suppliers.

Figure 2 illustrates the structure of our data set using the example of a treated firm, U.S. Steel, which owns a facility near San Francisco with emissions sufficient to be regulated under the cap-and-trade program. U.S. Steel and its direct competitors, such as Steel Dynamics, NUCOR, and AK Steel, supply Worthington Industries with products. In our preferred analysis, we compare the time evolution of supplier-customer pairs involving a treated supplier (e.g., the pair U.S. Steel-Worthington Industries) with pairs involving a competitor of the treated supplier selling to the same customer (e.g., NUCOR-Worthington Industries).

Table A1 in the Appendix provides the definitions of all other variables used in our study.

3.3 Summary statistics

Table 1 presents summary statistics for the initial (unmatched) sample. Panel A indicates that, on average, about one in five supplier-customer relationships are terminated each year (i.e., the average probability of termination of an existing relationship is 21%). The maximum duration of supplier-customer relationships in our sample is 15 years, with an average duration slightly exceeding three years. This suggests that the time window of our study, three years before the implementation of the cap-and-trade program and up to four years after it, is adequate to capture both dynamic effects and their persistence, while minimizing potential confounding factors.

4 Results

In this section, we present the results of the baseline difference-in-differences regression that examines the restructuring of supply chains in response to the California cap-and-trade program. We then discuss the underlying identification assumptions and perform robustness checks to validate our baseline findings. Finally, we explore alternative interpretations for our baseline results.

4.1 Business relationship termination

Table 2 shows the results of the estimation of equation (1) in our baseline sample with different sets of control variables. The regression in column (1) includes supplier, customer-by-supplier sector, and year fixed effects. The coefficient for the interaction term $Treated \times Post$ is positive and statistically significant at the 1% level, suggesting a higher likelihood of terminating a relationship for treated suppliers than for control suppliers. This estimate is our preferred specification and it indicates that the cap-and-trade program increases the likelihood of a customer ending its relationship with a policy-affected supplier by 11.4 percentage points

compared to a similar supplier not subject to the program. This coefficient is both statistically and economically significant, indicating a 28% increase relative to the standard deviation of this probability, which is 41% in the sample (see Table 1). Moreover, this coefficient corresponds to a 54% increase relative to the average unconditional probability of terminating a supplier relationship, which is 21% in the same sample.

One concern with the specification in column (1) is that the results could be driven by different time-varying supplier-specific characteristics correlated with the treatment. In column (2), we address this concern by estimating an alternative specification with supplier-level financial controls. Results remain economically and statistically significant, supporting the comparability of treated and control suppliers in our empirical setup.

Another concern is that customer-level shocks, rather than suppliers' exposure to the cap-and-trade program, may be driving our results. We address this concern by excluding from the sample customers that are subject to the cap-and-trade. In column (3), we exclude from the estimate in column (2) all customers with facilities in California that emit more than 25,000 tons of CO2 equivalents (i.e., treated customers). This exclusion reduces the concern that the observed effect is driven by treated customers instead of treated suppliers. The estimated economic impact is consistent with that in column (1), suggesting that the exclusion restriction is valid in our preferred baseline empirical strategy.

Furthermore, to address further concerns that demand-driven shocks may drive our results, we estimate an alternative specification with customer-by-year fixed effects. This setup allows for comparisons between treated suppliers and control suppliers that sell to the same customer in a given year, but it requires restricting the sample to suppliers with at least two business relationships every year, reducing our sample by approximately 30%, since customers with only one supplier in a given year are excluded.

Restricting the estimation sample to customers with multiple suppliers could potentially bias upward the estimates of our key coefficient. For instance, this subsample may select

customers that find it relatively easier to discontinue their relationship with treated suppliers due to their pre-existing ties with alternative suppliers. However, when we restrict the sample in column (3), employing the same specification as in column (2), the coefficient of the interaction term, *Treated* × *Post*, is 0.100 and statistically significant at the 1% level, closely matching the estimate from column (1).

Introducing customer-by-year fixed effects could also potentially bias downward the estimates of our key coefficient. For instance, cutting ties with treated suppliers may be unnecessary for customers that could flexibly increase the (unobserved) quantity of input sourced from their comparable alternative non-treated suppliers in a given year. Column (5) presents the interaction term coefficient using the specification with customer-by-year fixed effects and the restricted sample. The coefficient remains positive and statistically significant at the 10% level and, compared to column (4), it is lower at 0.038, or equal to an 18% increase, compared to the unconditional probability of terminating a relationship in the sample, equal to 21%. This range of the economic magnitude of the impacts of climate transition risks on supply chain relationships is in line with the effects observed in the literature that studies climate risks and supply chain relationships. Pankratz and Schiller (2023), for instance, estimate that suppliers exposed to heat days exceeding their customers' expectations are 28% more likely than other suppliers to experience the termination of pre-existing customer relationships in the same year relative to the unconditional probability of terminating relationships, which is higher, at 27% in their sample.

4.2 Identifying assumption

In this section, we test the validity of the identifying assumption underlying our empirical approach. Figure 3 displays the dynamic difference-in-differences effect before and after the introduction of the cap-and-trade program. The coefficients of the interaction term are not statistically different from zero in the years preceding the introduction of the policy, suggest-

ing that there are no preexisting differential trends between treated and control suppliers. The figure also indicates that treated suppliers see an increase in the probability that their pre-existing customer relationships are terminated after the program is implemented. After the introduction of the cap-and-trade, the coefficient is statistically significant and relatively flat around 10 percentage points.

To further corroborate the validity of our identifying assumption, in the Internet Appendix we study the annual financial performance of the suppliers in our sample from 2010 to 2017. Our identification strategy relies on the assumption that customers of the treated suppliers did not reconfigure their supply chains in anticipation of the cap-and-trade policy's implementation. Figure 4 shows that these suppliers' revenues, total assets, investment ratio and capital expenditures only decreased significantly after 2013, suggesting that their customers did not reduce their business activity with the treated suppliers in anticipation of the policy. Additionally, Figure IA1 in the Internet Appendix shows that suppliers might have acted on their expectation of rising costs due to the cap-and-trade program by significantly increasing their inventories during the pre-treatment period between 2010 and 2012, relative to 2013. These findings indicate that the rewiring away from suppliers subject to the cap-and-trade occurred only after the introduction of the policy.

4.3 Robustness and extensions

Our results so far indicate that the introduction of the California cap-and-trade program leads to an increased probability that treated suppliers experience termination of customer relationships compared to control suppliers. In this section, we test the robustness of our baseline results using alternative outcomes, samples, and specifications.

Generalized difference-in-differences. If the cap-and-trade program drives the observed effects, we would expect the likelihood of terminating relationships with treated firms to be

more pronounced for suppliers with higher emission levels in California. Since the intensity of a supplier's treatment is determined by its emissions in California, and the associated costs for affected firms increase with their emissions, we test the robustness of our baseline estimates using a continuous treatment variable that reflects firms' emissions levels in California (above 25,000 tons of CO2 equivalents). To do this, we estimate equation (1) by replacing the dummy variable $Treated_i$ with a continuous treatment variable $Treatment\ Intensity_i$. This measure is calculated as the ratio of a firm's total emissions in California above 25,000 tons of CO2 equivalents in 2010, divided by the firm's total assets. For ease of interpretation, this measure is standardized by dividing it by its standard deviation. Suppliers in the control group are assigned a value of zero. Consistent with our baseline results, Table IA1 in the Internet Appendix reports positive and statistically significant coefficients for the interaction term, $Treatment\ Intensity \times Post$, indicating that the probability of terminating a relationship with a treated supplier increases with the intensity of the supplier's emissions in California. A one-standard-deviation increase in emissions intensity in California is associated with a 1 percentage point higher likelihood of terminating pre-existing customer relationships with treated suppliers compared to control suppliers.

Alternative standard error clustering. In our baseline specification, we cluster standard errors at the supplier level to account for the correlation of standard errors for a given supplier over time. This approach is motivated by the fact that treatment is assigned at the supplier level at the beginning of the sample period. However, we also perform robustness checks using standard errors clustered at the pair level, consistent with similar empirical strategies in the literature (e.g., Pankratz and Schiller, 2023). Column (1) of Table IA2 in the Internet Appendix indicates that our results remain robust to this alternative clustering approach, using the specification in column (1) of Table 2.

Alternative treatment dummy variable. In Table IA2, column (2), we present baseline results using an alternative method for allocating suppliers into treatment and control groups. In this alternative approach, treated suppliers are defined as those with facilities in California emitting more than 25,000 tons of CO2 equivalents in 2012 (one year before the program's introduction) rather than in 2010, as in our baseline case. These treated suppliers are matched with control suppliers using the same matching and propensity score weighting approach as in the baseline estimation. In particular, approximately 80% of the treated observations under the baseline definition are also treated in this alternative approach. Although selecting treated suppliers based on emissions from the year before the event raises concerns about potential biases due to anticipatory effects, the baseline results remain robust under this alternative specification.

Within group of competitors. Table IA2 column (3), presents the estimates of the baseline specification after matching each treated supplier with its group of direct competitors, including suppliers headquartered outside the U.S. The effect of interest remains statistically significant and economically meaningful.

Suppliers geographically concentrated in California. Table IA2 column (4), presents the estimates of the baseline specification after excluding geographically diversified treated suppliers from the sample. To identify these suppliers, we replicate the geographical diversification measure at the firm level used by Bartram et al. (2022). Suppliers geographically concentrated in California lack the ability to easily mitigate their exposure to the cap-and-trade program by shifting production to facilities outside of California. The results indicate that geographically concentrated suppliers face a 4.9 percentage point higher increase in the probability of terminating a customer relationship compared to control suppliers. This suggests that our findings are not driven solely by the within-firm reallocation of production to

facilities outside of California.

Financially unconstrained suppliers. Table IA2 column (5), presents the estimates of the baseline specification for a sub-sample consisting exclusively of treated suppliers that are not financially constrained. To identify these suppliers, we replicate the approach proposed by Bartram et al. (2022), classifying firms as financially constrained if they meet at least four of the six financial constraint measures (i.e., firm size, dividend payout, short- and long-term debt rating, the index introduced by Kaplan and Zingales, 1997, the index introduced by Whited and Wu, 2006, and the index introduced by Hadlock and Pierce, 2010) calculated before the start of the sample period (between 2003 and 2008). This approach allows us to focus on treated suppliers that are less likely to significantly relocate their economic activities to facilities outside of California, aligning with previous evidence of carbon leakage among financially constrained suppliers (Bartram et al., 2022). If within-firm carbon leakage were the sole driver of our results, we would not expect to find a significant effect in the sub-sample of treated suppliers that are not financially constrained. The results confirm that the baseline findings remain robust even within this sub-sample.

Placebo test. Table IA2 column (6), presents the baseline results using an alternative treatment group. Suppliers are included in this placebo group if they have headquarters in California but do not have facilities in the state that emit more than 25,000 tons of equivalent CO2 in 2010. For instance, suppliers in this placebo group are "tech firms" headquartered in California such as HP Inc. and DSP Group. These placebo suppliers are matched to other non-treated suppliers using the same propensity score weighting approach employed in the baseline estimation. The results indicate that suppliers in the placebo group do not experience a significant increase in the probability of having their pre-existing customer re-

lationships terminated following the introduction of the cap-and-trade program.[11]

Supply chain turnover. Throughout our analysis, we focus on a large and representative sample of supplier-customer relationships and study the extensive margin of such relationships. However, we also perform a robustness test by replicating our analysis over a relatively smaller sample of significant supplier-customer relationships based on the Statements of Financial Standards 14 and 131, which require publicly listed U.S. firms to disclose the identity of their customers that represent at least 10% of their total sales.[12] Column (7) of Table IA2 studies the intensive margin of these relationships by using the logarithm of the sales of a given supplier to each of its reported customers in a given year as a dependent variable. The estimates indicate that for each customer, sales from treated suppliers experience a greater drop than sales from control suppliers after the introduction of the cap-and-trade program. Column (8) studies the extensive margin of these major relationships and rejects the hypothesis that, in this sample, supply chains are more likely to rewire away following the cap-and-trade. Overall, this suggests that major relationships seem to become less important but are not more likely to terminate.

Conditional logit. Table IA2 column (9), presents the estimates of the baseline specification using a conditional logit estimator. This approach addresses the concern that using a linear probability model might result in predicted probabilities falling outside the 0-1 interval. For this specification, we use customer-by-year fixed effects and supplier-industry fixed effects. Due to computational constraints, we omit supplier fixed effects, and the regression is estimated on our matched baseline sample (using propensity score matching). The results

[11] Although these suppliers are headquartered in California and may be affected by the cap-and-trade-induced increase in electricity prices at their headquarters, they do not directly incur all the costs associated with the new climate policy. As a result, their customers are less likely to have the incentive to reconfigure their supply chains away from these placebo-treated suppliers.

[12] This data is available in the Compustat Segment database. We extract supply chain relationships for the period 2010–2017.

are consistent with the baseline estimates, with the coefficient of interest remaining positive and statistically significant.

Business relationship initiation. We begin by examining possible alternative interpretations of our baseline results. So far, we interpret our baseline results as evidence suggesting a rewiring of supply chains away from the cap-and-trade program. However, if different customers began new relationships with treated suppliers, our baseline results could indicate a shift in the customer base of treated suppliers rather than a broader rewiring of supply chains outside of the California cap-and-trade system. To explore this alternative interpretation of our results, we examine the probability of initiating new relationships with treated suppliers, relative to any other possible supplier in the same sector a customer may source from in a given year. We use a difference-in-differences approach on an augmented sample. Specifically, for each customer in our baseline sample, we include every supplier operating in the same sector as other suppliers with whom the customer has an active business relationship in a given year. We estimate the following linear probability model:

$$Beginning_{i,j,t} = \beta\, Treated_i \times Post_t + \gamma_1' X_{i,t} + \sigma_t + \mu_i + \eta_{j,s(i)} + \epsilon_{i,j,t}, \tag{2}$$

where $Beginning_{i,j,t}$ is a dummy variable that equals one if a relationship between supplier i and customer j is observed in year t for the first time, and zero if a relationship between supplier i, in the same sector $s(i)$ as other existing suppliers of customer j is not observed in year t.

Table IA3 in the Internet Appendix shows that suppliers affected by the introduction of the cap-and-trade program are not more likely to establish new relationships with customers. Rather, we find some evidence consistent with treated suppliers experiencing a reduction in the probability of starting new relationships after the event. Overall, these findings reinforce our interpretation that supply chains are rewired in avoidance of the California cap-and-trade program.

5 Economic mechanisms

So far, we have established a link between the introduction of the California cap-and-trade program and the restructuring of supply chain networks away from suppliers exposed to the policy. In this section, we explore potential economic mechanisms that could be driving this reorganization of supply chains.

5.1 Switching costs

When faced with the introduction of climate policies that affect their suppliers, customers are arguably more likely to shift their supply chains away from suppliers exposed to climate transition risks when the switching costs faced are relatively lower. This may occur when affected suppliers operate in more competitive industries and when input specificity is relatively lower. In such cases, customers can avoid possible supply chain disruptions arising from the suppliers' exposure to climate transition risks by selecting among a large number of substitute suppliers that are not subject to climate policies.

We first examine this economic mechanism underlying the rewiring of supply chains away from the cap-and-trade by re-estimating our baseline regression on sub-samples defined by different levels of product market competition. Table 3, panel A, presents the baseline coefficient estimates for sub-samples characterized by a pre-treatment Herfindahl-Hirschman Index (HHI) concentration above or below the median. The HHI is calculated at the three-digit SIC code level in columns (1) and (2). In columns (3) and (4), we focus on sub-samples of suppliers above and below the median Lerner Index—computed as the average ratio of net income over sales (at the three-digit SIC code level), capped between zero and one. Columns (5) and (6) provide estimates for suppliers exposed to a pre-treatment number of competitors in their matched direct competitors' group below or above the median. The $Treated \times Post$

coefficient always remains statistically and economically significant for suppliers operating in more competitive industries. In fact, the effects are more pronounced in industries with a lower HHI and with more competitors.

Secondly, we examine this economic mechanism investigating the role of input specificity, which may similarly influence the restructuring of supply chains. Industries producing standardized inputs, which allow for easier substitution, are more likely to see the termination of business relationships when faced with increased physical climate risks (Barrot and Sauvagnat, 2016) or transition climate risks. Furthermore, this effect is expected to be concentrated among customer-supplier pairs with weaker ties, where customers are less likely to be locked in their existing relationships with suppliers.

To assess the role of switching costs in the rewiring of supply chains away from the cap-and-trade, we estimate equation (1) but split the sample based on the presence of high and low switching costs. In Panel B of Table 3, we categorize the sample according to the characteristics of the supplier: differentiated vs. standardized goods, based on the two-digit SIC code classification proposed by Giannetti, Burkart, and Ellingsen (2011), above-median vs. below-median R&D stock-to-assets ratio, and the duration of the relationships (above vs. below median duration).

Consistent with the proposed economic mechanism, Panel B of Table 3 shows that the probability that treated suppliers experience the termination of customer relationships is concentrated among suppliers that produce standardized goods (column (2)) and those with a below-median investment in innovation as proxied by R&D stock (column (4))[13] and those with relationships with below-median duration (in column (6)). Establishing and maintaining customized supplier-customer relationships is costly, indicating that firms face important decisions regarding establishing or terminating these relationships.

[13]Notably, approximately 86% of the customers in our sample have at least one standardized supplier.

5.2 Real effects

Next, we refine our analysis of the economic mechanisms underlying our results by examining the financial performance of affected suppliers. As indicated by anecdotal evidence from affected suppliers' SEC filings (see Section B of the Internet Appendix), the introduction of the cap-and-trade program appears to place these suppliers at a competitive disadvantage.

To test this hypothesis with our data, we collapse our customer-supplier pair-year panel into a supplier-year panel and estimate the following regression model:

$$Y_{i,t+1} = \beta_1 \ \textit{Treated}_i \times \textit{Post}_t + \beta_2 \ \textit{Differentiated}_i \times \textit{Treated}_i \times \textit{Post}_t + \mu_i + \mu_{s(i),t} + \epsilon_{i,t}, \quad (3)$$

where $Y_{i,t+1}$ is the supplier's output (logarithm of revenues), the average cost of sales (COGS-to-lagged total assets ratio), size (logarithm of total assets), change in tangible assets (change in PPE-to-lagged total assets ratio), capital expenditures (logarithm of capex) and profitability (ROA). $\textit{Differentiated}_i$ is a dummy variable that takes the value of one for suppliers that produce differentiated goods following Giannetti et al. (2011). The regressions include supplier fixed effects (μ_i) and supplier industry-by-year ($\mu_{s(i),t}$) fixed effects.

Table 4 presents the results. Column (1) shows that the revenues of the treated suppliers are relatively lower by a statistically significant 11% after the introduction of the cap-and-trade program. Furthermore, column (2) shows that the reduction in revenues is about 14% and statistically significant among suppliers producing standardized inputs, while it is not statistically significant among the less affected suppliers producing differentiated inputs. Columns (3) and (4) does not provide evidence of an increase in the average cost of sales for affected suppliers. Although our measure does not directly capture suppliers' marginal costs, this result is consistent with the low realized direct costs of the initial phase of the program and with the nature of the event being rather consistent with an increase in climate transition risks. Columns (5) and (6) show that treated suppliers experience a significant differential reduction in assets relative to control suppliers of 12% after the policy's implementation,

driven primarily by those producing standardized inputs with a reduction of 21%.

Columns (7) to (10) show that suppliers of standardized inputs reduce the investment rate (measured as the change in PPE divided by total assets) and capital expenditures by 2.2 percentage points and 26%, respectively. While columns (11) and (12) do not indicate a significant decline in profitability (ROA) for affected suppliers, this is possibly due to the significant observed reduction in total assets. Overall, these findings confirm that the cap-and-trade program negatively affected treated suppliers, especially when their customers face relatively low switching costs to alternative options. In contrast, suppliers in industries that produce differentiated goods appear to be more able to maintain customer relationships, helping to explain the observed differential effects between treated and control suppliers.

We conclude the analysis of the economic mechanisms underlying the rewiring of supply chains following the introduction of the program by investigating whether the climate transition risk shock propagates through the production network to customers indirectly exposed to the program. If customers can promptly and effectively terminate their relationships with suppliers subject to the program, they should be shielded from the shocks propagation. Furthermore, we do not anticipate that customers will incur significant financial losses when switching to a different supplier after the program's introduction, as we observe that the termination of supplier relationships predominantly occurs when switching costs are relatively low.

We collapse the customer-supplier pair-year panel into a customer-year panel, excluding customers directly affected by the cap-and-trade and estimate the following regression model:

$$Y_{j,t+1} = \beta \textit{Has Treated Supplier}_j \times \textit{Post}_t + \eta_j + \eta_{s(j),t} + \epsilon_{j,t}, \tag{4}$$

where $Y_{j,t+1}$ is the customer's output (logarithm of revenues), the average cost of sales (COGS-to-assets ratio), size (logarithm of total assets), change in tangible assets (change in PPE-to-assets ratio) and profitability (ROA). $\textit{Has Treated Supplier}_j$ is a dummy variable that takes the value of one if customer j has at least one treated supplier after the introduction of the

program and zero otherwise. We saturate the specification using customer fixed effects η_j and industry-by-year fixed effects $\eta_{s(j),t}$.

Table 5 presents the results. We find that customers exposed to treated suppliers do not experience a significant decrease in revenues or an increase in the average cost of sales. Furthermore, these affected customers do not reduce their assets, while there is evidence of these customers reducing their investment rate, without significantly cutting capital expenditures, they do not experience a significant deterioration in their profitability. Overall, the results indicate that customers can promptly and effectively realign their supply chain by moving away from suppliers subject to the program, especially if they face low switching costs.

5.3 Customers' climate transition risks

We have presented evidence that competitiveness in product markets and switching costs prompt a restructuring of supply chains away from suppliers affected by the cap-and-trade program. In this section, we explore whether customers who exhibit greater environmental awareness or who anticipate future regulation of supply chain emissions may prefer to maintain relationships with affected suppliers to potentially benefit from reductions in their scope 3 emissions.

To test this hypothesis, we analyze sub-samples of customers above vs. below the pre-treatment industry-level median of climate change attention, as measured by Sautner et al. (2023).[14] Table 6 presents the estimates of the regression in equation (1) for these subsamples. Columns (1) and (2) show that supply chain restructuring is predominantly observed among customers with less environmental awareness.

We further investigate this issue using alternative firm-level proxies for exposure to climate transition risks. In columns (3) and (4), we examine the effects within sub-samples of customers headquartered in states classified as Democrat or Republican based on the

[14] This measure is based on the frequency of climate change-related language in discussions between analysts and management during earnings calls defined at the firm-year level. Data are available at: `https://osf.io/fd6jq`.

2016 presidential elections, and in columns (5) and (6) for states that are either members or non-members of the U.S. Climate Alliance coalition (as of 2017). These measures serve as proxies for the sensitivity to these states' environmental issues, particularly climate transition risks. Our estimates in columns (3) and (4) confirm that the rewiring of the supply chain is concentrated among customers headquartered in Republican states, which generally exhibit lower sensitivity to environmental issues. Columns (5) and (6) also indicate that customers headquartered in states that are not part of the U.S. Climate Alliance are more likely to restructure their supply chain.

6 Environmental effects

This section explores the environmental implications of supply chain-driven carbon leakage resulting from uncoordinated climate action.

Firstly, we construct emission measures at the firm and supply chain level to perform this analysis. We aggregate each supplier's facility-level emissions data from the EPA database for all U.S. facilities for the firm-level measures. We then create a measure of the emission intensity, defined as total emissions normalized by the firm's assets. We obtain U.S.-wide EPA emissions data for 501 suppliers included in FactSet Revere between 2010 and 2017.

Secondly, we construct supply chain-level emissions for each customer in our sample. We use our supply chain linkages data to compute the logarithm of the total and average emissions and the average emission intensity (emissions over total assets) of the direct suppliers of each customer in a given year.

Our self-constructed measures of supply chain emissions allow us to avoid the known quality issues associated with inferred scope 3 emissions data (Aswani, Raghunandan, and Rajgopal, 2024), which researchers often rely on due to the limited availability of compre-

hensive corporate scope 3 emissions reporting.[15]

We collapse the client-supplier pair-year panel into a customer-year panel and estimate the following regression model:

$$Y_{j,t+1} = \beta Rewired_j \times Post_t + \eta_j + \eta_{s(j),t} + \epsilon_{j,t}, \tag{5}$$

where $Y_{j,t+1}$ is one of the three alternative measures of supply chain environmental performance. *Rewired* is a dummy that takes the value of one if a customer sources its inputs from a supply chain that is rewired away from the cap-and-trade in a given year (i.e., if that year, the customer has fewer treated suppliers than in the previous year), and zero otherwise. In an alternative specification, we replace this dummy with *# Rewired Suppliers* which measures the number of treated suppliers a given customer rewires away from in a given year, or with *# Rewired Suppliers since 2010* which cumulates *# Rewired Suppliers* since 2010. We saturate the specification using customer fixed effects η_j and industry-by-year fixed effects $\eta_{s(j),t}$. Effectively, we estimate the differential carbon footprint of supply chains rewired away from the cap-and-trade program by comparing customers affected by this rewiring to "counterfactual" customers whose supply chains remained linked to suppliers subject to the program in a given year.

Table 7 shows that, following the introduction of the program, the average U.S.-wide carbon footprint of rewired supply chains is relatively higher (in columns (1), (4) and (7)) and it is positively related to the extent to which customers rewire away from California each year (in columns (2), (5) and (8)), and relative to 2010 (in columns (3), (6) and (9)). These results suggest that uncoordinated climate action may lead to a shift toward more polluting supply chains.

[15] Our proxy for supply chain emissions of each customer purposefully differs from the traditional measure of customers' scope 3 emissions. Our estimate captures the total emissions footprint of each customer's direct suppliers, allowing us to address the question of whether new suppliers selected by customers after the rewiring show a different environmental performance compared to treated suppliers. In contrast, upstream scope 3 emissions, as inferred by data providers, account for emissions produced throughout the entire supply chain of a customer. Additionally, these inferred scope 3 emissions are scaled by the volume of goods and services a customer purchases from each supplier.

7 Conclusion

Over the past decades, many countries have implemented climate policies to regulate firms' carbon emissions. However, many of these initiatives have not been coordinated across different regions and countries. In this paper, we present evidence that the rewiring of supply chains is a mechanism through which firms contribute to the practice of carbon leakage.

Using a difference-in-differences methodology, we find that suppliers subject to the California cap-and-trade policy are significantly more likely to experience the termination of pre-existing customer relationships than suppliers not subject to the program. These findings suggest a rewiring of supply chain networks away from participants in the cap-and-trade program.

Our results are more pronounced among suppliers operating in more competitive markets, those producing standardized inputs or investing less in R&D, and those maintaining weaker ties with their customers. These treated suppliers, exposed to higher climate transition risks, face a competitive disadvantage following the introduction of the cap-and-trade program, leading to a deterioration in their financial performance, particularly if they produce standardized inputs. In addition, these effects are concentrated along supply chains with customers headquartered in states with relatively weaker environmental awareness.

We also examine the environmental implications of carbon pricing policies in an uncoordinated climate action setting. Our analysis documents a worsening of the carbon footprint of rewired supply chains and it suggests that carbon leakage can partially undermine the intended positive environmental impacts of climate policies.

Our paper highlights the implications of a lack of coordination in climate policies on production networks. In the context of the EU ETS, similar concerns have led to the introduction of a Carbon Border Adjustment Mechanism. Although a comparable policy might not be feasible in the context of U.S. industrial production networks, our paper contributes to the ongoing discussion on the need for more coordinated climate action worldwide.

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Table 1: Summary Statistics

	Mean	Std. Dev.	Median	Minimum	Maximum	Obs.
Panel A: Pair-Year Data						
Ending	0.21	0.41	0.00	0.00	1.00	166,525
Starting	0.09	3.08	0.00	0.00	100.00	360,735
Treated	0.06	0.24	0.00	0.00	1.00	166,525
Supplier total assets (billion $)	38.42	90.60	4.46	0.01	531.86	166,206
Supplier ROA	0.10	0.12	0.12	-0.80	0.34	160,893
Supplier leverage	0.25	0.19	0.23	0.00	0.99	165,342
Supplier Tobin's Q	2.00	1.22	1.63	0.61	8.81	151,040
Supplier R&D stock	0.39	0.70	0.15	0.00	5.50	166,525
Relationship duration (years)	3.38	2.79	2.00	1.00	15.00	166,525
Panel B: Supplier-Year Data						
Treated	0.04	0.20	0.00	0.00	1.00	10,103
Sales (billion $)	8.17	19.64	1.64	0.00	126.72	10,079
COGS/Sales	0.75	0.90	0.65	0.06	7.98	8199
Total assets (billion $)	16.27	51.49	2.04	0.01	392.65	10,084
ΔPPE/Assets	0.01	0.05	0.00	-0.12	0.34	7,988
Capex (billion $)	0.41	1.21	0.05	0.00	9.04	9,660
ROA	0.08	0.18	0.12	-0.81	0.40	7,986
Panel C: Customer-Year Data						
Has treated supplier	0.27	0.44	0.00	0.00	1.00	15,300
Sales (billion $)	7.17	17.34	1.30	0.00	117.11	15,223
COGS/Sales	0.77	1.02	0.63	0.04	8.92	11,199
Total assets (billion $)	19.94	77.09	1.85	0.01	650.77	15,243
ΔPPE/Assets	0.02	0.07	0.00	-0.19	0.40	11,187
Capex (billion $)	0.48	1.45	0.048	0.00	106.56	14,464
ROA	0.07	0.21	0.11	-1.04	0.46	11,208
Average supplier emission intensity	167.22	323.49	51.16	0.00	1,905.77	2,180
Total supplier emissions	22,469.71	29,129.97	7,630.15	0.00	116,828.09	2,180
Average supplier emissions	12,204.97	17,719.21	3,113.82	0.00	70,806.12	2,180
Rewired	0.14	0.35	0.00	0.00	1.00	2,509
# Rewired suppliers	0.04	0.60	0.00	-5.00	4.00	2,509
# Rewired suppliers since 2010	-0.51	1.01	0.00	-7.00	3.00	2,509

This table presents the mean, standard deviation, median, minimum, maximum and number of observations for the main variables of interest. Panel A contains statistics for the entire sample of customer-supplier-year observations. This sample is obtained from merging FactSet Revere data on customer-supplier pairs, EPA data on facility-level emissions, and Compustat data on financial information about suppliers. Panels B and C contain statistics for supplier-year and customer-year observations in our base sample, respectively. The sample period is from 2010 to 2017. All continuous variables are winsorized at the 1 and 99% level. Refer to Table A1 in the Appendix for variable definitions.

Table 2: Probability of Termination and Cap-and-Trade Program: Baseline

	(1)	(2)	(3)	(4)	(5)
Treated × Post	0.114***	0.108***	0.112***	0.100***	0.038*
	(0.029)	(0.031)	(0.031)	(0.025)	(0.019)
Supplier total assets (log)		-0.015	-0.016	-0.019	-0.013
		(0.023)	(0.024)	(0.023)	(0.020)
Supplier ROA		-0.061	-0.066	-0.036	-0.030
		(0.070)	(0.072)	(0.060)	(0.059)
Supplier leverage		-0.023	-0.021	-0.013	-0.027
		(0.060)	(0.061)	(0.060)	(0.055)
Supplier Tobin Q		-0.001	-0.001	-0.003	-0.001
		(0.007)	(0.007)	(0.007)	(0.007)
Supplier R&D stock		-0.041*	-0.042*	-0.046***	-0.036**
		(0.024)	(0.025)	(0.016)	(0.015)
Excl. Treated Customer	No	No	Yes	No	No
Year FE	Yes	Yes	Yes	Yes	No
Supplier FE	Yes	Yes	Yes	Yes	Yes
Customer × Supplier Sector FE	Yes	Yes	Yes	Yes	Yes
Customer × Year FE	No	No	No	No	Yes
Obs.	150,168	130,347	125,412	101,713	101,713
R^2	0.248	0.257	0.259	0.246	0.488

This table presents difference-in-differences estimates of customer-supplier-level panel regressions using equation (1). The dependent variable *Ending* is a dummy variable that takes the value of one if the supplier-customer relationship ends in a given year, and zero if the relationship persists in both the current and previous year. *Treated* is a dummy variable that takes the value of one for suppliers that have at least one facility in California reporting emissions above 25,000 tons of CO2 equivalents in 2010. *Post* is a dummy variable that takes the value of one in 2013 and subsequent years, and zero otherwise. In column (3), customers that produce emissions above 25,000 tons of CO2e in California in 2010 are excluded. The sample consists of supplier-customer pair-year observations from FactSet Revere, with corresponding supplier financial data sourced from Compustat, in the 2010-2017 period. Supplier-customer pairs that terminate their relationship in the same year that either firm exits the Compustat sample are excluded from the sample. Robust standard errors adjusted for supplier-level clustering are reported in parentheses. Refer to Table A1 in the Appendix for variable definitions. * $p<.10$; ** $p<.05$; *** $p<.01$.

Table 3: Probability of Termination and Cap-and-Trade Program: Market Competition and Input Specificity

Panel A: Market Competition						
	(1)	(2)	(3)	(4)	(5)	(6)
	HHI		Lerner Index		Number of Competitors	
	High	Low	High	Low	Low	High
Treated × After	0.082**	0.183***	0.105***	0.112***	-0.030	0.036**
	(0.034)	(0.060)	(0.037)	(0.043)	(0.034)	(0.018)
Year FE	Yes	Yes	Yes	Yes	No	No
Supplier FE	Yes	Yes	Yes	Yes	No	No
Customer × Supplier Sector FE	Yes	Yes	Yes	Yes	No	No
Year × Competitor group FE	No	No	No	No	Yes	Yes
Supplier × Competitor group FE	No	No	No	No	Yes	Yes
Customer × Competitor group FE	No	No	No	No	Yes	Yes
Obs.	71,918	77,584	67,964	81,634	101,539	119,872
R^2	0.267	0.243	0.260	0.249	0.511	0.259
Panel B: Input Specificity						
	(1)	(2)	(3)	(4)	(5)	(6)
	Good Type		R&D Stock		Relationship Duration	
	Differentiated	Standardized	High	Low	High	Low
Treated × After	0.083	0.129***	0.079	0.082***	0.047	0.061*
	(0.075)	(0.036)	(0.089)	(0.031)	(0.029)	(0.034)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Customer × Supplier sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Supplier FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	60,405	65,843	66,820	75,432	28,159	58,207
R^2	0.234	0.252	0.229	0.279	0.326	0.277

This table presents difference-in-differences estimates of customer-supplier-level panel regressions using equation (1). The dependent variable *Ending* is a dummy variable that takes the value of one if the supplier-customer relationship ends in a given year, and zero if the relationship persists in both the current and previous year. *Treated* is a dummy variable that takes the value of one for suppliers that have at least one facility in California reporting emissions above 25,000 tons of CO2 equivalents in 2010. *Post* is a dummy variable that takes the value of one in 2013 and subsequent years, and zero otherwise. In columns (1) and (2), panel A, suppliers are in a sector with average pre-treatment SIC three-digit HHI above or below the median, respectively. In columns (3) and (4), suppliers are in SIC three-digit sectors with average pre-treatment Lerner Index above or below the median, respectively. In columns (5) and (6), suppliers have a mean pre-treatment number of matched competitors in their group below or above the median, respectively. The sample consists of supplier-customer pair-year observations from FactSet Revere, with corresponding supplier financial data sourced from Compustat, in the 2010-2017 period. Supplier-customer pairs that terminate their relationship in the same year that either firm exits the Compustat sample are excluded from the sample. In columns (1) and (2), panel B, suppliers produce differentiated or standardized inputs, respectively (according to the classification provided by Giannetti et al., 2011). In columns (3) and (4), suppliers have an average R&D over total assets throughout the pre-treatment period above or below the median, respectively. In columns (5) and (6), pairs have a pre-treatment length of relationship above or below the median. Robust standard errors reported in parentheses are adjusted for supplier-level clustering and for supplier-by-competitor group and competitor group level clustering in columns (5) and (6), panel A. Refer to Table A1 in the Appendix for variable definitions. * $p<.10$; ** $p<.05$; *** $p<.01$.

Table 4: Supplier's Real Effects of Cap-and-Trade Program

	(1) Revenues (log)	(2) Revenues (log)	(3) Cogs/Sales	(4) Cogs/Sales	(5) Assets (log)	(6) Assets (log)	(7) Δ PPE/Assets	(8) Δ PPE/Assets	(9) Capex (log)	(10) Capex (log)	(11) ROA	(12) ROA
Treated × Post	-0.108**	-0.144***	-0.034	-0.024	-0.119**	-0.213***	-0.009	-0.022*	-0.140*	-0.257**	0.006	0.003
	(0.053)	(0.039)	(0.071)	(0.045)	(0.053)	(0.041)	(0.013)	(0.013)	(0.084)	(0.103)	(0.008)	(0.008)
Differentiated × Treated × Post		0.057		-0.023		0.187**		0.029*		0.311**		0.007
		(0.092)		(0.053)		(0.076)		(0.017)		(0.145)		(0.020)
Supplier FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Supplier Sector × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	9,857	8,098	7,991	6,563	9,908	8,143	7,780	6,581	9,223	8,025	7,779	6,579
R^2	0.982	0.982	0.764	0.784	0.984	0.983	0.427	0.419	0.960	0.959	0.850	0.843

This table presents difference-in-differences estimates of supplier-level panel regressions using equation (3). The dependent variable in columns (1) and (2) is the logarithm of revenues; in columns (3) and (4), it is the ratio of COGS over lag of total assets; in columns (5) and (6), it is the logarithm of total assets; in columns (7) and (8), it is the yearly change in PPE over lag of total assets; in columns (9) and (10) it is EBITDA over lag of total assets. *Treated* is a dummy variable that takes the value of one for suppliers that have at least one facility in California reporting emissions above 25,000 tons of CO2 equivalents in 2010. *Post* is a dummy variable that takes the value of one in 2013 and subsequent years, and zero otherwise. *Differentiated* is a dummy variable that takes the value of one for suppliers in industries that produce differentiated goods, and zero otherwise (according to the classification provided by Giannetti et al., 2011). The sample consists of FactSet Revere suppliers with financial information sourced from Compustat in the 2010-2017 period. Robust standard errors adjusted for supplier-level clustering are reported in parentheses. Refer to Table A1 in the Appendix for variable definitions. * $p<.10$; ** $p<.05$; *** $p<.01$.

Table 5: Customer's Real Effects of Cap-and-Trade Program

	(1) Revenues (log)	(2) Cogs/Sales	(3) Assets (log)	(4) Δ PPE/Assets	(5) Capex (log)	(6) ROA
Has treated supplier × Post	-0.018	-0.068	-0.016	-0.008**	0.038	0.001
	(0.032)	(0.048)	(0.027)	(0.004)	(0.047)	(0.006)
Customer FE	Yes	Yes	Yes	Yes	Yes	Yes
Customer Sector × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	14,423	10,552	14,702	10,551	13,585	10,570
R^2	0.979	0.773	0.984	0.465	0.959	0.877

This table presents difference-in-differences estimates of customer-level panel regressions using equation (4). The dependent variable in column (1) is the logarithm of revenues; in column (2), it is the ratio of COGS over lag of total assets; in column (3), it is the logarithm of total assets; in column (4), it is the yearly change in PPE over lag of total assets; in column (5), it is EBITDA over lag of total assets. *Has Treated Supplier* is a dummy variable that takes the value of one for customers that have at least one treated supplier after the introduction of the cap-and-trade program. *Post* is a dummy variable that takes the value of one in 2013 and subsequent years, and zero otherwise. The sample consists of FactSet Revere customers with financial information sourced from Compustat in the 2010-2017 period. Customers directly affected by the California cap-and-trade program are excluded from the sample. Robust standard errors adjusted for customer-level clustering are reported in parentheses. Refer to Table A1 in the Appendix for variable definitions. * $p<.10$; ** $p<.05$; *** $p<.01$.

Table 6: Probability of Termination and Cap-and-Trade Program: Customers' Climate Transition Awareness

	(1)	(2)	(3)	(4)	(5)	(6)
	Climate Change Awareness		Political Party		U.S. Climate Alliance	
	High	Low	Democrat	Republican	Yes	No
Treated × Post	0.029	0.091***	-0.043	0.124***	-0.014	0.109***
	(0.028)	(0.029)	(0.033)	(0.035)	(0.031)	(0.035)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Supplier FE	Yes	Yes	Yes	Yes	Yes	Yes
Customer × Supplier sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	27,454	26,187	22,497	22,192	22,237	22,444
R^2	0.240	0.240	0.273	0.287	0.272	0.291

This table presents difference-in-differences estimates of customer-supplier-level panel regressions using equation (1). The dependent variable *Ending* is a dummy variable that takes the value of one if the supplier-customer relationship ends in a given year, and zero if the relationship persists in both the current and previous year. *Treated* is a dummy variable that takes the value of one for suppliers that have at least one facility in California reporting emissions above 25,000 tons of CO2 equivalents in 2010. *Post* is a dummy variable that takes the value of one in 2013 and subsequent years, and zero otherwise. Columns (1) and (2) are based on customers with relatively high or low average pre-treatment attention to climate change relative to the median computed at the SIC two-digit and headquarters' country level (Sautner et al., 2023 construct the proxy for customers' attentiveness to climate change. Columns (3) and (4) are based on customers with headquarters in democratic or republican states as per their 2016 elections, respectively. Columns (5) and (6) are based on the sub-sample of customers with headquarters in states members of the U.S. Climate Alliance and in the remaining states, respectively (according to the data and approach provided by von Meyerinck et al., 2021, members of the U.S. Climate Alliance at the onset of the coalition, in 2017, are California, Colorado, Connecticut, Delaware, Hawaii, Massachusetts, Minnesota, New York, North Carolina, Oregon, Rhode Island, Vermont, Virginia, and Washington; democrat states are California, Connecticut, Delaware, Hawaii, Illinois, Maryland, Massachusetts, New Jersey, New York, Oregon, Rhode Island, Vermont, Washington). The sample is based on supplier-customer pair-year observations from FactSet Revere spanning the period from 2010 to 2017. It includes suppliers whose financial information is available in Compustat and it excludes supplier-customer pairs that end their relationship in the year in which one of the firms exits the Compustat sample. Robust standard errors adjusted for supplier-level clustering are reported in parentheses. Refer to Table A1 in the Appendix for variable definitions. * $p<.10$; ** $p<.05$; *** $p<.01$.

Table 7: Environmental Effects of Cap-and-Trade Program

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Sum of suppliers emissions			Average supplier emissions			Average supplier emission intensity		
Rewired × Post	1.248***			0.942***			82.143*		
	(0.352)			(0.346)			(49.220)		
# Rewired suppliers × Post		0.311*			0.269*			26.464	
		(0.159)			(0.140)			(25.600)	
# Rewired suppliers since 2010 × Post			0.342**			0.269*			10.982
			(0.172)			(0.157)			(18.547)
Rewired	-2.047***			-1.455***			-84.997*		
	(0.333)			(0.332)			(43.327)		
# Rewired suppliers		-0.781***			-0.549***			-36.847	
		(0.153)			(0.138)			(23.147)	
# Rewired suppliers since 2010			-1.023***			-0.667***			-36.253*
			(0.171)			(0.157)			(20.849)
Customer FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Customer Sector × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	1,877	1,877	1,262	1,877	1,877	1,262	1,880	1,880	1,269
R^2	0.866	0.868	0.821	0.884	0.884	0.810	0.836	0.836	0.821

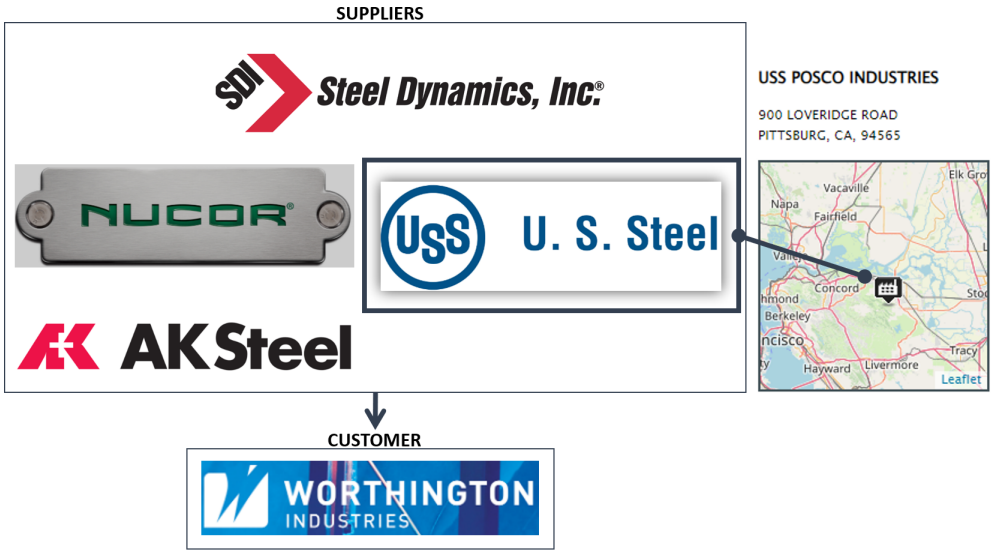
This table presents difference-in-differences estimates of customer-level panel regressions using equation (5). The dependent variable is the logarithm of the sum or average of U.S. supply chain emissions or average U.S. supply chain emission intensity of a given customer j in year t. *Rewired* is a dummy that takes the value of one if a customer sources its inputs from a supply chain that is rewired away from the cap-and-trade in a given year (i.e., if that year, the customer has fewer treated suppliers than in the previous year), and zero otherwise. *# Rewired Suppliers* is the number of treated suppliers a given customer rewires away from in a given year. *# Rewired Suppliers since 2010* is the cumulation of *# Rewired Suppliers* since 2010. *Post* is a dummy variable that takes the value of one in 2013 and subsequent years, and zero otherwise. The sample consists of FactSet Revere customers with financial information sourced from Compustat in the 2010-2017 period. Robust standard errors adjusted for customer-level clustering are reported in parentheses. Refer to Table A1 in the Appendix for variable definitions. * $p<.10$; ** $p<.05$; *** $p<.01$.

Figure 1: Frequency of Trigrams associated with the California Cap-and-Trade Program in affected Suppliers' 10-K Forms



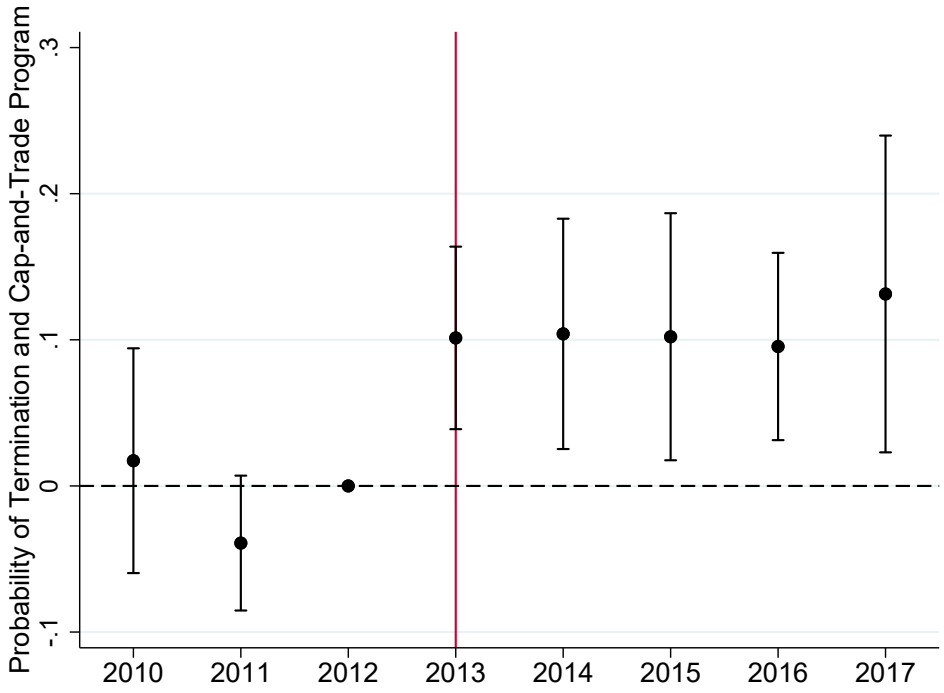
This figure illustrates the 30 most frequent trigrams that appear in the SEC 10-K Forms filed by treated suppliers in 2013 and 2014, specifically focusing on the paragraphs that discuss the cap-and-trade program. The size of each trigram is proportional to the number of times it is mentioned across the filings.

Figure 2: Illustration of the Structure of the Data



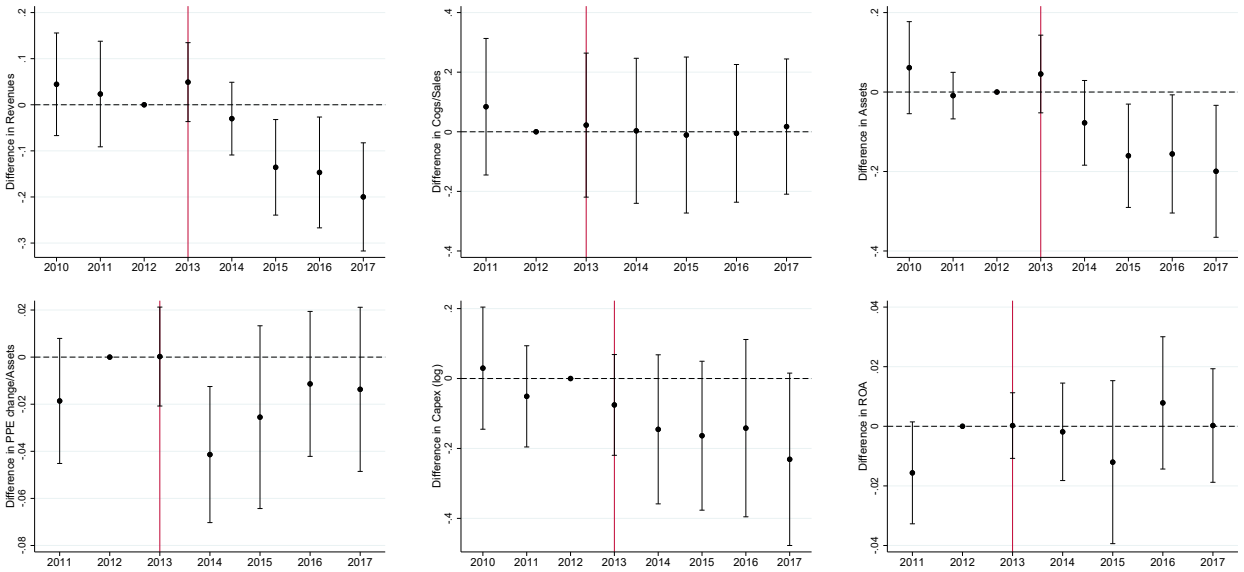
This figure illustrates the structure of the dataset through the example of the treated supplier U.S. Steel and its customer Worthington Industries. According to the EPA FLIGHT data, the U.S. Steel POSCO facility mapped close to San Francisco on the right-hand side of the figure produced sufficient emissions to be subject to the cap-and-trade. We map this facility to its owner, U.S. Steel. U.S. Steel is a supplier of Worthington Industries, visualized at the bottom of the figure, and a direct competitor of other suppliers of the same company (i.e., Steel Dynamics, NUCOR and AK Steel, visualized in the left-hand side of the figure). Our final dataset follows supplier-customer pairs such as U.S. Steel-Worthington Industries or NUCOR-Worthington Industries over time.

Figure 3: Probability of Termination around Cap-and-Trade Program



This figure shows the difference and 95% confidence intervals in the probability of termination of a business relationship between treated and control suppliers around the introduction of the Californian cap-and-trade program in 2013 using the specification in column (1) of Table 2. Treated suppliers are defined as firms with at least one facility in California reporting emissions exceeding 25,000 tons of CO2 equivalents in 2010. The sample consists of supplier-customer pair-year observations from FactSet Revere, with corresponding supplier financial data sourced from Compustat, in the 2010-2017 period. Supplier-customer pairs that terminate their relationship in the same year that either firm exits the Compustat sample are excluded from the sample. Standard errors are adjusted for supplier-level clustering.

Figure 4: Difference in Suppliers' Financials around Cap-and-Trade Program



This figure shows the difference and 95% confidence intervals in the financial outcomes between treated and control suppliers around the introduction of the Californian cap-and-trade program in 2013. The sample consists of FactSet Revere suppliers with financial information sourced from Compustat in the 2010-2017 period. Standard errors are adjusted for supplier-level clustering.

Appendix

Table A1: Variable Definitions

Variable	Definition
Ending	Dummy variable that takes the value one if the relationship between supplier i and customer j is not observed in year $t+1$, and zero if the relationship is observed in year t and continues into $t+1$ (FactSet Revere).
Beginning	Dummy variable that equals one hundred if a relationship between supplier i, in sector $s(i)$ and customer j is observed in year t but not in year $t-1$, and zero if a relationship between supplier in sector $s(i)$ and customer j is not observed in year t (FactSet Revere).
Treated	Dummy variable that takes the value of one if the supplier produced more than 25,000 tons of CO2 equivalents in one of its California facilities in 2010, and zero otherwise (EPA, FactSet Revere).
Has treated supplier	Dummy variable that takes the value of one if the customer has at least one supplier including and after 2013 that has produced at least 25,000 tons of CO2 equivalents in its California facilities in 2010, and zero otherwise (EPA, FactSet Revere).
Supplier total assets	Total assets in billion \$ (Compustat (AT)).
Supplier ROA	Ratio of earnings before interest, taxes, depreciation, and amortization to total assets (Compustat EBITDA / AT).
Supplier leverage	Long-term debt plus debt in current liabilities, divided by total assets (Compustat (DLTT + DLC) / AT).
Supplier Tobin's Q	Tobin's Q (Compustat (AT + Market Value − BE) / AT where Market Value = CSHO × PRCC F).
Supplier R&D stock	Ratio of stock of research and development (R&D) computed as the sum of R&D expenses from 2005 to year t, assuming R&D expenditures are zero when the observation is missing, divided by total assets (Compustat $\sum_{t=2005}^{t} XRD$ / AT).
Supplier revenues	Total revenues in billion \$ (Compustat REVT).
Supplier COGS / Assets	Ratio of cost of goods sold to lagged total assets (Compustat COGS / AT).
Supplier ΔPPE / Assets	Change in supplier property, plant, and equipment in levels, divided by lagged total assets (Compustat ΔPPE / AT).
Supplier capex	Supplier capex in billion \$ (Compustat CAPX).
Supplier U.S. emission intensity	Total emissions produced by the supplier in its EPA facilities located in the U.S., divided by total assets (EPA, FactSet Revere, Compustat).

(Continued)

Table A1: Variable Definitions - *Continued*

Variable	Definition
Customer total assets	Total assets in billion $ (Compustat AT).
Customer ROA	Ratio of earnings before interest, taxes, depreciation, and amortization to total assets (Compustat EBITDA / AT).
Customer leverage	Long term debt plus debt in current liabilities, divided by total assets (Compustat (DLTT + DLC) / AT).
Customer revenues	Total revenues in billion $ (Compustat REVT).
Customer COGS / Assets	Ratio of cost of goods sold to lagged total assets (Compustat COGS / AT).
Customer ΔPPE / Assets	Change in customer property, plant, and equipment in levels, divided by lagged total assets(Compustat ΔPPE / AT).
Customer capex	Customer capex in billion $ (Compustat CAPX).
Total supplier emissions	Total U.S. emissions of the suppliers of each customer (EPA, FactSet Revere).
Average supplier emissions	Average U.S. emissions of the suppliers of each customer (EPA, FactSet Revere).
Average supplier emission intensity	Average U.S. emission intensity of the suppliers of each customer (EPA, FactSet Revere).
Rewired	Dummy variable that equals one if a customer j sources its inputs from a supply chain that is rewired away from the cap-and-trade in a given year (i.e., if that year, the customer has fewer treated suppliers than in the previous year), and zero otherwise (EPA, FactSet Revere).
# Rewired Suppliers	Number of treated suppliers a given customer rewires away from in a given year (EPA, FactSet Revere).
# Rewired Suppliers since 2010	Cumulative sum of *# Rewired Suppliers* since 2010 (EPA, FactSet Revere).

Internet Appendix for

Rewiring Supply Chains Through Uncoordinated Climate Policy

A Robustness

Table IA1: Probability of Termination and Cap-and-Trade Program: Treatment Intensity

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment intensity × Post	0.010***	0.008***	0.010***	0.009***	0.006**	0.004*
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Treated × Post						0.024
						(0.020)
Supplier total assets (log)		-0.032	-0.032	-0.034	-0.019	-0.018
		(0.025)	(0.026)	(0.024)	(0.022)	(0.022)
Supplier ROA		-0.095	-0.100	-0.058	-0.036	-0.036
		(0.070)	(0.072)	(0.058)	(0.059)	(0.059)
Supplier leverage		-0.036	-0.035	-0.026	-0.040	-0.039
		(0.066)	(0.067)	(0.064)	(0.058)	(0.058)
Supplier Tobin Q		-0.001	-0.001	-0.004	-0.002	-0.002
		(0.007)	(0.006)	(0.007)	(0.007)	(0.007)
Supplier R&D stock		-0.054**	-0.055**	-0.054***	-0.034**	-0.034**
		(0.025)	(0.025)	(0.016)	(0.014)	(0.015)
Excl. Treated customer	No	No	Yes	No	No	No
Year FE	Yes	Yes	Yes	Yes	No	No
Supplier FE	Yes	Yes	Yes	Yes	Yes	Yes
Customer × Year FE	No	No	No	No	Yes	Yes
Customer × Supplier Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	132,557	116,058	111,635	88,927	88,927	88,927
R^2	0.242	0.249	0.251	0.237	0.488	0.488

This table presents difference-in-differences estimates of customer-supplier-level panel regressions using equation (1). The dependent variable *Ending* is a dummy variable that takes the value of one if the supplier-customer relationship ends in a given year, and zero if the relationship persists in both the current and previous year. *Treatment Intensity* is the ratio of each supplier's California emissions in 2010 and its total assets. This value is then standardized by its standard deviation. *Treated* is a dummy variable that takes the value of one for suppliers that have at least one facility in California reporting emissions above 25,000 tons of CO2 equivalents in 2010. *Post* is a dummy variable that takes the value of one in 2013 and subsequent years, and zero otherwise. In column (3), customers that report emissions above 25,000 tons of CO2e in California in 2010 are excluded. The sample consists of supplier-customer pair-year observations from FactSet Revere, with corresponding supplier financial data sourced from Compustat, in the 2010-2017 period. Supplier-customer pairs that terminate their relationship in the same year that either firm exits the Compustat sample are excluded from the sample. Robust standard errors adjusted for supplier-level clustering are reported in parentheses. Refer to Table A1 in the Appendix for variable definitions. * $p<.10$; ** $p<.05$; *** $p<.01$.

Table IA2: Probability of Termination and Cap-and-Trade Program: Robustness

	(1) Cluster by Pair	(2) Treated Supplier 2012	(3) Within Competitors group	(4) Geographically Concentrated	(5) Financially Unconstrained	(6) Placebo California HQ	(7) Segments Sales (log)	(8) Sample Ending	(9) Conditional Logit
Treated × Post	0.114***		0.029*	0.106**	0.090***		-0.112*	-0.186**	1.487***
	(0.013)		(0.017)	(0.046)	(0.033)		(0.062)	(0.073)	(0.133)
Treated 2012 × Post		0.124***							
		(0.031)							
HQ California × Post						-0.026			
						(0.023)			
Treated									-1.242***
									(0.138)
Year FE	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
Customer × Supplier sector FE	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
Supplier FE	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	No
Year × Competitor group FE	No	No	Yes	No	No	No	No	No	No
Customer × Competitor group FE	No	No	Yes	No	No	No	No	No	No
Supplier × Competitor group FE	No	No	Yes	No	No	No	No	No	No
Obs.	150,168	150,168	228,634	144,734	55,750	140,344	17,672	17,672	79,062
R^2	0.248	0.248	0.383	0.248	0.250	0.247	0.939	0.327	

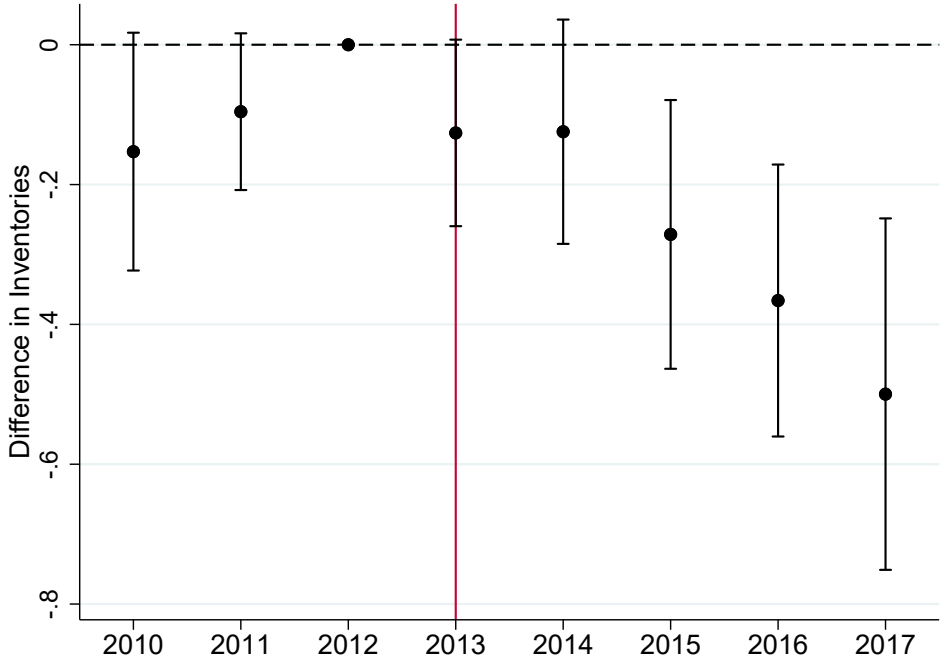
This table presents difference-in-differences estimates of customer-supplier-level panel regressions using equation (1). *Treated* takes the value of one for suppliers that have at least one facility in California reporting emissions above 25,000 tons of CO2 equivalents in 2010. *Post* takes the value of one in 2013 and subsequent years, and zero otherwise. Where not specified, the dependent variable is *Ending* (it takes the value of one if the supplier-customer relationship ends in a given year, and zero if it persists), the sample consists of supplier-customer pair-year observations from FactSet Revere, with corresponding supplier financial data in Compustat over the 2010-2017 period, and treated suppliers are matched to control suppliers as described in Section 3. In column (1), standard errors are clustered at the pair level. In column (2), the treatment is allocated according to firms' 2012 emissions in their Californian facilities. In column (3), each treated supplier is compared to a control supplier within its group of direct competitors. In column (4), treated suppliers are not geographically diversified. In column (5), treated suppliers are financially unconstrained, using the composite financial constraint indicator proposed by Bartram et al. (2022). In column (6) the placebo group consists of non-treated firms headquartered in California. Columns (6) and (7) are based on the Compustat Segment sample. The dependent variable in column (6) is the logarithm of the sales between the pair in a given year. In column (8), the model is a conditional logit with bootstrapped standard errors using the sample underlying column (1), Table 2. Refer to Table A1 in the Appendix for variable definitions. * $p<.10$; ** $p<.05$; *** $p<.01$.

Table IA3: Probability of Starting New Relationship and Cap-and-Trade Program

	(1)	(2)	(3)	(4)	(5)
Treated × Post	-0.136**	-0.119*	-0.116*	-0.119*	-0.017
	(0.060)	(0.063)	(0.063)	(0.063)	(0.038)
Supplier total assets (log)		0.052***	0.052***	0.052***	0.025***
		(0.016)	(0.015)	(0.016)	(0.009)
Supplier ROA		-0.041	-0.044	-0.041	-0.012
		(0.028)	(0.027)	(0.028)	(0.018)
Supplier leverage		-0.010	-0.008	-0.010	-0.014
		(0.036)	(0.036)	(0.036)	(0.021)
Supplier Tobin Q		0.006*	0.006*	0.006*	0.004**
		(0.003)	(0.003)	(0.003)	(0.002)
Supplier R&D stock		0.055***	0.055***	0.055***	0.029***
		(0.010)	(0.010)	(0.010)	(0.007)
Excl. Treated Customer	No	No	Yes	No	No
Supplier FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	No
Customer × Supplier Sector FE	Yes	Yes	Yes	Yes	Yes
Customer × Year FE	No	No	No	No	Yes
Obs.	50,681,088	43,600,235	42,591,960	43,600,182	43,600,182
R^2	0.007	0.007	0.008	0.007	0.011

This table presents difference-in-differences estimates of matched customer-supplier-level panel regressions using equation (2). The dependent variable *Beginning* is a dummy variable that takes the value of one hundred if the supplier-customer relationship starts in a given year, and zero if the relationship is not observed in FactSet (for ease of readability, the dependent variable is multiplied by 100). *Treated* is a dummy variable that takes the value of one for suppliers that have at least one facility in California reporting emissions above 25,000 tons of CO2 equivalents in 2010. *Post* is a dummy variable that takes the value of one in 2013 and subsequent years, and zero otherwise. In column (3), customers that produce emissions above 25,000 tons of CO2e in California in 2010 are excluded. The sample consists of new or possible supplier-customer pair-year observations from FactSet Revere, where corresponding supplier financial data sourced from Compustat is available in the 2010-2017 period. Supplier-customer pairs that terminate their relationship in the same year that either firm exits the Compustat sample are excluded from the sample. Robust standard errors adjusted for supplier-level clustering are reported in parentheses. Refer to Table A1 in the Appendix for variable definitions. * $p<.10$; ** $p<.05$; *** $p<.01$.

Figure IA1: Difference in Suppliers' Inventory around Cap-and-Trade Program



This figure shows the difference and 95% confidence intervals in the logarithm of inventories between treated and control suppliers around the introduction of the Californian cap-and-trade program in 2013. The sample consists of FactSet Revere suppliers with financial information sourced from Compustat in the 2010-2017 period. Standard errors are adjusted for supplier-level clustering.

B Anecdotal Evidence

In this section, we provide anecdotal evidence suggesting that uncoordinated climate actions in the United States, following the implementation of the California cap-and-trade program, expose affected suppliers to climate transition risks. The following excerpts are sourced from an analysis of 10-K filings by these suppliers.

I. Air Products and Chemicals

In this excerpt, the supplier warns that the cap-and-trade program and the uncertainty related to future climate policies can constitute a source of climate transition risk. The risk can negatively affect the firm's financial performance.

"Some of the Company's operations are within jurisdictions that have or are developing regulatory regimes governing emissions of greenhouse gases (GHG). [...] As of 1 January 2013, The Company's hydrogen production facilities in California and the EU will begin their compliance obligation under California's AB32 cap and trade program and Phase 3 EU ETS, respectively; however, these facilities have contractual terms to enable cost recovery. Increased public concern may result in more international, U.S. federal, and/or regional requirements to reduce or mitigate the effects of GHG. Although uncertain, these developments could increase the Company's costs related to consumption of electric power, hydrogen production, and fluorinated gases production. The Company believes it will be able to mitigate some of the increased costs through its contractual terms, but the lack of definitive legislation or regulatory requirements prevents accurate estimate of the long-term impact on the Company. Any legislation that limits or taxes GHG emissions could impact the Company's growth, increase its operating costs, or reduce demand for certain of its products." (Air Products and Chemicals Inc., Form 10-K `https://www.sec.gov/Archives/edgar/data/2969/000119312512476878/d409668d10k.htm`).

II. California Steel Industries

In this excerpt, the supplier refers to uncoordinated climate policies across different jurisdictions as a source of climate transition risk that might place the firm at a disadvantage relative to competitors operating in countries with no climate policies in place.

"The United States government or various governmental agencies have introduced or are contemplating regulatory changes in response to the potential impacts of climate change. International treaties or agreements may also result in increasing regulation of greenhouse gas emissions, including the introduction of carbon emissions trading mechanisms. Any such regulation regarding climate change and greenhouse gas, or GHG emissions could impose significant costs on our steelmaking operations and on the operations of our customers and suppliers, including increased energy, capital equipment, environmental monitoring and reporting and other costs in order to comply with current or future laws or regulations concerning and limitations imposed on our operations by virtue of climate change and GHG emissions laws and regulations. The potential costs of "allowance," "offsets" or "credits" that may be part of potential cap-and-trade programs or similar future regulatory measures are still uncertain. Any adopted future climate change and GHG regulations could negatively impact our ability (and that of our customers and suppliers) to compete with companies situated in areas not subject to such limitations. From a medium and long-term perspective, we are likely to see an increase in costs relating to our assets that emit significant amounts of greenhouse gases as a result of these regulatory initiatives. These regulatory initiatives will be either voluntary or mandatory and may impact our operations directly or through our suppliers or customers. Until the timing, scope and extent of any future regulation becomes known, we cannot predict the effect on our financial condition, operating performance and ability to compete." (California Steel Industries Inc., Form 10-K `https://www.sec.gov/Archives/edgar/data/751799/000119312511075101/d10k.htm`).

III. Campbell Soup Company

In this excerpt, the supplier mentions that exposure to climate policies might affect its supply chain due to possible disruptions to its facilities.

"Increased compliance costs and expenses due to the impacts of climate change and additional legal or regulatory requirements regarding climate change that are designed to reduce or mitigate the effects of carbon dioxide and other greenhouse gas emissions on the environment may cause disruptions in, or an increase in the costs associated with, the running of our manufacturing facilities and our business, as well as increase distribution and supply chain costs. Moreover, compliance with any such legal or regulatory requirements may require us to make significant changes in our business operations and strategy, which will likely require us to devote substantial time and attention to these matters and cause us to incur additional costs. [...] The effects of climate change and legal or regulatory initiatives to address climate change could have a long-term adverse impact on our business and results of operations." (Campbell Soup Company, Form 10-K `https://www.sec.gov/Archives/edgar/data/16732/000001673223000109/cpb-20230730.htm`).

IV. Freeport-McMoRan Copper & Gold Inc

In this excerpt, the supplier states that the exposure to climate policies might negatively affect its performance by reducing the demand for its products.

"California Air Resources Board (CARB) has developed "cap and trade" regulations [...] Some of our operations in California are subject to these regulations, which require us to purchase offsets and allowance instruments. The total amount of instruments we must purchase will vary annually. While we do not expect these costs to be material, similar or more onerous state regulations could substantially increase our costs. [...] From a medium and long-term perspective, we may experience increased costs relating to our greenhouse gas emissions as a result of regulatory initiatives in the U.S. and other countries in which we operate. In addition, the cost of electricity that we purchase from others may increase if our suppliers incur increased costs from the regulation of their greenhouse gas emissions. Although we have modeled different scenarios, we cannot predict the magnitude of increased costs with any certainty given the wide scope of potential regulatory changes in the many countries in which we operate. Increased regulation of greenhouse gas emissions may also reduce demand for the oil and gas we produce." (Freeport-McMoRan Copper & Gold Inc, Form 10-K `https://www.sec.gov/Archives/edgar/data/831259/000083125914000006/a2013form10-k.htm`).

V. United States Steel

In this excerpt, the supplier states that the heterogeneous exposure to climate transition risks of firms

belonging to the same group of competitors, due to globally uncoordinated climate action, might affect its financial performance.

Steel producers in the United States, along with their customers and suppliers, are subject to numerous federal, state and local laws and regulations relating to the protection of the environment. Steel producers in Canada and the EU are also subject to similar laws. These laws continue to evolve and are becoming increasingly stringent. The ultimate impact of complying with such laws and regulations is not always clearly known or determinable because regulations under some of these laws have not yet been promulgated or are undergoing revision. Environmental laws and regulations, particularly the CAA, could result in substantially increased capital, operating and compliance costs. International environmental requirements vary. While standards in the EU, Canada and Japan are generally comparable to U.S. standards, other nations, particularly China, have substantially lesser requirements that may give competitors in such nations a competitive advantage [...] GHG policies could negatively affect our results of operations and cash flows." (United States Steel Corporation, Form 10-K `https://www.sec.gov/Archives/edgar/data/1163302/000119312513061613/d448577d10k.htm`).

VI. USG

In this excerpt, the supplier states that the firm might be unable to pass through the costs resulting from the introduction of climate policies.

"From time to time, legislation has been introduced proposing a "carbon tax" on energy use or establishing a so-called "cap and trade" system. Such legislation would almost certainly increase the cost of energy used in our manufacturing processes. If energy becomes more expensive, we may not be able to pass these increased costs on to purchasers of our products. It is difficult to accurately predict if or when currently proposed or additional laws and regulations regarding emissions and other environmental concerns will be enacted or what capital expenditures might be required as a result of them. Stricter regulation of emissions might require us to install emissions control or other equipment at some or all of our manufacturing facilities, requiring significant additional capital investments." (USG Corporation, Form 10-K `www.sec.gov/Archives/edgar/data/757011/000075701113000023/usg-12312012x10k.htm`).

Insurers' Carbon Underwriting Policies*

Olimpia Carradori†, Felix von Meyerinck‡, Zacharias Sautner§

August 5, 2025

Abstract

We study the determinants, structure, implementation, and effects of carbon underwriting policies among the world's largest insurers. Adoption is more common among European insurers and less so among specialty and unlisted firms, with coal policies preceding those for oil and gas. Using novel mine-insurance data, we show that implementation is often incomplete, and some insurers expand coal coverage despite commitments. On average, insurers reduce the number of insured mines by 16%, insured coal volumes by 56%, and make continued coverage 13pp less likely. Affected mines are more likely to be abandoned and experience constrained operations.

JEL classification: G22; Q54; L71; L51; D22
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"*[T]he insurance sector has a key role to play. [...] We need net zero commitments to cover your underwriting portfolios, and this should include the underwriting of coal—and all fossil fuels.*"

– António Guterres, UN Secretary-General at the Insurance Development Forum 2021

1 Introduction

To combat climate change, some insurers have begun to restrict insurance coverage for coal, oil, and gas projects through formalized "carbon underwriting policies." While expectations for these policies are high, as underscored by the remarks of UN Secretary-General Guterres, it remains ambiguous whether underwriting decisions meaningfully influence decarbonization. On the one hand, insurers have strong incentives to act, as they bear significant costs of climate-related disasters, and possess a powerful lever to shape firm behavior, because insurance coverage is often a legal requirement for carbon-intensive projects. On the other hand, actions by individual insurers may be undermined by coordination and horizon challenges, as the costs of climate change build up in the future, while insurers can still profit from ongoing underwriting in the short run. This paper provides the first systematic analysis of insurers' carbon underwriting policies and their effects on carbon-intensive projects, seeking to determine whether insurance companies facilitate the net-zero transition via their carbon underwriting policies.

An empirical analysis of carbon underwriting policies and their implications faces three major challenges. First, carbon underwriting policies are a recent phenomenon in the insurance sector and lack a standardized format, making a systematic assessment difficult. Second, insurance contracts for carbon-intensive projects are not publicly disclosed, complicating the evaluation of whether carbon underwriting restrictions are implemented in line with public commitments. Third, comprehensive data on insured projects are necessary to assess the real effects of carbon underwriting policies, but such data are difficult to obtain and analyze.

This study exploits a setting that allows us to make significant progress on each of these three challenges. Specifically, we link information on insurers' carbon underwriting policies to rich data on insured carbon-intensive projects via insurance certificates, enabling an empirical analysis of the policies' determinants, structure, implementation, and effects. First, to address the lack of standardization in carbon underwriting policies, we rely on scorecards from Insure Our Future (IOF), a coalition of NGOs. These scorecards provide consistent, comparable measures on the presence, scope, and stringency of carbon underwriting policies across the world's major Property & Casualty (P&C) insurers.[1] Second, to overcome the opacity of insurance contracting, we file Freedom of Information Act (FOIA) requests to obtain insurance certificates for coal mines in the United States, as possessed by state-level regulators. These certificates reveal the identity of the underwriting insurers, allowing us to assess whether insurers' underwriting practices align with public commitments. Third, to examine the real effects of these policies, we match insurer decisions to granular mine-level information from the Mine Safety and Health Administration (MSHA). This allows us to study whether carbon underwriting policies affect outcomes such as production, employment, and the suspension of operations in coal mining—the upstream source of the single largest driver of human-induced climate change (IEA, 2021; ClientEarth, 2022).

We present three main findings. First, we trace the evolution of policies for coal as well as oil & gas projects, examining their prevalence, structure, and stringency. We uncover that such policies have become more common and stringent since 2017. Early adopters primarily targeted the underwriting of coal projects, while policies addressing oil & gas began to emerge from 2020 onward. However, underwriting policies are highly heterogeneous across insurers and often remain vague—for example, by excluding only specific projects from insurance coverage rather than entire carbon-intensive clients. Only a minority of insurers commit to a full

[1] Green and Vallee (2025) use similar data on the exit policies of banks for fossil fuel lending, which are collected by Reclaim Finance, an NGO that is part of IOF.

coal phase-out with a clear transition strategy, and even fewer actively engage clients to support decarbonization. Compared to their European counterparts, U.S. insurers are 48–52% less likely to adopt a coal policy, while Asian insurers are 38–47% less likely, and those from other regions are 25–33% less likely. Specialty insurers are 24% less likely to adopt a policy, and unlisted insurers are 32% less likely, highlighting the role of reputation and transparency in driving policy adoption. We also document that carbon underwriting policies do not simply reflect insurers' ESG-related divestment or screening policies on the investment side.

Second, we study changes in underwriting activity around the adoption of such policies to uncover how coal underwriting policies are implemented. Adoptions of coal policies are associated with a reduction in insurers' coverage of coal projects. On average, policy-adopting insurers reduce the number of insured coal mines by 16% (about 3.3 mines) and the volume of insured coal by 56% (about 3.3 million short tons, MMst), relative to non-adopters. These averages, however, mask important heterogeneity: not all policy-adopting insurers reduce coverage, and some even increase the number of mines they insure. Nonetheless, the aggregate pattern is consistent with a meaningful reduction in coal coverage following policy adoptions.

To address the concern that some of these results reflect time-varying sorting effects between insurers and mines, rather than the causal effects of coal policies, we conduct tests that identify effects from changes in insurer-mine relationships. Specifically, we exploit that mines obtain coverage by multiple insurers at a given point in time, allowing us to compare how different insurers adjust coverage for the same mine around policy adoption. This approach holds constant the mine-insurer match as well as time-varying mine characteristics. Comparing the six quarters before and after policy adoption, we find that the probability of a mine-insurer relationship becoming inactive increases by 12.8 percentage points. Dynamic event-study estimates show no significant pre-trends and a sharp increase in inactivity by 8.4pp in the first quarter immediately after policy adoption. This effect rises to 18.3pp by

the fourth quarter, coinciding with the standard duration of insurance policies. These results support a causal interpretation and indicate that coal underwriting policies reduce insurers' willingness to maintain coverage for coal mines. They also suggest that at least some insurers begin implementing their policies upon adoption, even if others defer action through phased commitments or long-term targets.

Reductions in insurance coverage are more pronounced among insurers with more stringent coal policies, suggesting that the extent of implementation increases with policy stringency. The likelihood of an insurer-mine relationship becoming inactive also varies by insurance policy type: insurers are more likely to maintain relationships for coverage lines tied to worker safety and compensation, which allows insurers to maintain business links with mining companies while somewhat reducing reputational risks associated with insuring the core mining activities directly. These findings highlight that coal policies are not uniform in their implementation.

Third, underwriting decisions by insurers have real effects on the operations of insured projects. Initial evidence shows that non-renewal of insurance, regardless of cause, is associated with a higher likelihood of mine abandonment and reductions in coal output and employment. Exploiting plausibly exogenous variation in non-renewal induced by adoptions of coal underwriting policies, we find that mines are 3.6pp more likely to be abandoned in the quarters following policy adoption. Among mines that remain productive throughout our period of study, employment declines by about 15%. Effects on production are smaller and less precisely estimated.[2] Overall, the results suggest that reductions in insurance coverage can meaningfully constrain coal mining activity.

[2]The estimates for production are not statistically significant across all specifications, and those for employment show some anticipation effects. Anticipation effects in employment likely reflect adjustment frictions, with cuts occurring before output declines. Such pre-trends are attenuated and post-treatment effects become cleaner when restricting to long-lasting insurer relationships that likely generate more meaningful exposure.

We complement this baseline analysis with a shift-share (Bartik) design that captures variation in the exposure intensity to coal underwriting policies, thereby exploiting that additional insurers adopt policies over time. Specifically, we construct a continuous exposure measure for each mine based on the share of its 2014-2016 insurers that have adopted a policy in a given quarter. Estimated effects mirror the baseline regressions: coal policy exposure increases the likelihood of mine abandonment, and—conditional on a mine remaining operational—leads to declines in coal production and employment. The effect on mine abandonment is directionally and economically consistent with the baseline analysis, but estimated with greater imprecision. This shift-share research provides additional evidence of a link between insurer policies and mine operations.

We examine cross-sectional heterogeneity in mine outcomes to assess whether the effects of coal policy adoptions depend on mine characteristics or the structure of insurer relationships. Abandonment decisions do not systematically vary with mine productivity or size, suggesting that opportunity costs play a limited role in how coal policy adoptions affect mine operations. In contrast, outcomes vary meaningfully with the structure of insurer relationships: mines more dependent on a single insurer are more likely to be abandoned, whereas those with access to a broader set of alternative providers are less likely to be affected. These findings indicate that insurer dependency amplifies, and insurer substitutability mitigates, the impact of coal policies on mine operations.

Our paper provides the first systematic evidence on insurers' carbon underwriting policies and their impact on insured carbon-intensive projects. Our results suggest that while such policies have become more prevalent, their implementation remains heterogeneous, with insurers differing in their commitment and enforcement levels. Underwriting decisions affect the operations of insured projects, suggesting that insurers can influence decarbonization via policies guiding such decisions. This interpretation is supported by anecdotal reports

of mine operators being caught off guard by non-renewals from insurers that had adopted carbon underwriting policies, as well as by regulatory filings in which mining companies cite the potential loss of insurance coverage due to climate-related underwriting restrictions as a material threat to continued operations. Although our analysis focuses on coal mining, the mechanisms we document may extend to other carbon-intensive sectors, as underwriting policies for oil & gas projects exhibit similar adoption patterns and determinants. However, assessing these broader effects systematically requires greater transparency on underwriting practices and insured projects.

We contribute to three streams of the literature. First, we add to the literature on how financial institutions manage climate-related risks. While much of this research has focused on banks (e.g., Kacperczyk and Peydró, 2022; Haushalter et al., 2023; Sastry et al., 2023; Morse and Sastry, 2024; Green and Vallee, 2025), there is a growing call to examine the role of insurance companies (e.g., Acharya et al., 2023). However, research on insurers remains limited, with existing studies primarily quantifying their exposures to and pricing of climate risks (e.g., Ge, 2022; Jung et al., 2024; Boomhower et al., 2024; Sastry et al., 2024; Oh et al., 2025; Ge et al., 2025a,b; Kim et al., 2025). We are the first to study insurers' carbon underwriting decisions, a potentially powerful yet understudied tool of financial institutions to manage climate risks and contribute to the net-zero transition. Unlike banks, insurers often hold direct operational leverage: in sectors like coal mining, liability insurance is legally required to obtain or maintain permits, giving insurers the ability to constrain or halt operations by withdrawing coverage.

Second, we contribute to the literature on how providing insurance shapes firm behavior. While theoretical work highlights insurers' potential role as private regulators (e.g., Ben-Sahar and Logue, 2012), empirical evidence remains limited. Existing studies focus largely on insurers' responses to immediate environmental risks (e.g., Yin et al., 2011) or their role

in mitigating judgment-proof problems, where firms in hazardous industries take on excessive environmental risks while limiting liability through bankruptcy protection (e.g., Shavell, 1982, 2000, 2005; Boomhower, 2019). We extend this literature by showing that insurers can address systemic and long-term risks posed by firms through underwriting decisions, thereby acting as an external form of governance that disciplines firm behavior.

Finally, we contribute to a separate strand of literature on how terminating insurance can serve as an enforcement tool when legal authority is limited. Maritime shipping is a prominent example: it underpins global trade but often evades regulation through fragmented ownership and flags of convenience (Vuillemey, 2020). These features complicate sanctions enforcement, most recently against Russia, Iran, Venezuela, North Korea, and Syria (e.g., Fernandez-Villaverde et al., 2025). In response, policymakers targeted insurers, prohibiting coverage for vessels involved in sanctions evasion (e.g., Smith and Dempsey, 2022; Triebert et al., 2024). To the best of our knowledge, no study has systematically examined the use of insurance withdrawal as a policy enforcement tool. Our results support the logic of these strategies: coverage reductions have real consequences, suggesting that insurance-based interventions can exert meaningful leverage where legal tools such as sanctions fall short.

2 Design and adoption of carbon underwriting policies

2.1 Data and sample

We use data on the underwriting policies of carbon-intensive projects by the world's leading insurance groups in the P&C business from 2017 to 2023.[3] The data are made available to us by IOF, a global coalition of NGOs. IOF publishes annual reports that include a scorecard evaluating insurers' carbon underwriting policies in a quantitative and standardized way.[4] Green and Vallee (2025) use similar data on banks' exit policies for fossil fuel lending obtained

[3]We use "insurance groups" and "insurers" interchangeably. When relevant, we ensure that the data are consolidated at the group level, which is where underwriting policies are formulated.

[4]IOF also collects data on the policies for coal and oil & gas projects in insurers' investment portfolios.

from Reclaim Finance, an NGO that is part of IOF and performed some of the data collection. One of IOF's goals in publishing the scorecards is to put pressure on insurers to restrict the underwriting business of carbon-intensive projects, which is why it focuses on underwriting policies for coal and oil & gas projects. The data are assembled from insurers' responses to an IOF survey and a manual review of publicly available information. During our sample period, IOF evaluates the underwriting policies of 37 insurance groups. According to IOF, these insurers represent the leading carbon-sector underwriters in the global P&C business.[5] The number of insurers evaluated by IOF varies across sample years, ranging from 24 to 30.

We construct two sets of variables for each insurer and year based on IOF's scorecards. The first set consists of two dummy variables indicating the presence of a coal or oil & gas policy, respectively. Since 2017, the IOF scorecards provide information on coal policies, and since 2020, IOF also scores oil & gas policies. The second set consists of scores that reflect the stringency of the underwriting policies. The scores range from 0 to 10, with higher values denoting more restrictive policies. The scores assess insurers' policies along five dimensions, capturing, among other features, whether insurers exclude all or only certain carbon-intensive projects, the degree of exclusion for high-emission projects, or whether emissions reduction targets are specified for new or ongoing projects (see Table IA.1 for details). Coal stringency scores are available from 2018 onward, and oil & gas scores from 2020.

We supplement the IOF data with hand-collected information on the introduction dates of insurers' underwriting policies. These dates are sourced from insurers' websites and news arti-

[5]According to IOF, the insurers covered by the scorecard are compiled based on a market analysis of the key players in the carbon underwriting business. A 2023 market study by S&P Global, listing the top 50 global P&C insurers by insurance premiums, includes 25 insurers that are in our sample (S&P Global, 2023). Insurers missing from the sample but included in the S&P list typically focus on businesses unrelated to the underwriting of carbon-intensive projects. Conversely, insurers in our sample but absent from the S&P study are specialty line insurers, such as Axis Capital, which focus on specific corporate insurance lines.

cles referencing the public announcements.[6] As coal policies are adopted earlier than oil & gas policies, the first policy introduction pertains to coal. While the timing of public announcements does not always indicate when underwriting restrictions are fully implemented—some insurers commit to gradual phase-outs or long-term targets—we treat these introduction dates as the relevant starting point for our analysis. This reflects our empirical objective: to test whether policy introduction is followed by meaningful implementation in the form of reduced underwriting activity and downstream effects in coal mining. In addition, we enrich the IOF data with insurer-level characteristics that are from ORBIS or hand-collected. Table IA.2 reports policy introduction dates and key insurer characteristics.

2.2 Prevalence and components of carbon underwriting policies

In Table 1, we list the presence of coal underwriting policies from 2017 to 2023 among insurers in IOF's scorecards. For each year and insurer, we report the dummy variable that indicates whether a coal policy is in place. Missing values reflect that an insurer was either not included in IOF's scorecard in a year or its policies were not assessed by IOF. Only four insurers—AXA, SCOR, Swiss Re, and Zurich—maintain a coal policy throughout the entire sample period. In contrast, Berkshire Hathaway, PICC, Sinosure, and WR Berkley never implemented such a policy (and are in the sample for at least three years). By 2023, 24 out of 30 insurers (80%) had implemented some form of coal policy.

In Table IA.3, we provide corresponding statistics for oil & gas policies. Compared to coal policies, restrictions on oil & gas projects are less prevalent, with coal policies generally preceding them. One explanation is that coal underwriting typically accounts for a smaller share of total premiums written, making it less costly for insurers to restrict underwriting of coal. In addition, as coal combustion generates more carbon emissions than oil or gas (MIT

[6] We are able to identify precise dates for 23 out of 25 insurers that have policies in place according to IOF. For Aviva, no specific introduction date could be determined. For Ping An, we found no official public announcement or media reporting. The missing data for these two insurers does not affect the coal mining analysis, as neither of them underwrites risks of U.S. coal mines in our sample.

Climate Portal, 2022), the "social license to operate" for coal may erode earlier.

In Figure 1, we plot the number of insurers with coal policies and decompose these policies into key structural components. Over time, coal policies have become more specific. Early policies were relatively generic, but more recent ones increasingly feature explicit commitments such as not insuring new coal projects or, less commonly, terminating coverage for clients in the coal sector altogether. Insurers now more frequently pledge to phase out certain coal clients (e.g., those generating a large share of their revenues from coal), while a smaller subset commits to adopting detailed phase-out plans over time. Engagement strategies aimed at the closure of certain coal projects remain rare.

In Figure IA.1, we plot corresponding dynamics for oil & gas policies. Compared to coal, oil & gas policies have evolved more slowly. Their structure is less standardized, with the only consistently observed feature being the commitment to not insure new oil & gas projects.

Overall, we document a gradual increase in the adoption of coal underwriting policies since 2017. These policies have become more specific over time, increasingly including explicit exclusions for new projects and, in some cases, commitments to phase out existing clients. In contrast, oil & gas policies began to emerge in 2020, remain less prevalent and less standardized, and typically restrict only the underwriting of new projects.

2.3 Stringency of carbon underwriting policies

Some insurers may adopt underwriting policies primarily to signal environmental commitment without imposing material restrictions on their business activities. It is therefore important to assess variation in the stringency of underwriting policies. In Table 2, we report the stringency scores of coal policies by insurer and year, ranking insurers based on their 2023 scores from high to low. Allianz, AXA, and Swiss Re lead the ranking, achieving scores close to the maximum value of 10 in 2023—scores in earlier years are generally lower. Many

insurers' scores fall below 10 due to fine-print exceptions that weaken the policies' strictness. For instance, Lloyd's coal policy received a score of 0 in 2022 because it was not binding, while Chubb's 2021 policy scored 1.47 due to numerous exceptions, including carve-outs for regions lacking near-term alternative energy sources.

Table IA.4 presents corresponding statistics for oil & gas policies. These policies are less restrictive overall—even the top-ranked insurer, Generali, scores a value of only 4 by 2023.

In Figure 2, we plot the stringency scores of coal policies over time. For each year, we display average scores and scores by insurer. There is a clear upward trajectory in policy stringency since 2020, but also substantial variation across insurers. The mean coal policy scores declined between 2018 and 2019, largely due to the entry of insurers with weaker policies (Aviva, HDI, and Ping An). While policy heterogeneity persists, some insurers have emerged as leaders or laggards. Allianz, for instance, became the leader for coal policy stringency between 2021 and 2023, after having matched the industry average between 2018 and 2020. Insurers such as Generali followed Allianz's lead with a lag of about one year.

In Figure IA.2, we display the dynamics for oil & gas policies. In contrast with coal policies, the stringency of oil & gas policies increased only modestly, remaining at a low average level. Dispersion in stringency has widened over time, suggesting a lack of global convergence in the adoption of strict underwriting policies. The stringency of the oil & gas policies of insurers with strict coal policies, such as Allianz and Generali, show only modest improvements over time. In contrast, insurers like Liberty Mutual and Lloyd's exhibit persistently low scores, with little evidence of policy tightening throughout the sample period.

Taken together, there is substantial heterogeneity in the stringency of carbon underwriting policies. While coal policies have generally become stricter since 2020, many insurers still maintain weak or non-binding commitments. Oil and gas policies remain notably less stringent, underscoring a lack of convergence in underwriting standards.

2.4 Carbon underwriting and investment policies

Insurers also manage climate risks through investment policies. In Table IA.5, we report correlations between insurers' underwriting and investment policies for coal and oil & gas. Panel A shows correlations based on the existence of such policies, while Panel B examines their stringency. Correlations in both panels are positive but far from perfect, suggesting substantial within-insurer variation in how carbon policies are adopted on the asset (investment) and liability (underwriting) side of the balance sheet. Figure IA.3 offers a more granular perspective, plotting individual insurers' underwriting and investment policy stringency for 2023. For some insurers, observed combinations of policy stringency lie close to the diagonal, indicating that they align closely across underwriting and investment activities, such as for AXA's coal policies (Panel A) or Generali's oil & gas policies (Panel B). In other cases, either underwriting policies (e.g., Allianz's coal policies in Panel A) or investment policies are stronger (e.g., SCOR's oil & gas policies in Panel B). These patterns highlight that underwriting policies do not merely reflect ESG-related divestment or screening strategies on the investment side.

2.5 Determinants of carbon underwriting policies

The substantial variation in carbon underwriting policies raises the question of what drives insurers to adopt such policies. To shed light on this question, we construct an annual insurer panel and regress two measures of carbon underwriting policies on a range of potential determinants. The dependent variable is either the dummy variable indicating whether an insurer has a coal policy or the score capturing the stringency of the coal policy. We estimate the same regressions for oil & gas policies. As determinants, we examine (i) time trends, (ii) regional patterns, (iii) whether the insurer has committed to aligning its underwriting portfolio with net-zero emissions,[7] (iv) whether the insurer is primarily a specialty insurer (i.e.,

[7] As a proxy for such commitments, we use membership in the Net-Zero Insurance Alliance (NZIA), which was launched in 2021.

focused on corporate business lines), (v) listing status, (vi) size, and (vii) profitability.[8]

Table 3 presents results for coal policies. Columns 1 to 3 analyze the extensive margin—whether an insurer has a coal policy in place—over the 2017-2023 period. Four main insights emerge. First, coal policy adoption has risen steadily over time: by 2023, insurers are 58.9%, 63.8%, and 71.0% more likely to have adopted a policy compared to 2017, depending on specification. Second, adoption is significantly more common among European insurers. U.S. insurers are 51.6% and 48.2% less likely to adopt a policy relative to European insurers; insurers from Asia are 37.8% and 46.8% less likely; and insurers from the rest of the world are 32.7% and 25.0% less likely, respectively. These differences may reflect green preferences in the home country or among the local labor force (Dechezleprêtre et al., 2025). Third, reputation effects and stakeholder pressure likely play a role: specialty insurers are 24.3% less likely, unlisted insurers are 31.6% less likely, and even members of the NZIA are 16.4% less likely to have a coal policy. Fourth, coal policies are negatively associated with profitability.

Columns 4–6 examine the intensive margin—policy stringency conditional on adoption—over the 2018-2023 period. U.S. and Asian insurers have less stringent policies compared to their European counterparts and listed insurers exhibit stricter policies. Different from the extensive margin results, policy stringency is unrelated to a retail customer base and profitability. The lower scores observed in 2019 and 2020, relative to 2018, reflect a temporary decline in policy stringency during the early sample years (see Figure 2).

In Table IA.7, we present the corresponding results for oil & gas policies, which can be studied from 2020 onward and over a smaller sample due to their later and less frequent adoption. The determinants of policy adoption and stringency are similar to those for coal. This alignment suggests that the same institutional and strategic factors share underwriting

[8]Table IA.6 presents descriptive statistics on the variables, and definitions are provided in Table A.1. The numbers show, for example, that 44% of all observations pertain to insurers from Europe, 25% from the U.S., and 20% from Asia. About 15% of the observations are from unlisted insurers and 23% from specialty insurers. Insurers in the sample have total assets of around $300 billion and achieve a return on assets of about 1.5%.

behavior across carbon-intensive sectors, and that the mechanisms we document in the coal mining analysis may generalize to other carbon-intensive sectors.

This section shows that coal underwriting policies have become more widespread and stringent since 2017, while oil & gas policies remain less developed. Policy adoption and stringency vary across insurers and regions, with non-European, unlisted, and specialty insurers more likely to adopt and implement restrictive policies.

3 Implementation and consequences of carbon underwriting policies

3.1 Motivation for U.S. coal mining context

The previous section documents the rise of carbon underwriting policies across insurers. Yet, it remains unclear whether these policies are meaningfully implemented and whether they have real effects on carbon-intensive projects. On the implementation side, announcements of policy adoptions do not always mark the start of actual restrictions—some insurers adopt immediate limitations, while others commit to phased actions or long-term targets. On the consequences side, it is unclear whether a reduction in insurance coverage—if it occurs—has tangible impacts on the operations of carbon-intensive projects.

We address these questions in the context of coal mining in the United States, which represents an ideal setting to study whether carbon underwriting commitments translate into real changes in underwriting behavior and impact carbon-intensive projects. First, the mining of coal is a critical upstream activity for coal combustion, the single largest source of human-induced climate change (IEA, 2021; ClientEarth, 2022), and insurance is required for coal mine operations under U.S. law. Second, coal underwriting policies are more prevalent and have existed for longer than those targeting oil & gas, increasing the availability of data. Third, the U.S. offers unusually detailed mine-level data, including quarterly production,

employment, and insurance relationships, which—despite being difficult to obtain—enable a direct link between insurer decisions and mine outcomes.

3.2 Data and empirical setting

3.2.1 Data on coal mines and their liability insurance

Data on U.S. coal mines come from the Mine Safety and Health Administration (MSHA). Under federal regulation, the MSHA collects data on coal production, employment, operating status, and other operational metrics at the mine-quarter level.[9] We construct a quarterly panel dataset of all coal mines that report at least one quarter of non-zero coal production between 2014Q1 and 2024Q2. By starting in 2014, we include data prior to the introduction of coal underwriting policies to compare mine behavior before and after policy adoptions.

Coal mine operators in the U.S. are required to maintain liability insurance as a condition for obtaining or renewing their operating permits. This mandate is enforced by state mine regulators under the Surface Mining Control and Reclamation Act of 1977 (SMCRA) and Title 30 of the Code of Federal Regulations. Operators must also meet minimum coverage standards set by relevant state agencies, typically covering workplace injuries, third-party claims, and environmental risks. Because proof of adequate coverage is a legal prerequisite for continued operation, insurers hold substantial influence over coal mines: by choosing whether to issue or renew policies, they can effectively determine whether a mine is allowed to operate. Leveraging this institutional feature, we filed FOIA requests with the relevant agencies to obtain information on mines' insurance coverage.[10] Specifically, we requested the release of Certificates of Liability Insurance for all mines in the MSHA database since

[9] We obtain the data directly from MSHA's Mine Data Retrieval System. For further information on these data, see Charles et al. (2022), Gilje and Wittry (2022), Qiu et al. (2024), and Chen and Wittry (2025).

[10] A FOIA request is a formal petition for access to public records. FOIA provisions vary across U.S. states in terms of scope and eligibility. For our sample of states, Alabama, Arkansas, Kansas, Kentucky, Oklahoma, Pennsylvania, and Tennessee do not accept FOIA requests from non-U.S. citizens/residents or out-of-state requesters. We are grateful to the two requesters and FOIA advocacy groups who assisted us in obtaining records from these states. We verified that the obtained data can be used by non-U.S. citizens/residents.

2014. These mines span 25 states, of which eight states—Colorado, Illinois, Montana, North Dakota, Utah, Virginia, West Virginia, and Wyoming—supplied the requested certificates in the form of standardized ACORD 25 filings.[11] The states that did not supply data play a negligible role in U.S. coal mining. Using MSHA data, we calculate that the sampled insurance certificates refer to mines that produce 78% of U.S. coal from 2014Q1 to 2024Q2. We also collect data from the relevant state agencies to identify whether a certificate pertains to a specific mine or to an operator covering multiple mines.

Figure IA.4 shows the ACORD 25 insurance certificate for the Black Butte Coal Mine in Wyoming. Each certificate comprises three main sections. The top section lists the insurance broker ("producer"), the insured entity ("insured"), and the insurers ("insurer(s) affording coverage").[12] The middle section details policy-level information, including the start and end dates of coverage. Insurance policies are categorized into one of four types: commercial and general liability, automobile liability, workers' compensation and employer's liability, and umbrella/excess liability (which includes other liability types such as pollution liability). The bottom section identifies the regulatory agency responsible for oversight and compliance.

Our dataset comprises 9,745 insurance policies across 456 coal mines. We define a policy as a unique insurer-policy type combination listed on a certificate. A single mine may thus hold multiple policies providing different types of coverage from different insurers. Around 37% of all policies cover commercial and general liability, 30% umbrella/excess liability, 17% automobile liability, and 16% workers' compensation and employer's liability. Roughly 90% of all

[11] We exclude Maryland and New Mexico because they only provided the most recent certificates and no historical records, despite follow-up efforts. We also exclude Alaska, Arizona, Arkansas, Kansas, Louisiana, Missouri, Mississippi, Oklahoma and Tennessee—states that each have five or fewer mines—as we did not receive a response to the FOIA requests. We exclude Ohio, Kentucky, and Texas because their agencies do not use standardized certificate forms, making their data difficult to compare with that of other states. Indiana did not provide any information. Our requests in Alabama and Pennsylvania are still pending.

[12] Mining companies may opt for self-insurance, also known as captive insurance. We identify such cases either when the top section of the certificate explicitly mentions a captive insurance company or when it explicitly states that the mine is self-insured. Fewer than 20 certificates in our sample indicate self-insurance. This low number may reflect substantial costs and regulatory requirements associated with establishing and maintaining captive insurers.

policies have a duration of one year, with additional bunching around quarters and full years.

3.2.2 Linking mine data to insurer-level policies

We link mine-level data from the MSHA with insurer-level coal underwriting policies using the Certificates of Liability Insurance to create a mine-quarter-insurance policy panel. This task requires mapping the insurers listed on the certificates to their corresponding insurance groups.[13] Although insurance certificates specify the start and end dates for each insurance policy, the MSHA data are at the mine-quarter level. To merge the two datasets, we assume a mine is covered by a given policy in a quarter if the insurance policy's coverage includes the start date of that quarter.[14] For mines listed in MSHA that lack insurance certificates or exhibit coverage gaps following our initial FOIA request, we submitted follow-up FOIA requests targeted at the missing data. These follow-ups yielded additional filings.

The final dataset covers 456 coal mines insured between 2014Q1 and 2024Q2 by 46 insurance groups. Of these, 15 appear in the IOF scorecards, with 11 of them introducing a coal underwriting policy during the sample period. We assume that the 31 insurance groups not covered by IOF do not adopt such policies, an assumption supported by extensive news searches. Hence, a total of 35 insurance groups do not have a coal policy (four categorized and 31 uncategorized). While the sample may miss some insurers from the IOF scorecards that may insure coal mines outside of the U.S. or implement underwriting restrictions differently in other markets, it reliably captures the primary actors in U.S. coal mine insurance.

[13] This is a nontrivial task as insurance groups typically operate through subsidiaries whose names often do not indicate their group affiliation. We perform this mapping using the 5-digit NAIC company codes provided on the certificates, cross-referenced with group affiliations from NAIC's semiannual publication *Listing of Companies* as of June 2017, prior to the announcement of any carbon policies. We evaluate the possibility of ownership changes after 2017 (e.g., through mergers and acquisitions) and find these to be rare. In fact, only one coal mine-insuring subsidiary changes group affiliation after this date. Policies that cannot be matched to an insurance group are excluded, though such cases are rare (fewer than 2% of all policies).

[14] This rule effectively lags mine-level outcomes by up to one quarter, since coverage is assigned based on whether the quarter's start date falls within the coverage period.

3.2.3 Summary statistics

Table 4 reports descriptive statistics for the sample at different levels of aggregation. The raw dataset is at the mine-quarter-policy level, identifying for a given mine-quarter which insurance policies are active and which insurance group provides coverage.

Panel A reports statistics at the mine-quarter-insurance level, enabling us to track mine-insurer relationships over time and distinguish periods of active and inactive coverage. For each mine, we include all observed insurer relationships and populate inactive quarters—i.e., when a certificate does not indicate coverage—whenever we have liability insurance information available for that mine. The average relationship lasts 14.2 quarters, or roughly 3.5 years. In 47% of all observations, the insurer has a coal policy in place and in 57% of observations, the relationship is inactive, that is, the mine is not insured by the respective insurer.

Panel B provides statistics at the insurer-quarter level, which we use to study insurer-level changes around policy adoptions. On average, an insurer underwrites about 20 coal mines, corresponding to 6 MMst of coal per quarter. A coal policy is in place in 28% of insurer-quarter observations.

Finally, Panel C presents statistics at the mine-quarter level, which is used to analyze operational outcomes of coal mines. In 12% of mine-quarters, a mine is classified as abandoned (i.e., it is abandoned or abandoned and sealed). On average, a mine produces 0.39 MMst of coal and employs 79 workers per quarter.[15] In 7% of mine-quarters an insurance contract is not renewed. Around four insurers provide coverage in a given quarter.

[15] Production and employment are typically zero when a mine is abandoned. However, active mines can also report zero production and employment in some quarters.

3.3 Do insurers reduce coverage following coal policy adoption?

3.3.1 Trends in insurance coverage by coal policy status

To provide initial evidence on the reduction of coverage following the adoption of coal underwriting policies, Figure 3 plots the number of coal mines insured by insurers that adopted a coal policy at some point during the sample period (Panel A), and by those that never did (Panel B). In Panel A, the number of insured mines remains relatively stable at around 200 throughout most of the sample period. However, there is considerable heterogeneity within this group, particularly in the cases of AIG and Zurich. AIG, which dominates the market in the early sample years alongside Chubb, insures fewer mines over time, while Zurich emerges as the main insurer in later years due to increased insurance activity.[16] In Panel B, among the insurers that never adopt a coal policy, the number of insured mines increases steadily and almost linearly, from about 100 at the beginning of the sample period to nearly 300 by the end. Houston and BrickStreet are the primary underwriters in this group throughout the sample period. However, there is also heterogeneity within the group of non-adopters. For example, Berkshire Hathaway—one of the larger insurers in this group in the early years—reduced its coverage considerably. The contrasting trends in the two panels offer suggestive evidence that many—but not all—insurers with coal policies reduced their coverage of coal mines, while those without such policies expand their underwriting activity, albeit with exceptions.

In Figure IA.5, we find similar aggregate patterns using the volume of insured coal production. In Panel A, which reports dynamics among insurers with a coal policy, Chubb reduces insured coal volume most strongly over time. AIG's coverage reduction based on the number of insured mines does not translate into lower insured coal volume, and the same

[16] There is a visible shift in the number of insured mines from AIG to Zurich in 2019Q4. We manually reviewed these entries and found no indication that Zurich acquired a subsidiary of AIG. A more likely explanation for this shift is that AIG chose not to renew policies covering a substantial number of mines, and Zurich subsequently stepped in to provide coverage (we find that Zurich provides coverage for over 75% of the mines that AIG drops). The increase in Zurich's insurance coverage may reflect exceptions in its coal policies, for instance, cases where less than 30% of a firm's revenues originate from thermal coal.

holds for the increase in coverage by Zurich. This suggests that Chubb primarily exits, and Zurich enters, coverage for smaller mines.

3.3.2 Insurer-level effects of coal policy adoption

To formally test whether coal underwriting activity declines following the adoption of coal policies, we estimate the following OLS regression at the insurer-quarter level:

$$Coal\ insurance_{i,t} = \beta \times Post\ coal\ policy_{i,t} + \alpha_i + \alpha_t + \epsilon_{i,t}, \tag{1}$$

where i indexes an insurer and t a quarter. *Coal insurance* denotes the log of either the number of insured coal mines (*# Coal mines insured*) or the volume of produced coal at insured mines (*Coal volume insured*). *Post coal policy* equals one for insurer-quarters after a coal underwriting policy is introduced, and zero otherwise. α_i and α_t are insurer and time fixed effects, respectively. We use robust standard errors. The coefficient β captures the change in coal underwriting activity among insurers that adopt a coal policy, relative to those that do not, at the same point in time. If insurers adjust their underwriting activity in line with public commitments, we expect β to be negative and statistically significant.

Table 5 reports estimates of Eq. 1. In Column 1, which uses *# Coal mines insured*, the estimate for β is negative and statistically significant, indicating that insurers, on average, reduce the number of insured coal mines by about 16% (or 3.3 mines) following policy adoption. In Column 2, using *Coal volume insured*, the estimate continues to be negative and statistically significant, implying a reduction in the volume of insured coal by about 56% (or 3.3 MMst) after policy adoption. Hence, coal policies substantially reduce coal underwriting activity.

A limitation of the analysis in Columns 1 and 2 is that we observe whether insurance contracts are renewed, but not the reason why insurance coverage ends. In particular, we cannot distinguish whether coverage ends due to insurer decisions or because the mine operator chooses not to renew, perhaps due to operational or financial considerations. To address

this limitation, Columns 3 and 4 restrict the sample to mines that remain operationally active throughout the sample period. The continued negative and significant estimates confirm that results are not driven by changes in mine operating status.

3.3.3 Mine-insurer relationship effects of coal policy adoption

The results in the previous section do not allow for a causal interpretation for two main reasons. First, they do not account for the endogenous matching of coal mines to insurers, and we cannot rule out that insurers adopt coal policies because of the specific mines they insure. Second, they do not control for time-varying mine characteristics that could influence insurers' renewal decisions, for example, deteriorating mine performance or rising operational risk. To address these concerns, we exploit that mines obtain coverage by multiple insurers at a given point in time, allowing us to compare how different insurers adjust coverage for the same mine around an insurer's policy adoption. This approach holds constant the mine-insurer match and time-varying mine characteristics.

We implement this identification strategy using a stacked difference-in-differences (DiD) design. A stack is defined as one policy-adopting insurer and the set of coal mines it insures. For each mine-insurer pair within that stack, we construct a balanced panel of 13 quarterly observations, six quarters before and six quarters after the policy adoption quarter. These mine-insurer pairs constitute the treated observations in the stack. Control observations are all other mine-insurer pairs involving the same mines but insurers that never adopt a coal policy. Intuitively, each stack tracks how mine–insurer relationships evolve around the adoption of a policy by one insurer, comparing changes in coverage between the adopting insurer and insurers that never adopt a policy. We repeat this procedure for each of the 11 policy-adopting insurers and stack the resulting samples to estimate the average effect of coal policies on the likelihood of observing inactive insurer-mine relationships. Specifically, we estimate the following OLS regression:

$$Inactive\ insurance_{s,m,i,t} = \sum_{t=-6}^{t=6} \beta_t \times Coal\ policy_i \times Event\ quarter_{s,t} + \alpha_{s,m,i} + \alpha_{s,m,t} + \epsilon_{s,m,i,t}, \tag{2}$$

where s indexes a stack, m indexes a mine, i an insurer, and t a quarter. *Inactive insurance* equals one if an insurer does not insure a mine in a given quarter, and zero if it does. *Coal policy* equals one for insurer-mine pairs involving a policy-adopting insurer, and zero otherwise. *Event quarter* are dummies indicating quarters relative to policy adoption. $\alpha_{s,m,i}$ are stack-mine-insurer fixed effects, while $\alpha_{s,m,t}$ are stack-mine-time fixed effects. These fixed effects ensure that we compare the evolution of mine-insurer relationships within the same mine and time across insurers, thereby controlling for the match of mines to insurers as well as unobservable time-varying mine characteristics.[17] Standard errors are clustered at the insurer-stack level. If insurers adjust their underwriting activity in line with public commitments, we expect the β_t coefficients to be indistinguishable from zero prior to adoption and significantly positive afterwards.

Figure 4 presents the estimated quarterly regression coefficients from Eq. 2, relative to the omitted quarter preceding policy adoption. In the quarters leading up to adoption and in the adoption quarter, there are no significant differences in the likelihood of inactive insurance relationships, supporting the parallel trends assumption. Starting in the first post-adoption quarter, however, relationships between mines and policy-adopting insurers are significantly more likely to become inactive. This likelihood increases steadily, from 8.4pp in the first quarter to 18.3pp by the fourth quarter. The effect then levels off, consistent with the typical one-year duration of insurance policies, by which time most non-renewals would have taken effect. The sharp discontinuity around policy adoption provides compelling evidence that coal policies drive changes in underwriting activity, and it suggests that at least some insurers begin to implement their policies upon introduction, even though others commit only to phased actions or long-term targets. This interpretation is supported by anecdotal

[17] The fixed effects absorb the standalone effect of *Coal policy* and the *Event quarter* dummies.

reports that some coal mines were caught off guard by non-renewals.[18]

To complement the dynamic estimates in Figure 4, we define a post-adoption indicator (*Post*) that equals one for the quarters after the introduction of a coal policy, and interact it with the *Coal policy* indicator. We then estimate the following OLS regression:

$$Inactive\ insurance_{s,m,i,t} = \beta \times Coal\ policy_i \times Post_{s,t} + \alpha_{s,m,i} + \alpha_{s,m,t} + \epsilon_{s,m,i,t}. \quad (3)$$

Table 6 reports estimates of Eq. 3. In Column 1, the coefficient on the interaction term indicates that relationships between a mine and a policy-adopting insurer are 12.8pp more likely to become inactive in the six quarters following adoption. In Column 2, among mines that remain active throughout the sample, the estimate increases to 13.7pp. These results suggest that non-renewals reflect changes in insurer behavior, rather than cancellations initiated by the mines themselves.

3.3.4 Heterogeneity in mine-insurer relationship effects of coal policy adoption

We explore two dimensions of cross-sectional heterogeneity to better understand how coal policy adoption affects insurer-mine relationships. First, we assess whether the incidence of inactive insurance relationships varies with the stringency of the coal policy. To do so, we augment Eq. 3 by interacting our baseline post-adoption indicator with two measures of policy strength: (i) *First coal policy score*, capturing an insurer's policy score at adoption[19], and (ii) *Max coal policy score*, capturing the highest policy score observed for a given insurer during the sample period. These interaction terms allow us to gauge whether the likelihood of an inactive relationship increases with the ambition of the insurer's commitment. Table 7, Columns 1 and 2, present the results. The positive and significant coefficients on the in-

[18]For example, several member companies of the Lignite Energy Council (LEC), a trade association for the coal industry in the United States, reported receiving non-renewal notices from multinational insurers such as Chubb and Zurich. One company was forced to scramble for alternative coverage within months (see Guidehouse Inc., 2022).

[19]As the first IOF scorecard, published in 2017, does not include stringency scores, we use the 2018 stringency scores for insurers that adopted a policy in 2017.

teraction terms indicate that insurers with stronger coal policies are more likely to exhibit inactive relationships with mines following policy adoption. This suggests that the extent of implementation increases with policy stringency.

Second, we explore whether the likelihood of inactive insurance relationships varies across policy types, that is, commercial and general liability, umbrella and excess liability, automobile liability, and workers' compensation. To assess these differences, we first replicate the stacked DiD specification at the policy type level. The static estimates in Column 3 of Table 7, and the dynamic estimates in Figure IA.6, closely mirror the baseline analysis, indicating that the aggregate pattern, on average, holds across policy types. Next, Table 7, Column 4, reports results when adding interaction terms with dummies for specific policy types. The results show that relationships involving workers' compensation insurance are less likely to become inactive relative to relationships covering commercial and general liability (the omitted category). This pattern is consistent with the idea that insurers are more hesitant to withdraw coverage types related to worker safety and compensation.[20]

Overall, the findings show that coal underwriting policies lead to meaningful reductions in insurance coverage for coal mines, particularly among insurers with stronger policy commitments. The effects also vary systematically across insurance policy types, suggesting that insurers selectively implement coverage reductions based on the nature of the coverage.

[20]These results are consistent with anecdotal evidence suggesting that insurers are willing to maintain certain types of coverage for social or safety reasons even when restricting others. For example, in the press release announcing its carbon underwriting policy, SCOR explicitly carved out exceptions for coverage related to worker and community protection: "*SCOR will not issue insurance or facultative reinsurance that would specifically encourage new greenfield thermal coal mines or stand-alone lignite mines or plants. The only exception to this rule would be the implementation of cover for the benefit of the local community or workers.*" (see, e.g., SCOR, 2017). Similarly, Zurich's latest coal underwriting policy exempts "*workers' compensation, other employee protections, or considerations, which have positive impact on human health and the environment.*" (see, e.g., Zurich, 2025).

3.4 Do coal underwriting policies affect coal mine operations?

3.4.1 Does insurance non-renewal affect coal mine operations?

Having established that insurers reduce the underwriting of carbon-intensive projects following the adoption of coal policies, we now examine the consequences for insured projects. As a starting point, we test whether the end of insurance coverage, regardless of whether it is driven by policy adoption or other factors, has an effect on mine operations by estimating the following OLS regression at the mine-quarter level:

$$Coal\ mine\ outcome_{m,t} = \sum_{k=1}^{k=4} \beta_k \times Non\text{-}renewed\ insurance_{m,t-k} + \alpha_m + \alpha_t + \epsilon_{m,t}, \quad (4)$$

where m indexes a mine and t a quarter. *Coal mine outcome* is one of three measures: (i) a dummy for whether a mine is abandoned (*Abandoned*), (ii) the volume of coal produced (*Production*), or (iii) the number of employees (*# Employees*).[21] *Non-renewed insurance* equals one if, in quarter $t-k$, with $k \in [1,4]$, at least one of the mine's insurers did not renew coverage, and zero if all insurers maintain coverage. α_m and α_t are mine and time fixed effects, respectively. Standard errors are clustered at the mine level. The β_k coefficients capture how mine outcomes change in each of the four quarters following non-renewal, relative to other mines in the same quarters. If non-renewals negatively affect operations, we expect positive coefficients on abandonment and negative coefficients on production and employment.

Table 8 reports estimates of Eq. 4. Column 1 shows that the likelihood of being abandoned increases significantly by up to 1.4pp over the four quarters after non-renewal. The effects in the first three quarters after non-renewal are also positive, but are estimated more noisily. Columns 2 and 3 reveal that active mines reduce output and employment after coverage ends. Coal production declines by about 4 to 11% over the four-quarter window, although only the second-quarter estimate is statistically significant. Employment falls by about 5 to 11%, with all four estimates being statistically significant. These results suggest that non-renewals—

[21] We do not observe mine-level profitability, which would offer a direct measure of economic performance.

regardless of the reason—are associated with a higher likelihood of mine abandonment and reduced operational activity. Combined with our earlier findings that insurer-mine relationships are more likely to end following policy adoption, this provides initial—yet suggestive—evidence that underwriting policies can translate into meaningful real effects.

3.4.2 Do coal policy-induced non-renewals affect coal mine operations?

While informative, the previous results do not reflect causal effects of non-renewal of insurance on mine operations for three key reasons. First, sample selection bias may arise because insurance certificates are more likely to be missing when a mine is abandoned, which hampers accurate identification of the effects of non-renewals. Second, non-renewals may be endogenous to future mine outcomes if insurers or mines anticipate declining output or impending closure and choose not to renew. Third, pooling non-renewals across insurers may bias estimates if insurers that adopt coal policies systematically differ in the types of mines they insure.

To mitigate these concerns, we implement a stacked DiD design that aims to isolate plausibly exogenous variation in insurance coverage induced by coal policy adoptions. A mine is defined as "coal policy exposed" if it was insured at the beginning of the sample period—before any coal policies were introduced—by at least one insurer that subsequently adopted a coal policy. This approach has three benefits: it avoids the need to observe insurance status across the full sample period, ensures that treatment is predetermined relative to later mine outcomes, and controls for the selection of mines into the underwriting portfolios of policy-adopting insurers. For each group of insurers adopting a coal policy in the same quarter, we construct a stack comprising all their mines if they are affected for the first time, observed over a symmetric 13-quarter window around policy adoption. Control observations are drawn from mines that, at the beginning of the sample period, are insured exclusively by insurers that never adopted a coal policy. We then estimate the following OLS regression:

$$Coal\ mine\ outcome_{s,m,t} = \sum_{t=-6}^{t=6} \beta_t \times Coal\ policy\ exposed_{s,m} \times Event\ quarter_{s,t} + \alpha_{s,m} + \alpha_{s,t} + \epsilon_{s,m,t}, \quad (5)$$

where s indexes a stack, m a mine, and t a quarter. *Coal mine outcome* is one of the three mine outcomes used above (*Abandoned*, *Production*, *# Employees*). *Coal policy exposed* equals one if a mine is insured by a policy-adopting insurer between 2014 and 2016, and zero otherwise. *Event quarter* are dummies for quarters relative to the earliest quarter in which the insurers adopts a coal underwriting policy. $\alpha_{s,m}$ are stack-mine fixed effects, and $\alpha_{s,t}$ are stack-time fixed effects, ensuring that we study within-mine variation and compare changes in mine outcomes with non-exposed mines at the same point in time.[22] Standard errors are clustered at the mine-stack level. If coal policies constrain operations, the β_t coefficients should be zero prior to adoption and positive (for abandonment) or negative (for production and employment) thereafter.

Figure 5 plots the dynamic estimates of Eq. 5, relative to the omitted quarter before policy adoption. Panel A documents the absence of pre-trends for mine abandonment, alongside a significant and sustained increase in the likelihood that a mine is abandoned, beginning in the second post-policy quarter. Panel B reveals no consistent pattern for coal production. Panel C shows modest upward pre-trends in employment and a significant decline after policy adoption. While the timing of the employment response suggests some anticipatory behavior, the overall patterns are consistent with coal underwriting policies constraining mine operations.

To complement this dynamic specification, we run a static DiD regression in which we replace the event-time indicators with a single post-treatment indicator (*Post*):

$$Coal\ mine\ outcome_{s,m,t} = \beta \times Coal\ policy\ exposed_{s,m} \times Post_{s,t} + \alpha_{s,m,i} + \alpha_{s,m} + \epsilon_{s,t}. \quad (6)$$

Table 9 reports the results of Eq. 6. Column 1 shows that mines exposed to coal policies

[22] The fixed effects absorb the standalone effect of *Coal policy* and the *Event quarter* dummies.

are 3.6pp more likely to be abandoned (about 33% relative to the sample mean). Columns 2 and 3 examine the intensive margin, restricting the sample to continuously active mines. The estimates indicate post-adoption declines in production by about 8% (or about 0.024 MMst) and in employment by 15% (or 10 employees). While the production effect is not statistically significant, the effects on mine abandonment and employment are, again suggesting that coal policies have real effects on coal mine operations.

So far, we have studied the first time a mine is exposed to the adoption of a coal underwriting policy. We now allow for a given mine to be treated repeatedly as its insurers adopt policies in a staggered manner, thereby capturing variation in treatment intensity. To this end, we implement a shift-share (Bartik) design, following Goldsmith-Pinkham et al. (2020) or Green and Vallee (2025). We define the Bartik instrument of mine m in quarter t, *Share coal policy exposed*, as $\sum_i w_{m,i} \times Post\ coal\ policy_{i,t}$, thereby capturing the share of insurers ($w_{m,i}$) that provided coverage to mine m during between 2014 and 2016 and had adopted a coal policy by quarter t ($Post\ coal\ policy_{i,t} = 1$). A mine's exposure thus increases with each additional policy-adopting insurer, generating shift-share variation in treatment intensity across mines and over time. This analysis relies on two additional assumptions. First, due to the lack of information on the relative importance of insurers for a given mine, we assume that all insurers contribute equally to exposure (i.e., we assign equal weights to each insurer). Second, we limit the set of mines and insurers to those insurer-mine relationships lasting at least eight quarters during the 2014-2016 period, ensuring that exposure reflects economically meaningful ties.[23] We implement this identification strategy by regressing each of the three quarterly mine outcomes on the shift-share instrument as well as time and mine fixed effects, using the mine-quarter panel described in Panel C of Table 4. The estimates, presented in Table IA.8, continue to show economically meaningful increases in the likelihood

[23]Pre-trends are attenuated, and the dynamic post-treatment effects become cleaner when using the refined eight-quarter relationship definition (see Figure IA.7), supporting the notion that longer-lasting insurer ties capture more meaningful exposure.

of mine abandonment (Column 1) and, conditional on being operational throughout the sample period, declines in employment (Column 2) and production (Column 3). However, the coefficient for abandoned mines is not statistically significant at conventional levels. These results—based on the intensity of exposure to coal policy-adopting insurers—reinforce the robustness of the association between insurer policies on mine operations.

The finding that the loss of insurance coverage induced by insurers' climate concerns can constrain mine operations is consistent with anecdotal evidence. Several documented cases support the interpretation that insurance-related regulatory constraints can affect mine operations, independently of coal mine operators' financial distress. For example, coal mine operators required to file with the Securities and Exchange Commission (SEC) regularly mention challenging insurance markets for coal mines because of ESG considerations, cite inability to obtain insurance as a risk factor, or warn that resulting non-renewal of insurance policies could adversely affect coal mine production activities and operating results (e.g., CONSOL Energy Inc., 2020; Arch Resources Inc., 2021; NACCO Industries, Inc, 2022; Alliance Resource Partners, 2023). Such frictions may manifest in employment reductions and precautionary abandonment before output is scaled back, especially when contractual obligations or stored inventories allow mines to sustain production temporarily.

3.4.3 Cross-sectional variation in mine outcomes

We examine cross-sectional heterogeneity in mine outcomes to assess whether the effects of coal policy adoptions are concentrated among specific subgroups. To do so, we augment Eq. 6 with triple interaction terms. This analysis focuses on mine abandonment, which not only provides the largest sample size relative to alternative outcomes such as production or employment but also constitutes the most drastic operational response.

We consider two dimensions of heterogeneity. First, we test whether mines that are more dependent on specific insurers or have fewer alternative providers are more affected. We

proxy dependency using a dummy for whether a mine has a relationship insurer, defined as having at least one insurer that provided coverage for at least eight quarters during the 2014-2016 period. Access to alternatives is measured by having an above-median number of distinct insurers covering a mine during the same period. Results in Columns 1 and 2 of Table 10 show that the effect of coal underwriting policies on mine abandonment is stronger for mines that are more reliant on specific insurers, either by having a relationship insurer or by lacking alternative providers. This indicates that insurer dependency amplifies, while insurer substitutability mitigates, the effects of coal policy adoptions.

Second, we test whether abandonment decisions are driven by opportunity cost considerations. While we do not observe mine-level profitability, we classify higher-opportunity-cost mines as those with above-median productivity, proxied by average output per working hour, or above-median size, measured by total production, during the 2014-2016 baseline period. The results in Column 1 indicate that more productive mines are generally less likely to be closed post-policy adoption, but this effect does not vary by exposure to coal policies. The results in Column 2 show no significant difference in abandonment probabilities between large and small mines, although the coefficient on the triple interaction term suggests a potentially higher likelihood of abandonment among larger mines. Overall, mine characteristics such as productivity and size do not materially influence abandonment decisions following coal policy adoptions, whereas dependency on insurers and availability of alternative coverage do.

Overall, the results in this section suggest that coal underwriting policies can have meaningful effects on mine operations. Non-renewal of insurance contracts, regardless of cause, is linked to a higher likelihood of mine abandonment and with reductions in output and employment. Leveraging plausibly exogenous variation in non-renewals induced by coal policy adoptions, we find similar effects, strengthening the interpretation that these policies, rather than broader market forces or mine-specific conditions, drive the operational changes. The

magnitude of these effects increases with exposure to policy-adopting insurers, pointing to a potential role for insurers in shaping coal mine activity through underwriting decisions. While abandonment decisions do not vary systematically with mine characteristics such as productivity or size, they are sensitive to the structure of insurer relationships: mines that are more dependent on specific insurers are more likely to be abandoned, whereas those with greater access to alternative insurance providers are less likely to be abandoned.

4 Conclusion

Insurance companies can facilitate the net-zero transition by restricting insurance coverage of carbon-intensive projects. We study the determinants, structure, implementation, and effects of insurers' carbon underwriting policies, focusing on their role in shaping U.S. coal mining. While such policies have become increasingly common among the world's major insurance groups, their design, stringency, and enforcement remain highly heterogeneous. Using novel mine-level insurance data, we show that coal underwriting policies reduce insurance coverage of coal mines, lower insured coal volumes, and make it less likely that existing mine-insurer relationships continue. Mines affected by changes in insurance coverage are more likely to be abandoned and reduce scale, particularly employment, suggesting that underwriting decisions provide a viable channel through which insurers can influence decarbonization. Our findings provide the first systematic evidence on insurers' carbon underwriting policies and underscore that their effectiveness depends critically on policy design and implementation. Greater transparency—particularly public disclosure of underwriting commitments and insured exposures—is essential to assess insurers' contributions to climate goals.

Table A.1: Variable definitions

Variable	Definition	Source
	Panel A: Insurer characteristics	
Coal policy	Equals one for insurers with a coal underwriting policy in place, and zero for insurers without a coal underwriting policy	IOF scorecards
Coal policy score	Score ranging from 0 to 10 to reflect the stringency of coal underwriting policies. Higher values indicate stricter policies	IOF scorecards
First coal policy score	An insurer's *Coal policy score* at adoption of the policy. For insurers that adopted a policy in 2017, we use the 2018 stringency scores.	IOF scorecards
Max coal policy score	Maximum of *Coal policy score* for a given insurer	IOF scorecards
Oil & gas policy	Equals one for insurers with an oil & gas underwriting policy, and zero for insurers without an oil & gas underwriting policy	IOF scorecards
Oil & gas policy score	Score ranging from 0 to 10 to reflect the stringency of oil & gas underwriting policies. Higher values indicate stricter policies	IOF scorecards
Coal investment policy	Equals one for insurers with a coal investment policy in place, and zero for insurers without a coal underwriting policy	IOF scorecards
Coal investment score	Score ranging from 0 to 10 to reflect the stringency of coal investment policies. Higher values indicate stricter policies	IOF scorecards
Oil & gas investment policy	Equals one for insurers with an oil & gas investment policy, and zero for insurers without an oil & gas underwriting policy	IOF scorecards
Oil & gas investment score	Score ranging from 0 to 10 to reflect the stringency of oil & gas investment policies. Higher values indicate stricter policies	IOF scorecards
Europe	Equals one if an insurer (parent company of an insurance group) is headquartered in Europe, and zero otherwise	IOF scorecards
US	Equals one if an insurer (parent company of an insurance group) is headquartered in the U.S., and zero otherwise	IOF scorecards
Asia	Equals one if an insurer (parent company of an insurance group) is headquartered in Asia, and zero otherwise	IOF scorecards
RoW	Equals one if an insurer (parent company of an insurance group) is headquartered outside Europe, the U.S, or Asia, and zero otherwise	IOF scorecards
Unlisted insurer	Equals one if the parent company of an insurance group is listed, zero otherwise	Orbis
Specialty insurer	Equals one if an insurer (parent company of an insurance group) is a specialty insurer, and zero otherwise	Online searches
NZIA	Equals one if an insurer (parent company of an insurance group) is a member of the Net Zero Insurance Alliance (NZIA), and zero otherwise	NZIA website

Total assets	Consolidated total assets of the insurer	Orbis
ROA (%)	Consolidated pre-tax return on assets of the insurer	Orbis
# Coal mines insured	Number of unique coal mines insured in a given quarter	Certificates of Liability Insurance
Coal volume insured	Million short tons (MMst) of coal produced by the mines insured in a given quarter	Certificates of Liability Insurance
	Panel B: Policy characteristics	
Commercial general liability	Equals one if a policy covers commercial general liability, and zero otherwise	Certificates of Liability Insurance
Automobile	Equals one if a policy covers automobile liability, and zero otherwise	Certificates of Liability Insurance
Umbrella & excess	Equals one if a policy covers Umbrella or excess liability, and zero otherwise	Certificates of Liability Insurance
Workers compensation	Equals one if a policy covers workers compensation and employers' liability, and zero otherwise	Certificates of Liability Insurance
	Panel C: Insurer-mine relationship characteristics	
Coal policy	Equals one for quarters in which the insurer has a coal underwriting policy in place, and zero otherwise	Certificates of Liability Insurance
Inactive insurance	Equals one for quarters in which the insurer does not insure the mine, zero if it does	Certificates of Liability Insurance
Length active insurance	Time in quarters since which the insurer insures the mine	Certificates of Liability Insurance
	Panel D: Mine characteristics	
Coal policy exposed	Equals one if, between 2014 and 2016, a mine is insured by an insurer that will eventually adopt a coal underwriting policy, and zero otherwise	Certificates of Liability Insurance
Share coal policy exposed	Equals $\sum_i w_{m,i} \times Post\ coal\ policy_{i,t}$ for a mine-quarter, where i indexes an insurer, m a mine and t a quarter. w are equal weights allocated to each insurer with at least eight quarters of active insurance relationship with a mine between 2014 and 2016, and *Post coal policy* equals one for insurer-quarters after a coal underwriting policy is introduced, and zero otherwise.	Certificates of Liability Insurance
Non-renewed insurance	Equals one if, in a given quarter, the insurance coverage of a given mine by a given insurer is not renewed, and zero as long as the same coverage exists and continues over the following year.	Certificates of Liability Insurance
Abandoned	Equals one if mine status in a quarter is either abandoned or abandoned and sealed, and zero otherwise	MSHA
Production	Million short tons (MMst) of coal produced by a mine in a quarter	MSHA
# Employees	Average number of employees of a mine in a quarter	MSHA
# Insurers per quarter	Number of different insurers of a mine in a quarter	Certificates of Liability Insurance
Relationship insurer	Equals one if a mine has a relationship of eight quarters or more with at least one insurer between 2014 and 2016, and zero otherwise	Certificates of Liability Insurance

Many insurers	Equals one if a mine has an above-median average number of insurers between 2014 and 2016, and zero otherwise	Certificates of Liability Insurance
Productive mine	Equals one if a mine has an above-median average output per working hour between 2014 and 2016, and zero otherwise	MSHA
Large mine	Equals one if a mine has an above-median total coal production between 2014 and 2016, and zero otherwise	MSHA

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Figure 1: Prevalence and structure of coal underwriting policies over time

The figure shows the number of insurers whose coal underwriting policies incorporate specific policy features. The sample is based on annual scorecards compiled by Insure Our Future (IOF) and includes up to 25 insurers with coal underwriting policies in place.

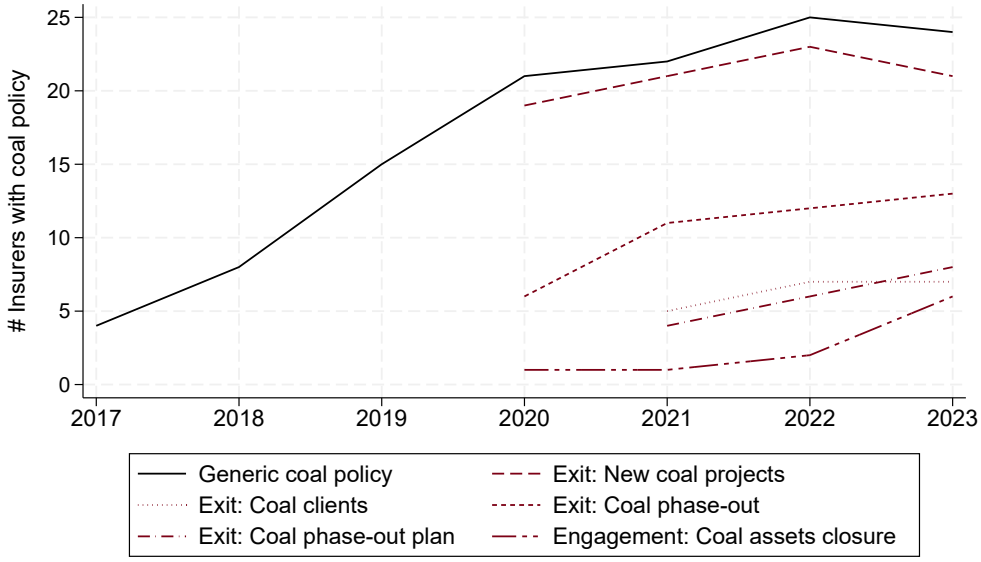


Figure 2: Stringency of coal underwriting policies over time

The figure displays scores capturing the stringency of coal underwriting policies over time. Scores range from 0 to 10, with higher values indicating more restrictive policies. Each dot represents an individual insurer in a given year. The solid line connects average scores by year. The two insurers with the highest and lowest overall carbon policy scores in the final sample year (2023) are highlighted with colored dots. The sample is based on annual scorecards compiled by Insure Our Future (IOF) and includes 25 insurers with coal underwriting policies in place.

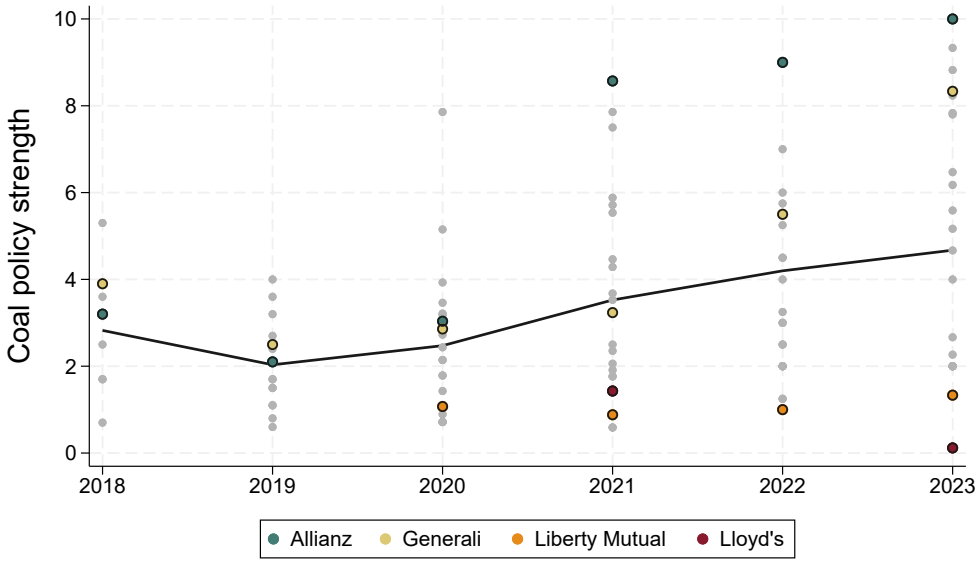
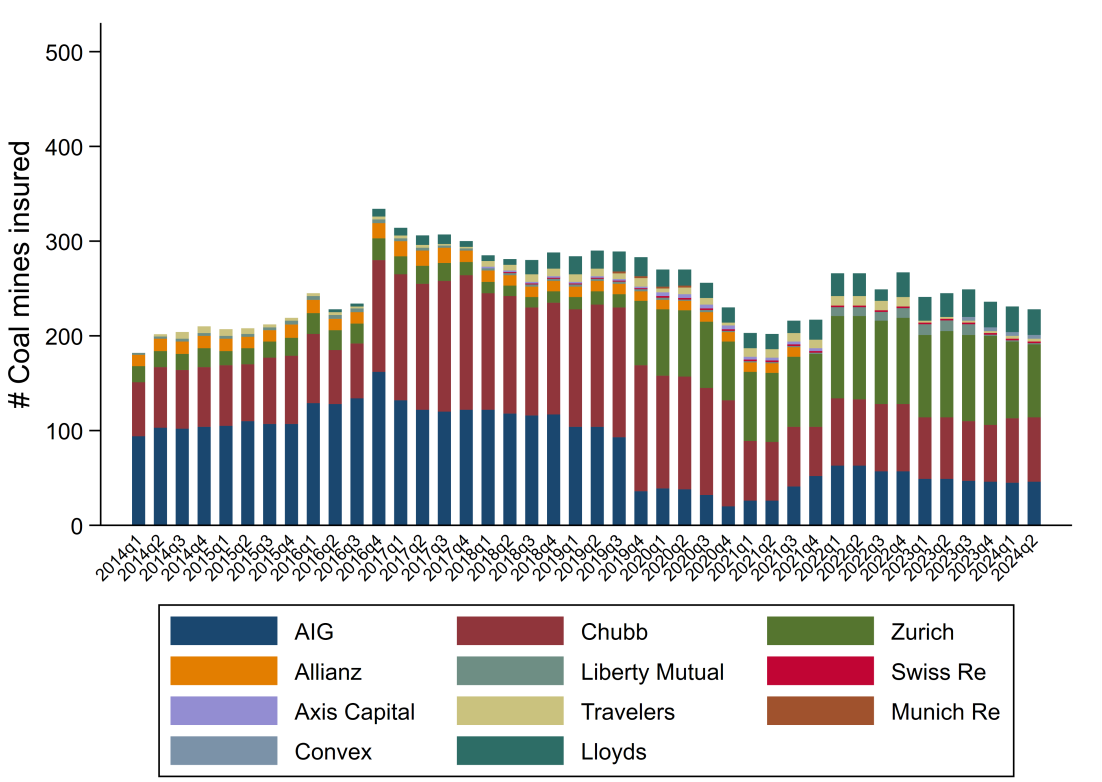


Figure 3: Number of insured coal mines over time

The figure displays the number of insured coal mines over time. For each quarter, we count the number of unique mines insured by each insurer. Panel A presents this information for insurers that adopted a coal underwriting policy at any point during the sample period, while Panel B presents this information for those that never did. The sample includes all coal mines listed in MSHA's Mine Data Retrieval System between 2014Q1 and 2024Q2, provided they report at least one quarter of non-zero coal production, and for which we successfully obtained Certificates of Liability Insurance of ACORD 25 forms from state-level agencies. Table A.1 provides descriptions of all variables.

Panel A: Insurer with a coal underwriting policy



Panel B: Insurers without a coal underwriting policy

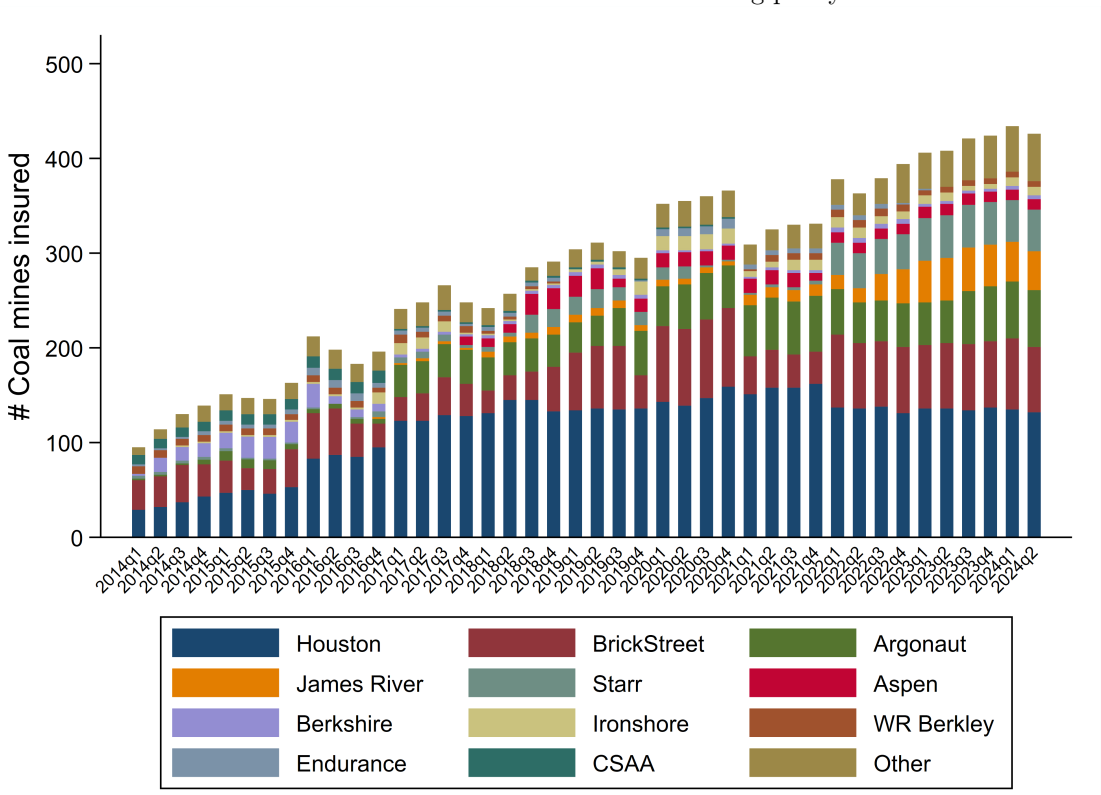


Figure 4: Coal mines' insurance status and underwriting policies

This figure displays coefficient estimates and 90% confidence intervals from the following OLS regression:

$$Inactive\ insurance_{s,m,i,t} = \sum_{t=-6}^{t=6} \beta_t \times Coal\ policy_i \times Event\ quarter_{s,t} + \alpha_{s,m,i} + \alpha_{s,m,t} + \epsilon_{s,m,i,t},$$

where s indexes a stack, m indexes a mine, i an insurer, and t a quarter. The dependent variable, *Inactive insurance*, equals one if an insurer does not insure a mine in a given quarter, and zero if it does. *Coal policy* equals one for insurer-mine pairs involving a policy-adopting insurer, and zero otherwise. *Event quarter* are dummy variables indicating quarters relative to the policy adoption quarter. $\alpha_{s,m,i}$ are stack-mine-insurer fixed effects, while $\alpha_{s,m,t}$ are stack-mine-time fixed effects. The initial sample includes all coal mines that appear in MSHA's Mine Data Retrieval System between 2014Q1 and 2024Q2, report at least one quarter of non-zero production, and have insurance certificate data available for those quarters. For each insurer that adopts a coal underwriting policy, we identify all mines it insures and construct a balanced sample of 13 quarterly observations per mine-insurance pair, six quarters before and six quarters after the policy adoption quarter, whenever information on the liability insurance certificates of a given mine is available. These mine-insurer pairs form the treated relationships in the stack. As a control group, we include all other relationships that the same mines have with insurers that never adopt a coal underwriting policy. Standard errors are clustered at the insurer-stack level. Table A.1 provides descriptions of all variables.

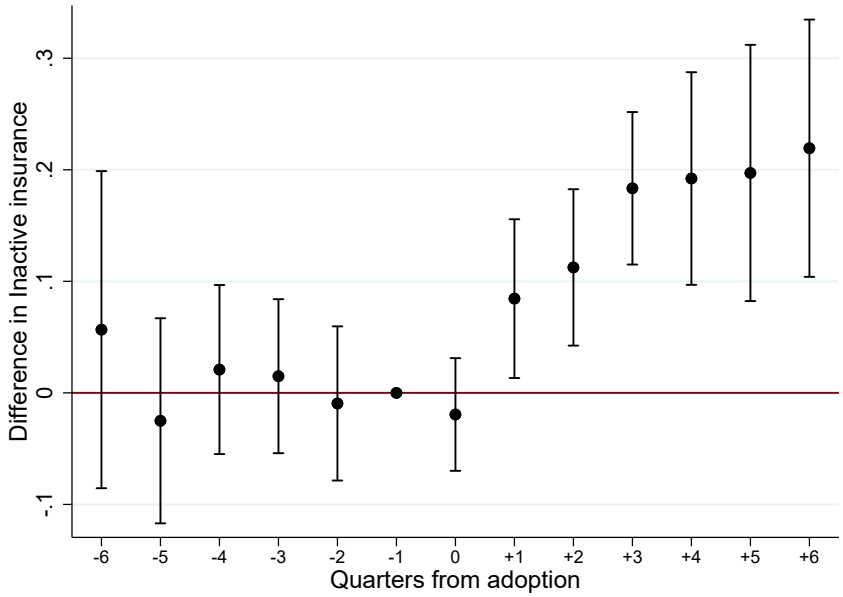


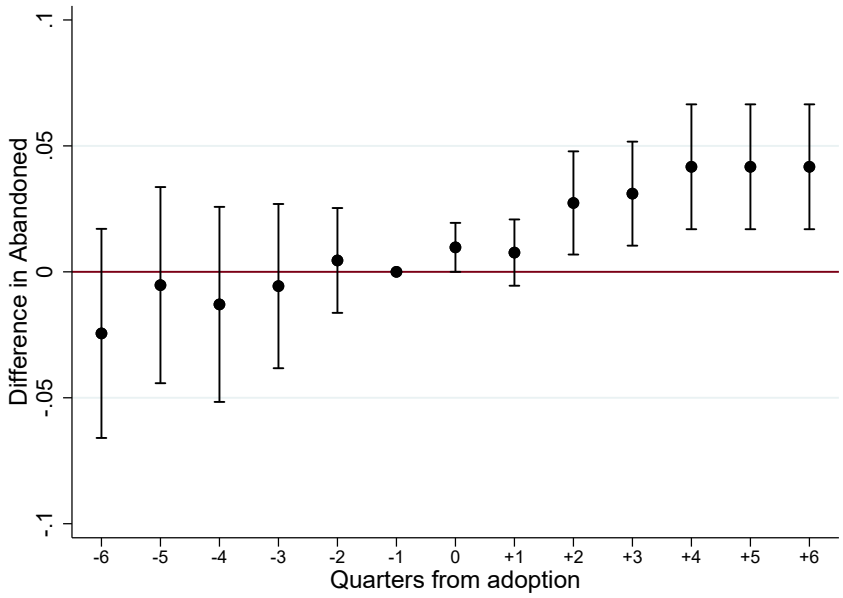
Figure 5: Coal mine outcomes and underwriting policies

This figure displays coefficient estimates and 90% confidence intervals from the following OLS regression:

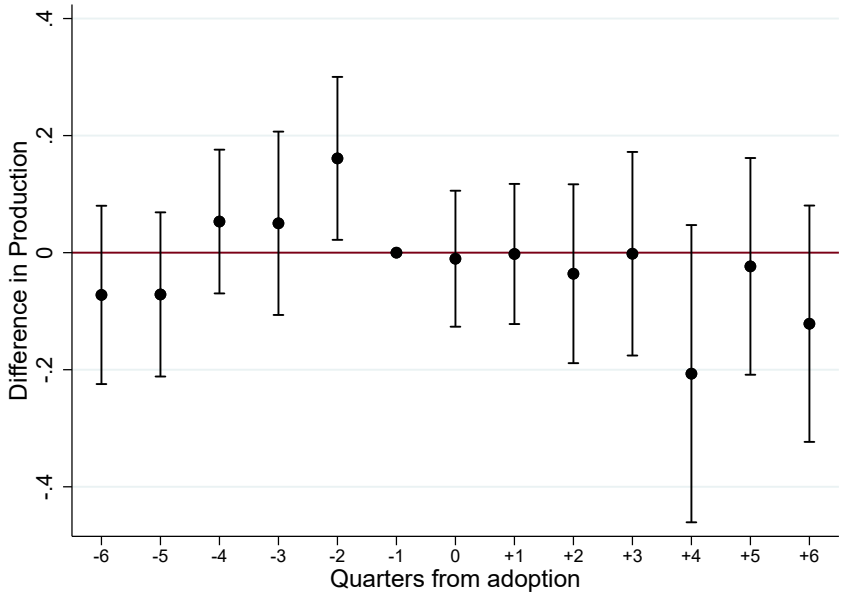
$$Coal\ mine\ outcome_{s,m,t} = \sum_{t=-6}^{t=6} \beta_t \times Coal\ policy\ exposed_{s,m} \times Event\ quarter_{s,t} + \alpha_{s,m} + \alpha_{s,t} + \epsilon_{s,m,t},$$

where s indexes a stack, m a mine, and t a quarter. The dependent variable, *Coal mine outcome*, is a dummy variable indicating whether a mine is abandoned (Panel A), the volume of coal produced (Panel B), or the average number of employees (Panel C). *Coal policy exposed* equals one for mines that, between 2014 and 2016, have at least one insurer with a coal underwriting policy. *Event quarter* are dummy variables indicating quarters relative to the policy adoption quarter. If a mine is exposed to multiple such adoptions, only the first instance is used to define treatment. The sample includes all mines from Table 6, provided that we successfully obtained Certificates of Liability Insurance on ACORD 25 forms from state-level agencies between 2014 and 2016. In Panels B and C, the sample includes only mines that are active throughout the entire sample period. Standard errors are clustered at the mine-stack level. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively. Table A.1 provides descriptions of all variables.

Panel A: Abandoned



Panel B: Production



Panel C: # Employees

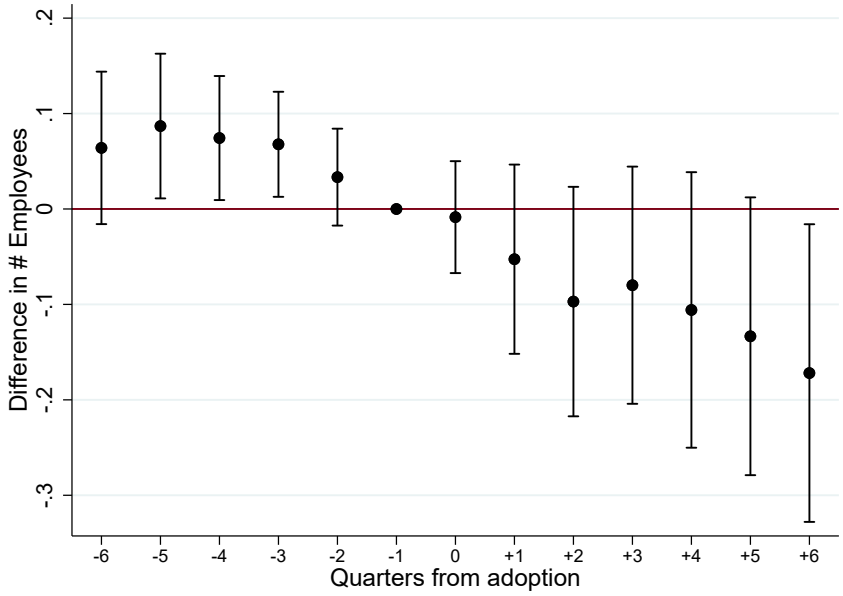


Table 1: Prevalence of coal underwriting policies by insurer

This table lists coal underwriting policies over time for the insurers in the sample. A value of one indicates that an insurer has a coal underwriting policy in place in a year, and a value of zero indicates that an insurer has no coal underwriting policy. Missing values reflect that an insurer was not included in the IOF scorecards or that its policies were not assessed by IOF in a year (e.g., for Legal & General, the coal investment—not the coal underwriting—policies are assessed; therefor the insurer is included in the sample but with missing values). The sample is based on annual scorecards compiled by Insure Our Future (IOF).

Name	2017	2018	2019	2020	2021	2022	2023	% Years w/ policy
AIG	0	0	0	0	0	1	1	29
AXA	1	1	1	1	1	1	1	100
Allianz	0	1	1	1	1	1	1	86
Aviva			1	1	1	1	1	100
Axis Capital	0	0	1	1	1	1	1	71
Berkshire Hathaway	0	0	0	0	0	0	0	0
Chubb	0	0	1	1	1	1	1	71
Convex					0	1		50
Everest Re					0	0	0	0
FM Global			0					0
Generali	0	1	1	1	1	1	1	86
HDI			1	1	1	1	1	100
Hannover Re	0	0	1	1	1	1	1	71
Legal & General								
Liberty Mutual	0	0	0	1	1	1	1	57
Lloyd's		0	0	0	1	1	1	50
MS&AD			0	1	1	1	1	80
Mapfre	0	1	1	1	1	1	1	86
MetLife								
Munich Re	0	1	1	1	1	1	1	86
PICC					0	0	0	0
Ping An			1	1	1	1	1	100
Prudential								
QBE	0	0	1	1	1	1	1	71
SCOR	1	1	1	1	1	1	1	100
Samsung FM			0	1	1	1	1	80
Sinosure			0	0	0	0	0	0
Sompo	0	0	0	1	1	1	1	57
Starr						0	0	0
Swiss Re	1	1	1	1	1	1	1	100
TIAA								
The Hartford				1	1	1	1	100
Tokio Marine	0	0	0	1	1	1	1	57
Travelers				0	0	1	1	50
WR Berkley	0	0	0	0	0		0	0
XL Catlin	0							0
Zurich	1	1	1	1	1	1	1	100
% Insurance groups w/ policy	21	42	58	78	73	80	80	
# Insurance groups	25	24	30	30	30	30	30	

Table 2: Stringency of coal underwriting policies by insurer

This table reports scores reflecting the stringency of coal underwriting policies over time of the insurers in the sample. The scores range from 0 to 10, with higher values reflecting stricter coal underwriting policies. Insurers are ranked based on 2023 score values, from high to low. The sample period starts in 2018 because policy strength was not assessed before. The sample is based on annual scorecards compiled by Insure Our Future (IOF).

Name	2017	2018	2019	2020	2021	2022	2023	Average score
Allianz		3.20	2.10	1.33	8.57	9.00	10.00	5.70
AXA		2.50	3.20	4.38	7.86	9.00	9.33	6.05
Swiss Re		5.30	4.00	4.29	5.54	6.00	8.82	5.66
Generali		3.90	2.50	2.34	3.24	5.50	8.33	4.30
Axis Capital		0.00	2.70	2.17	7.50	9.00	8.24	4.93
Aviva			1.10	1.09	3.68	5.75	7.83	3.89
Zurich		3.60	3.60	2.66	5.88	7.00	7.80	5.09
SCOR		1.70	1.70	1.67	4.29	4.50	6.47	3.39
Hannover Re		0.00	1.50	2.86	5.71	5.25	6.18	3.58
Munich Re		1.70	1.10	2.62	2.50	4.50	5.59	3.00
Mapfre		0.70	0.80	0.63	4.46	5.50	5.17	2.88
HDI			1.50	1.48	1.91	3.25	4.67	2.56
QBE		0.00	2.40	1.41	3.53	4.00	4.00	2.56
AIG		0.00	0.00	0.00	0.00	3.00	2.67	0.94
Samsung FM			0.00	0.78	1.76	2.50	2.27	1.46
Travelers				0.00	0.00	2.00	2.00	1.00
The Hartford				1.25	2.06	2.00	2.00	1.83
Sompo		0.00	0.00	0.31	0.59	2.00	2.00	0.82
Tokio Marine		0.00	0.00	0.31	2.35	2.00	2.00	1.11
MS&AD			0.00	0.31	1.76	3.00	2.00	1.41
Chubb		0.00	1.70	0.39	1.47	1.25	2.00	1.14
Liberty Mutual		0.00	0.00	0.47	0.88	1.00	1.33	0.61
Ping An			0.60	0.31	0.59	1.25	1.33	0.82
Lloyd's		0.00	0.00	0.00	1.43	0.00	0.12	0.26
Starr						0.00	0.00	0.00
PICC					0.00	0.00	0.00	0.00
WR Berkley		0.00	0.00	0.00	0.00		0.00	0.00
Sinosure			0.00	0.00	0.00	0.00	0.00	0.00
Berkshire Hathaway		0.00	0.00	0.00	0.00	0.00	0.00	0.00
Everest Re					0.00	0.00	0.00	0.00
Legal & General								
TIAA								
MetLife								
XL Catlin								
Prudential								
Convex					0.00	2.50		1.25
FM Global			0.00					0.00
Average score		1.19	1.17	1.22	2.59	3.36	3.74	
# Insurance groups	25	24	30	30	30	30	30	
# Missing scores	25	5	4	3	0	0	0	

Table 3: Determinants of coal underwriting policies

This table presents results from the following OLS regression:

$$Y_{i,t} = \beta \times X_{i,t-1} + \epsilon_{i,t},$$

where i indexes an insurer and t a year. In Columns 1 to 3, Y is *Coal policy*, which equals one if the insurer has a coal underwriting policy, and zero otherwise. In Columns 4–6, Y is *Coal policy score*, which is a score ranging from 0 to 10, with higher scores indicating stricter coal underwriting policies. X is a vector of insurer-level characteristics. The sample is based on annual scorecards compiled by Insure Our Future (IOF). Standard errors are clustered at the insurer level and reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively. Table A.1 provides descriptions of all variables.

	Coal policy			*Coal policy score*		
	(1)	(2)	(3)	(4)	(5)	(6)
2018	0.211**	0.211*	0.158			
	(0.099)	(0.105)	(0.112)			
2019	0.366***	0.388***	0.380***	-0.792**	-0.352	-0.129
	(0.116)	(0.119)	(0.124)	(0.377)	(0.440)	(0.446)
2020	0.567***	0.599***	0.598***	-0.351	0.898	1.222*
	(0.115)	(0.117)	(0.113)	(0.553)	(0.584)	(0.610)
2021	0.523***	0.565***	0.544***	0.701	1.892**	2.187***
	(0.117)	(0.109)	(0.114)	(0.684)	(0.686)	(0.639)
2022	0.623***	0.665***	0.754***	1.205*	2.515***	2.752***
	(0.115)	(0.104)	(0.104)	(0.697)	(0.714)	(0.673)
2023	0.589***	0.638***	0.715***	1.848**	3.211***	3.056***
	(0.115)	(0.107)	(0.133)	(0.720)	(0.688)	(0.655)
US		-0.516***	-0.482***		-3.247***	-2.332***
		(0.119)	(0.136)		(0.601)	(0.597)
Asia		-0.378***	-0.468***		-3.281***	-3.408***
		(0.128)	(0.108)		(0.563)	(0.506)
RoW		-0.327**	-0.350*		-0.037	0.402
		(0.159)	(0.174)		(1.055)	(1.230)
NZIA			-0.164*			0.853
			(0.088)			(0.531)
Specialty insurer			-0.243**			-0.357
			(0.101)			(0.830)
Unlisted insurer			-0.316**			-2.498**
			(0.149)			(1.091)
Total assets (log)			-0.020			0.179
			(0.048)			(0.284)
ROA			-0.044**			-0.218
			(0.020)			(0.173)
Obs.	181	181	180	115	115	115
Adj. R^2	0.146	0.359	0.441	0.103	0.471	0.606
# Insurers in sample	33	33	32	25	25	25

Table 4: Descriptive statistics on coal mines and underwriting policies

This table presents descriptive statistics on coal mines and underwriting policies. The sample includes all coal mines listed in MSHA's Mine Data Retrieval System between 2014Q1 and 2024Q2, provided they report at least one quarter of non-zero coal production, and for which we successfully obtained Certificates of Liability Insurance on ACORD 25 forms from state-level agencies. In Panel A, the sample is at the mine-quarter-insurer level. In Panel B, the sample is collapsed at the insurer-quarter level. In Panel C, the sample is collapsed at the mine-quarter level. Table A.1 provides descriptions of all variables.

	Obs.	Mean	Std.	25th	Median	75th
Panel A: Insurer-mine-quarter sample						
Coal policy	51,710	0.47				
Inactive insurance	51,710	0.57				
Length active insurance (quarters)	51,710	14.20	8.91	8.00	12.00	19.00
Panel B: Insurer-quarter sample						
Coal policy	1,145	0.28				
# Coal mines insured	1,145	19.61	33.65	2.00	4.00	16.00
Coal volume insured (MMst)	1,145	5.94	10.85	0.04	1.13	4.91
Panel C: Mine-quarter sample						
Coal policy exposed	10,883	0.80				
Non-renewed insurance	12,812	0.07				
Share coal policy exposed	7,342	0.25	0.40	0.00	0.00	0.50
Abandoned mine	12,812	0.11				
Production (MMst)	12,812	0.35	1.68	0.00	0.00	0.12
# Employees	12,812	71.40	147.91	0.00	9.00	78.00
# Insurers per quarter	12,812	4.15	1.84	3.00	4.00	5.00
Relationship insurer	12,932	0.57				
Many insurers	11,254	0.45				
Productive mine	10,129	0.50				
Large mine	11,254	0.50				

Table 5: Insurers' coal underwriting activity and underwriting policies

This table presents results from the following OLS regression:

$$Coal\ insurance_{i,t} = \beta \times Post\ coal\ policy_{i,t} + \alpha_i + \alpha_t + \epsilon_{i,t},$$

where i indexes an insurer and t a quarter. In Columns 1 and 3, the dependent variable, *Coal insurance*, is *# Coal mines insured*, which is the number of mines insured by an insurer in a given quarter. In Columns 2 and 4, *Coal insurance* is *Coal volume insured*, which is the volume of insured coal that is produced. *Post coal policy* equals one for insurer-quarters after a coal underwriting policy is introduced, and zero otherwise. The sample includes all coal mines listed in MSHA's Mine Data Retrieval System between 2014Q1 and 2024Q2, provided they report at least one quarter of non-zero coal production, and for which we successfully obtained Certificates of Liability Insurance on ACORD 25 forms from state-level agencies. In Columns 3 and 4, the sample includes only mines that are active throughout the entire sample period. Robust standard errors are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively. Table A.1 provides descriptions of all variables.

	# Coal mines insured (log)	*Coal volume insured* (log)	*# Coal mines insured* (log)	*Coal volume insured* (log)
	All coal mines		Active coal mines	
	(1)	(2)	(3)	(4)
Post coal policy	-0.172**	-0.824***	-0.582***	-0.651***
	(0.080)	(0.145)	(0.085)	(0.152)
Insurance group FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Obs.	1,145	951	680	671
Adj. R^2	0.828	0.767	0.750	0.754
# Insurance groups	46	40	29	28

Table 6: Coal mines' insurance status and underwriting policies

This table presents results from the following OLS regression:

$$Inactive\ insurance_{s,m,i,t} = \beta \times Coal\ policy_i \times Post_{s,t} + \alpha_{s,m,i} + \alpha_{s,m,t} + \epsilon_{s,m,i,t},$$

where s indexes a stack, m a mine, i an insurer, and t a quarter. The dependent variable, *Inactive insurance*, equals one if an insurer does not insure a mine in a given quarter, and zero if it does. *Coal policy* equals one for insurer-mine pairs involving a policy-adopting insurer, and zero otherwise. *Post* equals one for the quarter of the adoption of coal policy and all quarters thereafter, and zero otherwise. The initial sample includes all coal mines that appear in MSHA's Mine Data Retrieval System between 2014Q1 and 2024Q2, report at least one quarter of non-zero production, and have insurance certificate data available for those quarters. For each insurer that adopts a coal underwriting policy, we identify all mines it insures and construct a balanced sample of 13 quarterly observations per mine-insurance pair, six quarters before and six quarters after the policy adoption quarter, whenever information on the liability insurance certificates of a given mine is available. These mine-insurer pairs form the treated observations in the stack. As control observations, we include all other mine-insurer pairs that include the same mines but insurers that never adopt a coal underwriting policy. In Column 2, the sample includes only mines that are active throughout the entire sample period. Standard errors are clustered at the insurer-stack level and reported in parentheses. Table A.1 provides descriptions of all variables.

	Inactive insurance	
	All coal mines	Active coal mines
	(1)	(2)
Coal policy × *Post*	0.128**	0.137**
	(0.055)	(0.066)
Stack × Insurance group × Coal mine FE	Yes	Yes
Stack × Coal mine × Time FE	Yes	Yes
Obs.	24,907	4,330
Adj. R^2	0.587	0.604
# Insurance groups	46	26

Table 7: Cross-sectional variation in changes in underwriting activity

In Columns 1 and 2, this table presents variants of the following OLS regression:

$$Inactive\ insurance_{s,m,i,t} = \beta \times Coal\ policy\ score_i \times Post_{s,t} + \alpha_{s,m,i} + \alpha_{s,m,t} + \epsilon_{s,m,i,t},$$

while Columns 3 and 4 present variants of the following OLS regression:

$$Inactive\ insurance_{s,m,p,i,t} = \beta \times Coal\ policy_i \times Post_{s,t} \times Insurance\ type + \alpha_{s,m,p,i} + \alpha_{s,m,t} + \epsilon_{s,m,p,i,t},$$

where s indexes a stack, m a mine, p a policy type, i an insurer, and t a quarter. The dependent variable, *Inactive insurance*, equals one if an insurer does not insure a mine in a given quarter, and zero if it does. *Coal policy* equals one for mines insured by an insurer adopting a carbon underwriting policy, and zero otherwise. *Post* equals one for the quarter of the adoption of coal policy and all quarters thereafter, and zero otherwise. The initial sample of mines includes all coal mines listed in MSHA's Mine Data Retrieval System between 2014Q1 and 2024Q2, provided they report at least one quarter of non-zero coal production, and for which we successfully obtained Certificates of Liability Insurance on ACORD 25 forms from state-level agencies. For Columns 1 and 2, we define the unit of observation as a mine-insurer pair. For each insurer that adopts a coal underwriting policy, we identify all mines it insures and construct a balanced sample of 13 quarterly observations per pair—six quarters before and six quarters after the policy adoption quarter—whenever liability insurance certificate data are available. These mine-insurer pairs form the treated observations in the stack. As controls, we include all other mine-insurer pairs involving the same mines but insurers that never adopt a coal underwriting policy. Standard errors are clustered at the insurer-stack level. For Columns 3 and 4, we repeat this procedure at the finer level of mine-policy type-insurer triplets. That is, we construct balanced panels of 13 quarterly observations per triplet and stack them analogously. Standard errors are clustered at the insurer-policy type-stack level and reported in parentheses. Table A.1 provides descriptions of all variables.

	Inactive insurance			
	(1)	(2)	(3)	(4)
First coal policy score × Post	0.042*			
	(0.023)			
Max coal policy score × Post		0.026***		
		(0.010)		
Coal policy × Post			0.087*	0.125**
			(0.047)	(0.059)
Coal policy × Post × Automobile				-0.040
				(0.084)
Coal policy × Post × Umbrella & excess				-0.072
				(0.100)
Coal policy × Post × Workers compensation				-0.135*
				(0.079)
Post × Automobile				0.011
				(0.081)
Post × Umbrella & excess				0.029
				(0.065)
Post × Workers compensation				-0.042
				(0.063)
Stack × Insurance group × Coal mine FE	Yes	Yes		
Stack × Coal mine × Time FE	Yes	Yes		
Stack × Insurance group × Coal mine × Insurance type FE			Yes	Yes
Stack × Coal mine × Time FE			Yes	Yes
Obs.	24,907	24,907	41,615	41,615
Adj. R^2	0.585	0.585	0.623	0.625
# Insurance groups	46	46	46	46

Table 8: Coal mine outcomes and insurance non-renewal

This table presents results from the following OLS regression estimated at the mine-quarter level:

$$Coal\ mine\ outcome_{m,t} = \sum_{k=1}^{k=4} \beta_k \times Non\text{-}renewed\ insurance_{m,t-k} + \alpha_m + \alpha_t + \epsilon_{m,t}$$

where m indexes a mine and t a quarter. The dependent variable, *Coal mine outcome*, is a dummy variable indicating whether a mine is abandoned (Column 1), the volume of coal produced (Column 2), or the average number of employees (Column 3). *Non-renewed insurance* equals one if in quarter $t-k$, with $k \in [1,4]$, at least one insurer of mine m does not renew its coverage, it equals zero as long as insurance coverage for mine m by the set of its insurers exists and is renewed. The sample includes all coal mines that appear in MSHA's Mine Data Retrieval System between 2014Q1 and 2024Q2, report at least one quarter of non-zero production, and have insurance certificate data available for those quarters. In Columns 2 and 3, the sample includes only mines that are active throughout the entire sample period. Standard errors are clustered at the mine level and reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively. Table A.1 provides descriptions of all variables.

	Abandoned	*Production (log)*	*# Employees (log)*
	(1)	(2)	(3)
$Non\text{-}renewed\ insurance_{t-1}$	0.003	-0.066	-0.048*
	(0.007)	(0.059)	(0.027)
$Non\text{-}renewed\ insurance_{t-2}$	0.002	-0.112**	-0.112***
	(0.007)	(0.054)	(0.040)
$Non\text{-}renewed\ insurance_{t-3}$	0.006	-0.043	-0.082*
	(0.008)	(0.047)	(0.043)
$Non\text{-}renewed\ insurance_{t-4}$	0.014*	-0.088	-0.086*
	(0.008)	(0.064)	(0.046)
Coal mine FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Obs.	12,812	2,173	2,256
Adj. R^2	0.601	0.911	0.911
# Coal mines	456	62	63

Table 9: Coal mine outcomes and underwriting policies

This table presents results from the following OLS regression estimated at the mine-quarter level:

$$Coal\ mine\ outcome_{s,m,t} = \beta \times Coal\ policy\ exposed_m \times Post_{s,t} + \alpha_{s,m} + \alpha_{s,t} + \epsilon_{s,m,t}$$

where s indexes a stack, m a mine, and t a quarter. The dependent variable, *Coal mine outcome*, is a dummy variable indicating whether a mine is abandoned (Column 1), the volume of coal produced (Column 2), or the average number of employees (Column 3). *Coal policy exposed* equals one for mines that, between 2014 and 2016, have at least one insurer with a coal underwriting policy, and zero otherwise. *Post* equals one for the first and all subsequent quarters following the earliest quarter in which the insurer of the mine adopted a coal underwriting policy. If a mine is exposed to multiple such adoptions, only the first instance is used to define treatment. The sample includes all mines from Table 6. In Columns 2 and 3, the sample includes only mines that are active throughout the entire sample period. Standard errors are clustered at the mine-stack level and reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively. Table A.1 provides descriptions of all variables.

	Abandoned	*Production (log)*	*# Employees (log)*
	(1)	(2)	(3)
Coal policy exposed × *Post*	0.036*	-0.078	-0.147**
	(0.018)	(0.082)	(0.069)
Stack × Coal mine FE	Yes	Yes	Yes
Stack × Time FE	Yes	Yes	Yes
Obs.	5,525	932	936
Adj. R^2	0.835	0.912	0.947
# Coal mines	270	57	57

Table 10: Cross-sectional variation in changes in coal mine outcomes

This table presents results from the following OLS regression estimated at the mine-quarter level:

$$\begin{aligned} \textit{Coal mine outcome}_{s,m,t} = \beta_1 \times \textit{Coal policy exposed}_m \times \textit{Post}_{s,t} + \\ \beta_2 \times \textit{Coal policy exposed}_m \times \textit{Post}_{s,t} \times X_m + \beta_3 \times \textit{Post}_{s,t} \times X_m + \alpha_{s,m} + \alpha_{s,t} + \epsilon_{s,m,t} \end{aligned}$$

where s indexes a stack, m a mine, and t a quarter. The dependent variable, *Coal mine outcome* is a dummy variable indicating whether a mine is abandoned. *Coal policy exposed* equals one for mines that, between 2014 and 2016, have at least one insurer with a coal underwriting policy, and zero otherwise. *Post* equals one for the first and all subsequent quarters following the earliest quarter in which the insurer of the mine adopted a coal underwriting policy. If a mine is exposed to multiple such adoptions, only the first instance is used to define treatment. X is a dummy indicating whether a mine has a relationship of eight quarters or more with at least one insurer between 2014 and 2016 (Column 1), a dummy indicating above-median average number of insurers between 2014 and 2016 (Column 2), a dummy indicating above-median average productivity between 2014 and 2016 (Column 3), a dummy indicating above-median total production between 2014 and 2016 (Column 4). The sample includes all mines from Table 6. Standard errors are clustered at the mine-stack level and reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively. Table A.1 provides descriptions of all variables.

	Abandoned			
	(1)	(2)	(3)	(4)
Coal policy exposed × *Post*	-0.016	0.051***	0.033	0.011
	(0.030)	(0.019)	(0.024)	(0.030)
Coal policy exposed × *Post* × *Relationship insurer*	0.076**			
	(0.032)			
Post × *Relationship insurer*	-0.034*			
	(0.018)			
Coal policy exposed × *Post* × *Many insurers*		-0.057*		
		(0.033)		
Post × *Many insurers*		0.006		
		(0.011)		
Coal policy exposed × *Post* × *Productive mine*			0.001	
			(0.032)	
Post × *Productive mine*			-0.030*	
			(0.016)	
Coal policy exposed × *Post* × *Large mine*				0.036
				(0.030)
Post × *Large mine*				0.007
				(0.011)
Stack × Coal mine FE	Yes	Yes	Yes	Yes
Stack × Time FE	Yes	Yes	Yes	Yes
Obs.	5,525	5,525	4,836	5,525
Adj. R^2	0.836	0.836	0.845	0.836
# Coal mines	425	425	372	425

Internet Appendix

for

Insurers' Carbon Underwriting Policies

Figure IA.1: Prevalence and structure of oil & gas underwriting policies over time

The figure shows the number of insurers whose oil & gas underwriting policies incorporate specific policy features. The sample is based on annual scorecards compiled by Insure Our Future (IOF) and includes up to 22 insurers with oil & gas underwriting policies in place.

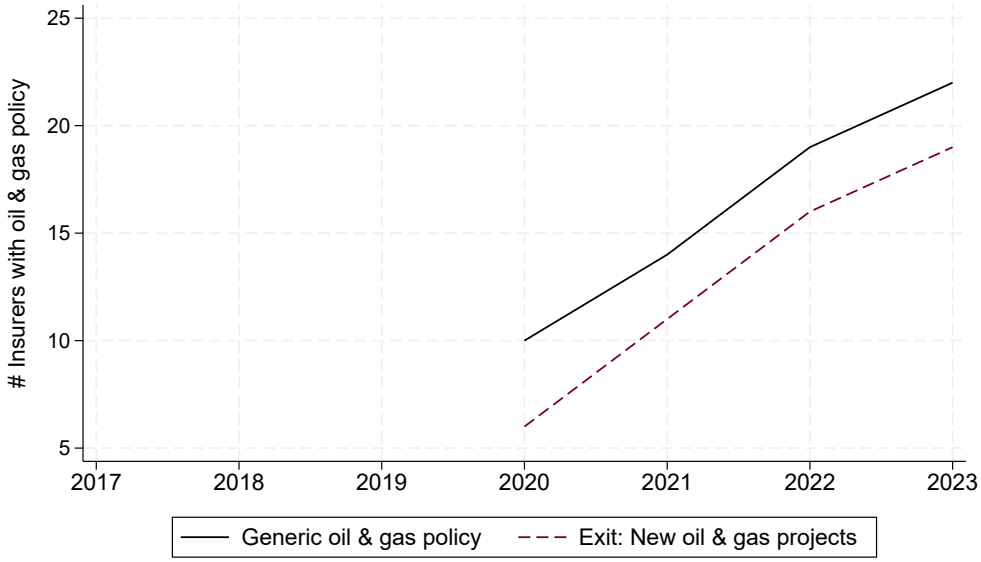


Figure IA.2: Stringency of oil & gas underwriting policies over time

The figure displays scores capturing the stringency of oil & gas underwriting policies over time. Scores range from 0 to 10, with higher values indicating more restrictive policies. Each dot represents an individual insurer in a given year. The solid line connects average scores by year. The two insurers with the highest and lowest overall carbon policy scores in the final sample year (2023) are highlighted with colored dots. The sample is based on annual scorecards compiled by Insure Our Future (IOF) and includes 22 insurers with oil & gas underwriting policies in place.

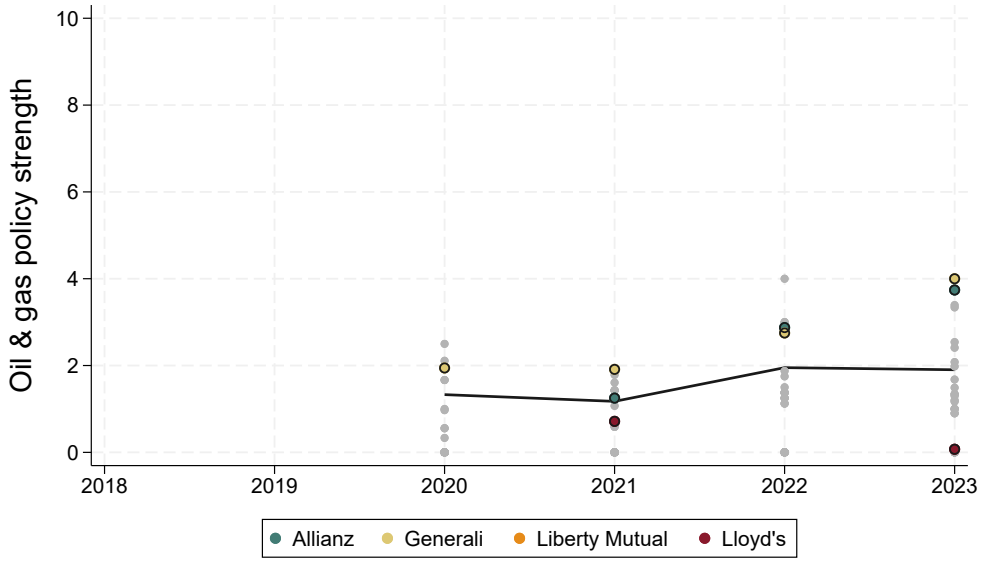
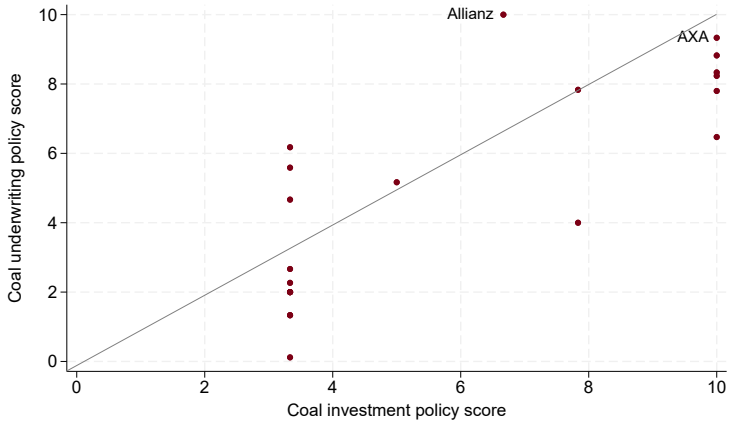


Figure IA.3: Alignment of carbon underwriting and investment policy stringency

The figure shows the relationship between the stringency of carbon underwriting and carbon investment policies for the year sample 2023. Each dot represents the combination of underwriting and investment policies of an insurer. Panel A shows the relationship for coal policies, while Panel B shows the relationship for oil & gas policies. The sample is based on annual scorecards compiled by Insure Our Future (IOF) and comprises 25 insurers with carbon policies in place.

Panel A: Coal underwriting policies



Panel B: Oil & gas underwriting policies

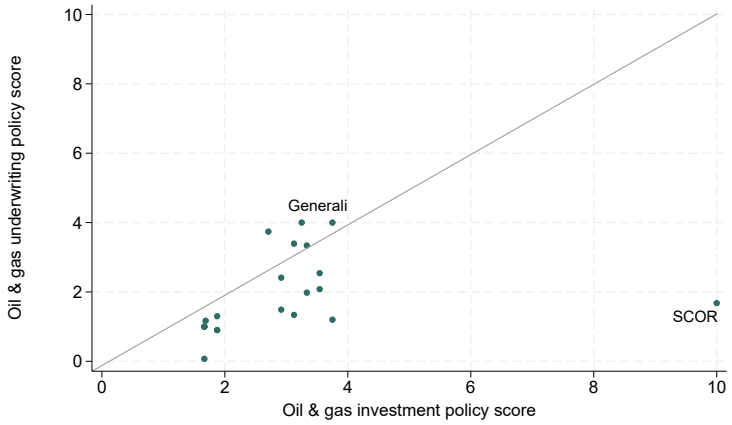
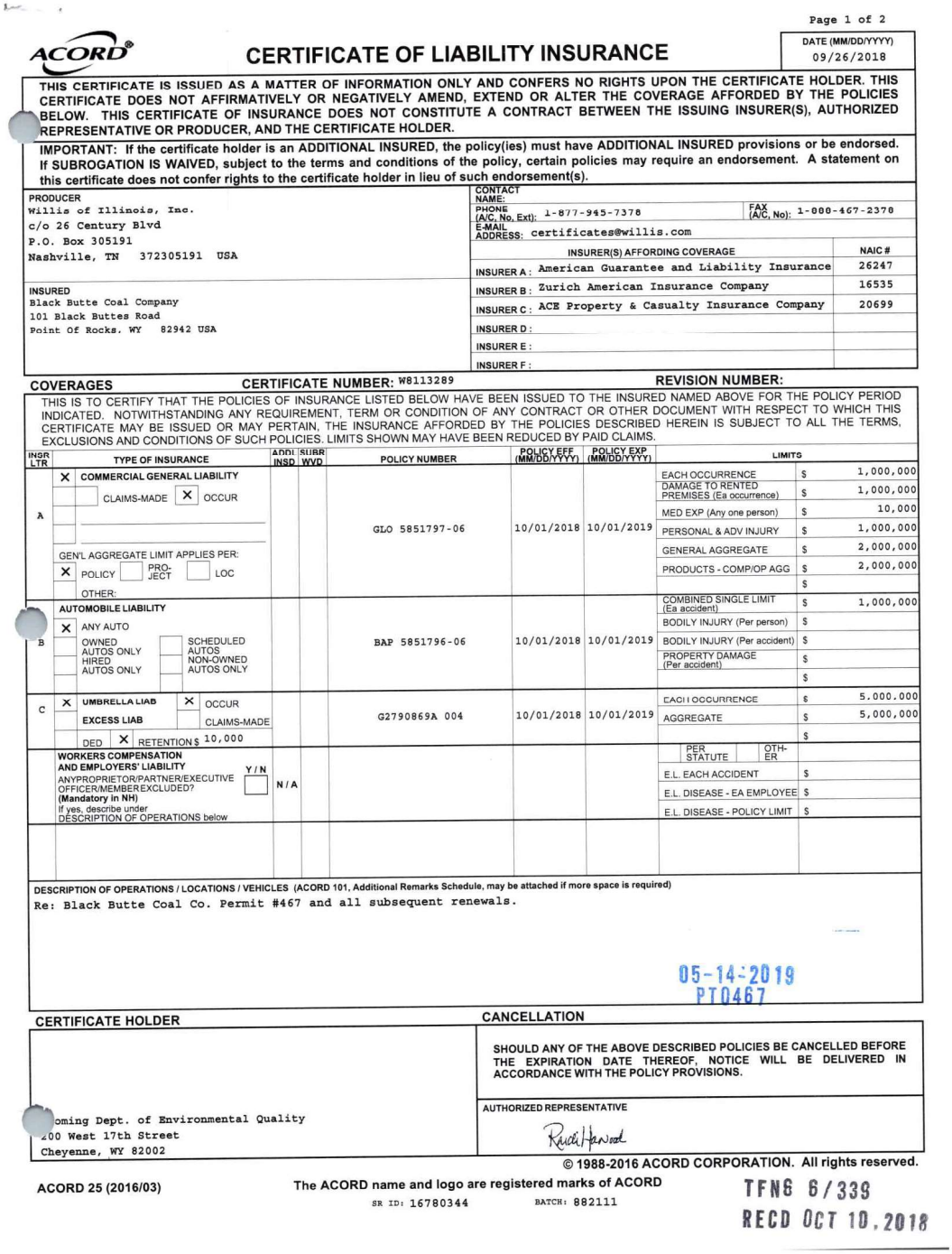


Figure IA.4: Certificate of Liability Insurance of Black Butte Coal Mine

The figure shows the Certificate of Liability Insurance of Black Butte Coal Mine dated September 26, 2018, on a standardized ACORD 25 form filed with the Wyoming Department of Environmental Quality.



Page 1 of 2

ACORD

CERTIFICATE OF LIABILITY INSURANCE

DATE (MM/DD/YYYY): 09/26/2018

THIS CERTIFICATE IS ISSUED AS A MATTER OF INFORMATION ONLY AND CONFERS NO RIGHTS UPON THE CERTIFICATE HOLDER. THIS CERTIFICATE DOES NOT AFFIRMATIVELY OR NEGATIVELY AMEND, EXTEND OR ALTER THE COVERAGE AFFORDED BY THE POLICIES BELOW. THIS CERTIFICATE OF INSURANCE DOES NOT CONSTITUTE A CONTRACT BETWEEN THE ISSUING INSURER(S), AUTHORIZED REPRESENTATIVE OR PRODUCER, AND THE CERTIFICATE HOLDER.

IMPORTANT: If the certificate holder is an ADDITIONAL INSURED, the policy(ies) must have ADDITIONAL INSURED provisions or be endorsed. If SUBROGATION IS WAIVED, subject to the terms and conditions of the policy, certain policies may require an endorsement. A statement on this certificate does not confer rights to the certificate holder in lieu of such endorsement(s).

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INSURER(S) AFFORDING COVERAGE	NAIC #
INSURER A: American Guarantee and Liability Insurance	26247
INSURER B: Zurich American Insurance Company	16535
INSURER C: ACE Property & Casualty Insurance Company	20699
INSURER D:	
INSURER E:	
INSURER F:	

INSURED
Black Butte Coal Company
101 Black Buttes Road
Point Of Rocks, WY 82942 USA

COVERAGES **CERTIFICATE NUMBER:** W8113289 **REVISION NUMBER:**

THIS IS TO CERTIFY THAT THE POLICIES OF INSURANCE LISTED BELOW HAVE BEEN ISSUED TO THE INSURED NAMED ABOVE FOR THE POLICY PERIOD INDICATED. NOTWITHSTANDING ANY REQUIREMENT, TERM OR CONDITION OF ANY CONTRACT OR OTHER DOCUMENT WITH RESPECT TO WHICH THIS CERTIFICATE MAY BE ISSUED OR MAY PERTAIN, THE INSURANCE AFFORDED BY THE POLICIES DESCRIBED HEREIN IS SUBJECT TO ALL THE TERMS, EXCLUSIONS AND CONDITIONS OF SUCH POLICIES. LIMITS SHOWN MAY HAVE BEEN REDUCED BY PAID CLAIMS.

INSR LTR	TYPE OF INSURANCE	ADDL INSD	SUBR WVD	POLICY NUMBER	POLICY EFF (MM/DD/YYYY)	POLICY EXP (MM/DD/YYYY)	LIMITS	
A	X COMMERCIAL GENERAL LIABILITY CLAIMS-MADE X OCCUR GEN'L AGGREGATE LIMIT APPLIES PER: X POLICY PRO-JECT LOC OTHER:			GLO 5851797-06	10/01/2018	10/01/2019	EACH OCCURRENCE DAMAGE TO RENTED PREMISES (Ea occurrence) MED EXP (Any one person) PERSONAL & ADV INJURY GENERAL AGGREGATE PRODUCTS - COMP/OP AGG	$ 1,000,000 $ 1,000,000 $ 10,000 $ 1,000,000 $ 2,000,000 $ 2,000,000
B	AUTOMOBILE LIABILITY X ANY AUTO OWNED AUTOS ONLY HIRED AUTOS ONLY SCHEDULED AUTOS NON-OWNED AUTOS ONLY			BAP 5851796-06	10/01/2018	10/01/2019	COMBINED SINGLE LIMIT (Ea accident) BODILY INJURY (Per person) BODILY INJURY (Per accident) PROPERTY DAMAGE (Per accident)	$ 1,000,000 $ $ $
C	X UMBRELLA LIAB X OCCUR EXCESS LIAB CLAIMS-MADE DED X RETENTION $ 10,000			G2790869A 004	10/01/2018	10/01/2019	EACH OCCURRENCE AGGREGATE	$ 5,000,000 $ 5,000,000 $
	WORKERS COMPENSATION AND EMPLOYERS' LIABILITY ANY PROPRIETOR/PARTNER/EXECUTIVE OFFICER/MEMBER EXCLUDED? (Mandatory in NH) Y/N If yes, describe under DESCRIPTION OF OPERATIONS below		N/A				PER STATUTE OTH-ER E.L. EACH ACCIDENT E.L. DISEASE - EA EMPLOYEE E.L. DISEASE - POLICY LIMIT	$ $ $

DESCRIPTION OF OPERATIONS / LOCATIONS / VEHICLES (ACORD 101, Additional Remarks Schedule, may be attached if more space is required)
Re: Black Butte Coal Co. Permit #467 and all subsequent renewals.

05-14-2019
PT0467

CERTIFICATE HOLDER

oming Dept. of Environmental Quality
200 West 17th Street
Cheyenne, WY 82002

CANCELLATION

SHOULD ANY OF THE ABOVE DESCRIBED POLICIES BE CANCELLED BEFORE THE EXPIRATION DATE THEREOF, NOTICE WILL BE DELIVERED IN ACCORDANCE WITH THE POLICY PROVISIONS.

AUTHORIZED REPRESENTATIVE
Randi Harwood

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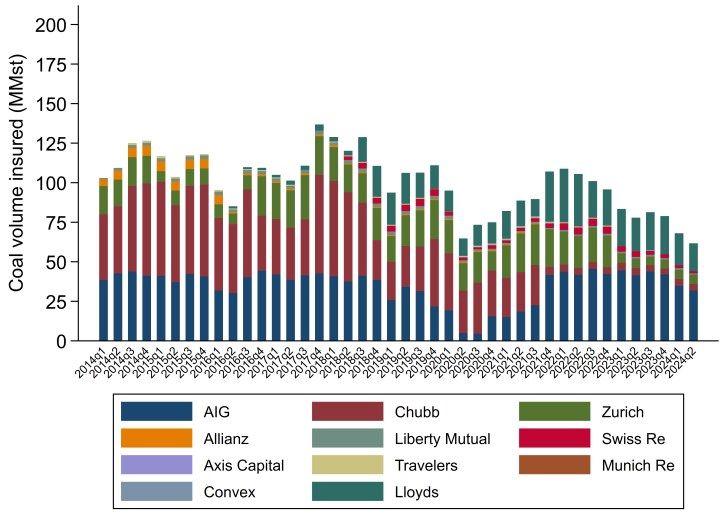
SR ID: 16780344 BATCH: 882111

TFNG 6/339
RECD OCT 10, 2018

Figure IA.5: Volume of coal insured by insurers

The figure displays the volume of coal insured by insurance group. For each quarter, we aggregate the volume of coal produced by the mines that are insured by each insurer. Panel A presents insurers that adopted a coal underwriting policy at any point during the sample period, while Panel B presents those that never did. The sample includes all coal mines listed in MSHA's Mine Data Retrieval System between the first quarter of 2014 and the second quarter of 2024, provided they reported at least one quarter of non-zero coal production, and for which we successfully obtained ACORD 25 certificates of liability insurance from state-level agencies.

Panel A: Insurer with a coal policy



Panel B: Insurers without a coal policy

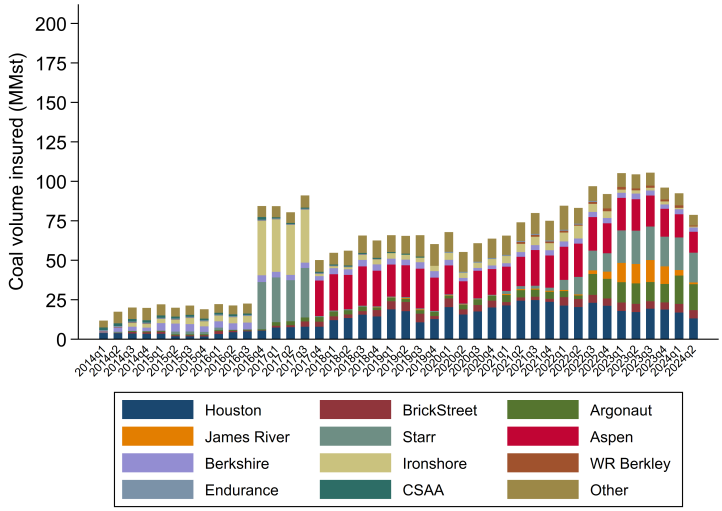


Figure IA.6: Coal mines' insurance status and underwriting policies by policy type

This figure displays 90% confidence intervals and coefficient estimates from the following OLS regression:

$$Inactive\ insurance_{s,m,p,i,t} = \sum_{t=-6}^{t=6} \beta_t \times Coal\ policy_i \times Event\ quarter_{s,t} + \alpha_{s,m,p,i} + \alpha_{s,m,t} + \epsilon_{s,m,p,i,t},$$

where s indexes a stack, m a mine, p a policy type, i an insurer, and t a quarter. The dependent variable, *Inactive insurance*, equals one if an insurer does not insure a mine in a given quarter, and zero if it does. *Coal policy* equals one for mines insured by an insurer adopting a coal underwriting policy, and zero otherwise. $\alpha_{s,m,p,i}$ are stack-mine-policy type-insurer fixed effects, while $\alpha_{s,m,t}$ are stack-mine-time fixed effects. The initial sample of mines includes all coal mines listed in MSHA's Mine Data Retrieval System between 2014Q1 and 2024Q2, provided they report at least one quarter of non-zero coal production, and for which we successfully obtained Certificates of Liability Insurance on ACORD 25 forms from state-level agencies. For each insurer that adopts a coal underwriting policy, we identify all mines it insures and construct a balanced sample of 13 quarterly observations per mine-policy type-insurance triplet, six quarters before and six quarters after the policy adoption quarter, whenever information on the liability insurance certificates of a given mine is available. These mine-policy type-insurance triplets form the treated observations in the stack. As control observations, we include all other mine-policy type-insurance triplets that include the same mines but insurers that never adopt a coal underwriting policy. Standard errors are clustered at the insurance group-policy type-stack level. Table A.1 provides descriptions of all variables.

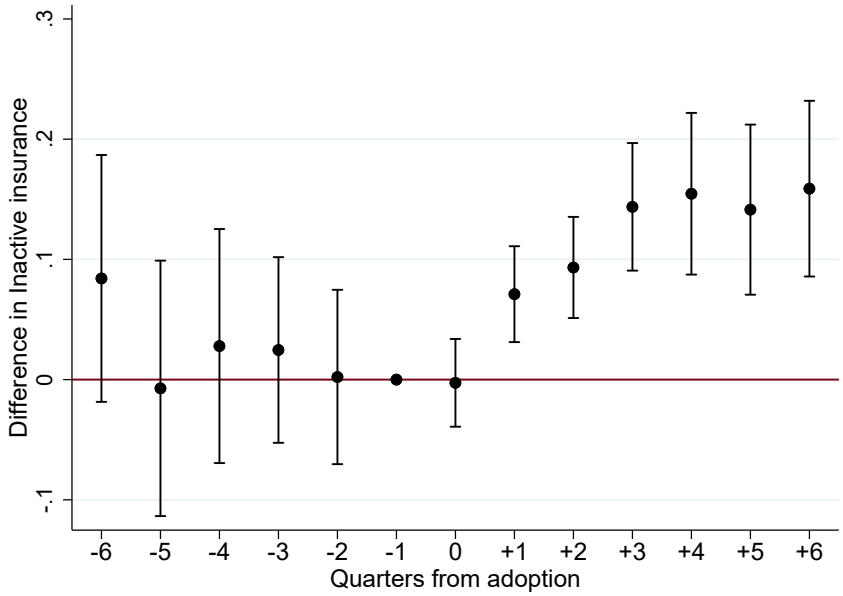


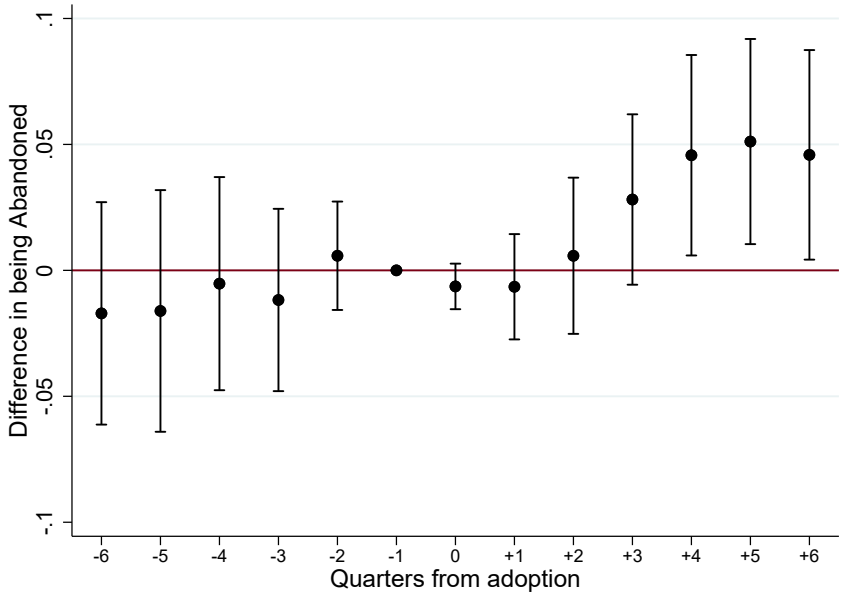
Figure IA.7: Coal mine outcomes and underwriting policies

This figure displays coefficient estimates and 90% confidence intervals from the following OLS regression:

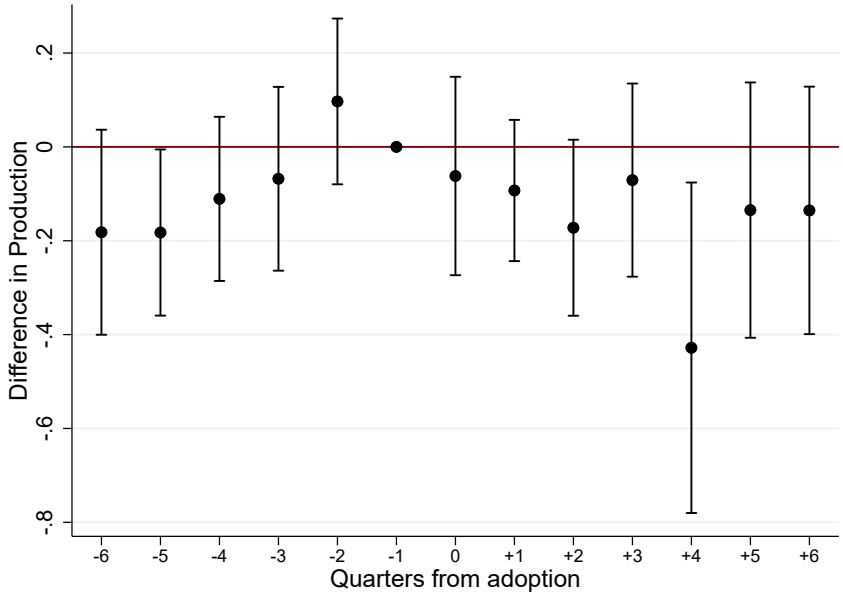
$$Coal\ mine\ outcome_{s,m,t} = \sum_{t=-6}^{t=6} \beta_t \times Coal\ policy\ exposed_{s,m} \times Event\ quarter_{s,t} + \alpha_{s,m} + \alpha_{s,t} + \epsilon_{s,m,t},$$

where s indexes a stack, m a mine, and t a quarter. The dependent variable, *Coal mine outcome*, is a dummy variable indicating whether a mine is abandoned (Panel A), the volume of coal produced (Panel B), or the average number of employees (Panel C). *Coal policy exposed* equals one for mines that, between 2014 and 2016, have a relationship lasting at least eight quarters with at least one insurer with a coal underwriting policy. *Event quarter* are dummy variables indicating quarters relative to policy adoption. If a mine is exposed to multiple such adoptions, only the first instance is used to define treatment. The sample includes all mines from Table 6, provided that we successfully obtained Certificates of Liability Insurance on ACORD 25 forms from state-level agencies between 2014 and 2016. In Panels B and C, the sample includes only mines that are active throughout the entire sample period. Standard errors are clustered at the mine-stack level. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively. Table A.1 provides descriptions of all variables.

Panel A: Abandoned



Panel B: Production



Panel C: # Employees

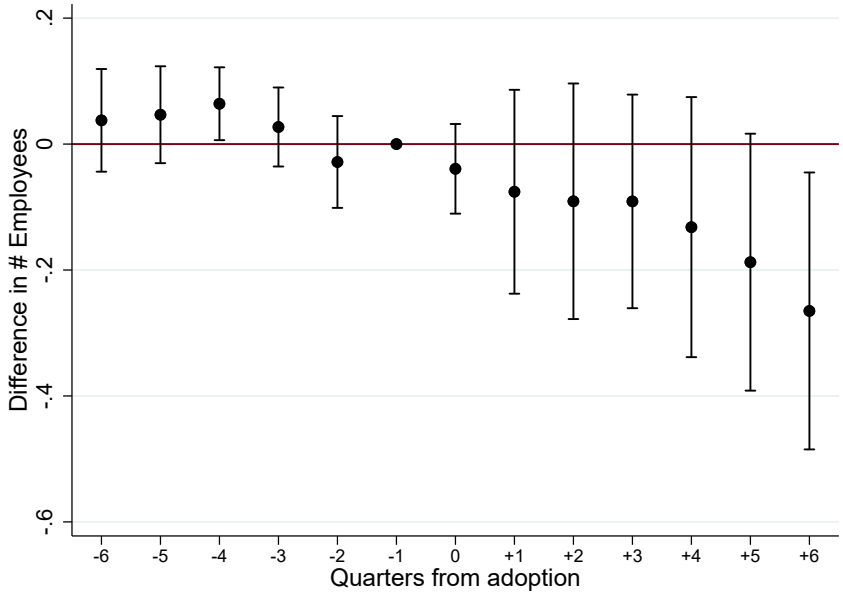


Table IA.1: Assessment of carbon underwriting policies

This table reports the five dimensions over which carbon underwriting policies are assessed by IOF and the respective description.

IOF scoring dimension	Description
Scope	Whether policies rule out insurance for (i) all types of new coal infrastructure (including mines and power plants as well as transport facilities); (ii) extreme fossil fuels such as tar sands, associated pipelines, Arctic and ultra-deepwater drilling; and (iii) any oil and gas expansion projects that drive increased production.
Types of cover	Whether policies apply to all lines of business for new and existing projects and companies, except cover for protecting workers, ringfenced renewable energy projects, and existing mine reclamation surety bonds. Reinsurers' policies should apply to treaty as well as facultative reinsurance.
Fossil fuel companies	Whether policies apply comprehensive criteria to define companies operating in coal, oil and gas, and tighten cover over time in line with the need to phase out these fossil fuels completely. Whether policies immediately exclude new customers from the fossil fuel sector not aligned with a 1.5°C pathway and phase out all insurance services within two years for existing fossil fuel company customers not aligned with such a pathway.
Emissions reduction targets	Whether insurers set emissions reduction targets for new projects as well as ongoing operations which they insure, and define short- and medium-term targets across their entire commercial property & casualty portfolio. The targets must cover the Scope 3 emissions of all carbon-intensive sectors, including energy and power, and aim for a reduction of insured emissions of at least 43% by 2030, in line with IPCC findings.
Human rights	Whether policies include robust due diligence to ensure that clients fully respect all human rights, including a requirement that they obtain and document the Free, Prior, and Informed Consent of impacted Indigenous Peoples.

Table IA.2: Insurer characteristics

This table reports insurance characteristics for all insurers in IOF's scorecard. *Unlisted insurer* and *Specialty insurer* are header variables. *NZIA* is time-varying and we report the maximum value per insurer.

Name	Adoption date	Region	Unlisted insurer	Specialty insurer	NZIA
AIG	01-03-2022	US (United States)	0	0	0
AXA	12-12-2017	Europe (France)	0	0	1
Allianz	04-05-2018	Europe (Germany)	0	0	1
Aviva	n/a (unknown)	Europe (United Kingdom)	0	0	1
Axis Capital	16-10-2019	RoW (Bermuda)	0	1	0
Berkshire Hathaway		US (United States)	0	0	0
Chubb	01-07-2019	US (United States)	0	0	0
Convex	01-02-2022	RoW (Bermuda)	1	1	0
Everest Re		RoW (Bermuda)	0	1	0
FM Global		US (United States)	1	1	0
Generali	21-02-2018	Europe (Italy)	0	0	1
HDI	18-04-2019	Europe (Germany)	1	0	0
Hannover Re	18-04-2019	Europe (Germany)	0	1	1
Legal and General		Europe (United Kingdom)	0	0	0
Liberty Mutual	13-12-2019	US (United States)	1	0	0
Lloyd's	16-12-2020	Europe (United Kingdom)	1	1	1
MS&AD	30-09-2020	Asia (Japan)	0	0	0
Mapfre	11-03-2019	Europe (Spain)	0	0	1
MetLife		US (United States)	0	0	0
Munich Re	06-08-2018	Europe (Germany)	0	0	1
PICC		Asia (China)	0	0	0
Ping An	n/a (unknown)	Asia (China)	0	0	0
Prudential		US (United States)	0	0	0
QBE	01-04-2019	RoW (Australia)	0	0	1
SCOR	06-09-2017	Europe (France)	0	1	1
Samsung FM	12-11-2020	Asia (Korea, Rep.)	0	0	0
Sinosure		Asia (China)	1	0	0
Sompo	23-09-2020	Asia (Japan)	0	0	1
Starr		US (United States)	1	1	0
Swiss Re	02-07-2018	Europe (Switzerland)	0	0	1
TIAA		US (United States)	1	0	0
The Hartford	20-12-2019	US (United States)	0	0	0
Tokio Marine	28-09-2020	Asia (Japan)	0	0	1
Travelers	07-02-2022	US (United States)	0	0	0
WR Berkley		US (United States)	0	1	0
XL Catlin		Europe (Ireland)	0	1	0
Zurich	13-11-2017	Europe (Switzerland)	0	0	1

Table IA.3: Prevalence of oil & gas underwriting policies by insurer

This table lists oil & gas underwriting policies over time for the insurers in the sample. A value of one indicates that an insurer has an oil & gas underwriting policy in place in a year, and a value of zero indicates that an insurer has no underwriting policy. Missing values reflect that an insurer was not included in the IOF scorecards or that its policies were not assessed by IOF in a year. The sample is based on annual scorecards compiled by Insure Our Future (IOF).

Name	2017	2018	2019	2020	2021	2022	2023	% Years w/ policy
AIG				0	0	1	1	50
AXA				1	1	1	1	100
Allianz				0	1	1	1	75
Aviva				1	0	1	1	75
Axis Capital				1	1	1	1	100
Berkshire Hathaway				0	0	0	0	0
Chubb				0	0	0	1	25
Convex					0	0		0
Everest Re					0	0	0	0
FM Global								
Generali				1	1	1	1	100
HDI				1	1	1	1	100
Hannover Re				1	1	1	1	100
Legal & General								
Liberty Mutual				0	0	0	0	0
Lloyd's				0	1	1	1	75
MS&AD				0	0	0	1	25
Mapfre				0	1	1	1	75
MetLife								
Munich Re				1	1	1	1	100
PICC					0	0	0	0
Ping An				0	0	0	0	0
Prudential								
QBE				0	1	1	1	75
SCOR				0	0	1	1	50
Samsung FM				0	0	0	1	25
Sinosure				0	0	0	0	0
Sompo				0	0	1	1	50
Starr						0	0	0
Swiss Re				1	1	1	1	100
TIAA								
The Hartford				1	1	1	1	100
Tokio Marine				0	0	1	1	50
Travelers				0	0	1	1	50
WR Berkley				0	0		0	0
XL Catlin								
Zurich				1	1	1	1	100
% Insurance groups w/ policy				37	43	60	73	
# Insurance groups	25	24	30	30	30	30	30	

Table IA.4: Stringency of oil & gas underwriting policies by insurer

This table reports scores reflecting the stringency of oil & gas underwriting policies over time of the insurers in the sample. The scores range from 0 to 10, with higher values reflecting stricter policies. We rank insurers in all panels based on the 2023 score values (from high to low). The sample period starts in 2020 because policy strength was not assessed before. The sample is based on annual scorecards compiled by Insure Our Future (IOF).

Name	2017	2018	2019	2020	2021	2022	2023	Average score
Generali				1.94	1.91	2.75	4.00	2.65
Aviva				0.56	0.00	4.00	4.00	2.14
Allianz				0.00	1.25	2.88	3.74	1.97
Hannover Re				1.00	1.43	3.00	3.39	2.21
Swiss Re				0.33	1.61	2.88	3.34	2.04
HDI				0.97	0.74	1.75	2.54	1.50
Munich Re				2.50	1.43	3.00	2.41	2.33
Mapfre				0.00	1.43	1.50	2.08	1.25
AXA				1.67	1.79	1.88	1.98	1.83
SCOR				0.00	0.00	1.25	1.68	0.73
Zurich				1.67	0.59	1.38	1.49	1.28
Chubb				0.00	0.00	0.00	1.34	0.34
Axis Capital				2.11	1.07	1.38	1.34	1.47
AIG				0.00	0.00	1.25	1.30	0.64
QBE				0.00	0.74	1.38	1.20	0.83
Sompo				0.00	0.00	1.38	1.17	0.64
Tokio Marine				0.00	0.00	1.25	1.00	0.56
Samsung FM				0.00	0.00	0.00	1.00	0.25
MS&AD				0.00	0.00	0.00	1.00	0.25
The Hartford				0.56	0.59	1.12	0.90	0.79
Travelers				0.00	0.00	1.12	0.90	0.51
Lloyd's				0.00	0.71	0.00	0.07	0.20
Everest Re					0.00	0.00	0.00	0.00
WR Berkley				0.00	0.00		0.00	0.00
Sinosure				0.00	0.00	0.00	0.00	0.00
Liberty Mutual				0.00	0.00	0.00	0.00	0.00
Ping An				0.00	0.00	0.00	0.00	0.00
Berkshire Hathaway				0.00	0.00	0.00	0.00	0.00
PICC					0.00	0.00	0.00	0.00
Starr						0.00	0.00	0.00
MetLife								
Legal & General								
Prudential								
FM Global								
XL Catlin								
Convex						0.00	0.00	0.00
TIAA								
Average score				0.49	0.51	1.17	1.40	
# Insurance groups	25	24	30	30	30	30	30	
# Missing scores	25	24	30	3	0	0	0	

Table IA.5: Correlations between carbon underwriting and investment policies

This table reports correlations between carbon underwriting and investment policies. In Panel A, the table reports the correlations between carbon underwriting policies and carbon investment policies. The sample comprises 32 insurers and spans the period from 2017 to 2023. In Panel B, the table reports the correlations between the scores assigned to carbon underwriting policies and carbon investment policies of insurers. The sample comprises 25 insurers with carbon policies in place and spans the period from 2018 to 2023.

Panel A: Prevalence of policies				
	Coal policy	*Oil & gas policy*	*Coal inv. policy*	*Oil & gas inv. policy*
Coal policy	1.00			
Oil & gas policy	0.62	1.00		
Coal investment policy	0.79	0.65	1.00	
Oil & gas investment policy	0.59	0.87	0.62	1.00
Panel B: Stringency of policies				
	Coal score	*Oil & gas score*	*Coal inv. score*	*Oil & gas inv. score*
Coal score	1.00			
Oil & gas score	0.55	1.00		
Coal investment score	0.81	0.34	1.00	
Oil & gas investment score	0.36	0.33	0.55	1.00

Table IA.6: Descriptive statistics on carbon underwriting policies

This table presents descriptive statistics on carbon underwriting policies. The sample is based on annual scorecards compiled by Insure Our Future (IOF). Table A.1 provides descriptions of all variables.

	Obs.	Mean	Std.	25th	Median	75th
Coal policy	181	0.66				
Coal policy score	162	2.44	2.59	0.00	1.85	3.60
Oil & gas policy	117	0.55				
Oil & gas policy score	117	0.90	1.09	0.00	0.59	1.43
Europe	181	0.44				
US	181	0.25				
Asia	181	0.20				
RoW	181	0.10				
Unlisted insurer	181	0.15				
Reinsurer	181	0.29				
Specialty insurer	181	0.23				
NZIA	181	0.13				
Total assets (billion $)	180	299.98	350.52	61.31	148.14	437.56
ROA (%)	180	1.51	1.83	0.70	1.27	2.23

Table IA.7: Determinants of oil & gas underwriting policies

This table presents results from the following OLS regression:

$$Y_{i,t} = \beta \times X_{i,t-1} + \epsilon_{i,t},$$

where i indexes an insurer and t a year. In Columns 1–3, Y is *Oil & gas policy*, which equals one if the insurer has an oil & gas underwriting policy, and zero otherwise. In Columns 4–6, Y is *Oil & gas policy score*, which is a score ranging from 0 to 10, with higher scores indicating stricter oil & gas underwriting policies. X is a vector of insurer-level characteristics. The sample is based on annual scorecards compiled by Insure Our Future (IOF). Standard errors are clustered at the insurer level and reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively. Table A.1 provides descriptions of all variables.

	Oil & gas policy			*Oil & gas policy score*		
	(1)	(2)	(3)	(4)	(5)	(6)
2021	0.063	0.076	0.073	-0.156	-0.149	-0.161
	(0.083)	(0.081)	(0.079)	(0.243)	(0.262)	(0.311)
2022	0.263**	0.276***	0.308***	0.518	0.710*	0.735*
	(0.101)	(0.095)	(0.097)	(0.345)	(0.398)	(0.398)
2023	0.363***	0.384***	0.390***	0.573	0.873*	0.814*
	(0.106)	(0.100)	(0.122)	(0.366)	(0.428)	(0.435)
US		-0.572***	-0.566***		-1.098***	-1.100***
		(0.140)	(0.132)		(0.253)	(0.388)
Asia		-0.660***	-0.750***		-1.186***	-1.368***
		(0.097)	(0.110)		(0.317)	(0.322)
RoW		-0.349	-0.390		-0.594**	-0.759
		(0.239)	(0.239)		(0.280)	(0.609)
NZIA			-0.002			0.130
			(0.109)			(0.300)
Specialty insurer			-0.275*			-0.333
			(0.154)			(0.399)
Unlisted insurer			-0.324**			-1.149*
			(0.152)			(0.564)
Total assets (log)			-0.069			-0.055
			(0.053)			(0.148)
ROA			-0.021			-0.103
			(0.017)			(0.131)
Obs.	117	117	117	64	64	64
Adj. R^2	0.062	0.395	0.451	0.061	0.275	0.395
# Insurance groups	31	31	31	22	22	22

Table IA.8: Coal mine outcomes and share of underwriters with policies

This table presents results from the following OLS regression estimated at the mine-quarter level:

$$Coal\ mine\ outcome_{m,t} = \beta \times Share\ coal\ policy\ exposed_{m,t} + \alpha_m + \alpha_t + \epsilon_{m,t}$$

where m indexes a mine and t a quarter. The dependent variable, *Coal mine outcome*, is a dummy variable indicating whether a mine is abandoned (Column 1), the volume of coal produced (Column 2), or the average number of employees (Column 3). *Share coal policy exposed* is defined as $\sum_i w_{m,i} Post\ coal\ policy_{i,t}$ where i indexes an insurer, w are equal weights allocated to each insurer with at least eight quarters of active insurance relationship with a given mine between 2014 and 2016, and *Post coal policy* equals one for insurer-quarters after a coal underwriting policy is introduced, and zero otherwise. The sample includes all mines from Table 6, excluding those that do not have an active insurance relationship with the same insurer for at least eight quarters between 2014 and 2016. In Columns 2 and 3, the sample includes only mines that are active throughout the entire sample period. Standard errors are clustered at the mine level and reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively. Table A.1 provides descriptions of all variables.

	Abandoned	*Production (log)*	*# Employees (log)*
	(1)	(2)	(3)
Share coal policy exposed	0.047	-0.220**	-0.207**
	(0.037)	(0.094)	(0.100)
Coal mine FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Obs.	7,342	1,559	1,595
Adj. R^2	0.577	0.896	0.894
# Coal mines	175	38	38